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Is a Guaranteed Living Wage a Good Anti-Poverty Policy?

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1. Introduction

While almost all governments intervene in labor markets (and often for good reasons), the labor markets facing the developing world's poorest are probably the least regulated of all. Yet the poor in much of South Asia and elsewhere depend heavily on their earnings from supplying unskilled, typically casual, labor, primarily in agriculture. The agricultural wage rate is known to be a significant predictor of the rural poverty rate in this setting.[2] A living wage in rural areas might then seem an obvious policy instrument for fighting poverty.

Simply legislating a minimum wage will not make it happen; the ease of evasion and costs of enforcement mean that a legislated "statutory minimum wage" may well be largely irrelevant to labor market outcomes for many low-wage workers.[3] While legislating a "living wage" is often advocated as an anti-poverty policy (in both developed and developing countries), limited coverage in practice has led some observers to question the efficacy of such a policy and to suggest other objectives are being served, such as protecting workers in organized sub-sectors; see, for example, Neumark (2004) in the context on minimum wage laws in the US.

This paper's point of departure from past research on the efficacy of legislated minimum wages as an anti-poverty policy is the recognition that, in all likelihood, the only effective way to achieve a more-or-less binding minimum wage rate for the poorest in the developing world is for the government to act as the "employer of last resort." The government commits to employ the entire excess supply of unskilled labor at the stipulated wage rate. We call this an Employment Guarantee Scheme (EGS). This is not a far fetched idea. Indeed, a coalition led by the Congress Party of India (that came to power in April 2004) developed a "Common Minimum

2 Evidence on this point for India can be found in Datt and Ravallion (1998).

3 Indeed, the classic theoretical characterization of the labor market in developing economies (following Harris and Todaro, 1970) postulates that minimum wages are only binding in the urban formal sector though there can still be (theoretically ambiguous) spillover effects to the unregulated sector.

Program" that includes as one of its main promises a national EGS. A lively debate ensued, as summarized in *The Economist* (2005a,b).[4] Supporters point to the potential for such a scheme to provide a secure safety net that is available to all, and does not require any form of administrative targeting. Critics have questioned the proposal's likely impact on poverty and have been concerned about the fiscal cost.

This debate has not been well informed about the costs and benefits of such a scheme. In the context of the proposed EGS for India, Drèze (2004) claims that the scheme "..would enable most poor households in rural India to cross the poverty line." However, this is based on the assumption that the poor — and only the poor — will turn up for such work and they will incur little or no opportunity cost in doing so. Yet there may be potential EGS workers in non-poor families, and poor families with no such workers. Nor can it be assumed that the net income gain to participants is the entire EGS wage rate (as some observers have implicitly assumed; see, for example, Mehrotra, 2004). The poor cannot afford to be idle in the absence of an EGS. Assuming perfect targeting and no forgone income, a guaranteed wage sufficient to reach the poverty line could virtually eliminate poverty. The reality may fall far short of that ideal.

The methods of costing the proposed EGS for India also appear to have been *ad hoc*, with little obvious relationship to the way in which the labor market is likely to respond to such an intervention. For example, the existing assessments of the cost of the proposed national EGS in India have assumed that all of the poor will participate, and none of the non-poor. Such cost estimates could be wildly off the mark, and so misguide policy makers.

Much of our current knowledge about such policies stems from research on the Maharashtra Employment Guarantee Scheme (MEGS) introduced by the Indian state of the same

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[4] A useful compilation of various contributions to this debate can be found at http://www.righttofoodindia.org/rtowork/rtw_articles.htm

name in the early 1970s. This is often cited as a model safety net for a developing country (see, for example, Drèze and Sen, 1989). The evidence on MEGS does not indicate that only the poor will turn up for such work, and that all of the poor will do so; while MEGS does have an in-built self-targeting mechanism, it can hardly be considered to be perfectly targeted (Ravallion and Datt, 1995). However, it is also questionable that MEGS has acted as a true EGS, at least since the late 1980s; there is evidence that access to the scheme was heavily rationed in the wake of a doubling of the minimum wage rate in 1988 (Ravallion et al., 1993). There is also evidence that the scheme's targeting performance deteriorated when the wage rate rose substantially (Gaiha, 1997). Possibly a true EGS would better reach the poor.

This paper provides an *ex ante* assessment of the cost-effectiveness against poverty of introducing an EGS for India. We compare such a scheme to a feasible alternative anti-poverty program based on untargeted cash transfers. Our evaluation aims to capture the trade off between targeting performance (whereby the work requirement helps to self-select the poor) and efficiency costs (whereby the work requirement creates deadweight costs to the poor). We ask:

- *How much impact on poverty can be achieved by a guaranteed wage sufficient to reach the poverty line?*
- *Would such a policy be more effective against poverty than an un-targeted allocation of the same public expenditure?*

The following section outlines our methodology. Section 3 describes our data, while section 4 presents our results. Section 5 concludes with a number of caveats.

2. Estimating impact and incidence of a guaranteed minimum wage

The key feature of the scheme we model here is that it establishes a firm lower bound to the wage distribution, at the scheme's wage rate w^{EGS}. This will be the case if employment is guaranteed, for then no able-bodied worker would accept non-EGS work at any wage rate below the EGS wage. The impact is then found amongst those workers whose (actual or imputed) wage rate is below the wage floor set at w^{EGS} and who have labor market characteristics consistent with doing casual labor; salaried workers in permanent work, for example, are very unlikely to participate. In allowing for heterogeneity in labor market characteristics we recognize that a given worker's choice of whether or not to take up such work is not determined solely by a comparison of w^{EGS} with the going expected wage rate for that worker but will also be influenced by longer-term economic considerations (such as the security of work) and social factors (such as possible stigma associated with performing low-skilled manual work).

Our method allows us to estimate the incidence of the program's outlays across different types of households, including across levels of living measured by consumption expenditure per person. Thus we can also estimate the program's impact on measures of poverty and its incidence through the whole distribution of consumption.

2.1 Models of wages and casual labor force participation

In calculating the gains we must take account of the foregone income from any work displaced by the scheme. In other words, we need to establish the distribution of pre-intervention wages. If w^{EGS} is above the wage for any adult willing to supply casual labor then there is a corresponding gain in earnings. If w^{EGS} is below the current wage or the worker's characteristics suggest that she is unlikely to do such work then there is assumed to be no impact.

Casual wage rates are only observed for a minority of workers (not including salaried workers, the self-employed or "discouraged workers"). We must impute the wage rate for the rest. For this purpose we use a wage equation estimated for those for whom the wage is observed to impute the wages of those for whom it is not, allowing for the selection process (whereby those for whom wages are observed are not a random sub-sample).

We assume that wages in the casual labor market are determined by worker characteristics and region-specific and seasonal factors. Letting Ω denote the set of all workers employed in casual labor and letting w_i denote the wage rate of casual worker *i*, we assume that:

$$\ln w_i = Z_i\beta + \nu_i \quad \text{for all } i \in \Omega \tag{1}$$

where Z_i is a vector of control variables for individual and household characteristics (including geographic and seasonal effects) and ν_i is an error term. (Separate wage equations are run for men and women.) To allow for selection into casual labor we use the standard Heckman correction whereby ν_i comprises a non-zero component and a zero-mean normally distributed innovation error term (ε_i) with variance σ_ε^2. A person participates in the casual labor market if $X_i\pi + \mu_i > 0$ and does not if $X_i\pi + \mu_i \le 0$ where X_i is a vector with parameters π and μ_i is a standard normal error term with a correlation coefficient of ρ with an additive innovation error component ε_i included in the error term in (1).[5] We then have a probit for participation:

$$P_i = \Phi(X_i\pi) \text{ for all } i \in A \tag{2}$$

where P_i is the probability of supplying casual labor, $\Phi(.)$ is the normal distribution function (with density function $\phi(.)$) and A is the set of able-bodied adults. Then:

$$E[\ln w_i | P_i = 1, Z_i, X_i] = Z_i\beta + \rho\sigma_\varepsilon\lambda(X_i\pi) \tag{3}$$

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[5] Our treatment of selection confounds two possibly rather different choices, namely whether to enter the labor force and (if so) whether to do casual labor. It might be of interest to separate these two components, but there is no obvious identification strategy. In any case, correcting for selection bias in our wage regression only requires a model of participation in casual labor within the set of people on which the wage regression is estimated.

where $\lambda(X_i\pi) \equiv \phi(X_i\pi)/\Phi(X_i\pi)$. The nonlinearity of $\lambda(X_i\pi)$ permits identification even if *X* and *Z* are the same. However, we follow common practice in also assuming that there is information about workers that influences labor-force participation (i.e., included in the *X* vector) but is not observable by employers and so cannot affect wages (i.e., excluded from the *Z* vector). We discuss our precise identifying assumptions when we come to our results.

