Heterogenous Impacts of Unconditional Cash Transfers: Applying Machine Learning Methods to Evidence from Uganda

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Motivation

Cash transfers have risen in popularity as a poverty alleviation tool in the past decade. Economic theory suggests that because of capital constraints, the poor are limited to less efficient and diminishing returns technology in their microenterprises, creating a vicious poverty cycle (Banerjee et al., 2019). Cash transfers and credit access alleviate this by reducing the minimum wealth level at which microentrepreneurs can switch into more efficient technology, allowing them to escape the poverty trap (Gertler et al., 2012; McKenzie and Woodruff, 2006). Studies have shown that cash transfers consistently produce positive productivity impacts (Bastagli et al., 2016). However, average treatment effects often mask important heterogeneity. This is important for unconditional cash transfer (UCT) programmes since usage of funds is not monitored and donors have limited funds to disburse. Knowing who is most likely to benefit allows for targeted transfers that achieve the best bang for the buck.

Literature review

Because the poor are often credit-constrained (Berg, 2013), multiple studies have focused on initial wealth and human capital in their search for heterogeneity. In the absence of cheap credit, an unexpected windfall should benefit high-ability but capital-starved types more compared to the low-ability and less credit-constrained. High ability types will invest in enterprise until efficient scale is reached, while low ability types will consume or save their windfall and will not invest even under perfect credit markets. Rich, high ability individuals will already be investing at efficient scale and will likewise save or consume their windfall. However, empirical literature on heterogeneity in UCTs have turned up conflictual findings. A randomised study of a UCT in northern Uganda (Blattman et al., 2014) found that economic impacts were decreasing in initial wealth and increasing in entrepreneurial ability, though neither of these were statistically significant. Macours et al (2013) study an intervention in Nicaragua and find that earnings from and involvement in non-farm businesses and employment were not affected by initial asset endowment when the programme was switched from universal cash grants to targeted subgroups. Conversely, de Mel et al (2008) found that randomised shocks to capital stock in Sri Lankan microenterprises produced significantly varying returns depending on initial entrepreneurial ability and household wealth. Likewise, a study by Prifti et al (2020) on Lesotho's Child Grant Programme found greater economic outcomes in both the crop and livestock sector for those with greater land and labour capacity. These mixed findings cast doubt on the presence and direction of heterogeneity.

A plausible alternative is that heterogeneity exists, but it is only manifest in the long run. Adopting new technology may incur adjustment costs that lead to sub-optimal production in the short run even for high ability types. Firm level studies show that such costs can be substantial and delay technological adoption by workers or management (Atkin et al., 2017; Pavlova, 2001). If a cash transfer is restrictive, recipients will be unable to delay technological adoption and are likely to produce inefficiently in the short term. Alternatively, if high-ability types have extremely low levels of initial wealth then a windfall may be insufficient to overcome fixed cost of enterprise in the short run. In this case patient types will remain in traditional labour and accumulate savings until they reach the required threshold. In both cases the 'motivated poor' should reach optimal production in the long run. However, evidence of this in the literature is scant, partly because most evaluations do not follow households long enough for productive effects to establish themselves (Handa et al., 2018). It is also worth noting that heterogeneity is of secondary importance in many studies: research on large-scale cash transfers have either focused on average treatment effects or short run outcomes (Gertler et al., 2012; Handa et al., 2018; Haushofer and Shapiro, 2016), while studies that do tackle heterogeneity have elected to explore non productivity outcomes (Djebbari and Smith, 2008; Handa et al., 2010). This paucity of evidence represents a potential gap in the literature.

Given the above, this study will attempt to test the following hypotheses:

Hypothesis 1: Prior economic and human capital endowment partly determine cash transfer outcomes.

Hypothesis 2: Heterogenous outcomes are manifest in the long term, in which case inability to detect heterogeneity in the short term is not evidence of homogeneity.

Research design

Data:

To test these hypotheses, the proposed study will postprocess existing experimental data from Blattman et al (2014). In the experiment the authors conduct a randomised evaluation of a UCT in Northern Uganda known as the Youth Opportunities Programme (YOP). The programme was designed to help poor and unemployed adults become self-employed artisans but carried restrictions: to qualify, applicants had to form groups and submit a business proposal subject to approval, though utilisation of funds was not monitored. The first endline survey was taken in 2010, two years after the start of the experiment, and the second endline was carried out in 2012. Outcomes of interest are individual consumption, earnings, and asset value at both endlines. In their heterogeneity analysis, the authors pooled samples from both endlines and found that initial wealth endowment only affected treatment outcomes for females, while human capital endowment produced no heterogenous treatment effect.

