Benchmarking a Child Nutrition Program against Cash: Experimental Evidence from Rwanda*

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Abstract

We present the results of a study designed to 'benchmark' a major USAID-funded child malnutrition program against what would have occurred if the cost of the program had simply been disbursed directly to beneficiaries to spend as they see fit. Using a three-armed trial from 248 villages in Rwanda, the study measures impacts on households containing poor or underweight children, or pregnant or lactating women, as well as the broader population of study villages. We find that the bundled health program delivers benefits in an outcome directly targeted by specific sub-components of the intervention (savings), but does not improve household dietary diversity, child anthropometrics, or anemia within the year of the study. A cost-equivalent cash transfer boosts productive asset investment and allows households to pay down debt. The bundled program is significantly better in cost-equivalent terms at generating savings and worse for debt reduction, while cost-equivalent cash drives more asset investment. A much larger cash transfer of more than $500 per household improves a wide range of consumption measures including dietary diversity, as well as savings, assets, and housing values. Only the large cash transfer shows evidence of moving child outcomes, with significant but modest improvements in child height-for-age, weight-for-age, and mid upper-arm circumference (about 0.1 SD). The results indicate that programs targeted towards driving specific outcomes can do so at lower cost than cash, but large cash transfers drive substantial benefits across a wide range of impacts, including many of those targeted by the more tailored program.

Keywords: Experimental Design, Cash Transfers, Malnutrition

JEL Codes: O12, C93, I15

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1 Introduction

This study experimentally evaluates the relative cost-effectiveness of alternative programs to improve the nutritional status of vulnerable households. We pursue a cluster-randomized trial across 248 Rwandan villages to understand how a 'standard' package of nutritional, informational, and savings interventions compares not only to an experimental control group but to an additional arm that receives household grants of equal cost to the donor—a *cash benchmark*. The study follows households with children under the age of five or women of reproductive age, with an emphasis on the 1,000 day window of opportunity from pregnancy until a child's second birthday (Currie and Almond, 2011). The core program is called Gikuriro, which means *'well-growing child'* in Kinyarwanda; it follows USAID's strategy on multi-dimensional approaches to malnutrition, and is implemented by Catholic Relief Services. The benchmarking household grant program was implemented by GiveDirectly, a US-based nonprofit that specializes in making unconditional household grants via mobile money. These two treatments are compared to a control group, namely, a set of villages that receive neither program.

This study conducts a type of cost-effectiveness benchmarking increasingly called for in recent years: the comparison of a standard and widespread development intervention with the outcome that would occur if the cost of the intervention were simply given away to the beneficiaries. Proponents of cash transfers have suggested that they should be considered the 'index funds' of international development, meaning a benchmark against which other programs are compared (Blattman and Niehaus, 2014). Just as index funds have helped to provide a reference rate of return against which fee-charging financial managers can be compared, cash transfers of equal cost to the implementer provide an important counterfactual, and establish a hurdle rate that places the burden of proof on complex, overhead-heavy development programs to show that they can justify their costs by generating benefits superior to simply disbursing the cost of the program directly to beneficiaries.[1] The appeal of cash transfer programs as a benchmark lies in their simplicity and scaleability, their low overhead costs, and the extent to which they put aid beneficiaries in control of how resources are allocated.

The momentum for benchmarking has built as numerous studies have shown meaningful impacts

[1] For a discussion of the political economy and public finance dimensions of the tradeoffs between cash and in-kind programs, see Currie and Gahvari (2008) and Jones et al. (2016).

of cash transfers on important life outcomes in the short term, such as child nutrition (Aguero et al., 2006; Seidenfeld et al., 2014), schooling (Skoufias et al., 2001), mental health (Baird et al., 2013; Samuels and Stavropoulou, 2016), teen pregnancy and HIV (Baird et al., 2011), microenterprise outcomes (De Mel et al., 2012), consumer durables (Haushofer and Shapiro, 2016), and productive assets (Gertler et al., 2012). The evidence on the long-term impacts of cash transfers is more mixed, but some studies have found substantial impacts (Aizer et al., 2016; Barham et al., 2014; Fernald et al., 2009; Hoynes et al., 2016).[2] The largest extant literature on benchmarking is based on the comparison of cash aid to food aid (Ahmed et al., 2016; Cunha et al., forthcoming; Hidrobo et al., 2014; Hoddinott et al., 2014; Leroy et al., 2010; Schwab et al., 2013), which has uncovered a fairly consistent result that food aid leads to a larger change in total calories while cash aid leads to an improvement in the diversity of foods consumed. Efforts to benchmark more complex, multi-dimensional programs against cash include BRAC's Targeting the Ultra-Poor program (Chowdhury et al., 2016), microfranchising (Brudevold-Newman et al., 2017), agricultural inputs (Brudevold-Newman et al., 2017), and sustainable livelihoods (Sedlmayr et al., 2017). These studies have typically struggled with the question of how to anticipate costs and compliance well enough to realize an exact cost-equivalent comparison after the fact. Our study provides a methodology incorporating randomization of transfer amounts and ex-post, regression-based cost adjustment that can achieve this objective in a general way.

Using a village-level randomization across 248 villages, we compare the Gikuriro program to cash transfers.[3] Gikuriro deploys the type of multi-pronged approach advocated by Ruel et al. (2013), in which the program aims to improve child nutrition through superior information, direct transfer of productive assets, and improvements in household diet and santation. A similar program in adjacent Burundi was found to decrease child and maternal anemia (Leroy et al., 2016). Gikuriro consists of four components targeted directly at beneficiary households: a Village Nutritional School, Farmer Field Learning Schools, Savings and Internal Lending Communities (SILCs), and a Water, Sanitation, and Hygiene (WASH) intervention), as well as a Behavior Change Communication

[2]For examples of studies that find dissipating long-term benefits, see Baird et al. (2016) and Araujo et al. (2017). Evidence from systematic reviews of cash transfers on schooling (Molina-Millan et al., 2016) and child health (Manley et al., 2013; Pega et al., 2014) has been similarly uneven.

[3]The village-level study design was motivated both by the clustered nature of the Gikuriro intervention, and helps to allay concerns about the potential for negative spillovers of cash transfers on adjacent controls observed in Haushofer et al. (2015).

intervention implemented at the village level. This combination has been developed by CRS over the course of many years implementing anti-malnutrition programs across the world. The cash transfer arm, implemented by GiveDirectly, provided unconditional household grants via mobile money, an intervention that has been found to improve consumption and/or dietary diversity in many contexts across Sub-Saharan Africa (Aker et al., 2016; Haushofer and Shapiro, 2016). Transfer amounts were randomized across villages, and within GiveDirectly villages we implemented a household-level experiment whereby beneficiary households were randomized to receive one-time, lump-sum transfers, a monthly flow of cash transfers over the duration of the study, or a choice between these two alternatives.

The study takes place in Kayonza and Nyabihu, two districts that span the range of economic and health outcomes observed in Rwanda.[4] The endline survey took place 13 months after baseline, and we measure impacts on five primary outcomes: (a) household consumption, (b) household dietary diversity; (c) child and maternal anemia; (d) child growth; (e) value of household non-land net wealth. These are outcomes chosen to balance the theories of change of the two implementers, as well as being well documented in the literature as core drivers of improved long-run outcomes for children (Hoddinott et al., 2013; Maluccio et al., 2009). In addition, we report impacts on a set of secondary outcomes including borrowing and savings, fertility, health knowledge and sanitation practices, diseases and mortality, household assets, and the quality of housing stock.[5] Prior to randomization the survey firm classified households as 'eligible' (identifiable using administrative data sources as containing underweight children, or households in the bottom two income categories with children 5 years old or younger or with pregnant or lactating women), or 'ineligible' (everyone else). We can therefore measure impacts both on the mutually agreed-upon intended target population as well as on the study villages as a whole, even in the presence of potential differences in actual targeting across implementers. We can use the eligible sample to estimate experimental intention-to-treat

[4]Kayonza is a relatively prosperous district in the far East of the country, with a poverty rate of 24 percent that ranks it behind only the three districts of Kigali NISR (2017). The relative prosperity of the Eastern Province is further reflected in child health measures: outside of Kigali Province, the Eastern Province had in the 2015 Demographic and Health Surveys rates of stunting and underweight, at shares of under-five children with HAZ and WAZ below -2 standard deviations of 34.8 and 9.2 percent, respectively. However, this district was hit by a severe drought around the time of the baseline. Nyabihu is in the Northwest and is a relatively poor, mountainous, and remote area, with a poverty rate of nearly 40 percent placing it 16th in the country NISR (2017). The Western Province, in which Nyabihu is situated, ranks worst in the country on rates of children stunted and underweight, at 44.9 and 10.1 percent, respectively (DHS, 2016).

[5]All primary and secondary outcomes were registered prior to receipt of endline data no the American Economic Association RCT Registry, with ID AEARCTR-0002559.

effects, and the full sample (population weighted) to estimate total causal effects on the average household in study villages.

Both implementers made contact with the study subjects and began enrollment immediately after baseline. GD began implementation shortly after the baseline meaning that at endline individuals in that arm had experienced about 12 months of the household grants treatment (running up through the month before endline). Gikuriro was slower than the cash program to begin implementation on the ground; in that arm households had typically experienced 8-9 months of household-level implementation at the time of the endline.[6] The duration of the RCT component of the study was limited by the fact that local governments wanted to hit targets for the broader, national rollout of nutritional and WASH programming, of which the eight districts covered by the Gikuriro programming were a part, and hence we were not able to maintain the control groups for more than one year.

To permit a rigorous comparison of cost-effectiveness, we costed both programs in detail prior to, and after, the intervention period, following Levin and McEwan (2001). The ex-ante costing exercise was used to identify the approximate total cost of the Gikuriro intervention, as well as the estimated overhead costs to GiveDirectly of providing household grants in this context. It arrived at an ex-ante cost of $119 per eligible household. We then randomized transfer amounts at the village level in the cash arm across four possible transfer amounts. These amounts were chosen to provide informative benefit/cost comparisons across two different margins: Gikuriro vs cash, and small versus large cash transfer amounts. Three smaller cash transfer amounts bracket the anticipated cost of Gikuriro per household (ex-ante costs of $77, $119, and $152, with beneficiaries actually receiving $41, $84, and $117, respectively); these arms are provide a straightforward window on cost-equivalent impacts. The fourth and much larger transfer arm transferred $532, the amount chosen by GiveDirectly as likely to maximize their own cost-effectiveness given the fixed costs in providing cash transfers via mobile money. The inclusion of this arm provides a statistically high-powered way of examining how benefit/cost ratios shift as the transfer amount rises. The final, ex-post costing exercise arrived a cost for Gikuriro of $141.84, and actual GD costs of $66, $111, and $145, meaning realized Gikuriro costs were within the range over which we randomized but

[6]Since both programs had six months of notice that they would be implementing in the study sample in these two districts and began national-level implementation at the same time, this differential delay likely reflects a real difference in the relative ramp-up speeds of cash versus more complex programming.

28 percent higher than the ex-ante number. We present a method to adjust for the randomized GD cost differentials using linear regression, and hence can provide comparative impacts at exactly equivalent costs to the donor, USAID.

