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India Policy Forum
July 12–13, 2016



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Abstract

In the ten years since the rollout of India's National Rural Employment Guarantee Scheme, there has been much research on its implementation and impact. This paper attempts to synthesize knowledge from the vast array of studies. I present four key takeaways. First, there is large heterogeneity in implementation, with consequences for not only where impact is seen, but also interpretation of what "the program" entails. Second, the "Guarantee" in the title is a misnomer, as access is rationed. Third, NREGS seems to have increased rural private sector wages, but led to worse educational outcomes for older children, with contentiously net positive impacts on income and welfare. Fourth key questions pertaining to overall impacts on rural productivity remain unanswered. Although research on these questions will be welcome, current standards for causal inference and availability of data will remain high hurdles for those who wish to take on this challenge.

JEL Classification: H53, I38, J08, J38, J45, J68, O29

Keywords: NREGS, MNREGA, workfare, India, rural employment guarantee

* Preliminary draft. Please do not circulate beyond its discussion at the **NCAER India Policy Forum 2016**, for which this paper has been prepared.

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I thank Clement Imbert and Karthik Muralidharan for helpful discussions, and Michael Kaiser, Kevin Li, Frances Lu and Katherine McAvoy for excellent research assistance.

1 Introduction

It has now been ten years since the first workers were employed in the schemes launched by the Mahatma Gandhi National Rural Employment Guarantee Act (MNREGA).¹ MNREGA entitles every rural household the right to one hundred days of minimum-wage employment per year. The state-level public employment schemes - collectively referred to as National Rural Employment Guarantee Schemes (NREGS) - comprise the largest workfare program in the world. The program has spawned various monikers - “landmark”, “flagship” - as well as justifiable attention from policymakers, politicians, and academics in India and abroad.

Yet despite this attention, and reams of research and opinion on the program, there is still enormous debate over the impact of the NREGS. Supporters have hailed the program as a “tremendous success”, while opponents deride it as an “expensive gravy train”.² While these descriptions were coined in the early years of the program, the debate is still quite active: for example, last year witnessed a vigorous debate played out in the print media between groups of extremely high-profile social scientists.

This paper tries to synthesize knowledge based on serious theoretical and empirical research on this program, and presents avenues for future research. What complicates this task is the vast array of studies on NREGS, of varying quality, on almost every imaginable outcome and aspect of the program. This leads to what I facetiously call (with apologies to Newton) the “third law of NREGS”: *every result has an equal and opposite result*. While it is heartening to see the attention given to this important program, the volume of work makes it impossible for the casual reader to ascertain which direction the weight of evidence points to; finding a citation to bolster your favored opinion of the program is easy. One of the main goals of this article is hence to guide the reader in weighing the evidence on various aspects of the program. In addition, where results seem to contradict each other, I attempt to reconcile them based on differences in data, sample, or methodology. I aim to be clear about whether the evidence is anecdotal or representative, and whether it is merely correlational or causal.

¹MNREGA or NREGA or NREGS - which is correct? Technically the National Rural Employment Guarantee Act (NREGA) - ex post renamed the Mahatma Gandhi National Rural Employment Guarantee Act (MNREGA) - is a piece of legislation that created an entitlement to employment, which was to be fulfilled by state-level schemes, collectively referred to as National Rural Employment Guarantee Schemes (NREGS). But since no one is ever confused about which program is being referred to, many - including myself - refer to these acronyms interchangeably. I will attempt to use MNREGA when referring to the act and NREGS when referring to the actual implementation of the schemes.

²These terms were noted in the following op-ed by Jean Dreze: <http://www.thehindu.com/todays-paper/tp-opinion/employment-guarantee-beyond-propaganda/article1176799.ece>.

Digging through the mound of literature, some clarity emerges. I present four key take-aways:

First, almost every single empirical paper points out the large heterogeneity in implementation of NREGS. While this point may seem obvious, there are two nuances that are perhaps not so obvious. The heterogeneity means that much of the impacts are seen only in better implementing states; even impacts on average are driven largely by “star” implementing states. Even less obvious is the point that with these implementation issues, what exactly one is measuring the impact of is unclear. In other words, the “impact of the program” is not really a logical construct; empirical studies are basically estimating the effects of varying implementation quality.

Second, there is widespread agreement that despite the legal guarantee, on-demand employment is simply not available. Even in the best implementing states, access is rationed, even for the poorest. To what extent this matters is unclear; for example, if most of the demand would have been during the slack labor season, and this is season in which NREGS operates, then the harm from rationing may be limited.

Third, there are at least two outcomes for which the bulk of the evidence suggests impacts. Rural private sector wages seem to have increased modestly as a result of NREGS. On the other hand, this increase in wages may have led to worse educational outcomes for older children. A number of pieces of evidence indicate that incomes and welfare may have increased, but this evidence is not as strong and universal as the previous two outcomes.

Fourth, despite ten years, despite much attention from researchers, and despite access to data and transparency measures that are unprecedented for India, there are still large unanswered questions. Perhaps the most important questions - related to NREGS effects on productivity - remain contested. This is perhaps why there is still so much debate over the program.

Why have researchers not been able to provide definitive answers to big questions? It is a non-trivial task to identify effects of a large program, particularly given the non-random rollout and current high standards for causal inference. Complications arise given implementation problems and the consequent heterogeneity in implementation, and in particular identifying mechanisms demands even more from data and empirical methods. Moreover, even though data access reflects a vast improvement over other programs - compare, for example, the tiny handful of papers published on the Public Distribution System (PDS) in top social science journals versus the proliferation of quality papers in just ten years of the NREGS - problems still remain. For example, even obtaining consistent data over time on

basic aspects of the program for this paper has proven far more difficult than expected, with conflicting sources that are not easy to reconcile.

Finally, how exactly one measures standard for success of a program like NREGS is open to debate. For example, in the case of educational interventions, rupees spent per standard deviation increase in test scores might be one reasonable standard. In the NREGS case, however, there is likely disagreement over outcomes: should income, poverty, welfare, distress migration, insurance be counted? Further, what is the benchmark comparison? A natural benchmark might be cash transfers (Murgai and Ravallion, 2005); however, we likely know even less about the governments ability to target and deliver these payments as well as the potential cost-effectiveness of these alternatives.

The roadmap for the rest of the paper is as follows. Before delving into details on the impacts, I provide some background for the reader who is not well versed with the intricacies of the program. Section 2 begins with salient features of MNREGA, including main provisions and guidelines laid down by the act. The section also includes basic figures and numbers to give the reader a sense of the size of the program, its growth over time, and its reach across space and sectors of the population.

In Section 3, I describe how the program has actually been implemented on the ground - or the “Schemes” as distinct from the “Act”. I begin with highlighting the incredibly heterogeneity in implementation across states and districts, a fact that every interpretation of each empirical study of NREGS must take into account. This heterogeneity exists in key features of implementation such as access, the efficiency of payments, corruption, and other dimensions of of implementation such as worksite facilities and choice of projects.

The next section lays out conceptual frameworks for understanding the impact of NREGS. I begin by presenting the basic theoretical underpinnings for the program, as well as the main theoretical mechanisms for impacts on various outcomes that have been studied by researchers. Following this, I present the main empirical strategies that have been used to identify the impacts of NREGS, laying out the pros and cons of each.

In Section 5, I discuss empirical studies of the impact of NREGS on major outcomes such as wages and employment, income, education, migration, agricultural technology and investment, and conflict. For each outcome, I will attempt to not only summarize the existing evidence but also resolve the direction that the best evidence points to. The survey of the literature aims to be comprehensive, but it is impossible to review every single piece of writing on MNREGA; in order to make the task manageable, this paper includes papers based on objective criteria on sample selection, sample size, and identification strategy that

are defined below.³

Despite the large amount of research on MNREGA, there are still areas that are understudied. I highlight these areas about which we still know very little in Section 6. Finally, Section 7 concludes with some speculation about the future of the program.

2 Background

MNREGA was passed by Parliament and notified in 2005, following up on an electoral promise made by the UPA after it came into power in 2004. Schedule I of the act lays out its basic provisions. The employment guarantee schemes mandated by the act became operational in the 200 “poorest” districts in the country in February 2006.⁴ An additional 130 districts received the program in April and May 2007, while the remaining districts of the country became operational in April 2008. Below I describe the main provisions of the act, and the following subsection provides some basic numbers and figures.

2.1 MNREGA provisions and rules

While anyone with even a cursory acquaintance with MNREGA knows that the act establishes a legal guarantee of a hundred days of employment, there are many other provisions of the act as well as official guidelines that are less well known. For example, a little known feature that may be of interest to researchers is the fact that the Government invites proposals for studies on MNREGA, and will pay for them! (see http://nrega.nic.in/Circular_Archive/archive/Guideliness_Research_Studies_under_MGNREGA.pdf) The fourth and most recent edition of the MGNREGA operational guidelines produced by the Ministry of Rural Development is a rather detailed 232 page document listing workers rights and procedures to be followed by implementing agencies.⁵

Logistical challenges and deviations from these guidelines are to be expected given the current level of implementation capacity across India. I discuss these implementation issues

³Papers published in major economics, political science and development studies journals are automatically included. However, given the long delay in the publication process for these journals, there are numerous excellent pieces of research that exist only as working papers as of now; by using the criteria, I hope to have included as many of these as yet unpublished papers.

⁴In Section 4 below I describe in more detail how these districts were chosen.

⁵This comprehensive document does not preclude the production of other ad-hoc documents with procedures: for example, “Guidelines for Construction of Play Ground in IAP Districts”. See <http://nrega.nic.in/netnrega/guidelines.aspx> for more details.

in Section 3 below. The current section, however, is devoted to documenting the provisions of the Act on paper, which I categorize for ease of consumption into basics, projects, finances, administration, and monitoring.

