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ON THE WELFARE GAINS FROM TRADEABLE BENEFITS-IN-KIND

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ABSTRACT

Governments often prohibit resale of the benefits-in-kind provided by antipoverty programs. Yet the personal gains from those benefits are likely to vary and to be known privately, so there can be gains to poor people from trading their assignments. We know very little about those gains. To help address this knowledge gap, the paper models a competitive market for assignments, and simulates the market using an unusual survey of workers on a rural public-works scheme in a poor state of India. The results indicate large gains from tradeable assignments after first randomizing. The gains exceed those from poverty targeting without trade and are not lower for poorer households or female workers. Fully realizing the gains from trade in practice may require complementary policies to help people access the market and to support its administration and regulation.

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1. Introduction

In-kind benefits have been popular in social policy making.[2] There is a (long-standing) question as to whether it would be better instead to provide the benefits as cash. Let us put that issue aside for the moment and take it as given that a benefit-in-kind (BIK) is provided. The problem is then how a limited number of BIKs is to be assigned across a designated set of eligible individuals. The policy maker cares about the aggregate disbursement of the BIKs (viewed as merit goods) but also cares about the welfare gains from the BIKs, and those gains undoubtedly vary. Thus, the inter-household allocation of BIKs matters.

What then does the policy maker do? With information about the gains to all individuals, one could simply target the BIKs based on that information, with the first BIK going to those with highest gains and so on until the available budget is exhausted. What makes the problem difficult in practice is that how the gains differ is in large part unknown to the policy maker, although the personal gains may be known reasonably well at the individual level.

Three examples illustrate the problem. For the first, consider a food distribution scheme, that targets discrete food rations to poor families. For some, the ration is no more food than wanted, given their food demand functions, but for others it is more than they want. Thus, the value of this BIK varies.

In the second example, consider a training program with only so many slots available. The wage gains from training vary. Some plausible covariates of the gains may be observable to the policy maker. However, crucial variables are not observable, such as latent ability, although one can expect them to be reasonably well known privately. The policy problem is how to allocate the limited number of slots, with little or no information about the individual gains.

The third example is a workfare scheme, providing extra work at a wage rate common to all. Here the BIK is the extra work, but not all who want that work can be accommodated. The gains to individuals who join the program vary, given differing forgone earnings from other available work. While each person probably has a fairly good idea of their best alternative at the time, this is not known to the policy maker in deciding how to ration the available jobs.

[2] On the rationales for BIKs see Moffitt (2006) and Currie and Gahvari (2008).

In practice, the policy maker assigns the BIKs based on the available information, including priors about what types of individuals are likely to gain the most. If the benefits were cash instead of BIKs then that would be the end of the story from the policy maker's perspective. When it is BIK, a new policy option is available: to allow trade in assignments. Yet, having assigned the BIKs, a common (indeed, near-universal) practice is to try to prevent people trading them, such as by stipulating that vouchers are non-transferable, and penalizing violations. It can be expected that there are welfare costs from such restrictions since some of those eligible have larger gains than others (as in the examples above). Removing the restrictions on trade would tend to re-allocate the BIKs toward those with larger latent gains, thus increasing the aggregate impact of the social policy.

Undoubtedly, most economists would be opposed to preventing mutually beneficial trades. Yet, non-economists and policy makers are often supportive of bans on trade in BIKs. Why? One response might be that the policy maker wants the initial recipient to consume the in-kind good. But why does it need to be the initial recipients? Surely, as long as nothing less than the initial supply of BIKs is consumed among the eligible set of people, there can be no problem. Indeed, without the re-sale option, some of the BIKs may end up being wasted, which does not help anyone. So, this does not seem to be a credible reason for preventing re-sale.

Another possible reason is that policy makers only expect a small cost to program participants from restricting trade. We do not know that the cost of preventing trade is "high" in any policy-relevant sense. Furthermore, whatever the rules may say, informal trades might keep the cost down in practice. We may find that there are only small differences in the remaining gains from a specific BIK across the eligible population, such that the gains from further trade are also small. This is a conjecture; instead, we may find that there is a high welfare cost.

Why does this knowledge gap exist? An important reason is that individual gains from trade are typically unobserved—they are private information, which is (indeed) why we can imagine potential gains from allowing trade after the initial assignment. The fact that gains are not fully observable has made it hard to quantify the costs of making BIK assignments non-tradeable, so as to better inform the public debate on whether the prevailing practice is the best approach. The knowledge gap exists for the same reason that it is a concern.

Another common concern about market-like solutions in social policy design is that the gains may be captured disproportionately by the well-off. By this view, allowing re-sale of BIKs may bring less benefit to initially excluded poor people. Weitzman (1977) showed that the gains from a market-based allocation mechanism depend on how much individual gains differ <u>and</u> on the extent of income inequality. If one judges that incomes are too unequally distributed then one can also be concerned that a market mechanism for social programs will only make things worse. Yet the literature provides counterarguments. Sah (1987) demonstrates that, for poor people, allowing rationed BIKs to be tradeable (which he calls "convertible rations") can dominate the other allocation mechanisms he considers. Furthermore, Che et al. (2013) show that a competitive market allocation for an assignable good can attain higher utilitarian social welfare if it is introduced in the wake of an initially random assignment.

Recognizing the concern that a quasi-market assignment runs the risk of being captured by the non-poor, this paper studies the properties and performance of the Che et al. (2013) "randomization-with-resale" assignment mechanism when the welfare outcomes are to be judged by the pecuniary gains to poor people. The paper characterizes the competitive market allocation of assignments to a program following an initially randomized assignment across a set of eligible people. This is the allocation we would observe if those eligible could freely trade. The model is key to the empirical analysis of the costs of restricting trade in BIKs. The model also carries some implications for the interpretation of randomized controlled trials (RCTs).

