

# India's National Rural Employment Guarantee Scheme: What Do We Really Know about the World's Largest Workfare Program?†

**ABSTRACT** In the ten years since the rollout of India's National Rural Employment Guarantee Scheme (NREGS), 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 for interpreting what "the scheme" 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 has led to worse educational outcomes for older children, with net positive impacts on income and welfare that are debated. Fourth, key questions pertaining to overall impacts on rural productivity remain unanswered. Although serious research on these questions is badly needed, current standards for causal inference and the availability of data will remain high hurdles for those who wish to take on this challenge.

**Keywords:** *NREGS, MGNREGA, NREGA, Workfare, India, Rural Employment Guarantee Scheme, Rural Wages*

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

## 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

\* [sandip.sukhtankar@virginia.edu](mailto:sandip.sukhtankar@virginia.edu)

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Guarantee Act (MGNREGA).<sup>1</sup> MGNREGA entitles every adult member of a rural household the legal right to 100 days of minimum-wage employment per year, to be provided within 15 days of asking for it. The state-level public employment schemes required by MGNREGA—collectively referred to as the 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 NREGS. Supporters have hailed the program as a “tremendous success”, while opponents have derided it as an “expensive gravy train”.<sup>2</sup> 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 social scientists.

This paper synthesizes knowledge based on serious theoretical and empirical research on NREGS, 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.

1. MGNREGA 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 (MGNREGA)—is a piece of legislation that created an entitlement to rural employment, which was to be fulfilled by state-level schemes, collectively referred to as National Rural Employment Guarantee Schemes (NREGS). But so that no one, including myself, is confused about which program is being referred to in this paper, I refer to these acronyms interchangeably. I will attempt to use MGNREGA when referring to the Act and NREGS when referring to the actual implementation of the schemes.

2. These terms were noted in the following 2008 Op-ed in the *Hindu* newspaper by Jean Dreze, <http://www.thehindu.com/todays-paper/tp-opinion/Employment-guarantee-beyond-propaganda/article15141615.ece>.

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

1. 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 the better implementing states; even impacts, on average, are driven largely by the “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 the program combined with varying implementation quality.
2. 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 the season in which NREGS operates, then the harm from rationing may be limited. On the other hand, the poorest states, where the need for the program is the greatest, seem to lack the capacity to implement NREGS.
3. Third, there are at least two outcomes that the bulk of the evidence suggests that NREGS 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, as the opportunity cost of attending school has now gone up. A number of pieces of evidence indicate that incomes may have increased, but this evidence is not as strong and universal as the previous two outcomes.
4. Fourth, despite the passage of 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 impacts of the program.

Why have researchers not been able to provide definitive answers to the big questions? It is a non-trivial task to identify the effects of a large

program, particularly given the non-random rollout and current high standards for causal inference. Complications arise in view of 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 similar quality papers in just ten years of the NREGS—problems still remain. For example, even obtaining consistent data over time on the 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 the standard for the 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 probably know even less about the government's 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 impact of NREGS, 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 MGNREGA, including the main provisions and guidelines laid down by the Act. This 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 by highlighting the incredible 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 implementation such as worksite facilities and choice of projects.

Section 4 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 attempt not only to 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 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.<sup>3</sup>

Despite the large amount of research on MGNREGA, there are still areas that are under-studied. 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

MGNREGA was passed by Parliament and notified in 2005, following up on an electoral promise made by the United Progressive Alliance (UPA, a coalition led by the Congress party) 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.<sup>4</sup> An additional 130 districts received the program in April and May 2007, while the program became operational in the remaining districts of the country in April 2008. Below I describe the main provisions of the Act, and the following sub-section provides some basic numbers and figures.

### 2.1. MGNREGA Provisions and Rules

While most readers familiar with MGNREGA know that the Act establishes a legal guarantee of a 100 days of employment, there are many other provisions

3. Papers 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.

4. In Section 4, I describe in greater detail how these districts were chosen.

of the Act as well as official guidelines that are less well-known.<sup>5</sup> 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 the implementing agencies. This comprehensive document does not preclude the production of other ad-hoc documents with procedures.<sup>6</sup>

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 in Section 3. The current section is, however, devoted to documenting the provisions of the Act on paper, which I categorize 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 job cards from local government offices, which list all adult members of the household and have space for recording work done and payments received. Once the job card is obtained, households are supposed to apply for work whenever they need it. The local administration must provide employment within 15 days and within 5 kilometers of the applicant's home, failing which the applicant is to be paid unemployment allowance (for non-provision of work within 15 days) or a travel and subsistence allowance (if work is available only 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 percent of the expenditure incurred on the program must be spent on wages. In addition, 33 percent of the jobs must be reserved for women, and women are guaranteed the same wages as men (which is not de facto true in the private sector, thus making NREGS more valuable for women). Finally, wages are to be paid weekly, and no later than a fortnight of the work being done.

5. For example, a little known feature that may be of interest to researchers is the fact that the Government invites proposals for studies on MGNREGA, and will pay for them! See [http://nrega.nic.in/Circular\\_Archive/archive/Guidelines\\_Research\\_Studies\\_under\\_MGNREGA.pdf](http://nrega.nic.in/Circular_Archive/archive/Guidelines_Research_Studies_under_MGNREGA.pdf).

6. For example, "Guidelines for Construction of Play Ground in IAP Districts". See [http://nrega.nic.in/GUIDELINES\\_CONST\\_IAP.pdf](http://nrega.nic.in/GUIDELINES_CONST_IAP.pdf) for more details.

### 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 MGNREGA is related to water management, with both drought-proofing and flood management being 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 is supposed to 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 people belonging to Scheduled Castes/Scheduled Tribes (SC/ST). 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 supposed to be provisions for worksite facilities such as drinking water, shade, and a crèche for workers' children.

### 2.1.3. *FINANCE AND PAYMENTS*

In order to incentivize States to generate employment, the Central Government pays all labor costs fully, but only 75 percent of the 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 the Central and State governments since the inception of the Act. Workers may be paid either 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 the workers' bank or post office accounts, and also for separation between the 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 percent of the works in terms of cost, MGNREGA 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 assisted by a wider array of local officials,

some of whose positions were specifically created by the Ministry of Rural Development to assist with MGNREGA. These include the *Gram Rozgar Sahayak* or Employment Guarantee Assistant, Mates or worksite supervisors, Field Assistants, Technical Assistants to measure and monitor the work, Computer Assistants to maintain, update, and transmit records,<sup>7</sup> in addition to the elected officials (for example, *Sarpanch*) and appointed officials (for example, GP Secretary), who are in overall charge of the program.

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 the irrigation department). These entities are meant to have Program Officers in charge of MGNREGA implementation. These officials approve work plans, and in some cases also budgets, although this provision may be changing.

### 2.1.5. MONITORING

MGNREGA is unusual among large welfare programs 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.

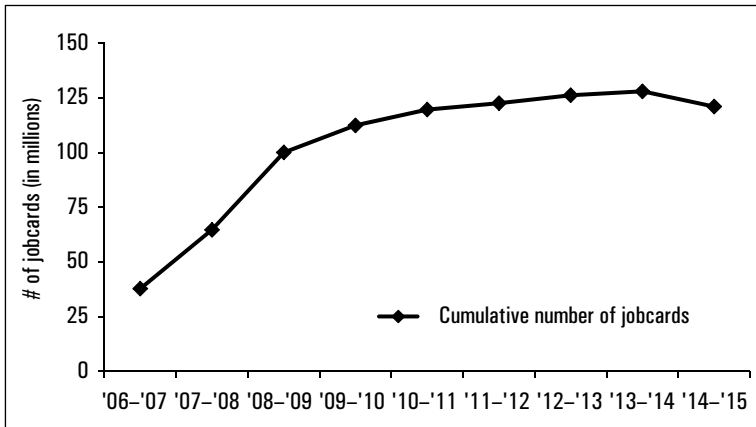
## 2.2. Figures and Numbers

NREGS is a large program by any account. Over 121 million rural job cards had been 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 job card, although that is likely not the case.<sup>8</sup> The total number of

7. Every GP does not necessarily have each of these officials; for example, the Technical Assistant and Computer Assistant are generally based at the block level.

