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

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Abstract

Workfare is a common anti-poverty policy in developing countries that involves hiring poor individuals for public construction work. One advantage of workfare is that participants voluntarily enroll in the program, which mitigates targeting errors that are often observed in other targeting methods such as proxy-means tests. However, despite its targeting efficiency, workfare may have unintended adverse effects on school enrollment. This raises the question of how to evaluate workfare as an anti-poverty policy when considering these dimensions. This paper aims to quantify this trade-off for a large workfare program in India by comparing it with hypothetical cash transfer programs that employ a less accurate targeting approach but improve school enrollment. I find that under fixed program expenditures, the workfare program has lower household welfare and lower school enrollment rates than the cash transfer programs. I also find that the workfare program would need to yield unreasonably high rates of social returns to achieve the same levels of household welfare generated by the cash transfer programs. These results suggest that the cash transfer programs are preferred over the workfare program, despite their less precise targeting.

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1 Introduction

The comparison of policy designs is a central challenge in policymaking. Policies differ in various aspects with no clear guideline on how to evaluate them jointly. Although it has been feasible to compare policies at a small scale using randomized controlled trials, running experiments to identify optimal policy designs can be too costly, particularly in developing countries with limited program budgets. In this paper, I present a comparative analysis of a workfare program in India and counterfactual cash transfer programs using a structural model, focusing on two key aspects of anti-poverty policies: targeting efficiency and effects on education.

Workfare is a major anti-poverty policy in developing countries and has a distinct way of targeting. People who need financial assistance self-select into workfare programs and engage in unskilled manual labor at predetermined wages. Compared to other targeting methods such as a proxy-means test, which targets the poor based on assets observed in household surveys, the self-targeting can have a lower rate of inclusion and exclusion errors (Hanna and Olken, 2018).[1] Since targeting is a crucial aspect of the design of anti-poverty policies in developing countries, workfare may be preferred over policies with less efficient targeting.

Despite the advantage in targeting, however, workfare may not be a desirable anti-poverty policy due to its unintended consequences on education. Parents' participation in workfare programs can lead to their children dropping out of school, because the children substitute for adults in labor supply for market and domestic work that is absorbed by workfare (Dammert et al., 2018). The negative effects on school enrollment can have long-lasting implications for poverty rates, as children who drop out of school are more likely to stay poor because of the lack of education. In this regard, other anti-poverty policies that improve children's education, such as conditional cash transfers, may be preferred over workfare.

This paper investigates the trade-off between targeting efficiency and adverse effects on education, focusing on the National Rural Employment Guarantee Scheme (NREGS) in India. NREGS is one of the largest workfare programs globally, which provides households in rural India with employment for 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 research context for this study for several reasons. First, NREGS implements self-targeting. Every household in rural India can apply for employment under NREGS at any time in a year without any

[1] Inclusion errors are the probability of the transfer delivered to ineligible individuals and exclusion error are the probability of the transfer not delivered to eligible individuals.

stringent eligibility criteria, except that the applicants are equal to or older than 18 and live in rural areas. Second, previous research has found the negative effects of NREGS on school enrollment. For instance, Shah and Steinberg (2021) found children from aged 5 to 17 in the districts where NREGS was in place were 2 percentage points less likely to report schooling as the primary activity, and 4 percentage points more likely to report working as the primary activity. Finally, education is crucial for low-income households in India as a means to escape poverty, emphasizing the importance of examining NREGS's impact on education (Kingdon, 2007).

I compare NREGS with two types of cash transfer programs that are also widespread anti-poverty policies in developing countries. These cash transfer programs differ in whether they provide cash conditional on school enrollment (i.e. conditional cash transfers, or CCTs) or unconditionally (i.e. unconditional cash transfers, or UCTs), whereas both target beneficiaries via a proxy-means test. Theoretically, none of the three programs dominates the others in both targeting efficiency and improving school enrollment. Although both cash transfers are likely to have positive effects on school enrollment, they are subject to targeting errors, resulting in less efficient spending of program budgets as anti-poverty policies. I illustrate this trade-off by simulating the three programs under a fixed program budget with a structural model of labor supply decisions, as there were no other anti-poverty policies implemented simultaneously with NREGS.[2]

I develop a dynamic discrete choice model that describes the labor supply and schooling decisions of households with a single child. In the model, each household decides jointly whether the mother works outside or works at home and whether the child works outside, works at home, or goes to school. When they work outside, they receive market wages that depend on whether NREGS was implemented in their district. When they work at home, they produce non-tradable goods. Importantly, the mother and the child are substitutes in the household production function. When the child chooses schooling, then he or she obtains one year of education. The household makes a series of the joint decisions until the child leaves the household, which is set at the age of 18. I estimate this model with the 64th Round of the National Sample Survey (2007-2008) by leveraging the sequential rollout of NREGS across districts.

[2]Even though it is theoretically possible to compare workfare with cash transfers by running a randomized control trial with multiple treatment arms, that approach may not be the most appropriate to answer the research question, given that previous research has shown the possibility of the negative effects of workfare on education.

The model encompasses two channels through which NREGS induced school dropout. One is the general 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 mothers and children. Since NREGS increased female labor force participation through the employment reservation for women (and the general equilibrium effects on market wages), this channel made children shift from schooling to working at home.[3]

Using the estimated model, I examine the two hypothetical cash transfer programs in comparison with NREGS. In the first scenario, I compare the three programs without imposing budget neutrality and targeting. That is, households whose children choose schooling would receive the transfers under the CCTs while all households receive them under the UCTs. I find that the cash transfer programs dominate NREGS in terms of total household welfare and school enrollment rates. This is ex-ante expected as the first scenario ignores target efficiency. In the second scenario, I introduce budget neutrality, meaning that the sum of the transfer amount does not exceed the total NREGS expenditures. Since the number of beneficiaries under NREGS is smaller than under the CCTs and the UCTs, the budget neutrality will reduce the transfer amount in each program. I find the CCTs still outperform NREGS in both dimensions, whereas the UCTs only dominate in household welfare. In the third scenario, I additionally introduce targeting based on per-capita household expenditures, which creates inclusion and exclusion errors and lowers the efficiency of the cash transfer programs. I find that the results remain unchanged from the second scenario.

Finally, I compute the rates of returns that NREGS should yield to achieve the levels of household welfare under the two cash transfer programs. Since NREGS built infrastructure such as roads and irrigation, it could have had positive externalities on social welfare that my model does not capture. It is thus important to compute the rates of social returns to NREGS in order to determine whether the cash transfer programs are preferred over NREGS. My calculation shows that the rates of returns to NREGS should be, on average, 33 percent to achieve the same welfare under the CCTs and 79 percent under the UCTs, both of which are unreasonable high (for instance, India's annual GDP growth rates have been between 3 to 9 percent). This suggests that NREGS may be less desirable as an anti-poverty policy than the cash transfer programs that are implementable under the same program budget.

[3]Since fathers worked outside even prior to NREGS, their employment did not cause school dropout through the second channel.

I contribute to three strands of literature. First, there is a large body of literature estimating the short-run effects of NREGS on education (Li and Sekhri, 2013; Islam and Sivasankaran, 2015; Afridi et al., 2016; Shah and Steinberg, 2021) and labor markets (Azam, 2011; Imbert and Papp, 2015; Muralidharan et al., 2017; Berg et al., 2018; Zimmermann, 2020). I build my structural model on their findings to embed the two channels through which NREGS caused school dropout . Second, I contribute to the literature on the design of anti-poverty policies with a focus on how to accurately target beneficiaries (Ravallion, 2009; Alatas et al., 2012, 2016; Klasen and Lange, 2016; McBride and Nichols, 2016; Hanna and Olken, 2018). I extend this literature by illustrating how to evaluate anti-poverty policies across multiple dimensions jointly. Finally, this paper also fits 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 of a credible source of identification. However, those results are not informative of counterfactual policies in the same context (Heckman, 2010; Todd and Wolpin, 2010; Keane et al., 2011). Consequently, when doing an ex-ante policy evaluation, development economists usually rely on the meta-analysis of previous findings in other contexts. The structural approach, however, enables researchers to simulate alternative policies in the same context and disentangle the competing underlying mechanisms. This paper contributes to this literature by providing a new application that integrates reduced form results with a structural approach, following the work of Todd and Wolpin (2006); Attanasio et al. (2012).

