Is Workfare Cost-effective against Poverty in a Poor Labor-Surplus Economy?

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Workfare has often seemed an attractive option for making self-targeted transfers to poor people. But is this incentive argument strong enough in practice to prefer unproductive workfare to even untargeted cash transfers? A nonparametric survey-based method is used to assess the cost-effectiveness of a large workfare scheme in a poor state of India with high unemployment. Forgone earnings are evident but fall short of market wages. For the same budget, unproductive workfare has less impact on poverty than either a basic-income scheme or transfers tied to the government's assignment of ration cards. The productivity of workfare is thus crucial to its justification as an antipoverty policy. JEL codes: I32, I38

Workfare schemes impose work requirements on welfare recipients. The policy arguments for doing so have rarely been based on the value of the outputs from that work. Rather they have been that workfare deals with the problem of targeting when informational and administrative constraints preclude optimal income taxes/transfers. By only attracting those in genuine need and encouraging a return to the regular workforce when help is no longer needed, workfare incentivizes behaviors that solve the problem of knowing who is genuinely "poor" and who is not (Besley and Coate 1992).

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In probably the most famous example of the use of this "self-targeted" feature of workfare, welfare recipients in pre-modern Europe were often incarcerated in "workhouses" where their "bad behaviors" could be controlled. From the outset, the idea was that only the poorest would be willing to be so confined. Workhouses were thus seen as a cost-effective means of poverty relief (Thane 2000, 115). Reforms to England's *Poor Laws* in 1834 famously used workhouses to assure better targeting, and this appears to be the main reason that public spending on relief fell from 2.5% of national income around 1830 to 1% in 1840 (Lindert 2013, figure 1).

But is unproductive workfare a cost-effective way to reduce consumption poverty? There is evidence that workfare is indeed self-targeted. For example, in the workfare scheme in India studied in this paper, the mean participation rate falls steadily from 35% of the population for the poorest few wealth percentiles to nearly zero for the richest (Dutta et al. 2014). Without any explicit effort at targeting the poor, participation favors poor families. However, even excellent targeting matters little if the net gains to workfare participants are small. This could be expected if the scheme offers market wage rates in a competitive, fully employed, economy. However, workfare tends to be advocated in places or at times with high unemployment, such as during recessions, famines, or lean agricultural seasons. Advocates often assume (explicitly or implicitly) that workers would be idle in the absence of the scheme and conclude that the net gain is the workfare wage.

Is that assumption plausible? The famous Lewis (1954) model of economic development assumes that labor can be absorbed from peasant farming into the modern sector with little or no loss of rural output. However, as Rosenzweig (1989) points out, this requires either zero marginal product of rural farm-labor or that, with one less worker on the family farm, other family members make up the difference by working harder. One can question the plausibility of both conditions. More generally, there is also a private rural labor market, with some probability of finding work during any period of workfare participation. We still know very little about the forgone earnings of workfare participants.

The forgone income is unlikely to be negligible even with reasonably high unemployment overall. Self-targeting is essentially assured when the private opportunity cost of participation is lower for poorer people. But it is unlikely to be zero for all. The magnitudes of these hidden costs clearly matters to the policy choice. In an influential policy report, World Bank (1986) argued that workfare schemes are unlikely to be cost-effective against poverty given the private opportunity costs of labor. No evidence was presented. As we will see, the distribution of forgone incomes also matters to the eventual impacts on poverty.

However, even if there is no forgone income, workfare schemes incur nonnegligible other costs that would not be incurred in alternative cash transfer schemes. Workfare requires outlays on relatively skilled labor for organizing and supervising the worksites and it calls for outlays on complementary material inputs. We call these the "nonwage costs." Even in the highly unskilled-labor intensive schemes in South Asia, these extra costs account for about one third of the public outlay.

We study a large workfare scheme in the Indian state of Bihar. This is one of the poorest states of India (the poorest by some measures), with 55% of its rural population of 90 million living below the official poverty line in 2009/10.[1] At the same time, the rural unemployment rate of 18% (16% for men and 32% for women) is twice the national average (Ministry of Labour and Employment, 2010). The rate of under-employment (workers whose normal status is employed but work less than they want) is likely to be even higher.

This is the type of poor labor-surplus economy in which workfare schemes have been seen to have much promise for fighting poverty. With that aim in mind, a large national workfare scheme was introduced in 2005, promising to give up to one hundred days of unskilled manual work per year to any rural household that wants it.

The paper asks whether the pure workfare aspect of this ambitious scheme is sufficiently pro-poor to justify it as an efficient means of transferring money to poor people. Could it be that the information constraints are so severe and the unemployment rate so high that the self-targeting mechanism using work requirements tilts the balance in favor of workfare even if the work produces nothing of value? Or are the latent forgone incomes and nonwage costs too large even in this poor labor-surplus economy?

One of the methodological challenges is estimating forgone earnings. We base our estimates on survey responses to counterfactual questions—essentially asking respondents to predict what they would have earned in the absence of the scheme. This has a number of advantages over the alternatives (as discussed later), and we find that it gives plausible results when compared with the mean wage earnings of casual workers concurrently employed. However, we recognize that our survey-based method may lead us to over-estimate forgone incomes when individual respondents do not allow for substitution possibilities, either over time or between people. These can generate double-counting of forgone opportunities in aggregation. (For example, the individual worker may know of another job that would have been available without the workfare scheme, but other workers could be thinking of the same job in their responses.) We test sensitivity to the possibility of even substantial over-estimation of forgone earnings using our method.

Another important issue in addressing these questions is the choice of the counterfactual. It would hardly be surprising that one can reduce poverty by spending public money on a large workfare program under ideal conditions (largely financed by taxation on the nonpoor). Arguably the more interesting question is

1. This is based on official Planning Commission poverty lines for 2009/10. The state has had one of the lowest long-run trend rates of poverty reduction in India (Datt and Ravallion, 2002).

whether a greater impact is possible with some feasible alternative allocation of the same public resources.

An obvious counterfactual is a basic-income scheme (BIS).[2] This guarantees a fixed cash transfer to every person, whether poor or not. There is no explicit effort at targeting. The administrative cost would probably be low, though not zero given that some form of personal registration system would be needed to avoid "double dipping" and to assure that larger households receive proportionately more.

The near-term feasibility of a BIS in this setting can be questioned, though the system of national identity cards (*Aadhaar*) being rolled out will change that. Given that a BIS would require a new public delivery mechanism, an alternative counterfactual of interest because of its near-term feasibility is to use existing targeting instruments, however imperfect, as the delivery mechanism for budget-neutral transfers. India also has a system of subsidized food rations based on an assignment of "Below Poverty Line" (BPL) cards done by state governments and delivered through local outlets with wide coverage. There have been many claims that BPL cards are not as well targeted to the poor as they could be.[3] Here we take the existing assignment of cards in our survey data as given and use it to constuct an alternative counterfactual for assessing the cost-effectiveness of workfare, namely an allocation of the same budget but assigned instead as transfers in cash or kind to those holding BPL cards.

The following section discusses the program under study, while section 3 describes our data and estimation methods. Section 4 examines what we learn from the survey data about the wages received by workfare participants. Section 5 turns to our findings on forgone incomes. Combining these elements, section 6 provides our estimates of the poverty impacts relative to the two budget-neutral alternatives described above. Section 7 concludes.

I. The Program

India has had a long history of workfare schemes. The essential idea was embodied in the Famine Codes introduced in British India around 1880, and such schemes have continued to play an important role to this day in the sub-continent. An important sub-class of workfare schemes has aimed to guarantee employment to anyone who wants it at a pre-determined (typically low) wage rate. Such Employment Guarantee Schemes (EGSs) have been popular in South Asia, notably

2. This has been called many things including a "poll transfer," "guaranteed income," "citizenship income," and an "unmodified social dividend." BISs have been proposed by (among others) Meade (1972), Atkinson and Sutherland (1989), Ravallion and Datt (1995), Raventós (2007), Bardhan (2011), and Davala et al. (2015).

3. For example, Besley et al. (2012) find that being a local politician makes it more likely that someone will have a BPL card after controlling for wealth indicators, including landlessness. At the time of writing, the use of the BPL card for food distribution is in flux in India. The 2013 National Food Security Act envisions higher coverage levels overall, with targeting left to the decision of individual State governments.

(though not only) in India where the Maharashtra EGS started in 1973 and was long considered a model (Drèze 1990; Ravallion 1991). In 2005, India's central government implemented a national version, now called the Mahatma Gandhi National Rural Employment Guarantee Scheme (MGNREGS). This promises one hundred days of work per year per rural household to those willing to do unskilled manual labor at the statutory minimum wage notified for the program. The work requirement is (more or less explicitly) seen as a means of assuring that the program is reaching India's poor. The available evidence does suggest that the scheme is quite well targeted to poor rural households; see Alik-Lagrange and Ravallion (2015) for India as a whole and Dutta et al. (2014) for Bihar.

MGNREGS aims to reduce rural poverty and the promise has been great. Indeed, advocates have claimed that it could largely eliminate poverty in rural India. For example, Drèze (2004) claims that the scheme "would enable most poor households in rural India to cross the poverty line." That might seem a tall order, but there can be no denying that this is an ambitious and well-intentioned effort to fight poverty in India and that, in principle, it has huge promise. The sheer scale is impressive; according to the administrative data, over 50 million households participated in MGNREGS in 2009/10.[4] The scheme is nation-wide. Here we focus on one state, Bihar, and we refer to MGNREGS in Bihar as BREGS.