2.1.1 Basics

To begin with, the 100 day guarantee established by the act applies at the household level for rural households in a financial year. Households are required to obtain jobcards from local government offices, which list all adult members of the household, and also have space for recording work done and payments received. Once the jobcard is obtained, households are supposed to apply for work whenever they need. The local administration must provide employment within 15 days and within 5 kilometers of the applicant's home, failing which the applicant is due to be paid unemployment allowance (for non-provision of work within 15 days) or a travel and subsistence allowance (if work is only available beyond 5 kilometers).

While in general the work involves doing manual unskilled labor and pays minimum wages, there are provisions for other types of work that can pay slightly higher wages (for example, supervisors at job sites). However, 60% of expenditure must be spent on wages. In addition, 33% of jobs must be reserved for women. Finally, wages are to be paid weekly, and no later than a fortnight of the work being done.

2.1.2 Projects

Projects taken up under NREGS are typical of public employment schemes, and include the construction of public goods such as roads and irrigation channels. One of the major goals of MNREGA is related to water management, with both drought-proofing and flood management as priority projects. Projects are meant to be chosen by the Gram Sabha, or the full meeting of the village, in consultation with the block and district administration. This process should produce a roster of projects in advance of the financial year that can be taken up as demand dictates.

It is also possible for NREGS work to take place on private land, if the land is owned by Scheduled Castes/Tribes. For example, clearing of land for cultivation is a common such activity in order to provide livelihoods to the most disadvantaged groups. In any type of project, there are also provisions for worksite facilities such as drinking water, shade, and a creche for workers' children.

2.1.3 Finances and payments

In order to incentivize States to generate employment, the Central government pays all labor costs fully, but only 75% of material costs. Since wage rates are set by States in order to adjust to local conditions, this feature also incentivizes States to increase wage rates, and has been a bone of contention between Central and State governments since the inception of the act. Workers may either be paid a daily wage rate or piece rates depending on the amount of work done.

Initially, payments were made in cash by the same administrative bodies in charge of implementation. However, the most recent guidelines explicitly call for payments to be made to bank or post office accounts, and also for separation between implementing and payment officials. Cash payments are still possible, particularly in areas in which bank or post office branches are inaccessible, although according to the rules administrative bodies must obtain prior permission to make such payments.

2.1.4 Administration

Given that the act explicitly states that Gram Panchayats (GPs) - the lowest administrative tier of the Indian bureaucracy - must implement at least 50% of the works in terms of cost, MNREGA has advanced the legitimization of GPs as state actors. The GP is in charge not only of implementing works, but also of keeping records (particularly “muster roles” of work attendance and payments) and transmitting them to higher levels of administration. In these tasks the GP is now assisted by a wider array of local officials, some of whose positions were created by the Ministry of Rural Development explicitly to assist with MNREGA. These include the *Gram Rozgar Sahayak* or Employment Guarantee Assistant, Mates or work site supervisors or Field Assistants, Technical Assistant to measure and monitor work, Computer Assistant to keep and update and transmit records, in addition to the elected (e.g. *Sarpanch*) and appointed (e.g. GP Secretary) officials who are in charge of the program overall.

The Block and District level administrations are meant to support and assist the GP in implementation, as well as implementing some work directly through line departments (for example Irrigation department). These entities are meant to have Programme Officers in charge of MNREGA implementation. These officials approve work plans and in some cases also budgets, although this provision may be changing recently.

2.1.5 Monitoring

MNREGA is unusual in creating provisions for transparency and monitoring from the outset. The Monitoring and Information System (MIS) gives anyone with an internet connection unprecedented access to official records of works and payments. It relies on custom-built and sophisticated software - “NREGASoft” - to document every aspect of the program electronically, with the updated operational guidelines including detailed instructions on how to enter information into the software.

District and state administrations are required to audit the works and expenses of GPs and Blocks. In addition, “Social audits” are meant to be performed once every six months in order to ensure accountability as per Rule 4 of Audit of Schemes Rules, 2011. These involve public verification of muster rolls and other expenditure in a Gram Sabha, and can involve workers as well as civil society organizations. The audits are also meant to be a platform for workers to air grievances.

2.2 Figures and numbers

NREGS is a large program by any account. Over 121 million rural jobcards are registered for the program as of 2014 (Figure 1), which would amount to practically all of India’s rural population if each household had only one jobcard, although that is surely not the case.⁶ The total number of person-days on the program in 2013-14 were 2.20 billion, which actually is a decline from the peak of 2.83 billion in 2009-10 (Figure 2). Nearly 48 million individuals actually worked on the program in 2013-14, corresponding to 24.4% of rural households (Desai, Vashishtha and Joshi, 2015).

A striking feature of the program is participation by women. Since the program pays the same amount for women as men, while women are likely underpaid in the private sector market, women’s participation is high, amounting to an average of 47.5% over the years 2006-2012. Participation by marginalized sections of society is also high, with SC/ST accounting for 49.4% of persondays (Figure 3).

Expenditure on the program amounted to about Rs. 37,000 crores in 2013-14.⁷ This makes it amongst the biggest public programs in India, although by far not the single largest public

⁶Calculations by Muralidharan, Niehaus and Sukhtankar (n.d.) suggest 1.9 jobcards per NSS defined household.

⁷Despite the huge efforts in data transparency, it has proven difficult to pin down a precise figure for expenditure in the last two financial years, with various conflicting figures available that are not easy to reconcile. Thankfully they are all in the same ballpark.

expense. In comparison, subsidies for kerosene alone amounted to Rs 30,574 crores in 2013-14. Fiscal expenditure on fertilizer subsidies - including consumer and producer subsidies - amounted to Rs. 73,790 crores in the same year (Ministry of Finance, 2014).

Comparisons to programs across the world can be made by calculating expenditure as a percentage of GDP. NREGS expenditure in 2013-14 was 0.33% of GDP in India. In comparison, US spending on Medicaid was 2.9% of GDP, while spending on TANF (welfare) was 0.19% of GDP.⁸

The trend in program statistics over its lifetime suggests that expenditure and participation on NREGS rose steadily as it scaled up from 2006 onwards, but have declined somewhat over the last three to four years (Figure 4). For example, total expenditure on NREGS (in 2006 rupees) dropped to Rs. 14,982 crore in 2014-15 from Rs. 27,736 crore, its peak in 2009-10. Person-days have also dropped, as has expenditure per person-day, although how much this is due to actual declines as opposed to reducing corruption (see Section 3 below) is not known. Meanwhile, the utilization of funds available has steadily increased, and recently state and local governments are using almost all the funds budgeted (Figure 6).

3 Implementation

It will come as no surprise to anyone that the exhaustive list of guidelines laid down by MoRD for MNREGA implementation is not followed to the letter, and the numbers cited above are not entirely perfect. What is important for practical purposes, however, is the extent and nuance of differences between on-the-ground practices and on-paper ideals. In this section I present evidence collected on implementation of key features of MNREGA.

3.1 Heterogeneity in implementation

The most remarkable characteristic of NREGS implementation is the enormous heterogeneity in implementation quality across and even within states. Heterogeneity of implementation across states has been a feature since the very beginning of the program. Dreze and Oldiges (2009) in commenting on performance in the first two years pointed out that just three states - Rajasthan, Madhya Pradesh and Chhattisgarh - were responsible for over half of the total employment generated. Imbert and Papp (2015a) coined the term “star states” to describe

⁸Sources: Medicaid spending and GDP from www.cms.gov, and TANF spending from www.acf.hhs.gov.

the seven leading states, where employment on NREGS accounted for over one percent of all working days in rural areas.

This situation has not changed much in recent years; Figures 7 and 8 show the large variation in NREGS employment and expenditure per capita across the 15 largest states in India in 2013-14. While Andhra Pradesh and Tamil Nadu consistently rank amongst the states with best implementation, poorer states like Bihar and Uttar Pradesh, where one might expect high demand for NREGS, implement with as much intensity as much richer states like Gujarat and Maharashtra (with presumably much lower demand).

There are a number of reasons for this heterogeneity. Most obviously, local labor market conditions and the need and demand for employment differ across states. However, as in previous examples of employment guarantee schemes (Ravallion, Datt and Chaudhuri, 1993), supply constraints are also extremely important. These supply constraints are related to both implementation capacity (and the way the program is implemented at the local level) as well as political incentives.

Witsoe (2014), using detailed anthropological work in one of the worst-performing states - Bihar - documents how the idealized world of MGNREGA is a far cry from the reality of how NREGS is implemented. He notes that “the state government lacks the capacity to run projects as documented... Limitations on state capacity are complex, ranging from inadequate staffing, training and salaries to an inability of officials to navigate panchayat politics and the entrenched opposition of landowners.” On the other hand, in Andhra Pradesh the government was able to build on the network of Self-Help Groups to employ women as Customer Service Providers (CSPs) and successfully implement the Smartcards initiative to markedly improve functioning of NREGS (Muralidharan, Niehaus and Sukhtankar, n.d.).

In addition to administrative capacity, the motivation of bureaucrats and politicians to implement NREGS also matters. Gulzar and Pasquale (2015) show that political incentives affect implementation of NREGS, using a clever empirical strategy to identify the effects these incentives. Boundaries of political constituencies and administrative units often do not coincide in India; thus administrative units (blocks) can be entirely within political constituencies and answerable to a single politician, or split across constituencies and answerable to multiple politicians. The authors find that person-days as well as the number of people employed in NREGS is higher when blocks are entirely within constituencies as opposed to split across constituencies, even when the comparison is restricted to boundaries of blocks within the same constituencies (where one block is split and the other is not). They argue that politicians are better able to motivate block officials to implement NREGS when blocks

are not split largely because the politicians can then more unambiguously claim credit for improvements. This paper makes clear how even within district differences in implementation quality may arise.

The heterogeneity in observed NREGS implementation is important for at least two reasons. First, it affects the interpretation of results and observations that rely on small and selected samples. This is the normal caveat related to the extrapolation of results from one area to another. Second, one-size-fits-all solutions must also be viewed with care.