Our results provide quite a nuanced view of the relative impact of a highly tailored child malnutrition program and the cost of the program in cash. The Gikuriro bundle of interventions, costing USAID \$142 and delivering \$73 in direct benefits per household, was successful at delivering gains in savings, a domain that was the target of the SILC intervention (the remaining costs were split between training/capacity building and overhead). It did not lead to improvements in consumption, dietary diversity, wealth, child anthropometrics, or anemia within the thirteen-month period of the study. A cash transfer of exactly the same cost to USAID could, because of lower overheads, deliver \$113 in direct benefits. Such a transfer allows households to pay down debt, and generates increased investment in productive and consumption assets. A much larger cash transfer costing \$567, and transferring \$532 per beneficiary household) led to across-the-board improvements in consumption-based welfare measures, a substantial improvement in dietary diversity, a drop in child mortality, and modest improvements of about 0.1 standard deviation in the anthropometric indicators of height-for-age, weight-for-age, and mid-upper arm circumference (all signficant at 10 percent or above). Despite 90.9 and 96.9 percent of the eligible households in treatment villages receiving Gikuriro and GiveDirectly, respectively (for the villages as a whole the treatment rates were 19 and 18.3 percent, respectively), neither treatment resulted in sufficiently widespread benefits as to be detectable in the general population, with the exception of an improvement in health knowledge and vaccination rates in Gikuriro villages and vaccination rates in GD large villages.

These results are intuitive in many ways. When a program uses targeted interventions, it can at relatively low cost shift a specific set of welfare indicators tied to these behaviors (for example, the strong impacts of Gikuriro's savings groups). If such a program is built on a solid theory of change connecting outcomes such as savings stocks to long-term outcomes, this can be a well-justified use of development assistance. Unconditional cash is spent on many different things in terms of sample averages, and hence is hard to detect when the tranfer amount is small but improves outcomes almost across the board as the transfer amount increases. The large cash transfer delivers benefits even on outcomes specifically targeted by the other program. While it is unsurprising that very large amounts of money show up in consumption and productive assets, the improvements in diet and particularly child anthropometrics over such a short period of time are impressive. Further, while it may be unsurprising that the impact of cash transfers scales with amount spent in the way found here, the same may not be true of other types of development intervention that would quickly hit diminishing marginal returns once certain core objectives were achieved.

This points to an inherently different way of thinking about cash-transfer programs as a 'benchmark'. While transfer programs maximize scope for choice and therefore provide an important window on beneficiary priorities, a comparison to other more targeted programs will inevitably require policymakers to explicitly make tradeoffs across outcome dimensions, across beneficiary populations, and between large benefits for concentrated subgroups or small benefits that are diffuse over a broader target population. By contrast with the index fund analogy, part of the value of cash transfer programs as a benchmark is that they may require donors to be explicit about their preferences, and to justify interventions that constrain beneficiary choices.

The rest of the paper is organized as follows. The next section of the paper lays out the study design, including a detailed decription of the interventions, sampling routine, costing principles, the experimental structure, as well as primary and secondary outcomes. Section 3 presents the core empirical results of the benchmarking exercise, as well as the results of sub-experiments on cash transfer modalities. Section 4 presents the pre-specified analysis of heterogeneity, including by child age and by baseline malnutrition The final section concludes, and provides specific examples of how the results of the study can be used to bound the preferences over benefit/cost ratios required to justify each program.

2 Study Design

2.1 Description of Interventions

The Gikuriro program was developed by USAID, Catholic Relief Services (CRS), and the Netherlands Development Organization (SNV) to combat food insecurity among pregnant women and children, particularly during the critical first 1,000 days of life that play such a dominant role in later-life outcomes and cognition (Figlio et al., 2014). The resulting multi-faceted program brings together several components in order to attack this problem from multiple directions at once, and is a central pillar of the Government of Rwanda's approach to combatting malnutrition in rural

Rwanda.[7] Gikuriro combines an integrated nutrition program with a standard WASH curriculum (water, sanitation, and hygiene), and seeks to build the capacity of the health infrastructure providing services to mothers and newborns, particularly Community Health Workers (CHWs). The program also seeks to build livelihoods by providing additional assistance to eligible households, including (a) Village Nutrition Schools (VNS); (b) Farmer field learning schools (FFLS), which potentially includes distribution of small livestock, fortified seed, etc.; (c) Savings and Internal Lending Communities (SILCs); and (d) the Government of Rwanda's Community-Based Environmental Health Promotion Program (CBEHPP). In addition, Gikuriro provided a program of Behavioral Change Communication (BCC), supporting participation in all components of the program including savings, agriculture, and nutrition, as well as hygiene. This comprehensive approach seeks to build supply and demand for child health services simultaneously, and is fairly typical of the kinds of multi-sectoral child health programs implemented by USAID in many parts of the developing world.[8]

To benchmark the impact of this program against cash we worked with GiveDirectly, a US-based 501(c)3 Non-Profit organization. GiveDirectly specializes in sending mobile money transfers directly to the mobile phones of beneficiary households to provide large-scale household grants in developing countries including Kenya, Uganda, and Rwanda. GiveDirectly's typical model has involved targeting households using mass-scale proxy targeting criteria such as roof quality. GiveDirectly builds an in-country infrastructure that allows them to enroll and make transfers to households while simultaneously validating via calls from a phone bank that transfers have been received by the correct people and in a timely manner. Their typical transfers are large and lump-sum, on the order of $1,000, and the organization provides a programatically relevant counterfactual to standard development aid programs because it has a scalable business model that would in fact be capable of providing transfers to the tens of thousands of households that are served by the Gikuriro program. Because of the nutritional focus of the Gikuriro intervention, GiveDirectly incorporated a 'nudge' into the way the program was introduced (Benhassine et al., 2015), utilizing a low-cost

[7] USAID's Global Health and Nutrition Strategy explicitly calls for multi-sectoral interventions that incorporate agriculture, WASH, education, and outreach to mothers in the first 1,000 days through the public health system. The agency reports reaching 27 million children worldwide under the age of 5 in 2016 alone through such programs, which represent the prescribed USAID modality for Scaling up Nutrition (SUN) countries.

[8] Examples of similar integrated WASH/agriculture/child nutrition programs funded by USAID include SPRING in Bangladesh, RING in Ghana, Yaajende in Senegal, and ENGINE in Ethiopia.

flyer emphasizing the importance of child nutrition that was given to households at the time of the intervention. An English translation of this flyer is included in Appendix A. Given observed impacts of cash transfers on other goods, e.g., productive assets and housing value, it is evident that households felt at liberty to spend the grants on items not directly related to child nutrition.

Rwanda may be a particularly interesting environment in which to pose the benchmarking question for several reasons. First, child malnutrition rates overall are high—the prevalence of stunting among children under age five in the 2014-15 Demographic and Health Survey was 37.9 percent, underweight 9.3 percent and wasted 2.2 percent—though this represents an improvement in recent years (DHS, 2016) Second, Rwanda is a country notable in Africa for its bureaucratic competence and the public health infrastructure has been successful in delivering substantial improvements in child and maternal health outcomes (NISR, 2015) through schemes such as Pay-for-Performance (Basinga et al., 2011). Hence, it may provide a relatively strong case in terms of interventions such as Gikuriro that are led through the public health system and lean heavily on Community Health Workers (CHWs). Third, the Government of Rwanda has been experimenting extensively with cash transfer programs over the past few years, such as the inclusion of cash in the flagship *Umurenge* poverty reduction program (Gahamanyi and Kettlewell, 2015), the $50 million 'Cash-to-poor' program supported by the World Bank, as well as a number of efforts to transition the support systems for the country's large population of refugees to cash transfers (such as a World Food Programme (WFP) program that is now supporting 15,000 refugees in Gihembe Camp using cash rather than traditional in-kind aid mechanisms (Taylor et al., 2016)). Hence there should be the bureaucratic capacity to implement Gikuriro well, and there is both experience with and interest in cash transfers as a safety net modality in the country.[9]

Gikuriro is in the midst of a full-scale rollout in 8 districts, and the randomized study design was based on delaying implementation of the program in a number of eligible villages for one year. For this reason the study is only able to measure impacts over the course of the 13 months from baseline to endline, which capture 12 months of on-the-ground implementation for GD and 8-9 months for Gikuriro. We cannot therefore speak to the long-term impacts of the interventions. Anticipating this issue, we took two approaches to measurement. One of them was to try capture the stocks

[9]Given the framing provided by GiveDirectly and the unusually strong degree of social control exerted by local officials in the Rwandan context, it is certainly possible that our 'unconditional' transfers have been more forcibly devoted to child consumption than they would have been in a different context.

of intertemporal assets that would be the obvious conduits to future consumption benefits for the households. The second was to emphasize outcomes such as dietary diversity and anemia that have the potential to respond quickly to changes in consumption patterns, while also retaining the more standard metrics of child malnutrition such as height for age (HAZ), weight for age (WAZ), and mid-upper arm circumference (MUAC).[10] Further, a number of recent RCTs have shown that programs can have meaningful impacts on biometric outcomes over timeframes similar to that analyzed in this study, such as Desai et al. (2015), Leroy et al. (2016), Fink et al. (2017), and Null et al. (2018).

2.2 Eligibility for the Study

The study aims to compare nutrition and health gains among poor households with young children across the two programs and a control. We therefore used a definition of eligibility tailored to Gikuriro's stated target population: namely, households that contained malnourished children, or pregnant and lactating mothers. A core challenge of the benchmarking endeavor is the need to use a measure of eligibility in a manner that can be defined identically across arms.[11] As a result, we established a set of 'hard' eligibility criteria on the basis of which beneficiaries would be selected and the survey would be stratified. Households meeting these criteria would be identified by the survey firm, Innovations for Poverty Action (IPA), prior to sampling for the baseline study, to establish a comparable population of eligible households in all arms—including control—of the study.

CRS and USAID agreed that the following criteria represent the target population for Gikuriro:

- Criteria 1. All households in a village with a malnourished child (defined by a threshold value of weight/age) were enrolled.
  - Weight/age is used because it is believed that this data is more consistently available than data on middle-upper arm circumference (MUAC) and height/age, and because it is used by CHWs as a basis for referring children to their local Health Centers.
  - The threshold weight/age value for inclusion was determined using the Rwandan Ministry of Health standards for malnutrition. The data used to identify eligibles was based on

[10]Dietary diversity is an immediate indicator of improvements in consumption, and the clinical literature has shown that anemia tests respond within 3 months of improvements in diet (Habicht and Pelletier, 1990).

[11]We did not intend the scope of the benchmarking exercise to include the implementers' (potentially different) ability to cost-effectively identify this target population, so as to maintain the interpretation of impacts as being differential impacts on a consistently defined beneficiary group.

the Community Health Worker data from Growth Monitoring and Promotion visits.

- Criteria 2. All households in Ubudehe 1 or 2 with children under the age of 5 (Ubudehe is the Rwandan government household-level poverty classification, with 1 being the poorest, 3 being non-poor, and rural areas containing very few of the wealthiest Ubudehe 4 households).

- Criteria 3. All households in Ubudehe 1 or 2 with a pregnant or lactating mother.

Both implementers agreed to attempt to treat all eligible households that were identified as meeting any of these criteria. CRS anticipated an average of 30 eligible households per village, and in principle had established a rationing rule in case that number was exceeded. As will be described below, the number of households per village that could be identified by the survey firm as meeting these targets turned out to be substantially lower. We did not try to impose restrictions on how Gikuriro would target outside of the households identified by the survey firm to be eligible.