2.2 Income gains and cost of the program

In estimating the direct income gains from the EGS we assume that those workers currently employed as casual laborers gain according to the difference between the EGS wage rate and their expected wage rate in the absence of the EGS (when that difference is positive). It is of course more complicated for those workers who are attracted into casual labor after introducing the EGS. Here we allow for the fact that there are certain types of people for whom it is unlikely that they would ever participate in casual work of this sort, whether private or public. For example, individuals who are employed as regular salaried labor are unlikely to ever participate in the casual labor market even if we find that some of them have a predicted wage rate for casual labor that is lower than the EGS wage rate. Rather than apply *ad hoc* filters, we use our predicted participation equation for casual labor (equation 2) to identify the types of individuals who are unlikely to benefit directly. We do this by only attributing positive gains when it is more likely than not that the person will participate in casual labor market, as indicated by a predicted P>0.5.[6] We test sensitivity to this cut-off point.

In the absence of the EGS, some casual laborers would no doubt have been unemployed for some of the time that they work at the EGS wage rate (either for the EGS directly or doing

[6] An alternative is to weight the gains by the estimated probabilities of doing casual labor. However, this gives nonsensical results for estimated labor supply to the EGS, in that anyone with a positive income gain, however small, should be counted in our estimate of casual labor supply.

non-EGS casual labor). So in calculating the distribution of earnings in the absence of the program we discount the actual wage currently earned for casual work by the probability of employment. The probability of being unemployed is assumed to be given by:

$$U_i = \Phi(X_i\gamma) \text{ for all } i \in A \tag{4}$$

where U_i =1 if worker *i* is unemployed at the date of interview and 0 otherwise. The imputed expected wage rate is then $\hat{w}_i\,(1-\hat{U}_i)$ where:

$$\hat{w}_i \equiv \exp[Z_{ij}\hat{\beta} + \rho\hat{\sigma}_\varepsilon\lambda_{ij} + \hat{\sigma}_\varepsilon^2/2] \tag{5}$$

is the predicted casual wage rate (noting that $E(e^\varepsilon) = e^{\sigma_\varepsilon^2/2}$ when ε is normally distributed).

Combining these arguments, the earnings gain to person *i* is:

$$g_i = \max[w^{EGS} - w_i\,(1-\hat{U}_i),0] \text{ for all } i \in \Omega \tag{6.1}$$

$$= I(\hat{P}_i - 0.5).\max[w^{EGS} - \hat{w}_i\,(1-\hat{U}_i),0] \text{ for all } i \in A \text{ and } i \notin \Omega \tag{6.2}$$

where *I*(.) is the indicator function (taking the value 1 if the term in parentheses is positive and zero otherwise). Equation (6.1) gives the gains to existing casual laborers, while (6.2) gives the gains to those induced to switch by the scheme. Two remarks are in order. Firstly it should be noted that there is no gain from the scheme for those people whose expected pre-EGS wage is already above w^{EGS}. Secondly, note that the gains are not confined to those who actually join the EGS. All workers earning less than w^{EGS} will enjoy an income gain, whether or note they participate directly in the scheme. That is an immediate implication of the fact that the entire excess supply of casual labor is absorbed at the EGS wage rate.

Combining these assumptions we can estimate the impact on the distribution of consumption. Let the pre-EGS consumption of household *j* be Y_j (*j*=1,…,*n*) while post-EGS consumption is Y_j^*. Household *j* comprises n_j able-bodied adults and the set of able-bodied adults in household *j* is denoted H_j. (Thus $A = \bigcup_{j=1}^{n} H_j$.) We assume that the income gain from the program is consumed fully. Post EGS consumption can then be written as:

$$Y_j^* = Y_j + \sum_{i=1}^{n_j} g_i \Big| i \in H_j \tag{7}$$

Letting y_i denote consumption per person (Y_i divided by household size), the pre-EGS distribution of consumption per person is $(y_1, y_2, ..., y_m)$ across all *m* individuals and poverty measures can be calculated from that distribution by standard methods. Similarly, the post-EGS distribution of consumption per person $(y_1^*, y_2^*, ..., y_m^*)$ yields the post-intervention poverty measures. It is also of interest to see how the per capita gain, $y_i^* - y_i$, varies with y_i. The incidence can be plotted against (pre-intervention) household consumption per person.

What is the cost to the government? All those who gain from the scheme (i.e., $g_i > 0$) are assumed to supply casual labor. To this we must add those who did not gain, but were already supplying casual labor; since they are unaffected we assume that they continue to supply casual labor. Thus the aggregate supply of casual labor (to both EGS and non-EGS work) is:

$$S(w^{EGS}) = \sum_{i=1}^{n} I(g_i) + N(i \in \Omega, g_i = 0) \tag{8}$$

where *N*(.) denotes the number of elements in the set defined by the conditions in parentheses. Note that this is a counterfactual labor supply function, in the presence of the EGS.

The cost will also depend on how labor demand responds. Under our assumptions, any response will be solely for low-wage workers. However, the demand for labor conditional on individual-specific wage rates is unobserved, so there is no way of identifying the relevant demand response. The only feasible route is to assume that labor demand is unaffected. We can, however, sign the bias in our estimates of the cost of the scheme if this assumption fails to hold (assuming that the labor demand function is downward sloping in the wage rate).

So the cost of the scheme is:

$$C(w^{EGS}) = w^{EGS}[S(w^{EGS}) - L]/s \tag{9}$$

where L is the initial level of employment and s is the scheme's labor share in total cost. This will be an underestimate, to the extent that the higher wage rates induced by the scheme reduce private demand for casual labor. However, note that the wage effects are only at the lower end of the wage distribution (at wages initially below the EGS wage). It should also be noted that we are ignoring any effects of the assets created by the scheme; to the extent that the assets created increase the productivity of casual labor in the private sector this will at least partly compensate for the effect on of the higher wage rates due to the scheme.

There are many alternative uses of the same fiscal outlay. Here we consider the simplest alternative, namely an un-targeted family allowance scheme. Specifically, we simulate a uniform allocation of the same aggregate budget, net of administrative costs, across the whole population (whether poor or not). In other words, per capita consumption for every household rises by $(1-k)C(w^{EGS})/m$ where k is the share of administrative costs in the budget (and, as before, m is the population size). We then re-calculate the poverty measures for this new distribution. We acknowledge that in practice it will almost certainly be the case that information will be available to the policy maker on differences between households in their "needs" — information that should be factored into the design of a cash transfer scheme.[7] Optimally incorporating such

[7] Although the precise way this should be done will depend on a number of factors, including how the poverty-reduction objective is measured; see Keen (1992).

information would clearly enhance the relative effectiveness of the transfer scheme over the simple untargeted scheme considered here.

Which policy has more impact on poverty is unclear on *a priori* grounds. The self-targeting mechanism in a workfare program is expected to assure that the gains from an EGS are not uniform but tend to be higher for the poor (although the extent of self-selection is an empirical question).[8] Against this, the EGS incurs deadweight losses associated with the foregone incomes of participants and the extra non-wage costs incurred for non-labor inputs and supervision. Our empirical results to follow will reveal which scheme has the greater impact on poverty, given the trade off between targeting performance and efficiency cost.

3. Data

Our analysis is based on the Employment-Unemployment Schedule ("Schedule 10") of India's National Sample Survey (NSS) for 1999-00. At the time of writing, this is the most recent available "thick sample" NSS with the complete version of Schedule 10.[9] In addition to standard data on household characteristics (religion, caste, land ownership, demographics, schooling), Schedule 10 includes detailed information on employment characteristics for all members of every household. For each person, information is collected on her principal activity in the year preceding the survey and daily activities during the week preceding the survey.

Schedule 10 also collected consumption expenditures (including imputed values for consumption in kind), which we use in studying the impacts on poverty. However, Schedule 10 used an abbreviated version of the main consumption module used by the NSS (including in the

[8] On the comparative targeting performance of workfare schemes see Coady et al. (2004). For a more general discussion of the incentive properties of such schemes see Besley and Coate (1994).

[9] The larger "thick" samples are surveyed every five years. Smaller ("thin") samples are surveyed annually, with an abbreviated version of Schedule 10.

same survey round, but not linked to the same households), The abbreviated version tends to give lower consumption levels and hence higher poverty measures.