Based on this model, the paper simulates market allocations using a sample of surveyed workers in a large workfare scheme in India. The data studied here provide an unusual—indeed, unique (to my knowledge)—opportunity for addressing this issue, given that a plausible measure of the personal gains can be retrieved using survey data. The sample is treated as the universe from which artificial programs are simulated, consistently with the predictions of the theoretical model. The simulations are used to estimate the participant's mean monetary gains, interpretable as the impacts on the aggregate poverty gap, beyond what has been attained already by trade in assignments (legal or otherwise). Various counterfactuals are considered, including a "needs-based" assignment, based on household consumption expenditure per person, as widely used for measuring poverty in India.

This is also a setting in which we can learn about how the gains are distributed. The data come from a population of poor households; indeed, three-quarters come from families living below the World Bank's (frugal) international poverty line. However, they are not all equally poor—indeed, the inequality in household consumption per person is similar to rural India as a whole. Gender inequality is also an issue. The paper looks at heterogeneity along both dimensions.

The following section provides the theoretical model of the market for assignments, which carries the key insights needed for the subsequent empirical analysis. Section 3 applies the model to the survey data for workers on the public works scheme in India. The results indicate that allowing tradeable assignments increases the scheme's aggregate gains by a factor of 2 to 3. Similar gains are found when the comparison is with the needs-based assignment. It is found that a market-based assignment method yields large potential gains to both workers from poor families and female workers. Section 4 identifies some potential impediments to realizing the gains in practice. The impediments relate to deeper features of the market and institutional/governmental environment that can be thought of as being among the reasons why poverty exists in this setting. Complementary policies are identified that may be necessary to realize the potential gains to poor people of allowing tradeable BIK assignments. Section 5 concludes.

2. The market equilibrium with tradeable assignments

The theoretical problem is how to assign a lumpy BIK across a pre-determined set of eligible recipients when there is not enough for everyone. The BIKs are provided free of charge. The nature of the BIK is such that nobody would want a second, and it cannot be stored for later use. There is some fixed cost of creating the market for trading BIK assignments.

Let $D_i = 1$ if individual $i = 1, .., n$ (with n fixed) receives the BIK initially while D_i=0 if not, with mean $\bar{D} \equiv E(D)$, which we can call the coverage rate. There are both BIK recipients and non-recipients, so $0 < \bar{D} < 1$. There is a fixed number ($n\bar{D}$) of BIKs available (as determined by the budget), so $\bar{D}$ is exogenous. Using the (Neyman–Rubin) potential outcomes framework, whether or not an individual actually receives the BIK, one can define two numbers for all $i = 1, \dots n$, namely the outcome under the BIK, Y_{i1}, and that in the absence of it, Y_{i0}. The gain is $G_i \equiv Y_{i1} - Y_{i0}$, with cumulative distribution function $F(.)$ and mean $E(G)$. When it helps to simplify the analysis, G is treated as a continuous variable with a continuous (strictly increasing) distribution function on the support $[G^{min}, G^{max}]$.

The information structure is key. The individual gains from a BIK are unknown to the policy maker, given that it is hard to know outcomes in two different states of nature at the same time (analogously to the usual missing data problem in impact evaluation). Granted, we may have some observable characteristics deemed relevant (on *a priori* grounds) to the likely gains, and various methods of targeting based on observables have been used in practice. It is unclear how well any of this works in practice, given that we cannot test effectiveness in predicting the gains when they are unobserved. The information available for targeting social programs has proved to be especially deficient in poor countries.[3]

However, each person clearly knows a lot more about his or her own likely gain. Indeed, in some settings (including the examples in the introduction) it can be expected that each individual is reasonably well-informed about the G_i, and acts accordingly.[4]

In the literature on the standard "evaluation problem" the main task is usually to estimate the mean gain $E(G)$ (or a conditional mean of interest such as $E(G|D=1)$). No attempt is made to estimate the individual gains. The classic RCT randomly assigns the treatment and compares mean outcomes for those treated and those not. As is well known, under standard assumptions (including that there are no spillover effects, contaminating the controls), randomized assignment delivers an unbiased estimate of the mean gain (though, of course, any one trial will contain an experimental error).

Here we address a different problem: how should the program be assigned to maximize mean gain? Call this the "assignment problem." With perfect information, the solution is obvious: give the BIK first to the individual with G^{max} then to the next highest and continue until all the available BIKs have been allocated. Of course, information is far from perfect. The fact that G_i is typically unobserved by the policy maker naturally constrains applications. What should be done in practice?

[3] For example, in the context of targeting in Africa based on "proxy-means tests," see Brown et al. (2019).
[4] This specific information asymmetry is an example of what Heckman et al. (2006) call "essential heterogeneity."

When the policy maker knows nothing about the individual gains, a random assignment among those eligible has obvious appeal. But one can do better. Allowing trade clearly changes the assignment in an economically relevant way. Those who were assigned a BIK can sell their assignment at a price P. Assuming that the personal gain from the program is known to each person, the sellers will be those who receive the program initially but for whom $G_i < P$; they do better by selling it than keeping it. Buyers will be those who did not receive it initially, but with $G_i > P$.

The market equilibrium can now be characterized. Randomization justifies assuming that the distribution of gains is the same for those who are initially assigned the program and those not. The share of the population that received the BIK and want to sell at the price P is $\overline{D} \cdot F(P)$. The corresponding share who did not receive the BIK but want to buy an assignment at price P is $(1 - \overline{D})(1 - F(P))$. Let us further assume that $F\left(G^{min}\right) < 1 - \overline{D}$. (A sufficient condition for this to hold is that $F\left(G^{min}\right) = 0$ but a point mass at G^{min} is also allowed.) Then there is a positive excess demand for assignments at G^{min}. By definition $F(G^{max}) = 1$, so there must be a positive excess supply at G^{max}. Then, by continuity of $F(.)$, a unique equilibrium exists.[5] The market-clearing price solves $F(P) = 1 - \overline{D}$, i.e., the equilibrium price is the quantile of gains corresponding to the share of the population not receiving a BIK ($P = F^{-1}(1 - \overline{D})$).