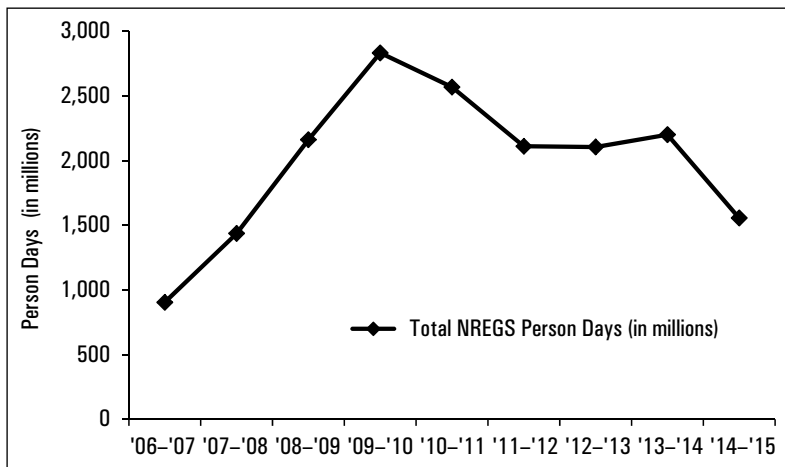
8. Calculations by Muralidharan et al. (2016a) suggest 1.9 job cards per NSS-defined household in Andhra Pradesh in 2011–12.



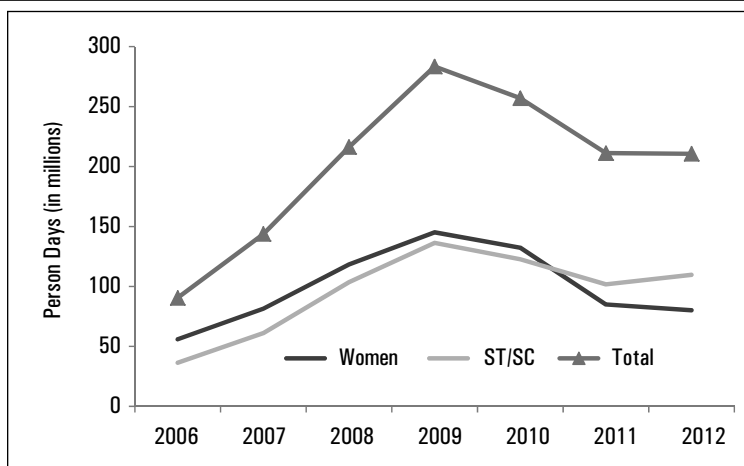
**FIGURE 1. NREGS Jobcards**

Source: Ministry of Rural Development, Government of India.

person days on the program in 2013–14 was 2.20 billion, which is actually a drop from the peak of 2.83 billion in 2009–10 (Figure 2). Nearly 48 million individuals actually worked on the program in 2013–14, belonging to 24.4 percent of rural households (Desai et al. 2015).

**FIGURE 2. Person Days Worked on NREGS**

Source: Ministry of Rural Development, Government of India.

**FIGURE 3. Person Days: Women and SC/ST**

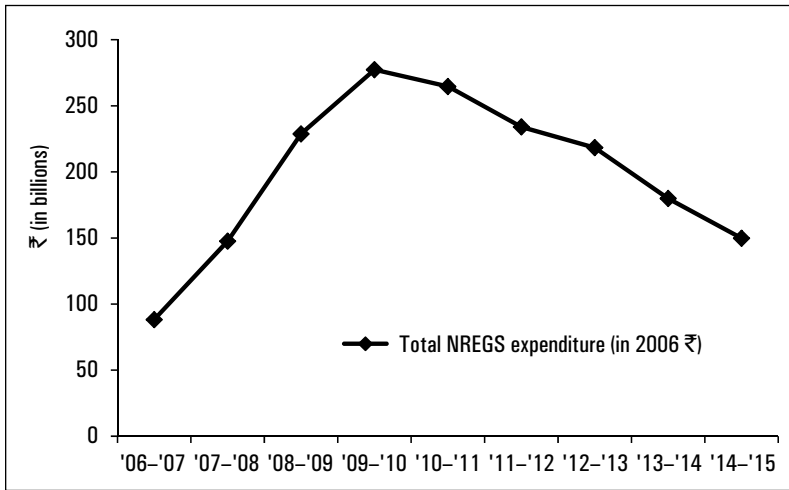
Source: Ministry of Rural Development, Government of India.

A striking feature of the program is participation by women. Since the program pays the same wages for women as men, while women are paid less than men in the private sector, women's participation is high, amounting to an average of 47.5 percent over the years 2006–2012. Participation by marginalized sections of society is also high, with SC/ST accounting for 49.4 percent of person days paid for (Figure 3).

Expenditure on the program amounted to about ₹37,000 crores in 2013–14.<sup>9</sup> This makes it amongst the biggest public programs in India, although not the single largest public expense. In comparison, subsidies for kerosene alone amounted to ₹30,574 crores in 2013–14. Fiscal expenditure on fertilizer subsidies, including consumer and producer subsidies, amounted to ₹73,790 crores in the same year (Ministry of Finance 2014).

Comparisons with programs across the world can be made by calculating expenditure as a percentage of GDP. NREGS expenditure in 2013–14 was 0.33 percent of GDP in India. In comparison, US spending on Medicaid (assistance with medical costs for people with limited resources) was

9. Despite the huge efforts made in ensuring 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.

**FIGURE 4. Total Expenditure on NREGS**

Source: Ministry of Rural Development, Government of India.

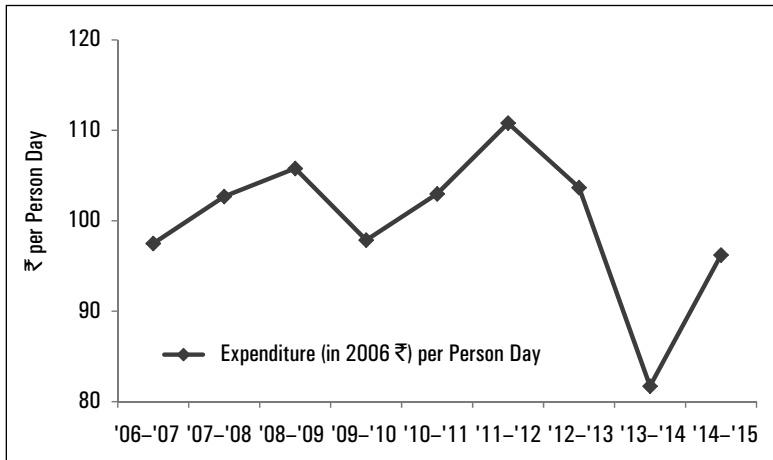
2.9 percent of GDP, while spending on the Temporary Assistance for Needy Families TANF program (providing temporary financial assistance for pregnant women and families with one or more dependent children) was 0.19 percent of GDP.<sup>10</sup>

The trend in program statistics so far suggests that expenditure and participation on NREGS rose steadily as the program 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 ₹14,982 crore in 2014–15 from ₹27,736 crore, its peak in 2009–10. The number of person days has also dropped, as has the expenditure per person day, although how much of this is due to actual declines as opposed to reduction in corruption (see Section 3) is not known (Figure 5). Meanwhile, the utilization of available funds has increased steadily, and recently state and local governments have been using almost all their budgeted funds (Figure 6).

The almost 50 percent drop in real expenditures on NREGS between 2009–10 and 2014–05 deserves some explanation. A number of factors

10. Sources: Medicaid spending and GDP from [www.cms.gov](http://www.cms.gov), and TANF spending from [www.acf.hhs.gov](http://www.acf.hhs.gov).