3.2 Implementation of key features of MNREGA

The deviation of the abstract ideal of MNREGA from the reality of NREGS is best captured by a quote from Witsoe (2014):

There are, in fact, three distinct NREGAs. The first is the NREGA enacted through legislation, the vision of which is operationalized through a centrally maintained documentary system. The second is the NREGA practiced by a vast bureaucracy under control of state governments, whose main task is the production of documentation within the broad parameters of the centrally maintained architecture. Since this documentation is compiled into data and reports, this is the NREGA most visible to academics. And lastly, there is the NREGA as practiced in villages.

Below, I present evidence on the extent of differences between the act on paper and the schemes as practiced in villages on the key dimensions of the act, including access to work and payments.

3.2.1 Access

Of all the features of MNREGA, the most important is simply access to work on demand. How has the NREGS fared in fulfilling this key provision? The most comprehensive work on this suggests that there is still a long way to go to fulfill the ambition of employment when desired. Dutta et al. (2012) use data from the 2009/2010 National Sample Survey (NSS) which is representative across India to find that 44% of those who desired work on NREGS did not get it. As suggested by the previous section, there is wide variation across states, with rates varying from 15% to 84%. Similar results are found in data from the India Human Development Survey 2011-12 (IHDS), with 60% of participating households desiring more work but not able to obtain it, and 29% of all rural households experiencing rationing of some

kind (Desai, Vashishtha and Joshi, 2015). Moreover, employment and spending on NREGS is not strongly correlated with poverty across states, and poorer states have greater unmet demand for NREGS. The lack of capacity described above could be one factor explaining this.

Evidence from our work in Andhra Pradesh suggests that this problem has not gone away. Andhra Pradesh is generally considered one of the better implementing states - it is a “star state” as defined by (Imbert and Papp, 2015*a*). However, even here in our endline survey in 2012 we found that only 4% of respondents answered yes to a question that asked “In general, can anyone in this village who wants work on NREGS get it?” Access looks better when asked about specific personal experiences, with only 20% of respondents answering that they were unable to get work despite asking for it in May, while 42% of respondents answered that they were unable to get work despite asking for it in January.⁹

The seasonality indicated by these differences in May and January is common across India. As suggested by the fact that a higher number of respondents were unable to get work in January despite asking, the seasonality is not simply due to variation in demand. It is accepted that no one really “applies” for work; projects happen when planned for the slack labor season. For example, Witsoe (2014) notes that “contractors are the ones who initiate projects and they are therefore the ones who actually generate demand for work.” Further evidence from Dutta et al. (2013) based on a randomized experiment in Bihar suggests that increasing awareness of workers rights, including the fact that workers can demand work when needed, does not lead to actual increases in employment. This strengthens the argument that supply, rather than demand, constrains NREGS employment.

In general, state and local governments seem to plan NREGS projects for the slack labor months of May and June prior to the monsoon, while work generally does not happen in peak harvest seasons of December and January. There is some suggestion that part of the seasonality may due to elite pressure. For example, Lakha and Taneja (2009) suggest that “in many parts of rural India, poverty-alleviation schemes such as the NREGS face resistance from landlords whose control over labour is threatened by the availability of employment opportunities outside their authority.” Similar broad arguments have been made by Anderson, Francois and Kotwal (2015), who contend that landlords seek to control local governance in order to suppress wage pressure from schemes like the NREGS. More conclusive evidence

⁹If more people wanted work in May rather than January - which seems likely - then the weighted average rate would be very similar to the 25% reported rate for Andhra Pradesh from the Dutta et al. (2012) data in 2009-10.

of widespread collusion between local governments and landlords to smother NREGS implementation is lacking, however, since such evidence would clearly be difficult to obtain directly.

3.2.2 Targeting

How well does the self-selection aspect of MNREGA work to target the poor? On the one hand, the poor are more likely to work on NREGS than the non-poor, with 30% of poor households participating as compared to 21% of non-poor (Desai, Vashishtha and Joshi, 2015). Households in which no adult is literate are also much more likely (30%) to participate than households where at least one adult is a graduate (13%). Again, implementation matters, as 60% of poor households participate in NREGS in better implementing states, as suggested by data from the India Human Development Survey 2011-12. In the glass half empty column, however, is the fact that 70% of poor households did not participate in NREGS that year, and mostly because they were not able to access work, not because they did not desire to work.

Other papers corroborate both the targeting and rationing results above (Gaiha et al., 2010). This paper also suggests that increasing statutory wages on NREGS to the point where it is substantially higher than the agricultural wage worsens targeting.

3.2.3 Payments

Operational guidelines clearly state that payment for work must be made within two weeks of the work being done. This is far easier said than done; unless funds are made available in advance to GPs, work must be recorded, these records uploaded, which triggers disbursement of payments down the hierarchy from states to districts to blocks to GPs. While attempts to circumvent this system are underway (Banerjee et al., 2015), the best available evidence suggests that simply getting payments on time and in a predictable fashion is a recurring problem. Even the operational guidelines state that “Timely payment of wages has emerged as one of the main challenges of Mahatma Gandhi NREGA over the last few years.” (Ministry of Rural Development, 2013)

Anecdotal evidence on payment delays abounds, with extreme cases such as suicides related to delayed wages capturing media attention (Pai, 2013). Representative data at the all India level is difficult to obtain, with the NSS not asking specific enough questions about the lag between work and payment receipt. State level evidence from our work in Andhra Pradesh

suggests that the mean lag is over a month (34 days), easily more than the two weeks allowed on paper (Muralidharan, Niehaus and Sukhtankar, n.d.). Again, Andhra Pradesh is one of the better performing states.

While delays in payments receive media attention, the banal act of collecting payments does not. The fact that there is no mention of this in extensive operational guidelines suggests the lack of importance to this given by policymakers. However, the time spent on collecting payments can result in significant lost wages. On average in Andhra Pradesh workers spent close to two hours collecting their money. Moving to electronic funds transfer and biometric authentication using Smartcards reduced this collection time, as well as delays in payments, considerably (Muralidharan, Niehaus and Sukhtankar, n.d.).

Perhaps the biggest stick wielded by NREGS critics is that of corruption in the program. For example, Bhalla (2011) contends that programs such as NREGS comprise scams that are annually the size of the famous 2G corruption scandal. While even its most fervent supporters would not deny that some funds are captured by middlemen, putting a precise number on country-wide annual leakage is very difficult.

A good starting point is the estimate by Imbert and Papp (2011), updated in Imbert (2014). They use representative National Sample Survey data from 2009-10 and 2011-12, which asks respondents how many days they were employed on NREGS, and compare these numbers to official figures on employment provided from the Ministry of Rural Development. They find that the household reports can only account for 42-56% of the official reports in 2009-10, while the number improves to about 80% in 2011-12. However, these numbers are not a precise estimate of leakage of NREGS funds since they are in terms of days not Rupees. A major complicating fact is that many states pay piece rates rather than daily wages, and workers often work on NREGS in the mornings while working on their own farms later in the day, making a comparison in terms of days biased.

If we did take these figures seriously, we could calculate fiscal losses due to leakages. This would amount to approximately Rs. 19,000 crore in 2009-10, and Rs. 7,500 crore in 2011-12. For comparison, leakage rates on the PDS range from 44-58% in the years 2004-05 and 2007-08. The fiscal loss to the government of these leakages in 2013-14 amounts to Rs. 28,500 crores (Ministry of Finance, 2014), compared to the mean “scam” value of Rs. 36,000 crore as calculated in Sukhtankar and Vaishnav (2015).

The only way to improve on the basic methodology would be to obtain official records of employment and payments and attempt to track these down to actual beneficiaries. Clearly this is labor intensive and would be difficult to do on a nationwide basis. Moreover, any

partial sample would need re-weighting to obtain population estimates, and this is a non-trivial task since for many states the number of households in existence does not correspond exactly to the number of jobcards in existence. Our level estimates using this methodology in Andhra Pradesh suggest leakage rates of 30.7% in 2012 (Muralidharan, Niehaus and Sukhtankar, n.d.). Previous estimates in Orissa with a similar methodology estimated rates of 70-80%, but that survey had a much longer recall period of 6-8 months; moreover, they pertained to early years of the program, and to what are regarded as particularly poorly administered districts (Niehaus and Sukhtankar, 2013*a,b*).

In addition to levels of corruption, there is also some limited evidence on determinants of corruption. The broad takeaway from Niehaus and Sukhtankar (2013*a*) is that bureaucrats respond acutely to incentives to steal from the NREGS, trading off increased rents today for the ability to stay in the job and steal more tomorrow. In Niehaus and Sukhtankar (2013*b*), we find that workers' ability to use their voice to tackle corrupt bureaucrats may be limited, unless possibly helped by NGOs.

Finally, the AP Smartcards experience suggests that strengthening state capacity to implement the program may reduce corruption in a highly cost-effective manner. By building a new payments infrastructure relying on electronic transfers to NREGS workers and biometric authentication at the time of payment collection, leakage reduced by 40% (Muralidharan, Niehaus and Sukhtankar, n.d.). Another technological innovation - this time from the other end of the implementation capacity spectrum, in Bihar - suggests that using an electronic system for fund request and transfer that bypassed the middle layers of districts and blocks also reduced corruption in NREGS (Banerjee et al., 2015).

4 Conceptual frameworks

In order to understand the impact of NREGS, I present first the theoretical basis for impact, and then the most commonly used empirical strategies for estimating impact.

4.1 Theoretical

The basic premise underlying a workfare program like the NREGS as opposed to welfare involves self-selection (Besley and Coate, 1992). Since it is difficult for governments, particularly in developing countries like India, to evaluate households' income, adding on a work component helps target funds to those who are poor. In addition, the on-demand nature of

NREGS is meant to tackle the information problem that the government may not know exactly when particular households need assistance. The self-selection logic has a long history; apparently it was the rationale for public works programs in India under the British as well as similar programs in the US (Dreze and Sen, 1990).

The starting point for examining the impact of a program like NREGS is of course its impact on labor markets, particularly on private sector wages and employment. It is straightforward to show that in the case of perfectly competitive markets, the introduction of NREGS with a wage above the market wage will lead to increases in the private sector wage and falls in private sector employment (Imbert and Papp, 2015*a*). Under non-competitive markets, such programs may actually increase both wages and employment (Basu, Chau and Kanbur, 2009). Whether these labor market impacts lead to increases in household annual income depends on labor supply and demand elasticities in both cases, although income increases are clearly more likely in the latter case with non-competitive markets.