We asked IPA to identify the universe of households that they could locate who met these criteria, using three sources. First, CHW records from the national 'Growth Monitoring and Promotion' exercise, which is intended to provide monthly height and weight measurements for all children under two and annual measurements for all children under five; second, government (census) records of household *Ubudehe* classifications; and finally local health facility information, which provides an alternative data point on children's nutritional status.[12] Children were defined as malnourished if they had at least one measurement that met government thresholds for malnourishment definitions in the past year, and households were defined as eligible if they had any individual meeting the criteria above. In each village we recorded the number of households in each stratum and sampled up to eight eligibles and four ineligibles for inclusion in the study. Throughout this document we use the words 'eligible' and 'ingeligible' to refer to the classification made by the survey firm at baseline.

While the primary analysis focuses on outcomes in the eligible group, we randomly sampled ineligibles into the survey so as to be able to consider broader Total Causal Effects (TCEs). Impacts among ineligibles may arise either because the implementers treated some households outside of the IPA-defined eligible group, or because of spillovers from beneficiary to non-beneficiary households.

[12] In practice, most children attending local clinics are referred by a CHW and so are also recorded as malnourished in the Growth Monitoring process.

The primary analysis is weighted to be representative of all eligible households in study villages, and the analysis including ineligibles is weighted to be representative of all households in study villages. Eligibility lists were shared with both implementers at the same time in the same way, but the sampling of eligible and eligible households for the survey was not revealed to implementers, so as to avoid the possibility that the implementers would specifically target the research sample. Our sample of ineligible households lets us understand both how treatment across the implementers may have varied in this ineligible sample, as well as the nature of the impacts observed in this group.Both implementers concurred closely with our definition of treatment on the ground, and compliance was high: we have 90.9 percent of the survey-defined eligibles treated by Gikuriro, and 96.9 percent of the survey-defined eligibles treated by GiveDirectly.[13] This means that the Intention-to-Treat effects estimated on the eligibles should be well powered and are close to providing the Average Treatment Effect within this group.

We did not however encounter the number of eligible households anticipated; despite having expanded the eligibility criteria beyond what was originally envisioned, we nonetheless found an average of only 13.9 eligible households per village using the hard targeting information. Some villages did not even contain the 8 eligible households we intended to sample and hence we end up with fewer eligibles than 8 in smaller or wealthier villages. On average we have 7.23 eligible households and 4.01 ineligible households sampled per village.

When Gikuriro began their actual program implementation, they continued their standard consultative process for beneficiary identification, which included the use of soft targeting information not available to IPA. Using this additional, richer information set to target, they identified and treated an average of 25.8 households in study villages in Kayonza district, and 26.97 in Nyabihu. Since our first tranche of GiveDirectly treatments were only among IPA-defined eligible households, we found ourselves with a substantial discrepency in the intensity of treatment across implementers. We responded to this asymmetry by drawing in an additional sample of the poorest ineligible households in GiveDirectly villages to receive household grants so as to maintain parity in village-level

[13]Because eligible was determined from records rather than from face-to-face visits, it was possible that some identified households were not in fact resident in the village, or that the individuals whose presence made a household eligible had moved out. These were the only reasons that GiveDirectly did not treat a household, and it should be noted that households rejected for the former reason would also not appear in the study sample. In addition, CRS implemented a 'consultative' process with community members and determined that official Ubudehe status was incorrect or outdated for some IPA-determined eligible households.

treatment intensity. One month after baseline we sampled from within the broader set of ineligible Ubudehe 1 and 2 households (e.g. without young children) and passed this additional list to GiveDirectly to be treated with household grants. We already knew the realized treatment intensity from CRS at the time we drew in this additional 'top-up' sample, and so we selected the fraction of ineligibles to be treated by GD such that the realized fraction of households treated per village was identical for Gikuriro and CRS within each district. Given the treatment of the entire eligible stratum by GD, we gave them top-up lists that brought in an additional 11.26 households in Kayonza and 12.56 households in Nyabihu during tranche 2. The top-up lists were presented in a randomized order with instructions that GD should replace any non-complying households with the next one on the list to get as close as possible to the assigned number of treated households in Tranche 2. In the end, although the targeting of ineligibles will differ across implementers, the treatment intensities across the two arms are therefore identical by construction.

2.3 Design of the Experiment

Randomization occurred at the village level across 248 villages, using a blocked randomization where the blocks were formed by the combination of districts and village-level poverty scores within district, creating a total of 22 blocks with between 10 and 13 villages per block. Fixed effects for these blocks are included in all analysis. A computer was used by the researchers to conduct the randomization based on a frame of villages agreed to by CRS and government officials.

Table 1 presents a schematic of the design of the study. 74 villages were assigned to the Gikuriro intervention, 74 were assigned to the control group (no intervention), and 100 were assigned to GiveDirectly household grants. The GiveDirectly villages were further split into four transfer amounts, randomized at the village level. Three treatment amount arms, with 22 villages in each, received transfer amounts in a range around the anticipated cost of Gikuriro. A final 34 villages were assigned to the 'large' GiveDirectly transfer amount which was selected by GiveDirectly as the amount anticipated to maximize the cost effectiveness of cash. The transfers actually received by households in the GD 'small' arms (not inclusive of overhead) were \$41.32, \$83.63, and \$116.91. Then, the large transfer amount selected to optimize GiveDirectly's benefit/cost ratio was \$532 actually transferred to households. All transfer amounts were translated into Rwandan Francs at an exchange rate of 790 RwF per US dollar, and were rounded to the nearest hundred.

Subject to the constraint of maintaining the assigned average household transfer value at the village level, GiveDirectly believed that most cost-effective use of these funds would be to attempt to equalize the amount transferred per household member, rather than to have households of very different sizes receiving the same transfer amount. To accomplish this, we scaled the transfer amounts within a village by household size, such that larger households received larger transfers, but leaving the mean transfer amount at the village level unaffected. This formula first calculated the per-capita transfer for a village using household sizes and the desired average household transfer value. Second, it scaled household-level transfer amounts with household size, applying a minimum of 3 members and a maximum of 8 members, so as to achieve the intended mean transfer amount per household per village. Household sizes for scaling transfer amounts were derived from administrative data (Community Health Worker reports), and not from baseline surveys. Figure 1 provides a box and whisker plot of the randomly assigned mean transfer amount per village relative to the actual amount received per household observed in the GD institutional data, and shows that the two correspond closely.

Within the GD arm we conducted a number of additional, individual-level experiments.

1. Transfer Timing. Evidence from other contexts suggests that a regular, monthly flow of transfers is likely to be a more effective way of delivering the kinds of nutrition and health outcomes that are the target of Gikuriro, rather than large lump-sum transfers (Haushofer and Shapiro, 2016). We randomized eligible beneficiaries in the household grants arm of the study to three groups designed to measure the effect of frequency: flow transfers divided into a sequence of monthly transfers; lump-sum transfers given all up front; and a choice arm that could decide which of these two modalities they wanted.

2. Choice experiment. The modality for the choice experiment, conducted only in the GD arm, is as follows:

   (a) First, all respondents were given a menu illustrating the choice between a single lump-sum transfer delivered in any of the 12 months from August 2016 to July 2017 and a flow of monthly payments totaling the same amount. The choice was recorded for each month for each household.

   (b) Then, the household was randomized to one of three conditions:

i. with 5/8 probability, they were assigned to the monthly flow treatment.

ii. with 1/4th probability, they were assigned to the lump sum treatment.

iii. with 1/8th probability, they were assigned to the choice arm.

(c) The large majority of the choice arm were given whichever they chose of the Lump Sum versus Flow treatment in the first month. 1 out of 20 individuals in the choice arm were given their choice in a randomly selected month so as to preserve the incentivization of the monthly choice questions.

2.4 Study Outcomes

Primary Outcomes. The study focuses on five dimensions. Here we briefly summarize each; details of the construction of these outcomes are included in Appendix A.

1. Household monthly consumption per capita (inverse hyperbolic sine—henceforth IHS—to deal with skewness).

2. Household Dietary Diversity, measured using hte WHO standard Household Dietary Diversity Score.

3. Anemia: measured with a biomarker test following DHS protocols at endline only.

4. Child growth and development: measured using in height-for-age, weight-for-age and Mid Upper Arm Circumference at baseline and endline for children under the age of 6 in eligible households.

5. Value of household non-land net wealth. This outcome is the sum of productive and consumption assets; the value of the household's dwelling, if owned; and the value of the stock of net savings, less the stock of debt (IHS).

Secondary Outcomes. Three types of outcomes are selected to be secondary: proximate outcomes of one or both interventions that do not have an intrinsic welfare interpretation (such as borrowing and saving stocks); outcomes that have welfare weight but are not within the causal chain of both programs (such as investments in health-seeking behavior, which Gikuriro seeks to impact, or housing quality, which has been identified as a dimension of benefit in prior evaluations of GiveDirectly (Haushofer and Shapiro, 2016)); or outcomes of common interest on which power is limited (such as disease burden and mortality).

1. Stock of borrowing and stock of savings (IHS).
2. Birth outcomes: the likelihood of pregnancy and likelihood of live birth within 12 months prior to endline.
3. Health knowledge and sanitation practices.
4. Disease burden and mortality. Mortality is measured as the likelihood that an individual member of the household from baseline has died prior to endline. Disease burden is measured as the prevalence of fever, fever with diarrhea or vomiting, or coughing with blood at endline,
5. Health-seeking behavior/preventative care. We focus on the share of pregnancies resulting in births in medical facilities, the share of children under two years of age with at least one vaccination in the prior year, and the share of children under two years of age with a complete dose of vaccines.
6. Household productive assets (IHS).
7. Housing quality. Two measures are used: the self-reported replacement cost of the current dwelling (irrespective of ownership status, IHS), and an index of housing construction quality, constructed from measures of wall and roof materials and from the number of rooms in the dwelling.

The inverse hyperbolic sine is commonly used in analysis of outcomes such as consumption, savings, and asset values that tend to be highly right-skewed and also to contain zeros. The IHS transformation preserves the interpretation of a log (meaning that impacts can be interpreted as percent changes) but does not drop zeros. Only outcomes that we expected to be skewed were pre-registered to be analyzed using IHS. All non-binary outcomes are also Winsorized at the 1 percent and 99 percent level (values above the 99th percentile are overwritten with the value at the 99th percentile to reduce skewness and increase statistical power). Because we restrict the analysis in this paper to the pre-specified primary and secondary outcomes only, we do not correct the results for multiple inference (Anderson, 2008).

