Is Workfare a Good Anti-Poverty Policy? An Assessment Based on Household Welfare, School Enrollment, and Program Expenditures*

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Abstract

A workfare program is a common anti-poverty policy in developing countries, which hires the unemployed for public construction work. While other anti-poverty policies such as cash transfer programs select participants based on a proxy-mean test, participants self-select into a workfare program, which could improve the efficiency of the program and participants' welfare by reducing inclusion and exclusion errors. On the other hand, a workfare program induces school dropout among the participants' children, which would potentially increase poverty rates in the future. This paper evaluates a large workfare program in India against hypothetical cash transfer programs by quantifying these aspects. Our empirical results show that under the equivalent program expenditures, the workfare program increases household welfare and enrollment rates less than the cash transfer programs. We further show that to achieve the levels of household welfare under the cash transfer programs, the workfare program needs to yield unreasonably high rates of social returns, suggesting that the cash transfer programs are preferred in terms of our evaluation metrics.

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1 Introduction

Workfare programs have been adopted as an anti-poverty policy across developing countries. Workfare programs aim to reduce poverty by employing the poor. Poor households who want to work usually select into the programs and engage in unskilled manual labor. This self-targeting design should increase the programs' cost-effectiveness by improving the accuracy in targeting the poor. Targeting is a vital aspect of anti-poverty policies because the tax revenues to finance them are scarce in developing countries due to large informal sectors (Hanna and Olken 2018). Thus, workfare programs should outperform other policies that require the central governments to target the poor in this regard.

Notwithstanding such a financial advantage, workfare programs can cause an unintended increase in school dropout among children whose parents are the programs' participants (Dammert et al. 2018). If returns to education are positive, then workfare programs may decline their wages in the future, which implies that workfare programs reduce today's poverty by increasing future poverty. This intertemporal trade-off contradicts the fact that workfare programs serve as anti-poverty policies. Therefore, the central government needs to consider potential long-term consequences when choosing a type of anti-poverty policy to implement. The natural questions are whether the cost-effectiveness of workfare programs outweighs the unintended effect on children's education, and for comparison, what the relationship is in other anti-poverty policies.

We take those questions to the most extensive workfare program in the world, the National Rural Employment Guarantee Scheme (hereafter NREGS) in India. The Government of India initiated NREGS in 2006 to provide with households in rural India employment up to 100 days every year, with a reservation of one-third of employment for women to encourage female labor participation. NREGS is an excellent context for our study for several reasons. First, NREGS is self-targeting. Every household in rural India can apply for employment under NREGS at any time in a year. The only eligibility conditions are that the participants are equal to or older than 18 and live in rural areas. Second, previous research finds the negative effects of NREGS on school enrollment. For instance, Shah and Steinberg (2019) find children from ages 5 to 17 in the districts where NREGS was in place were two percentage points less likely to report that schooling is the primary activity, and four percentage points more likely to do that working is the primary activity. Finally, there is suggestive evidence that in India, low-income households demand education, and returns to education are positive (Kingdon 2007). Thus, the continuation of schooling is a crucial tool for escaping poverty in India.

Our analysis is to study the performance of NREGS in terms of cost-effectiveness and adverse effects on school enrollment, compared with alternative anti-poverty policies. The main empirical challenge is that there were no other anti-poverty policies implemented simultaneously with NREGS, so it is impossible to compare NREGS with them directly.[1] We address this problem by estimating a behavioral model of the household under NREGS and simulating the model under counterfactual policies. Then we evaluate those policies based on household welfare, school enrollment, and public expenditure.

We consider two alternative anti-poverty policies that are popular in developing countries; a cash transfer program conditional on school enrollment (CCT) and an unconditional cash transfer program (UCT). Interestingly, which policy among them surpasses the others in those three dimensions is ex-ante ambiguous. CCT should be more effective in increasing enrollment rates than the others because it incentivizes households to send their children to school. UCT will improve household welfare by the largest margin if the transfer amount is the same across the policies. How UCT affects enrollment depends on income effects and substitution effects. However, those predictions would be different if we take into account public expenditure. Because CCT and UCT usually require the central government first to target the poor households who are eligible for the programs, they are subject to inclusion and exclusion errors.[2] Thus, those programs should be more costly than NREGS, resulting in the small transfer amount under the fixed amount of public expenditure, which may not be sufficient to change household behaviors. If this is the case, then NREGS should outperform the others. We investigate those conjectures in our simulation analysis.

We construct a dynamic discrete choice model of a household that consists of the mother and the child. The household determines jointly whether the mother works outside or works at home chores and whether the child works outside, works at home, or goes to school. The distinct features of the model are that the wages the mother and the child receive when working outside depend on whether NREGS enters their districts or not, and that they are substitutes in working at home. These structures allow us to describe the key mechanisms that underly the substitution between schooling and working among children under NREGS. One is the general

[1]It is theoretically possible that we compare anti-poverty policies by running an randomized control trial with multiple treatment arms. However, given that the previous research shows the possibility of the negative effects on education, that approach may not be the most appropriate to answer our research question.

[2]Inclusion error is that the transfer is made to ineligible households and exclusion error is that the transfer is not made to eligible households.

equilibrium effects of NREGS on labor markets, which raised wages in private sectors. This channel made children shift from schooling to working outside by increasing the opportunity costs of education. The other is the intrahousehold labor substitution between the mother and the child, which is caused by the mother's labor force participation due to the employment reservation for women and the general equilibrium effects on the market wages. This channel made children shift from schooling to working at home. We build our model on these two channels and estimate it using the 64th Round of the National Sample Survey (2007-2008). Because the National Sample Survey is a repeated cross-section data and because NREGS rolled out in all the districts in 2008, this round is the only household survey in which we observe the geographic variation in the implementation of NREGS.

We first show our model approximates the distribution of the observed choices well. We take this as a validation of the model. Then, we move to the policy simulation. We set the transfer amount the household receives in each cash transfer program as a fraction of total NREGS expenditure in 2007-2008 per household. Our first simulation is to compare NREGS with the cash transfer programs without imposing the budget neutrality nor targeting. Thus, households who choose schooling under CCT and all households under UCT would receive the transfer in this case. We find that relative to NREGS, CCT will improve household welfare by 1% and enrollment rate by 6% and that UCT will do household welfare by 3% and enrollment rate by a small margin. Those results are consistent with our ex-ante predictions. The second simulation is to impose the budget neutrality but not targeting. Since the number of households that benefit from NREGS is smaller than those from CCT and UCT in our data, the budget neutrality will reduce the transfer amount in each program. We find CCT still outperforms NREGS in both dimensions, while UCT does so only in household welfare. However, the differences relative to NREGS are now smaller due to the smaller transfer amount. Finally, we impose targeting on the cash transfer programs. Our targeting strategy is based on per-capita household expenditure. This is more or less correlated with household income, implying that the targeting is imperfect. Therefore, the cash transfer programs, in this case, are still likely to have inclusion and exclusion errors in targeting, although the transfer amount in each program is now higher than the second case. The results are mostly unchanged from those in the second case. Based on our analysis, we argue that NREGS should be less desirable as an anti-poverty policy relative to the cash transfer programs.

However, the above analysis does not take into account social returns that NREGS yields.

Since NREGS operates on a large scale, it could have an aggregate impact, which our model does not adequately describe. Thus, we try to quantify the rates of such returns enough to eliminate differences in household welfare between NREGS and the cash transfer programs. Specifically, we first compute them by solving for the amount of money each household would additionally need under NREGS in order to achieve the welfare level under each cash transfer program. Then dividing those figures by the per-household expenditure of NREGS, we obtain the minimum rates of returns NREGS should yield to make households indifferent across the policies. Our calculation shows the rates of returns should be, on average, 33% to achieve the same welfare under CCT and 79% under UCT, both of which are substantially higher than India's annual GDP growth rate (3% to 9%). It is thus likely that NREGS is not the optimal anti-poverty policy to improve household welfare and enrollment rate.

We contribute to three strands of literature. First, there is a large body of literature on the effects of NREGS. This paper is closely related to the papers that estimate the effects on education (Li et al. 2013; Islam and Sivasankaran 2014; Afridi et al. 2016; Shah and Steinberg 2019) and those that on labor markets (Azam 2011; Imbert and Papp 2015; Muralidharan et al. 2017; Berg et al. 2018; Zimmermann 2020). We build our structural model based on their findings. Second, we contribute to the literature on the design of anti-poverty policies with a focus on how to accurately target the beneficiaries (Ravallion 2009; Alatas et al. 2012, 2016; Klasen and Lange 2016; McBride and Nichols 2018; Hanna and Olken 2018). We extend this literature by proposing a way of evaluating anti-poverty policies based not only on targeting but also on the effects on education, which is crucial for the purposes of the policies. Finally, this paper also fits into the small literature on program evaluation using a structural approach in development economics. In development economics, randomized control trials are frequently used for program evaluation because they are useful to estimate causal relationships thanks to credible identification. However, those results are not informative of the performance of counterfactual policies in the same context (Heckman 2010; Todd and Wolpin 2010; Keane et al. 2011). Consequently, when doing ex-ante policy evaluation, development economists usually do the meta-analysis of the previous research in other contexts. Although the structural approach is no different in that it also makes out-of-sample predictions, it allows researchers to speak to the underlying mechanisms by modeling them explicitly. This paper provides a new example to the emerging literature on program evaluation with the integration of the reduced form results and the structural approach (Todd and Wolpin 2006; Attanasio et al. 2011).