To this standard framework Basu, Chau and Kanbur (2009) add important twists related to access and credibility. As highlighted multiple times already, implementation of NREGS is uneven and access imperfect; even with an on-paper guarantee, discretion over access is afforded to local agents. Under this scenario, the government must choose both the wage and access; Basu, Chau and Kanbur (2009) show theoretically that the government could target aggregate employment and achieve the right mix of private and government employment. However, in order to be able to do so, its commitment to provide the set wage and access targets must be credible.

These labor market channels are the main channels of impact of NREGS. There are two additional potential channels of impact. The first additional channel relates to the public goods or assets created. If these public goods lead to an increase in productivity - for example irrigation canals could increase agricultural productivity - this may lead to increased income. As second additional possibility is related to the insurance channel; if NREGS is able to protect households against shocks and improve insurance, this may - for example - lead farmers to invest in higher variance but higher average yield crops.

Finally, it is worth mentioning channels that work simply through the flypaper effect, or additional funds that flow into rural areas, and hence are not necessarily extra channels compared to standard welfare schemes. The first channel is the aggregate demand/ multiplier channel: the additional flow of funds could increase local economic activity if there are local scale economies or internal trade frictions (Krugman, 1991). A second channel could be that the additional money helps reduce credit constraints more generally in areas receiving

NREGS.

Impacts on other outcomes derive chiefly from these above described impacts on labor market outcomes, incomes and insurance. For example, one might see impacts on education related to changing labor market opportunities for child versus adult labor, or to simple income effects. Similarly, changes in labor market opportunities as well as income might affect migration, civil conflict, or health.

4.2 Empirical

In this section I discuss the main empirical strategies used to identify the effects of the program. At the outset it is important to figure out what the strategies are identifying the impact of. This is not as obvious as it seems. Given the wide deviations from the abstract world of MNREGA discussed in Section 3 above, in fact there is no logical construct pertaining to the effects of “the program”. Any empirical study claiming to study the impact of NREGS is thus simply estimating the effects of varying implementation quality. One could conceptualize a scale from 0-100, with zero being no program and 100 being a perfectly implemented program; every paper that identifies the “impact of the program” actually identifies the effect of moving along the scale. For example, studies that use the rollout of the program for identification might be identifying the impact of going from 0 to 20 on this scale; our paper on the Andhra Pradesh Smartcards experiment might be identifying the impact of moving from 50 to 70 (all numbers arbitrary).

Given the vast proliferation of studies attempting to examine the impact of NREGS, it is impossible to cover the results of every single study. The difficulty is magnified since many are still in working paper stage - given long publication processes - making it harder to judge quality. To be as inclusive as possible, I used a variety of academic search platforms, including Google Scholar, EconLit, and JSTOR, with various combinations of search terms including India, national rural employment guarantee, and every acronym used for the act and schemes. From this large list, papers that were published in the main economics, political science, and development journals were automatically included.¹⁰ The remaining unpublished papers were subjected to the following inclusion criteria

1. *Sample selection and representativeness.* The first criterion is that samples selected must be representative, i.e. randomly drawn from a larger universal sample, particularly at

¹⁰Despite these efforts, it is more than likely that I will have missed some papers, particularly working papers. Since the aim is to be inclusionary rather than exclusionary, I welcome suggestions for papers to be considered, and apologize in advance to authors.

the level of the unit of analysis: generally households and villages. Some arbitrariness in selection at larger units of aggregation - for example states and districts - is inevitable given logistical challenges as well as simply studies that intentionally plan to study just state.¹¹ However, the unit that is analyzed must not suffer from selection bias.

2. *Plausible identification strategy.* The second criterion, and the hurdle that most empirical studies of NREGS impact stumble upon, is that papers must aim to separate out causation from correlation. For reasons described below, this hurdle is particularly challenging in the case of NREGS, but papers must at least attempt to deal with the problem head-on, and describe the threats and challenges clearly.
3. *Sample size and effective sample size.* This criterion applies in the following cases: i) a study attempts to show no effect, but does not adequately discuss whether it has the statistical power and sample size to detect an effect and ii) a study attempts to show an effect, but does not adequately cluster standard errors for intra-cluster correlation.

4.2.1 Common strategies: advantages and pitfalls

In order to not repeat when discussing individual papers, I describe below the main empirical strategies used in assessing impact, and discuss their advantages and pitfalls as they relate to the particular instance of the NREGS.

1. *Experiments.* The gold standard for causal inference is a randomized experiment. In the most famous case of evidence from an RCT used to determine public policy, the initial rollout of Mexico's PROGRESA was randomly assigned, and provided incontrovertible evidence in favor of its impact, thus leading to both scale-up as well as the ability to withstand political pressure after a change in government. In the MNREGA case, the initial rollout was obviously not randomized. However, given difficulties in implementation, there is scope for experimentation via interventions that improve functioning, as in the Muralidharan, Niehaus and Sukhtankar (n.d.) case. Nonetheless, there are at least two difficulties in conducting RCTs on NREGS; first, getting governments to agree to randomize, and second, doing so at a large enough scale. The latter, in particular, is an underappreciated issue: given the size of NREGS, it is bound to have general equilibrium effects, and in order to capture these effects the *size* of units randomized, not just the *number* of units randomized, must be large (Muralidharan, Niehaus and

¹¹Of course most large states in India are larger than most countries in the world.

Sukhtankar, 2016). The common criticism of experiments is that while they have strong internal validity, they may lack external validity as they may be done on smaller samples. Of course, the same issue applies to many non-experimental studies as well, for example those that only focus on one state.

2. *Difference-in-differences.* Given the non-experimental but staggered rollout, the obvious and commonly used strategy is a difference-in-differences approach. This strategy allows for level differences between districts in various phases of rollout, as long as the trend in outcomes prior to implementation of MNREGA was the same. Unfortunately, the major problem with this strategy is that trends were not actually parallel for the vast majority of outcomes studied. For example, wages in Phase 1 and 2 districts seemed to be converging to wages in Phase 3 districts, as seen in Figure 2 in Berg et al. (2012). Figure 6 in Imbert and Papp (2015a) also confirms that pre-trends in wages were not parallel, using NSS data. Given that rural wage series were not parallel, the likelihood is that other outcomes also do not exhibit parallel trends. A second, possibly under-appreciated issue with this strategy is related to the implementation issues highlighted in Section 3. Given these difficulties, the estimated impacts may be of limited value when extrapolating to steady-state NREGS implementation. Moreover, since NREGS mainly works in the summer months, one year of difference between Phase 1 and Phase 2 implementation may not correspond to a large difference in intensity of treatment.
3. *Regression Discontinuity.* Given the lack of parallel trends in difference-in-differences designs, some researchers have used regression discontinuity designs to estimate the causal impact of NREGS implementation. This idea is based on the assumption that MNREGA phases were assigned by an algorithm based on an underlying continuous variable that measured backwardness, with an arbitrary cutoff determining the 200 districts assigned to Phase 1. Thus districts just above and below the cutoff value are quasi-randomly allocated to receive NREGS treatment early or late. The problem with this strategy is that the selection of districts for Phases was not fully based on a technocratic algorithm. There are at least two known deviations, with states and districts facing Naxalite issues prioritized, and also each state had to have at least one district in Phase 1. Other political considerations, particularly at the state level, are also possible (Chowdhury, 2014; Gupta, 2006). Thus the discontinuity is not very sharp, and the controls used for the underlying forcing variable assume importance. This strategy also faces the issue highlighted above, which is related to limited implementation in

early years.

4. *Other.* While the above two represent main empirical strategies, other strategies including those based on using household panels and those based on using instrumental variables (IV) have also been used by researchers examining the impact of NREGS. For these strategies, the standard issues remain: panels don't account for selection in and out of NREGS, and IV strategies must not have weak instruments and must satisfy the exclusion restriction.

5 The Impact of NREGS

In this section I discuss the impact of the NREGS on six main categories of outcomes: labor markets, income and consumption, education and health, migration, agricultural choices, and conflict. In each case, I attempt to distill the key takeaways from the current literature, and also highlight areas of conflicting research or missing knowledge.

5.1 Labor markets

Perhaps unsurprisingly, the impact of NREGS on labor markets has been intensely studied. At least four papers study the impact using nationwide data and the phased in rollout for identification, with another paper relying on experimental variation in Andhra Pradesh. The main outcomes studied are private sector wages and employment.

The only published paper (Imbert and Papp, 2015a) uses the difference-in-differences approach (empirical strategy 2 above) and NSS data for the periods 2004-05 (pre-MNREGA) and 2007-08 (post-MNREGA) to find that private sector wages for casual labor (temporary, informal contracts as defined by the NSS) rose by 4.7 percent as a result of NREGS. This strategy basically compares trends in Phase 1 and 2 districts (“early phase”) to Phase 3 districts (“late phase”). They further find that the wage effect is concentrated in “star states” - those states that implemented NREGS well. They attempt to control for the fact that wages in the early phase districts were already converging to wages in late phase districts prior to MNREGA by using district-level pre-program controls for caste composition, agricultural wages and agricultural output per worker, as well as time-varying controls capturing rainfall and the election cycle (separately for early and late phase districts). Worker level controls attempt to control for selection into the labor force. The paper also finds that NREGS employment crowds-out private sector employment (defined here as including casual labor,

salaried work, domestic work, and self-employment) almost one-for-one - a 1.17 percent increase in public sector employment to a 1.46 percent decline in private sector work.

Using a similar approach and the same data, Azam (2012) finds broadly similar results. He finds an increase in public employment resulting from NREGS, as well as an increase in labor force participation, particularly for women. The results on wages are strikingly similar, with a 5 percent overall increase in real wages due to NREGS. This paper also presents the increase in wages separately for men and women, and finds that the increase for women (8 percent) is higher than the increase for men (3.8 percent).