**FIGURE 5. Spending per Person Day**



Source: Ministry of Rural Development, Government of India.

**FIGURE 6. Proportion of Funds Utilized**



Source: Ministry of Rural Development, Government of India.

contributed to this drop. First and foremost, both the monsoon and annual rainfall in 2008 and 2009 were significantly lower than average, and also significantly lower than in 2013 and 2014, suggesting that greater demand for work had a possible role to play in the earlier period. Second, as discussed below, it is likely that corruption has gone down over time, thereby reducing outlays.

### 3. Implementing NREGS

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

#### 3.1. Heterogeneity in Implementation

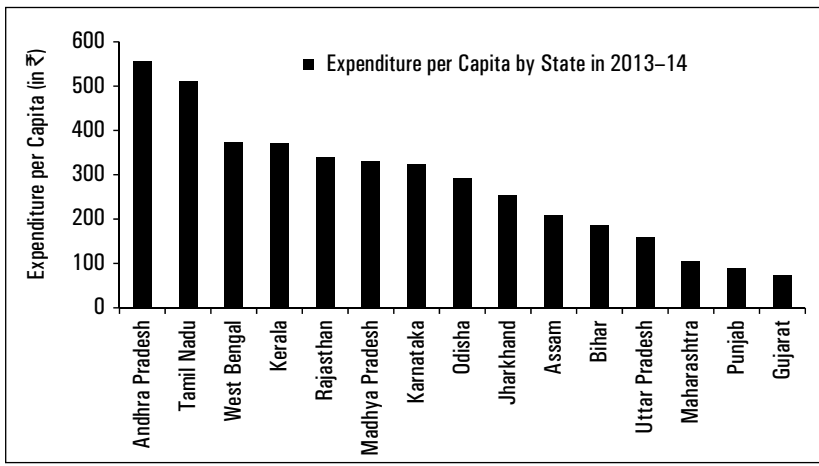
The most remarkable characteristic of NREGS is the enormous heterogeneity in implementation quality across states, and even within them. Heterogeneity of implementation across states has been a feature since the very beginning of the program. Dreze and Oldiges (2009), in commenting on performance of the program during 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 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

**FIGURE 7. Person Days by State**



Source: Ministry of Rural Development, Government of India.

**FIGURE 8. Expenditure per Capita by State**

Source: Ministry of Rural Development, Government of India.

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). Overall, there is no relationship between spending per capita or NREGS participation and the rural poverty headcount across states (Dutta et al. 2012).

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 et al. 1993), supply constraints are also extremely important. These supply constraints are related to fiscal capacity, implementation capacity (particularly the way the program is implemented at the local level), as well as political factors.

While the Central Government pays for most expenses under the program, states are still responsible for a share of the costs as explained below, including some of the administrative costs. If state governments have limited fiscal capacity to pay their part, corresponding transfers from the Central Government would be affected, which perhaps explains the apparent regressivity across states noted above.

Administrative capacity in poorer states may also be limited. 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 the functioning of NREGS (Muralidharan et al. 2016a).

Finally, the motivation of bureaucrats and politicians to implement NREGS also matters. Gulzar and Pasquale (2015) show that political incentives affect NREGS implementation using a clever empirical strategy to identify the effects of 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 politicians can then more unambiguously claim credit for the improvements. This paper makes clear how implementation quality may differ even within districts.

The heterogeneity observed in NREGS implementation is important not only for the effectiveness of NREGS, but also for two other, less obvious, reasons. First, the interpretation of results and observations that rely on small and selected samples should be done with caution. This is the normal caveat for the extrapolation of results from one area to another. Second, one-size-fits-all solutions must also be viewed with caution.

### *3.2. Implementing Key Features of MGNREGA*

The deviation of the abstract ideal of MGNREGA from the reality of NREGS is best captured by the following 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 in the key dimensions of the Act between the Act on paper and the schemes as practiced in villages, including access to work and payments.

### *3.2.1. ACCESS*

Of all the features of MGNREGA, the most important is simply the access to work on demand. How has NREGS fared in fulfilling this key provision? The most comprehensive work on this suggests that there is still a long way to go to fulfil the ambition of employment when desired. Dutta et al. (2012) use data from the 2009–10 National Sample Survey (NSS), which is representative across India, to find that 44 percent of those who desired work from NREGS did not get it. As suggested by the previous section, there is wide variation across states, with rates varying from 15 percent to 84 percent. Similar results are found in the data from NCAER’s India Human Development Survey (IHDS), 2011–12, with 60 percent of the participating households desiring more work but not able to obtain it, and 29 percent of all rural households experiencing rationing of some kind (Desai et al. 2015). Moreover, the IHDS data suggest that 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 (2015a). However, even here in our end-line survey in 2012, we found that only 4 percent of respondents answered ‘yes’ to the question, “In general, can anyone in this village who wants work on NREGS get it?” Access looks better when respondents were asked about specific personal experiences, with only 20 percent of respondents answering that they were unable to get work despite asking for it in May (the slack season), while 42 percent of respondents answered that they were unable to get work despite asking for it in January.<sup>11</sup>

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



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 for it, seasonality is not simply due to variation in demand. It is accepted that no one really “applies” for work; projects generate labor demand 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 of jobs, rather than the demand, constrains NREGS employment.

In general, state and local governments seem to plan NREGS projects for the slack labor market months of May and June prior to the monsoons, while such work generally is not planned during the peak harvest months of December and January. There is some suggestion that part of the seasonality may be 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 labor is threatened by the availability of employment opportunities outside their authority.” Similar broad arguments have been made by Anderson et al. (2015), who contend that landlords seek to control local governance in order to dampen wage pressure from schemes like the NREGS. However, more conclusive evidence of widespread collusion between local governments and landlords to smother NREGS implementation is lacking, since such evidence would clearly be difficult to obtain directly.

### *3.2.2. TARGETING*

How well does the self-selection aspect of MGNREGA work to target the poor? The poor are more likely to work on NREGS than the non-poor, with 30 percent of poor households participating as compared to 21 percent of non-poor (Desai et al. 2015). Households in which no adult is literate are also much more likely (30 percent) to participate than households where at least one adult is a graduate (13 percent). Again, implementation matters, as 60 percent of poor households participate in NREGS in the better implementing states, as suggested by data from IHDS 2011–12. However, there is also the fact that 70 percent 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, since the program then draws in relatively richer workers. Dutta et al. (2014) summarize the situation best: “the rationing process is pro-poor and the scheme is reaching poor families, though richer households also share in the gains.”

Qualitative work also supports the ‘glass is half-full/half-empty’ nature of targeting. For example, Marcesse (2016) points out that in Uttar Pradesh, although the workers actually doing work were often from the poorest section of society, many other workers (from more fortunate castes) also received payments for not doing any work. Moreover, he notes that “variation in the extent to which work benefits were allocated, and whether the work was performed reflected patterns of political allegiance at the village level.”

### 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, and 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 under way (Banerjee et al. 2014), 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 are difficult to obtain, with the NSS not asking specific enough questions about the lag between work done and receipt of payment. 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 et al. 2016a). 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 given to this 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 for every payment. Moving to electronic funds transfer, biometric authentication using Smartcards, and CSPs at the village level considerably reduced both this collection time as well as delays in payments (Muralidharan et al. 2016a).

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 NSS data from 2009–10 and 2011–12, which asks respondents about the number of days they were employed on NREGS, and compares these numbers to official figures on employment provided by MoRD. They find that the NSS household data can only account for 42 to 56 percent of the official MoRD reports in 2009–10. The number improves to about 80 percent in 2011–12. However, these numbers are not a precise estimate of the 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, biasing the comparison in terms of days.