The first and most direct way this scheme tries to reduce poverty is by providing extra employment on demand in rural areas and in a way that is self-targeted to poor people. Intuitively, it is easy to imagine that low-wage manual labor, often under a hot sun, would not be attractive to anyone who is not in fact poor. Indeed, as already noted, the scheme is reaching poor people. By providing an assured fallback position in wage bargaining with private employers, BREGS may put upward pressure on wage rates for casual labor outside the scheme. A well-functioning scheme that guarantees employment can also provide crucial insurance and safety net benefits when economy-wide or idiosyncratic shocks occur. There may be other nonpecuniary benefits to participants: they may acquire skills, gain utility from working for the government rather than the local landowner and some among them may be empowered by such benefits and the government protected right to work.

However, we already know from the literature and field observations that there are a number of ways that the potential impact of this scheme might not be realized in practice:

- The supply side may be slow to respond to the demand for work on the scheme, leaving un-met demand (rationing).
- Workers may be unable to meet productivity norms for earning the minimum wage.
- There may be delays in payment (random or purposive).

4. See Government of India website for MGNREGS (http:\\nrega.nic.in).

- Corruption may be present, whereby local leaders or officials take their cut.[5]
- There may be exploitation, stemming from the monopsony power of the village leader, essentially acting as a contractor.
- There may be forgone income, that is, an opportunity cost to the worker from forgone economic activity such as similar work in the casual labor market.

A key factor in determining the benefits to poor people from this scheme is the extent of rationing; do BREGS workers get work on the scheme when needed? Dutta et al. (2012) report all-India survey results indicating substantial unmet demand for work on MGNREGS. This is based on the National Sample Survey (NSS) for 2009/10. With the survey instrument used in this paper (designed specifically for this purpose), Dutta et al. (2014) also find evidence of considerable rationing of BREGS work. Of those demanding work on the scheme, only one-third got that work. The extent of rationing has important implications for the scheme. Given that the BREGS wage rate is above the market wage rate for similar work, a credible prospect of getting a BREGS job would help non-BREGS workers bargain for higher wages. However, such a spillover effect onto the private wage-labor market appears unlikely given the extent of rationing in access to BREGS work. At the average rationing rates found by Dutta et al. (2014), the expected value of BREGS earnings for a casual worker considering a switch to BREGS would be well below the prevailing market wage rate, and this can be presumed to be common knowledge in village labor markets. BREGS would not then be a credible outside option in local wage bargaining. Rationing also undermines the insurance benefits of the scheme, which depend crucially on workers being able to turn to the scheme when needed. Dutta et al. (2014) report survey-based evidence that the scheme is quite unresponsive to shocks in this context.

A second way in which the scheme can reduce poverty is by creating assets of value to poor people (either directly or indirectly, such as through private employment effects). This aspect of MGNREGS has had less attention than the direct employment gains to participants. A widely heard characterization says that the assets are mostly worthless, but this is clearly an exaggeration. Verma (2011) reports field work assessing the returns to 140 water-based projects under MGNREGS. The results suggest that some MGNREGS projects do bring lasting positive benefits beyond the direct employment. However, the sample was purposively selected in favor of the "best-performing" projects and so cannot be used to generalize about the portfolio of projects. There have been many anecdotal observations about the lack of local capabilities for devising and maintaining projects and a lack of interest among local engineers.[6] There have also been anecdotal observations that any

5. For example, local officials may charge a "fee" for their services, such as providing wages in advance or collecting wages against "ghost workers."

6. See, for example, the comments in Verma (2011), Mann and Pande (2012) and Zimmerman (2013).

durable asset creation on the scheme has often favored local landowners and politicians, rather than poor households directly, who are typically landless.

II. Data and Methods

Survey data: For the purpose of this study we collected two rounds of data in a household panel structure from 150 villages spread across rural Bihar. The first round (R1) was implemented between May and July of 2009 and the second (R2) during the same months one year later. These periods were chosen for being lean periods for agricultural work and were thus expected to be peak periods for BREGS. Both rounds included questions with recall over the previous twelve months. The year preceding the first survey saw severe floods during the monsoon (July–August of 2008) in some districts falling in the catchment area of the Kosi river. In contrast, rainfall was scanty during the 2009 monsoons and drought was declared in many districts during the second survey period.

A two-stage sampling design was followed, based on the 2001 Census list of villages. In the first stage, 150 villages were randomly selected from two strata, classified by high and low BREGS coverage based on administrative data for 2008/9. In the second stage, twenty households per village were randomly selected, drawing from three strata based on an initial listing of all village members and a few selected attributes. This stratified approach ensured that the sample included both scheme participants and households with likely participants. We have used the appropriate sample weights to reflect the sampling design.

The surveys collected information on a range of household level characteristics including demographics, socio-economic status (including asset ownership and consumption), employment and wages, political participation, and social networks, as well as information on BREGS participation and process-related issues.

Up to two adult household members, one male and one female, were interviewed about their participation in BREGS, experience of BREGS at the most recent worksite, and knowledge and perceptions of the program, the village labor market and the role of women. In selecting individuals, we favored participants in the scheme. The mean number of adults per sampled household is 2.5 (the median is 2.0) although the range is 1–11. So we are not interviewing all adults in all sampled households, and in some cases there was only one adult available. Our aim was to sample a population of individuals representative of the population of participants. However, the reality is that women are more often at home than men and that it was not always easy to find the individuals we wanted to interview. This was true over time as well, as is reflected in a smaller panel of individuals than of total individuals surveyed in any one round. Close to 69% of interviewed participants were married male heads, while their spouses and unmarried children accounted for another 18% and 8%, respectively. In addition, in each village, key informants were interviewed about physical and social infrastructure in the village and access to government programs.

In total, 3000 households and approximately 5000 individuals were interviewed in both rounds. The balanced panel comprises 2,728 households, containing 3,749 interviewed individuals. The overall attrition rate for households between the two rounds is 8% and is not concentrated in any particular stratum. There were relatively few refusals; two-thirds of the attrition was because a household was away temporarily when the survey team visited the village.

We also initiated qualitative research in purposively selected villages in six districts in north and south Bihar (Gaya, Khaimur, Kishanganj, Muzaffarpur, Purnea, and Saharsa) during February and August 2009.[7] The results of this qualitative work will be used in interpreting some of our quantitative findings.

Estimating forgone earnings: Two approaches to estimating forgone earnings can be identified from the literature. The first is a structural approach, which follows the longstanding practice in economics of modeling observed outcomes as if people hold reasonably well-informed and rational (informationally unbiased) expectations about their gains from participation in such a program. One can then model their participation choices and labor supply decisions and (under certain conditions) retrieve estimates of their derived benefits. The second route makes far fewer assumptions about behavior and instead employs standard "reduced-form" impact evaluation methods. This involves comparing means between those treated and a selected comparison group of nonparticipants. Following one or both approaches, various methods are used to assess impacts under maintained identifying assumptions, including econometric models of time allocation and matching estimators. Estimates of mean forgone income have varied from 25% of workfare earnings (in Maharashtra, India) to 50% (Argentina).[8]

However, a potentially large amount of economically relevant, individual-specific information on forgone opportunities is left unobserved by both of these methods. And this information is clearly known by those deciding whether to participate. This gives rise to what Heckman et al. (2006) term "essential heterogeneity" (also known as "correlated random coefficients"), which they show can confound inferences about even the overall mean impact from standard econometric estimators (including those using randomized assignment as an instrumental variable). Such heterogeneity has long been a concern in the evaluation literature.[9] The problem stems from the evaluator's lack of information about forgone opportunities.

There is a third route to estimating forgone incomes, which we use in this paper.[10] This is a nonparametric method of addressing the heterogeneity problem

7. The qualitative results are reported in Development Alternatives (2009), Indian Grameen Services (2009), and Sunai (2009). Dutta et al. (2013) provide summaries of the findings and of their implications.

8. From Datt and Ravallion (1994) and Jalan and Ravallion (2003), respectively. The result for Argentina was confirmed (using different methods) by Ravallion et al. (2005).

9. Early discussions include Heckman and Robb (1985) and Björklund (1987).

10. We do not know of any prior use of this method. Jha et al. (2012) also asked survey respondents working on MGNREGS whether they thought any other work was available. They did not, however, ask for forgone incomes, but instead used prevailing wages rate for valuation.

by posing counterfactual questions to individual participants. We invoke the key assumption of the structural approach, namely that participants hold un-biased expectations of their income gains from participation. The difference is that we try to retrieve that information from the participants directly. Then we do not need to make any of the standard assumptions of econometric estimators in either the structural or reduced form methods, notably the assumption that the regression error term has mean zero, conditional on either treatment status or some correlate (the instrumental variable) of that status. This method has the advantage that we can estimate mean impacts, including on poverty measures, nonparametrically, assuming nothing more than classical measurement errors in survey responses. Our individual-specific estimates of benefits assume that participants hold unbiased expectations of their gains from the program. Although this is the longstanding assumption of the structural approach it is still questionable. Measurement errors can be expected. We address this concern by taking averages conditional on income in assessing poverty impacts. We also offer observations from data on alternative work options that give some confidence in the validity of the mean forgone incomes implied by our method.

Essentially each sampled participant is asked for their expectation of employment and earnings if they did not have the workfare opportunity at the time. If the respondent said he or she would have worked in the absence of the program then we asked how many days and at what wage. The specific questions (in the local dialect) were fine tuned in the piloting stage. We also ask their actual earnings. So we are essentially asking for individual income gains. Our method has the advantage that we obtain individual-specific impacts, incorporating idiosyncratic information of the available opportunities—information that would be unlikely to be available as data in an observational study. We are thus able to implement quite fine distributional analysis, as required for assessing impacts on poverty.

Virtually the only options BREGS participants have for earning income in this village setting are casual manual work for a local landowner or on some nonagricultural activity. A large share of BREGS participants are landless and have lived in the villages for generations. They can be presumed to have a clear idea of their labor–earning options throughout the agricultural year.