Using a similar approach and *different* data, Berg et al. (2012) also find extremely similar overall wage results as the previous two papers. This paper uses the Agricultural Wages in India (AWI) series which provides wage rates at the month-district level, separating out wages for unskilled labor and skilled labor, and even including various sub-categories such as field labor including ploughing, sowing, weeding, and reaping. The headline result is that real agricultural wages increased by 4.8 percent per year as a result of NREGS, with the effects concentrated in the main agricultural season (i.e. the non-NREGS season). The authors find no difference in impact on men's versus women's wages.

A change in empirical approach, however, leads to somewhat more substantial differences in results. Zimmermann (2015) uses a regression discontinuity approach - empirical strategy 3 above - and the same NSS data to argue that NREGS did not lead to increases in public employment or private sector wages. The advantage of the RD approach is that it does not need the parallel trends assumption. On the other hand, it requires other assumptions on assignment to treatment and control, which are difficult to evaluate since the "algorithm values are not directly publicly available"; and also is most meaningful for the districts closest to the cutoff between Phases 2 and 3. In addition, it is important to note that the comparisons are between different samples: Zimmermann (2015) compares Phase 2 to Phase 3 districts, while the other papers all compare both Phase 1 and Phase 2 districts to Phase 3 districts. Since Phase 2 started in April and May 2007, while the NSS data collection started on 1 July 2007, some of these Phase 2 districts will have barely had any experience with NREGS; as the author notes, given implementation issues and the time taken to scale up the program, many Phase 3 districts did not actually receive the program until after July 2008 even though officially the program started in April 2008. So the non-results in this paper are not incompatible with the positive wage effects found in the other papers, particularly given the large standard error bounds.

Although the results from the different approaches are reconcilable, it would be ideal if

there were a way to positively adjudicate using an empirical strategy that relies on less strong assumptions. While experimental evidence at the all India level is not available, our experiment examining Smartcards which improved the functioning of NREGS in Andhra Pradesh provides a benchmark (Muralidharan, Niehaus and Sukhtankar, 2016). We find that private sector wages for unskilled labor increased by 5.7 percent in the month of June as a result of improvements in NREGS, consistent with results in Imbert and Papp (2015a); Azam (2012); Berg et al. (2012). It is interesting to note that the impact on wages of *improving* NREGS is of the same magnitude or even slightly higher than that of the *introduction* of NREGS, which again highlights the importance of implementation, and the fact that all papers are basically identifying the impact of a shift on a scale of implementation.

In addition to the wage results, we find no statistically significant effects on private sector employment, again for the month of June. However, these are not incompatible with the results in Imbert and Papp (2015a), since their measure of private sector employment includes domestic work and self-employment. Since we ran our own survey and are able to separate the types of work, we run separate regressions and find no effect on working for others but a decline in self-employment and idle time. This is thus compatible with the explanation provided in their paper which suggests that “the fall in private sector work may in part represent a fall in disguised unemployment or private sector work with close to zero productivity.”¹²

So what should the discerning reader take away from these results about the impact of NREGS on the labor market? It seems very reasonable to conclude the program had a modest positive effect on wages for agricultural/ unskilled labor. However, questions remain about the precise seasonality of the effects. While it is most likely that wage effects happened during times that NREGS was most active, it is also possible - particularly given nominal wage rigidities (Kaur, 2015) - that these effects persist throughout the year. Moreover, it does appear as though overall private sector work - which includes domestic work and self-employment - declined. But this may not necessarily reflect declines in productive work done for landlords, so the effect of this “crowd-out” on productivity is still uncertain. Finally, the channels via which wages increased are yet to be positively identified; while Imbert and Papp (2015a) and Azam (2012) both seem to rule out an increase in labor productivity as a result

¹²Zimmermann (2015) makes explicit in her paper the choice between working in the private sector for someone else, working for yourself, and working on NREGS, and points out that the former two may be substitutes. In this model, if NREGS provides an insurance benefit, then one might choose to work for yourself rather than work in the stable private sector. While this is a helpful model in distinguishing between labor choices, the predicted results are the opposite of what we find in Andhra Pradesh.

of NREGS assets, there is no actual evidence on the this.

5.2 Income and consumption

How did the changes in labor markets affect income and consumption for poor households? Given the uncertainty over private sector employment effects, it is not clear that an increase in wages would unambiguously lead to increases in income and consumption. The additional channel of insurance, which may lead to different agricultural choices (as discussed below), might also lead to changes in income.

At the all India level, Klonner and Oldiges (2013) use NSS data and a combination of strategies to attempt to tease out the effect of NREGS on consumption and poverty. They use a fuzzy RD design that recognizes the fact that there was political interference in assignment of districts to phases, combined with a difference-in-differences approach. They find no overall effect of the program on consumption and poverty. While they claim to find a reduction in poverty amongst SC/STs, this result is not robust, and the specification with the most straightforward application of the fuzzy RD design does not show any significant effects on consumption and poverty.

More unambiguous and robustly identified results come from Andhra Pradesh. The Smart-cards experiment mentioned above led to a substantial improvement in the functioning of NREGS, including higher payments to households, and also led to higher private sector wages. We find that the net effect of this improvement is a significant and robust increase in household income, verified not only in our own survey data, but also the entirely independent Socio-Economic and Caste Census (SECC) that was conducted concurrently (Muralidharan, Niehaus and Sukhtankar, 2016). Our survey data consists of a representative sample of job-card holders and pensioners (close to 70% of rural households), and we find an increase of Rs. 8500 (13.4%) in annual income. The SECC by definition tracks every single rural household, and here we find that the highest income earner in the household was 24.7% more likely to move out of the lowest income category ($<$ Rs. 5000 monthly income) in treatment areas.

Interestingly, the vast majority of the increases in income are due to effects on the private sector rather than directly through NREGS. Increases in NREGS income accounted for only 1/9th of the income gains for NREGS beneficiaries, with the remaining coming from increases in the private sector earnings, suggesting that general equilibrium effects of NREGS are important. While we do not find statistically significant effects on consumption, our standard errors are large given the difficulty in precisely measuring consumption in a survey

that mainly focused on NREGS functioning. We also do not find evidence of distortions from NREGS on allocation of land or labor, although absence of evidence is not of course evidence of absence. A dispositive identification of precise channels is not possible given our data.

Our experimental evidence from Andhra Pradesh is corroborated by other evidence from the same state. Ravi and Engler (2015), use panel data and the fact that some households were denied employment in order to identify effects, comparing households who applied for work and received it to households that also applied for work but did not receive it. They find increases in consumption and food security as a result of work on NREGS. Although the sample is somewhat restrictive - it consists of a thousand ultra-poor households from two hundred villages in one district - the results are robust. Deininger and Liu (2013) also use panel data from the same state, with a slightly larger sample (4000 households in 480 villages and 5 districts) and a difference-in-differences approach. They find increased consumption, particularly nutrition, for program participants compared to non-participants. The results however must be viewed with some caution given the possibility for selection bias to affect findings.

The key takeaway from this section is that it is likely that in places that implemented the program well - like Andhra Pradesh - there were indeed increases in income. Moreover, since NREGS income comprises a small part of overall income, any significant increases in income must come from effects on private sector labor markets. Nonetheless, the precise channels for any increase remain unclear, as does the evidence for median implementing state.

5.3 Education and health

Readers who have not followed the literature will perhaps be surprised to note that the impact of NREGS on education has been more heavily studied than either the impact on labor outcomes or income. Perhaps this is an artifact of data availability, or perhaps it is the fact that the theoretical impact on education is ambiguous. The ambiguity results from the potential of NREGS to have both income and substitution effects. If NREGS increases incomes, then educational outcomes are likely to be positively affected. However, NREGS also increases the opportunity cost of time, particularly for older kids who may be able to work on NREGS themselves (despite adult-only rules), or be close substitutes for adult labor and hence do more work in households or farms. These children may hence drop out of school, reducing enrollment and eventually test scores.

Consistent with the ambiguous theoretical impact, the empirical literature finds a variety of seemingly conflicting results. However, a closer examination of the breakdown of results and data sources actually reveals a number of consistent stories, which I highlight below.

Perhaps the most comprehensive and clear paper uses data from Pratham's Annual Status of Education Report (ASER) as well as the NSS, combined with a difference-in-differences strategy, to examine effects on test scores, enrollment, and child labor (Shah and Steinberg, 2015). The key advantage of ASER is that it tests children both in and out of school, with a consistent methodology and a large (500,000), representative, repeated cross section. This paper finds that NREGS leads to lower math and reading test scores for all children, with the effects concentrated on older children (13-16). In addition, they find that enrollment drops by two percentage points for 13-16 year olds with each additional year of exposure to NREGS. These results are corroborated by data from the NSS, which suggests that children aged 13-17 are 3 percentage points more likely to work, with girls doing more domestic work and boys doing more productive work.

Consistent with the opportunity costs of time hypothesis, there are no or limited effects for younger children (5-12). Meanwhile, the youngest children (2-4) benefit as a result of the income effect, with increased test scores and higher enrollment at age 5. Shah and Steinberg (2015) are able to show using NSS data that trends in educational outcomes were most likely parallel, and also include phase specific linear time trends to assuage concerns regarding the violation of the parallel trends assumption.

Islam and Sivasankaran (2015) also use NSS data and a difference-in-differences strategy to unsurprisingly find similar results. They find that older children (15-17) spend 18% more time working outside the household as a result of NREGS, while younger children (6-9) spend 3% more time in school. They corroborate these results with panel data from 3 states collected by the NSS, although it is not clear how the identification strategy in the latter case is any different.

Using a different dataset - District Information System for Education (DISE), based on school level data - Li and Sekhri (2013) examine the impact of NREGS on school enrollment and pass rates. Like the previous papers, this one also finds declines in primary school enrollment, on the order of 1 percentage point. A slight difference is that the results here are driven mainly by primary schools (grades 1 through 5, or roughly ages 6-10) rather than upper primary school (ages 11-13), but this could be explained by differences in the types of schools that only offer upper primary schooling, and the paper does not have data on the older kids in secondary school that might be most affected by NREGS. Li and Sekhri (2013)

also find declines in educational attainment, with pass rates declining and repeat rates rising in private schools. The authors are able to include school and year fixed effects, and can also show that differences in trends were not apparent in outcomes prior to the rollout of NREGS.