There are four groups of people in this model:

1. The keepers: those assigned the BIK who do not want to sell it ($G_i > P$). The proportion of the population who are keepers is $\overline{D}\left(1 - F(P)\right) = \overline{D}^2$ (in equilibrium) and their mean gain is $E(G_i|D_i = 1, G_i > P)$.
2. The sellers: those selected initially who would rather sell their assignment ($G_i < P$). Their population share is $\overline{D}(1 - \overline{D})$ in equilibrium, with a mean gain of P.
3. The buyers: those initially excluded who expect a net benefit from buying access ($G_i > P$). Their population share is $\overline{D}(1 - \overline{D})$ (in equilibrium) and their mean gain is $E(G_i|D_i = 0, G_i > P) - P$.
4. The rest, with population share $(1 - \overline{D})F(P)$ and zero gain.

[5] Stability is assured under the usual condition that the price rises with excess demand and falls with excess supply.

Notice that, in equilibrium, the share of the population participating in the market ($2\bar{D}(1-\bar{D})$) does not depend on the distribution of the gains; the price does all the adjustment given $\bar{D}$. Differences in that distribution do, of course, matter to the size of the aggregate gains from allowing tradeable assignments.

Summing the gains across all four groups, weighted by population shares, the total gain per capita of the population when trade is allowed is:[6]

$$E(G_i) = \bar{D}^2 E(G_i|D_i = 1, G_i > P) + \bar{D}(1-\bar{D})E(G_i|D_i = 0, G_i > P) = \bar{D}E(G_i|G_i > P) \quad (1)$$

The first term on the LHS is the gain to keepers while the second term is the gain to the traders (the gain to sellers plus that to buyers).

Now compare this to the maximum attainable aggregate gain with perfect information. For that allocation, there will be some threshold gain, Z, above which everyone receives the BIK, and below which no-one receives it. (Z is determined by the number of BIKs available.) The mean gain is $E(G_i|G_i > Z)$ and $1 - F(Z) = \bar{D}$, which implies that $Z = P$, giving the same mean gain as the market equilibrium attains after the initial randomized assignment.

Thus, despite the policy maker knowing nothing about individual gains, the proposed assignment mechanism attains the first-best optimum with perfect information. The market improves over the randomized assignment, since the gains to those who buy an assignment ($(E(G_i|D_i = 0, G_i > P)$) must exceed the gains to those who sell one ($E(G_i|D_i = 1, G_i < P)$).[7]

A further comparison of interest is with the expected gain <u>without</u> trade, as given by $\bar{D}E(G_i|D_i = 1)$, which is the mean gain we would estimate using a RCT under standard assumptions (including no trade in assignments). The gain (per capita) from allowing trade is then $\bar{D}(1-\bar{D})[E(G_i|D_i = 0, G_i > P) - E(G_i|D_i = 1, G_i < P)] > 0$ (invoking randomization).

There is an implication here for the interpretation of RCTs aiming to evaluate the impact of BIKs using a pilot, to inform a government's decision about scaling up. It would seem unlikely that the pilot will be able to prevent trades in assignments, as this would require laws

[6] Note that the term in $\bar{D}(1-\bar{D})P$ drops out as it is a pure transfer between groups 2 and 3.

[7] Note that randomized assignment implies that $(E(G_i|D_i = 0, G_i > P) = E(G_i|D_i = 1, G_i > P)$ (given that randomization assures that the assignment is uncorrelated with the potential individual gains). Then the market improves upon the randomized assignment if $E(G_i|D_i = 1, G_i > P) > E(G_i|D_i = 1, G_i < P)$, which must hold.

and the power to enforce them. Yet, governments routinely prevent trade in assignments at scale, and have the required power. So, we can imagine a scenario in which there is more trade in assignments at the pilot stage than for the program at scale. Assuming that the mean gain is correctly calculated in the RCT, allowing for trade, the RCT will tend to over-estimate the impact of the scaled-up program, given that the gains from trade are lost on scaling up. The RCT will provide undue encouragement for scaling up.

This argument assumes that the induced change in assignments is observable to the evaluator. That might not be the case. Suppose instead that the evaluator ignores the spillover effect to the control group implied by trade (as it is unobserved) and simply calculates the mean gain for those treated based on observable incomes. This calculation will also over-estimate the mean gain on scaling up (without trade) since the evaluator will over-estimate the gains to the sellers (attributing a gain of P per seller instead of $E(G_i|D_i = 1, G_i < P)$). Again, the RCT will deliver an excessively positive conclusion for scaling up.

The above model can be adapted to allow stratification by categories of individuals defined by observed characteristics, taken as fixed. For example, this may be based on gender or a poverty map (showing poverty measures by area). The value of $\overline{D}$ is then allowed to vary by group, yielding different (group-specific) prices. The policy aims to maximize aggregate gains for each category, which then assures a maximum of any fixed-weighted aggregate gain.

3. Simulations of a market for public-works jobs in rural India

The rest of this paper implements the model in Section 2 for a sample of workers participating in India's Mahatma Gandhi National Rural Employment Guarantee Scheme (MGNREGS). This provides up to 100 days per year per household of unskilled manual work on rural public-works projects, at stipulated wage rates for the scheme. The scheme is essentially workfare, with a more-or-less explicit aim of reducing poverty by providing jobs. As is often the case, requiring people to work for poverty relief is seen to have intrinsic merit. There is also a classic self-targeting argument, namely that non-poor people will not want to do such work, and nor will poor people with preferred options.[8]

[8] One the incentive arguments for workfare versus cash transfers see Besley and Coate (1992) and Alik Lagrange and Ravallion (2018).

While the scheme is intended to be demand driven, there is evidence that the assignments are heavily rationed in practice, and more so in poorer states of India (Dutta et al. 2012, 2014; Desai et al. 2015). Using national survey data for 2010, Dutta et al (2012) report that, for India as a whole, 44% of those rural households who say that they wanted work on the scheme did not get it. In all but three of India's 20 larger states, the reported rationing rate was over 20%.[9] Furthermore, the rationing rate tended to be higher in states with a higher poverty rate. In one of India's poorest states, Bihar, the rationing rate was 79%; barely one-in-five of those workers who wanted work on the scheme got it. Ravallion (2020) identifies reasons why rationing of the available jobs on MGNREGS can emerge as an equilibrium in the local political economy and argues that the conditions for this to occur are more likely in poorer states.