We found that with care in design, response rates were high; these questions did not turn out to be any more difficult than more common "objective" questions. With appropriate training for interviewers, we achieved overall response rates to our questions on forgone income of 92% in the first round of our survey and 98% in the second round.

Not surprisingly, there were some likely outliers, probably reflecting misunderstandings of the survey questions. However (as we will show), the answers make sense in that the mean forgone incomes reported are very close to the average wage rates obtained by similar workers who had not participated. We also provide some robustness tests to the possibility that we have over-estimated forgone incomes. Our main results are robust to even substantial over-estimation of foregone incomes.

As noted in the Introduction, there is a potential for upward bias in our estimates of aggregate forgone earnings due to double counting; two different survey respondents may have in mind the same forgone work opportunity, such that aggregate forgone income will be lower than the sum of the individual reports. There may also be substitution possibilities between family members, whereby one adult takes up the work foregone by a BREGS participant. We will test sensitivity of our results to the possibility that our methodology has led us to over-estimate forgone earnings.

Certain protocols were established for cleaning and analyzing these data:

- For housework or own-enterprise (typically own-farm) work, forgone income was assumed to be zero, on the plausible assumption that such work can be readily re-allocated over time to assure little or no forgone income.
- Forgone work and incomes were asked for each spell of BREGS work, for each individual. The gender-specific median was then used as the household value for household poverty calculations.
- Missing values of forgone income were replaced with the median for the household's stratum in the village or the village median (across all strata) if it was still missing.
- About 10% of respondents reported forgone earnings greater than their earnings from public works. Given that the work involved is similar between public works and other casual labor—both involving manual labor with little obvious no-wage benefit—it is implausible that someone would give up a higher wage job to join the scheme. We think it far more likely that these respondents misunderstood the survey question, or the time units were entered incorrectly. So we treat this as an error. We chose to truncate the data such that forgone income in any period cannot exceed earnings from public works.

In estimating poverty measures we follow standard practice in basing our measures on a comprehensive consumption aggregate (using a survey module based on the NSS Employment-Unemployment Schedule). The poverty line is the median per capita consumption level in R1, and we update this over time using the Consumer Price Index for Agricultural Laborers to get the R2 line. This gives poverty lines of Rs. 6988 per person per year in R1 and Rs. 7836 in R2. However, recognizing that any poverty line is bound to be somewhat arbitrary, we also provide estimates of the poverty impacts over a wide range of potential lines.

It should be noted that using consumption as the welfare metric in this context ignores any difference in the disutility of time spent on BREGS relative to the options. This is standard in the nonwelfarist tradition of program evaluation.[11] The type of work done on BREGS is very similar to all casual manual labor so this is unlikely to be an issue for fully employed workers. For unemployed workers,

11. See, for example, the discussion in Besley and Coate (1992), Datt and Ravallion (1994), and Alik-Lagrange and Ravallion (2015).

however, there will be an added welfare cost of participation on the presumption that the work gives disutility. The implications of this possibility are discussed further in Alik-Lagrange and Ravallion (2015).

In setting the cost of the scheme for the counterfactual analysis, we include all public expenditures attributed to BREGS in the central administrative data. These include materials and supervision as well as BREGS wages; the nonwage costs represented 36% and 39% of total spending on the program in Bihar in R1 and R2, respectively (Dutta et al. 2014). The precise budgets we used were Rs. 858.42 per household in R1 and Rs.1194.92 per household in R2.[12]

The calculation of counterfactual poverty measures is then a simple accounting exercise. The actual (observed) post-BREGS poverty measure is based on the observed distribution of consumption per person $\boldsymbol{y} = (y_1, \ldots, y_n)$ (where y_i is consumption per person for household i). The counterfactual in the absence of BREGS is based instead on the distribution $\boldsymbol{y} - \boldsymbol{w} + \boldsymbol{f}$, where $\boldsymbol{w}$ is the n-vector of actual wages received from BREGS and $\boldsymbol{f}$ is the n-vector of forgone incomes due to taking up that work. The difference between the measures based on these two distributions then gives the impact on poverty. When instead the counterfactual is the basic income scheme, the poverty measure is based on the distribution $\boldsymbol{y} - \boldsymbol{w} + \boldsymbol{f} + c$, where c is the cost of the scheme per person (wage plus nonwage costs). (This can be scaled down to allow for leakage.) For the counterfactual based on the current assignment of ration cards the relevant distribution is instead $\boldsymbol{y} - \boldsymbol{w} + \boldsymbol{f} + (c/p)\ \boldsymbol{r}$, where $\boldsymbol{r} = (r_1, \ldots, r_{\mathrm{n}})$ denotes the assignment of ration cards ($r_i = 1$ if i has a BPL card and $r_i = 0$ if not) and p is the proportion of households with a ration card.

It should also be noted that the post-BREGS distribution potentially reflects any general equilibrium effects of the scheme. Although we expect these to be small given the considerable rationing evidenced in Bihar (section 2), such effects could vary according to the design and delivery of cash transfers. Our calculations of counterfactual poverty rates assume that such effects are the same across the schemes.

III. Wages

There are a number of ways in which BREGS implementation differs from the formal guidelines, with bearing on the wages received and (hence) poverty impacts. During the survey period, the notified BREGS wage started at 89 rupees per day, rising to 102 in June 2009, and to 114 in May 2010. Productivity norms are stipulated such that an able-bodied worker can earn these notified wages. Scheme functionaries are required to ensure that these norms are being met by measuring the work done at the worksite. Yet, nearly half of the workers interviewed reported seeing no one measuring work at their most recent worksite. When measurement

12. The administrative data indicate total expenditures of Rs 13,058 million in FY 2008–09 and Rs 18,177 million for FY 2009–10 (Dutta et al. 2014, Chapter 1). These were divided by our count of 15.2 million households in rural Bihar, as implied by our survey weights. Recent Census projections gave slightly higher counts, but it is better to use our survey-based numbers for internal consistency.

was done, among those aware of the process, the majority (83% of the male and 75% of the female workers interviewed) reported equal payments to all workers at the site. When we asked Mukhiyas (elected village leaders) why workers reported not being paid the stipulated BREGS wage, they typically responded that workers had not performed to the productivity norms and hence were not owed the stipulated wage. They also told us that workers often did not understand this.

Since April 2008, in an effort to promote financial inclusion and transparency in payments, all wages are supposed to be paid through beneficiary bank or post office accounts rather than as direct cash payments. Adherence to this practice is unclear, based on our surveys and field work. Officials report that the majority of wage payments are made through bank or post office accounts. But workers report that they often receive cash at the worksite. In 2010, more than half of the workers interviewed—52% of women and 56% of men—reported receiving wages in cash from the Mukhiya, the contractor, the mate, or another official at the most recent worksite at which they worked. In R1 the percentages were 78% and 64% for female and male workers, respectively. Thus, while there is evidence of a decline over time, the share of total workers getting pay at worksites remains high. In some cases this may reflect partial cash payments to workers by the Mukhiya while funds are being transferred to worker accounts.

The Mukhiya, his/her spouse, or a close family member often acts as a money lender as well as a contractor, making advance payments to workers.[13] The reasons given include delays in work measurement, delays in obtaining post office/bank accounts, and delays in the flow of funds to the post office or bank accounts.[14] In principle, the scheme should move towards full reliance on formal accounts rather than cash. But at the time of our survey, practice was still a long way from that ideal. Weaknesses in the flow of funds or the administration of accounts, leading to delays in payment, create scope for intermediaries to profit by being able to provide advance cash payments to needy workers.[15] Fieldwork for this study indicated that local officials also take a cut from the wages due to workers, possibly in the process of making advance payments.

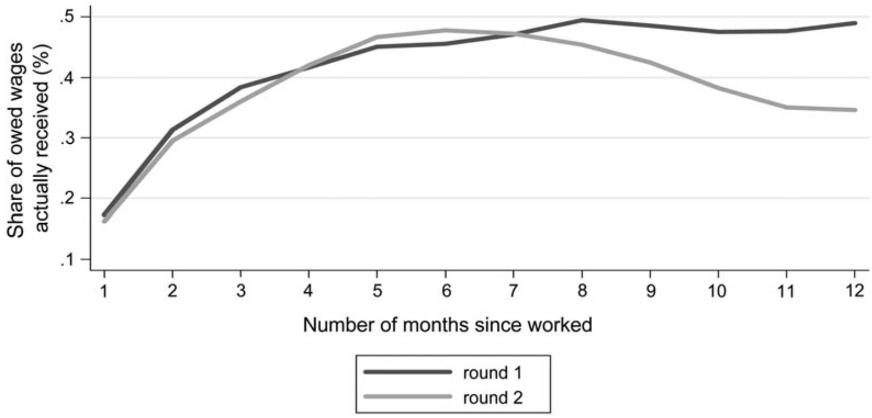
There have been qualitative reports from the field of partial payments and long delays in receiving wages. Our survey provides corroboration. We asked whether BREGS participants had received wages owed in full. 72% of our households' female participants in R1 and 67% in R2 had been paid in full by the time of our survey, while this was the case for 66% and 72% of participant men. Women who had not received their due were still owed 58% and 75% of wages on average in R1 and R2, respectively. Unpaid men were still waiting for 64%

13. As in other states, one third of elected positions are reserved for women in Bihar. Thus, elected Mukhiyas are sometimes female. However, once the election is over, a male surrogate, frequently a husband, often plays an active role in the job. When we refer to the Mukhiya we mean either the elected or surrogate one.

14. Also see the discussion in Khera (2011).

15. This is not confined to Bihar; Vanaik (2009) reports the same practice in Rajasthan—thought to be among the better performing states in implementing MGNREGS.