The rest of the paper is structured as follows. Section 2 provides detailed information about NREGS and discusses previous findings about the effects of NREGS on schooling. Section 3 explains my structural approach. Section 4 presents simulation results. Section 5 concludes.

2 NREGS

2.1 Background

In 2005, the Government of India enacted the National Rural Employment Guarantee Act to reduce poverty in rural India through a workfare program. The National Rural Employment Guarantee Scheme (NREGS) was subsequently initiated in 2006 with an annual budget up to 2.5 percent of the Union Budget Expenditure.[4] The program guarantees 100 days of casual

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

labor in the public sector for individuals who are 18 years or older and live in rural areas. People can apply for employment at any time in a year, and should be employed within 15 days of the application or receive unemployment benefits. The workers are hired at the agricultural minimum wages in their states and engage in labor-intensive and unskilled manual tasks such as road construction and irrigation development. To encourage female labor force participation, NREGS reserves one-third of employment opportunities for women. In 2006-2007, approximately 21 million households were employed in NREGS.

NREGS rolled out sequentially in rural districts between 2006 and 2008. Rural districts were first ranked by an index that was computed based on agricultural wages, agricultural productivity per worker, and minority population (SC and ST) (Planning Commission, 2003). Then, NREGS was first implemented in the 200 most backward districts in the ranking in February 2006. NREGS subsequently entered the next 130 districts from the bottom in April 2007 and the remaining districts in April 2008. Previous research that estimated the effects of NREGS exploited this staggered introduction of NREGS as a source of identifying variation.[5] Figure 1 shows the implementation of NREGS in 2007-2008 at the district level, when NREGS did not roll out to the last group of districts.

It is worth acknowledging that recent studies have shown that the operation of NREGS was crucially dependent on administrative capacity. For example, NREGS failed to provide employment to all beneficiaries in poor states due to weak state capacity (Dutta et al., 2012). In addition, NREGS was sometimes used as a political tool by legislators (Niehaus and Sukhtankar, 2013; Niehaus et al., 2013; Gupta and Mukhopadhyay, 2016; Gulzar and Pasquale, 2017; Bardhan et al., 2020).[6] While the implementation of NREGS could have varied substantially across the districts, I abstract away from this issue for the tractability of my structural model. Therefore, my results can be interpreted as a best-case scenario where households can work in NREGS whenever they demand.

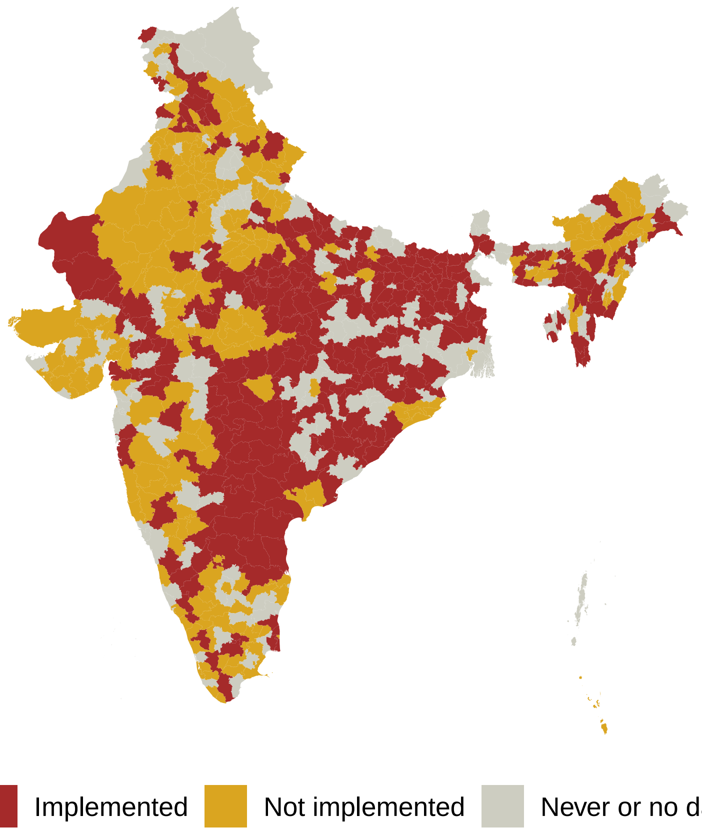
2.2 Effects of NREGS on schooling

As briefly reviewed in Section 1, the adverse effects of NREGS on school enrollment have been reported in the previous literature. Li and Sekhri (2013) found that school enrollment in primary

[5]Zimmermann (2020) instead used discontinuity in the index to estimate the effects of NREGS on labor markets.

[6]This could occur because block-level bureaucrats have discretion over the allocation of NREGS employment across villages. Since state-level politicians affect the promotion of these local bureaucrats, the politicians pressure the bureaucrats to allocate employment to their favorite places.

Figure 1: NREGS implementation in 2007-2008



schools in the districts where NREGS rolled out was lower than those in the absence of NREGS. Islam and Sivasankaran (2015) found that women spent 0.03 days more on work in the public sector, including NREGS employment per week, after NREGS was implemented. Regarding changes in children's time use in response to NREGS, the authors estimated that children aged 15 to 17 spent 0.2 days less on schooling and 0.13 days more on working outside. Shah and Steinberg (2021) found that in the districts where NREGS was in place, children aged 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 found heterogeneity across gender. While boys from those ages substituted schooling for working outside, girls did for domestic work such as household chores and taking care of younger siblings.[7] It is important to mention that these negative effects on education outcomes are not limited to NREGS. In fact, workfare programs in other developing countries also caused school dropout (Dammert et al., 2018).

[7]Given the varying enforcement of NREGS, there may be heterogeneity in the effects of NREGS on education across states. For instance, Afridi et al. (2016) showed that children in Andhra Pradesh spent 0.3 hours more at school per day when their mothers started working in NREGS.

Building on these previous findings, I consider two mechanisms for the increase in school dropout in response to the implementation of NREGS into my analysis. The first channel is the general equilibrium effects of NREGS on local labor markets (Azam, 2011; Imbert and Papp, 2015; Muralidharan et al., 2017; Berg et al., 2018; Zimmermann, 2020). When NREGS was implemented, it displaced local employment in the private sector, resulting in higher market wages due to the reduction in labor supply. The increased wages then may have induced school dropout through an increase in the opportunity costs of schooling. The second channel is the intrahousehold labor substitution between mothers and children. NREGS aimed to promote female labor force participation by reserving one-third of employment opportunities for women, who typically engaged in domestic work prior to the implementation of NREGS. This rationing, in addition to the general equilibrium effects mentioned above, raised the opportunity costs of domestic work for mothers. As a result, mothers started working outside once NREGS was implemented in their districts by making their children shift from schooling to domestic work, the degree of which varied across children's gender. In the next section, I construct a structural model of household labor supply decisions incorporating these two channels.