An important consideration when selecting the dataset was the study's external validity. In 2006 it was estimated that nearly two-thirds of northern Ugandans were unable to meet basic needs, just over half were literate, and most were underemployed in subsistence agriculture (Government of Uganda, 2007). At the time of the intervention in 2008, beneficiaries of the YOP had recently emerged from regional conflicts and had close to no access to formal finance and savings instruments. The setting thus closely resembles the credit-constrained environment ubiquitous to many poor communities and bolsters generalisability.

While the authors fail to detect heterogeneity in the original paper, this study will attempt to do so using newer statistical methods and richer covariates. The reasons for postprocessing this dataset are threefold. First, conventional methods used in the paper for detecting heterogeneity like subgroup analysis and interaction terms are sensitive to sample size and model selection. To mitigate this the authors pooled both endlines which raised statistical power, but their model specification precluded them from testing for heterogeneity at each individual endline. Using newer methods, this study seeks to isolate heterogenous effects at each endline and in turn test hypothesis (2). Second, because the intervention is self-selective by construction, being able to detect heterogeneity may allow us to extrapolate even stronger effects to the wider population. That is, detectable heterogeneity in treatment outcomes among the 'motivated poor' are likely to be an underestimate of the true heterogenous effect in the population. Third, the use of pre-analysis plans – designed to prevent 'P-hacking' – limits researchers to using only prespecified covariates, wasting rich covariates in the process. In the original experiment the authors limited themselves to using only 40-50 covariates out of over 300 in the final dataset. Such restrictions can make it difficult to discover strong but unexpected treatment effect heterogeneity (Wager and Athey, 2018). Variables omitted from the main heterogeneity analysis include risk-aversion, in-group dynamic, sociability, participant age – all plausible candidates for heterogeneity, even if supporting evidence is scarce. While the search for heterogeneity should be driven by theory, an ex-ante approach to covariate selection risks omitting important sources of heterogeneity, which, given the cost and difficulty in implementing randomised experiments, constitutes a substantial waste.

Empirical strategy:

This study will employ a generic machine learning (hereafter known as GML) method by Chernozhukov et al (2018) to detect treatment effect heterogeneity. In a standard OLS model, introducing all available covariates and their various permutations for interaction terms will likely lead to overfitting. By contrast the advantage with ML is its ability to handle models with large number of covariates. In effect it is possible to run all covariates through the model in our search for heterogeneity. This allows us to test a third, auxiliary hypothesis:

Hypothesis 3: Heterogeneity in cash transfer outcomes is not limited to human capital and wealth endowment.

Since ML is largely focused on prediction, a fundamental issue is that it does not produce consistent estimates and valid confidence intervals, thereby making causal inference difficult. GML sidesteps this problem by using ML solely as a proxy for predicting $S(Z)$, the Conditional Average Treatment Effect (CATE). Inference is made not on CATE but on the features of CATE itself. Multiple sample splits are utilised to mitigate estimation uncertainty conditional on the data split, while median values are taken to account for splitting uncertainty.

The GML method is split into three components: Best Linear Predictor (BLP), Sorted Group Average Treatment Effect (GATES), and Classification Analysis (CLAN).

BLP: To find the BLP, the data is split randomly into a main and auxiliary sample n times. A ML model is fitted onto the auxiliary sample and used to predict treatment and baseline outcomes for the main sample. We then estimate the following regression:

$$Y = \beta_0 + \beta_1(D - p(Z)) + \beta_2(D - p(Z))(S - E(S)) + \epsilon,$$

where Y refers to the outcome of interest, D is the treatment dummy, $p(Z)$ is the treatment propensity, S is the CATE and $E(S)$ is the expectation of S. Coefficients and P-values are obtained from the median of all splits. Rejecting the hypothesis $\beta_2 = 0$ means that there is both heterogeneity and $S(Z)$ is its relevant predictor, in which case we move on to GATES.