2.5 Regression Specifications

The data from the study are analyzed following our pre-analysis plan, consistent with the design being a three-armed, cluster randomized trial. Let the subscript i indicate the individual, c the cluster (village), and b the randomization block. E_{icb} is an indicator for eligibility status, defined at the household level. For outcomes observed both at baseline (Y_{icb1}) and at endline (Y_{icb2}), we conduct ANCOVA analysis including the baseline outcome; otherwise we omit the baseline outcome and run a simple post-treatment cross-sectional regression. Fixed effects for the village-level assignment blocks within which the randomization was conducted α_b are included, as well as a set of baseline control variables selected from the baseline data on the basis of their ability to predict the primary outcomes, denoted by X_{icb1}. In our simple experimental analysis we include two distinct dummies for GD treatment; one for the three smaller amounts T_c^{GDS} chosen to be close to the cost of Gikuriro, and one for the 'large' transfer amount T_c^{GDL}, whose impact is not cost comparable to any of the other treatments. Thus the regressions to estimate the Intention to Treat Effect among eligibles are:

$$Y_{icb2} = \alpha_b + \delta^{GK} T_c^{GK} + \delta^{GDS} T_c^{GDS} + \delta^{GDL} T_c^{GDL} + \beta X_{icb1} + \rho Y_{icb1} + \epsilon_{icb2} \quad \forall \quad i : E_{icb} = 1 \tag{1}$$

Standard errors are clustered at the village level to reflect the design effect in the study (Athey and Imbens, 2017). The block-level fixed effects are be included to account for the block-randomization of the study (blocks are defined by district/sector and village-level poverty rankings, there are 22 blocks in the study). Following the 'post-double-LASSO' procedure of Belloni et al. (2014b), a set of covariates were selected using a LASSO algorithm on the control data as described in our pre-analysis plan; this model selection procedure is detailed, together with the resulting set of baseline covariates for each primary and secondary outcome, in Appendix Section B. This regression includes sample weights equal to the number of eligible households in the village divided by the number of eligible households in the study in that village, so as to make the results representative of all eligible households in study villages. For outcomes such as anemia that are collected at endline only, we cannot include the lagged outcome to run the ANCOVA regression, and so use the simple cross-sectional analog to Equation (1).

The Total Causal Effect of the program on the average household in study villages can be estimated by running Equation (1) on the entire sample, ineligible and eligible alike. For this regression, the weights on the ineligible households equal the number of ineligible households in the village divided by the number of ineligible households in the sample in that village, so as to make the results representative of all households in study villages.

Following our pre-analysis plan, we have a regression adjustment strategy for analyzing the programs at an exactly cost-equivalent level using the ex-post costing data from both programs. First, begin with the total GD donor cost per eligible within each transfer amount arm, denoted by t_c. Subtract from this number the benchmarked Gikuriro cost per eligible household C described above, and denote the difference $t_c - C = \tau_c$; this is the deviation (positive or negative) of each GD arm from the benchmarked Gikuriro cost. Set τ_c to zero in the control and Gikuriro arms. We can then re-run regression (1) above controlling for a linear term in τ_c, a dummy for either treatment, and a dummy for receiving Gikuriro. Because τ absorbs the deviation of the GD arm from the benchmarked Gikuriro cost, the dummy coefficient on Gikuriro treatment will serve as an intercept measuring the impact of Gikuriro benchmarked an exactly donor cost-equivalent cash transfer. So, we have:

$$Y_{icb2} = \alpha_b + \delta^T T_c + \delta^{GK} T_c^{GK} + \beta X_{icb1} + \rho Y_{icb1} + \gamma_1 \tau_c + \epsilon_{icb2} \quad \forall \quad I : E_{icb} = 1 \tag{2}$$

In this specification T_c is a dummy variable indicating any treatment (Gikuriro or GD). Subject to the assumption of linear transfer amount effects, the slope coefficient τ_c captures impacts arising from deviations in GD cost from Gikuriro cost, the coefficient δ^T effectively gives the impact of GD at the cost of Gikuriro), and the dummy variable δ^{GK} provides a direct benchmarking test: the differential impact of Gikuriro over GD at the same cost per eligible. Per the pre-analysis plan, we impose the simple linear functional form to preserve as much statistical power as possible for the core cost-equivalent benchmarking comparison, although it is straightforward to make this more flexible.[14]

The Total Causal Effect can also be benchmarked at a cost-equivalent level. The methodology is very similar; we redefine τ_c as the deviation of GD spending per household in the overall village,

[14]Because in this study we have three very similar small transfer amounts and one much larger amount we have little ability to measure non-linear impacts of the transfer amount.

relative to the Gikuriro cost defined in the same way. By then cost-adjusting an estimate of the TCE weighted to be representative of all households in the village, we can measure how the overall village-level average impact of each program differs when spending per household is the same.

2.6 Cost Equivalence, Before and After the Fact

The costing exercise in the study utilized the 'ingredients method' which specifies all the ingredients (resources and inputs) used in performing the activities that produce the key outcomes of interest. In this costing, cost is defined as opportunity cost: the value of a good or service in its best alternative use. When a good or service is used for a specific purpose, the user "gives up" the possibility of employing it in another application (see Dhaliwal and Tulloch, 2012; Levin and McEwan, 2001, for more discussion).

The policy question is asked from the perspective of the donor (in this case, USAID): the policy objective is to achieve the highest benefit-cost ratio per intended beneficiary for each dollar that is spent on a program. Overhead expenditures in the implementation chain are an inherent part of these costs, and so the lower transactions costs in getting mobile money to the beneficiary play an important role in their potential attractiveness. We conducted two different costing exercises at two moments in time. The ex-ante exercise, which was based on projected budgets and staffing costs, was used to predict the cost at the time of the study design, and to choose the ranges over which the lower GiveDirectly transfer amounts would be randomized. Then, a rigorous ex-post costing exercise was conducted for both programs after the fact using actual expenditures.

Since the Gikuriro program covers eight districts (e.g. much larger than the study population only) we attempt to cost the full national program (not just the study sample), inclusive of all direct costs, all indirect in-country management costs including transport, real estate, utilities, and the staffing required to manage the program, and all international overhead costs entailed in managing the Gikuriro program. Beneficiary identification costs, incurred by the survey firm and identical across all arms of the study, are excluded from the cost-benefit calculation. Monitoring and Evaluation costs, similarly, were excluded so as to be costing only the implementation component. All administrative costs, including the appropriate share of the costs of maintaining international headquarters infrastructure, were included in the costing. Because we do not want differences in scale to drive differential costs per beneficiary, we asked GiveDirectly to artificially scale up their operations and provide us with numbers reflecting the costs per beneficiary if they were running a national-scale program across eight districts, including 56,127 beneficiary households like Gikuriro. This is the relevant question for a USAID program officer contemplating commissioning a program to move the outcomes studied at comparable scale.

We costed each GD arm separately, asking what the overhead rate would have been if GD had run a national program at the scale of Gikuriro giving only transfers of that amount. Overhead costs as a percentage of the amount transferred decline sharply with transfer amount for GD because fixed costs represent a large share of their total overhead. This allows us to conduct the benefit/cost comparisons 'at scale', rather than having the artificial, multi-amount environment of the study contaminate the costing exercise across arms.

Another important issue in costing is compliance. Our study impacts focus on the ITT, and the costing number that matches this estimand is the amount spent per *eligible* household, rather than per *beneficiary* household. We can incorporate non-compliance into the effective amount spent by each implementer per study household by differentiating costs of two types: first there are 'averted' costs, which are not spent on a household if they do not comply with treatment; second there are 'non-averted' costs which will be expended whether or not the household complies. For GiveDirectly all variable costs are averted, for Gikuriro all variable costs except for the village-level behavior change component are averted. Using this approach we can recover a cost-equivalent comparison even if the compliance rates are different across arms. To do this, we match institutional data from Gikuriro and GiveDirectly to the study village and study sample, and calculate compliance rates in both the eligible sample and the overall village population.[15]

Gikuriro can be broken into two kinds of costs which have different numbers of beneficiaries. First is the village-level treatments (WASH and BCC) which are applied very broadly to the village population. These costs drive 40 percent of the total cost of the program, and are applied to households whether or not they comply with Gikuriro treatment directly (non-averted). The second are costs that pertain only to households that directly participate in the program; these costs are

[15]Several details require further description here. First, our pre-analysis plan indicated that we would cost each sub-ingredient of Gikuriro and use survey data to calculate compliance with each ingredient. Costing every ingredient of that program proved infeasible, and in the event households that benefit from any one of the direct interventions in Gikuriro are likely to receive them all. So, with the exception of the Behavior Change component (which was provided to the entire village and is costed as such) we cost the two implementers in the same way: the product of cost per beneficiary and the compliance rate calculated from institutional data in the relevant population.

incurred only if the household complies with treatment and averted otherwise, hence we hit these costs with the compliance rates among eligibles and overall to calculate the cost per eligible and cost per household overall. As described in the Introduction, we designed the study around an anticipated Gikuriro cost of $119 per beneficiary household, which given the ex-ante costing of GD led us to transfer $41, $84, and $117 dollars to households in those arms.

Table 2 provides the exact costing numbers arrived at by the ex-post exercise. Gikuriro treatment rates are 90.9 percent among eligibles and 19 percent in the population as a whole. Given an actual cost to USAID of $141.84 per beneficiary, this gives a cost of $134.13 per eligible household and $72.94 per household in the village. GD faced few refusals for eligible individuals they attempted to treat (0.3 percent), but due to a remit from the government only to treat households in Ubudehe 1 and 2, they declined to treat specific households that IPA defined as eligible if they found the Community Health Worker-provided listing information to be incorrect when they approached them for enrollment (1.6 percent). They were also unable to locate 3.0 percent of surveyed eligible households. Total treatment rates in the GiveDireclty arm among eligibles are therefore 96.9 percent, and within the whole population 18.3 percent. Given costs to USAID of $66, $111, $145, and $567 across the GD arms, this implies costs per eligible of $64, $111, $136, and $555 (compliance rates are calculated separately for each GD arm), and costs per household in the village of $12, $21, $27, and $100.

3 Analysis

3.1 Attrition and Balance of the Experiment

Endline outcome measurement is subject to a number of distinct forms of attrition; we start our empirical analysis by considering each of these in turn. The most straightforward of these is standard household-level attrition, meaning that a household sampled into the baseline survey attrited from the endline survey. In Table 3, we see that overall rates of attrition at the household level were low, around 3.3 percent in the control. We see the pattern typical in RCT studies where attrition is somewhat lower in the treatment groups (where both ongoing contact and a sense of reciprocity may keep individuals in the endline), but these differentials are small, from 0.89 percentage point in the GD ‘small’ arm to 1.7 percentage point in the GD ‘large’ arm; only the latter is significant, and only at the 10 percent level. Looking at the other covariates of attrition in column 2 we see that attriters and non-attriting households are similar. Hence we conclude that household-level attrition is unlikely to be a source of bias in the study.

When we turn to the analysis of individual-level outcomes in Panel B the picture is more complex, because many of the primary and secondary outcomes are only measured for certain types of individuals (anthropometrics for children, birth outcomes only for those pregnant). Each of these forms of missingness may be driven by the treatments, so attrition for each type of outcome must be taken in turn. We present a basic analysis of attrition here, and return to a discussion of this issue when we show treatment effects on fertility and mortality.

We analyze in Panel B four types of differential missingness that may occur. First, we compare the attrition of all household members from the roster in the household survey; both the rates and the differentials here are very similar to the household attrition problem suggesting that there has been little additional differential attrition of individuals. Next we examine the anthropometric panel, whereby all children under 6 at baseline who were given anthropometrics at the baseline should have been followed up with at endline. Here the absolute rates of attrition are a little more than double what they are for individuals overall, presumably because of the greater difficulty of finding and measuring children for this exercise. More concerningly, the decline in attrition in the treatment groups now becomes strongly significant, particularly for Gikuriro villages (perhaps evidence of the superior monitoring of malnourished children taking place in those villages). Given this significance, we follow our Pre-Analysis Plan in also presenting results for the anthropometric impacts that are corrected by inverse propensity weights to correct for the observable determinants of selection. Third, we examine whether individuals who should have been anemia tested in the followup were; here we see no evidence of differential attrition across arms. Finally, we examine the likehood that a new household member appears (typically due to births subsequent to baseline), and find no significant differences. Overall, then, differential selection across treatment arms is not a major problem for study outcomes other than anthropometrics. We return to the issue of unequal attrition in anthropometrics in the following section.