3 Structural Model

3.1 Model description

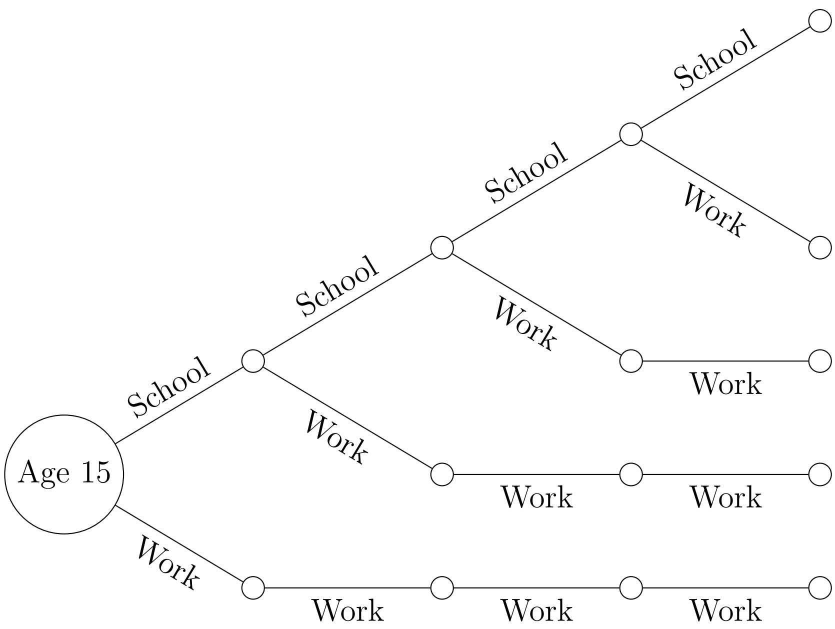
3.1.1 Overview

To conduct the comparative analysis of multiple anti-poverty policies in terms of targeting efficiency and effects on school enrollment, I take a structural approach. In particular, I construct a dynamic discrete choice model about household labor supply decisions, which can be estimated with data on NREGS, and simulate the effects of two cash transfer programs, cash transfers conditional on school enrollment (CCTs) and unconditional cash transfers (UCTs), with the estimated model, assuming that the cash transfers are purely income shocks to eligible households.

In the model, each household has a single child aged between 15 and 18, and determines labor supply decisions for the mother and the child jointly. The mother chooses one action from working outside (either in the private or the public sector) and working at home, whereas the child chooses an action from working outside (in the private sector only), working at home, and going to school.[8] The household chooses a pair of the mother's and the child's decisions to solve the utility maximization problem every year until the child becomes 18 years old. The dynamics of the model come from the accumulation of the child's education, bringing lump-sum returns to education at the terminal period. I assume that there is no interaction between households, and thus suppress the index for the household in the following description. The only index is thus t, which is the age of the child. I restrict my attention to this specific age range for two reasons; (1) only a small fraction of children younger than 15 years reported working as their primary activity in my data (0.65 percent for working outside and 3.3 percent for working at home), (2) a significant fraction of households in my data did not have a child older than 18 years (77.6 percent).

The key state variables are the child's age and years of education. While the age evolves deterministically, the transition of the years of education depends on the household's decisions. I make two assumptions on the evolution of the years of education to simplify the state space. First, the child does not repeat the same grade. In other words, conditional on going to school, the child always accumulates one additional year of education. Second, switching costs from working to schooling are so large that the child cannot choose schooling once choosing working. Figure 2 displays the possible sequences of decisions for children aged 15 under the second assumption.

Figure 2: Possible paths for children aged 15



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

While those assumptions are not testable in my data, I obtain supportive evidence from other datasets. First, the World Bank DataBank shows that the percentage of repeaters in lower secondary education (children in grades 9 and 10) in India in 2008 was around 5 percent.[9] Second, according to the Young Lives Survey, which is an individual panel dataset for a subset of districts in Andhra Pradesh and Telangana, less than 1 percent of children aged between 15 and 18 dropped out of school and re-enrolled in the subsequent years between 2009 and 2013 (Boyden, 2021).

3.1.2 Discrete choices

I denote by $A_t \in \{1,2,3,4,5,6\}$ the pair of choices made by the child $\left(= a_t^C \in \{1,2,3\}\right)$ and the mother $\left(= a_t^M \in \{1,2\}\right)$. Table 1 presents the numbering of these choices.

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)

3.1.3 Utility function and budget constraint for $t < 18$

The flow utility function consists of the Stone-Geary preferences over consumption, an additive separable preference over a non-tradable good that is produced via the household production function, and a choice-specific preference shock:

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

where C_t is the total consumption and $\underline{C}$ is the consumption of food, which proxies the subsistence level of consumption for the household. Q_t is the amount of the good produced in the household such as housework. β_Q is the parameter that governs substitution between utilities

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

from total consumption and the non-tradable good. $\varepsilon_t(A_t)$ is the choice-specific preference shock, which is unobservable to the econometrician.

I assume for simplicity that Q_t is produced by a linear production technology that employs the labor supply by the mother and the child:

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

where $g \in \{m, f\}$ denotes the gender of the child. X_t^H is the vector of household characteristics that affect the household production, such as the household size and the total number of children. This parameterization concisely describes the intrahousehold substitution between the mother and the child. I allow the degree of the labor substitution to vary across the gender of the child to reflect potentially gender-specific substitution patterns.

The flow budget constraint the household faces is defined below:

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

Y_t is the earnings by the household head, ω_t^j is the earnings by the household member $j \in \{C, M\}$, and S_t is the cost of schooling varying across the levels of education. I assume that the earnings are subject to a transitory shock in local labor markets that is unobservable to the household when making the decisions.[10] Therefore, the expected earnings, not the actual ones, enter the budget constraint.[11]

3.1.4 Earnings

The earnings for each household member $j \in \{C, M\}$ are obtained from the following reduced form equation:

$$\ln \omega_{td}^j = \gamma_1^j \mathrm{NREGS}_d + D_d \phi^j + X_{td}^j \gamma^{\omega,j} + \eta_{td}. \tag{2}$$

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

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

NREGS_d is the indicator of whether NREGS was implemented in district d, which corresponds to the general equilibrium effects of NREGS on labor markets. D_d is the vector of district characteristics that should account for differences between districts with and without NREGS, as NREGS did not roll out in districts randomly. The variables included are the total number of employees in 2005, the total number of establishments in 2005, the total population in 2001, the SC and the ST populations in 2001, and the population of the literate in 2001. X_{td}^j is the set of individual characteristics that affect the earnings, including age and the gender of the child. η_{td} is the transitory shock on earnings in the district.

Note that the earnings do not depend on education attainment. This is because a large fraction of adults in my data do not have any formal education and because I assume that the child will take up the jobs that such adults used to do.

3.1.5 Human capital accumulation and utility function at $t = 18$

As stated in Section 3.1.1, I consider the following process of human capital accumulation:

$$Edu_t = \begin{cases} Edu_{t-1} + 1 & \text{if } A_\tau \in \{3,6\} \text{ for } \forall \tau \leq 18 \\ Edu_{t-1} & \text{otherwise} \end{cases}$$

Thus, the household can keep accumulating human capital as long as the child chooses schooling every period.

Then, at the terminal period, the household receives lump-sum returns varying by years of education. I parameterize the returns as a flexible function of years of education:

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

Given the returns to education formulated above, the utility function at the terminal period becomes the following:

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

Unlike the standard life-cycle model where the returns to education enter a wage function, the returns are defined as an additive separable utility in this model. This is because the returns to education in my model can include non-pecuniary values that children could receive in the future. For instance, education attainment is beneficial in marriage markets in developing countries (Jayachandran, 2015; Ashraf et al., 2020). Since I cannot describe the entire path of children's lives due to the lack of long panel data, I allow flexibility in the definition of what education brings to children.

3.1.6 Maximization problem

The maximization problem the household solves is formulated as follows:

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

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

3.2 Data

My sample comprises households with a mother and a child aged between 15 and 18 who reside in the districts where NREGS entered eventually.[12] The primary dataset to construct the sample is the 64th Round of the National Sample Survey (NSS). The NSS is a nationally representative household survey conducted by the National Sample Survey Organization to gather information on labor force participation. The 64th Round was conducted between July 2007 and June 2008.[13] The sampling was based on the population census in 2001 to be representative at the district level. The survey was conducted in 4 sub-rounds, each of which lasted three months and covered the same number of villages and blocks. In the 64th Round survey, more than 60 percent of households had only one child aged between 15 and 18.