The rural sample includes about 61,000 households, of which the vast majority (96.5%) have at least one able-bodied adult. We base our analysis on the sample of 178,000 adults (15 to 59 years) from the 15 major states.[10] The analysis is done at the level of the NSS region, and all results are then aggregated up with appropriate weights. The set of beneficiaries from an EGS is assumed to include all adults who are likely to gain from an increase in labor market wages, either directly (as EGS participants) or indirectly due to an increase in wages paid in the market as a whole. The beneficiaries need not be currently in the labor force.

Amongst potential beneficiaries, roughly 65% participated in the labor force, 24% were employed as casual wage labor (either on public works or private) and 8% were unemployed for at least part of the time during the week preceding the survey. In terms of time allocation, on average, a person spent four days of the week in the labor force. The time spent in the labor force is much higher for males who spent 5.5 days on average employed and an additional half day seeking employment in the week preceding the survey. The survey gives daily wages earned by those employed as either casual or salaried labor during the week. The average wage earned by casual laborers in the sample is about Rs 40 per day.

There are two obvious ways one might define a "living wage" in rural India. The first is anchor the EGS wage rate to the official poverty line. At the average household size of 5 persons and average number of adults in the labor force of 2, a daily wage rate per working adult of Rs 38 would be sufficient to reach the mean official poverty line in 1999-2000. Using instead

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[10] These comprise Andhra Pradesh, Assam, Bihar, Gujarat, Haryana, Karnataka, Kerala, Madhya Pradesh, Maharashtra, Orissa, Punjab, Rajasthan, Tamil Nadu, Uttar Pradesh, and West Bengal.

the demographics of the poor, who have a higher dependency ratio (2 working adults with mean household size of 5.8), the wage rate would need to be Rs 42.[11]

The second approach is to base the calculations on the existing (State-level) minimum wage rates. This has been advocated by supporters of the EGS in India. At the time of writing, a wage rate has not been set for the proposed national EGS, though supporters have proposed a wage rate (in 2005) of Rs 60 per day, being an average of the State-level statutory minimum wage rates. Adjusting only for inflation (using the Consumer Price Index for Agricultural Laborers) this is equivalent to Rs 55 in 1999-2000. However, this is a deceptive comparison. While the statutory minimum wage rate refers to a full working day, and might be taken to reflect a reasonably high level of effort, the wage rates calculated from surveys average across workers with a range of effort levels and lengths of the working day.[12] An alternative source of wage data is the series Agricultural Wages in India collected by the Ministry of Agriculture, based on their monitoring of male agricultural wages for a full working day, at district level. Aggregating up the state level estimates for 1999-2000 (weighting by the shares of agricultural workers by state), the mean male agricultural wage from this source is Rs 51 per day, which is 13% higher than the male wage rate of Rs 45 from the NSS. Deflating the Rs 55 wage rate by the same factor gives a wage of Rs 49. Rounding off, we will use an EGS wage rate in 1999-2000 of Rs 50 per day as the comparator to the Rs 60 for 2005.

So one can defend at least two possible reference "living wage" for an EGS in 1999-00, namely around Rs 40 per day (which happens to also be the average wage rate) and Rs 50. We include both, and we test sensitivity to other wage rates over the range from Rs 25-55 per day.

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[11] The daily household expenditure requirement is calculated as household size times one thirtieth of the monthly poverty line appropriate to that household (depending on its location). The "living wage" is then the expenditure requirement for the day divided by the number of adults in the labor force.

[12] Schedule 10 distinguishes a "half day" from a "full day" though the former could be much less than four hours. (We take account of the full day-half day distinction in calculating wage rates.)

4. Results

In identifying the parameters of the wage equation (1), given non-random selection into the casual labor market, we assume that the education of other household members and demographic characteristics of the household can have no independent effect on wages. However, in village labor markets there may well be common knowledge about worker characteristics such as caste and religion, so we include these as covariates of wages.

Table 1 gives the wage regressions for casual rural labor for men and women. The regressions generally have the expected properties. Wages rise with age, marital status and education (though with little gradient above primary school). Returns to schooling are lower for women (indeed, there are no significant casual wage gains from female education for completed primary schooling or above). Correcting for selection bias lowers returns to schooling. Higher land holding comes with lower wages (though the gradient is higher for men, once one corrects for selection bias). Curiously, being a member of a Scheduled Caste/Tribe (SC/ST) increases female wages but has no significant effect on male wages (on correcting for selection bias). Religion matters for male wages but not female wages.

The probits for the probability of unemployment are given in Table 2 while Table 3 gives the probit estimates of the casual-labor participation equation. Again the results are generally unsurprising. The probability of unemployment rises with age as does participation in the casual labor market. Marriage increases the probability of men entering the casual labor force, but decreases the probability of women doing so. Higher levels of schooling decrease the probability of both unemployment and participation (with a larger impact for men). Having more land decreases the probability of either supplying casual labor or being unemployed (though both effects are stronger for men). Having more male adults in the family decreases the probability of women entering casual labor.

One concern about our model of participation in the casual labor market is that it may not be adequately capturing the household income effect on individual participation. This could be particularly important for women's choice about whether to supply casual labor. Another concern is that it may not sufficiently control for characteristics that explain why an individual's usual activity status during the year was unemployment rather than casual labor. To address these concerns we also estimated an augmented version in which we added household consumption expenditure per person and also a dummy variable for whether the usual status was unemployment. Both were statistically significant with the expected signs, though there are clearly concerns about the likely endogeneity of both variables. We will refer to this as the augmented specification and test the sensitivity of our main results to this specification.

4.1 Incidence of the gains from a guaranteed minimum wage

The incidence of the direct income gains from the program across quintiles of consumption expenditure per capita can be found in Table 4. Figures 1 and 2 plot the absolute and proportionate gains (respectively) against the rank in terms of consumption expenditure per person. Figure 3 gives the impact on the cumulative distribution function (indicating the reduction in poverty for a wide range of possible poverty lines). (Note that first-order dominance is automatic under our assumptions.)

The absolute mean gain from the EGS tends to fall as household consumption per person rises (Figure 1). At an EGS wage rate of Rs 40 per day, we find that the gain represents 27% of the consumption of the poorest 20%, falling to 3% for the richest 20% (Table 4, top panel). At the wage rate of Rs 50, the gain is 47% of consumption for the poorest quintile, falling to 6% for the richest quintile. When plotted against pre-EGS consumption we find percentage gains at the Rs 40 wage peaking at around 40% for the poorest few percentiles (Figure 2). At a Rs 50 wage rate the percentage gains peak at around 70% for the poorest.

At the Rs 40 wage rate, we find that 32% of the expected gainers come from the poorest quintile, and this drops only slightly to 30% at the Rs 50 wage rate. There is a deterioration in targeting performance as the wage rate rises, but it is more pronounced at lower wage rates, and even then it is not dramatic (Table 4, middle panel).

The bottom panel of Table 4 tells us what proportion of each quintile has a positive gain. In making these calculations, each member of a household with at least one worker who gains from the EGS is deemed to gain. Again focusing initially on an EGS wage set at the mean wage, we find that almost half of all those in the poorest quintile benefit, falling to 14% in the richest quintile. The self-targeting mechanism is clearly working, though it remains that there are non-negligible numbers of workers in non-poor families who would be attracted to the scheme.

The main reason why the self-targeting mechanism is less effective than might be expected is that the distribution of adults with predicted wages less than the EGS wage turns out to be nearly uniform across the distribution of household consumption per person, as can be seen from Table 5 (top panel). In other words, predicted wages for casual labor are poorly correlated with household consumption per person. Furthermore, this is affected little by changes in the EGS wage rate. The main reason why we find that there is a self-targeting mechanism at work is that willingness to do casual labor falls with consumption (Table 5, bottom panel). This is unaffected by the EGS wage rate. The results in Table 5 point to the likely importance of the filter we use for identifying likely casual workers; we return to this point.

No targeting mechanism is ever perfect. And one might question whether we are using the right welfare indicator; possibly some of those we classify as “rich” according to current consumption (as measured in our survey data) would be deemed poorer by other (unobserved) criteria. So one might not want to overstate the significance of our finding that the self-targeting mechanism is far from perfect. However, the results in Table 4 do warn against the robustness of the generalizations made by some advocates of the EGS.

Table 6 gives our estimated aggregate supply of casual labor at each EGS wage rate. The overall wage elasticity (estimated as a regression of log labor supply on log EGS wage rate across the 7 observations) is 0.19 (with a standard error of 0.02). The elasticity is slightly lower for the poor; for the poorest quintile the elasticity is 0.16 (standard error of 0.02).