FIGURE 1. Delays in BREGS Wage Payments



Notes: Sample restricted to participants who have not been fully paid BREGS wages owed to them.
Source: Authors' estimates based on BREGS Survey.

and 57% of their earned wages. The amount owed is likely to decline with time since participation increases. Our data confirm that the share of wages received is higher for participants for whom more time has elapsed since they participated in BREGS. This can be seen in figure 1, plotting the share of wages owed that were actually received by months since the work was completed for the sample of participants who have not received their full wages. The mean share of wages paid rises with time up to six months, then stabilizes (R1) or falls (R2). However, even at its peak, the share of wages received among those receiving less than they felt they were due is no more than 50%.

Advocates have shared a hope that the scheme would reduce the exploitation of rural workers stemming from the labor-market power of large farmers or contractors. This might well have been a tall order given that the local leaders in charge of implementing the scheme often overlap with the set of people who are the employers. For example, the Mukhiya, acting as a contractor directly or via a close ally, can maintain similarly exploitative relations in implementing the scheme. The scheme officially bans contractors, but they are common, with half or more of the responding workers reporting that contractors were present at worksites.

Wages Received by Households

The BREGS survey obtains wages from two sources. The household questionnaire asks about earnings and days of work in the week preceding the survey for each adult household member.[16] It differentiates between public works (PW) and other

16. Here we adapted the standard weekly module in the NSS Employment-Unemployment (Schedule 10) surveys.

TABLE 1. Daily Wage Rate in Rupees for the Week before Interview

		Mean	St.dev.	Median	N
Round 1					
All	Public works	82.7	27.4	89.0	54
	Other casual labor	72.2	31.2	70.0	1031
Men	Public works	85.8	25.2	89.0	41
	Other casual labor	79.1	29.2	80.0	815
Women	Public works	73.0	32.6	80.0	13
	Other casual labor	46.0	23.6	45.0	216
Round 2					
All	Public works	94.8	22.3	100.0	118
	Other casual labor	87.2	42.0	100.0	796
Men	Public works	99.4	18.2	100.0	75
	Other casual labor	97.9	39.3	100.0	574
Women	Public works	86.8	26.6	100.0	43
	Other casual labor	59.4	35.2	50.0	222

Notes: Based on Block 23. We treat wages over 200 or less than ten Rupees per day as missing values. Wage data are in nominal terms and reported as unweighted means and medians from the sample.
Source: Authors' estimates based on BREGS Survey.

casual wage work but does not differentiate between BREGS and other PW employment.[17] Wage earnings include cash payments and the value of in-kind payments. In addition, for each work activity, the questionnaire records information separately on wages owed and wages already received. We expect one-week recall to be excellent but the data have the disadvantage that they provide relatively few observations. Since the survey was fielded in the May–July months, wage information from this block pertains only to these months.

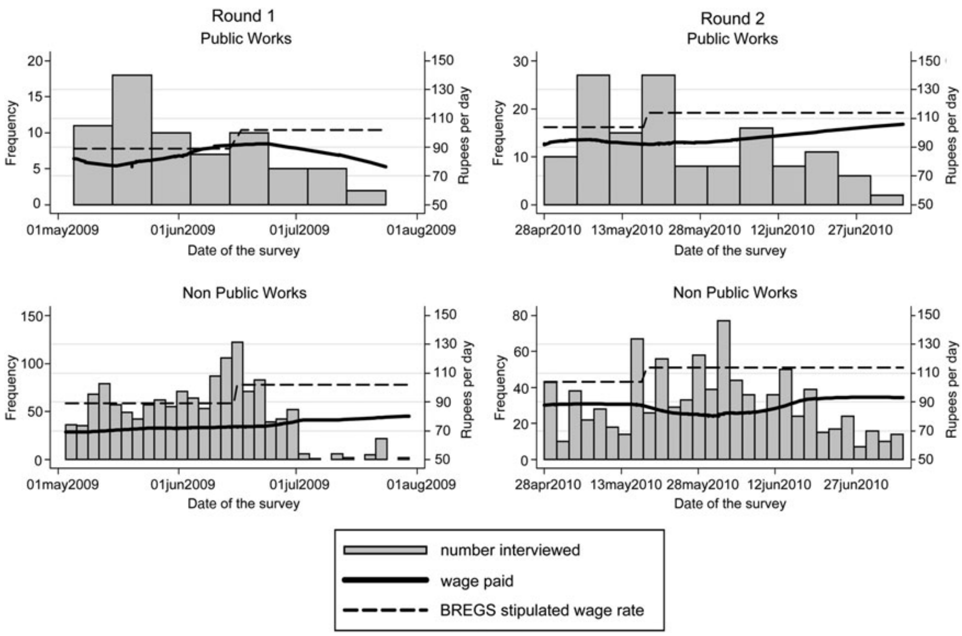
The second source of information about wages is from the individual level questionnaire, which asked (up to) one male and one female adult in each household about their involvement in public works specifically, including type (whether BREGS or other), days worked, wages owed, and wages received separately for each episode of public works employment over the last year. These are the data already referred to with respect to delays in wages paid. In addition to providing details specifically about BREGS, this source gives many more observations than the household questionnaire. The drawbacks are that there may be mismeasurement due to the long recall period and that this does not give wages for non-PW work.[18] Given the different pros and cons of each source, we make use of both.

Table 1 reports summary statistics on casual wages for the week prior to the survey. In R1 (2009), median PW wages were nearly 30% higher than the casual wage. Wages are higher for men than for women in both segments of the labor market, but for women, PW pays much better than the private sector. Between

17. Other public works employment could include BREGS, road building projects, or other public works schemes run by the state government.

18. To reduce sensitivity to measurement errors we treat the (very few) recorded wages over 200 Rupees per day or under ten Rupees from both sources as missing values.

FIGURE 2. Wages over Time in the Study Villages



Notes: The figure reports data on wages owed, based on questions about casual labor done in the week preceding the survey. Wages are nominal.
Source: Authors' estimates based on BREGS Survey.

2009 and 2010, average PW wages maintained value in real terms (increasing by 14.6% in nominal terms, compared to a 12% rate of inflation),[19] but the gap between public works and labor market wages narrowed as mean casual wages rose by 21%, and median casual wages rose by 43%. Women, however, still earned significantly higher wages under BREGS in R2 than in the casual labor market ($t = 3.91$ in R1 and $t = 4.85$ in R2).

Figure 2 and table 2 take a closer look at wages for casual labor in relation to the stipulated BREGS wage, and their evolution over time. It can be seen that wages owed, as reported by participants, are lower than the stipulated BREGS wage rate (top panels of figure 2). Summary statistics and tests reported in table 2 show that on average, workers received Rs.10 less per day than the stipulated wage, for much of the recall period.[20] Note that this gap is not due to payment delays, as these are total wages *owed* to the individual, not the amount actually received by the time of the survey interview.

Turning to the second source of data on wages earned on BREGS from our survey, figure 3 plots the mean wage rate by month for both men and women based on the household questionnaire. The figure also gives total days of work and

19. The inflation rate is based on the consumer price index for agricultural laborers in the state.
20. Neihaus and Sukhtankar (2013) find similar discrepancies in the state of Orissa.

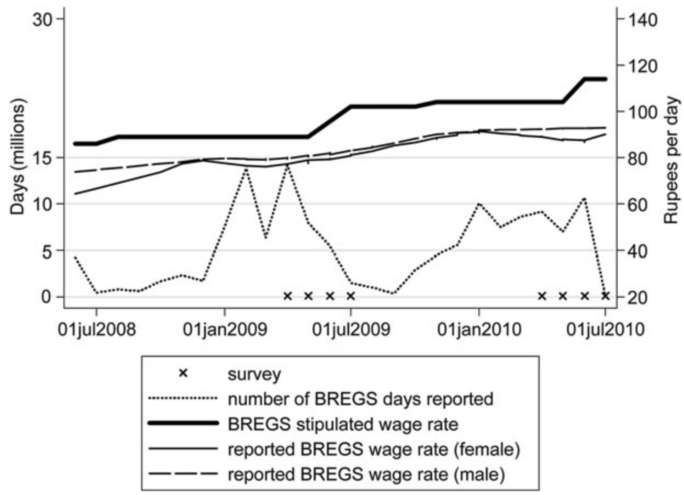
TABLE 2. Average Wage Rates before and after Increases in the BREGS Stipulated Wage

		Round 1			Round 2		
		Public works wage	Non-PW wage	Stipulated BREGS wage	Public works wage	Non-PW wage	Stipulated BREGS wage
Before increase of stipulated wage	mean	80.7	71.4	89.0	93.8	91.5	104.0
	s.d.	20.4	30.6	.	24.8	39.8	.
	N	46	946	.	54	226	.
t-test: Ho: mean actual		−2.8	−17.8	.	−3.0	−4.7	.
wage = stip. wage (prob.)		(0.008)	(0.000)	.	(0.004)	(0.000)	.
After increase of stipulated wage	mean	92.2	74.7	102.0	95.5	83.9	114.0
	s.d.	32.8	30.1	.	24.3	39.2	.
	N	20	377	.	81.0	649.0	.
t-test: Ho: mean actual		−1.3	−17.6	.	−6.9	−19.6	.
wage = stip. wage (prob.)		(0.199)	(0.000)	.	(0.000)	(0.000)	.

Notes: The wage was increased to Rs 102 on June 16, 2009 in R1, then to Rs 104, and to Rs 114 on May 18, 2010 in R2. T-tests use only daily wages greater than ten and lower than 200 Rupees/day. Wage data are unweighted.

Source: Authors' estimates based on BREGS Survey.

FIGURE 3. Evolution of BREGS Wages and Days Worked over the Entire Survey Period



Notes: Based on individual questionnaires with recall over last year. Wage data reported are household-weighted wages owed to the worker.