The scheme allows (implicitly) its assignments to be transferred within households. One sees signs of such intra-household "work-sharing." On visiting worksites and interviewing workers, I found that families often make joint decisions about participation in the program, with the clear intention of increasing the net income gain to the family as a whole by assuring that extra work opportunities go to family members with lower forgone earnings. This is consistent with the econometric model of intra-household time allocation in Datt and Ravallion (1994), using data related to an antecedent program to MGNREGS, in the state of Maharashtra. One cannot rule out the possibility that some <u>inter</u>-household trade in assignments is also occurring, but it is naturally harder to observe. So, what is being measured here can be interpreted as the remaining, unexploited, gains from competitive inter-household trade in assignments.

Survey of workers on MGNREGS: The survey was done in two rounds over 2009/10 and is described more fully in Dutta et al. (2014).[10] The simulations implementing the model in Section 2 are only possible for the surveyed sample of existing workers under the scheme. This is clearly a selected sample, rather than being representative of rural India, or even rural Bihar. 75% of this sample of workers live in households with consumption per person below the World Bank's international poverty line of $1.90 a day, at 2011 Purchasing Power Parity.[11] This is well

[9] Evidence of rationing is also reported by Ravallion et al. (1993) for the antecedent programs to MGNREGS, in the state of Maharashtra.
[10] Workers' surveys in the two rounds are pooled, but standard errors are adjusted upwards by $\sqrt{2}$ to allow for re-surveying the same workers in different rounds. Since not all were re-surveyed, this adjustment is conservative.
[11] The consumption aggregate used here follows the same methods as for India's National Sample Survey.

above the corresponding poverty rate for rural India in the same year, which was 36%. That said, the workers in the sample are not all "equally poor." For example, the Gini index of household consumption per person among the surveyed workers is 0.27, which is only slightly lower than the corresponding Gini index for rural India at this time of 0.29 (Himanshu 2019).

The fact that this is a selected sample does not, of itself, reduce interest in these calculations. Social programs typically identify an eligible group of participants, as identified by poverty proxies or criteria such as employment status. However, creating a market in BIK assignments can generate new incentives for being declared eligible, with implications for policy design. Section 4 returns to this issue in the context of the application studied here.

Surveyed MGNREGS workers were asked to report both their wages under the scheme and to estimate their forgone earnings, i.e., how many days work they think they would have found and at what daily wage rate. In this setting, the participants are likely to have a good idea of their options. Dutta et al. (2014) found that the answers given accorded well with prevailing earnings from the casual (mostly part-time) work available at the time. Response rates to the questions on forgone earnings were high (92% and 98% in the two survey rounds). These questions were clearly no more difficult than the more familiar "objective" questions. The most common response to the question on what activity would have been forgone was "casual labor," which was the answer given by 42% of the respondents. This was casual manual work for a local landowner or some similar, relatively un-skilled, non-farm work (18% of respondents gave "casual agricultural labor" as their response, while 24% gave "casual non-agricultural labor"). "Work on own land" was the next most common (23%), followed by "remain unemployed" (19%) and "search for work" (14%). Very few (0.3%) of the respondents said that they "don't know" what activity they would have been doing.

The survey only allows us to measure the <u>monetary</u> gain from obtaining a job on the scheme. There may be non-pecuniary gains or losses that are not being picked up. The work available on MGNREGS is manual labor that is very similar to the type of casual work normally available in this setting. So, one would not expect much difference in non-pecuniary aspects related to the work itself. (Possibly the fact that the MGNREGS work is for the government makes it more attractive, though that is a conjecture at best.)

Thus, in this setting, we have credible self-reported data on the individual gains, as given by the actual earnings less forgone earnings (both reported).[12] This is very unusual; we rarely have data on the individual gains from social programs, and only aim to estimate the mean gain. Furthermore, the gains are obviously known to participants, and it would seem reasonable to assume that they are the relevant gains if trade in assignments had been allowed. (Since the survey asked actual participants, it is not likely that there would be an incentive to under-report forgone earnings to help gain access to the program.) Table 1 provides summary statistics on the gains, expressed as a proportion of the overall mean wage rate.

Simulations of the market and comparisons with policy options: Applying the model of Section 2, let $W_i(1)$ denote the wage received by worker *i* when participating in the scheme while $W_i(0)$ is her forgone earnings while on the program, so $G_i = W_i(1) - W_i(0)$ for all i. A worker who receives an initial (random) assignment will sell it if $G_i < P$, or (equivalently) her income if she sells, $W_i(0) + P$, exceeds that if she does not, $W_i(1)$. A worker who did not get assigned to the program initially will buy one if $G_i > P$ (or, equivalently, her income if she buys, $W_i(1) - P$, exceeds that if she does not, $W_i(0)$). The value of P clears this market.

One issue is whether the gain should be measured by the total wages received net of forgone earnings (reported forgone daily wage times days of work forgone) or the daily net wage rate. The latter is total wages received under the program less forgone earnings, both normalized by the total days worked on the scheme. In this setting, the daily net wage rate seems more relevant as the space for defining the price of a BIK assignment, leaving each individual to determine days to be worked. That is how the following calculations are done.

Figure 1 plots the conditional means over the range of gains, i.e., $\varphi(X) \equiv \hat{E}(G_i|G_i > X)$ for $X \in [G^{min}, G^{max}]$. (Some high values are dropped as the sample sizes become too small to be considered reliable.) To aid interpretation, the gain is expressed as a proportion of the overall sample mean wage rate (84.28 INR per day in 2009/10 prices). A key number to focus on for now is the sample mean gain, $\bar{G} = 0.393$ (s.e.=0.017; N=2307), meaning that the average gain from the existing assignment to the program represents just under 40% of the mean wage rate.

[12] There are lags in actual wage receipts (Dutta et al. 2014, Chapter 4). I include wages owed. There were some cases where forgone earnings exceeded wages received (or owed). These were treated as measurement errors (probably reflecting some misunderstanding of the survey question); the net gain was then set to zero.