While past estimates of the labor supply function to casual labor may not be a good guide to the labor supply function in the presence of an EGS, it is still of interest to compare these elasticities to previous estimates. In a study using household level data for West Bengal in 1972-73, Bardhan (1984) obtained wage elasticities of labor supply to casual agricultural labor in the range 0.2 to 0.3 — similar to those implied by our estimates of labor supply at various EGS wage rates. However, our estimates are considerably lower than those reported in Kanwar (2004) who obtained labor supply elasticities in the range 0.8-1.5. (These estimates used the ICRISAT Village Level Surveys for semi-arid areas over 1975-1984.)

What are the characteristics of those likely to benefit from the scheme? Table 7 helps answer this question by comparing a range of descriptive statistics between the adult sample as a whole, those who gain from the EGS and the sub-sample of gainers who are attracted into casual work at an EGS wage rate of Rs 50 per day. We find that most adults likely to benefit from an income gain are those who are already in the labor force, and in particular, are already employed as casual laborers (73% among males and 79% among females). New recruits to the casual labor market (including the EGS) tend to have less schooling than current casual laborers who gain (who are in turn less well educated than the adult population as a whole), are less likely to have a literate head of household and are more likely to be from scheduled caste/tribes.

Table 8 reports the estimated income gains per adult and per capita. At Rs 50 per day, the average gain per adult is about 40% of the EGS wage. Foregone incomes are higher at lower EGS wages, and slightly higher amongst the poor. Note, however, that these are foregone incomes conditional on gaining from EGS; foregone incomes for non-poor non-beneficiaries are likely to be considerably higher.[13] Although we find that income gains conditional on benefiting from the scheme do not vary much between the poor and non-poor, the probability of receiving an income gain is much higher amongst the poor. As a result, income gains per capita (reported in the second panel in Table 8) are one and a half times the gains per capita among the non-poor.

The estimated impacts on poverty measures for rural India are given in Table 9. Note that these are annual impacts of the scheme, assuming it operates over the whole year. At an EGS wage rate of Rs 40 we find that the poverty rate falls from 34% to 25%. An EGS wage of Rs 50 brings the poverty rate down to 21%. Proportionate impacts are higher for the poverty gap index and squared poverty gap indices, indicating that mean income of the poor rises and distribution amongst the poor improves.[14]

Table 9 also gives results for the augmented specification for our model of participation in casual labor discussed above. Recall that there are concerns about endogeneity bias in this specification, though (against that consideration) it may be better able to pick up household

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[13] For further discussion of this point see Jalan and Ravallion (2003).

[14] The poverty gap index gives the mean distance below the poverty line as a proportion of the poverty line (where the mean is taken over the whole population, counting the non-poor as having zero poverty gaps.) In calculating the squared poverty gap index, the individual poverty gaps are weighted by the gaps themselves, so as to reflect inequality amongst the poor (following Foster et al., 1984).

income effects on participation in casual labor, particularly for women. This specification gives about the same impacts on poverty at low EGS wage rates, rising to about two percentage points at higher wages. At the Rs 40 wage rate, the headcount index fell to 23% while at the Rs 50 wage rate it fell to 18%. This specification also gave higher female participation in the EGS, though the difference was small.

We also tested the sensitivity of our results to using a different cut-off point in identifying those likely to supply casual labor. Recall that we used a cut-off of 0.5 for the predicted probability. Lowering this to 0.4 adds about one percentage point to the impact on poverty; at the Rs 40 wage rate the headcount index drops to 23.3% and at Rs 50 it falls to 18.7%. Raising the cut-off to 0.6, conversely reduces the impact on poverty by similar magnitudes; at the Rs 40 wage rate, the headcount rate is estimated to be 25.5% and at Rs 50 it is 21.9%.

4.2 Budgetary cost and comparison with untargeted transfers

Table 10 gives our estimates of the net labor supply to the EGS. At an EGS wage rate of Rs 40, we expect a daily attendance at EGS sites of 35 million people. This rises to 38 million at a wage of Rs 50. At a living wage anchored to the official poverty line, the EGS would account for 33% of the casual labor force, rising to 35% at the Rs 50 wage rate. It may be noted that the bulk of the excess supply response to higher EGS wage rates comes from men. Indeed, at all except the low EGS wage rates the increments to female take-up of the EGS as the wage rate rises tend to be small. This occurs in large part because of saturation of the number of female workers with characteristics that imply they would participate in the casual labor market.

The last column of Table 10 gives an estimate of the total cost as a percentage of GDP in 1999-00. We assume that the EGS operates for 300 days and we assume a labor share in EGS projects of 60%, in keeping with policy discussions on the proposed national EGS. We find that at an EGS wage rate of Rs 40 the annual cost represents 3.7% of GDP. This rises to 4.9% at a wage rate of Rs 50. (The labor supply to the scheme and hence cost was virtually identical for the augmented specification.)

However, we found that our estimates of the cost are quite sensitive to the cut-off point in predicted probability of doing casual labor used in identifying those likely to supply casual labor. While a cut-off of 0.5 is a natural choice (given that it identifies whether a person is more likely than not to do such work), it is worth checking sensitivity to a different cut-off. We find that lowering the cut-off point to a predicted probability of 0.4 raises the cost to the equivalent of 4.9% of GDP at the Rs 40 wage rate, and 6.8% at Rs 50, while raising the cut-off to 0.6 lowers the cost to 2.8% and 3.6% of GDP at the Rs 40 and Rs 50 wage rates, respectively.

What would the impact on poverty be if the same budgetary outlay was allocated on a uniform basis? We have seen that the EGS is targeted to the poor; in particular the mean gain rises as household expenditure per person of a household falls (Figure 1). By contrast, a uniform transfer gives the same amount to everyone, whether poor or not. So the EGS has a clear advantage in terms of targeting. This can be seen in Figure 4, which plots the mean percentage gain against household rank by expenditure per capita. However, this targeting advantage of EGS comes with extra costs, in the form of foregone incomes for participants and non-wage costs to the government. So the outcome of this comparison is unclear on *a priori* grounds.

As described in section 2, to simulate an un-targeted transfer scheme we take the same estimated gross cost of the EGS and allocate it uniformly to all, whether poor or not. We assume that 10% of the budget available for transfers must be set aside to cover administrative costs.[15] We provide calculations for two assumptions about the extent of cost recovery from the assets

[15] In a survey of administrative costs for transfer programs requiring individual assessment in Latin America, Grosh (1995) reports a median administrative cost of 9% of the budget.

created by the EGS. In the first case, we assume no cost recovery, as appears to have been typical of past experience with workfare programs in India; then 90% of the gross EGS cost is available for the hypothetical un-targeted transfer scheme. In the second case, we assume that all of the non-wage cost of EGS can be recovered by charges to (non-poor) beneficiaries, so that only the wage cost of the EGS is available for the transfer scheme. Notice that we are allocating the same budget across the rural population only, so there is a sense in which our counterfactual transfer scheme is targeted, but only to rural areas as a whole, not within.

The striking finding in Table 11 is that a budget-neutral un-targeted allocation has greater impact on poverty at any EGS wage rate (comparing Tables 9 and 11). Indeed, the poverty impacts only come close at low EGS wage rates and with full cost-recovery of non-wage costs; with moderate to high wages, the un-targeted allocation has substantially greater impact on poverty. The uniform transfer dominates over the entire range of the distribution (Figure 5). Using the augmented specification narrows the differential, but uniform transfers still dominate. Note, however, that cost-recovery for the non-wage costs greatly narrows the advantage of uniform transfers across a wide range of poverty lines (as is evident from how close the middle curves are in Figure 5).

4.3 Living wage for the lean season only

So far we have considered an EGS operating all year. We also simulated a scheme that is constrained to operate for only three months, in the lean season, defined as the first quarter as in the NSS, namely June, July and August. We checked whether this was the low wage season in each state, based on the estimated parameters on the interaction terms between the state dummies and the season dummies in the wage regressions in Table 1. The first (NSS) quarter was the low wage season in all except two states. Note, however, that this is also the monsoon period, which would make it hard to do some public works projects; in reality, a three-month scheme would have to be in dryer seasons. If anything, this would probably mean lower impacts on poverty then we report here. More detailed results are available in Murgai and Ravallion (2005).