I focus on this round for two reasons. First, the 64th Round was the only year when there was spatial variation in the rollout of NREGS. As stated in Section 2.1, NREGS entered the final group of districts in April 2008. I assume that households in those districts did not have opportunities to work in NREGS at the time of the survey.[14] Second, I cannot construct

[12] There are districts where NREGS is never implemented such as urban areas.

[13] This round did not cover the following areas: Ladakh 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.

[14] I cannot exclude the possibility that 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 downward in my

household panel data by combining the 64th Round with the previous rounds (though it is possible to construct panel data at the district level), as the NSS is a repeated cross-section survey. This could be a limitation on identification because the source of identifying variation comes from across households only. I discuss how the cross-sectional variation across households helps my identification in Section 3.4.

The discrete choices are constructed based on each household member's reported primary activity over the last 365 days. I 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." Similarly, I define a choice 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, I define a choice as "school" if the primary activity is "attended educational institutions."

Other key variables are constructed as follows. The food consumption comes from data on household expenditures over the last 30 days. I then multiply the expenditures on food consumption by 12 to convert them into an annual term. The cost of schooling at each education level is estimated based on household expenditures on children's education, as described in Appendix A. For the earnings of household heads, I use the self-reported income of household heads over the last 7 days, and convert it into an annual term by multiplying it by 52. For the human capital accumulation, I calculate years of education based on the highest levels of education attained by the time of the survey. I assume 5 years of education if the respondent completed the primary school, 8 if the upper primary school, 10 if the secondary school, and 12 if the upper secondary school.[15]

I supplement the NSS data with several datasets. First, I obtain the list of districts where NREGS was implemented from the Ministry of Rural Development website.[16] I also use the population census in 2001 and the economic census in 2005 available in the SHRUG (Asher et al., 2021). Because NREGS did not enter rural districts randomly and my dataset is a single cross-section survey, I need to account for the selection of NREGS districts, which depended

model.

[15]In India, the school system is structured as follows: 8 years for elementary education, which is compulsory, 2 years for lower secondary education, 2 years for secondary education, and 3 years for higher education. Thus children in that age range are after elementary education but before higher education.

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

on agricultural production and the size of the marginalized population in 1997. To avoid collinearity with the NREGS dummy variable by including the exact variables, I construct the SC and ST population shares and literacy rates 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. I include those variables in Equation (2).

Table 2 presents the summary statistics of household and district characteristics. In my sample, households are more likely to have sons aged from 15 to 18 than daughters (57.3 percent and 42.7 percent). On average, children's years of education are 6.8 years, which is much lower than the expected level of education for their age group if they continue schooling. While I focus on mothers and their children from that age group, households are likely to have one more younger child. Lastly, NREGS rolled out into slightly more than half of the districts in my data.[17]

3.3 Estimation

3.3.1 Parametric assumptions

In order to estimate this model, I need to make several parametric assumptions. First, I assume that the preference shocks are observed by the household, are i.i.d across the choices and time, and follow type I extreme value 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 of $N(0, 0.1)$. Third, preference and wage shocks are independent of each other. Fourth, the discount factor is set at 0.98, as it is not identified in standard dynamic discrete choice models (Rust, 1994; Magnac and Thesmar, 2002; Kasahara and Shimotsu, 2009).

3.3.2 Discretization

The estimation of my model requires continuous variables to be discretized in order to ease the computational burden. I discretize the variables using k-means clustering, which classifies observations into clusters of similar characteristics. I choose the optimal number of clusters by minimizing the total sum of within-cluster variance.[18] Combining with other discrete variables

[17] I lose some of the districts where NREGS was eventually implemented in the data construction process due to the different spelling of the district names across the datasets.

[18] I find that the optimal number of clusters is 4 when using a function *fviz_nbclust* with *wss* with a given algorithm in R. I also try different algorithms available in R, all of which suggest that the optimal number of clusters should be close to 4.

Table 2: Household and district characteristics

	(1) Obs.	(2) Mean	(3) SD	(4) Min	(5) Max
A: Household characteristics					
Mother's age	10908	41.792	6.234	20	60
Child's age	10908	16.183	1.009	15	18
= 1 if daughter	10908	0.427	0.495	0	1
Child's years of education	10908	6.795	3.513	0	12
Household size	10908	5.550	1.914	3	23
Number of children	10908	2.293	1.355	1	9
Household head's earnings (Rs)	10908	24988	54204	0	1560000
Household expenditures (Rs)	10908	52266	32904	4788	635868
Food expenditures (Rs)	10908	26838	12281	2433	192355
B: District characteristics					
= 1 if NREGS	451	0.534	0.499	0	1
Primary schools (per 10000 population, 2001)	451	8.507	5.365	0.588	41.219
Upper primary schools (per 10000 population, 2001)	451	2.559	1.822	0.000	10.213
Secondary schools (per 10000 population, 2001)	451	0.931	0.710	0.000	6.255
Upper secondary schools (per 10000 population, 2001)	451	0.259	0.245	0.000	2.066
Colleges (per 10000 population, 2001)	451	0.052	0.055	0.000	0.332
Share of nonagricultural employment (2005)	451	0.065	0.038	0.000	0.471
Number of establishment (per capita, 2005)	451	0.029	0.013	0.000	0.085
Share of SC population (2001)	451	0.154	0.084	0.000	0.419
Share of ST population (2001)	451	0.148	0.248	0.000	0.981
Literacy rate (2001)	451	0.533	0.118	0.242	0.854

Note: Panel A uses the main dataset from the 64th Round of the NSS. Panel B uses the 2001 population censuse and the 2005 economic cnesus available in the SHRUG.

in the model, I obtain the sample of 241 types for estimation. This discretization allows me to solve the utility maximization problem for 241 types of households, instead of 10908 households.

3.3.3 Maximum likelihood estimation

Given the parametric assumptions and the discretization, I run a maximum likelihood estimation. Because of the discretization, I compute the total likelihood by multiplying the individual likelihoods for each type of household by the number of households in that type. Therefore, my maximization problem is defined as follows:

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

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

The log-likelihood function of type g household choosing the discrete choice $a \in \{1,\ldots,6\}$ can be decomposed into two terms:

$$\begin{aligned}\ln \mathcal{L}_g(k:\Theta) &\equiv \ln \mathcal{L}(A_g = a, Edu_g : \Theta|\Omega_g) \\ &= \ln P(A_g = a, Edu_g : \Theta|\Omega_g) \\ &= \ln P(A_g = a : \Theta_1|\Omega_g, Edu_g) + \ln P(Edu_g : \Theta_2|\Omega_g),\end{aligned} \tag{3}$$

where $\Theta = \begin{pmatrix} \Theta_1 & \Theta_2 \end{pmatrix}'$. The third line follows from the Bayes' theorem. The first term in Equation (3) is the conditional choice probability, which is the solution to the household maximization problem. I have a closed-form expression for this probability due to the logit preference errors. The derivation of the conditional choice probability is described in Appendix B.

The second term in Equation (3) is the likelihood of the years of education observed in the data. Since I do not observe the entire history of human capital investment decisions in my data, I parameterize this term with an ordered probit function, following Attanasio et al. (2012). For each household type g, the probability of having Edu_g years of education is defined as follows:

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

where $Z = \begin{pmatrix} X^H & X^C \end{pmatrix}$ and K is the vector of school availability in district d prior to NREGS such as the number of primary, upper primary, secondary, upper secondary schools, and colleges per 10000 population from the population census 2001. These variables help the identification of the parameters in the probit function as they are excluded from the conditional choice probability.