A paper that also uses the ASER data but a regression discontinuity based empirical strategy seems to find contradictory results to the papers above (Thomas, 2015). This paper suggests that educational outcomes, including enrollment and test scores, actually improved as a result of NREGS. However, the same paper shows strongly significant pre-program differences in educational outcomes; and while it claims to show parallel trends in educational outcomes pre-program, this is simply not possible as the ASER data only starts in 2005-06, providing only one year of pre-program data. These results must thus be discounted. The same paper, however, uses the District Level Household and Facility Survey (DLHS-3 and DLHS-2) for 2007-08 and 2002-04, and seems to find improvements in child and maternal health outcomes. These results would be consistent with the improvements in schooling outcomes for younger children that the other papers find.

Another two studies which seemingly contradict the negative education results above are in fact both based on data from only one state - Andhra Pradesh. The first uses data from the Young Lives Panel Study, which covers 6 districts in Andhra Pradesh with 757 children residing in rural areas (Mani et al., 2014). Given this small sample of districts, 4 of which were in early phases of NREGS and 2 in later phases, the effective variation in the sample is limited. The second paper uses mandal-level rainfall to instrument for female participation in NREGS, and finds that this led to increased girls time spent in school, grade progression, and female bargaining power (Afridi, Mukhopadhyay and Sahoo, 2012).

While the specifics may vary, and reading only the titles and abstracts of the papers may give one a misleading impression, closer examination of all the other papers on the impact of NREGS on education reveals that the set of results in Shah and Steinberg (2015) are mostly corroborated. Educational enrollment and attainment worsened for older children, while these outcomes and possibly health outcomes too improved for the youngest children.

5.4 Migration

Compared to education, papers examining the impact of NREGS on migration are few and far between. Preventing short term, distress migration was one of the goals of NREGS as a safety net Khera (2011). Moreover, the impact of NREGS on migration is ambiguous -

while distress migration might decrease, longer term rural-urban migration might increase as a result of increased income, which may help mitigate risky migration (Bryan, Chowdhury and Mobarak, 2014). But the lack of data on migration is a binding constraint.

Imbert and Papp (2015*b*) is the notable exception. They use original survey data from three states as well as NSS data to show that short-term migration is lower as a result of NREGS. In their survey, eight percent of adults report not having migrated as a result of NREGS. In addition, places that provide more government work exhibit less short-term migration. Finally, data from the NSS in 2007-08 shows a lower incidence of short-term migration in early phase districts as compared to late phase districts.

Our results from Andhra Pradesh are not exactly consistent with the above results (Muralidharan, Niehaus and Sukhtankar, 2016). We find no significant results on migration, with the coefficient being small and positive on both the extensive and intensive margins. However, these results are based on slightly different questions related to migration, which ask how many days respondents spent working outside their village. Moreover, the results relate to only one state; thus, they do not necessarily contradict Imbert and Papp (2015*b*).

Overall, while it is likely that NREGS reduced short-term distress migration, more research on this topic would be welcome.

5.5 Agricultural choices

Given the impacts on labor markets, and the potential insurance provided by the scheme, potential impacts on agricultural choices may be expected. For example, if wages increase, landlords may switch to more capital-intensive technologies. If households are confident about the insurance value of NREGS, they may invest in higher yield, higher variance crops.

Bhargava (2014) examines the first possibility, focusing on technology adoption decisions by landlords. The paper uses data from the Indian Agricultural Census Input Survey (ACIS), collected in 2007, and the regression discontinuity approach, comparing districts just around the Phase 1 and Phase 2 cutoff. It finds that NREGS reduces the proportion of farms using labor-intensive technologies, while increasing the proportion of farms using animal-intensive technologies, both by approximately 20 percentage points. These results are consistent with farmers moving to labor-saving methods in response to higher wages.

Gehrke (2015) examines the second possibility, focusing on crop choices by farmers. The paper uses panel data from the Young Lives Panel Study in six districts in Andhra Pradesh, four of which were part of Phase 1 and the remaining two part of Phases 2 and III. The

results suggest that households in the phase 1 districts were more likely to increase the risk profile of their crop mix. However, as in the Mani et al. (2014) paper that uses the same data above, the variation in treatment is limited, and the results must be viewed with caution.

This is another topic on which further research would be welcome, since while the labor-saving technologies result appears reasonable, there is not much else that can be said on this very important issue.

5.6 Conflict

Another area which has received substantial attention - to the extent that an entire panel for a broad development conference in the United States was titled “Impact of NREGS on Conflict” - is civil violence. Perhaps this topic particularly interests researchers since an explicit goal of MNREGA was to assist areas afflicted by the Maoist insurgency, or perhaps it is interesting since theoretically effects could go different direction. For example, the competition over resources theory of conflict would suggest that the greater financial resources that NREGS brings in would lead to increased conflict, while the opportunity cost of time theory of conflict would predict reduced violence given increased higher wages result in an increased opportunity cost of participating in violence.

Dasgupta, Gawande and Kapur (2015) use an originally constructed panel dataset on Maoist conflict violence combined with a difference-in-differences approach to examine the impact of NREGS on both violent incidents and deaths. The dataset is based on local language press sources, allowing for a more comprehensive cataloguing of violence, from 144 districts in six states in the red belt. The paper finds that NREGS leads to reduced violence, with a large (50%) drop in violent incidents, that is larger in districts experiencing negative rainfall shocks. Importantly, the results are entirely driven by Andhra Pradesh and Chattisgarh, two high capacity states, to the extent that the authors state that there is no effect of NREGS in the non-star states.

Given that NREGS was targeted to Maoist-affected districts, this paper faces an increased burden of convincing readers that the parallel trends assumption holds. It attempts to do so by including as controls a “backwardness index” which strongly predicts NREGS assignment to phases, as well as an indicator for “left-wing extremist” district interacted by year. With both district and year fixed effects in addition to the latter control, however, it is not clear how much variation remains to identify the effects.

Fetzer (2014) focuses not on the overall impact of NREGS rollout on violence, but rather

on the impact of NREGS on the link between rainfall and violence. The paper points out that rainfall shocks increase civil violence, and also affect agricultural output in wages. The author also collects his own data on violence, using a semi-automatic technique based on Natural Language Processing Tools. The paper uses an interaction of rainfall variation with NREGS rollout, and shows basically that NREGS buffers violence related to rainfall, likely by providing insurance. While the results seem plausible, how the precise timing of rainfall shocks interact with violence and NREGS rollout is a bit unclear.

Khanna and Zimmerman (2014) use a regression discontinuity design, with both cutoffs (between Phase 1 and Phase 2 as well as Phase 2 and Phase 3), and data from the South Asian Terrorism Portal (SATP), to examine impacts on civil violence. They find that violence increases in the very short run, with increases in police-insurgent violence as well as attacks on civilians by insurgents. However, the results are not apparent from the discontinuity graphs. Moreover, the paper again must deal with the issue of violations of RD specifically related to Maoist insurgency, which it attempts to do by dropping districts.

Finally, Amaral, Bandyopadhyay and Sensarma (2015) examine the impact of NREGS on domestic, rather than civil violence. They use data from the National Crime Record Bureau focusing on violence against women, combined with a difference-in-differences approach that compares Phase 1 to Phase 3 districts. The idea is that increased female labor force participation - induced by NREGS - could lead to women's empowerment, but also increase domestic conflict. They find that NREGS actually increases reports of domestic violence, but reduces cases of dowry deaths. The paper uses controls to attempt to control for trends, but does not show the basic parallel trends graph that would be helpful to gauge the assumption. Moreover, given massive underreporting of incidents (Iyer et al., 2011), it is difficult to know whether the impacts are on actual violence or merely reports.

Given the differing data sources, results, and issues with identification, it is difficult to conclude much about the impact of NREGS on civil insurgency or domestic violence.

6 Outstanding questions

As the previous section makes clear, while we have learned from studies of NREGS impact, there are still large holes in our knowledge that need to be filled. In addition to the specific issues highlighted in each subsection above, the following three broad areas represent important open questions that will help our understanding of NREGS:

1. *Labor efficiency impact.* While we know that NREGS raised rural private sector wages,

and at the very least crowded out self-employment labor, we don't know the net impact on efficiency. This is very important to settle the debate over whether NREGS is drawing away labor from productive uses in order to simply dig holes and fill them up, whether it is simply providing employment to under-employed labor, or possibly even breaking up non-competitive rural labor markets and enhancing efficiency.

2. *Public goods creation.* There are a large number of papers that study the assets created by NREGS. However, a large scale, representative study that actually measures return on investment is lacking. Moreover, whether these assets actually raise labor productivity is a question that no reasonable study has been able to answer. This is disappointing, because we are currently stuck with the extreme (and likely extremely inaccurate) views of supporters and detractors.
3. *Longer term effects.* Finally, many of the impact studies use the rollout of NREGS for identification. As pointed out earlier, given implementation problems at the outset these studies only capture a small part of the impact of steady state NREGS. Steady state, long run impacts on major outcomes such as rural labor supply, rural labor demand, migration, and technology adoption are nowhere to be found. Perhaps we will never be able to identify these impacts, given that NREGS is universal and the gap between phases is too small, so that long run impacts are not detectable. But at the very least an attempt at capturing these impacts would be welcome.

7 Conclusion

I conclude not with a summary of findings, but rather lessons for the future from the experience of doing research on NREGS.

Perhaps the abiding lesson is to build in evaluation prior to the rollout of any large programs. Early evaluation might help in answering the big questions, and quell pointless debate. This is not to criticize the founders of MNREGA: I appreciate that doing public policy and getting acts passed through Parliament is difficult, and what precious political momentum is available has to be seized. Moreover, the pressure to implement is enormous; everyone wants to do, not necessarily do right. Nonetheless, since knowledge and learning are public goods and no individual bureaucrat has the incentive to provide them, future programs may do well to build in incentives to learn. The research environment of today, in which large and rigorous experimentation is not unheard of, makes such learning possible.