The results for the lean season EGS indicate a slightly higher absolute drop in the poverty rate during the lean season at each wage rate (though the pre-intervention poverty rate is of course higher). The Rs 40 wage rate brings the headcount index in the lean season down from 37% to 27%, while the Rs 50 wage rate cuts a further four points off the poverty rate. However, note that this is the impact <u>during</u> the lean season. At the Rs 40 wage rate, an EGS operating solely in the lean season brings the annual poverty rate down from 34% to 31%, while at the Rs 50 wage rate poverty incidence falls by an additional percentage point.

Naturally the cost falls substantially when the scheme is confined to just one quarter, though it actually rises slightly per 100 days of operation. We find that the cost of the lean-season EGS is equivalent to 1.3% of GDP at a living wage of Rs 40, and rises to 1.7% at Rs 50.

5. Conclusions

Arguably the only feasible way to achieve a binding minimum wage in a developing rural economy is for the government to act as the employer of last resort. We have tried to throw light on the cost effectiveness of such a policy for reducing poverty in rural India, recognizing that the government faces a trade off between targeting performance and efficiency costs, even when the sole objective is to reduce poverty.

At the time of writing, the Government of India is planning to implement an Employment Guarantee Scheme in rural areas, similar to the stylized scheme we have studied here. This was a prominent electoral promise that helped secure a victory for India's Congress Party in the 2004 national election. However, the paucity of credible data and analysis of this proposal has been noted by many observers.

In assessing *ex ante* the likely impact of a guaranteed wage policy, we have allowed for foregone earnings from other work and have derived the incidence of gains empirically (rather than by assumption). We find that guaranteeing a wage rate sufficient for the average rural family to reach the poverty line would bring gains to the poor, though well short of eliminating poverty. Such a scheme operating for the whole year would achieve slightly more than a one-quarter reduction in the incidence of poverty (from a headcount index of 34% to 25%). For an EGS confined solely to the lean season at the same wage rate, the poverty rate in that season falls from 37% to 27%. The annual poverty rate falls from 34% to 31% with a lean-season EGS.

Of course, this impact on poverty comes at a cost, which we reckon to be equivalent to 3.7% of GDP for a year-long EGS at a wage rate sufficient for the average rural family to reach the poverty line, which is also close to the overall mean wage rate. At a wage rate 25% above the mean — which gives a defensible comparison point to the wage levels currently proposed by advocates of the EGS in India — the scheme cuts almost 14 percentage points off the rural poverty rate (bringing it down from 34% to 21%) at a cost equivalent to about 5% of GDP. Operating the scheme in the lean season for 100 days brings the cost down to under 1.5% of GDP at the lower wage rate, and about 2% at the higher wage. As expected, targeting performance deteriorates as the wage rate rises, but this effect is small in going between the mean wage and a wage rate 25% above the mean. Predicted wages for casual labor are found to be poorly correlated with household consumption, thus dulling the self-targeting mechanism.

We have compared our simulated guaranteed wage policy to a hypothetical family-allowance scheme, entailing a budget-neutral transfer targeted to rural areas but uniform across people within rural areas. We find that the un-targeted policy would have a greater impact on poverty over the entire range of our assumptions. The greater cost in terms of leakage to the non-poor from un-targeted transfers is not enough to outweigh the extra costs (to both the poor directly and the government) associated with implementing a guaranteed living wage.

We point to six caveats to our analysis. Firstly, these are *ex ante* simulations, under certain assumptions that may or may not hold. In addition to the level of the guaranteed minimum wage rate, we have noted the sensitivity of our precise findings to changes in our assumptions related to the specification of explanatory variables for the casual labor force participation model, the cut-off point in probability of doing casual labor, the wage rate and the poverty line. Some of our results, including the cost of the scheme, are more sensitive than others; our conclusion that, in terms of its direct impact on poverty, the guaranteed wage rate scheme is dominated by un-targeted transfers appears to be quite robust.

Secondly, given that our purpose has been to assess the cost effectiveness of a binding minimum wage rate in a developing rural economy, we have naturally focused on an employment guarantee scheme rather than the heavily rationed workfare programs often found in practice. With rationing of the available work, one probably loses the bulk of the positive spillover effects to non-participants, and the assignment of work becomes prone to bureaucratic manipulation — in ways that may or may not be consistent with the aim of reaching the poor.

Thirdly, in modeling the scheme we have assumed that the work requirement is binding on participants; only those who do the work receive a payment, and all those who work are paid fully. This can also be subject to corruption in practice. Supporters of an EGS for India have argued that monitoring through social audits and local public disclosure of payments made would go a long way toward avoiding such problems (see, for example, Drèze, 2004). In support of that view, the little quantitative evidence available on the performance of Gram Sabhas (GS) (organized public meetings at village level called by the elected local government) suggests that when the GS is held it does improve the performance of public programs in reaching the poor in southern states (Besley et al., 2004). However, generalizing from this evidence to the rest of India would clearly be hazardous; the panchayat system is thought to be weaker or virtually non-existent in some regions of India, including much of India's Bimaru region.[16] Strengthening the Panchayati Raj in such regions could well be crucial to the success of an EGS, and other social services intended for the poor. Even with strong local governments, experience with the implementation of living wage ordinances in the US suggests that advocates and NGOs are also likely to play a positive role (Luce, 2005).

Fourthly, we have treated demand for casual labor as fixed. The higher wage rates for low-wage workers induced by the scheme will reduce private demand for such workers while the scheme is in operation. To the extent that labor demand contracts, this way we will have underestimated the cost of the scheme, as it would have to employ those workers who become unemployed as a result of the scheme. Incorporating this effect of the scheme would further favor uniform transfers as the more effective anti-poverty policy.

The fifth caveat is that our cost-effectiveness comparisons with an un-targeted cash transfer scheme have ignored any benefits to the poor from the outputs of the work done under an EGS. However, it appears that these benefits would need to be sizeable to justify favoring this as an anti-poverty scheme. While the returns on the assets created with the labor employed would not need to pass a conventional cost-benefit test, we have seen that even returns to the government that can cover the entire non-wage cost would not be enough to tilt the balance in

[16] For evidence on the differing performances of the panchayats across states of India see Mathew and Buch (2000).

favor of an EGS (except at low wage rates and elastic labor demand). However, there are possible indirect benefits we have not considered. For example, a further cost saving is possible if the assets created are labor-productivity enhancing, thus attenuating the contraction in demand for private labor attendant to the higher wage rates under the scheme. Productivity effects could arise if the scheme is used to create local public goods that are cooperant in production with casual labor. (There is also a potentially important role here for the panchayats in assuring that the assets created are of value to the community.) Further indirect benefits might also be expected through previously uninsured people adopting new (farm and non-farm) income generating activities that were perceived as too risky to living standards in the absence of an effective safety net. (The consumption floor provided by a uniform transfer to everyone could also have indirect benefits, though without the property of an EGS that the extent of relief varies with the extent of the shock at household level.) Naturally, achieving these insurance benefits would require that the scheme operates as a true EGS, in that anyone who wants work at the stipulated wage rate can get it.

Finally, it should be noted that a complete assessment must also take account of how the scheme is financed. If the fiscal cost is covered by cutting other programs that benefit the poor, or by raising the taxes they face, then the net impacts on poverty will be lower than our estimates suggest. Financing the scheme by borrowing will probably also entail costs to the poor, through effects on growth and employment.

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Table 1: Determinants of casual wages in rural India, 1999-00

	Males		Females	
Independent variables	MLE	OLS	MLE	OLS
Individual characteristics:				
Age (yrs)	0.026	0.023	0.009	0.008
	(12.20)	(11.23)	(3.55)	(3.52)
Age-squared	0.000	0.000	0.000	0.000
	(12.07)	(10.97)	(3.33)	(3.30)
Marital status (1=married)	0.035	0.030	-0.009	-0.006
	(3.61)	(3.15)	(0.82)	(0.61)
Education, below prim	0.057	0.065	0.046	0.048
	(6.71)	(7.75)	(2.45)	(2.58)
Education, primary	0.057	0.072	0.004	0.007
	(5.45)	(7.27)	(0.23)	(0.43)
Education, middle	0.046	0.073	-0.003	0.003
	(3.74)	(6.51)	(0.11)	(0.14)
Education, secondary	0.045	0.095	0.03	0.04
	(2.53)	(6.07)	(0.79)	(1.11)
Household characteristics:				
Land owned (ha)	-0.032	-0.011	-0.013	-0.011
	(4.68)	(2.25)	(2.04)	(2.13)
Caste (1=SC/ST)	-0.004	-0.018	0.029	0.026
	(0.45)	(2.53)	(2.92)	(2.91)
Religion (1=Hindu)	-0.03	-0.032	0.011	0.009
	(2.72)	(2.97)	(0.64)	(0.51)
Constant	3.106	3.189	3.112	3.131
	(70.84)	(76.67)	(54.70)	(64.09)
Heckman's: atanh(rho)	0.160	---	0.026	---
	(4.87)		(0.66)	
Heckman's: log(sigma)	-0.969	---	-1.028	---
	(83.91)		(68.66)	
No. of observations	89101	22910	88250	11227
R-squared	---	0.37	---	0.28

Notes: Absolute t-values in parentheses, adjusted for clustering at the household level. All regressions include dummies for NSS regions, quarters during which the survey was administered, and interaction terms between quarters and state dummies. Omitted category is illiterate for individual education level dummies. The participation regression used to correct for sample selection bias also included a range of household characteristics as given in Table 3.