This is the status quo of the existing scheme, or, in expectation, the mean for a random subsample. We see that the conditional mean rises sharply once one includes the positives ($\hat{E}(G_i|G_i \geq 0) = 0.393$ but $\hat{E}(G_i|G_i > 0) = 0.644$). In the positive range, the conditional mean rises roughly linearly with X.

The implied values of the equilibrium price and expected mean gains with the "randomization-with-resale" allocation are found in Table 2 for selected coverage rates ranging from $\overline{D} = 0.10$ to $\overline{D} = 0.50$. Recall that the equilibrium price is $P = F^{-1}(1 - \overline{D})$, i.e., the value of gains below which one finds $1 - \overline{D}$ of the workers.

We see that allowing tradeable assignments substantially increases the mean gains relative to the status quo. The average gains in Table 2 are 2 to 3 times higher (depending on the scale of the hypothetical program). Naturally, the mean gain per participant rises as one reduces the overall coverage rate since the BIKs tend to be picked up by those with higher gains.

Turning to the "needs-based" counterfactual, the obvious criterion is household consumption per person, as used for measuring poverty in India.[13] Table 3 gives mean gains for the same coverage rates, but now assigning from the lowest consumption per person upwards until the coverage rate is met. (Table 3 also gives the required thresholds.) We see that the "poverty-based" allocation achieves slightly higher mean gains than the actual mean ($\bar{G}$ =0.393), but that it falls far short of the gains attainable with the "randomization plus resale" policy.

A common policy option to workfare is cash transfers. Starting from the same assignment and with the same budget, tradeable BIKs can attain different welfare outcomes to cash transfers. The interesting question in this context is how the policy option of allowing BIKs to be traded alters the evaluative comparison of workfare versus cash transfers. Murgai et al. (2016) provide revenue-neutral comparisons of MGNREGS in Bihar with hypothetical alternatives using cash transfers. They show that, on taking account of forgone earnings and other costs in implementation, the workfare scheme does no better than a (revenue-neutral) universal basic income or transfers paid to those holding a ration card targeted to the poor, known as the "Below Poverty Line" card. The outcomes for poverty are essentially the same across these options.

[13] In practice this would probably be based on some form of proxy-means test, which would add extra errors of targeting; see, for example, Brown et al. (2019).

Since the comparison is a tie, the extent of the increase in mean gains found here when trade in assignments is allowed suggests that this change in policy design would tilt the balance in favor of the workfare scheme. Removing the current restrictions on transferability of assignments would make this a more effective antipoverty policy than (revenue-neutral) cash transfers.

Heterogeneity in the gains: A key aspect of heterogeneity in this context is the extent of poverty. We see from Figure 2 that the conditional mean gains tend to fall with higher household consumption, although the (non-parametric) regression line is being pulled up at the bottom by a few outliers. It is clear, however, that the mean gains from the market assignment are no lower for those workers coming from households living below the median (Table 2, Columns (5) and (7)). The potential gains are spread through the distribution of living standards.

Gender differences are also of interest. 29% of the sampled workers are women. The overall mean gain is slightly lower for women. The relationship with household consumption is similar by gender, though the tendency for the gains to fall as household consumption per person rises tends to be less evident for women (Figure 2(b)). When one calculates mean gains separately by gender (conditional on gains exceeding the market-clearing price), the differences are small at all levels of coverage (Table 2).

The gender split in Table 2 assumes a common market for both genders. Instead, one might split the market by gender (with trades only allowed among the same gender). This raises the market-clearing price for men, and lowers it for women, while the aggregate gains remain similar to Table 2. For example, at $\overline{D} = 0.50$, the equilibrium price rises to 0.31 for men and falls to 0.18 for women, with mean gains of 0.760 and 0.724 respectively. The population weighted mean gain across genders is 0.750 (Table 2).

4. Impediments to realizing the gains in practice

The evidence presented above is at least suggestive of large costs to poor people of preventing trade in BIK assignments. However, there are reasons why the gains from allowing trade may not be fully realized in practice. The reasons relate to market and institutional impediments that also play a role in creating poverty in the first place. Thus, realizing the potential gains from allowing tradeable assignments may require complementary policies.

Four main concerns can be identified. The first relates to credit-market imperfections, such that poor potential beneficiaries simply cannot afford to purchase a BIK assignment. Finding that the mean gains are no lower for poorer household does not, of course, imply that poor households who did not get an initial assignment could afford to buy one. Given liquidity constraints, the benefits may still be disproportionately captured by the relatively well off.

In support of this claim, Figure 3 gives the market price as a share of monthly consumption for a family of five. The cost rises to a (clearly) prohibitive share of consumption among poor families. Even the average shares for those living below the median are high; at a coverage rate of 0.5, the purchase cost is a little over 20% of consumption; it rises to over 80% at a coverage rate of 0.1. (Column 6 of Table 2 gives the share of monthly consumption for those living below the median and the standard errors.) If the government insists on full payment of the market price up-front this could be a prohibitively large share of consumption for poor households.

One possible policy response is to introduce a "pay-as-you-go" option for those who do not receive the program in the initial assignment but would benefit from purchasing it. Given that this is a government-regulated market, the problem could be readily solved by introducing such an option. Granted, one can anticipate resistance to such a step, as it can be interpreted as a lowering of take-home wages for some. That concern would need to be weighed against the potential benefits to poor workers in gaining access to the program.

Second, there will be scope for less liquidity-constrained, non-poor, people to participate in the new market for BIKs. This is a concern, though it could also be addressed by policy design features. In the context of explicitly targeted programs, one may want to maintain or even tighten existing eligibility criteria. The norm in antipoverty and other social programs appears to be under-coverage of those deemed eligible, with rationing among the set of eligible participants.[14] While the results of this paper indicate large potential gains from allowing trade among those eligible, there would be new risks in substantially expanding the set of those eligible since the potential gains from trading assignments would attract more affluent participants not facing the same liquidity constraints on participating in the market.

[14] For an overview of evidence on this point see Ravallion (2017).