Another lesson is to think harder about how mandates to capture data will actually be implemented, and also plan for easy data dissemination from the outset. For example, Witsoe (2014) notes that “The actors who create and maintain NREGASoft in Delhi have almost no control over the documentary practices of the officials who produce the paper documentation that is entered into the software platform.” Figuring out incentives for these local actors to provide accurate data is of course far easier said than done, but must be attempted.

Finally, the fact that heterogeneity is enormous has been repeated ad nauseam. The implications for policy, however, are that there is a balance needed between central schemes and rules and allowing states to experiment to find local solutions. Down this path lies the likeliest probability of success for large public programs such as the NREGS.

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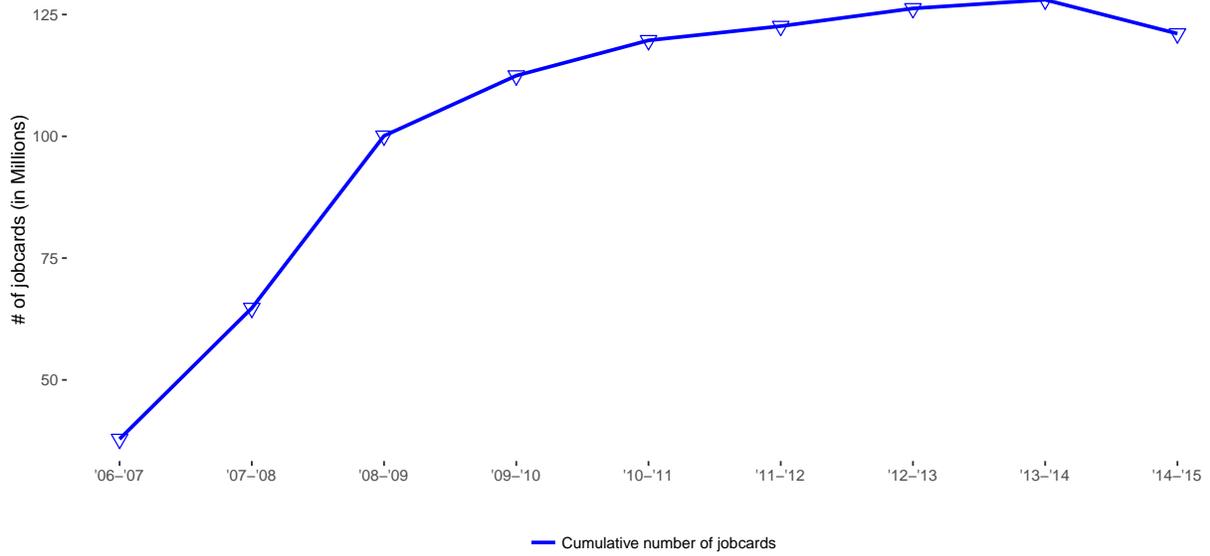
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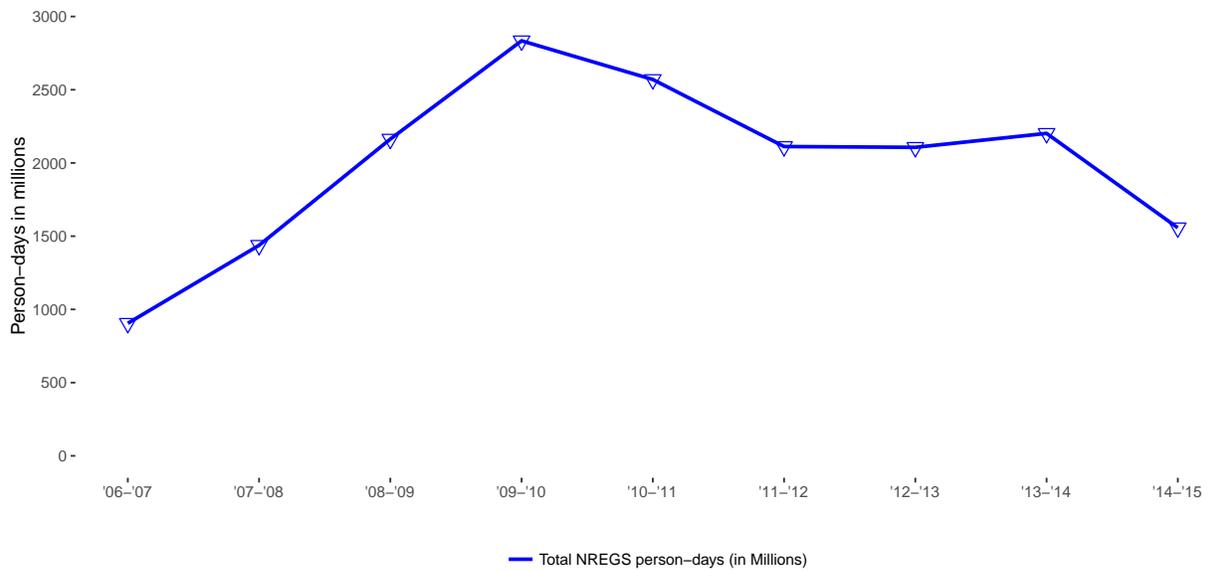
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Figure 1: NREGS Jobcards



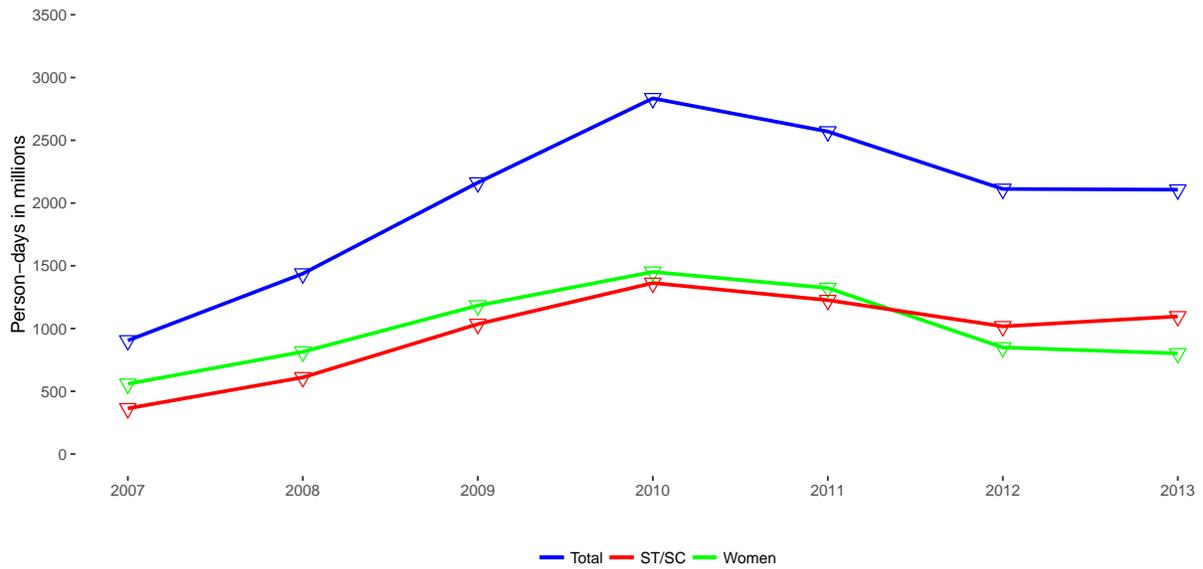
Source: Ministry of Rural Development.

Figure 2: Person-days Worked on NREGS



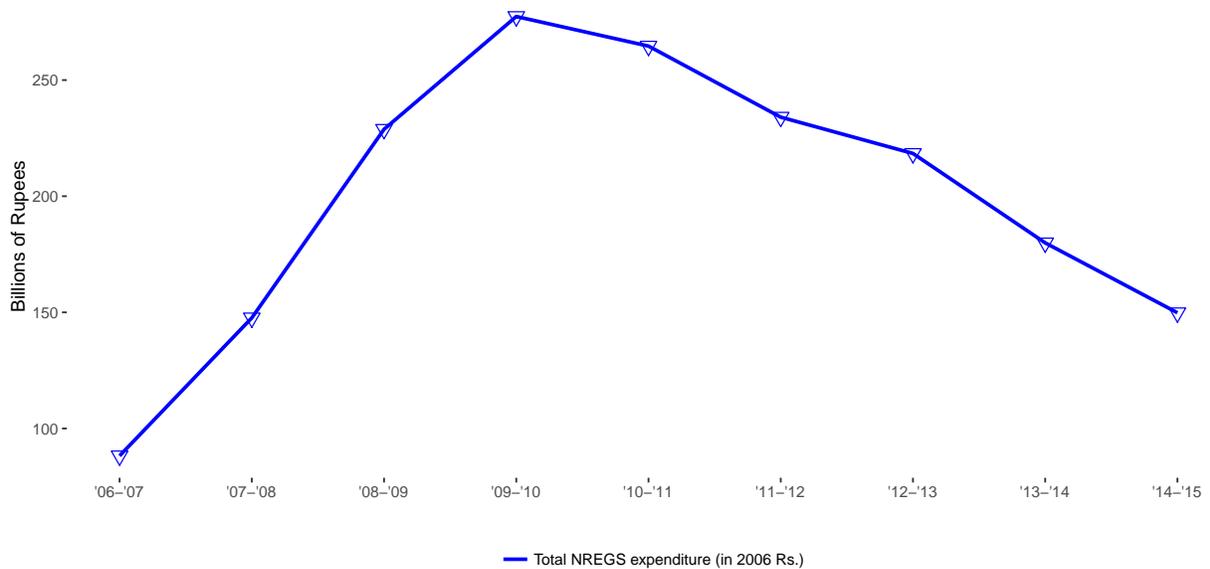
Source: Ministry of Rural Development.