Table 2: Probit regressions for the probability of unemployment

Independent variables	Males	Females
Individual characteristics:		
Age (yrs)	0.007	0.002
	(8.97)	(4.82)
Age-squared	0.000	0.000
	(11.02)	(6.03)
Marital Status (1=married)	-0.017	-0.015
	(3.80)	(8.12)
Education, below prim	-0.013	-0.009
	(2.84)	(5.02)
Education, primary	-0.022	-0.011
	(5.22)	(6.01)
Education, middle	-0.034	-0.015
	(8.45)	(9.00)
Education, secondary	-0.015	0.002
	(2.88)	(0.89)
Household characteristics:		
Land owned (ha)	-0.02	-0.008
	(8.98)	(6.20)
Caste (1=SC/ST)	0.031	0.01
	(10.22)	(7.72)
Religion (1=Hindu)	-0.013	0.003
	(3.13)	(1.61)
At least one person with secondary education in household	-0.018	-0.008
	(4.08)	(5.12)
Sex of household head (1=male)	0.001	0.002
	(0.19)	(0.90)
Age (yrs) of household head	0.001	0.000
	(3.55)	(0.70)
Literate head of household	-0.009	-0.006
	(2.25)	(4.14)
Log(household size)	0.003	0.002
	(0.57)	(0.77)
Share of kids(<15 yrs)	0.027	-0.003
	(1.76)	(0.46)
Share of male adults(15-59 yrs)	0.043	-0.002
	(2.73)	(0.29)
Share of female adults (15-59 yrs)	0.085	0.02
	(5.31)	(2.95)
No. of observations	89893	88092
Percent correct predictions	89.1	95.2
Pseudo-R^2	0.12	0.17

Notes: Coefficients reported are *change* in probability at sample mean. Absolute t-values in parentheses, adjusted for clustering at the household level. SC/ST=scheduled caste/tribe. All regressions include dummies for NSS regions, quarters, and interaction terms between quarters and state dummies. Omitted category is illiterate for individual education level dummies.

Table 3: Probit regressions of probability of participation in casual labor market

Independent variables	Males	Females
Individual characteristics:		
Age (yrs)	0.018	0.012
	(12.68)	(18.06)
Age-squared	0.000	0.000
	(14.75)	(18.15)
Marital Status (1=married)	0.032	-0.042
	(4.75)	(11.19)
Education, below prim	-0.026	-0.023
	(3.56)	(6.32)
Education, primary	-0.075	-0.039
	(10.68)	(10.69)
Education, middle	-0.141	-0.063
	(21.25)	(18.12)
Education, secondary	-0.201	-0.076
	(25.42)	(17.36)
Household characteristics:		
Land owned (ha)	-0.131	-0.05
	(16.12)	(10.58)
Caste (1=SC/ST)	0.119	0.068
	(23.60)	(23.95)
Religion (1=Hindu)	0.013	0.026
	(1.95)	(6.72)
At least one person with secondary education in household	-0.076	-0.036
	(10.65)	(10.32)
Sex of household head (1=male)	0.011	-0.004
	(1.03)	(0.84)
Age (yrs) of household head	0.000	0.000
	(1.15)	(2.00)
Literate head of household	-0.046	-0.035
	(6.76)	(12.52)
Log(household size)	-0.016	-0.017
	(2.20)	(4.41)
Share of kids(<15 yrs)	0.023	0.009
	(0.92)	(0.67)
Share of male adults(15-59 yrs)	0.003	-0.047
	(0.12)	(3.72)
Share of female adults (15-59 yrs)	0.038	0.028
	(1.42)	(1.86)
No. of observations	89893	88540
Percent correct predictions	76.3	85.5
Pseudo-R^2	0.23	0.27

Notes: Coefficients reported are *change* in probability at sample mean. Absolute t-values in parentheses, adjusted for clustering at the household level. All regressions include dummies for NSS regions, quarters, and interaction terms between quarters and state dummies. Omitted category is illiterate for individual education level dummies.

Table 4: Incidence of gains at alternative EGS wage rates

Quintiles of expenditure per person	Income gains and incidence of gains at EGS wage of:						
	25	30	35	40	45	50	55
I. Gain as a % of pre-EGS consumption:							
Poorest 20%	5.2	10.7	18.1	27.0	36.7	46.9	57.4
Quintile 2	2.5	5.4	9.6	14.8	20.6	26.9	33.4
Quintile 3	1.7	3.8	6.7	10.5	14.8	19.4	24.3
Quintile 4	1.1	2.4	4.3	6.8	9.6	12.8	16.2
Richest 20%	0.4	1.1	2.0	3.1	4.5	6.1	7.8
Overall	2.2	4.7	8.2	12.5	17.3	22.4	27.8
II. Distribution of gainers across quintiles:							
Poorest 20%	35.0	34.1	33.2	31.8	31.0	30.2	29.8
Quintile 2	24.3	24.4	24.7	24.8	24.6	24.5	24.5
Quintile 3	18.3	19.2	19.4	19.8	20.0	20.1	20.1
Quintile 4	14.2	13.9	14.2	15.0	15.5	15.7	15.9
Richest 20%	8.2	8.3	8.5	8.7	9.1	9.5	9.8
Overall	100.0	100.0	100.0	100.0	100.0	100.0	100.0
III. Percent of individuals in each quintile with positive gain:							
Poorest 20%	23.7	34.9	43.8	49.5	52.1	54.8	55.9
Quintile 2	16.4	25.0	32.6	38.7	41.3	44.4	46.0
Quintile 3	12.4	19.7	25.6	30.8	33.6	36.4	37.7
Quintile 4	9.7	14.3	18.8	23.3	26.0	28.5	29.8
Richest 20%	5.5	8.5	11.2	13.5	15.3	17.3	18.4
Overall	13.5	20.5	26.4	31.2	33.7	36.3	37.6

Table 5: Decomposition of the sources of targeting performance

Quintiles	Distribution of adults with imputed expected wage rate less than EGS wage rate (Rs/day) of:						
	25	30	35	40	45	50	55
Poorest 20%	25.5	23.0	21.2	19.6	18.7	18.0	17.4
Quintile 2	21.8	21.1	21.0	20.5	19.9	19.5	19.1
Quintile 3	19.1	19.5	20.3	20.5	20.4	20.2	20.1
Quintile 4	18.2	18.7	19.3	20.1	20.5	20.8	21.0
Richest 20%	15.3	17.7	18.2	19.4	20.4	21.6	22.5
Overall	100.0	100.0	100.0	100.0	100.0	100.0	100.0
	Distribution of adults willing to do casual labor *and* who gain at EGS wage rate (Rs/day) of:						
	25	30	35	40	45	50	55
Poorest 20%	31.5	30.5	29.5	28.1	27.3	26.6	26.2
Quintile 2	23.7	24.0	23.7	23.6	23.2	23.2	23.0
Quintile 3	19.4	20.0	20.3	20.6	20.8	20.8	20.8
Quintile 4	15.6	15.5	16.0	16.6	17.1	17.4	17.6
Richest 20%	9.9	10.1	10.6	11.1	11.6	12.1	12.4
Overall	100.0	100.0	100.0	100.0	100.0	100.0	100.0

Table 6: Estimated casual labor supply, as a percent of rural adult population

Quintiles of expenditure per person	Pre-EGS employment	Casual labor supply at EGS wage rate (Rs/day) of:						
		25	30	35	40	45	50	55
Poorest 20%	35.1	37.7	39.4	41.2	42.0	42.3	42.5	42.7
Quintile 2	29.0	30.7	32.0	33.5	34.3	34.6	34.9	35.1
Quintile 3	25.1	26.2	27.2	28.6	29.4	29.9	30.2	30.3
Quintile 4	20.5	21.4	22.2	23.4	24.2	24.7	25.0	25.2
Richest 20%	13.3	14.1	14.7	15.5	16.1	16.5	16.8	17.0
Overall	23.8	25.2	26.2	27.5	28.2	28.7	28.9	29.1