In Table 4 we present the comparison of baseline outcomes and control variables for eligibles at both the individual and household level, using the unattrited panel sample that will be the basis of the evaluation. The regressions used here mimic as closely as possible the impact regressions, using fixed effects for randomization blocks, including a battery of baseline control variables, weighting to make the sample representative of all eligibles, and clustering standard errors at the village level to account for the design effect. At the household level the experiment is generally well balanced; we present 33 comparisons in this table and find four of them to be significant at the 10 percent level, much as we would expect. In Panel B we present all of the individual-level primary and secondary outcomes that are observed at baseline, and again find the experiment to be very well-balanced with only two outcomes of 27 significant at the 10 percent level. Unfortunately the only individual imbalance significant at the 5 percent level is in one of the core study outcomes; weight for age. The anthropometric indicators generally appear superior at baseline in the GD Large arm, an issue to which we return in the discussion of our results where we focus on the ANCOVA analysis to deal with this issue. Overall, the experiment is well-balanced at baseline.

3.2 Basic Experimental Results on Eligibles

Table 5 presents the core results of the study on the eligible population. Panel A shows household-level impacts on the panel sample, and Panel B presents impacts on the individual-level primary and secondary outcomes, where the relevant sample is outcome-specific and follows the Pre-Analysis Plan.

Taking the Gikuriro treatment first, we see impacts on a set of proximate and directly targeted outcomes of the program components. Household savings increases by a massive 109 percent (consistent with the creation of SILCs). Dietary diversity, anthropometrics, and maternal anemia all move in the right direction but none of these changes is significant. No consistent impacts appear for consumption and wealth outcomes, or for health knowledge and sanitation practices. Hence the program has been successful in moving an indicator closely related to one of its sub-components, but at least within the timeframe of the study these changes in savings have not yet translated into signficant improvements in the anthropometric child outcomes.

We turn next to to the impact of the three smaller GiveDirectly arms whose average cost is $111, 78 percent of the cost of Gikuriro. Here, we see quite a different set of outcomes move. Instead of increasing savings, small GD transfers lead to a 76 percent paydown of debt, and an increase in the value of productive and consumption assets, by 26 percent and 37 percent respectively. A number of surpring negative results also emerge; we find a small deterioration in household wealth and home values, and some of the child biometric measures move in the wrong direction.[16] Thus far, then, the comparison of Gikuriro to cash breaks down into two distinct dimensions of improvement, each of which has a different and entirely plausible pathway to long-term improvements: savings (Gikuriro), or debt reduction and asset investment (GiveDirectly).

When we examine the third row, however, a more transformative impact arising from of the 'large' cash transfer is clearly apparent. Not only do omnibus measures of consumption and wealth go up across the board, but metrics of consumption closely linked to child health improve. The dietary diversity score increases by 15 percent off a base of 4.16. Productive assets increase by 76 percent, consumer assets almost double in value, and home value increases substantially. In the individual outcomes the benefits of this surge in consumption are evident as well; within the course of one year we see a 0.091 SD improvement in HAZ, a 0.067 SD improvement in WAZ, and a 0.13 SD improvement in MUAC, all significant at least at the 10 percent level.[17]. The ANCOVA specification should be particularly important in the analysis of the anthropometric indicators that showed signs of imbalance at baseline; indeed if we examine these outcomes in post-treatment levels we see substantially stronger apparent treatment effects. Anemia falls slightly (not significant), and there is a substantial decrease in child mortality of almost 1 percentage point(or 70 percent off of the baseline value). To contextualize these effects using unweighted numbers, the control group eligibles saw 13 cases of child mortality out of 2,596 children (0.5 percent) while the GD Large arm saw 2 cases out of 1,200 children (0.16 percent). Hence the GD Large arm saw meaningful improvements in consumption and child health. At the bottom of this table we provide the t-statistics on an F-test that the ratio of the benefits across the GD large and small arm differs from the ratio of their costs (5.01 to 1). This statistic asks whether we can say that the impact scale in a manner different to the costs; only in the case of debt reduction (where small transfers have a big effect and big transfers do not) and house quality (where small transfers have a engative and large transfers a positive effect) can we reject cost-symmetric benefit scaling for cash transfers.

Before taking the individual-level impacts at face value, it is important to recognize that most of

[16]The pattern here would be consistent with the theoretical insight presented in (Duflo et al., 2013), where a new opportunity to invest in productive assets can cause a 'piling in' of other sources of liquidity in the household, meaning that individuals may choose to consume less or buy fewer consumer durables during the investment phase.

[17]These improvements should be viewed against the backdrop of a sharp deterioration in anthropometrics subsequent to birth that typically occurs in LDCs, leaving rural African children often two full SDs below the international norm by age 3 (Shrimpton et al., 2001), (Victora et al., 2010).

the endline outcomes are only observed for in potentially endogenously selected (surviving children, women who had children during the study, etc.). If the treatments led to substantial changes in fertility or mortality patterns, then the average outcome among surviving children or mothers is subject to both extensive and an intensive margin drivers, and cannot be interpreted simply as a ceteris paribus impact on a given baseline individual (see Baird et al. (2016) for more discussion of this issue). In this sense the lack of impacts on pregnancy rates, and the small absolute value of the impacts on live births and mortality, suggest that shifts in the composition of living children are unlikely to be large drivers of the treatment/control differentials. It therefore appears very unlikely that differential patterns of fertility or mortality in response to the treatments are leading to large shifts in the composition of surviving children or mothers across arms, and hence we can interpret these impacts in a standard way.

One of the most fundamental results in theoretical development economics is that poor households should have a single 'shadow value' of cash which pulls down investment in all capital-hungry endeavors in a symmetric way. The above findings are generally consistent with this view of the world, as an intervention that relaxes credit constraints leads to shifts in consumption patterns that are very broadly spread across domains. This property means that small cash transfers are hard to detect because they move too many outcomes by too small an amount to be significant, while large cash transfers result in a broad-based increase in consumption in many dimensions.

Our pre-analysis plan states that for any outcomes where we find differential attrition, we would estimate a propensity to remain in the sample incorporating covariates, dummies for treatments, and their interactions on the right-hand side, and then re-weight the analysis by the product of the inverse of this probability and the standard sampling weight. This procedure corrects the impacts for the observable determinants of attrition, and uses regression weighting to attempt to make the treatment and control samples comparable on important covariates even after attrition. Because we primarily found significantly differential attrition for the anthropometric outcomes, in Table IPW we present the results of this correction. We interact with each treatment dummy the same right-hand side covariates we use the same controls in the anthropometric regressions: gender, a linear, quadratic, and cubic for age in months, baseline household wealth, and a dummy for membership in Ubudehe poverty category 1. The first three columns show the standard results, as in the previous table, and the next three show the corrected estimates. The results are virtually identical, indicating that the types of children who attrited from the study are similar across arms and hence differential attrition is unlikely to be driving our impacts.

3.3 Cost-Equivalent Benchmarking

The core purpose of the comparative experiment conducted is to exploit the randomized variation in transfer amounts to conduct an exact cost-equivalent benchmarking exercise. Using the costing numbers emerging from the ex-post exercise, we use the observed costs, overhead rates, and compliance rates to calculate the donor cost per eligible household in each arm of the study. Using Equation 3 from Section 3.1, we can control for 'any treatment', for the monetary deviation of the cash transfer arm amount from the ex-post Gikuriro cost, and then the inclusion of a dummy for 'Gikuriro treatment' will test the differential impact of Gikuriro over GD at precisely the same ex-post donor cost (subject to the assumption of linearity implicit in this formulation). A graphical representation of our strategy is provided in Figure 2, which plots the IHS of savings on the y-axis for all four GD treatment amounts (black circles), for GK (gray diamond), and the control (white circle). The line represents the fitted average savings by GD transfer amount, and by predicting the outcome on this line at the exact cost of Gikuriro (gray triangle), the benchmarked differential is then the vertical difference between the Gikuriro impact and the projected cost-equivalent GD impact.

The results of this analysis are presented in Table 7. Starting with the third row first, we have a direct estimate of the marginal effect of an additional 100 dollars in donor cost via cash transfers the primary and secondary outcomes.[18] As could be inferred from previous tables, this coefficient is strongly significant across a wide range of outcomes, particularly those most related to household consumption. An extra 100 dollars leads to a 5.6 percent increase in consumption, a 7.7 percent increase in dietary diversity, a 17 percent increase in savings, an 11 percent and 12 percent increase in productive and consumption assets, respectively, and leads housing value to improve by 4.6 percent and the index of housing quality to increase by 0.1 SD. In terms of anthropometrics, the change in value of transfer is significant only for height-for-age (where small transfers had a slight depressive effect). An extra \$100 per beneficiary household—with eligible households containing an average

[18] While GiveDirectly does of course have fixed costs, nearly all of the *marginal* increase in transfer value to a fixed population of recipients is received by the beneficiary: of the USD 421 increase from the upper cost-equivalent transfer and the GD-large transfer, USD 415.09 was received by beneficiaries themselves.

of 2.7 children under the age of six—increases HAZ by 0.022 standard deviations. Comparing a positive and insignicant effect of small transfers and a negative and insignificant effect of large transfers on the rate of live births, the transfer amount slope turns out to be significantly negative. Beyond this, none of the other individual outcomes respond to transfer amount in a manner that we can reject at 95 percent significance.

With the third row estimating the linear heterogeneity in impacts by transfer size around the cost-equivalent transfer, the second row (dummy for 'any treatment') becomes an intercept term that estimates the impact of cash transfers at a cost equivalent to Gikuriro, although this exact amount was not included in the experiment. Given that the mean transfer amount in the 'small' arm is only slightly lower than the GK cost, this estimate looks generally similar to the second row of table 5 (the simple average experimental effect across the 'small' transfer amounts). At the exact cost of Gikuriro, we estimate that cash transfers would have led to a significant 73 percent decrease in the stock of debt, and a 30 percent and 40 percent increase in productive and consumption assets, respectively.

The first row of this table contains the heart of the comparative benchmarking exercise. Looking first at the household outcomes, we see that Gikuriro is superior at driving up savings balances, while cash generates more debt reduction, a greater increase in consumption, and a larger accumulation of assets. The differential effect of the programs on savings and borrowing is interesting, and suggests that while both interventions serve to improve the net stock of liquid wealth (savings net of borrowing), the focused push on SILC groups in Gikuriro drives this more strongly through the vehicle of new savings while households making their own choices are more strongly disposed to reduce debt instead. Which of these strategies makes more sense? A simple comparison of interest rates is revealing. Gikuriro SILCs were free to set their own interest rates, but typically paid about 5 percent per annum nominal. Credit interest rates, by comparison, vary from an average of 22 percent in the MFI sector to upwards of 60 percent in informal credit markets. Given that 32 percent of eligible households reported having both borrowing and savings at baseline (and 79 percent had either borrowing or saving) it seems that the desire to pay down debt might be warranted.