Notice that there is no parameter that appears in both the conditional choice probability and the probability of the initial years of education. This is because I do not include time-invariant unobserved heterogeneity in my model due to the non-identification of that variable in cross-sectional data (Kasahara and Shimotsu, 2009). Consequently, the lack of the unobserved heterogeneity avoids the initial conditions problem for identification (Heckman, 1987). This model structure allows me to estimate the parameters by maximizing each term in Equation (3) separately.

3.4 Identification

Since my dataset consists of the single cross-section survey, the identification relies on cross-sectional variation across households. The parameter that governs substitution between the marginal utility from consumption and the non-tradable good should be identified through variation in consumption levels and the concavity of the utility function. The parameters associated with the returns to education are identified through the covariation of consumption levels and years of education. For instance, a household with a low level of consumption but a large number of years of education must have substantial returns to education. While it is evident from my data that the returns to education should be increasing as a considerable fraction of households in the data choose schooling, it is ex ante not obvious whether the returns should be concave or convex. Thus, my identification argument should be valid up to the first derivative of the returns to education.

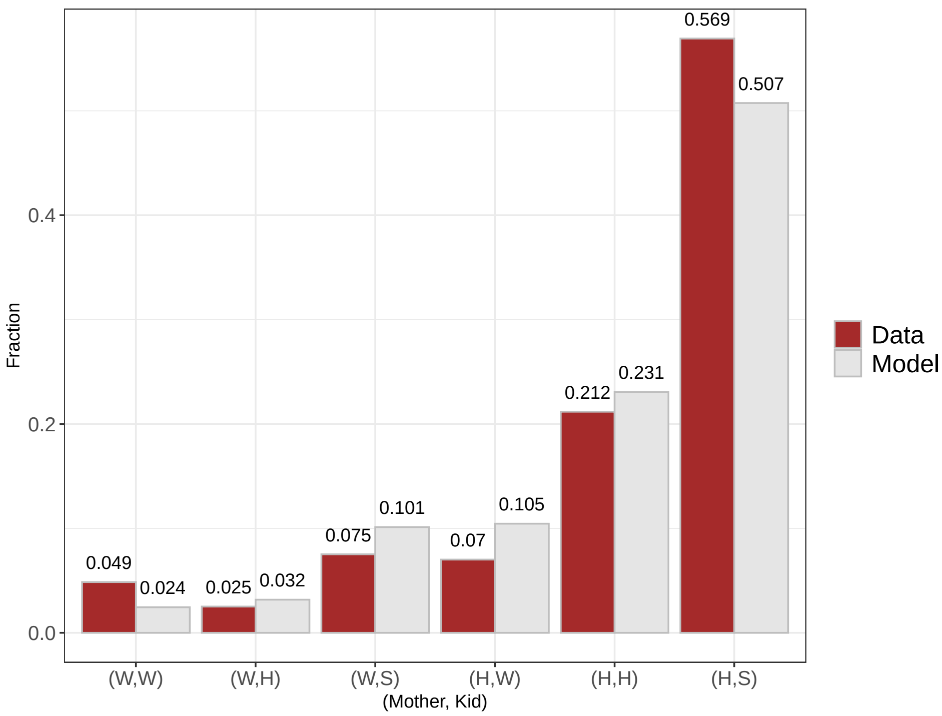
In addition to cross-sectional variation across households, my identification also exploits the excluded control variables. For instance, the parameter associated with NREGS is identified at the district level, conditional on the district characteristics that account for the selection of NREGS districts. This means that the identifying variation for the parameter comes from variation in the NREGS implementation across the districts as well as variation in the control variables within the same NREGS implementation status. A similar argument should hold for the parameters in the household production function and the ordered probit, where the excluded variables are household characteristics and the availability of schools for each education level, respectively.

4 Comparative Analysis

4.1 Estimation results

To validate my estimation results, I compare the discrete choices observed in the data with those simulated within my model. Figure 3 summarizes the comparison. First of all, despite the parsimonious model structure, my model captures the overall pattern of the distribution of the observed choices. For instance, my model predicts that the largest fraction of households choose schooling for the child and working at home for the mother, and the second-largest choose working at home for both of them, both of which are aligned with the observed distribution. The discrepancy between observed and predicted choices comes mainly from the choices of the child. In particular, I underestimate the overall fraction of households choosing schooling by 3.5 percentage points, whereas I overestimate the fraction of households with the children working at home by a similar magnitude. Overall, however, my model is a good approximation of the household behaviors under NREGS.

Figure 3: Model fitness



4.2 Cash transfer programs

Using the estimated model, I compare the performance of counterfactual cash transfer programs with NREGS in terms of targeting efficiency and effects on school enrollment. The candidate policies are a cash transfer program conditional on schooling (CCTs) and an unconditional cash transfer program (UCTs), both of which have been widely implemented in developing countries. Targeting efficiency is measured by the total household welfare each program generates under a given program budget. In contrast, effects on schooling are computed as the share of households choosing schooling for their children in each program.

Making an ex ante prediction about which policy outperforms the others is difficult. While NREGS should dominate the cash transfer programs in targeting efficiency if the latter have large targeting errors. This is because the cash transfers may be delivered to households with a low marginal utility of consumption, while those with a high marginal utility of consumption may not receive them when targeting is not accurate. Due to the concavity of the flow utility function, this may lead to lower total household welfare. The cash transfer programs, however, dominate NREGS with respect to effects on school enrollment as they are likely to induce schooling, whereas NREGS causes school dropout. Comparing the two cash transfer programs, the UCTs are expected to have higher targeting efficiency, as they are distributed to a larger fraction of households, while the CCTs are expected to have a greater impact on school enrollment, as they are directly tied to schooling decisions.

In order to simulate the household behaviors under the cash transfer programs, I need to determine the amount of cash transfer for each program. I set the amount equal to the per-household total expenditures of NREGS in 2007-2008, which varied across the districts.[19] Table 3 is the summary statistics of the per-household expenditures. Around 70 percent of the expenditures were spent on labor-related expenses such as wage payment in 2007-2008. The resulting transfer is, on average, 2.65 percent of the annual household expenditures in my data. When simulating my model under the cash transfer programs, I shut down the effects of NREGS on the potential earnings, and add the transfer amount to the budget constraints for households in the NREGS districts every period.[20]

4.3 Scenario 1: benchmark

I start my analysis by comparing NREGS with the cash transfer programs, ignoring budget neutrality and targeting. This scenario serves as a sanity check because NREGS should perform worse than the cash transfer programs when targeting errors are not taken into account. I plot

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

[20] It is worth noting that I do not consider administrative costs of the cash transfer programs, such as expenditures on planning surveys. Since I do not take them into account when calculating the transfer amount, the resulting amount should be considered as the upper bound of the transfer amount households could receive.