Source: Authors' estimates based on BREGS Survey.

TABLE 3. Daily Wages for Farm and Nonfarm Casual Labor in Bihar from 2009/10 NSS, by Sub-round

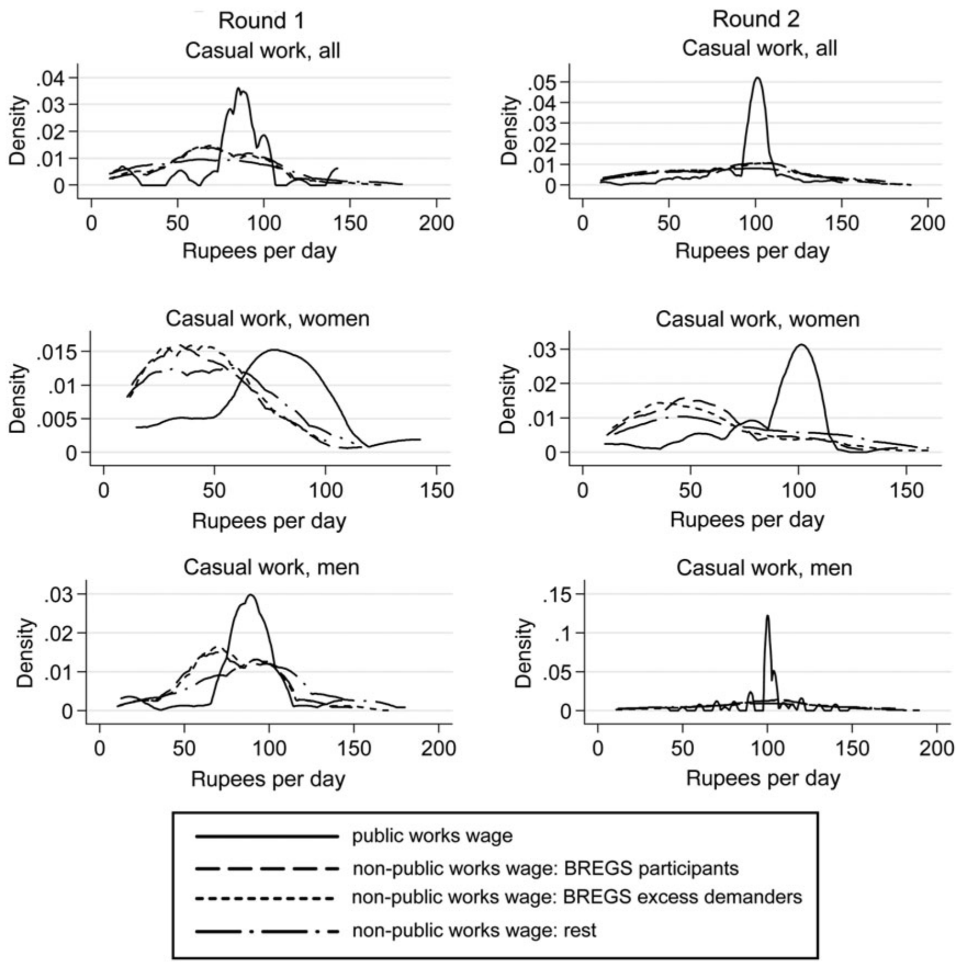
	Agricultural work				Manual non-agricultural	
	All operations		Cultivation			
Sub-Round	Mean	Median	Mean	Median	Mean	Median
1 July–Sept 2009	74.8	77.5	72.4	73.3	86.9	80.0
2 Oct–Dec 2009	72.8	80.0	67.6	70.0	103.4	95.0
3 Jan–Mar 2010	84.6	80.0	80.2	80.0	97.1	90.0
4 Apr–June 2010	86.0	81.4	78.9	80.0	120.7	100.0
Total	79.0	80.0	74.4	76.7	100.2	90.0
Percentage change 1–4	15	5	9	9	39	25

Source: Authors' calculations from NSS 66th round.

identifies the survey periods. There is a marked seasonality in days of employment. As before, we see a persistent gap between the stipulated BREGS wage rate and the wage actually reported. The absolute gap is roughly unchanged over time. There is some sign of convergence in male and female wages, but this is possibly deceptive, given that there were very few observations in the early months when the female wage was lower. The longer recall periods required by this source of wage data also raise doubts about the early data points.

A third source of wage data is the NSS for 2009/10. Table 3 reports mean and median wage rates, spanning the period between R1 and R2 of the BREGS survey.

FIGURE 4. Density of Daily Casual Wages by BREGS Participation Status



Notes: Based on questions about casual work done in the last week.
Source: Authors' estimates based on BREGS Survey.

Here too we see an increase in agricultural wages, notably between sub-rounds 2 and 3, corresponding to the last quarter of 2009 and first quarter of 2010. BREGS activity picks up in the first quarter of the year, so this agricultural wage increase does coincide with BREGS. However, also note that there was an even steeper increase in manual nonagricultural wages over the year.

How do the wages compare? Figure 4 provides the density functions for daily casual work wages (in the week preceding the survey) for public works (PW) and three comparators: (i) the non-PW wages for BREGS participants; (ii) the non-PW wages for the excess demanders (those who wanted but did not get work on BREGS), and (iii) the non-PW wages of all others. Wage rates were calculated by taking total wage earnings by type of work in the week prior to the interview and dividing by the total number of days of such work reported.

Two points are worth noting. First, as already discussed, PW wages are higher than other casual wages earned by BREGS participants, for both men and women. We can reject the null hypothesis of equality between the PW wage and the non-PW distributions for both men and women in R1 (probability less than 0.0005 in both cases). This is true for other comparator groups as well: the people who said they wanted work on BREGS but did not get it (the excess demanders) were typically earning less than those working on PW.

Second, the difference between BREGS wages and other casual wages does not appear to be due to different abilities of the workers. It could be possible that piece work schedules such as used by BREGS reward physically stronger workers. However, this does not appear to be the explanation, since we also see that BREGS participants were earning significantly less in non-PW work than in PW. In fact there is no statistically significant difference between the wage distributions of the three comparators for either women or men.[21]

And there is essentially no difference between the wage distribution for the "excess demanders" and the non-PW wage distribution of those who also do PW. Those who get the jobs on PW are essentially drawn from the same wage distribution as those who do not get that work, but want it. This is again suggestive of unmet demand for work stemming from rationing in the assignment of jobs.

Impact on Wages

If BREGS provided an unconditional guarantee of work at a wage at or above the wage for alternative work, then the BREGS wage rate would become binding on the casual (farm and nonfarm) labor market. Nobody would be willing to work at less than the BREGS wage rate. There may be lags in the adjustment process, but we would expect to see casual wages catching up to BREGS wages. If so, then this would greatly enhance the scheme's impact on poverty.

There are a priori reasons to doubt whether BREGS is putting upward pressure on wages in the casual labor market. The guarantee is only conditional, up to one hundred days, and in practice, this is confined largely to the lean season, when there is less likelihood of a spillover effect on agricultural wages.[22] But, probably more importantly, as is shown in Dutta et al. (2014) for Bihar (and in Dutta et al., 2012, using NSS data for all of India), there is substantial unmet demand for work on the scheme even among those with less than one hundred

21. The Kolmogorov-Smirnov (KS) test does not reject the null hypothesis that the distributions are identical in the three binary comparisons between the three comparison wage distributions for either men or women.

22. A few papers examine the impact of MGNREGS on wages with mixed results. Zimmerman (2013) finds little evidence of any labor market effects. In contrast, Berg et al. (2012) and Imbert and Papp (2013) find positive effects on agricultural and private sector wages, respectively, but only in states where implementation is of high quality and intensity and hence where the demand for work is more likely to be met. Azam (2012) finds wage impacts but only for women.

TABLE 4. Total Person Days of Non-PW Work by Operation and Sub-round 2009/10

Sub-round	Manual agricultural	Manual non-agri	Non-manual non-agri	Missing operation code	All operations	Household own farm enterprise
1 July–Sept 2009	1340.0	440.0	150.0	21.0	1951.0	2354.0
2 Oct–Dec 2009	1284.5	473.0	324.0	32.0	2120.5	2406.0
3 Jan–Mar 2010	973.0	577.0	235.0	7.0	1792.0	2728.0
4 Apr–June 2010	1035.0	399.5	297.0	7.0	1759.5	2280.5
Total	4632.5	1889.5	1006.0	67.0	7623.0	9768.5

Notes: Public works excluded. The total for all operations includes some minor omitted categories.

Source: Authors' calculations from NSS 66th round.

days of work. The option value of BREGS in wage bargaining in the casual labor market depends critically on employers believing that the scheme is available. That might not require a strict guarantee of employment under BREGS; the scheme might still help workers bargain up their non-BREGS wages as long as there is a reasonably good chance of obtaining BREGS work. However, as noted in section 2, it is hard to believe that this would be the case with the degree of rationing in BREGS jobs that we observe in our data.

Our comparisons of the average wage gaps in R1 and R2 suggest that market wages started catching up with PW wages in R2, consistent with labor market tightening, possibly *due* to the scheme. On the other hand, non-PW wages do not respond in a predictable pattern to changes in the BREGS stipulated wage. In R1, other (non-PW) wages did not rise after the BREGS wage was increased; and in R2, non-PW wages actually fell even as the BREGS wage was raised during the reporting period. (Note that these are nominal wage rates. So the trend increase in wages reflects in part inflation.)

Wage trends do show that the gap between non-PW and PW wages closed, more so for men, even though women began with a larger gap between the two wages in R1.