The rest of the paper is structured as follows. Section 2 provides detailed information about NREGS and discusses the empirical results about the effects of NREGS on schooling. Section 3 describes our structural model and how to estimate it. Section 4 is for the estimation results and the model fit. Section 5 shows our main empirical analysis. Section 6 concludes the paper.

2 NREGS

2.1 Basic Information

In 2005, the Government of India enacted the National Rural Employment Guarantee Act to reduce poverty in rural India through a workfare program. Accordingly, NREGS was initiated in 2006 with an annual budget of 2.5% of Union Budget Expenditure.[3] NREGS guarantees 100 days of casual labor in public sectors to all rural households every year. Households in rural India can apply for employment at any time in a year. Work is provided within 15 days from the applications, or unemployment payment should be made. NREGS is managed by state governments in coordination with local governments, thus the wage is set at the agricultural minimum wage in each state, which effectively serves as a wage floor in the labor markets. While there are almost no eligibility conditions as NREGS is self-targeting, it reserves one-third of employment for women.[4] This accounts for an increase in female labor force participation, which then generates the labor substitution between mothers and their children. We will explain this in more detail in the next subsection. The type of employment provided in NREGS is labor-intensive and unskilled manual work such as road construction and irrigation development. In 2006-2007, approximately 21 million households were employed in NREGS.

NREGS rolled out sequentially in rural districts between 2006 and 2008. The rural districts were first ranked by an index that is based on agricultural wages, agricultural productivity per worker, and minority population (SC/ST) (Planning Commission 2003). Then, NREGS was first implemented in the 200 most backward districts based on the ranking in February 2006. It entered the next 130 districts in April 2007 and the remaining districts in April 2008. The existing literature exploits this staggered introduction of NREGS to estimate its short-run effects.[5] Figure 1 is the distribution of the districts in which NREGS was implemented in

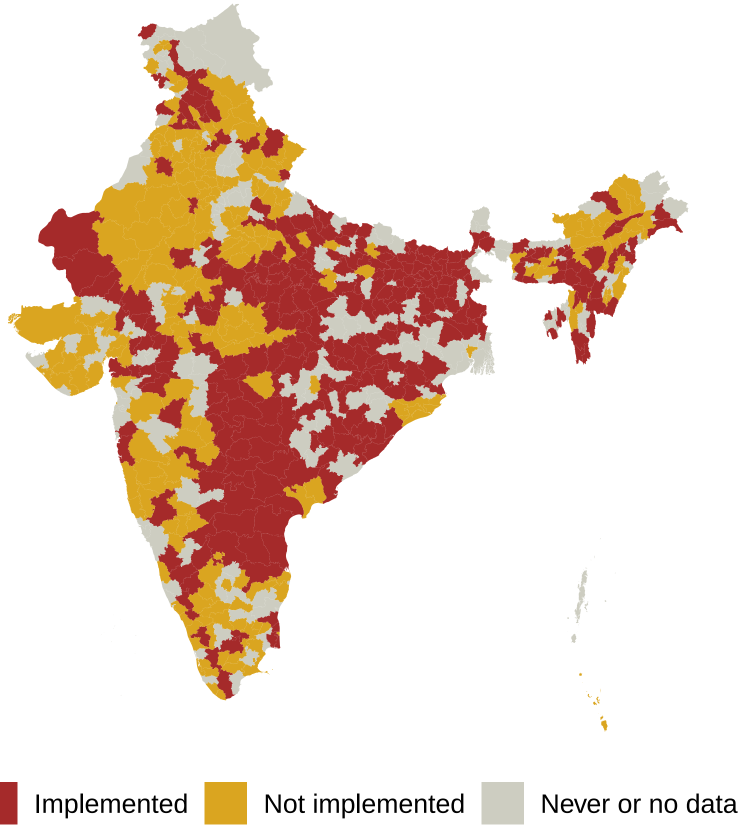
[3] https://www.indiabudget.gov.in/previous_union_budget.php, accessed on August 14th, 2020.

[4] The only eligibility conditions are being older than or equal to 18 and residency in rural areas.

[5] Zimmermann (2020) is one of the exceptions that exploits discontinuity in the index used to rank districts to estimate the effects of NREGS.

2007-2008. As seen in the map, NREGS rolled out spatially across the country.

Figure 1
NREGS implementation in 2007-2008



While NREGS is supposed to provide employment when requested, there has been an unmet demand for employment. This is especially salient in poorer states where the governments are not able to manage the demand due to weak state capacity (Dutta et al. 2012). In addition, corruption undermines the roles of NREGS as an anti-poverty policy.[6] While we are aware of the uneven implementation of NREGS across the districts, in our analysis below, we will abstract away from it for tractability of our structural model. Therefore, we can interpret our results as a best-case scenario where households can work in NREGS whenever they want.

2.2 Effects of NREGS on Schooling

As briefly reviewed in Section 1, the short-run effects of NREGS on school enrollment have been adverse in the existing literature. Li et al. (2013) find enrollment in primary schools in

[6]Corruption occurs because block-level bureaucrats have discretion over the allocation of the work across villages. This structure is exploited by politicians who affect where employment is created by pressuring them. See Niehaus and Sukhtankar (2013a,b); Gupta and Mukhopadhyay (2016); Gulzar and Pasquale (2017); Bardhan et al. (2020) for empirical evidence on this.

the districts where NREGS rolled out was lower than the districts without NREGS. Islam and Sivasankaran (2014) find that women spent 0.03 days more on work in pubic sectors, including NREGS employment, per week, once NREGS was implemented. Regarding changes in children's time use in response to NREGS, the authors find that children from ages 15 to 17 spent 0.2 days less on schooling and 0.13 days more on working outside. Shah and Steinberg (2019) find that in the districts with NREGS, children from ages 13 to 17 were less likely to report schooling as their primary activity by 3.5 percentage points and more likely to report working by 4.0 percentage points. Furthermore, they find heterogeneity across gender. While boys from that age range substituted more into working outside, girls did into domestic work such as household chores and taking care of younger siblings.[7]

Two mechanisms can underly the increase in school dropout due to NREGS. The first one is through the general equilibrium effects of NREGS on local labor markets. When NREGS was implemented, it crowded out local employment in private sectors. As a result, there was a vacancy in private sector jobs, and the wages increased (Azam 2011; Imbert and Papp 2015; Muralidharan et al. 2017; Berg et al. 2018; Zimmermann 2020). These general equilibrium effects would increase school dropout directly through an increase in the opportunity costs of schooling and indirectly through intrahousehold labor substitution that allows the unemployed household members to enter the labor markets. The other is through the intrahousehold labor substitution between mothers and their children. As stated in Section 2.1, NREGS encourages female labor force participation by reserving one-third of employment for women, who usually did domestic work before NREGS. This rationing, in addition to the wage increase by the general equilibrium effects, made the opportunity costs of domestic work higher. Therefore, mothers started to work outside once NREGS was implemented by making their children shift from schooling to domestic work. Based on the literature findings, the first channel is more relevant to boys, while the second one is more so to girls.

It is worth noting that not only NREGS but other workfare programs in developing countries can cause school dropout.[8] Moreover, any anti-poverty policies that induce mothers to shift from domestic work could increase child labor through the intrahousehold labor substitution we

[7]There may be heterogeneity in the effects of NREGS on education across the states. For instance, Afridi et al. (2016) show children in Andhra Pradesh spent 0.3 hours more per day at school when their mothers started to work outside due to the income effects.

[8]See Dammert et al. (2018) for a review of the literature on the effects of workfare programs on education in developing countries.

discussed above.[9] Therefore, while we exclusively focus on NREGS in our analysis, our results will provide a benchmark to think about the adverse effects of such anti-poverty policies in other contexts.

3 Structural Model

3.1 Overview

Because of its scale, NREGS has attracted researchers' attention, and its effects have been examined extensively. Among them, the adverse effects on school enrollment should be particularly concerned. If NREGS induces children to drop out of school today, which would make them less productive in the labor markets, then it effectively reduces poverty today by potentially increasing it tomorrow, which other anti-poverty policies could prevent. This conjecture motivates us to think about the comparison of anti-poverty policies based on their effects on school enrollment. Since there were no other anti-poverty policies implemented with NREGS, it is impossible to compare them with NREGS directly. Hence our approach is to estimate a behavioral model of the household under NREGS and run simulations under alternative policies. As stated in Section 2.2, there are two channels through which NREGS increased school dropout. One is the general equilibrium effects on local labor markets. The other is the intrahousehold labor substitution between the mothers and her children, especially daughters. Our model explicitly incorporate those channels.