Table 3: NREGS expenditures per household in 2007-2008

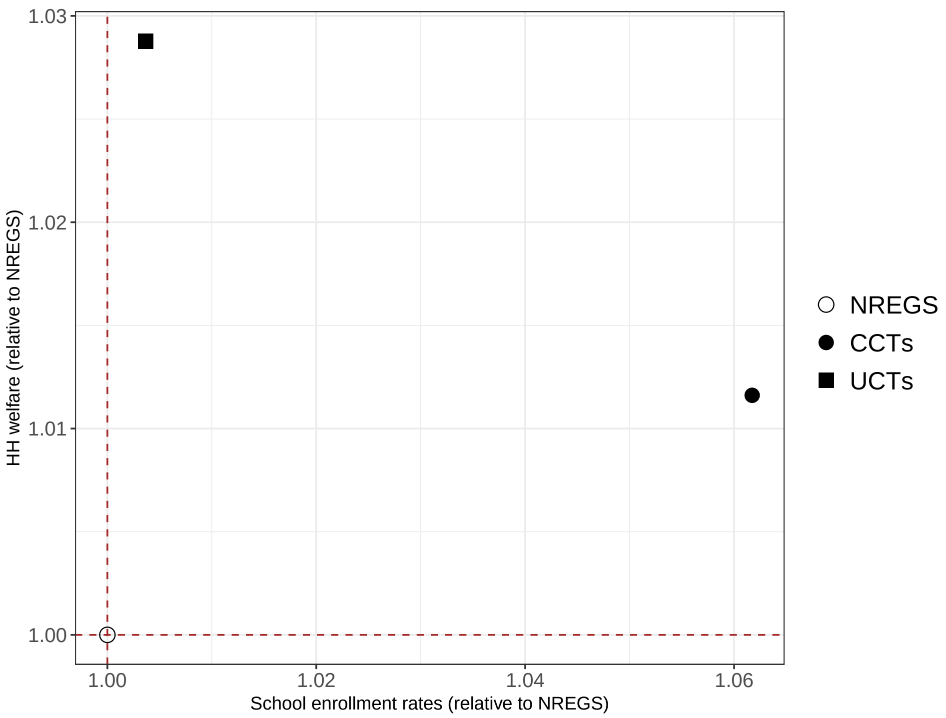
	(1) Obs.	(2) Mean	(3) SD	(4) Min	(5) Max
Total expenditures (Rs, per household)	451	1384	1884	0	7788
Share on labor	240	0.729	0.146	0	1
Share on materials	240	0.271	0.146	0	1

Note: Total expenditures consist of labor- and matrial-related expenditures. Share on labor and Share on materials are not computed for districts in which NREGS was not implemented.

the results in Figure 4. The horizontal axis is school enrollment rates, and the vertical axis is the total household welfare, both of which are expressed relative to the values under NREGS. The total welfare and school enrollment rates are computed only for households in the NREGS districts.

In Scenario 1, both the CCTs and the UCTs are in the top-right region of NREGS, which indicates that they dominate NREGS in both metrics. The CCTs have a 6 percent higher school enrollment rate and 1 percent greater total household welfare. The UCTs achieve much higher household welfare and a slightly higher school enrollment rate. These results are consistent with my ex ante predictions.

Figure 4: Household welfare and school enrollment rates: Scenario 1



Note: The figure plots the total household welfare and school enrollment rates under each program, measured relative to those under NREGS. The transfer amount is the per-household total expenditures of NREGS in both cash transfer programs. The school enrollment rate under NREGS is 0.576.

4.4 Scenario 2: budget neutrality

I now introduce budget neutrality to my simulation analysis, which changes the amount of cash transfers in the CCTs and the UCTs. I compute the new transfer amount by identifying the fraction of per-household NREGS expenditures that do not exceed the total NREGS expenditures when multiplied by the number of the beneficiaries. That is, for the CCTs, I solve the following problem:

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

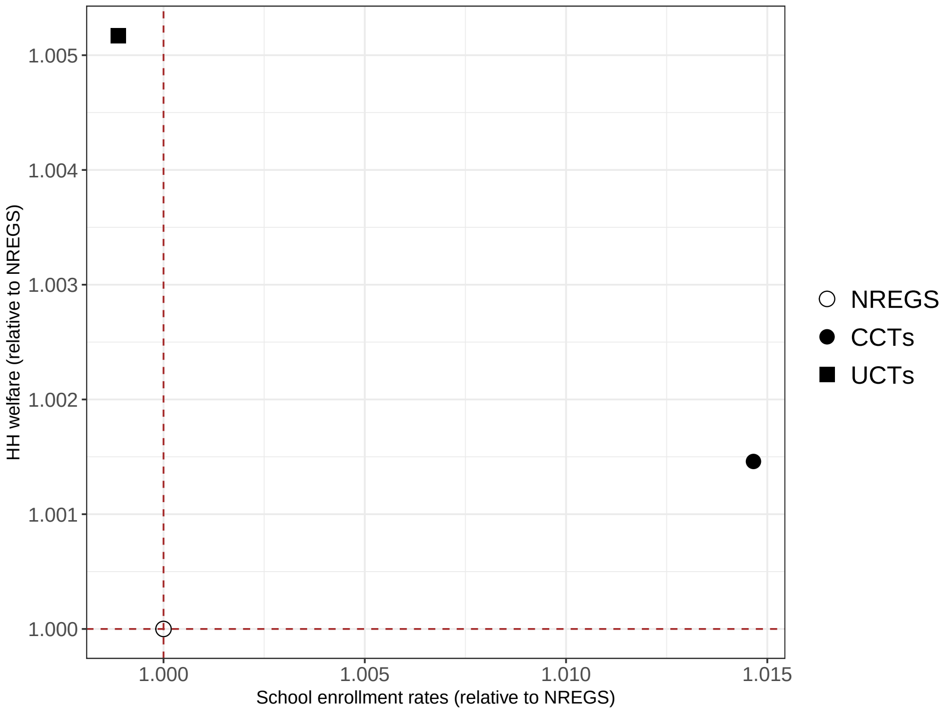
where N_d^S is the number of households who choose schooling under the CCTs and N_d^W is the number of households who choose working outside under NREGS, in district d. I compute the transfer amount under the UCTs similarly. I find $\delta^* = 27$ for the CCTs and $\delta^* = 16$ for the UCTs.

Figure 5 show the simulation results under Scenario 2. First, compared to Scenario 1, the effects of both cash transfers are smaller. The CCTs in Scenario 2 increase the school enrollment rate by 1.5 percent and the total household welfare by 0.15 percent, relative to NREGS, whereas 6 percent and 1 percent in Scenario 1, respectively. This is consistent with the fact that the transfer amount is reduced to 27 percent of the previous amount. Second, unlike Scenario 1, the UCTs now have a lower school enrollment rate than NREGS. Since the transfer amount under the UCTs in Scenario 2 may not be enough to compensate for wage gains from NREGS, some households may have a higher marginal utility of consumption today than tomorrow. As a result, they may have the children switch from schooling to work to increase today's consumption. This change does not happen in the CCT program because households can gain additional consumption today by sending their children to school.

4.5 Scenario 3: targeting

Finally, I incorporate targeting into the cash transfer programs to highlight the importance of targeting efficiency. Targeting is a crucial component when discussing the cost-effectiveness of anti-poverty policies. Because household or individual income is rarely observed in developing countries, targeting needs to rely on a proxy such as observed assets, leading to both inclusion and exclusion errors (Klasen and Lange, 2016; Hanna and Olken, 2018). This implies that

Figure 5: Household welfare and school enrollment rates: Scenario 2



Note: The figure plots the total household welfare and school enrollment rates under each program, measured relative to those under NREGS. The transfer amount is 27 percent of the per-household total expenditures of NREGS in the CCTs and 16 percent in the UCTs. The school enrollment rate under NREGS is 0.576.