If we took these figures seriously, we could calculate fiscal losses due to leakages. This would amount to approximately ₹19,000 crores in 2009–10, and ₹7,500 crores in 2011–12. For comparison, leakage rates for the PDS ranged from 44 to 58 percent in the years 2004–05 and 2007–08. The fiscal loss to the government of these leakages in the PDS in 2013–14 amounts to ₹28,500 crores (Ministry of Finance 2014), as compared to the mean value of about ₹36,000 crores and a median value of ₹12,000 crores of the 28 largest Indian scams between 2000 and 2014 that received the greatest media attention as chosen by 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 the 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, since the number of households in existence does not correspond exactly to the number of job cards in existence. Our level estimates using this methodology in Andhra Pradesh suggest leakage rates

of 30.7 percent in 2012 (Muralidharan et al. 2016a). Previous estimates in Odisha using a similar methodology estimated rates of 70–80 percent, 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 2013a; 2013b).

In addition to levels of corruption, there is also some limited evidence on the determinants of corruption. The broad takeaway from Niehaus and Sukhtankar (2013a) 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 (2013b), we find that workers' ability to use their voice to tackle corrupt bureaucrats may be limited, unless possibly helped by NGOs.

Finally, the Smartcards experience in Andhra Pradesh suggests that strengthening state capacity to implement the program may reduce corruption in a highly cost-effective manner. Building a new payments infrastructure that relies on electronic transfers to NREGS workers and biometric authentication at the time of payment collection reduced leakage by 40 percent (Muralidharan et al. 2016a). 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). In this case, a reduction in outlays rather than an increase in receipts was the major source of reduced leakage.

## 4. Conceptual Frameworks for Assessing Impact

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 Frameworks

The basic premise underlying a workfare program like 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 household 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 impacts of NREGS on a number of outcomes can be understood through its primary micro and macro impacts. First, at the micro level, the program affects the household time optimization problem by allowing households to work at a (potentially) higher wage on NREGS. It also provides insurance for household members who may not be able to find employment in the lean season. At the basic macro level, it channels additional funds into rural areas. In addition, the program also creates public goods that may improve productivity.

For households that choose to work on NREGS, the time that is now allocated to NREGS work in the household time-allocation decision might come from leisure or other non- or less productive activity, or it might come from other market work. The extent to which it comes from the former versus the latter is an important factor in determining the efficiency impact of NREGS on the economy. How households re-optimize given the new budget constraint determines the impact on various other outcomes, including potential general equilibrium impacts that change factor prices in the economy.

For example, much attention has been given to the impact of NREGS 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 declines in private sector employment (Imbert and Papp 2015a). Under non-competitive markets, such programs may actually increase both wages and employment (Basu et al. 2009). Whether these labor market impacts lead to increases in average 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 et al. (2009) add important twists related to access and credibility. As already highlighted, the 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 et al. (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.

A variant of the household optimization decision 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. At the macro level, the first channel relates to public goods or assets created through NREGS. If these public goods lead to an increase in productivity—for example, irrigation canals could increase agricultural productivity—this may lead to increased employment and household income.

Finally, it is worth mentioning channels that work simply through additional funds flowing into rural areas, and hence are not necessarily extra channels as 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.

The impact of NREGS on other outcomes derives chiefly from the primary channels described above. For example, one might see an impact 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. Choice of Papers for This Review*

Given the vast proliferation of studies attempting to examine the impact of NREGS, it is difficult to parse 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. 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 the level of the unit of analysis: generally households and villages. Some arbitrariness in selection of larger units of aggregation—for example, states and districts—is inevitable given logistical challenges, as well as simply that some

studies intentionally plan to study a particular state.<sup>12</sup> 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 that were excluded: 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.

#### ***4.3. Empirical Strategies: Advantages and Pitfalls***

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 ideal world of MGNREGA discussed in Section 3, in fact there is no single 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 to 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 are arbitrary).

In order to avoid repetition 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 particulars of NREGS.

1. *Experiments.* The gold standard for causal inference is a randomized control trial (RCT) experiment. In the most famous case of evidence from an RCT used to determine public policy, the initial rollout

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

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 MGNREGA 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 et al. (2016a) 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 under-appreciated 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 et al. 2016b). 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 MGNREGA 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: it is difficult to interpret impacts from the rollout. 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 MGNREGA 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. Moreover, since the actual algorithm is not known, there is also a lack of transparency when compared to the difference-in-differences approach, in which one can easily evaluate whether pre-trends are parallel. This strategy also faces the issue highlighted above, which is related to limited implementation in early years.
4. *Other.* While the second and third approaches represent the main empirical strategies, other strategies including those based on using household panels and those based on using instrumental variables have also been used by researchers examining the impact of NREGS. For these strategies, the standard issues remain: panels do not account for selection in and out of NREGS, and instrument variable strategies must not have weak instruments and must satisfy the exclusion restriction. Moreover, studies based on comparing participants to non-participants may be biased if the program also affects non-participants, for example via general equilibrium effects.

## 5. The Impact of NREGS

In this section, I discuss the impact of 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. Impact on Labor Markets

Not surprisingly, the impact of NREGS on labor markets has been studied intensely. At least four papers study the impact using nationwide data and the phased-in rollout of NREGS 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-MGNREGA) and 2007–08 (post-MGNREGA) 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”) with those in Phase 3 districts (“late phase”). They further find that the wage effect is concentrated in the “star states”, the label given to the 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 the late-phase districts prior to MGNREGA 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 the 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 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 discussed 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 regression discontinuity 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 groups, which are difficult to evaluate since the “algorithm values are not directly publicly available”; and is also 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 all the other papers compare both Phase 1 and Phase 2 districts to Phase 3 districts. Since Phase 2 started in April and May 2007, whereas the NSS data collection started on July 1, 2007, some of these Phase 2 districts would 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 et al. 2016b). We find that private sector wages for unskilled labor increased by 6.2 percent in the month of June as a result of improvements in NREGS, consistent with the 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. Moreover, this magnitude may, in fact, be biased downward due to spillover effects onto control areas, for which we find strong evidence.

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.”<sup>13</sup>

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 that 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 when NREGS was most active, it is also possible—particularly given nominal wage rigidities (Kaur 2015)—that these effects persist throughout the year. Moreover, the effect of this “crowding-out” on productivity is still uncertain. Finally, the channels through 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 of NREGS assets, there is no actual evidence on this. Indirect evidence from Muralidharan et al. (2016b) suggests that perhaps there is a productivity effect; total employment in treated areas—which saw a rise in work done on NREGS—increased (although not statistically significantly), while total employment in control areas, which only saw wages increase as a result of labor market competition, declined, suggesting perhaps that there was a productivity increase.

## *5.2. Impact on Income and Consumption*

How did the changes in labor markets affect the income and consumption of poor households? Given the uncertainty over private sector employment effects, it is not clear that an increase in wages would unambiguously lead to

13. Zimmermann (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 oneself 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.

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 regression discontinuity design that recognizes the fact that there was political interference in the 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/ST, this result is not robust, and the specification with the most straightforward application of the fuzzy regression discontinuity design does not show any significant effects on consumption and poverty.

More unambiguous and robustly identified results come from Andhra Pradesh. The Smartcards 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. The paper finds that the net effect of this improvement is a significant and robust increase in household income, verified not only in the survey data, but also by the entirely independent Socio-Economic and Caste Census (SECC) that was conducted by the Government concurrently (Muralidharan et al. 2016b). The survey data consist of a representative sample of job card holders and pensioners (close to 70 percent of rural households) for whom the paper shows an increase of ₹8,761 (12.7 percent) in annual income. The SECC by definition tracks every single rural household, and here they find that the highest income earner in the household was 24.7 percent more likely to move out of the lowest income category (less than ₹5,000 monthly income) in treatment areas.

Interestingly, the bulk of the increase in income is due to effects through private sector earnings rather than directly through NREGS. Increases in NREGS income accounted for only one-ninth of the income gains for NREGS beneficiaries, with the remaining coming from increases in private sector earnings, suggesting that the 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 that 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 1,000 ultra-poor households from 200 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 (4,000 households in 480 villages and five 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 of selection bias affecting the findings; also, given the fact that private wage effects obviously affect non-participants as well, households may not be the right unit for comparison.