We construct a dynamic discrete choice model of a household consisting of a mother and a child aged between 15 and 18.[10] We restrict our attention to this specific age range for two reasons; (1) only a small fraction of children younger than 15 reported working as their primary activity in our data (0.65% for working outside and 3.3% for working at home), (2) a significant fraction of households in our data did not have a child older than 18 (77.6%). Our discrete choice is a pair of the mother's decision and the child's decision. The mother chooses one action from working outside (in the private or public sector) and working at home, and the child chooses

[9] For example, Edmonds and Theoharides (2020) show the transfers of productive assets in the Philippines increased labor supply by adolescent children to make use of the assets.

[10] In India, the school system is eight years for elementary education, which is compulsory, two years for lower secondary education, two years for secondary education, and three years for higher education. Thus children in that age range are after elementary education and before higher education. This is the period when children are most likely to exit the school system.

from working outside (in the private sector only), working at home, and going to school.[11] The household solves the utility maximization problem every year until the child becomes 18 years old. The dynamics of the model comes from the accumulation of education, which brings the returns at the terminal period. Since there is no interaction between households, we suppress the index for the household in the following description. The only index is t, which is the age of the child.

The key state variables are the age of the child and the years of education completed. While the age evolves deterministically, the transition of years of education depends on the household's choice. In particular, we make two assumptions on that to simplify the state space. First, the child does not repeat the same grade. In other words, conditional on going to school, the child accumulates one additional year of education with probability one. Second, the switching cost from working to schooling is so large that the child cannot choose schooling once choosing working. While those assumptions are not testable in our data, we obtain the supporting evidence from other datasets. First, World Bank statistics show the percentage of repeaters in lower secondary education (children in grades 9 and 10) in India in 2008 was around 5%.[12] Second, according to Young Lives Survey, which is a panel dataset for the selected districts in Andhra Pradesh and Telangana, the ratio of children between 15 and 18 who drop out of school and re-enroll in the subsequent years is less than 1% between 2009 and 2013 (Boyden 2018). Figure 2 shows the model structure for the household with the child aged 15.

3.1.1 Discrete Choices

We denote by $A_t = \left(a_t^C, a_t^M\right) \in \{1,2,3\} \times \{1,2\}$ the pair of choices the pair of choices made by the child $\left(= a_t^C\right)$ and the mother $\left(= a_t^M\right)$.

3.1.2 Utility Function and Constraints for $t < 18$

Utility function consists of Stone-Geary preferences over consumption, an additive separable preference over a non-tradable good that is produced within the household, and a choice-specific preference shock that is unobservable to the econometrician.

[11] We do not distinguish work in private sectors from that in public sectors because the labor substitution between mothers and their children is not sector-specific.

[12] https://databank.worldbank.org/reports.aspx?source=1159&series=UIS.REPP.2.GPV, accessed on August 8th, 2020.

Figure 2
Model structure at age 15

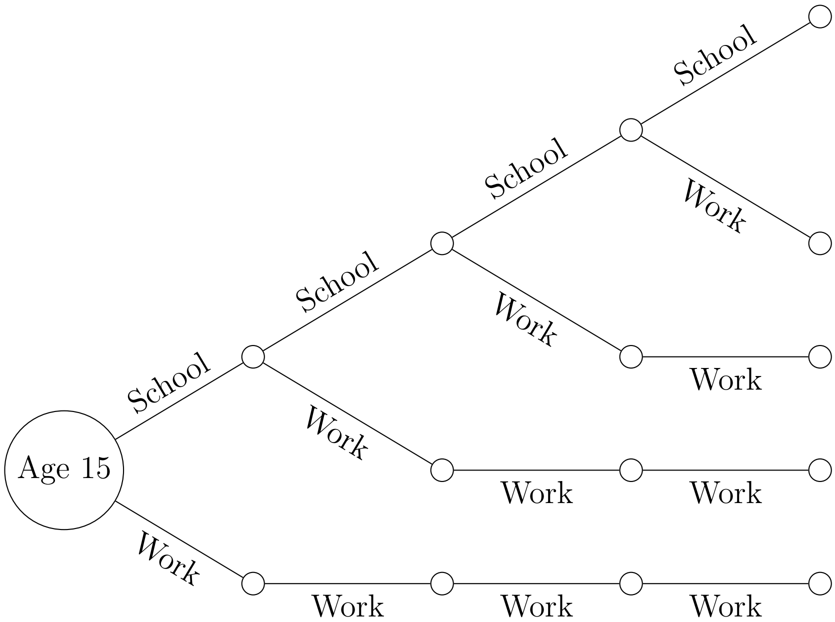


Table 1
Discrete Choices

A_t	$\left(a_t^C, a_t^M\right)$	Description
1	$(1,1)$	(Work outside, Work outside)
2	$(2,1)$	(Work at home, Work outside)
3	$(3,1)$	(School, Work outside)
4	$(1,2)$	(Work outside, Work at home)
5	$(2,2)$	(Work at home, Work at home)
6	$(3,2)$	(School, Work at home)

$$\begin{aligned} U_t &= u\left(C_t - \underline{C}, A_t\right) + v\left(Q\left(A_t\right)\right) + \varepsilon\left(A_t\right) \\ &= \ln\left(C_t - \underline{C}\right) + \beta_Q \times \ln\left(Q_t\right) + \varepsilon_t\left(A_t\right) \end{aligned}$$

where C_t is the total consumption and $\underline{C}$ is the food consumption, which proxies the subsistence level of consumption for the household. Q_t is the amount of the good produced by the household, which is essentially the housework. β_Q is the the parameter that governs substitution between consumption and housework. $\varepsilon_t\left(A_t\right)$ is the choice-specific preference shock that is unobservable to the econometrician.

We assume for simplicity that Q_t is produced by a linear production technology that employs the labor supply by the mother and/or the child. That is,

$$Q_t = \sum_{g \in \{m,f\}} \alpha_{C(g)} \times \mathbb{I}\left\{a_t^{C(g)} = 2\right\} + \alpha_M \times \mathbb{I}\left\{a_t^M = 2\right\} + X_t^H \gamma^Q,$$

where $g = m, f$ denotes the gender of the child. X_t^H is the household characteristics that affect the household production, such as the household size and the number of children in total. This structure concisely describes the substitution between the mother and the child. We allow the degree of substitution to vary across the gender of the kid, consistent with the findings from the literature.

The budget constraint the household faces is

$$Y_t + \mathbb{I}\left\{a_t^C = 1\right\} \times E\left[\omega_t^C\right] + \mathbb{I}\left\{a_t^M = 1\right\} \times E\left[\omega_t^M\right] \geq C_t + \mathbb{I}\left\{a_t^C = 3\right\} \times S_t. \quad (1)$$

Y_t is the earnings by the household head, ω_t^j for $j = C, M$ is the earnings for the household member j, and S_t is the cost of schooling varying across the levels of education. We assume that the earnings are subject to a transitory shock in the local labor market that is unobservable to the household when making the decision.[13] Therefore, the expected earnings, not the actual ones, are in the budget constraint.[14]

3.1.3 Earnings

The earnings for each household member $j \in \{C, M\}$ take the following reduced form.

$$\ln \omega_{td}^j = \gamma_1^j NREGS_d + D_d \phi^j + X_{td}^j \gamma^{\omega,j} + \eta_{td}.$$

$NREGS_d$ is the indicator of whether NREGS was implemented in district d. This will capture the general equilibrium effects of NREGS, which corresponds to the labor market channel to school dropout. D_d is the vector of district level characteristics that would control

[13]One example is a rainfall shock. This will affect the earnings through labor demand for casual labor including NREGS. Imbert and Papp (2015) document heterogeneity across dry and rainy seasons in the effects of NREGS on employment in private sector.

[14]This assumption is convenient for estimation because we can integrate the shocks out. In estimation, we do so by generating the shocks from the standard normal distribution 10000 times, computing the earnings for each shock, and taking the arithmetic mean of them.

the differences between districts with and without NREGS, as NREGS did not enter the districts randomly. The variables included are the total number of employment in 2005, the total number of establishments in 2005, the total population in 2001, the SC population and the ST population in 2001, and the population of the literate in 2001. X_{td}^{j} is the individual characteristics that would affect the earnings, including age and the gender for the child. η_{td} is the transitory shocks on earnings in the district. It is worth mentioning that the earnings do not depend on education attainment. This is because a large fraction of the adults in our data does not have any formal education and because we assume that the child will take up the jobs that such adults used to do.