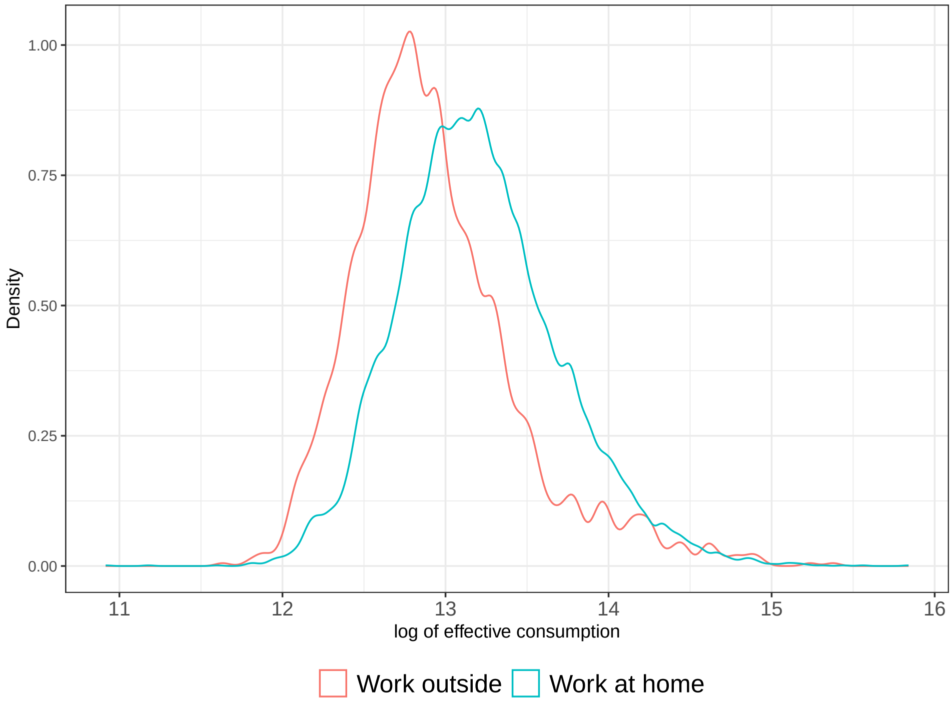
self-targeting programs have an advantage in targeting efficiency (Alatas et al., 2016). Figure 6 illustrates that households whose mothers choose working outside have lower consumption than those who choose working at home, suggesting that poorer households select into NREGS in my data.

The targeting I consider here is based on per-capita household expenditures. I do this targeting for three reasons. First, targeting based on assets is not possible in my context due to data limitations. Second, it is also impossible to use the targeting strategy used in other welfare programs in India, which is the possession of the Below Poverty Line (BPL) cards, as this information is not available in my data. Households can receive the BPL cards if they satisfy the inclusion and exclusion criteria set by the central government.[21] However, since those criteria require detailed information about asset holdings, it is not plausible to replicate them in my data.[22] Third, targeting based on per-capita household expenditures demonstrates the potential inefficiency in targeting. The cross-tabulation in Table 4 shows that 42 percent of households with the per-capita household expenditures below their median have the household

[21] See Alkire and Seth (2013) for more details about these criteria.

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

Figure 6: Self-targeting into NREGS



Note: The effective consumption is defined as the total household expenditures less food consumption (or $C_t - \underline{C}$ in the model). "Work outside" and "Work at home" are based on mothers' choices.

head's earnings higher than their median. Given that the household head's earnings account for a large portion of total household income, this indicates that targeting based on the per-capita expenditure may result in the inclusion and exclusion errors when identifying beneficiaries. Therefore, this targeting underlines the importance of self-targeting in NREGS. After identifying beneficiaries for the CCTs and the UCTs, I compute the transfer amount for each program as I did in Section 4.4.

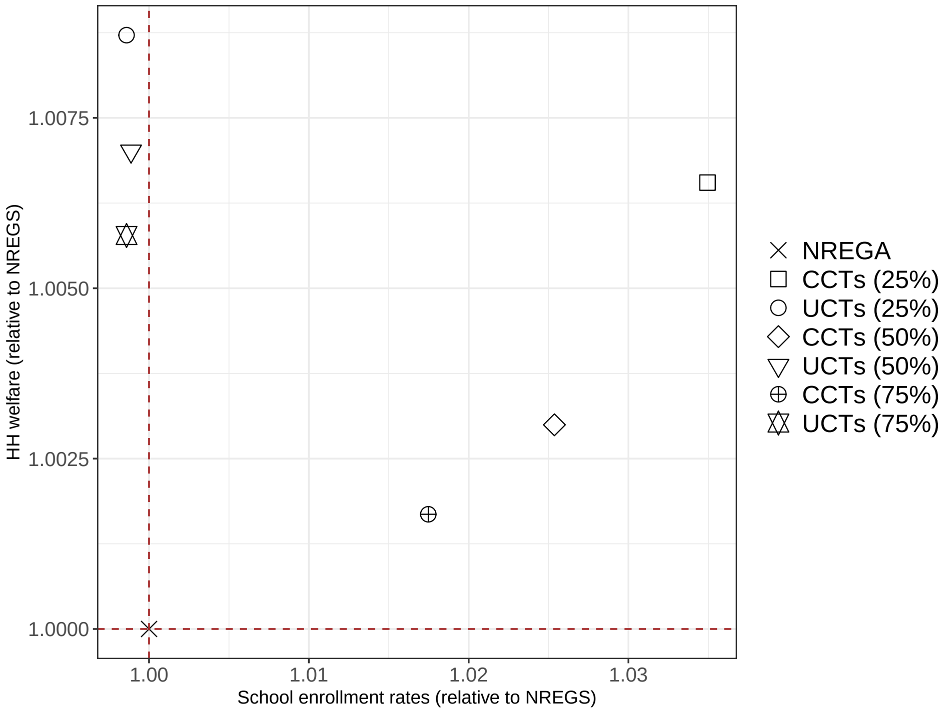
Table 4: Earnings of household head and household expenditure below sample median

	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.

Figure 7 presents the results in Scenario 3. The transfer amount is set at 51 percent of the per-household expenditures for the CCTs and 26 percent for the UCTs. The relative performance of the two cash transfer programs remains unchanged from Scenario 2. The CCTs still dominate NREGS in both dimensions while the UCTs outperform in the household welfare. To check the robustness, I also plot results when the eligibility condition is set as below the bottom quartile and the top quartile of the sample. The relative performance is robust across the different eligibility conditions.

Figure 7: Household welfare and school enrollment rates: Scenario 3



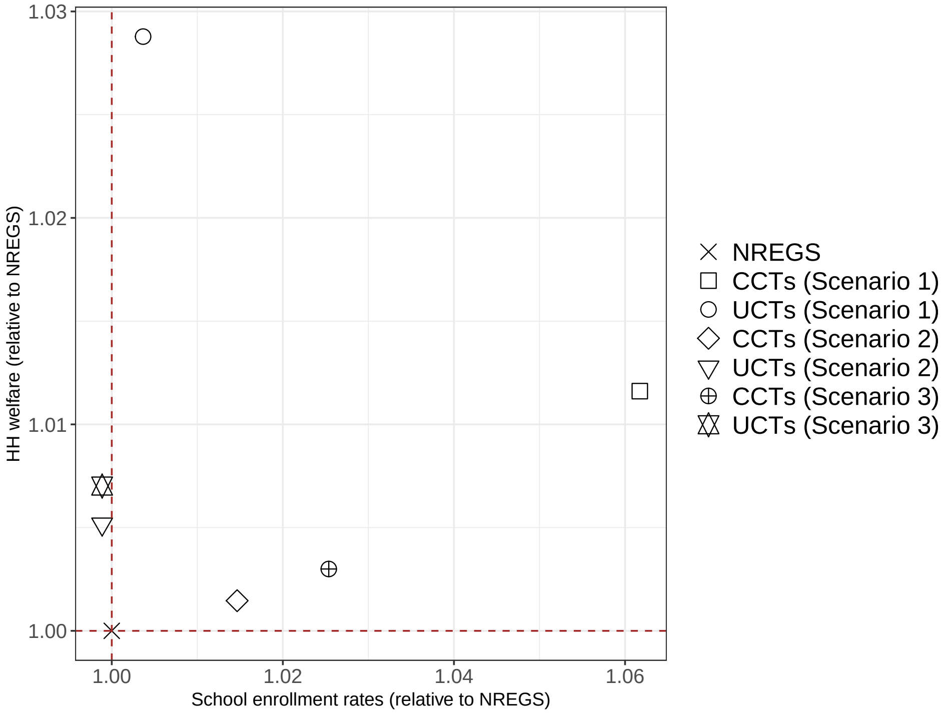
Note: The figure plots the total household welfare and school enrollment rates under each program, measured relative to those under NREGS. The transfer amount is 51 percent of the per-household total expenditures of NREGS in the CCTs and 26 percent in the UCTs when targeting the households with the per-capita expenditures below the sample median. When targeting those with the per-capita expenditures below the bottom quartile, the transfer amount is 100 percent for the CCTs and 49 percent for the UCTs. When targeting below the top quartile, the transfer amount is 35 percent for the CCTs and 16 percent for the UCTs. The school enrollment rate under NREGS is 0.576.