Figure 3: Persondays Women and SC/ST



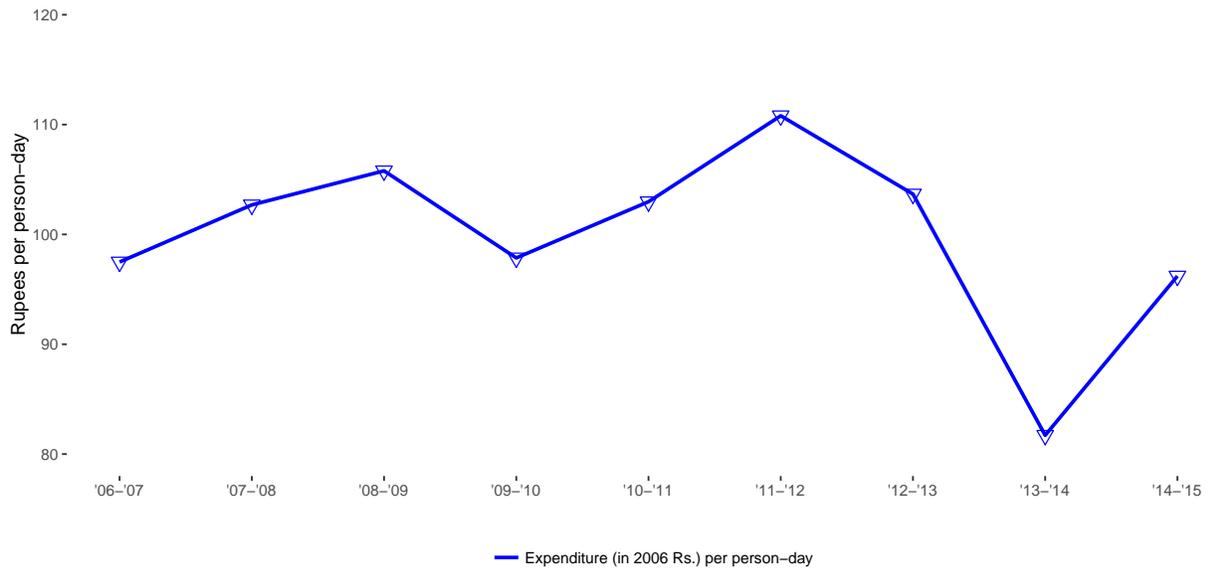
Source: Ministry of Rural Development.

Figure 4: Total Expenditure on NREG



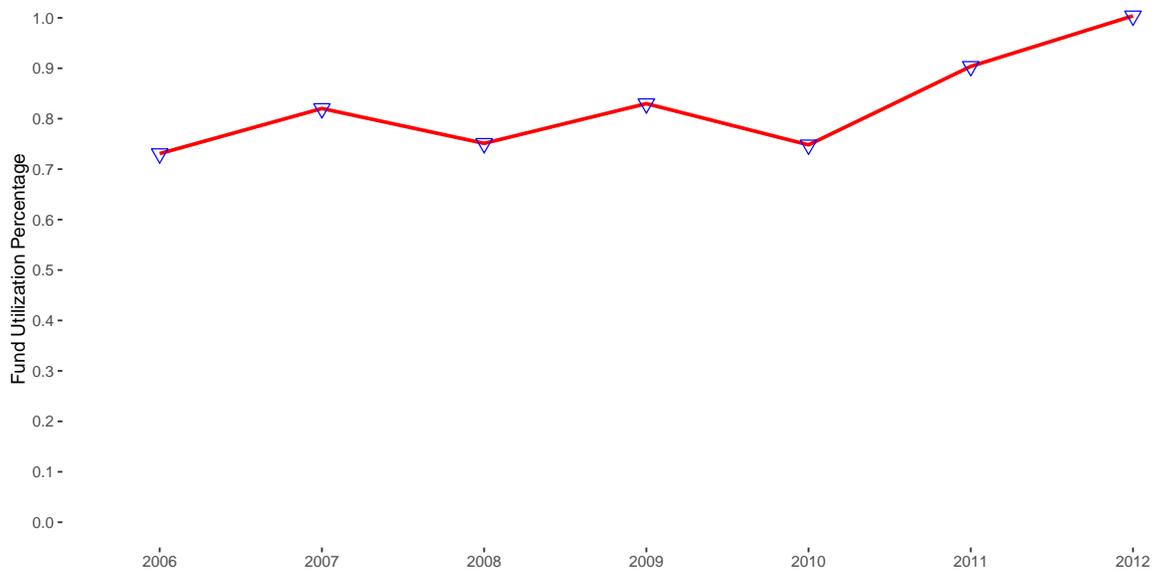
Source: Ministry of Rural Development.

Figure 5: Spending per person-day



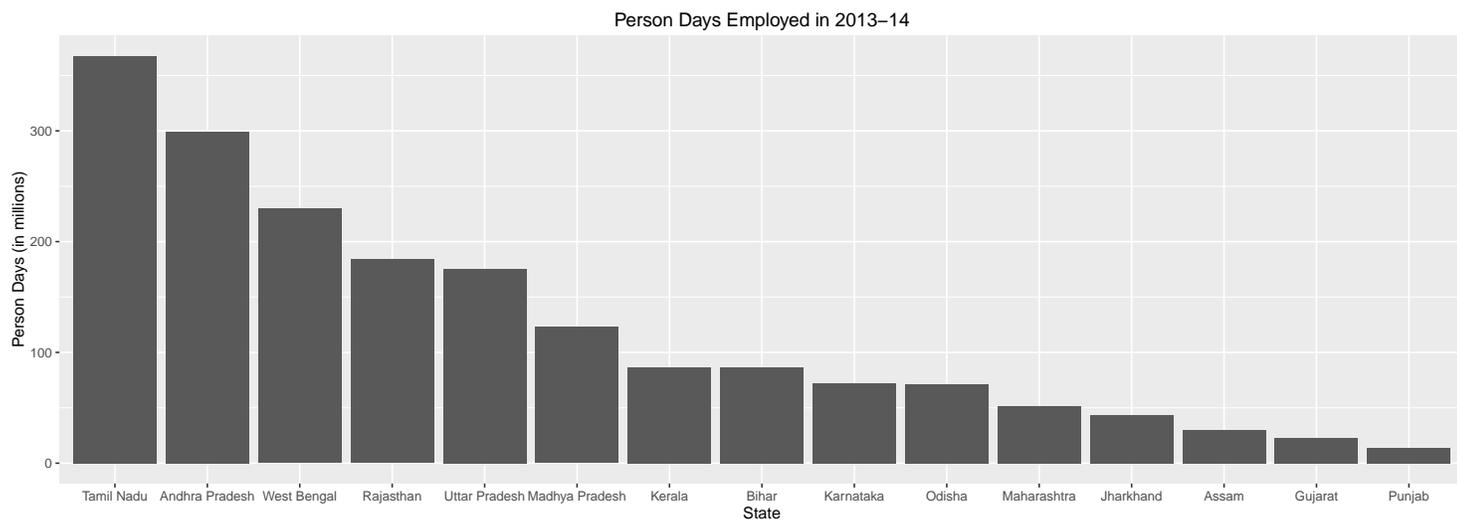
Source: Ministry of Rural Development.

Figure 6: Proportion of Funds Utilized



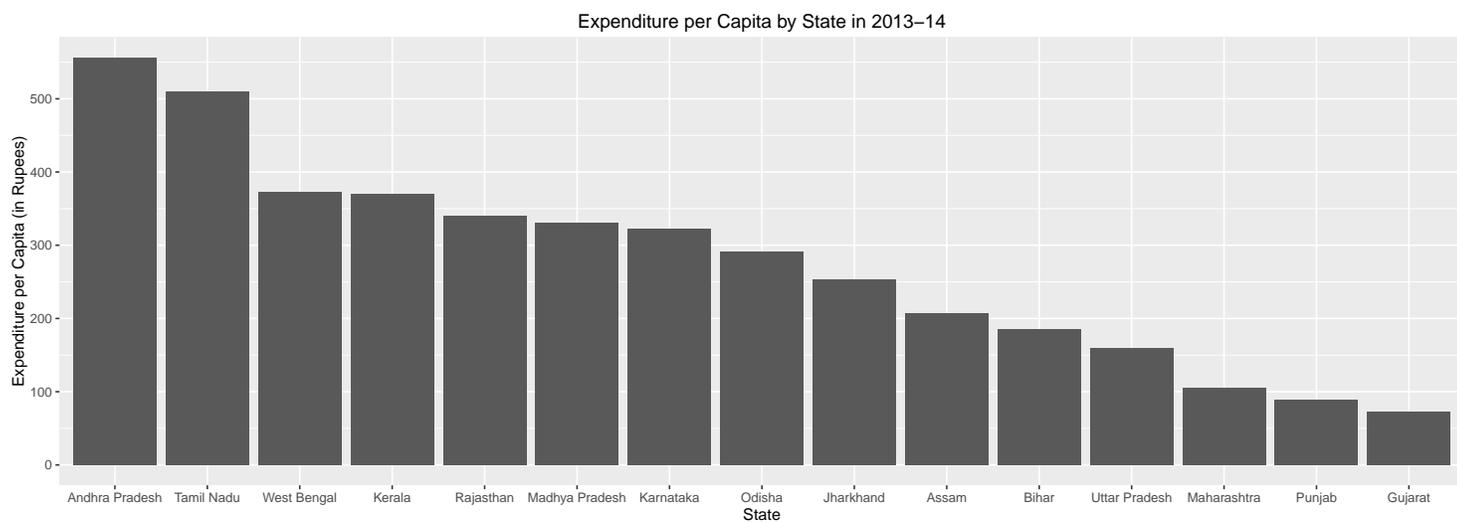
Source: Ministry of Rural Development.

Figure 7: Person-days by state



Source: Ministry of Rural Development.

Figure 8: Expenditure per capita by State



Source: Ministry of Rural Development.