Virtually none of the individual-level outcomes are significantly different across the interventions, arising from the fact that both of the inexpensive interventions in this study were ineffective at moving child outcomes, and only large cash transfers did this. There are two outcomes significant in the differential comparisons that are not significant in either intervention in Table 5, namely sanitation practices and births in facilities (both of which somewhat surprisingly favor GD). We do not emphasize these results because in absolute comparison to the control group both arms are ineffective.

3.4 Total Causal Effects

Because of the random sampling of ineligibles, we can conduct an analysis representive of the population of study villages by pooling the strata together and using population sampling weights. The average weight in the ineligible sample is 24.4 and in the eligible sample it is 2, meaning that while the unweighted eligible sample is larger, it is the ineligibles who will dominate the weighted sample. Recall that the treatment effects on ineligible households may arise either from the treatment and targeting of the two interventions among ineligible households (with saturations set to be the same at just over 18 percent on average in both arms, but with targeting differing), or from spillovers between eligible and ineligible households. With 11.4 percent of all households being defined as eligible, the treatment rate in the ineligible sample is 8.4 percent. This means that the large majority of the additional sample included in the TCE analysis only receive impacts through spillover effects to untreated households.

These impacts, using (1) but including the ineligible sample and using weights to reflect the whole village population, are presented in Table 8. Here, the overall picture is very different from the impact among eligibles. For Gikuriro, we see improvements at the 99 percent signficance level in the index of health knowledge, a core component of the program and one which was broadly targeted at the village population by the program (as reflected by our accounting of these costs as 'non-averted'). Vaccinations, presumably provided by government health facilities but not a central focus of Gikuriro, also improve significantly. So there is some real evidence of holistic benefits in health-related domains for the population of Gikuriro villages. These changes, it is true, do not translate into observable improvements in health outcomes for children or adults within the timeframe of the study, but still suggest that Gikuriro implementers have been successful in driving community-level health knowledge.

With cash transfers, on the other hand, improvements appear to be more narrowly limited to the beneficiaries of the transfers. The 'small' transfers do not change any village-level outcome at the 10 percent level. The 'large' transfers, so positive among beneficiaries, in general see negative signs across the consumption indicators, lead to a significant drop in savings at the village level, and are only positively associated with vaccination rates. These results are consistent with GD 'large' transfers having some negative spillover effects on non-beneficiaries, such as might be generated by an increase in local consumer prices (consistent with Cunha et al. (forthcoming)) or a decrease in interest rates as transfers are consumed and saved. On net there is little evidence that the widespread benefits observed in the eligibles carry over to the broader population of the village when the transfers are targeted at a relatively small fraction of the households.

3.5 Benchmarked Total Causal Effects

We can perform a similar cost-equivalent benchmarking exercise for the TCEs, adjusting now by cost per household in the overall village. This allows us to ask how the two programs differ in their impact on households in the village as a whole when the same amount is spent by each program on average. This analysis is presented in table 9. Again beginning with the third row, we see that an increase in transfers of $100 (now to the average household in the village) increases the value of productive assets by a half of a percent. Health knowledge appears to deteriorate with the amount transferred. The cost benchmarked cash transfer in the second row has no impact on household-level variables at the village level, but does improve vaccinations. The core comparative benchmarking exercise in the first row shows Gikuriro strongly superior at improving health knowledge at the village level. A significantly larger improvement in dietary diversity under Gikuriro is based on a comparison between two insignificant effects and so again is not emphasized.

3.6 Lump Sum versus Flow Transfers

GiveDirectly households were further randomized to Lump Sum, Flow, and Choice treatments. We can set aside the (random) group assigned to choice and begin our analysis of the GD sub-experiments by comparing how receiving money as a lump sum drives differential impact relative to the same amount of money assigned as a flow. We include separate dummy variables for the impact of the lump sum transfer in the 'small' and 'large' arms, using the flow transfers as the base category in both arms. Our hypothesis for this analysis was that lump sum transfers would be less good at improving outcomes such as nutrition, anthropometrics, and anemia that are based on a cumulate flow of consumption over the duration of the study, while the lump sum transfers would be superior in driving large one-time investments such as productive assets and consumer durables.

Table 10 shows the results of this analysis. In the 'large' arm we see results that are largely in line with expectations, in that lump sum transfers generate weakly better household wealth and strongly superior value of consumption assets, leading to a doubling of value relative to flow transfers. In the 'small' arm we see a weakly larger pay-down of debt for lump sum transfers, but contrary to expectations housing values are somewhat lower. Overall, we see some confirmation at the superiority of lump-sum transfers in driving fixed investment. In the individual outcomes, we also see some confirmation of expectations in that 5 out of 6 estimates of anthropometrics have negative signs on the lump sum interactions, and improvements in MUAC for the 'large' arm were limited to the flow transfers.

3.7 Analysis of Transfer Timing Choice within the GD Arm

An intriguing benefit of offering a choice of treatment modalities is that individuals might be able to use private information to select a welfare-optimizing treatment in a way that a central targeter could not. While we have relatively low power to examine this question, we can attempt to shed light on the direct benefits of choice using our experimental variation. To do this, we evaluate the outcome in the choice arm relative to a counterfactual that would be generated if individuals had no ability to select according to treatment effects and so choose effectively at random, generating an outcome that would be the weighted average of the outcomes of those assigned to lump sum and flow (where weights come from the fraction of the choice arm choosing each alternative).

We define dummy variables for the flow transfer, the lump-sum transfer, and the choice treatment. We can run the ANCOVA impact regression as follows:

$$Y_{ic2} = \beta_0 + \delta^F T_c^F + \delta^{LS} T_c^{LS} + \delta^C T_c^C + \beta X_{ic1} + \rho Y_{ic1} + \epsilon_{ic2} \quad \forall \quad E_i = 1, T^{GK} = 0 \tag{3}$$

The benefit of choice than then be tested via an F-test of the hypothesis that $\delta^C = \mu\delta^F + (1-\mu)\delta^{LS}$, where μ is the fraction of the choice arm that chooses the Flow arm. This tests for the domains in which individuals are able to obtain superior outcomes via choice than would be expected given the average outcome that would be expected if they had been assigned to those arms.

The results of this exercise are presented in Table 11. This test is based on only 89 eligible panel households assigned to the choice arm, but it nonetheless provides some suprisingly strong evidence in favor of flexibility in cash transfer modality. Dietary diversity, maternal anemia, live births, and births in facilities all not only improve significantly in the choice arm, but improve significantly more than would be expected based on the proportionately weighted averages of the lump sum and flow arms. Only health knowledge looks worse with choice. While these results are more speculative due to the small sample size, this suggests that choice itself is generating superior health outcomes relative to an external assignment mechanism.

4 Analysis of Heterogeneity

4.1 Anthropometric effects by baseline malnourishment

We hypothesized in the Pre-Analysis Plan that the benefits of the treatments in terms of child anthropometrics would be largest for those who began the study most malnourished. To test this, we run a regression with child anthropometrics (HAZ, WAZ, and MUAC) as the outcomes, using the structure of Equation 1 above and controlling for our battery of baseline covariates, a dummy for all three treatments (GK, GD, and GD large), the baseline biometric outcome, and the interaction between the treatments and baseline biometrics. The hypothesis is that the interaction terms will be negative, meaning that the programs are most effective for those who had the worst baseline biometric outcomes. Table 12 the results of this analysis. The interpretation of the impacts in this table are as follows: rows 4-6 give the simple impact of the programs when the interacted term is zero (which, in this case, is at the mean). Rows 1-3 provide a test of the differential impact of the program across baseline anthropometric measures, so the lack of significance in these rows means that the impacts are not heterogeneous by nutrition status at baseline. The implication is that the improvement in anthropometrics induced by the GD large treatment were experienced broadly across the baseline distribution of HAZ and WAZ, and were not concentrated among those who began the study most malnourished.

4.2 Anthropometric impacts by child age

We can use a similar approach to examine heterogeneity by child age. Given that we have children who start the study outside of the first 1,000 days (those 2-5 years old at baseline), we might expect that the impact of the program on these more fully developed children would be smaller. Similarly, we can examine the relative impacts of the program for children born during the study to examine the relative benefits of newborn/in utero exposure relative to children who were eating solid foods when the program began. Given the impacts on household dietary diversity this latter outcome may be particularly relevant.

In Table 13 we run regressions on the eligible sample, allowing treatment interactions with an indicator for child age at endline (we do not use the ANCOVA specification in this regression so as to be able to include children born subsequent to the baseline survey). We include two different interactions; one using a dummy for 'first 1,000 days at baseline', and a second more stringent dummy for 'born since baseline'. The results are presented in Table 13, and are in many ways surprising. Again, rows 4-6 provide the simple effect of the program in the older group (where the interaction term is set to zero) and the first three rows provide a test of whether the impact is differential for those in their first 1000 days (columns 1-3) or for newborns (columns 4-6). The small GD treatment is *less* successful for HAZ among children in the first 1,000 days, and the Gikuriro and large GD treatment effects display no consistent relationship to age.

Because of the strong overall impacts of the large GD arm on HAZ, we delve deeper into the heterogeneity of this result. Figure 3 uses a Fan regression to present a non-parametric picture of the GD large treatment effects across the age distribution for HAZ, and Figure 4 presents the same graphic using anemia as the outcome. Both of these figures provide suggestive evidence that larger treatment effects are emerging among the very youngest children who were exposed to the program in utero. This pattern is similar to the medium-term results in Baird et al. (2016), who find unconditional transfers in Malawi to have the largest effect on children exposed in utero. More speculatively, for both outcomes effects are also significant again for children in the 2-4 year age range when the program began, who would have been eating solid food, and hence the impacts appear least significant among children of age to be being breastfed at the time of the intervention.

To summarize, treatment effects prove to be relatively homogeneous across both age and baseline malnutrition status, although there is a suggestion that children exposed in utero may have benefitted the most from the consumption increases seen in the GD Large arm. Given the relatively short-term nature of our followup survey, we consider this analysis to illustrate the speed at which the dramatic improvements in consumption and dietary diversity at the household level seen under the GD Large treatment translate into improved biometric outcomes. The longer-term literature has typically found impacts of large cash transfer programs on HAZ in the range of 0.2–0.45 standard deviations (Aguero et al., 2006; Barham et al., 2014); our results suggest that these measures improve roughly 0.1 standard deviation within a year of the inception of treatment, are all significant at 10 percent level or above, and are relatively invariant to baseline age or malnutrition. Comparison to the broader literature suggests that these impacts may grow over time.

5 Conclusion

This study compares a multi-pronged child nutrition program against cash transfers that have been benchmarked to have equal cost to the donor. We find that this integrated program was able within 8-9 months to move an important outcome directly generated by a component of its intervention, namely SILC-driven savings, and also to significantly improve health knowledge at the village level, a core component of the WASH and BCC interventions. The program appears to have improved the trackability of malnourished children in treatment villages. Nonetheless, it did not significantly shift the core child anthropometric outcomes that these interventions are intended to target, nor child or maternal anemia. An equivalent amount of money provided in the form of a cash transfer led to small increases across many consumption outcomes, and significantly increased the paydown of debt, as well as production and consumption assets. A much larger cash transfer of over $500 per household delivered comprehensive consumption benefits and also moved key health outcomes such as dietary diversity, height and weight for age, and mortality. The results are nuanced in terms of the modality of delivery, the choice of outcome, and the magnitude of transfers.