3.1.4 Human Capital Accumulation and Utility Function at $t = 18$

As stated in Section 3.1, we consider the following process of human capital accumulation.

$$Edu_t = \begin{cases} Edu_{t-1} + 1 & \text{if } A_\tau \in \{3, 6\} \text{ for } \forall \tau \leq t \\ Edu_{t-1} & \text{otherwise} \end{cases}$$

Thus, the household can keep accumulating human capital as long as they choose schooling every period. Then, at the terminal period, the household receives the returns that depend on the amount of human capital. Since we have no prior on the functional form of the returns, we parametrize it as a series of polynomials up to second degree in order to allow flexibility to some extent.

$$V\left(Edu_{18}\right) = \delta_0 + \delta_1 \times Edu_{18} + \delta_2 \times \left(Edu_{18}\right)^2 .$$

Given the returns to education above, the utility function at the terminal period now becomes

$$U_{18} = \ln\left(C_{18} - \underline{C}\right) + \beta_Q \times \ln\left(Q_{18}\right) + V\left(Edu_{18}\right) + \varepsilon_{18}\left(A_{18}\right) .$$

It is worth noting that unlike the standard life-cycle model where the returns to education enter a wage function, the returns are formulated as an additive separable utility in this model. This is because the returns to education can be a composite of pecuniary and non-pecuniary values that children could receive in the future. For instance, higher education attainment is beneficial in marriage markets in India. Since we cannot describe the entire path of children's lives due to the lack of a long panel data, we allow flexibility in what education brings to them.

3.1.5 Maximization Problem

The maximization problem the household solves is

$$\begin{aligned} \max_{\{A_\tau\}_{t\leq\tau\leq 18}} \quad & E_t\left[\sum_{t\leq\tau\leq 18}\beta^{\tau-t}U_\tau|\Omega_t\right] \\ \text{s.t.} \quad & (1) \ \text{ for } \forall\tau. \end{aligned}$$

Ω_t denotes the information set at age t. The expectation is taken over the distributions of future preference shocks and those of current and future wage shocks.

3.2 Data

Our sample is households consisting of a mother and a child aged between 15 and 18 in the districts where NREGS entered eventually.[15] The main dataset is the 64th Round of the National Sample Survey (NSS). The NSS is a nationally representative household survey on employment and unemployment, conducted by the National Sample Survey Organization. The 64th Round is the one done between July 2007 and June 2008.[16] The sampling was based on the population census in 2001, and the survey was conducted through 4 sub-rounds. Each sub-round lasted three months and covered the same number of villages and blocks. We focus only on this round for two reasons. One is that the 64th Round was the only year when there was geographic variation in the rollout of NREGS. As stated in Section 2.1, NREGS entered the final group of districts in April 2008. We treat households in those districts as those who did not have opportunities to work in NREGS.[17] Second is that the NSS is repeated cross-section

[15]There are districts where NREGS is never implemented. For instance, those in urban areas are excluded.

[16]This round did not cover the following areas; Ledakh and Kargil districts in Jammu and Kashmir, some of the interior villages of Nagaland, and villages in Andaman and Nicobar Islands that were inaccessible during the survey.

[17]We cannot exclude the possibility that the households surveyed between April 2008 and June 2008 were able to work in NREGS, which would bias the estimate of the effect of NREGS on potential earnings in our

data so that we cannot construct a household panel data by combining with the previous rounds.[18] This could be a limitation on identification because the source of variation to identify the parameters in our model comes only across households. We will discuss this in detail in Section 3.4. In the 64th Round survey, more than 60% of households have only one child aged between 15 and 18.

The discrete choices are constructed based on each household member's primary activity for the last 365 days. We define a choice as "work outside" if the reported primary activity is "worked as regular salaried/wage employee," "worked as casual wage labour: in public work," or "worked as casual wage labour: in other types of work." We do so as "work at home" if it is "worked in hh enterprise (self-employed) as own account worker," "worked in hh enterprise (self-employed) as employer," "worked as helper in hh enterprises (unpaid family worker)," "attended domestic duties only," or "attended domestic duties and was also engaged in free collection of goods for hh use." Finally, "school" if "attended educational institutions." The NSS includes detailed data on household expenditure for the last 30 days, from which we obtain information about expenditure on food consumption. We multiply it by 12 to convert into an annual term. For the cost of schooling at each education level, we compute them based on household expenditure on children's education due to data availability. We describe the procedure in Appendix A. For the earnings of household heads, we use the self-reported income of household heads for the last seven days and convert it into an annual term by multiplying by 52. For human capital accumulation, we use the years of education computed based on the highest education attainment completed at the survey. We would assign five years of education if the respondent has completed the primary school, eight if the upper primary school, ten if the secondary school, and twelve if the upper secondary school. Finally, we obtain information about household size and the total number of children in the household from the household questionnaire.

We supplement the NSS data with several datasets. First, we obtain the list of districts where NREGS was implemented from the Ministry of Rural Development website.[19] We also use the population census in 2001 and the economic census in 2005.[20] Because NREGS did not enter rural districts randomly and because our dataset is a cross-section, we need to control

model downward.

[18]It is possible to construct a panel data at the district level.

[19]https://nrega.nic.in/MNREGA_Dist.pdf, accessed on June 18, 2019.

[20]The census data are from SHRUG (Asher et al. 2019).

for the selection of NREGS districts, which depended on agricultural production and the size of the marginalized population prior in 1997. To avoid collinearity with the NREGS dummy variable by controlling such variables directly, we construct SC population share, ST population share, and illiteracy rate from the population census in 2001 and the share of employment in non-agricultural sectors and the number of establishments per capita from the economic census in 2005. We include those variables in the earnings equation in the model.

Table 2-1 and Table 2-2 show the summary statistics of household characteristics and district characteristics, respectively. In our sample, households are more likely to have sons from ages 15 to 18 than daughters (57.3 % and 42.7%), and children's years of education are on average 6.8 years, which is much lower than when children would have continued schooling since they enrolled in primary school. Households are likely to have one more child who is younger than the one we study. Lastly, NREGS rolled out into slightly more than half of the districts in our data.[21]

Table 2-1
Summary statistics: household charactertistcs

Statistic	N	Mean	St. Dev.	Min	Max
Mother age	10,908	41.792	6.234	20	60
Child age	10,908	16.183	1.009	15	18
= 1 if daughter	10,908	0.427	0.495	0	1
Years of education (child)	10,908	6.795	3.513	0	12
Household size	10,908	5.550	1.914	3	23
Number of children	10,908	2.293	1.355	1	9
Earnings by Household head (Rs)	10,908	24,987.550	54,203.920	0	1,560,000
Household expenditure (Rs)	10,908	52,266.150	32,903.870	4,788	635,868
Expenditure on food (Rs)	10,908	26,837.850	12,280.700	2,433	192,355

3.3 Estimation

3.3.1 Assumptions

In order to estimate this model, we need to make assumptions on the shocks. Note that all the shocks are not observable to the econometrician. First, we assume that the preference shocks observed by the household are i.i.d across the choices and time, and follow type 1 extreme value

[21] There are more than 451 districts where NREGS is implemented. We lost some of them during our data construction process due to different spelling of the district names across datasets.

Table 2-2
Summary statistics: district charactertistcs

Statistic	N	Mean	St. Dev.	Min	Max
= 1 if NREGS	451	0.534	0.499	0	1
Primary schools (per 10000)	451	8.507	5.365	0.588	41.219
Primary schools (per 10000)	451	2.559	1.822	0.000	10.213
Primary schools (per 10000)	451	0.931	0.710	0.000	6.255
Upper pri. schools (per 10000)	451	0.259	0.245	0.000	2.066
Secondary schools (per 10000)	451	0.052	0.055	0.000	0.332
Upper sec. schools (per 10000)	451	0.065	0.038	0.0001	0.471
College (per 10000)	451	0.029	0.013	0.0001	0.085
Non agri. employment share	451	0.154	0.084	0.000	0.419
Establishments (per capita)	451	0.148	0.248	0.000	0.981
SC share	451	0.533	0.118	0.242	0.854

distribution. Second, the shocks to the earnings are unobservable to the household, i.i.d across time and the districts, and follow a normal distribution $N(0, 0.1)$. Finally, those shocks are independent of each other. Since the discount factor is not identified in the standard dynamic discrete choice model, we set it to 0.98 (Rust 1987; Magnac and Thesmar 2002; Kasahara and Shimotsu 2009).

3.3.2 Discretization

Our model involves continuous variables that should be discretized in estimation to ease the computational burden. We do it by k-means clustering, which classifies observations into clusters. This discretization allows us to solve the utility maximization problem for each type of household. We choose the optimal number of clusters by minimizing the total sum of within-cluster variance, and find that it is four.[22] Combining with other discrete variables in the model, our sample for estimation becomes 241 types.

3.3.3 Maximum Likelihood Estimation

Given the assumptions we make in the previous subsection, we do a maximum likelihood estimation. Because of the discretization, we compute the likelihood for each type of household and multiply it by the size of the type. Therefore, our maximization problem is

[22] We use a function *fviz_nbclust* with *wss* for an algorithm in R. We also try different algorithms available in R, all of which suggests the optimal number of clusters should be close to four.

$$\max_{\Theta} \quad \sum_{g=1}^{241}\sum_{k=1}^{6}\ln\mathcal{L}_g\left(k:\Theta\right)\times N_g\left(k\right),$$

where $\mathcal{L}_g\left(k:\Theta\right)$ is the likelihood of type g household choosing the discrete choice k and $N_g\left(k\right)$ is the number of households in type g choosing k.