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 the impact on private sector labor markets. Nonetheless, the precise channels for any increase remain unclear, as does the evidence for median implementing state.

### *5.3. Impact on 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 artefact 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 enrolment 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 clearest 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, enrolment, 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 years). In addition, they find that enrolment 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-cost-of-time hypothesis, there are no or limited effects for younger children (5–12 years). Meanwhile, the youngest children (2–4 years) benefit as a result of the income effect, with increased test scores and higher enrolment 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 years) spend 18 percent more time working outside the household as a result of NREGS, while younger children (6–9 years) spend 3 percent more time in school. They corroborate these results with panel data from three 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—the District Information System for Education (DISE), based on school level data—Li and Sekhri (2013) examine the impact of NREGS on school enrolment and pass rates. Like the previous papers, this one also finds declines in primary school enrolment, 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 cited above (Thomas 2015). This paper suggests that educational outcomes, including enrolment 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 uses the District Level Household and Facility Survey data (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 both based on data from only one state—Andhra Pradesh. The first uses data from the Young Lives Panel Study, which covers six districts in Andhra Pradesh with 757 children residing in rural areas (Mani et al. 2014). Given this small sample of districts, four of which were in the early phases of NREGS and two in the later phases, the effective variation in the sample is limited. The second paper uses sub-district level rainfall to instrument for female participation in NREGS, and finds that this led to increased time by girls spent in school, grade progression, and female bargaining power (Afridi et al. 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 enrolment and attainment worsened for older children, while these outcomes, and possibly health outcomes, improved for the youngest children.

#### *5.4. Impact on Migration*

As 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 et al. 2014). But the lack of data on migration is a binding constraint.

The study by Imbert and Papp (2015b) 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 et al. 2016b). 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 (2015b).

Overall, while it is likely that NREGS reduced short-term distress migration, more research on this topic, particularly on impacts related to long-term structural transformation, would be welcome.

### *5.5. Impact on Agricultural Choices*

Given the impacts on labor markets, and the potential insurance provided by the scheme, potential impacts on agricultural choices may be expected. First, for example, if wages increase, landlords may switch to more capital-intensive technologies. Second, 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 a 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 3. 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. While the result on labor-saving technologies appears reasonable, there is not much else that can be said on this very important issue of what impact NREGS has on agricultural choices.

### *5.6. Impact on 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 MGNREGA was to assist areas afflicted by the Maoist insurgency, or perhaps it is interesting since theoretically effects could go in either 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 that the higher wages would result in an increased opportunity cost of participating in violence.

Dasgupta et al. (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 Maoist affected “red belt.” The paper finds that NREGS leads to reduced violence, with a large, 50 percent drop in violent incidents, which is larger in districts experiencing negative rainfall shocks. Importantly, the results are entirely driven by Andhra Pradesh and Chhattisgarh, two high capacity states, as 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 attenuates violence related to rainfall, possibly by providing insurance. While the results seem plausible, how the precise timing of rainfall shocks interacts with violence and NREGS rollout is a bit unclear.

Khanna and Zimmerman (2015) use a regression discontinuity design, with both cutoffs (between Phase 1 and Phase 2 as well as between 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 seen in police-insurgent violence as well as in 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 regression discontinuity design specifically related to Maoist insurgency, which it attempts to do by dropping districts.

Finally, Amaral et al. (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 the massive under-reporting of domestic violence incidents (Iyer et al. 2011), it is difficult to know whether the impacts relate to actual violence or merely to reports of such violence.

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 there is much to be learned from the existing 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 sub-section above, the following three broad areas

represent important open questions to be explored further to 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 for settling the debate over whether NREGS is drawing away labor from productive uses in order to dig holes and fill them up, whether it is providing employment to under-employed labor, or possibly even breaking up non-competitive rural labor markets and enhancing efficiency. Given data and identification constraints, it seems as though it will be difficult to answer this question for the roll-out of the program. However, intensive margin changes as well as improvements in implementation may allow us to make some progress on this subject.
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. Unlike other questions, the answers to which rely on (difficult to find) exogenous variation, simply documenting the existence and value of these assets is eminently doable. For example, Ranaware et al. (2015) survey assets and households in 20 of the 33 districts in Maharashtra, documenting existence, type, and villager perceptions of usefulness. While in principle this study could have been informative, sample selection is flawed: the study selects the five best performing GPs in the best performing block in each district. This severely limits the interpretation of a statistic like 87 percent of completed works were found to be in existence a year and a half after completion.
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 a 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, 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 with lessons for the future from the experience of doing research on NREGS, as well as prospects for the near- and middle-term future of the program.

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 original proponents of MGNREGA: 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 individual bureaucrats may not have 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, “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.

What about the future of the program itself? Does the evidence suggest that it is a well-designed program that might perhaps be badly implemented, but has scope for improvement, or a fundamentally faulty program that should not be expected to alleviate poverty? This question is particularly important in light of the recent advances in payments infrastructure, via which a cash transfer program might be implemented. For example, calculations by Dutta et al. (2014) suggest that a (hypothetical) crude cash transfer scheme would dominate the NREGS in poverty reduction in Bihar.

Given that specific evidence is not available at the moment to answer the above questions, I must step now into the realms of conjecture. I have two thoughts, deriving from my own experience in conducting research on NREGS and on Direct Benefit Transfer (DBT) initiatives, as well as from reading the literature. The first is that at the moment, the state of infrastructure and implementation capacity does not support a large-scale shift to pure cash transfers (the Bihar hypothetical situation cited above notwithstanding—most states have better NREGS implementation). While this may change in the

future—the demonetization policy just introduced might push electronic payments further—purely based on anticipated problems with logistics and targeting, such a shift does not seem to be currently warranted.

Second, NREGS has two features that are not shared by cash transfers: the creation of public assets and changes in bargaining power for recipients, which may impact non-participants through general equilibrium effects on wages. Both these features may increase output in the rural economy beyond what might be possible via simple cash transfers (the latter via effects on potential monopsonistic labor markets). To date, evidence that these features of NREGS may be influential is limited to the Muralidharan et al. (2016b) study in Andhra Pradesh; however, if these results extrapolate to other states, this would indicate that the asset creation and bargaining power design features of NREGS are truly important and worth persevering.

Ultimately, as Amarjeet Sinha (Secretary, Ministry of Rural Development, at the time of this writing) has pointed out, the NREGS is not meant to be seen as a program in eternity: “No developed country has a pool of 5 crore households wanting to do unskilled wage labor.” In the interim, the NREGS is likely to remain a key policy for protecting rural households.

I end on a hopeful note. The fact that heterogeneity is enormous has been repeated ad nauseam. The implications for policy, however, are that a balance is needed between Central schemes and rules, allowing states to experiment to find local solutions. The Andhra Pradesh experience with Smartcards suggests both that continued tinkering with NREGS implementation may have large benefits, and also that these modular changes to the program may allow for careful and rigorous evaluation. Down this path lies the likeliest probability of success for large public programs such as NREGS.

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# Comments and Discussion\*

**Rinku Murgai**

*The World Bank*

I want to first thank Shekhar Shah for asking me to comment on the paper. NREGS is a program that I have done some research on, but there are so many papers that have been produced on it that it is difficult to keep track of the literature. So, I think what Sandip has done, by summarizing the lessons emerging across papers, is marvelous. I know about half the papers well, I agree with his read of all the ones that I know well, and I trust his judgment on the others. He has done a service to the development community by sorting through these papers. For the reasons that he has mentioned, it is incredibly difficult to come to a judgment on what we have learned so far about the program. The methods are different, the samples are different, and questionnaire designs are different. We have learnt that questionnaire design can make a huge difference, in the same context, in the same time, on what you can conclude about the program. We get very different estimates even on leakage, on take-up rates, things that should be relatively simple, even setting aside questions of impact.