Could the tightening of the agricultural labor market have come instead from expanding nonfarm opportunities other than BREGS? Table 4 reports the number of person-days in all types of non-PW operations from the Bihar sample of the 2009/10 NSS. We see substitution between casual labor (on someone else's farm) and own-farm work between sub-rounds 2 and 3, while the total amount of agricultural work remained roughly constant. What increased between these two sub-rounds was the amount of manual nonfarm work. The rising availability of this work could well be driving up the agricultural wage rate in this period, rather than BREGS.

Our survey respondents did not believe that improvements in wages or employment opportunities were connected to BREGS (see Dutta et al. 2014). Workers can be expected to know whether BREGS is enhancing their bargaining power in the labor market, but they don't think so overall. Also, recall that the gap between public and nonpublic works wages was much less affected for women than for men over the period. The fact that men are generally more likely than women to be engaged in casual off-farm work gives added weight to our interpretation.

There may well be larger impacts on wages in states of India where there is less rationing. As Dutta et al. (2012) show, there is far more rationing in some states (including Bihar) than in others. The scheme may be having larger impacts on private sector wages in states with less rationing. Imbert and Papp (2013) and Berg et al. (2012) present evidence that in states with more effective implementation, the scheme has had more impact on casual wages.

IV. Forgone Earnings

Recall that to measure forgone earnings we asked counterfactual questions of BREGS participants in our surveys, to obtain their assessment of how many days they would have otherwise worked and what they think they would have earned if they had not been doing BREGS work during that period (section 2).

We found that forgone opportunities varied considerably across workers. Table 5 summarizes the types of activities that BREGS-participants identified as being displaced by their BREGS work. For men, about 14% in R2 (less in R1) said they would have migrated if not for BREGS; this was only true of 1% of women in R2. Casual work in agriculture was identified as the forgone work opportunity for about 22% of men and 25% of women in R1. Casual nonfarm work was more important for men than for women. In R1, 44% of men and 36% of women said they would have been unemployed in the absence of the scheme; in R2 the percentages were 38% and 13%. Work on own land or in the house was the most common answer given by women.

Table 5. Forgone Work Opportunities Identified by BREGS Participants

% BREGS participants who say that, if not for BREGS, they would have . . .	Round 1		Round 2	
	Men	Women	Men	Women
Migrated for work	10.7	2.9	14.0	1.0
Type of paid work:				
Casual work (agricultural)	22.4	24.8	19.4	23.6
Casual work (nonagricultural)	23.8	5.7	25.7	5.0
Other	1.0	0.8	2.1	0.0
Searched for work or remained unemployed	44.0	35.7	37.6	13.2
Worked on own land or house	8.8	33.1	14.9	58.2
	100.0	100.0	100.0	100.0

Notes: The questions were asked of BREGS participants by work episode. The means are formed over all work episodes such that an individual who worked more than once is also counted more than once. Would-be migrants are included in totals according to the type of activities they would have done.

Source: Authors' estimates based on BREGS Survey.

TABLE 6. Mean Reported Forgone Wages for Casual Work Compared to Reported Actual Wages for those Working in the Same Month

		Reported forgone wage for those working on BREGS (Rs/day)				Casual wage earnings for those not working on BREGS (Rs/day)			
		Mean	Median	SD	N	Mean	Median	SD	N
All individuals									
R1	May 09	67	70	21	49	71	70	30	418
	June 09	68	70	17	32	72	70	30	571
R2	May 10	85	85	33	45	87	100	38	361
	June 10	81	100	27	31	83	90	41	337
Men only									
R1	May 09	71	70	20	38	78	75	29	325
	June 09	68	70	17	32	79	80	27	454
R2	May 10	96	100	29	29	96	100	36	260
	June 10	90	100	23	18	95	100	37	240

Notes: The right-hand panel is based on responses to the wages actually received by casual workers in the week preceding the survey for all non-BREGS work (this could include other public works).

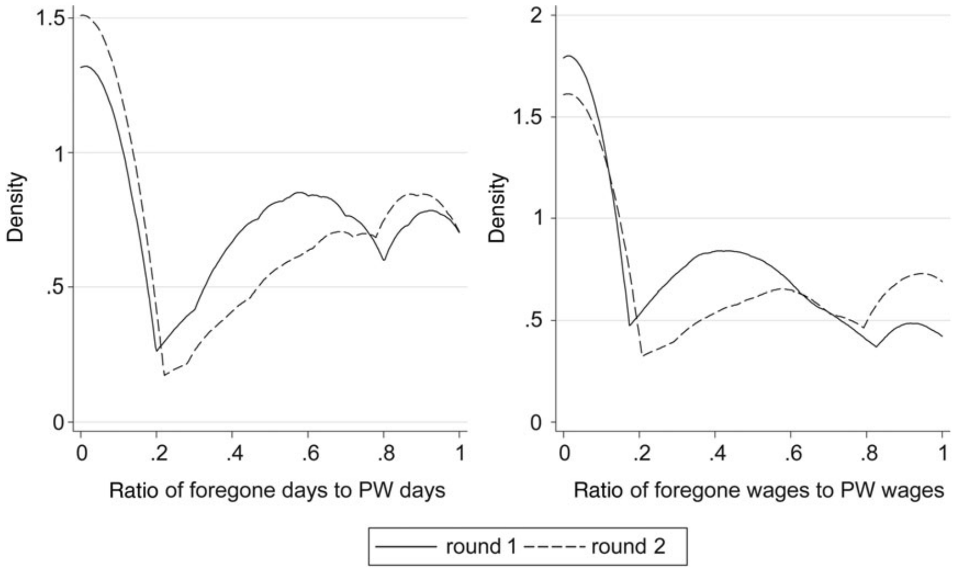
Source: Authors' estimates based on BREGS Survey.

Recall that the unemployment rates for rural Bihar in 2009/10 were 16% for men and 32% for women, with an overall rate of 18%. So the unemployment rates expected by our BREGS participants in table 5 are appreciably higher than these numbers, especially for men. Male BREGS participants appear to be self-selected workers with well above average unemployment rates, although this is less evident for women.

The survey design allowed us a test of the reliability of reported forgone incomes for the main relevant activity, namely casual work. We compare the reported month-specific forgone wage for casual work with the mean wage earnings actually received by casual workers (in the week before the survey) for that same month. Sample sizes entail that the test is only feasible for May and June of 2009 and 2010. Table 6 presents the results. Mean reported forgone earnings are lower, but the difference is small. Overall we find a fairly close correspondence, and more so in R2 than R1 suggesting that there may have been some learning. (Recall also that the response rate was higher in R2.) The variance is greater for actual than for counterfactual wages. These results give us greater confidence that the counterfactual questions were understood and that the answers are sensible.

On average, workers had to give up work days equivalent to 40–45% of the total BREGS employment received. While BREGS provided the sampled households with 18,900 person-days of employment in R1, we calculate that 7,700 days of other employment were given up to take on this BREGS work. In R2, 20,400 person-days of employment were provided, but 9,300 days had to be given up. Forgone employment is higher for men. In R1 the share of gross employment that was accounted for by forgone work was 0.42 for men, versus 0.36 for women. In R2, the corresponding ratios were 0.51 and 0.31.

FIGURE 5. Distribution of the Ratio of Self-Assessed Forgone Days to PW Days and Ratio of Forgone Wages to PW Wages



Note: These distributions are estimated for all public works at the household level.
Source: Authors' estimates based on BREGS Survey.

There are three distinct types of participants. The density functions in the left-hand side panel of figure 5 have three distinct modes. One is around zero, which is the overall mode. These participants would have not had any days of work had they not worked on BREGS. A second mode is around 0.6 and the third and smallest mode is about 0.9.

The mean ratio of forgone income to PW wages is 0.35 (st.dev. = 0.344; N = 930) in R1, rising to 0.39 (st.dev. = 0.392; N = 774) in R2.[23] The corresponding medians are 0.30 and 0.31. Density functions of the ratio of forgone income to public works wages (right hand panel in figure 5) also have three distinct modes. As with days, one is around zero and is the overall mode. This represents the BREGS participants who stated that they would not have been earning income had they not worked on the scheme. A second mode is around 0.5, where participants would have earned about half of the earnings on public works, and the third and smallest mode is about 0.9. The latter beneficiaries would have earned close to the equivalent amount but possibly have had to migrate and bear costs to do so. As noted in section 2, there may also be unobserved nonpecuniary

23. The corresponding means without truncation are 0.63 (st.dev. = 2.31) and 0.63 (st.dev. = 2.73). However, these means are distorted by some very large outliers (reaching a forgone income of sixty-eight times actual wage receipts from PW) that are clearly measurement errors.

benefits to work on BREGS that make it more desirable than alternative equally remunerated casual work.

In summary, these observations suggest that forgone income is significant, though falling well short of that implied by assuming that the opportunity cost of labor on the scheme is the casual market wage rate. There are three distinct groups of workfare participants: those for whom there is no likely income loss from joining the program, those for whom there is only a small net income gain from joining the program, and an intermediate group for whom around half of the BREGS wage represents a net income gain.

V. Impacts on Poverty

In estimating the impacts on poverty we use the household-specific reports of forgone earnings for men and women from the last section. The post-BREGS distribution of consumption is that observed in the data. The pre-BREGS distribution is derived from the post-BREGS distribution by subtracting the net earnings gains from public works employment, as given by gross wages less the estimated forgone income. Table 7 summarizes the various simulations of the impacts on poverty as discussed in detail below.

We estimate that the poverty rates (proportion of the population of Bihar living below the poverty line) among BREGS participants would have been 62.2% and 52.6% in R1 and R2, respectively, without the program. By contrast, what we observe in the data (including, of course, net earnings from the scheme) are corresponding poverty rates of 56.8% and 50.2%. Thus, we estimate that the extra earnings from the scheme reduced poverty among participants by 5.4 percentage points in R1 and 2.4 percentage points in R2.