The log-likelihood function of type g household choosing the discrete choice $a\in\{1,\ldots,6\}$ is

$$\begin{aligned}
\ln\mathcal{L}_g\left(k:\Theta\right) &\equiv \ln\mathcal{L}\left(A_g=a, Edu_g:\Theta|\Omega_g\right)\\
&= \ln P\left(A_g=a, Edu_g:\Theta|\Omega_g\right)\\
&= \ln P\left(A_g=a:\Theta_1|\Omega_g, Edu_g\right)+\ln P\left(Edu_g:\Theta_2|\Omega_g\right),
\end{aligned}$$

where $\Theta=\begin{pmatrix}\Theta_1 & \Theta_2\end{pmatrix}'$. The third line follows from the Bayes' theorem. The first term in the third line is the conditional choice probability that is the solution to our household problem. We have the closed-form of this probability, thanks to the logit errors in the preferences. We describe the derivation of it in Appendix B. The second term is the likelihood of the years of education observed in the data. Since we do not observe the entire history of human capital investment decisions in our data, the state where the household starts to solve the dynamic problem in the model is different for each household (Initial condition problem). Since such difference is not determined in our mode, following Attanasio et al. (2011), we parameterize it as an ordered probit function. That is, for each household type g, the probability is

$$P\left(Edu_g:\Theta_2|\Omega_g\right) = \begin{cases}\Phi\left(\theta_1-Z_i'\zeta-K_d'\xi\right) & \text{if } Edu_i=0\\ \Phi\left(\theta_2-Z_i'\zeta-K_d'\xi\right)-\Phi\left(\theta_1-Z_i'\zeta-K_d'\xi\right) & \text{if } Edu_i=5\\ \Phi\left(\theta_3-Z_i'\zeta-K_d'\xi\right)-\Phi\left(\theta_2-Z_i'\zeta-K_d'\xi\right) & \text{if } Edu_i=8\\ \Phi\left(\theta_4-Z_i'\zeta-K_d'\xi\right)-\Phi\left(\theta_3-Z_i'\zeta-K_d'\xi\right) & \text{if } Edu_i=10\\ 1-\Phi\left(\theta_4-Z_i'\zeta-K_d'\xi\right) & \text{if } Edu_i=12\end{cases},$$

where $Z=\begin{pmatrix}X^H & X^C\end{pmatrix}$ and K is the vector of school availability in district d prior to NREGS. Specifically, it contains the number of primary, upper primary, secondary, upper secondary schools, and colleges per 10000 people from the population census 2001. These variables help identification of the parameters in the probit function by being excluded from the construction of conditional choice probability.[23]

3.4 Identification

Since our dataset is a cross-section, our identification is based on variation across households. The parameter that governs substitution between the marginal utility of consumption and that of non-tradable goods produced within the household should be identified by variation in consumption level. This is because the utility functions are concave so that the household wants to consume both of them. The parameters associated with the returns to education are identified by covariation of consumption level and years of education. For instance, if the household has a low level of consumption but more years of education, then we can say that it values education to a large extent. Hence we expect the parameters to be positive and large. While we are confident that the returns to education should be increasing because the substantial fraction of the households in the data choose schooling, we cannot say whether they should be concave or convex. Thus, our identification argument should be valid up to the first derivative of the functional form of the returns to education.

In addition to cross-sectional variation across households, our identification also relies on the exclusion restrictions of control variables. For instance, the parameter associated with NREGS is identified across the districts conditional on the district level controls that partially determined the rollout of NREGS. This means that the identifying variation for the parameter comes from variation in the NREGS implementation and the control variables within the same NREGS implementation status. A similar argument should hold for the parameters in the household production function and those in the initial condition probability, where the excluded variables are household characteristics and the availability of schools for each education level, respectively.

4 Estimation Results

[23]Notice that there is no parameter to estimate that appears in both the conditional choice probability and the probability of the initial condition of the years of education so that we estimate the parameters by maximizing each probability separately. This is for simplicity and different from the literature on the dynamic discrete choice model with the initial condition problem, which often estimates unobserved time-invariant heterogeneity that affects both probabilities.

4.1 Parameter Estimates

Table 3 shows the estimates of the selected parameters. The standard errors are computed via bootstrap.[24] The signs of the parameters are consistent with our expectations. The utility from the non-tradable goods is positive, and the marginal rate of substitution with consumption is 1.46. The returns to education function is increasing in years of education if greater than 4. This functional form will be justified if not completing primary education (5 years of education) is equivalent to no education at all in the labor markets. For the household production function, daughters are more productive than sons, and mothers are more so than children. These are consistent with the findings in the literature. Lastly, the potential earnings of children and mothers are higher than without NREGS, though the magnitudes may be too large compared to those found in the literature (Azam 2011; Imbert and Papp 2015; Berg et al. 2018; Zimmermann 2020).[25] One potential interpretation of this overestimation is that they are estimated on the sample including households out of labor force (thus their earnings are zero).

Table 3
Parameter Estimates (Selected)

	Estimates	Standard errors
Preferences		
β_Q	1.46	
δ_0	5.00	
δ_1	-1.05	
δ_2	0.13	
Household production		
$\alpha_{C(m)}$	1.15	
$\beta_{C(f)}$	3.51	
β_M	4.71	
NREGS		
${\gamma_1}^C$	0.55	
${\gamma_1}^M$	0.62	

Note: Standard errors are yet to be computed.

[24] We draw a sample of size 241 from our discretized data, weighing by the group size. We do it 500 times.

[25] The effect of NREGS on log daily wage for adults has been estimated at around 0.05, thus a 5% increase.

4.2 Model Fit

To justify our estimates, we compare the distribution of the discrete choices observed in the data with that computed from our model. Table 4-1 below summarizes the comparison. First of all, despite its simplicity, our model captures the overall pattern of the distribution well. For instance, our model predicts the largest fraction of households choose to schooling for the child and working at home for the mother and the second-largest working at home for both of them, both of which are true in the data. To investigate where our model deviates from the data, we show the marginal distributions in Table 4-2. Most of the difference comes from the choices for the child. In particular, we underestimate the fraction of households choosing schooling by 3.5 percentage points. We instead overestimate the fraction with the child working at home by a similar magnitude. Those results imply that in our model, relative to what we observe in the data, households are myopic, or the marginal utility from the non-tradable goods is high. All in all, our model is a good approximation of the household behaviors in the data.

Table 4-1
Distribution of discrete choices

Discrete choice	Data (%)	Model (%)
(Work outside, Work outside)	4.86	2.45
(Work at home, Work outside)	2.51	3.17
(School, Work outside)	7.52	10.12
(Work outside, Work at home)	7.01	10.45
(Work at home, Work at home)	21.18	23.07
(School, Work at home)	56.92	50.74

Note: The total may not be 100 due to rounding errors.

Table 4-2
Marginal distribution of discrete choices

Discrete choice	Data (%)	Model (%)
Child: Work outside	11.87	12.9
Child: Work at home	23.69	26.24
Child: School	64.44	60.86
Mother: Work outside	14.89	15.74
Mother: Work at home	85.11	84.26

5 Comparison of Anti-Poverty Policies

5.1 Cash Transfer Programs

Using our estimated model, we now compare the performance of anti-poverty policies with NREGS. Our purpose of this analysis is to understand when NREGS should be chosen as an anti-poverty policy. This analysis is motivated by two facts. One is that because of self-selection, NREGS should be more cost-effective than other policies that require the central government to target the poor. Second is that NREGS increased school dropout, which implies children would not be able to escape poverty in the future.

The candidate policies are cash transfer program conditional on schooling (CCT) and unconditional cash transfer program (UCT), both of which have been widespread in developing countries.[26] Our evaluation metric is thee-fold. First is total household welfare. We define it as the summation of household utilities from their choices. Second is enrollment rate. It is the share of households who choose schooling as their optimal choices. The third is public expenditure. We will discuss how to compute that for CCT and UCT in the following subsections.

Ex-ante prediction about which policy surpasses the others is ambiguous. In terms of household welfare, UCT should outperform if the transfer amount that households receive is large enough. It is because, unlike the other two policies, all the households should benefit from UCT. CCT should achieve the highest enrollment rate as it incentivizes households to do so. Finally, in terms of public expenditure, NREGS should have an advantage because it is self-selection so that only the households who need the benefits should participate in NREGS, while the cash transfer programs may be costly due to inclusion and exclusion errors in targeting.

5.2 Scenario 1: Benchmark

We start our analysis by comparing NREGS with the cash transfer programs without considering public expenditure. It is to test whether our counterfactual analysis is consistent with the predictions in the previous section. Because NREGS have an advantage in this aspect, comparison without taking into account that should rank NREGS lowest among the policies under consideration.

In order to simulate the household behaviors under the cash transfer programs, we need

[26] While CCT and UCT are target cash transfers, we use those words tentatively when considering the comparison with cash transfers without targeting in Section 5.1.1 and 5.1.2 to emphasize that we analyze the same policies throughout the section.

to determine the amount of transfer for each program. We set the amount equal to the total expenditure of NREGS per household in 2007-2008.[27] Table 5 is the summary statistics of it. Around 70% of the expenditure was on labor-related expenses such as wage payment. The average size of the transfer is 2.65% of the average household expenditure in our data. Since the expenditure data are available at the district level, we vary the transfer amount across the districts. In the following simulation, we shut down the effect of NREGS on the potential earnings and add the transfer to the budget constraints of households in the districts with NREGS every period.