Also inherent to the design of the program is the fact that we should *expect* to see a lot of heterogeneity. Local government plays a big role, so you should see heterogeneity in different states, you should see heterogeneity in different districts. Implementation capacity varies across states, so we should see heterogeneity as a result of that, and the nature of bottlenecks has continued to evolve over time. Therefore, it is not surprising that we see a very diverse set of results across the studies that have been done. Sandip's work is incredibly useful because keeping in mind the heterogeneity that may arise due to differences in methods across studies, he is able to ask what can be safely concluded from the set of studies currently available on NREGS.

\* To preserve the sense of the discussions at the India Policy Forum, these discussants' comments reflect the views expressed at the IPF and do not necessarily take into account revisions to the conference version of the paper in response to these and other comments in preparing the final, revised version published in this volume. The original conference version of the paper is available on [www.ncaer.org](http://www.ncaer.org).

My main quibble with the paper is that it shies away from any reflections on what the wealth of analysis done so far implies for the *future* of the program. It is much too coy here. Let me caricaturize this to be provocative.

There is a large set of papers that focus on challenges of program implementation. There are advocates who focus on building awareness, on employing tools of e-governance, community-based monitoring, strengthening local capacity, improving the productivity of assets, and so on, with the premise that the program design itself is sound, but does not work well in practice. The key questions these papers address are how the program's implementation could be strengthened.

Then there is a set of papers and views, at the other extreme, that are asking the following question: Aren't there simpler alternatives to achieve the poverty reduction objectives within the same budget, in particular, an unconditional cash transfer? This isn't just a hypothetical discussion because the architecture for moving towards cash transfers is being put into place with the emphasis on the so called JAM (*Jan-Dhan* accounts, *Aadhaar* ID, and ubiquitous mobile payments). It is also not hypothetical because India does implement social pension schemes that are unconditional cash transfers, and which work better, at least on some of the issues that NREGA is faulted for. For example, on leakage, on the time taken to deliver payments, on the difficulties that people incur when they are trying to collect payments, the pension schemes actually work reasonably well. So, it is not the case that moving to cash transfers is entirely hypothetical.

Given these two extreme sets of views about the future evolution of the program, I would pose a few questions and suggestions for extending the paper further, and for Sandip to speculate which way the program should go. The first question that we should ask is: Is MGNREGA cost-effective? How does it compare to other schemes, what alternatives should it be evaluated against?

My work with my co-authors, based on panel survey data that we collected for about 3,000 households, about 6,000 workers, in Bihar, compares the performance of NREGS to a hypothetical cash transfer scheme that was totally untargeted, and to one that was targeted based on the current distribution of BPL cards. If we compare the status quo in NREGS and compare it in Bihar with the hypothetical ones, making similar assumptions for example on leakage, we find that MGNREGA would be dominated by this very crude cash transfer program. Why is that? We think that it comes from the fact that there are relatively high levels of foregone income, because people give up income to work on the scheme and that reduces by itself the net impact of the program for a given budget.

But also, in our analysis, we don't take into account any potential spillover impacts of asset creation. So, one of the conclusions I draw from this is that for NREGS to succeed fully, the emphasis on asset creation is incredibly important. This draws me to the point that Amarjeet Sinha made in his comments, immediately following the paper presentation, that we have simply not focused much on the assets being created by NREGS, and that this remains quite an under-studied subject, especially if we were to apply the standards of evidence that Sandip poses in his paper.

The second question that we are not thinking about enough, but is important given the direction in which the Ministry has been moving, and how often it seems to come up in policy discussions, is: Is there mission creep in the NREGS program, and what does that imply for its effectiveness? Partly, it may be mission creep that is what is making it difficult to evaluate. If you say NREGS is not doing well on employment generation, then people say that it is not about employment, the objective is empowerment and asset creation. If somebody faults the program on asset creation, well, then it is not about assets, but it is about preventing distress migration. The direction NREGS is moving in is towards further over-burdening the program on the grounds of 'convergence' with other schemes. Policymakers are pushing not for just vertical performance but also coordination horizontally. As a research community, how can one evaluate these movements?

The third suggestion is to extend and deepen the discussion of implementation challenges. The paper notes these challenges, but they deserve more attention, precisely because the devil really is in the implementation details. Given that the program is already nationwide, as researchers there is room to work with government to understand how the program can work better and think about different modalities of implementation. Even if not at the gold-standard, the Muralidharan, Niehaus, Sukhtankar (2016) paper clearly shows that we can even do RCTs on specific implementation aspects as a worthwhile strategy to address implementation challenges.

Finally, I will not go into my own views on what the key directions for NREGS reform are, but I think Sandip should, having read all the literature. It would be worth having an IPF paper on this.

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## Surjit S. Bhalla

*Oxus Research & Investments and NCAER*

Sandip Sukhtankar’s paper on NREGS is a comprehensive survey of the issues related to the operation of the world’s largest employment program. While informative and rigorous, I find that the literature surveyed reaches some questionable conclusions about the efficacy, or “performance” of the NREGS scheme.

While MGNREGA became law in 2005, the first nation-wide implementation of the NREGS took place in 2009–10. Many of the studies Sukhtankar surveys are for years *prior* to 2009, i.e., they are partial in coverage, and hence in the scope and validity of their conclusions.

The efficacy of NREGS has been discussed ever since its inception. As the evidence has accumulated, the NREGS debate today, and for the last several years, looks to be no different than the evergreen debate about absolute poverty and poverty levels. All conclusions are “valid”, i.e., poverty has gone up, gone down, and everything in between. In this circumstance, it is incumbent on economists to point the way towards the most reliable of conclusions.

The conclusions that do *not* appear from Sukhtankar’s survey is that NREGS provides much less employment to the poor than popularly believed, that it cannot possibly have increased real wages for non-NREGS workers, its record on targeting is very poor (less than 16 percent of the NREGS expenditure reached the poor), and it is one of the most expensive (corrupt?) programs in terms of government expenditure—₹24,000 were spent to bring one person out of poverty in 2009–10 (see Table 2 and below). Let me elaborate.

### *What Does the Evidence Say about NREGS?*

Before presenting the results on performance, following is a brief summary of the claims made in favor of both the introduction, and continuation, of the NREGS program (Bhalla 2016):

1. It is a self-selecting program and so targeting of beneficiaries is not a problem.
2. The program is *large* in scale, and by mandating and providing minimum wages, it helps raise rural wages.
3. It helps reduce poverty by large amounts and, therefore, may be the most efficacious poverty program not only in India, but in the entire developing world.

4. It also costs very little: less than 0.45 percent of GDP in 2009–10, the first full year of implementation, and also a drought year, so one can attest to its importance.

An important reason for the extraordinary debate on the efficacy of NREGS is that there are two data sources, including on rural employment and wages. The official NREGS source is the data provided by Ministry of Rural Development, the “owner” of the program. The other source is household surveys (the most comprehensive being those of the NSS), which asks individuals about their participation in the program.

In 2009–10, the NSS tagged two or three questions about NREGS onto their five-yearly survey on employment, unemployment, wages, days worked, etc. The survey contained a headline question asked of all 1.25 lakh households: did any member of the household participate in NREGS work, and if they did, how many days were worked, and at what wage rate? In the same NSS survey in 2011–12, the NSS authorities repeated all the questions, including how much salt and toddy was consumed, but the aggregate question on NREGS employment was not asked. Thus, NSS 2009–10 is the best (and only) cross-check on the data provided by the Ministry of Rural Development (MoRD). It is the only data to test the claims of the advocates, and critics, of the NREGS program.

### *What Does the NSS 2009–10 Data Show?*

The political economy of NREGS is indicated by the fact that the data put out by the MoRD (a ministry that has been keen to fund research on NREGS) does *not* match data on NREGS not published by the MoRD.