Another policy to address this second concern in the context of self-targeted programs (such as the scheme studied here) is to introduce a minimum participation threshold. While the self-targeting mechanism will tend to discourage participation by the well-off, creating the market option could well impact selection into the program—attracting "speculators" and middlemen who do not intend to work, but only re-sell their assignment if they get one. However, one can avoid this by requiring a sufficiently long initial period of work before re-sale is available as an option.

Third, there are impediments to the flow of knowledge in this setting. Dutta et al. (2014) also surveyed participants' knowledge about MGNREGS in Bihar. Most of the sample (including over 90% of men) had heard of the scheme, but knowledge about the scheme's rules and provisions was poor, especially for women. The above calculations may overstate gains to women relative to men.

Realizing the benefits from removing restrictions on trade requires that individual participants are reasonably well-informed about their personal gains. Otherwise, there can be no presumption that allowing resale will achieve an allocation that is any better than the status quo or randomization on its own. Creating a market for BIK assignments would presumably change the incentives for seeking and spreading information on the scheme. Even so, information dissemination efforts would probably be needed, to complement a switch to tradeable assignments in social programs.

There is evidence from the same setting that external intervention through "infotainment" can enhance individual knowledge about this program and (hence) the individual gains. Ravallion et al. (2015) report results from their RCT using an entertaining movie (produced for this purpose) to teach people their rights and the rules and administrative procedures under the scheme. The movie did enhance knowledge when assessed by a quiz given before and after seeing the movie, and especially so for women. However, in studying the impacts of this RCT, Alik-Lagrange and Ravallion (2019) find evidence of social frictions on information dispersal within villages—frictions that disadvantage lower caste and poorer individuals.

The fourth concern is whether the administrative capacity will be present in poor places to implement an efficient, largely corruption free, secondary market in BIK assignments. This is no small matter. MGNREGA does have a quite sophisticated (public-access) web-based information system, and it would seem plausible that the software to support a market for assignments could also be developed, preferably integrated with the existing information system.[15] However, what actually happens on the ground could deviate appreciably from the program's formal operating rules. Dutta et al. (2014) identify a number of administrative performance problems in poor areas of India that make it hard for the scheme studied here to attain its potential impact on poverty even without allowing tradeable assignments. Ravallion (2021) explains how rationing of jobs under MGNREGS could emerge in equilibrium given local administrative costs in implementation and the scope for corruption by local leaders. These features can also be thought of as examples of institutional failures that create poverty in the first place. Of course, enhancing public administrative capacity is an important element of development policy more broadly—a channel that is relevant to the efficacy of a wide range of policies.

5. Conclusions

Governments often strive to prevent trade in assignments to antipoverty programs, despite the lack of evidence on the welfare costs of doing so. Letting well-informed people trade their assignments of benefits-in-kind can help programs attain the maximum aggregate gain with perfect information even when the policy maker is ignorant of the individual gains. The existence and relevance of private information is clearly one of the reasons why we know so little about how much poor people might lose from such restrictions. This also has implications for how we interpret evaluative evidence on the impacts of antipoverty programs.

Based on a theoretical model of a competitive market for assignments, the paper has offered some evidence on the costs of preventing trade for a large antipoverty program in India. The program aims to reduce poverty by providing jobs on labor-intensive public works projects. A novel feature of the setting is that it is feasible to use surveys to measure individual pecuniary gains from the jobs provided, even though this information is unlikely to be available for implementing a scheme of transferable in-kind benefits. The surveyed workers were asked about both actual wages received and labor-market options at the time. Using these data, the paper has

[15] The existing information system does not record the extent of rationing under the scheme. Under the scheme's rules, the state government is responsible for paying an unemployment allowance if work cannot be found. This gives the state government a strong incentive to report that all requests for work were honored. That is inconsistent with what has been observed based on surveys (Dutta et al. 2012, 2014; Desai et al. 2015).

simulated various stylized schemes of randomized assignment with resale, at different levels of coverage, and studied the gains from such a policy, including their incidence by household levels of living and the gender of workers.

The calculations indicate that there are large unexploited gains to poor people from allowing trade in work assignments. A competitive market for tradeable assignments would generate aggregate gains that are around 2 to 3 times the current mean gains to these (mostly very poor) families. It would also have greater impact (by a similar magnitude in terms of mean gains) than an allocation without re-sale options targeted to workers from consumption-poor families. Allowing the assignments to be tradeable in this setting can also make workfare more effective against poverty than (budget-neutral) cash transfers.

The results do not suggest that the gains from the market-based assignment would tend to favor more affluent families; the mean gains were very similar between the poorest half in terms of household consumption per person as for the rest. The simulated allocations with tradeable assignments imply a tendency for somewhat larger gains among poorer households, not the opposite. Gains are similar between male and female workers.

The paper has also pointed to some likely impediments to fully realizing these gains in practice, including credit market failures, capture by non-poor speculators, and imperfect information. There are complementary policies that may well be needed to realize the gains to poor people from allowing tradeable assignments. A "pay-as-you-go" option could help address liquidity constraints stemming from credit market imperfections. Eligibility criteria and minimum participation requirements (to encourage self-targeting) could help dissuade non-poor speculators and middlemen from entering the market. And information campaigns are clearly desirable, and not so hard to do. Limitations on local-level administrative capacity and the (related) scope for corruption also warn for caution. The same features impede many other aspects of economic development.

As is often the case, success in one policy effort—in this case assuring that the benefits to poor people of an antipoverty program are fully realized—may require success on multiple fronts.