Table 5
NREGS expenditure per household in 2007-2008

Statistic	N	Mean	St. Dev.	Min	Max
Expenditure on labor (Rs, per hh)	451	988.090	1,342.875	0.000	7,042.188
Expenditure on material (Rs, per hh)	451	396.123	616.347	0.000	3,065.916
Expenditure on total (Rs, per hh)	451	1,384.213	1,883.975	0.000	7,787.564

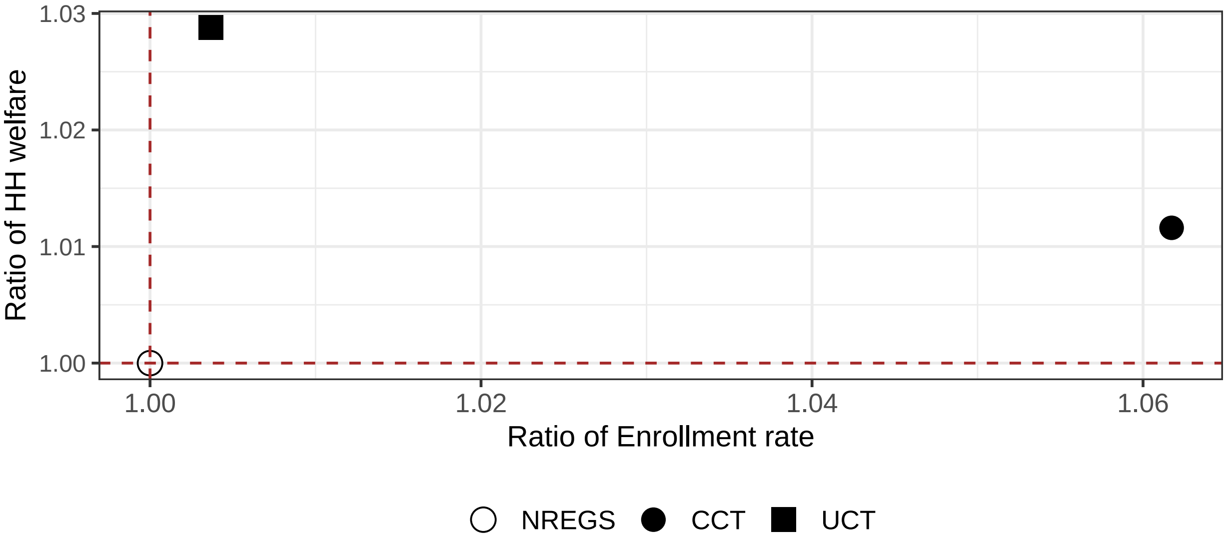
We plot the results in Figure 3. The horizontal axis is enrollment rate, and the vertical axis is total household welfare, both of which are normalized by those under NREGS. The enrollment rate is the fraction of households choosing schooling. The total household welfare is the summation of household utilities based on the optimal choices. Both numbers are computed only for households in the NREGS districts. Both CCT and UCT in Scenario 1 are in the top-right region of NREGS, which means they dominate NREGS in both metrics. CCT will have a 6% higher enrollment rate and 1% higher total welfare. UCT will achieve much higher household welfare but a slightly higher enrollment rate. Those results are consistent with our ex-ante predictions.

5.3 Scenario 2: Budget Neutrality

We now include the expenditure aspect in our evaluation. Since the public expenditure is not observable for both CCT and UCT, we compute it by choosing the transfer amount as a fraction of per-household NREGS expenditure that does not exceed the total NREGS

[27] The annual expenditure data are available at MGNREGA Public Data Portal (https://nregarep2.nic.in/netnrega/dynamic2/DynamicReport_new4.aspx, accessed on August 8th, 2020). Since the data earlier than 2011-2012 do not have the total number of households worked in NREGS, we proxy it by the total number of job cards issued in that year. This approximation is likely to be an overestimate (hence the per-household expenditure would result in an underestimate) due to the imperfect enforcement of NREGS.

Figure 3
Household welfare and enrollment: Scenario 1



Note: Enrollment rate under NREGS is 0.576.

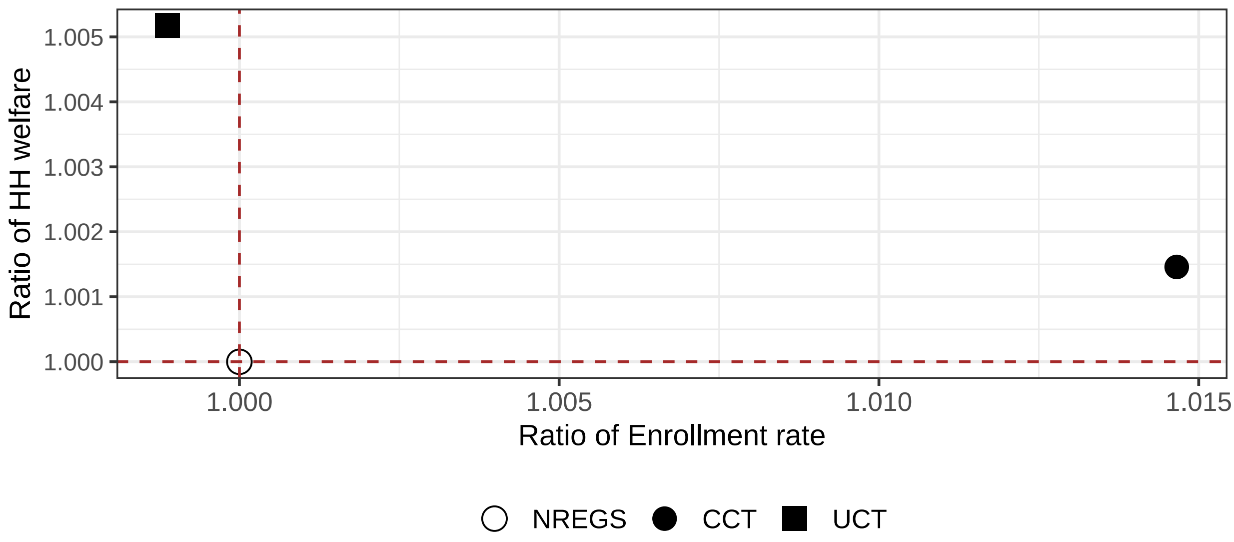
expenditure when multiplied by the number of the beneficiaries. That is, for CCT, we try to find

$$\delta^* = \max\left\{\delta \in \{1, 2, \ldots 100\} \,|\, \tfrac{\delta}{100} \times \textstyle\sum_d \left(\mathrm{Exp}_d \times N_d^S\right) \leq \textstyle\sum_d \left(\mathrm{Exp}_d \times N_d^W\right)\right\},$$

where N_d^S is the number of households who would choose schooling under CCT and N_d^W is the number of households who choose working outside under NREGS, in district d. We compute the transfer amount in UCT in a similar fashion. We find $\delta^* = 27$ for CCT and $\delta^* = 16$ for UCT.

Figure 4 is a summary of the results under Scenario 2. First, compared to Scenario 1, the effects of both cash transfers are smaller. For instance, CCT in Scenario 2 will increase the enrollment rate by 1.5% and the welfare by 0.15% (6% and 1% in Scenario 1, respectively). The result is consistent with the fact that the transfer amount now becomes much smaller. Second, unlike in Scenario 1, UCT now has a lower enrollment rate than in NREGS, though CCT still dominates NREGS. Since transfer amount under UCT in Scenario 2 is not enough to compensate wage gains from NREGS, some households now have higher marginal utility today relative to tomorrow. As a result, they have the children switch from schooling to work. This change does not happen in CCT because households can gain additional consumption today by sending their children to school.

Figure 4
Household welfare and enrollment: Scenario 2



Note: Enrollment rate under NREGS is 0.576.

5.4 Scenario 3: Targeting

Finally, we incorporate targeting into our cash transfers to make them closer to CCT and UCT in the real world. Targeting is a crucial component when discussing the cost-effectiveness of anti-poverty policies. Because income is rarely observed in developing countries, targeting the poor should rely on its proxy that is usually assets, which would have both inclusion and exclusion errors (Klasen and Lange 2016; Hanna and Olken 2018). It implies that self-targeting programs have an advantage in this regard (Alatas et al. 2016). Figure 5 illustrates that households whose mothers chose working outside have lower consumption than those who chose working at home. While this is not a causal relationship, we take this as suggestive evidence on self-targeting behaviors in our data.

The targeting we consider here is based on per-capita household expenditure. We do this for three reasons. First, targeting based on assets is not possible in our context due to data limitations. Second, related to the first point, it is impossible to use the targeting strategies that have been used in other welfare programs in India for the same reason. In India, the eligibility of welfare programs that requires targeting the poor is based on the possession of Below Poverty Line (BPL) cards, which is not contained in our data. Households can receive BPL cards if they satisfy the inclusion and exclusion criteria set by the central government.[28] However, since those criteria require detailed information about asset holdings, it is not easy to replicate it

[28] See Alkire and Seth (2013) for the history of the criteria.