In concluding on the impact of Gikuriro, it is certainly possible that investments in behavior change (such as the substantial village-level improvement in health knowledge) as well as improvements in the Community Health Worker system will have longer term impacts that may do more for the population's nutrition over time than even a large cash transfer to a single cohort today. If an increasingly capable local health infrastructure can deliver nutrition interventions over the next ten years, then a huge number of future children could benefit and their well-being certainly should be part of the policy calculation. However a recent RCT evaluation of the Government of Rwanda's CBEHPP, which is the WASH program implemented under Gikuriro, found that it was not effective at decreasing diarrheal disease (Sinharoy et al., 2017). Given that the Gikuriro bundle which also included direct in-kind transfers of livestock and seed was not found here to improve short-term consumption, these broader institutional impacts would need to be substantial and improving over time if they are lead to large long-term impacts. Therefore while Gikuriro did shift some outcomes closely related to pieces of the intervention, we uncover little evidence that directly confirms the program's theory of change in driving primary child outcomes.

Our study design places some limitations on what can be learned. First, the cross-village experiment lets us measure the impact of those parts of the Gikuro intervention that are implemented at the village or household level only. Further, the relatively short time frame of the study means that we are measuring impacts over a shorter timeframe than would be ideal, particularly for outcomes such as malnutrition and anthropometrics that may respond slowly to improvements in nutrition. Gikuriro made substantial investments in local capacity around health and sanitation, and this infrastructure may drive future benefits in a way not captured in our study. We do however cost both interventions in a manner that reflects only the ingredients to the spatial and temporal scales over which we can measure impacts (cross-village, one year) and hence can cleanly benchmark the benefit/cost ratios of these two very different interventions over their first year of operation.

We can use our results to make concrete two types of tradeoffs that policymakers considering counterfactual uses of funds are forced to confront. First, tradeoffs across outcomes—for a given set of beneficiaries—and second, tradeoffs between larger impacts concentrated among a smaller number of beneficiaries and more diffuse impacts in a broader swathe of the target population.

To illustrate the first of these margins, consider the a policymaker who values impacts on savings and productive assets; for simplicity of exposition we ignore other dimensions of impact.[19] Both of these represent investments in the future consumption of the household. When compared with a cost-equivalent cash transfer, Gikuriro has relatively larger effects on savings: a differential impact

[19]In general, it is appropriate to ignore dimensions of impact only when either the policymaker places no weight on them, or when there is no differnetial impact of available interventions on these dimensions.

of 118 percent as shown in Table 7. On the other hand, GiveDirectly's cost-equivalent transfer delivers an impact on the value of productive assets of approximately 30 percent relative to control, and our estimates suggest the impact of Gikuriro is 29 percent smaller. Then, a policy preference for Gikuriro is equivalent to asserting that it is worth foregoing the increase in productive assets in order to obtain the improvement in savings, or that the relative benefit from percent increases in savings versus productive assets exceeds 4.06, the ratio of the differential treatment effects. This kind of precision can help policymakers be much more exact as to the types of tradeoffs required to justify one type of intervention over another.

Our design further reveals tradeoffs on the second, interpersonal margin. Within the cash arm, variation in the transfer amount provides one simple way of posing this question. The ratio of consumption impacts between the GD Small and Large treatment effects, for example, is 5.2, very close to the ratio of transfer amounts. This suggests that consumption impacts will scale linearly with transfer amounts. Other outcomes, such as savings or height-for-age, actually deteriorate slightly (and statistically insignificantly) under small transfers and are only improved with large transfers, suggesting that more resources must be concentrated on fewer households in order to drive these outcomes. We can also compare Gikuriro to cash in this respect. While at cost-equivalent values, Gikuriro demonstrates larger impacts on savings than a household grant, the results in Table 7 demonstrate that this is specific to the relatively small value of the cash transfer involved. We can use the slope term on the transfer amount to calculate the smallest transfer at which the benefit of cash would exceed Gikuriro; for savings this number is \$694, and for HAZ it is \$277. Obviously, on a fixed budget large cash transfers can only be given to a smaller number of households. So we can use our estimates to paramaterize tradeoffs between cash transfer and Gikuriro in terms of the types of outcomes we would be able to drive as we concentrate more resources on fewer households. The elasticity of cash's impacts as amounts are varied illustrates that programmatic choices hinge not only on preferences for equality across beneficiaries, but the specific outcomes being targeted.[20] The strong consumption benefits that have quite consistently been found in large cash transfer programs may well tip the scales in favor of concentrated programs, even for inequality-averse planners.

[20]More generally, one might characterize funders as having, e.g., CES preferences of the form $W = \sum_i \left(\sum_k \alpha_k x_{ik}^{\rho}\right)^{\phi/\rho}$, where i indexes beneficiaries and k indexes dimensions of program objectives x_{ik}. Here, α_k captures the relative weight of outcome k in funder preferences, ρ captures the extent of substitutability across these outcomes, and $\phi > 0$ captures the degree of substitutability across individuals (with a low value of ϕ implying a strong preference for equality across beneficiaries).

Given the nuance of our results, it is hard to square these results with any simple idea of cash transfers as a kind of uni-dimensional 'index fund'. While business investment has a single, cardinal objective (financial profit), humanitarian investment is undertaken with many goals in mind, and a perfect reconciling of these competing benefits would require a clear statement of tradeoffs in this multi-dimensional space. Perhaps a clearer way of stating the counterfactual provided by unconditional cash is that it gives us a statement of the priorities that the beneficiaries themselves hold when credit constraints are relaxed, and thereby motivates us to be clearer about the logic underlying paternalistic development programs. While beneficiary decisions may not be 'optimal' in terms of long-term social welfare (due high discount rates, to self- or other-control problem, or to resource and information constraints for example), the impact of unconditional money is nonetheless a powerful statement of the outcomes that the beneficiaries themselves want changed. For us to argue that a program is justified in using resources to drive outcomes different from the ones the beneficaries would choose, we should have a clear reason why they fail to arrive at the right decision themselves. This is a view of benchmarking that is not about picking a winner but rather about quantifying tradeoffs.

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Tables

Table 1: Research Design

	Control	Gikuriro	Give Directly				Total
			Lower transfer **$66**	Middle transfer **$111**	Upper transfer **$145**	Large transfer **$566**	
	74 villages	74 villages	22 villages	22 villages	22 villages	34 villages	248 villages
Ineligible Households	298	297	88	87	88	137	995
Eligible Households	521	541	165	154	167	246	1,794
		Household-level randomization of cash payment modality among eligibles:					
		Flow Transfers:	83	87	104	147	421
		Lump-Sum Transfers:	51	50	41	68	210
		Choice:	31	17	22	31	101

Table 2: Intervention Costs and Compliance Rates

Treatment Arm:	Cost per beneficiary to USAID	Fraction of cost that is overhead (IDC/(DC+IDC))	Value received by beneficiary households	Fraction spending not averted if household not treated (fraction costs @ village level)	Compliance Rate among eligibles	Cost to USAID per eligible household	Compliance rate in population	Cost to USAID per household in village
Gikuriro	$141.84	51.4%	$72.91	40%	90.9%	$134.13	19.0%	$72.94
GD lower	$66.02	37.4%	$41.32	0%	96.5%	$63.74	18.5%	$12.20
GD mid	$111.09	24.7%	$83.63	0%	99.7%	$110.73	18.8%	$20.83
GD upper	$145.43	19.6%	$116.91	0%	93.3%	$135.72	18.4%	$26.69
GD large	$566.55	6.1%	$532.00	0%	98.0%	$555.15	17.6%	$99.56

Table 3: Attrition

Panel A: Household-Level Attrition:

	Outcome:	
	Household Attrites	
VARIABLES	(1)	(2)
Gikuriro treatment village	-0.013	-0.011
	(0.0092)	(0.0085)
GiveDirectly Normal treatment village	-0.0089	-0.0097
	(0.010)	(0.010)
GiveDirectly Huge treatment village	-0.017*	-0.015
	(0.0097)	(0.0093)
HH Head Female		0.0011
		(0.011)
HH does engage in agriculture at baseline		-0.015
		(0.012)
HH has wage worker at baseline		0.0076
		(0.0093)
HH does operate microenterprise at baseline		-0.0083
		(0.0075)
HH does participate in SILC at baseline		0.00040
		(0.0085)
Fraction of village defined eligible by IPA		0.021
		(0.053)
Age of household head in years		-0.00034
		(0.00022)
Household head is in school		0.11
		(0.14)
HH dependency ratio (#old + #young)/(#hh members)		0.035
		(0.030)
How many people live in your household?		-0.0036
		(0.0023)
Ubudehe poverty Category 1 (poorest)		0.0097
		(0.011)
Ubudehe poverty Category 1 (next poorest)		0.0061
		(0.0085)
Control group mean	0.033	0.033
Observations	1793	1793
R^2	0.0019	0.023

Regressions are baseline cross-sections, standard errors are clustered at the village level to

*** p<0.01, ** p<0.05, * p<0.1

Panel B: Individual-Level Attrition:

	Outcome:						
	Individual Attrites from Roster		Child Attrites from Anthropometrics		R2 Anemia is observed (among those who should)		New individual appears in survey data
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(5)
Gikuriro treatment village	-0.010	-0.0086	-0.037***	-0.038***	-0.0044	-0.0046	-0.00050
	(0.0083)	(0.0079)	(0.013)	(0.012)	(0.021)	(0.016)	(0.0081)
GiveDirectly Normal treatment village	-0.0077	-0.0076	-0.024*	-0.025**	-0.0069	-0.0013	-0.0027
	(0.0093)	(0.0097)	(0.013)	(0.013)	(0.019)	(0.016)	(0.0088)
GiveDirectly Large treatment village	-0.015*	-0.013	-0.022	-0.020	0.027	0.0056	-0.0051
	(0.0086)	(0.0088)	(0.016)	(0.015)	(0.026)	(0.023)	(0.0096)
Female		0.0011		0.0012		-0.045***	
		(0.0026)		(0.0088)		(0.014)	
HH does engage in agriculture at baseline		-0.020		-0.037*		0.023	
		(0.013)		(0.020)		(0.023)	
HH has wage worker at baseline		0.016		0.019		0.0038	
		(0.0099)		(0.015)		(0.016)	
HH does operate microenterprise at baseline		-0.0085		-0.00060		-0.039*	
		(0.0063)		(0.017)		(0.022)	
HH does participate in SILC at baseline		-0.0051		-0.015		-0.0020	
		(0.0071)		(0.012)		(0.017)	
Fraction of village defined eligible by IPA		0.015		0.032		-0.0030	
		(0.048)		(0.050)		(0.060)	
Age of household head in years		-0.00025		0.00013		-0.0017***	
		(0.00019)		(0.00034)		(0.00052)	
Household head is in school		0.21		0.053		0.13***	
		(0.21)		(0.11)		(0.025)	
HH dependency ratio (#old + #young)/(#hh members)		0.027		0.058		0.029	
		(0.032)		(0.056)		(0.063)	
How many people live in your household?		-0.0031		-0.0060		-0.040***	
		(0.0023)		(0.0040)		(0.0058)	
Ubudehe poverty Category 1 (poorest)		-0.0055		0.0065		-0.017	
		(0.0098)		(0.018)		(0.021)	
Ubudehe poverty Category 1 (next poorest)		-0.0048		0.0059		0.0064	
		(0.0094)		(0.014)		(0.017)	
Control group mean	0.030	0.030	0.071	0.071	0.80	0.80	0.057
Observations	9189	9189	2265	2265	5192	5192	9509
R^2	0.0014	0.028	0.0049	0.025	0.00068	0.068	0.000054