Table 7: Descriptive statistics for sample as a whole and predicted gainers from EGS

	Males			Females		
	All able-bodied adults	Gainers at Rs 50/day		All able-bodied adults	Gainers at Rs 50/day	
		All gainers	Switches into casual labor		All gainers	Switches into casual labor
Labor Force Participation Characteristics						
Share in labor force	0.88	0.97	0.89	0.42	0.89	0.48
Share employed as casual labor	0.31	0.73	0.00	0.16	0.79	0.00
Share employed as salaried labor	0.09	0.05	0.13	0.02	0.03	0.05
Share unemployed	0.11	0.13	0.13	0.04	0.10	0.06
Other Individual Characteristics						
Age (years)	32.0	32.3	32.7	32.2	33.5	34.1
Marital Status (1=married)	0.68	0.77	0.82	0.79	0.76	0.72
Education, below prim	0.12	0.15	0.16	0.09	0.07	0.05
Education, primary	0.14	0.11	0.09	0.10	0.05	0.02
Education, middle	0.19	0.10	0.04	0.11	0.03	0.01
Education, secondary	0.21	0.04	0.00	0.09	0.01	0.0
Household Characteristics						
Land owned (ha)	1.0	0.3	0.2	1.0	0.3	0.2
Caste (1=SC/ST)	0.32	0.52	0.61	0.32	0.57	0.71
Religion (1=Hindu)	0.86	0.88	0.90	0.85	0.92	0.93
At least one person with secondary education in h'hold	0.34	0.10	0.03	0.31	0.11	0.05
Sex of h'hold head (1=male)	0.96	0.96	0.96	0.9	0.84	0.79
Age of h'hold head (years)	45.6	42.2	40.9	45.8	42.8	41.82
Literate head of h'hold	0.53	0.31	0.22	0.51	0.28	0.18
Ln(household size)	1.7	1.6	1.6	1.7	1.5	1.4
Share of kids (<15 years)	0.3	0.33	0.34	0.32	0.32	0.32
Share of male adults (15-59 yrs)	0.37	0.35	0.34	0.29	0.27	0.25
Share of female adults (15-59)	0.28	0.28	0.28	0.34	0.36	0.39
Expenditure/person (Rs/day)	15.5	12.3	12.8	15.2	12.2	12.8
No. of observations	89893	20186	5668	88540	9585	1964

Table 8: Income gains at alternative EGS wages

	Non-Poor	Poor	All
I. Income gains amongst adults:			
Avg. gain (Rs/day) amongst those who benefit at:			
30 Rs/day	7.3	7.8	7.5
40 Rs/day	12.5	13.8	13.1
50 Rs/day	19.5	21.8	20.5
Percentage of adults who gain at:			
30 Rs/day	8.4	17.7	11.2
40 Rs/day	13.7	26.1	17.4
50 Rs/day	16.6	29.2	20.3
II. Income gains in the population:			
Avg. gain (Rs/day) per capita at:			
30 Rs/day	0.36	0.67	0.46
40 Rs/day	1.01	1.75	1.26
50 Rs/day	1.90	3.10	2.31
Percentage of population that gains at:			
30 Rs/day	14.9	31.3	20.5
40 Rs/day	23.7	45.7	31.2
50 Rs/day	28.7	51.0	36.3
Per capita expenditure (Rs/day)	17.7	8.7	14.6

Table 9: Poverty measures at alternative EGS wage rates

	Basic specification			Augmented specification		
	Headcount index (%)	Poverty gap Index (x100)	Squared poverty gap index (x100)	Headcount index (%)	Poverty gap Index (x100)	Squared poverty gap index (x100)
Pre-EGS	34.0	7.1	2.2	34.0	7.1	2.2
25	31.9	6.4	1.9	31.2	6.2	1.8
30	29.8	5.8	1.7	28.7	5.5	1.6
35	27.2	5.1	1.5	25.4	4.7	1.3
40	24.6	4.5	1.3	22.5	4.0	1.1
45	22.2	4.1	1.2	19.9	3.5	1.0
50	20.5	3.8	1.1	17.9	3.2	0.9
55	19.1	3.5	1.0	16.5	2.9	0.8

Notes: Poverty measures are based on the distribution of per capita expenditures from the 1999-00 Employment-Unemployment NSS survey and estimated using the Official Planning Commission poverty lines.

Table 10: Estimated labor supply to the EGS at alternative wage rates

EGS wage	Excess supply of labor as % of rural adult population		Estimated no. of EGS applicants on a typical day (millions):				Estimated cost
(Rs/day)	Males	females	males	females	Total	as % of total casual labor	(% of GDP)
25	8.0	8.0	12.8	11.4	24.2	25.5	1.6
30	9.9	8.8	15.5	12.4	27.9	28.4	2.2
35	12.4	9.0	19.9	12.7	32.6	31.2	3.0
40	14.0	9.2	22.6	12.8	35.4	32.9	3.7
45	14.9	9.2	24.1	12.9	37.0	33.9	4.3
50	15.5	9.2	25.0	12.9	37.9	34.6	4.9
55	15.9	9.2	25.7	12.9	38.6	35.2	5.5

Table 11: Impacts for an alternative un-targeted allocation of the same budget assuming 10% administrative cost

EGS wage rate (Rs/day)	Headcount index (%)	Poverty gap index (x100)	Squared poverty gap index (x100)
I. Gross EGS cost			
25	24.3	4.0	1.0
30	20.7	3.1	0.7
35	16.0	2.2	0.5
40	12.5	1.5	0.3
45	9.4	1.0	0.2
50	7.2	0.7	0.1
55	5.2	0.5	0.1
II. EGS labor cost			
25	28.1	5.1	1.4
30	25.8	4.5	1.2
35	23.1	3.7	0.9
40	20.5	3.1	0.7
45	18.1	2.6	0.6
50	16.0	2.2	0.5
55	14.2	1.8	0.4