First, the MoRD data vastly overstates the employment provided by NREGS. The MoRD website claims that in 2009–10, 2,840 million man-days of work were provided to 52.9 million households. The NSS survey indicates that 36.2 million households worked on NREGS for 1,360 million workdays. The overstatement by MoRD is more than double the (actual?) NSS estimate (Table 1).

Second, the total number of days worked in the rural areas in 2009–10 according to the NSS was 90.9 billion (Table 2). The largest employment program in the world provided 1.36 billion workdays, or 1.5 percent of the total. This has to be the smallest tail wagging the biggest dog in the world.

### *NREGS and Impact on Wage Levels and Wage Growth*

In theory, NREGS workers are the poorest of poor, doing back-bending work. So conceptually, we need to ask: what theory suggests that the poorest

TABLE 1. NREGS: Comparing NSS &amp; Ministry of Rural Development Data

Variable	2009–10	2011–12	Growth(%)
<i>All figures in millions; unless stated otherwise</i>			
<b>National Sample Survey Statistics</b>			
Households with NREGS work	36.2		
Work-days of NREGS work	1,360		
Average rural wage (₹/day)	156	244	56.0
NREGS worker wages (₹/day)	89	106	19.1
Ploughman wages (₹/day)	82	123	50.0
<b>Ministry of Rural Development Statistics</b>			
Households with NREGS work	52.9		
Work-days of NREGS work	2,840		
Average NREGS wage (₹/day)	89	115	29.2
Ploughman wages (₹/day)*	126	177	40.5

Sources: As stated, except for ploughman wages marked\*, which are from the *Labour Bureau, Government of India*.

segment of the population can cause the average wage to move upwards? Those who claim that 1.5 percent of the poorest workers in the economy, and doing unskilled wage work, lifted the average real wages of the remaining richer 98.5 percent of the population, by as much as 5 percent (as reported by Sukhtankar), have to provide some explanation other than the contention that they did a randomized control experiment (RCT). Even RCTs have to pass a smell test.

As documented in Table 1, the NSS estimate of the wages of NREGS workers and that reported by MoRD are identical at ₹89 per day. However, the rate of growth of NREGS wages differs substantially; NSS estimates this growth to be 19 percent between 2009–10 and 2011–12, while MoRD estimates the growth to be much larger, i.e., 29 percent.

When NREGS went national in 2009–10, the wages it paid were slightly above those of a ploughman according to the NSS. Two years later, the ploughman was making almost half as much again, while the NREGS worker had a wage increase below the rate of inflation. The key point is that *nominal* rural wages did indeed rise after the introduction of NREGS. If NREGS wages were pushing up other rural wages, they would probably be growing faster, not slower, than non-NREGS wages.

Also reported is the average wage per day in the rural economy: ₹156/day in 2009–10 increasing to ₹244 in 2011–12, a 56 percent increase. Given that



NREGS workers account for only 1.5 percent of workers in rural India, and given that the rural wage rate is nearly half of the average, it is a bit inconceivable (and worrisome) that researchers conclude that NREGS made possible a 5 percent real wage increase in the aggregate rural economy.

### *Poverty Reduction—The Influence of NREGS*

The other claim of NREGS advocates is that the NREGS expenditure was well spent because it helped reduce absolute poverty. This is a testable proposition (Table 2).

Less than one-third of poor households received NREGS payments. The average payment received per household in 2009–10 was ₹284/month; per individual, an average of ₹2 per day or ₹61 a month. The impact on poverty was consequently quite limited, while the costs of the program have been substantial.

The NREGS program involved an expenditure of ₹38,900 crores and, in its absence, rural poverty incidence would have increased from 38.3 percent poor to 40.5 percent. In other words, NREGS allowed 2.2 percent of the rural population, or 16.5 million individuals to become non-poor in 2009–10. The cost of this policy: about ₹24,000 to make one individual not poor.

The total subsidy transfer to the poor amounted to an average per capita monthly payment of ₹61; for twelve months, this is equal to a transfer level of ₹732 per person. A total of 89.4 million individuals received this benefit

**TABLE 2. NREGS Poverty Reduction: How Successful?**

<i>Variable</i>	<i>Rural</i>			<i>NREGS</i>		
	<i>Total</i>	<i>Poor</i>	<i>Non-poor</i>	<i>Total</i>	<i>Poor</i>	<i>Non-poor</i>
Workers ( <i>in million</i> )	282	97	185	78	34	44
Total workdays in the year ( <i>in billion</i> )	90.9	30.8	59.6	1.36	0.53	0.83
Population ( <i>in million</i> )	742	284	458	183.7	89.4	94.3
<i>as % of total</i>	100	38.3	61.7	100	48.7	51.3
If no NREGS program:						
Population ( <i>in million</i> )	742	300.5	441.5			
<i>as % of total</i>	100	40.5	59.5			
<b><i>Reduction in Poverty due to NREGS</i></b>						
Population ( <i>in millions</i> )	16.51					
Population ( <i>in percent</i> )	2.2					

Sources: NSS Survey, 2009–10; Author's calculations.

for a total transfer amount of ₹6,544 crores. Hence, with a total NREGS expenditure level of ₹38,900 crores, the government was able to transfer 16.8 percent.

Prime Minister Rajiv Gandhi famously said in 1985 that only 15 percent of the money spent on the poor (he was mainly talking about PDS—the public distribution system for food grains) actually reached the poor. This conclusion has, in 2017, also been accepted by the Supreme Court. In their hearings on *Aadhaar*, a bench of justices A. K. Sikri and Ashok Bhushan said:

A former prime minister of this country has gone on record to say that out of one rupee spent by the government for welfare of the downtrodden, only 15 paise thereof actually reaches those persons for whom it is meant. It cannot be doubted that with UID/Aadhaar much of the malaise in this field can be taken care of... (*Indian Express*, June 9, 2017).

Both PDS and NREGS are very close to Rajiv Gandhi's assessment made 30 years ago. I want to conclude by stating that the data suggests that India has three of the four most corrupt institutions in the world. Though hard numbers are difficult to obtain, I would guess that FIFA, by universal agreement, would be allegedly the most corrupt institution in the world; that BCCI, allegedly, would be number 2; PDS wins the race and edges past NREGS to be number 3 (less than 15 percent of the expenditure reaches the poor), and NREGS is placed at 4<sup>th</sup> with 16.8 percent of the expenditures actually reaching the poor.

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## General Discussion

R. Nagaraj asked for a more detailed explanation of the paper's results on household participation in NREGS and education. Govinda Rao urged the author to examine the design of the program in greater depth. Though the conceptual design stipulates that resources be allocated to states based on the incidence of rural poverty, the actual funds following the Act are released

based on utilization certificates, and therefore spending capacity. A basic design flaw is that MoRD has no incentive to return any unused funds to the Ministry of Finance, so the entire unused NREGS budget at the end of the fiscal year is reallocated to the states that have utilized the money. As a result, there is little incentive to improve state implementation capacity of the poorest states where such capacity is likely to be the worst, and therefore no assurance that states with the largest concentration of rural poverty will get the most money.

Indira Rajaraman said that in the initial phase NREGS was confined to a small set of districts because there was evidence pointing to the clustering of rural poverty in these districts. The eventual inclusion of all districts in the later phase meant that less and less deserving districts were brought under its ambit. She asked if the author had looked into the question of access to NREGS in the initial, most deserving districts versus access in the final, least deserving districts, to examine whether there was any inter-phase inequality in access.

Pranab Bardhan recalled the days when the NREGA Bill was still under political and popular discussion and had not become an Act, when it was being considered as a safety net and not as a big anti-poverty program. He asked the author that if we took its safety net goal—the last fallback option for workers in the lean season—as the main objective, how far had NREGS fulfilled that goal? Second, Bardhan asked if the literature suggested what the estimates of foregone earnings might be if we looked only at foregone income in the lean season, and not on an annual basis (again, seeing NREGS only as a seasonal relief program and a fallback option during the lean season). Third, he noted in the context of rural labor markets that female participation in NREGS, particularly in recent years, had grown substantially, driven by an identical NREGS wage rate for men and women. Does the literature have any evidence to show that NREGS has had a significant impact on rural male–female wage disparity?