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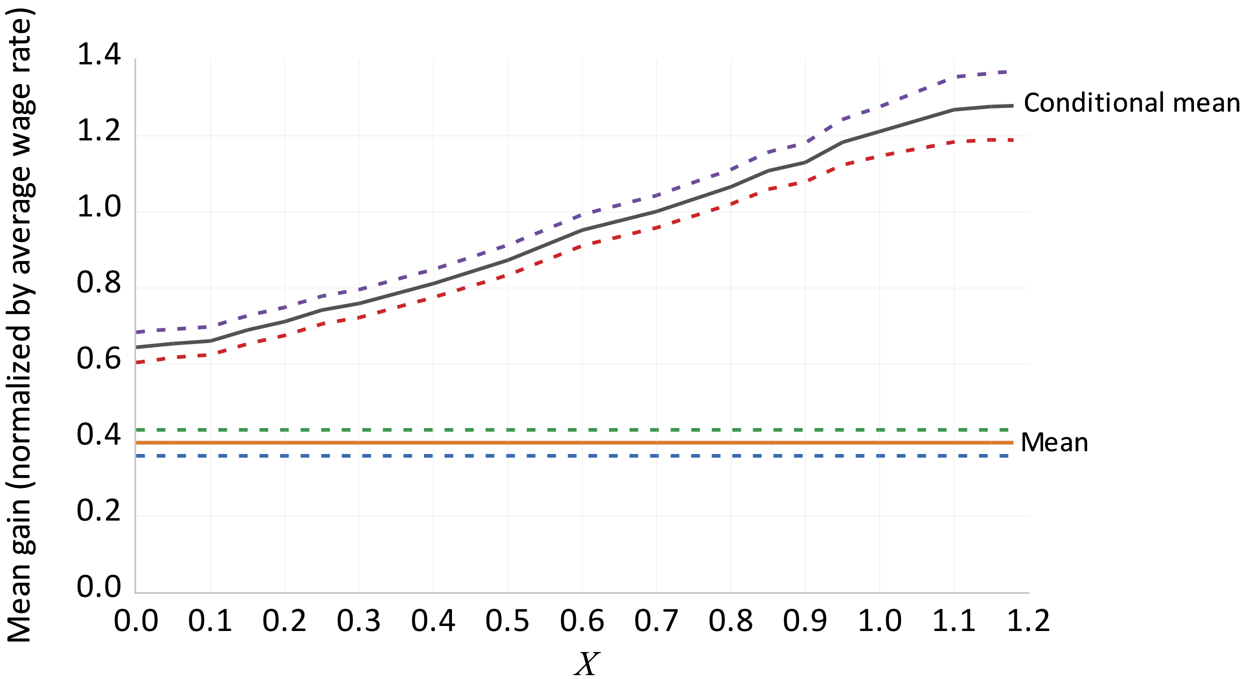
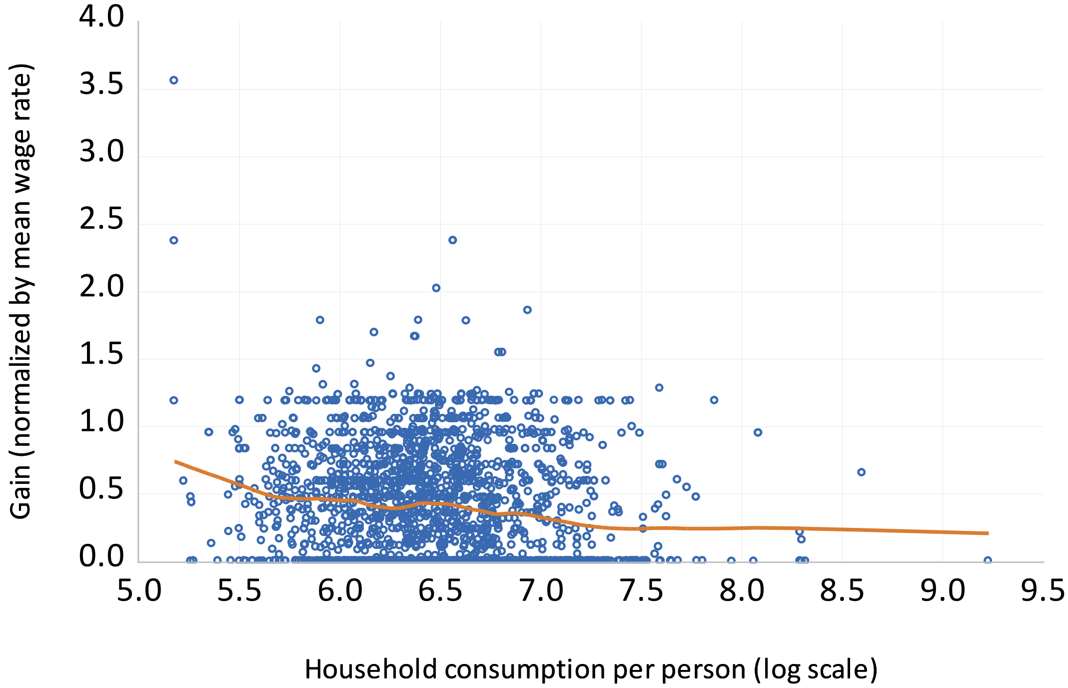
Figure 1: Conditional mean gains ($\hat{E}(G_i|G_i > X)$)



Note: Gain from employment in the National Rural Employment Guarantee Scheme in Bihar, India. The gain is normalized by the overall mean wage rate (84.28 INR per day). Lower and upper bounds of the 95% confidence intervals. Standard errors are clustered at the household level and scaled up by a factor of $\sqrt{2}$ to allow for the re-surveying of individuals across rounds.
Source: Author's calculations from the survey data collected in two rounds, 2009-10, by a team including the author and documented in Dutta et al. (2014). N=2307 after deleting 17 implausibly high outliers (exceeding 400 INR per day).