Figure 5
Self-targeting into NREGS

Note: Effective consumption is total household expenditure minus food consumption ($C_t - \underline{C}$ in the model). "Work outside" and "Work at home" are based on the mother's choice.

with our data.[29] We thus need to construct our targeting strategy. Third, targeting based on per-capita household expenditure shows the possibility of inefficiency in targeting. Table 5 is the cross-tabulation based on whether annual earnings of household heads are below the sample median and whether per-capita household expenditure is below the sample median. In our data, 42% of households with per-capita household expenditure below the sample median have the household head's earnings higher than the sample median. Given that the household head's earnings account for the large portion of total household income, this indicates targeting on the expenditure would make inclusion and exclusion errors when identifying the poor. Therefore, equipping the cash transfers with that enables us to highlight the importance of self-targeting in NREGS.

Table 5
Earnings of household head and household expenditure below sample median

	HH exp. $<$ median	HH exp. $\geq$ median
Earnings $<$ median	0.29	0.29
Earnings$\geq$ median	0.21	0.21

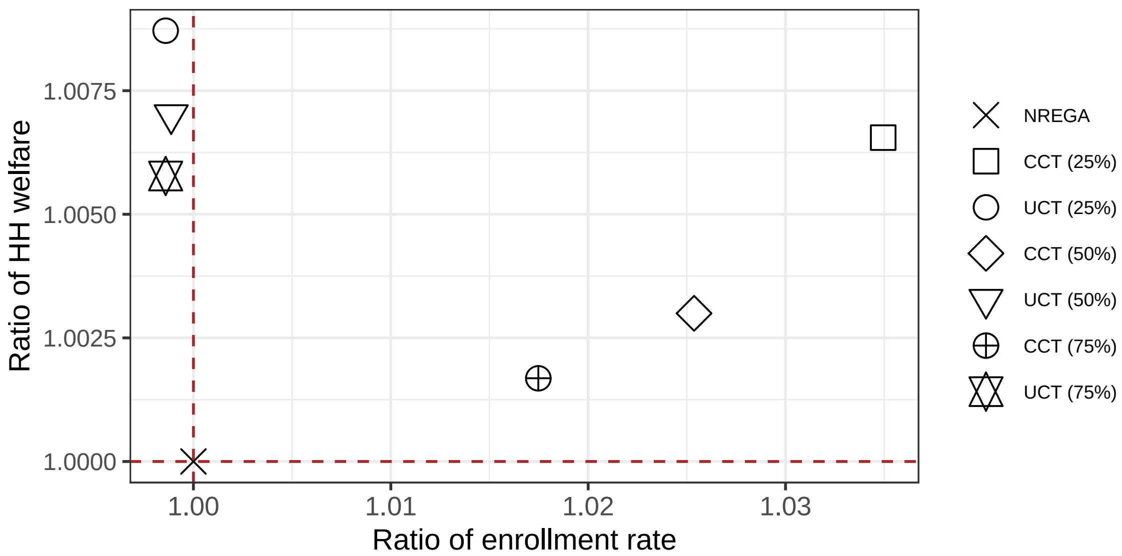
Note: Since the sample median of earnings by the household head is 0, the fraction of households above the median is higher than 0.5.

After identifying households eligible for CCT and UCT, we compute the transfer amount

[29] Another problem is that the distribution of BPL cards is distorted by corruption (Niehaus et al. 2013). This undermines the validity of BPL card possession as a benchmark of targeting in our analysis.

for each program, as we did in the previous section.[30] Figure 6 illustrates the results. To check the robustness, we show the results with the eligibility below the bottom quartile, the median, and the top quartile of the sample. Compared to Scenario 2, the patterns remain the same, and the changes are larger because of the larger transfer amount.[31] CCT will still dominate NREGS in both dimensions, and UCT will do in the welfare. This result is robust across the eligibility conditions.

Figure 6
Household welfare and enrollment: Scenario 3



Note: Enrollment rate under NREGS is 0.576.

5.5 Social Returns to NREGS

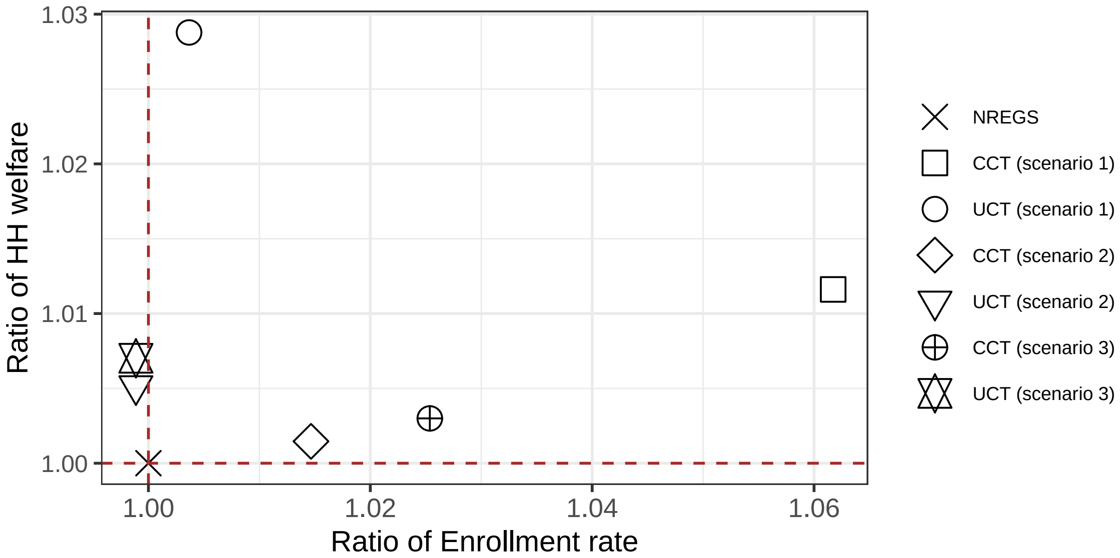
Figure 7 summarizes our counterfactual analysis above. We show CCT will strictly dominate NREGS both in the total household welfare and the enrollment rate. UCT will be the best in terms of the total welfare, while the enrollment rate will be close to the one under NREGS. Therefore, we can conclude that cash transfers should be more desirable than NREGS if the central government considers its impact on household welfare and school enrollment.

However, since NREGS has operated on such a large scale, it should have an aggregate (positive) impact on the economy that our previous analysis does not adequately describe. One

[30] It is worth noting that the actual targeting procedures incur administrative costs, such as ones for conducting surveys. Since we do not take them into account when calculating the transfer amount, we should think it as the upper bound of the transfer amount households could receive.

[31] The transfer amount with the eligibility condition being below the bottom quartile is 100% for CCT and 49% for UCT. Similarly, for the eligibility condition being below the sample median, the transfer amount is 51% for CCT and 26% for UCT, and for that below the top quartile is 35% for CCT and 16% for UCT.

Figure 7
Household welfare and enrollment: All



Note: Enrollment rate under NREGS is 0.576. In Scenario 3, we display the results of eligibility being below the sample median of per capita household expenditure.

example is that because work provided in NREGS is usually construction of infrastructure, NREGS may stimulate economic activities in the NREGS districts (Cook and Shah 2020).[32] Thus, there is a possibility that the central government chose NREGS because it prioritized that aspect. Besides, such aggregate effects are likely to be absent in CCT and UCT, which are interventions at the household level. The natural question is how large they should be to make NREGS preferred over the cash transfers programs.

We address this question by calculating the amount of money required to eliminate the difference in the total household welfare between NREGS and the cash transfer programs under study. We denote by $W_i = W\left(C_i - \underline{C}\right)$ the welfare of household i as a function of today's consumption minus food consumption. Then, for each i, we solve

$$\min_{M_i^{\text{CT}}} \quad \left\{W^{\text{NREGS}}\left(C_i - \underline{C} + M_i^{\text{CT}}\right) - W^{\text{CT}}\left(C_i - \underline{C}\right)\right\}^2,$$

where $\text{CT} \in \{\text{CCT}, \text{UCT}\}$ is an index for the cash transfer programs. We consider those with the eligibility condition below the sample median in Scenario 3. After obtaining M_i^{CT} for each household, we aggregate it at the district level, separately for each cash transfer program.

[32] Another example is a reduction in the number of civil conflicts (Khanna and Zimmermann 2017; Fetzer et al. 2019).

Then, we divide it by the total NREGS expenditure to define the rates of returns in each program.

Table 6
Amount of money to eliminate welfare differences

Statistic	N	Mean	St. Dev.	Min	Max
CCT	239	0.327	1.802	0.010	20.682
UCT	239	0.791	3.922	0.010	37.232

Note: The amount of money that makes each household indifferent in our welfare measure across the anti-poverty policies is divided by the per household NREGS total expenditure and is aggregated at district level.