Robert Lawrence was concerned that the paper did not critically evaluate the supply and demand frameworks being used in the literature that it had reviewed. What is the counterfactual that is being considered in the literature? What are the opportunity costs for NREGS workers at a given point in time? The question is not whether labor markets are competitive or not, but what would the workers accepting NREGS work have done in the absence of such work? What if the relevant counterfactual is that people would have been idle, and the impact of NREGS has been to shift the labor supply curve out? Or what if they were doing something already but at lower wages, and have now been drawn into the program? So, there would

be different expectations under different counterfactuals. The relevant criterion is not if the market is competitive or not, but rather if the author can explain what the program is trying to do, whether it is emergency work for the idle or it is attracting people earning lower wages into the labor market. What are the counterfactuals?

Dilip Mookherjee turned to Rinku Murgai's comments on her Bihar work comparing NREGS with targeted cash transfers and pointed out that Bihar was particularly poor in implementing NREGS, that the results were sensitive to the poverty measure used, and new work attempting to extend these results to all states and using alternative poverty measures would be useful to review, particularly given the heterogeneity of program implementation. He also noted that the work assumed that the average leakage rate was the same for cash transfers and NREGS. At least in West Bengal, his observation was that many cannot access cash transfers into their accounts, and commissions have to be paid to an intermediary to help with the process of withdrawal. This underscores the need for a lot more work with careful data collection on leakages related even to cash transfers, whether into accounts, through mobile transactions, or through other means, in comparing with NREGS leakages.

Rajnish Mehra suggested that we could view NREGS from an asset pricing perspective as a form of insurance, as a safety net. Insurance on average has a negative rate of return, but is nonetheless important because it pays off in periods of extremely high marginal utility. So it is possible to have a low or negative rate of return for an asset that is being used for consumption smoothening, but which pays off in bad times with very high marginal utilities for society.

Karthik Muralidharan felt that the two positions on NREGS that emerged from the paper framed the debate exactly right: one said that the program is well designed but poorly implemented, and the other said that the program design itself was fundamentally flawed, i.e., NREGS may be implemented efficiently, but its design may be weak, the weakness being one of the fundamental misallocation of resources. For example, if the core driver of economic development is urbanization and structural transformation, and if NREGS holds back the relocation of labor and other resources from migrating to urban areas, that would be the deep design concern about NREGS.

He went on to say that Imbert and Papp (2015) had found wage effects, but no one before the Andhra Pradesh (AP) work of Muralidharan, Niehaus, and Sukhtankar (2016a and 2016b) had found reliable income effects. The SECC data, which was independent of the experiment they ran, showed a significant reduction in poverty. Their work showed the mechanism by

which this may have happened: that NREGS not only caused an increase in wages but also an increase in the reservation wage. As basic minimum wage theory would tell us, it was not the number of days worked under NREGS that would affect the market, but it was also the credibility of the outside option, the impact on the reservation wage, that would affect what the market had to pay. In the AP case, smartcards were not just reducing leakage, but also improving the payments process, allowing beneficiaries to get their money faster without paying commissions, and enabling an increase in access. These were improvements in implementation with regard to every metric of what NREGS was supposed to do. So all this was amplifying the general equilibrium (GE) impact of NREGS on the market.

Muralidharan said in responding to Surjit Bhalla's discussant's comment that this was the reason, through the GE effects of NREGS, that the tail could wag the dog. It was difficult to parse the positive impact on incomes in terms of worker productivity effects or the impact on local monopsonies for employment. He did agree with Bhalla that these results came from AP, where the government had made a political decision to invest in the implementation of NREGS. Their AP results show that a well-implemented NREGS is clearly redistributive as landlords were worse off because the wages and bargaining power of landless labor had gone up, signifying that improving NREGS implementation was a conscious political decision. The evidence thus suggests that if the program is well implemented, it has not only GE effects but also direct poverty effects, and in the case of NREGS, the GE effects can be much bigger than just the impact of the cash transfer.

Martin Rama asked a question about whether we could tie NREGS impact to distress land sales during times of drought. Kirit Parikh noted that adverse selection was the basis of the self-targeting approach of NREGS, given the difficulty of identifying the poor, or ensuring that there are no significant leakages to the rich. But if the wage rate was set too high, that advantage would of course be lost.

Prem Vashishtha noted that the problem of low allocations to the poorer states has its roots in the fundamental problem of low capacity at the Gram Sabha level. It is here that communities are supposed to come up with an annual labor budget, and it is not surprising that the results are not very good because capacity at this level varies tremendously. He recalled his own experience in Tripura, where even in a forest village he found a very active Gram Sabha, and contrasted that with his experience in parts of Bihar or UP where there was no such capacity or interest.

Amarjeet Sinha (Chair) concluded the discussion by applauding the paper and the incisive comments of the discussants and other participants. He

agreed with the author that for programs like NREGS to succeed, they had to be led by the states, as seen in the case of NREGS from the examples of Andhra Pradesh and Tripura. He noted that levels of corruption have come down in NREGS over the years, thanks to payment mechanisms promoting transparency and greater efficiency in getting the transfers to reach beneficiaries. The Ministry of Rural Development was working with the Department of Financial Services to help deal with these last mile problems.

He noted that self-help groups (SHGs) were vital to success because they led to community ownership and greater accountability, as the example of AP again showed. Women SHGs have been particularly important here. Even Bihar, with not a great record of NREGS implementation, had one of the most vibrant SHGs in its *Jeevika* program. Corruption levels have come down wherever the *Jeevika* SHGs ran the PDS. Thus, MGNREGA has to be seen in the larger framework of the political economy rather than as a standalone program. This is also important from the perspective of the Act's emphasis on livelihood security that can only come from integration with SHGs and with individual beneficiary schemes. It is not the case that all poor households are landless, but assets are required to increase the productivity of the land for a safe one crop, or for multiple crops, or to promote animal husbandry, poultry, and horticulture, among other things.

Sinha agreed that the Act may need changes, especially in the context of its stipulation that work has to be provided wherever it is demanded. That meant that while we may use poverty criteria for allocation of resources to states, those can't be formally promulgated because they would not align with the Act. The labor budget, while it is provided, is a completely tentative figure because if the state ends up using or providing more work, money has to be made available for it according to the Act. There are thus issues with the Act about the allocation of resources to states that need to be resolved. He also touched upon the legal stipulation of maintaining a 60:40 wage and materials ratio under NREGA, and said that the Government was tweaking this rule where possible to augment employment opportunities and improvements in the assets created under NREGS at the Gram Panchayat level.

Sinha noted that in the lean season there clearly are states where there is an element of rationing due to the gap between demand and supply. For instance, a recent, national level study found that even the articulation of the demand for work under NREGS could be conveyed to people responsible for initiating employment works in only 45 percent of designated villages. He suggested that the paper should carefully look at the feminization of labor in NREGS, particularly in states like Tamil Nadu. On wage rates, he mentioned that NREGS exhibits a mixed picture, with 17 states with a minimum wage

for agricultural laborers higher than the NREGA wage, and 17 other states and UTs with minimum wages lower than the NREGS wage.

Other measures that should have a positive impact for the beneficiaries of NREGS include universalization of *Aadhaar*-linked bank accounts for payments, geotagging of all assets, periodic outcome-based performance assessments, and the promotion of more independent research studies, all of which should enable the program to improve.

Sinha concluded his comments by noting that NREGS also needs to embrace a skilled workforce since India cannot continue to nurture a pool of 50 million rural households engaging only in unskilled wage labor. He mentioned that the Government had introduced the Livelihood in Full Employment (LIFE) program in 2015 catering to NREGS workers who had done 100 days of work and could then acquire skills for wage employment, self-employment, or livelihood augmentation.

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