The upper panel of figure 6 gives the observed (post-BREGS) cumulative distribution function and the estimated counterfactual distribution of consumption in R1 for BREGS participants only.[24] The difference between the distribution functions is plotted in the lower panel. So the lower panel plots the impact on the poverty rate at a given poverty line. We will call this the "poverty impact graph." The peak reduction in the poverty rate is (coincidentally) near the R1 median expenditure per person. At about one-third of the median, poverty falls by about one percentage point, and at one third above, it reduces by over three points. (Naturally, impacts go to zero at the extremes.)

Of course, the average impact is lower for the population of rural Bihar as a whole. We find that, without the program, the poverty rate would have been 51.4% and 42.3% for R1 and R2, respectively. The estimated post-program poverty rates are 50.0% (by construction) and 41.8%, respectively. So we conclude that the scheme reduced the poverty rate in rural Bihar as a whole by 1.4 percentage points in R1 and 0.5 points in R2.

24. Note that estimates in table 7 do not accord precisely with the figures as a consequence of the smoothing used in creating the figures.

TABLE 7. Summary of Estimated Poverty Impacts

	Round 1		Round 2	
	% below poverty line			
	Participants only	Population	Participants only	Population
Pre-intervention				
Deducting gross earnings gains from BREGS	63.0	51.7	56.4	43.2
Deducting net earnings gains from BREGS	62.2	51.4	52.6	42.3
Post-intervention				
Observed with BREGS	56.8	50.0	50.2	41.8
Basic-income scheme (BIS)	60.8	49.5	50.9	39.1
BIS with 20% leakage	61.1	49.8	51.4	39.9
Transfers based on ration cards	60.5	49.8	50.1	40.0
Ration card transfers with 20% leakage	60.8	50.0	50.9	40.3

Notes: The poverty line is set at the median for R1 as observed (post-intervention). Pre-intervention poverty rates in the first row, estimated by deducting gross earnings gains are equivalent to assuming zero foregone incomes for all BREGS participants.

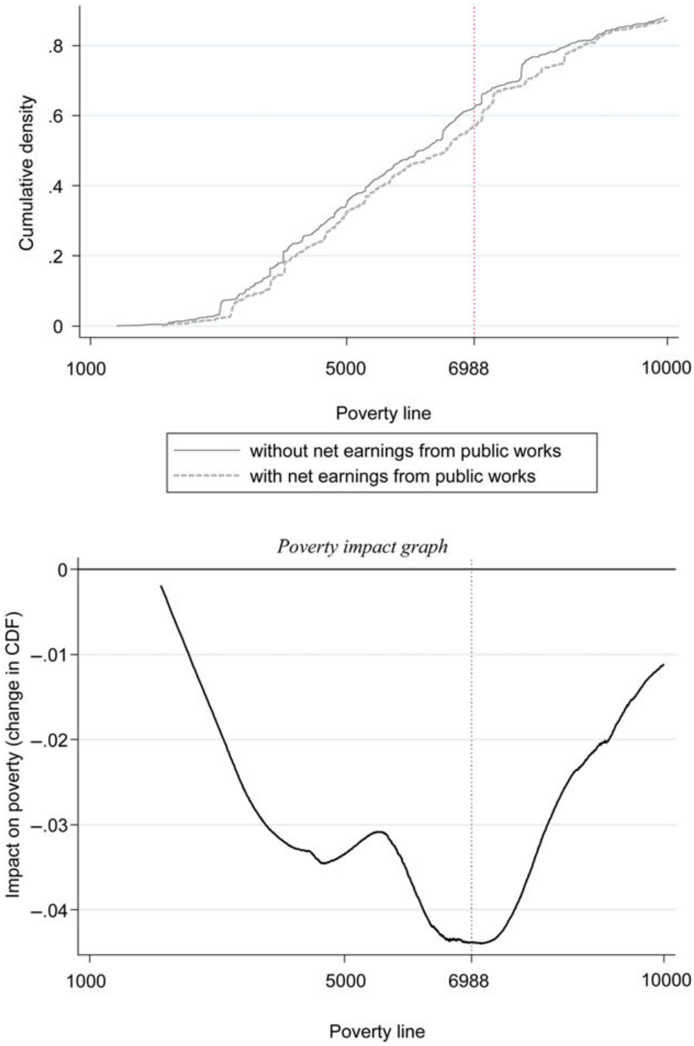
Source: Authors' estimates based on the BREGS survey.

Figure 7 reproduces the poverty impact graph from the lower panel of figure 6 but now compares it to the corresponding graph for the population as a whole for R1 as well as R2. In the entire sample the impact on poverty in R1 peaks near the median but falls off quickly on either side. In R2, the impacts on poverty peak just above the median.

As a sensitivity test, we re-estimated the poverty impacts at lower forgone incomes, given the possibility that some respondents are identifying the same forgone work opportunity. At 50% lower forgone income (across the board) poverty impacts of the scheme are greater (as one would expect), but the difference is not large. In R1 the impact among participants at the median rises by less than one point. The extra impact is even lower in R2, but larger impacts—an extra 1–2 percentage points—are found within the region from one third below the median to one third above.[25] Naturally, eliminating all forgone income gives even higher poverty impacts, as shown in table 7, the top row of which gives the estimated pre-intervention poverty rate if forgone income is ignored (so that gross earnings are netted out). Instead of an impact of BREGS earnings resulting in a 1.4% point drop in the poverty rate in R1 (allowing for forgone income), we would have obtained an estimate of 1.7% points. The difference is greater in R2,

25. Assuming that true forgone incomes were half the levels reported in the survey, we find that the poverty rate for R1 among PW participants fell by 6.0 percentage points due to BREGS—as compared to 5.4% with full forgone income, as indicated by the respondents. For the rural population as a whole, the R1 poverty rate fell by 1.6% points, as compared to 1.4% with full forgone income. Differences in poverty impacts at lower forgone incomes as compared to full forgone incomes are similarly small in R2.

FIGURE 6. Impacts on Participants' Poverty in R1



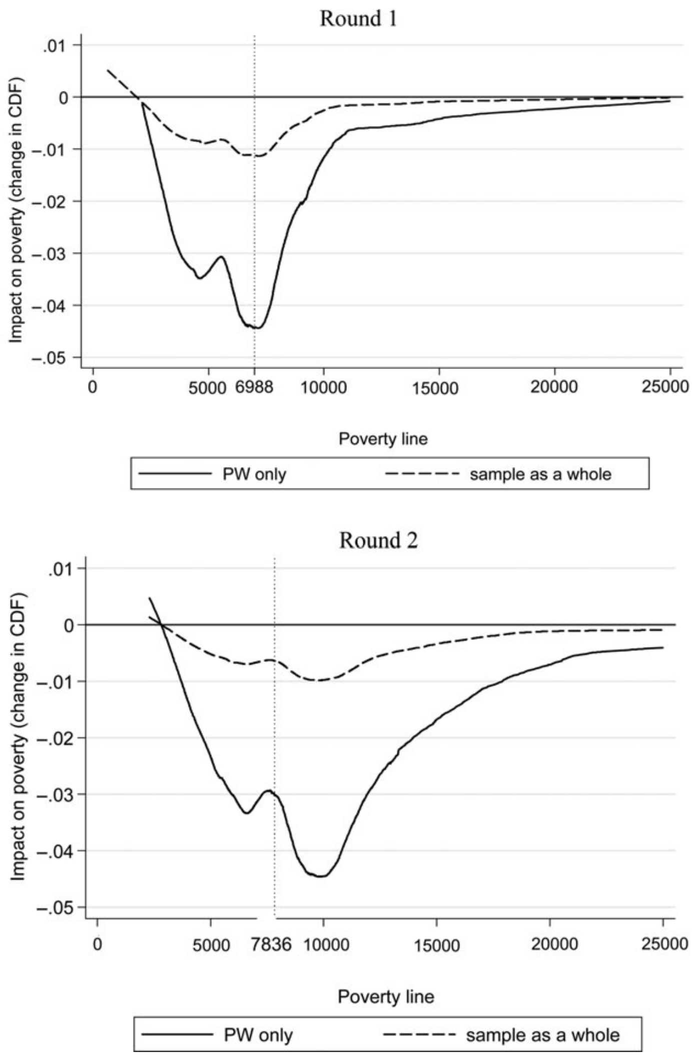
Notes: A smoothing parameter of 0.3 is used for all figures in this paper. The poverty impact graph is estimated as the difference between the cumulative distribution functions of consumption with and without the program.

Source: Authors' estimates based on BREGS Survey.

for which the poverty impact would have been 1.4% points based on gross earnings, as compared to 0.5% points when factoring in forgone earnings.

The above calculations assess impact relative to the absence of the program as the counterfactual state. It is of greater interest to assess impact relative to a feasible alternative allocation of the same total expenditure. The simplest alternative is to give every household (whether poor or not) their share of the same sum of money as was spent on BREGS—a BIS, as discussed in the introduction. Note (again) that

FIGURE 7. Poverty Impact Graph for Both Participants and the Whole Population



Source: Authors' estimates based on BREGS Survey.

this comparison does not take account of the asset creation under BREGS (or other potential nonpecuniary benefits). The question posed here is whether the wage earnings gains alone can justify the scheme as a cost-effective means of reducing poverty.

Using the same methods described above, we estimate that such a BIS would reduce the poverty rate from 62.2% to 60.8% among BREGS participants in R1, and from 52.6% to 50.9% in R2. Both resulting poverty rates are higher than under BREGS. Of course, for a BIS the more relevant comparison is for the population as a whole. We find that a BIS would reduce the overall poverty rate from

51.4% to 49.5% in R1, while in R2 it would reduce it from 42.3% to 39.1%. In this case, both poverty rates are lower than under BREGS (table 7).