Table 6 presents the results. To achieve the same level of the total household welfare under CCT, NREGS should yield the rate of returns equivalent to 32.7% on average. The rate is much higher for UCT (79%). These numbers are much higher than India's annual GDP growth rate, which has been between 3% to 9% since 2000, though they include any returns from non-economic activities.[33] Thus, it is likely that cash transfer programs are more cost-effective in improving household welfare and enrollment rate than NREGS.

6 Conclusion

This paper compares NREGS, India's workfare program that guarantees 100 days of employment to rural households, with alternative anti-poverty policies. The previous research shows NREGS increased school dropout among children whose parents were the beneficiaries, especially children at secondary school age. This adverse effect on school enrollment implies that NREGS may increase future poverty to reduce today's poverty. On the other hand, because NREGS is self-targeting, it is expected to be cost-effective relative to other policies that require the central government to target the poor. This feature is appealing to the central governments in developing countries where the income of the poor is hard to observe so that the targeting is likely to entail inclusion and exclusion errors. To understand whether the advantage exceeds the disadvantage, we evaluate NREGS for household welfare, enrollment, and total expenditure, and compare it with other anti-poverty policies. We consider two cash transfer programs: one is conditional on schooling (CCT), and the other is unconditionally distributed (UCT). Because

[33] https://data.worldbank.org/indicator/NY.GDP.MKTP.KD.ZG?locations=IN, accessed on August 11th, 2020.

CCT and UCT did not operate at the same time NREGS did, we first estimate a behavioral model of the household under NREGS and then simulate it under CCT and UCT. Our model is a single agent dynamic discrete choice model where the mother and her child jointly determine labor supply decisions.

Our empirical results start by showing our model approximates household choices under NREGS. It successfully captures several characteristics of the distribution of the discrete choices observed in the data. Then we present our counterfactual analysis. We show under the same amount of public expenditure, NREGS is strictly dominated by CCT in household welfare and enrollment rate, and weakly so by UCT. However, this comparison does not take into account any social returns that NREGS should yield. Thus, we compute the size of such returns enough to eliminate the welfare differences across the policies. Our estimates show NREGS should generate much higher returns than India's annual growth rate of GDP, suggesting that the cash transfer programs are likely to be cost-effective in improving household welfare and enrollment rate.

NREGS has been extensively studied by economists. However, there are few papers that attempt to discuss the optimality of NREGS as an anti-poverty policy. This paper introduces a new methodology to evaluate NREGS in comparison with other policies. It is, however, worth noting a few caveats in our analysis. First, our sample is not too general. We focus on households with one child aged 15 and 18 for the tractability of the model. While those are the majority in our data, we could extend our model to include households with different family structures. Second, our counterfactual analysis does not consider any administrative costs of the cash transfer programs, which may be larger than the total amount of transfer. Thus, the performance of the cash transfer programs in this study would be worth than what we have shown, suggesting they may not dominate NREGS. Finally, the transfer under CCT and UCT can have larger effects on enrollment rate than the equivalent amount of cash earned by households (Attanasio et al. 2011). Thus, we may underestimate their performance in our simulation exercise. Nevertheless, we hope our empirical analysis will provide a useful benchmark to discuss the optimal choice of anti-poverty policies in developing countries.

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Appendix

A. Estimation of Costs of Schooling

To solve the dynamic model, the household should know the costs of schooling at each education level. However, the data on the annual costs of schooling may not exist in the Indian context. Therefore, we estimate it using the household expenditure data on schooling in the NSS data. In particular, for household i in state s, we run the following OLS regression.

$$\begin{aligned} \text{Exp}_{is} &= \beta_{1s} \times \mathbb{I}\left\{\text{Primary}_{is}\right\} + \beta_{2s} \times \mathbb{I}\left\{\text{Upper primary}_{is}\right\} \\ &\quad +\beta_{3s} \times \mathbb{I}\left\{\text{Secondary}_{is}\right\} + \beta_{4s} \times \mathbb{I}\left\{\text{Upper secondary}_{is}\right\} \\ &\quad +\beta_{5s} \times \mathbb{I}\left\{\text{College or higher}_{is}\right\} + u_{is}, \end{aligned}$$

where independent variables are dummy variables for each education level. Table A-1 presents a mapping between years of education and education levels. We then take the coefficients as the estimated costs of schooling. We allow them to vary across states because education is managed at the state level.[34] Table A-2 shows the estimates.

Table A-1
Years of education and education levels

Years of education	Education levels
< 5	Primary
$5 \sim 8$	Upper primary
$8 \sim 10$	Secondary
$10 \sim 12$	Upper secondary
> 12	College or higher

[34]Because of the sparsity of the data, some of the district-wise estimated costs are negative. We, therefore. stick to the state-wise estimates.

Table A-2
Estimates of costs of schooling

Statistic	N	Mean	St. Dev.	Min	Max
Primary school (Rs)	28	1,455.709	876.834	560.244	3,901.429
Upper pri. school (Rs)	28	1,468.317	686.012	622.526	3,298.588
Secondary school (Rs)	28	2,652.516	1,313.700	1,066.476	6,474.781
Upper sec. school (Rs)	28	5,987.468	4,261.463	2,998.210	26,058.900
College or higher (Rs)	28	7,958.701	4,529.800	2,770.051	24,505.510

B. Derivation of Conditional Choice Probability

In order to derive the conditional choice probability, we need to solve the maximization problem. Since our model is finite-horizon, we can do so by backward induction. At $t = 18$, the maximization problem is

$$\begin{array}{ll} \max\limits_{A_{18}} & u\left(C_{18} - \underline{C}, A_{18}\right) + v\left(Q\left(A_{18}\right)\right) + V\left(Edu_{18}\right) + \varepsilon\left(A_{18}\right) \\ \text{s.t.} & (1)\ \text{ for } t = 18. \end{array}$$

Given the assumptions on the distributions of the preference shocks, the probability of observing choice $A_{18} = a$, conditional on Edu_{18} is

$$P\left(A_{18} = a : \Theta_1 | \Omega, Edu_{18}\right) = \frac{\exp\left(u\left(C_{18} - \underline{C}, a\right) + v\left(Q\left(a\right)\right) + V\left(Edu_{18}\right)\right)}{\sum_{k=1}^{6} \exp\left(u\left(C_{18} - \underline{C}, k\right) + v\left(Q\left(k\right)\right) + V\left(Edu_{18}\right)\right)}.$$

Given the choice that the household will make at $t = 18$, the maximization problem at $t = 17$ now becomes

$$\begin{array}{ll} \max\limits_{A_{17}} & u\left(C_{18} - \underline{C}, A_{17}\right) + v\left(Q\left(A_{17}\right)\right) + \beta \times E_{17}\left[U_{18}^{*} | Edu_{17}, A_{17}\right] + \varepsilon\left(A_{17}\right) \\ \text{s.t.} & (1)\ \text{ for } t = 17, \end{array}$$

where U_{18}^{*} is the value function at $t = 18$ and β is the discount factor. Using the property of type I extreme distribution, we have

$$\begin{aligned} E_{17}\left[U_{18}^{*}|Edu_{17},A_{17}\right] &\equiv E_{17}\left[\max_{A_{18}} U_{18}\left(A_{18}, Edu_{18}\right) \text{ s.t. } (1) \text{ for } t=18|Edu_{17},A_{17}\right] \\ &= \gamma + \ln \sum_{k=1}^{6} \exp\left(\tilde{U}_{18}\left(k, Edu_{18}|Edu_{17},A_{17}\right)\right), \end{aligned}$$

where $\gamma = 0.577216$ is the Euler constant. $U_{18}(\cdot) = U_{18}$ is the instantaneous utility function at $t = 18$ and $\tilde{U}_{18}(\cdot)$ is that minus the preference shock. We can then derive the probability of observing choice $A_{17} = a$ as

$$P\left(A_{17} = a : \Theta_1|\Omega, Edu_{17}\right) = \frac{\exp\left(u\left(C_{18} - \underline{C}, a\right) + v\left(Q\left(a\right)\right) + \beta \times E_{17}\left[U_{18}^{*}|Edu_{17},a\right]\right)}{\sum_{k=1}^{6} \exp\left(u\left(C_{18} - \underline{C}, k\right) + v\left(Q\left(k\right)\right) + \beta \times E_{17}\left[U_{18}^{*}|Edu_{17},k\right]\right)}.$$

The conditional choice probabilities at $t = 15, 16$ are calculated in the same way.

C. Parameters in Initial Condition Problem

Table C-1 is the estimates of the parameters associated with the initial condition problem. While they are not the parameters of our interest, we show them for completeness.

Table C-1
Parameter Estimates: Initial condition (selected)

	Estimates	Standard errors
Thresholds		
θ_1	1.01	
θ_2	2.33	
θ_3	4.60	
θ_4	6.78	
School availability		
ξ_1	-0.18	
ξ_2	1.05	
ξ_3	-3.95	
ξ_4	14.32	
ξ_5	50.68	

Note: Standard errors are yet to be computed. ξ_1 is associated with primary schools, ξ_2 with upper primary schools, ξ_3 with secondary schools, ξ_4 with upper secondary schools, and ξ_5 with colleges.