4.6 Social returns to NREGS

Figure 8 reproduces my counterfactual simulations in each scenario. I show that the CCTs strictly dominate NREGS both in the total household welfare and the school enrollment rate. The UCTs have the largest total household welfare, whereas the school enrollment rate is close to that under NREGS. These results suggest that the cash transfer programs should be more desirable than NREGS, in spite of their targeting errors.

However, since NREGS operated at a large scale, it could have had an aggregate (and positive) impact on the economy that my structural model is not able to capture. One example is that because workers in NREGS engaged in the construction of infrastructure, NREGS may have stimulated economic activities around the NREGS districts (Cook and Shah, 2022).

Figure 8: Household welfare and school enrollment rates: all scenarios



Note: For Scenario 3, I display the results when the eligibility condition is the per-capita household expenditure below the sample median.

Another example is that NREGS may have contributed to reducing civil conflicts (Khanna and Zimmermann, 2017; Fetzer, 2020). These general equilibrium effects are missing in the model unless they affect labor supply decisions through the labor market channel. The natural question is how large these aggregate effects should be to make NREGS preferred over the cash transfer programs in terms of the total household welfare.

I address this question by calculating the amount of money required to eliminate differences in the total household welfare between NREGS and the cash transfer programs in Scenario 3. I 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, I solve the following minimization problem:

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

where $\mathrm{CT} \in \{\mathrm{CCTs}, \mathrm{UCTs}\}$ is an index for the cash transfer programs. After obtaining M_i^{CT} for each household, I aggregate it at the district level, separately for each cash transfer program. Then, I divide the total amount by the total NREGS expenditure to define the rates of returns for each program at the district level.

Table 5 shows the rates of returns to NREGS expenditures to yield the same levels of household welfare under each cash transfer program. To achieve the same level of the total household welfare under the CCTs, NREGS should yield a rate of returns equivalent to 32.7 percent on average. The rate is much higher to achieve the level of the total household welfare under the UCTs, which is 79 percent. Provided that India's annual GDP growth rate has been between 3 to 9 percent since 2000, these rates of returns to the NREGS expenditures appear to be unreasonably high.[23] Since the cash transfer programs, especially the CCTs, do not have adverse effects on school enrollment, I conclude that NREGS should be less preferred than the counterfactual cash transfer programs that can be implementable under the same program budget, or NREGS should be implemented without harming children's education.

Table 5: Amount of money to eliminate welfare differences

	(1) Obs.	(2) Mean	(3) SD	(4) Min	(5) Max
Equivalent to CCTs	239	0.327	1.802	0.010	20.682
Equivalent to UCTs	239	0.791	3.922	0.010	37.232

Note: Each row represents the rates of returns to NREGS to yield the same levels of household welfare under each cash transfer program. We divide the amount of money needed for each household to eliminate differences in household welfare by the per-household total NREGS expenditures. One district is dropped from the calculation due to an outlier.

5 Conclusion

This paper evaluates an Indian workfare program against counterfactual cash transfer programs in terms of targeting efficiency and effects on school enrollment. The workfare program can target beneficiaries accurately because of self-targeting relative to other targeting methods such as a proxy-means test. On the other hand, the workfare program caused school dropout among the beneficiaries' children, especially those of secondary school ages. To examine whether targeting efficiency outweighs adverse effects on school enrollment, I estimate a dynamic model of labor supply decisions under NREGS, and analyze the performance of conditional and unconditional cash transfer programs relative to NREGS.

My empirical findings show that under the same amount of public expenditures, NREGS is strictly dominated by the CCTs and weakly dominated by the UCTs. To explore the possibility that NREGS could outperform the two in the total household welfare when taking into account

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

its social returns, I compute the rates of returns to the total NREGS expenditures that eliminate the welfare differences between NREGS and the cash transfer programs. I find that NREGS should generate unreasonably high rates of social returns, suggesting that the cash transfer programs should be preferred over NREGS in improving the total household welfare and school enrollment rates. Although NREGS has been extensively studied in economics, a small number of papers have attempted to discuss whether NREGS is desirable as an anti-poverty policy. This paper introduces a new methodology to evaluate NREGS in comparison with other policies.

Some limitations of the analysis should be noted for future research. First, my sample is not necessarily representative. I focus on households with one child aged 15 and 18 not to consider birth order effects and son preferences that are prevalent in India (Jayachandran and Pande, 2017). While the majority of households in my data have such a child only, I could extend my model to include households with different family structures to obtain more generalizable insights. Second, my counterfactual analysis does not consider any administrative costs of the cash transfer programs such as targeting and setting up the distribution system, which may account for a sizable fraction of the program expenditures. For instance, Caldés et al. (2006) computed the cost-transfer ratio, which is the ratio of the administrative costs to the total amount of cash transfers, for three conditional cash transfer programs, and found that the cost-transfer ratio varied from 0.10 to 0.63 across the programs. This suggests that the administrative costs in those programs accounted for 10 to 40 percent of the total program expenditures. Ignoring the administrative costs hence may result in the amount of cash transfers in my simulation analysis being significantly overestimated. More details about the cost structure of the cash transfer programs should help validate my analysis. Finally, cash transfers under the CCTs and the UCTs can have larger effects on school enrollment rates than the equivalent amount of cash earned by households (Attanasio et al., 2012). If this is the case, then my analysis may underestimate the effects of the two programs. Despite these limitations, however, this paper provides an empirical framework to conduct the comparative analysis of multiple policies using an economic model.

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Appendix

A Estimation of costs of schooling

To solve the utility maximization problem in Section 3.1.6, I need to construct the costs of schooling at each education level. Since data on the annual costs of schooling may not exist in the Indian context, I estimate them using household expenditures on schooling in my data. In particular, for household i in state s, I run the following OLS regression:

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

where independent variables are the number of children attending school at each education level.

Table A.1 presents a mapping between years of education and education levels. I take the coefficient estimates as the estimated costs of schooling. The coefficients vary across states because education systems are decentralized at the state level in India.

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

Table A.2 presents the summary statistics of the estimated school costs. The estimated school costs are higher for higher education levels.

Table A.2: Estimated school costs

	(1) Obs.	(2) Mean	(3) SD	(4) Min	(5) Max
Primary schools (per capita, Rs)	28	1456	877	560	3901
Upper primary schools (per capita, Rs)	28	1468	686	623	3299
Secondary schools (per capita, Rs)	28	2653	1314	1066	6475
Upper secondary schools (per capita, Rs)	28	5987	4261	2998	26059
Colleges or higher (per capita, Rs)	28	7959	4530	2770	24506

Note: School costs are estimated with household expenditures on schooling and vary across education levels and states.

B Derivation of conditional choice probability

In order to derive the conditional choice probability in Equation (3), I solve the maximization problem using backward induction. At $t = 18$, the maximization problem is defined as follows:

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

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

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

Conditional on the choice that the household will make at $t = 18$, the maximization problem at $t = 17$ now becomes the following:

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

where U_{18}^{*} is the value function at $t = 18$ and β is the discount factor. Thus, the probability of observing choice $A_{17} = a$ is defined as follow:

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

Using the property of the type I extreme distribution, I obtain a closed-form expression for the continuation value:

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

where $\gamma = 0.577216$ is the Euler constant, $U_{18}\left(\cdot\right) = U_{18}$ is the flow utility function at $t = 18$, and $\tilde{U}_{18}\left(\cdot\right)$ is $U_{18}\left(\cdot\right)$ minus the preference shock. The conditional choice probabilities at $t = 15, 16$ are calculated similarly.