This simulation assumes that there is no leakage of money from the budget for the transfers, either due to administrative costs or corruption. Dutta et al. (2014) find that the grossed–up survey-based wages for BREGS account for roughly 80% of the wages reported in the administrative data. This gap could reflect a number of factors, including leakage. If we assume that it is only leakage and that the BIS transfer scheme incurs the same 20% leakage then the poverty rates fall to 49.8% and 39.9% in R1 and R2, respectively, for the population as a whole—still an improvement over BREGS.[26]

Another simulation of interest is to once again take the same budget but instead give it out equally (in cash or kind) to all households holding a BPL ration card. Of course this is feasible since the BPL cards are already issued by the government, and there is an existing infrastructure of ration shops for the subsidized food provided under this policy. We assume that the government only knows who has a BPL card; it does not know who is really poor and how poor they are. So everyone with a BPL card gets the same amount under the counterfactual, and those without the card get nothing.

Our calculations for this alternative counterfactual indicate that it would reduce the poverty rate from 51.4% to 49.8% in R1, while in R2 it would reduce the poverty rate from 42.3% to 40.0%. In both cases the poverty rates are lower than under BREGS. If we make a 20% allowance for leakage (as defined for the BIS) in the transfers, then the poverty rates fall to 50.0% and 41.8% in R1 and R2, respectively.

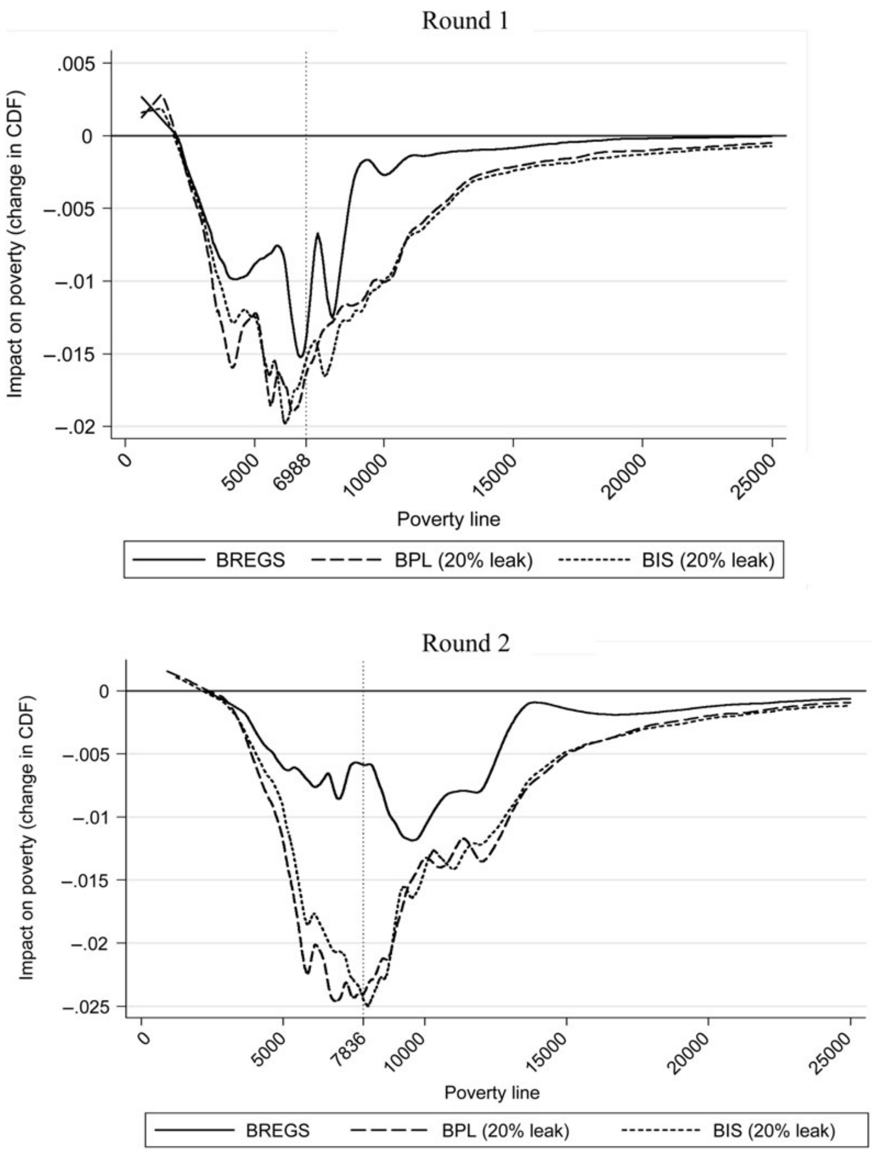
It is notable that, in terms of its impact on poverty, the BPL ration-card counterfactual is no better than the BIS. This confirms findings from other studies that the BPL cards are not well targeted—indeed, no better than a uniform (un-targeted) BIS in terms of impact on poverty.

It should be noticed that with 20% leakage the poverty rate attained by either of these counterfactual transfer schemes is almost identical to BREGS in R1 (table 7). This implies that the workfare scheme would dominate at slightly more than 20% leakage in the transfer schemes. The gap is somewhat larger in R2, so greater leakage would be needed to tilt the balance in favor of workfare.

We also tested sensitivity to the choice of poverty line, by repeating the calculations over a wide range of lines. BREGS has lower impact on poverty than BIS and BPL without leakage over the whole distribution, and BIS and BPL are very similar. For the PW participants alone, the rankings are far more ambiguous. In R1, BREGS has greater impact than either BIS or BPL over a wide range (but not

26. Note that here we define leakage to include anything not transferred to the intended beneficiaries including administrative costs. In one of the few comparative studies of the costs of transfer schemes, the results for Latin America reported in Grosh et al. (2008) imply that administrative costs represent 5–15% of total costs. For a large scheme such as studied in this paper the lower bound of this range appears to be more likely.

FIGURE 8. Poverty Impacts of BREGS for the Whole Population Compared to Cash Transfers with 20% Leakage



Source: Authors' estimates based on BREGS Survey.

all), while R2 BREGS has greater impact above the (R1) median but not below where the BPL allocation dominates even for PW participants.

The main conclusions are also robust to allowing for 20% leakage and poverty lines below the R1 median (as used in table 7). This is shown in figure 8, which is unsmoothed to illustrate the importance of considering more than a single poverty line; in R1 the median turns out to be an unusual point where the poverty impacts are very similar, but this is not true at lower lines. In R2, the two alternative schemes dominate BREGS for all potential poverty lines except at the lowest levels where there is no difference in impacts.

Overall, the superior targeting of unproductive workfare cannot outweigh two advantages of either BIS or BPL: lower opportunity costs of participation (zero forgone incomes) and avoiding the nonwage costs of workfare.

VI. Conclusions

We chose to focus here on the Indian state of Bihar since this is one of the poorest states of India. If the scheme was working well we would expect high participation rates in this state, but that is not the case. Indeed, Bihar has one of the lowest participation rates in the national workfare scheme. While Bihar is of obvious interest, one should be cautious about generalizing from Bihar to India as a whole.

Our results indicate that workfare participants in this setting are not drawn solely from the pool of the unemployed. Many report forgone earnings, though mostly a good deal less than the market wage. On average about one third of the workfare wage rate is forgone. The scheme also incurs costs for administration and material that amount to 40% of the budget.

Factoring in all of the costs, we find that the extra earnings from this large workfare scheme had less impact on poverty than either a basic-income guarantee—providing a uniform transfer of the same gross expenditure to everyone (whether poor or not)—or a uniform transfer to all those holding a government-issued ration card intended for poor families. Our main qualitative results are reasonably robust to the possibility of substantially overestimating forgone income and are also robust to the choice of poverty line over a wide range. The extent of leakage on the transfer schemes is a key unknown. Our main results on the qualitative ranking of BREGS relative to the transfer options also holds when we allow for 20% leakage in the transfer schemes, although, if leakage turned out to be much larger, then workfare would begin to dominate.

It is clear that even in this poor labor-surplus rural economy, the much-vaunted self-targeting mechanism that is achieved by imposing work requirements does not tilt the balance in favor of unproductive workfare over options using cash transfers with little or no targeting and with similar leakage as the workfare program.

Can the scheme be reformed to work better in practice? Forgone incomes are not easily controlled by such a program. The gaps between the stipulated wage rates and wages received might be reduced. Pro-poor reform could also reduce the substantial unmet demand for work on the scheme. This could be done by enhanced public information and a more responsive supply side; Dutta et al. (2014) identify a number of specific reforms. These would enhance the impact on poverty, including through larger impacts on wages in the private casual labor market. The greater impact on poverty would come at a greater cost to the public budget. Cost-effectiveness would need to be re-assessed at the implied higher level of funding.

A second direction for reforms is to assure that workfare is productive—that the assets created are of value to poor people (or that cost-recovery can be implemented for nonpoor beneficiaries). The creation of durable assets has not had much attention from the scheme's advocates and/or administrators. That may need to change. The results of this study suggest that, if the assets created are of sufficient value to poor people, then this workfare scheme would dominate the cash transfers considered.

Two qualifications should be noted, however. First, there may well be a trade-off. Meeting the extra demand for work may well make it harder to assure that the assets are indeed of lasting value. The public choice made in response to such a trade-off will depend on the weight attached to reducing current versus future poverty. Second, cash transfers can also be used to help create assets—notably in promoting human capital accumulation by incentivizing schooling and health care for children in poor families; however, for this to work it is crucial that the delivery system for these services is effective. The cost-effectiveness of "asset-creating workfare" would then need to be compared to such conditional cash transfers.

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