Figure 1: Incidence of absolute gains at alternative EGS wage rates

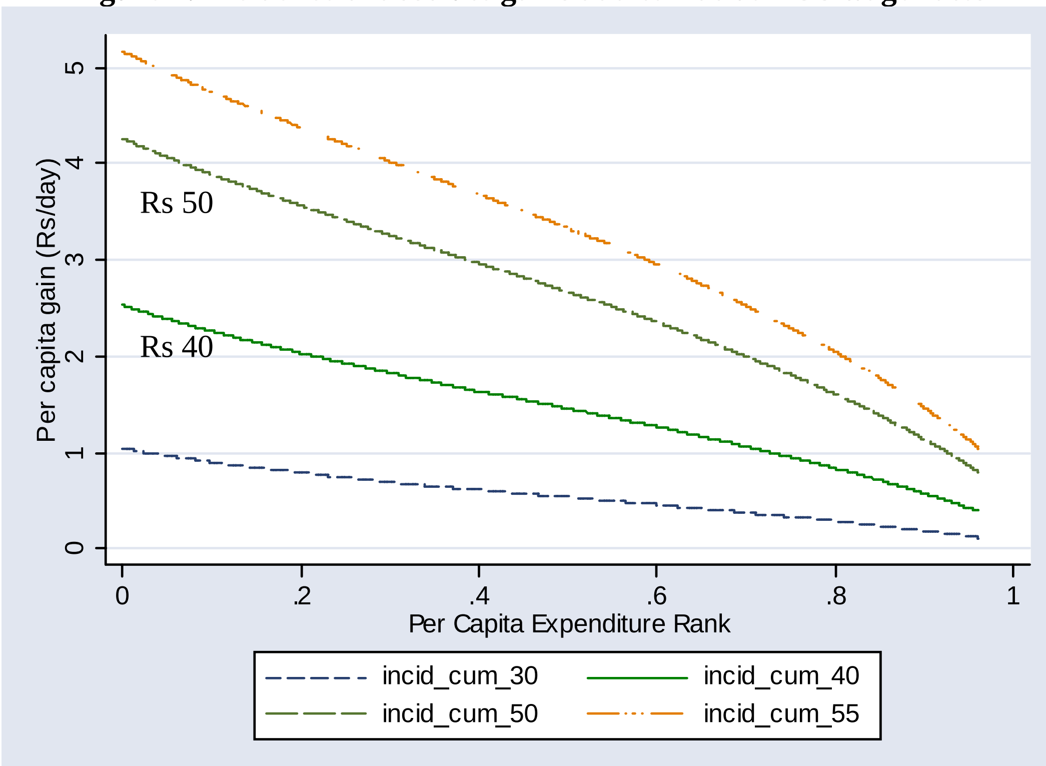

Figure 2: Incidence of proportionate gains

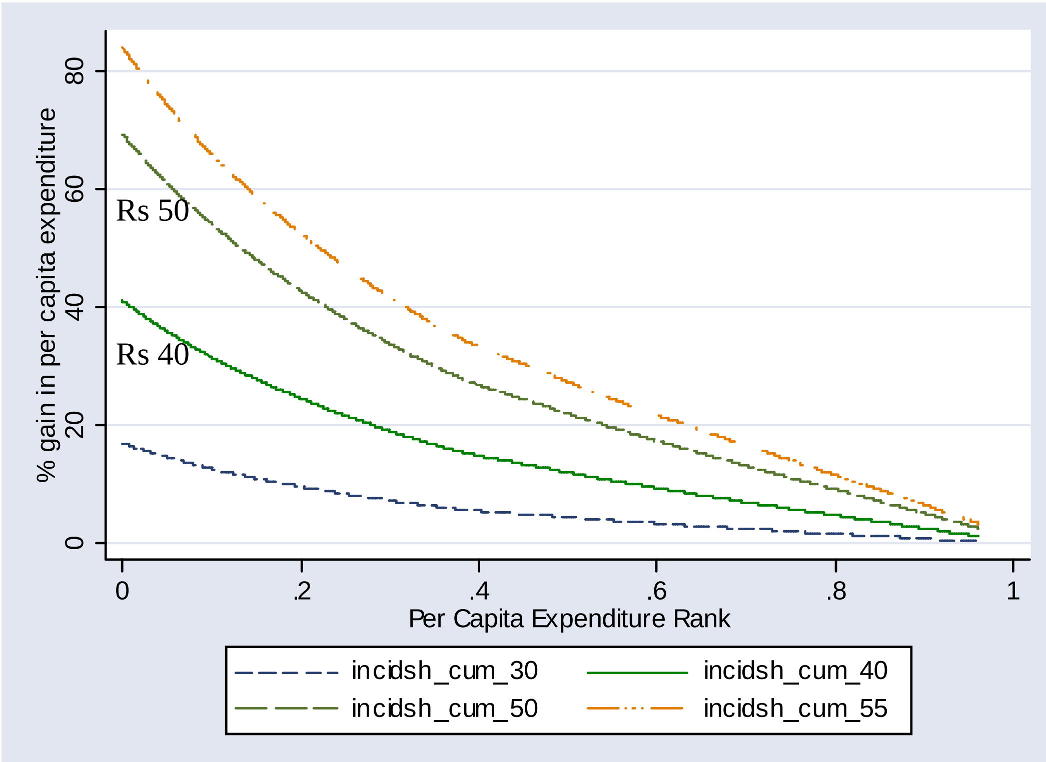

Figure 3: Cumulative distribution functions of consumption with and without EGS

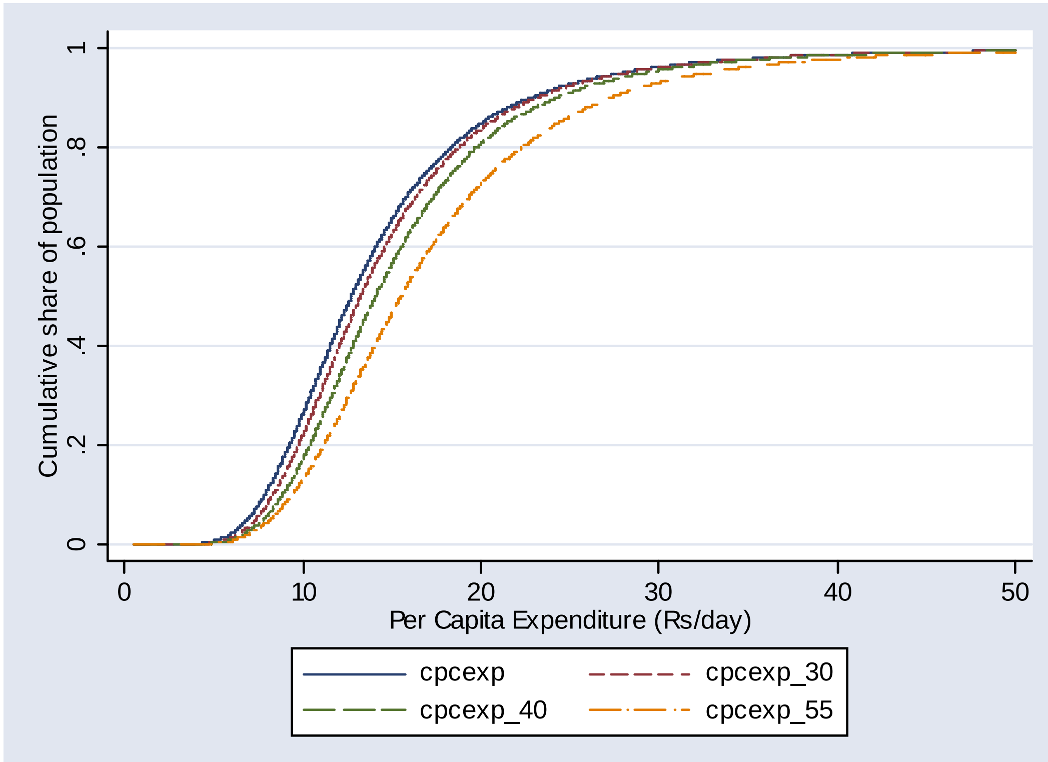

Figure 4: Proportionate gains for EGS compared to uniform transfers

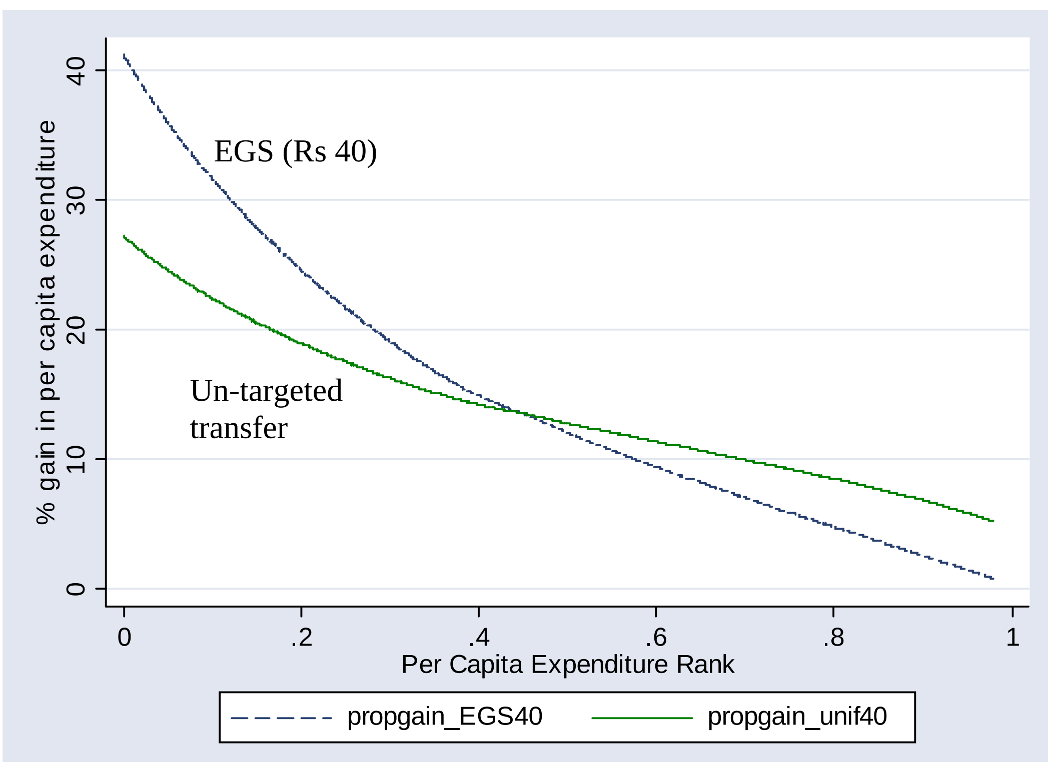

Figure 5: Cumulative distributions of consumption for EGS versus uniform transfer