GATES: We split the main sample into K quantiles based on $S(Z)$ and impose the following monotonicity restriction:

$$E[S(Z)|G_1] < ... < E[S(Z)|G_K]$$

To recover the GATES parameters, we assign a dummy to G_k and run the regression:

$$Y = \beta_0 + \beta_1 B(Z) + \sum_{k=1}^{K} \gamma_k (D - p(Z))(G_k) + \epsilon$$

for each k, where $B(Z)$ is the predicted baseline outcome. If GATES is significant and there is heterogeneity, we should see a large, positively significant coefficient for γ_K and a small insignificant coefficient for γ_1.

Figure 1 shows the GATES derived from 100 splits of a random forest model with $K = 5$ at endline 2. The price of splitting uncertainty is reflected in the discounting of the confidence level from $1 - \alpha$ to $1 - 2\alpha$.

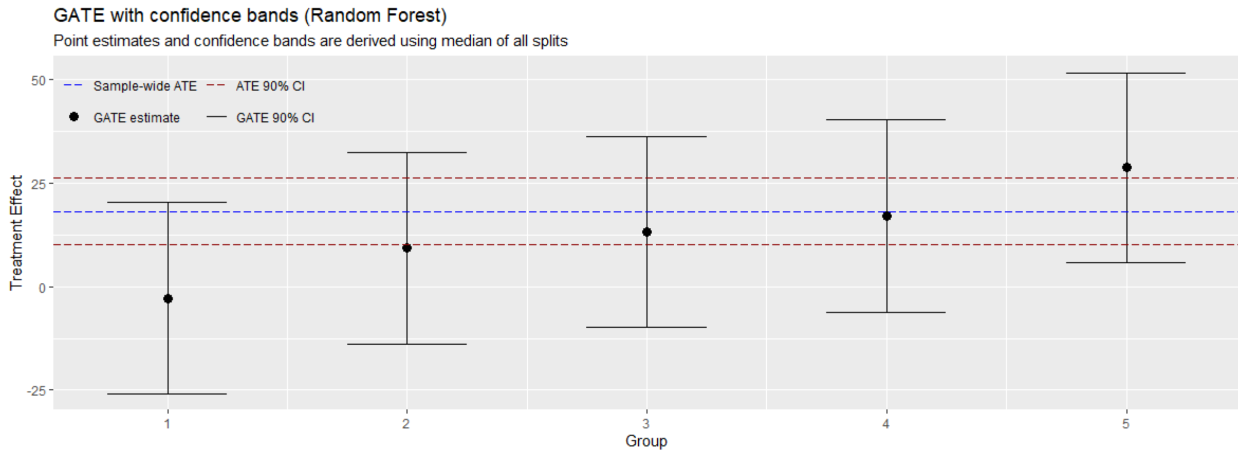

Figure 1: GATES

CLAN: Once heterogeneity is established, we report the median of the average characteristics of choice for G_1 and G_K and compare differences between the two using a two sample t test.

Analysis will be performed on both endlines, which allows us to test if heterogeneity manifests in the short or long term, if at all. To limit attrition bias, robustness checks will be performed using conservative imputations following Manski (1990) and Lee (2009).

Limitations

The study is not without limitations. First, even if heterogeneity is detected it is not possible to infer the magnitude of the effect due to ML's poor inferential properties. For instance, it is possible to say that younger individuals are more likely to benefit from UCTs but not by how much. Second, because survey responses are self-reported they may be biased upwards. If high-ability or wealthier individuals are more likely to inflate their responses then heterogeneity may be spurious. Third, at $n < 2000$ for each endline the dataset may still be too small for GML to be effective; datasets comprising hundreds of thousands of observations are not uncommon in ML applications. Fourth, the YOP requires participants to self-select into the programme and so may underrepresent the capital-rich and low-ability types. That is, the important heterogeneity may lie outside the sample. Fifth, the study says little about whether efficiency should take precedence over pro-poor targeting in the equity-efficiency debate; it assumes a priori that efficiency is a desirable policy goal and works with that assumption. Finally, computationally expensive methods like GML are meant to complement experimental findings, not replace them. It is dangerous to prescribe policy based on data that is a further level removed from the main findings. That said, what it can do is highlight new or existing sources of heterogeneity that may be ripe for future research, particularly in the context of northern Uganda. This is what the paper hopes to deliver.

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