Figure 2: Gains plotted against household consumption per person

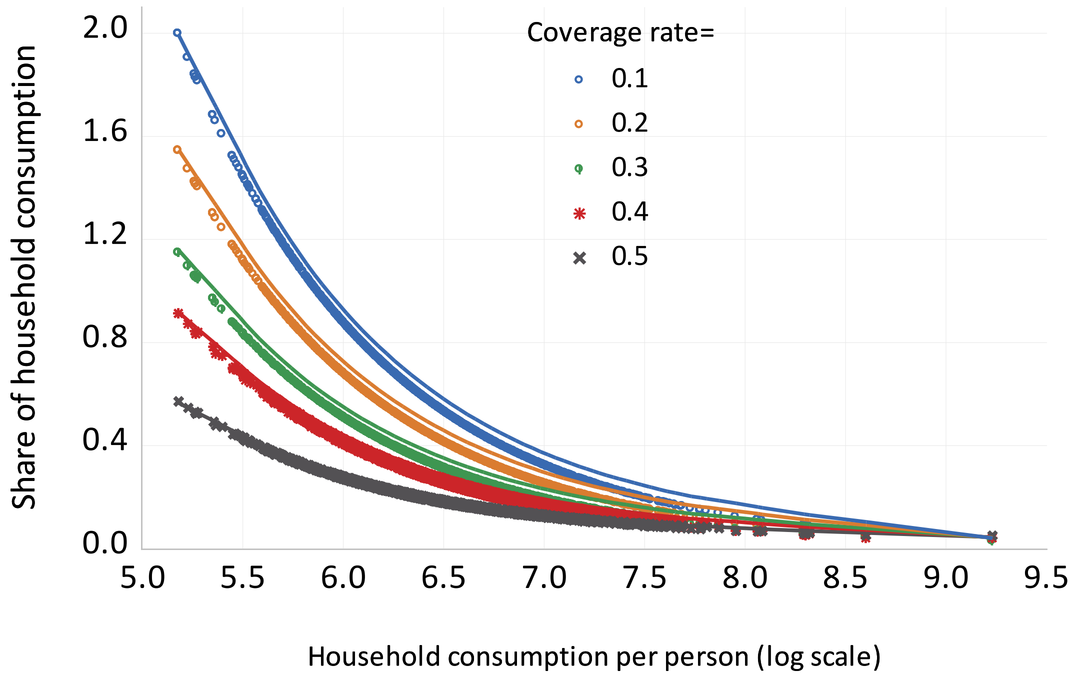
(a) All workers



(b) Split by gender

Note: See Figure 1. Nearest neighbor smoothed scatter plot.
Source: See Figure 1.

Figure 3: Market price of assignments as a share of household consumption



Note: Share of consumption calculated for a household of five people with one person selling an assignment for 20 days per month. Nearest neighbor smoothed scatter plots.
Source: See Figure 1.

Table 1: Summary statistics on monetary gains from jobs on a workfare scheme in Bihar, India

	Sample mean	Standard error	N
Gain	0.393	0.017	2307
Gain for workers in households with below median consumption per person	0.436	0.024	1150
Gain for workers in households with above median consumption per person	0.350	0.031	1151
Gain for female workers	0.363	0.031	669
Gain for male workers	0.406	0.020	1638
Gain for female workers in household with below median consumption per person	0.435	0.047	315
Gain for male workers in household with below median consumption per person	0.437	0.027	835
Gain for female workers in household with above median consumption per person	0.299	0.037	352
Gain for male workers in household with above median consumption per person	0.373	0.028	799

Notes: Mean gains (wage rate less forgone earnings) normalized by overall mean wage rate. 17 extreme values for the gains (exceeding 400% of mean wage rate) are dropped. Standard errors are clustered at the household level and scaled up by a factor of $\sqrt{2}$ to allow for the re-surveying of individuals across rounds.

Table 2: Mean gains from competitively tradeable assignments

(1) Coverage rate ($\overline{D}$)	(2) Market-clearing price (P)	(3) Pop. share trading	(4) Mean gain for treated ($\hat{E}(G_i \mid G_i > P)$)	(5) Mean gain for those below median consumption	(6) Share of consumption for below median household	(7) Mean gain for those above median consumption	(8) Mean gain for female workers	(9) Mean gain for male workers
0.5	0.28	0.25	0.750	0.736	0.217	0.767	0.757	0.748
			(0.018; 1154)	(0.024; 654)	(0.004; 654)	(0.028; 497)	(0.030; 312)	(0.030; 842)
0.4	0.47	0.24	0.845	0.826	0.363	0.869	0.827	0.852
			(0.018; 920)	(0.024; 522)	(0.007; 522)	(0.030; 395)	(0.030; 262)	(0.023; 658)
0.3	0.60	0.21	0.952	0.944	0.463	0.963	0.931	0.961
			(0.021; 677)	(0.027; 367)	(0.011; 367)	(0.030; 307)	(0.028; 190)	(0.025 487)
0.2	0.81	0.16	1.068	1.059	0.642	1.078	1.043	1.078
			(0.023; 465)	(0.030; 249)	(0.020; 249)	(0.033; 214)	(0.025;129)	(0.030; 336)
0.1	1.05	0.09	1.215	1.217	0.834	1.214	1.188	1.225
			(0.033; 244)	(0.042; 126)	(0.038; 126)	(0.047; 117)	(0.018; 63)	(0.044; 181)

Notes: Mean gains normalized by overall mean wage rate. The status quo mean is 0.393 (s.e.=0.017). Equilibrium prices calculated numerically to nearest second decimal place. Share of consumption (column 6) calculated for household of five people with one person selling an assignment for 20 days per month. Median consumption per person=INR 647 per month. Standard errors in parentheses, followed by sub-sample size. Standard errors are clustered at the household level and scaled up by a factor of $\sqrt{2}$ to allow for the re-surveying of individuals across rounds.
Source: See Figure 1.

Table 3: Mean gains from needs-based versus market-based assignments

Coverage rate ($\bar{D}$)	Cut-off point for consumption (INR/person/month)	Mean gain for needs-based allocation	Mean gain using market (Table 1)
0.5	646	0.436	0.750
		(0.024; 1145)	(0.018; 1154)
0.4	580	0.438	0.845
		(0.027; 925)	(0.018; 920)
0.3	515	0.453	0.952
		(0.032; 682)	(0.021; 677)
0.2	447	0.468	1.068
		(0.042; 463)	(0.023; 465)
0.1	372	0.472	1.215
		(0.058; 230)	(0.033; 244)

Notes: Mean gains normalized by overall mean wage rate. The needs-based allocation goes to the poorest households based on consumption per person, with the cut-off determined by the coverage rate. Standard errors in parentheses, followed by sub-sample size. Standard errors are clustered at the household level and scaled up by a factor of $\sqrt{2}$ to allow for the re-surveying of individuals across rounds.
Source: See Figure 1.