Regressions are baseline cross-sections, standard errors are clustered at the village level to reflect the design effect, and analysis is weighted to be

*** p<0.01, ** p<0.05, * p<0.1

Table 4: Balance

Baseline Balance Tests

Panel A: Baseline Household-level Outcomes:

	Primary Outcomes:			Secondary Outcomes:							
VARIABLES	Per Capita Monthly Consumption†	Dietary Diversity	Total Household Wealth†	Borrowing†	Saving†	Health Knowledge	Sanitation Practices	Productive Assets†	Consumption Assets†	House Value†	House Quality
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Gikuriro Treatment	0.044	-0.071	0.19	-0.43	-0.19	-0.59	0.29*	0.30**	0.15	-0.050	0.017
	(0.12)	(0.14)	(0.29)	(0.36)	(0.38)	(0.37)	(0.17)	(0.13)	(0.29)	(0.062)	(0.11)
GiveDirectly Small transfers	0.047	-0.071	0.038	-0.0069	-0.67*	-0.12	-0.10	0.21	-0.055	-0.019	-0.19
	(0.12)	(0.14)	(0.28)	(0.41)	(0.36)	(0.41)	(0.19)	(0.14)	(0.31)	(0.076)	(0.13)
GiveDirectly Large transfer	-0.10	-0.063	-0.070	-0.29	-0.24	-0.23	-0.069	0.28**	0.45	-0.075	-0.026
	(0.13)	(0.17)	(0.31)	(0.42)	(0.42)	(0.52)	(0.21)	(0.13)	(0.30)	(0.069)	(0.20)
Control Group Mean	10.4	4.16	12.7	5.96	5.18	0.19	-0.23	11.2	8.72	13.6	0.018
Observations	1744	1744	1745	1745	1744	1751	1751	1744	1744	1638	1750
R-squared	0.053	0.098	0.053	0.040	0.016	0.029	0.042	0.12	0.078	0.098	0.040

Regressions are baseline cross-sections, standard errors are clustered at the village level to reflect the design effect, and analysis is weighted to be representative of all eligible households in study villages. Outcomes marked with a † are analyzed in Inverse Hyperbolic Sines, meaning that impacts should be interepreted as percent changes in the outcome. *** p<0.01, ** p<0.05, * p<0.1

Panel B: Baseline Individual-level Outcomes:

	Primary Outcomes:			Secondary Outcomes:					
VARIABLES	Height-for-Age	Weight-for-Age	Mid-Upper Arm Circumference	Pregnancy	Live Birth	Births in Facilities	Vaccinations (any in past year)	Vaccinations complete	Diarrheal Prevalence
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Gikuriro Treatment	-0.021	-0.029	0.000099	-0.011	-0.0085	0.010	0.0044	-0.0018	0.023
	(0.086)	(0.067)	(0.068)	(0.026)	(0.042)	(0.038)	(0.019)	(0.036)	(0.039)
GiveDirectly Small transfers	0.064	0.031	0.058	-0.026	0.0065	-0.076	-0.0092	-0.0090	0.0042
	(0.091)	(0.065)	(0.067)	(0.024)	(0.044)	(0.047)	(0.022)	(0.041)	(0.031)
GiveDirectly Large transfer	0.15	0.15**	0.065	-0.043	0.091*	-0.032	0.0071	0.040	-0.0076
	(0.097)	(0.070)	(0.090)	(0.028)	(0.055)	(0.046)	(0.025)	(0.040)	(0.042)
Control Group Mean	-1.92	-1.04	-0.65	0.32	0.81	0.93	0.93	0.72	0.43
Observations	2125	2104	1629	2099	663	561	1386	1384	1163
R-squared	0.16	0.076	0.094	0.053	0.16	0.15	0.071	0.087	0.047
Sample	Under 6 Anthro panel			Women	Pregnancies	Births	Kids under 2		Kids under 5

Regressions are baseline cross-sections, standard errors are clustered at the village level to reflect the design effect, and analysis is weighted to be representative of all eligible households in study villages. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Impacts among Eligible Households

Panel A: Household-level Outcomes:

	Primary Outcomes:			Secondary Outcomes:							
VARIABLES	Per Capita Monthly Consumption†	Dietary Diversity	Total Household Wealth†	Borrowing†	Saving†	Health Knowledge	Sanitation Practices	Productive Assets†	Consumption Assets†	House Value†	House Quality
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Gikuriro Treatment	-0.097	0.17	-0.028	0.075	1.09***	-0.086	-0.27	0.00082	-0.38	-0.019	-0.20
	(0.099)	(0.13)	(0.19)	(0.35)	(0.32)	(0.37)	(0.22)	(0.10)	(0.24)	(0.057)	(0.15)
GiveDirectly Small transfers	0.062	0.16	-0.017	-0.76**	-0.14	0.15	0.17	0.26**	0.37*	-0.025	-0.22*
	(0.092)	(0.15)	(0.21)	(0.31)	(0.34)	(0.32)	(0.23)	(0.10)	(0.21)	(0.062)	(0.13)
GiveDirectly Large transfer	0.32***	0.52***	0.34	-0.33	0.60*	0.059	0.074	0.76***	0.92***	0.20***	0.21
	(0.11)	(0.13)	(0.28)	(0.40)	(0.32)	(0.47)	(0.29)	(0.11)	(0.25)	(0.063)	(0.18)
Baseline Control Mean	10.7	4.77	13.0	7.39	5.88	2.89	-0.68	11.2	8.70	13.8	-0.17
F-test: GD benefit ratio = cost ratio	0.98	0.69	0.67	0.012	0.42	0.64	0.47	0.25	0.32	0.26	0.048
Observations	1744	1744	1745	1745	1744	1751	1751	1744	1744	1638	1750
R-squared	0.12	0.18	0.23	0.12	0.16	0.036	0.064	0.29	0.35	0.34	0.098

All regressions are ANCOVA with lagged dependent variables as controls, run on the panel sample. Regressions are weighted so as to be representative of all eligible households in study villages. Standard errors are clustered at the village level to reflect the design effect. Outcomes marked with a † are analyzed as inverse hyperbolic sine, meaning that impacts should be interepreted as percent changes in the outcome. *** p<0.01, ** p<0.05, * p<0.1

Panel B: Individual-level Outcomes:

	Primary Outcomes:					Secondary Outcomes:							
VARIABLES	Height-for-Age	Weight-for-Age	Mid-Upper Arm Circumference	Anemia, Children	Anemia, Mothers	Child Mortality	Pregnancy	Live Birth	Births in Facilities	Vaccinations (any in past year)	Vaccinations complete	Disease Burden	Diarrheal Prevalence
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Gikuriro Treatment	0.052	0.039	0.022	0.0041	-0.023	-0.0061	-0.031	0.11	-0.050	0.010	0.011	-0.019	-0.0035
	(0.045)	(0.040)	(0.056)	(0.022)	(0.027)	(0.0051)	(0.025)	(0.079)	(0.059)	(0.034)	(0.039)	(0.032)	(0.015)
GiveDirectly Small transfers	-0.019	0.011	-0.0065	0.024	-0.0015	-0.0036	-0.035	0.092	0.067	-0.011	-0.013	-0.031	-0.00022
	(0.039)	(0.033)	(0.065)	(0.025)	(0.029)	(0.0064)	(0.031)	(0.072)	(0.051)	(0.032)	(0.035)	(0.030)	(0.016)
GiveDirectly Large transfer	0.091**	0.067*	0.13*	-0.0078	-0.026	-0.0090**	-0.0065	-0.067	-0.064	-0.0060	0.0050	-0.018	-0.0067
	(0.045)	(0.036)	(0.078)	(0.038)	(0.031)	(0.0044)	(0.027)	(0.081)	(0.099)	(0.039)	(0.038)	(0.033)	(0.016)
Endline Control Mean	-1.92	-1.06	-0.72	0.18	0.12	0.013	0.20	0.68	0.84	0.72	0.58	0.54	0.090
F-test: GD benefit ratio = cost ratio	0.30	0.93	0.60	0.30	0.88	0.76	0.24	0.11	0.10	0.75	0.66	0.33	0.94
Observations	2125	2104	1629	2372	1581	2687	2552	411	293	1291	1291	2680	2680
R-squared	0.71	0.68	0.51	0.070	0.11	0.0097	0.082	0.13	0.16	0.26	0.17	0.053	0.043
ANCOVA	Y	Y	Y	N	N	N	N	N	N	N	N	N	N
Sample	Under 6 Anthro panel			Under 6	Moms	Under 6	Women	Pregnancies	Births	Under 3		Under 6	

Regressions with both baseline and endline outcome measurement are ANCOVA with lagged dependent variables as controls, run on the panel sample. Anemia data was only collected at endline, and regresions on pregnancy outcomes and mortality are endline cross-sectional regressions. Regressions are weighted so as to be representative of all eligible households in study villages. Standard errors are clustered at the village level to reflect the design effect.

*** p<0.01, ** p<0.05, * p<0.1

Table 6: Anthropometric Impacts using Attrition IPWs

Analysis using Attrition IPWs for Anthropometric Outcomes:
Individual-level Outcomes:

	Normal Survey Weights			**Survey * Attrition Propensity Weights**		
VARIABLES	Height-for-Age	Weight-for-Age	Mid-Upper Arm Circumference	Height-for-Age	Weight-for-Age	Mid-Upper Arm Circumference
	(1)	(2)	(3)	(4)	(5)	(6)
Gikuriro Treatment	0.052	0.039	0.022	0.051	0.038	0.022
	(0.045)	(0.040)	(0.056)	(0.045)	(0.040)	(0.056)
GiveDirectly Small transfers	-0.019	0.011	-0.0065	-0.021	0.0100	-0.0065
	(0.039)	(0.033)	(0.065)	(0.039)	(0.034)	(0.066)
GiveDirectly Large transfer	0.091**	0.067*	0.13*	0.091**	0.066*	0.13*
	(0.045)	(0.036)	(0.078)	(0.046)	(0.036)	(0.078)
Endline Control Mean	-1.92	-1.06	-0.72	-1.92	-1.06	-0.72
F-test: GD benefit ratio = cost ratio	0.30	0.93	0.60	0.28	0.92	0.60
Observations	2125	2104	1629	2125	2104	1629
R-squared	0.71	0.68	0.51	0.71	0.68	0.50
ANCOVA	Y	Y	Y	Y	Y	Y
Sample	Under 6 Anthro panel					

Regressions are ANCOVA with lagged dependent variables as controls, run on the panel sample. Regressions are weighted so as to be representative of all eligible households in study villages. Standard errors are clustered at the village level to reflect the design effect. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 7: Cost Equivalent Benchmarking

Panel A: Household-level Outcomes:

	Primary Outcomes:			Secondary Outcomes:							
VARIABLES	Per Capita Monthly Consumption†	Dietary Diversity	Total Household Wealth†	Borrowing†	Saving†	Health Knowledge	Sanitation Practices	Productive Assets†	Consumption Assets†	House Value†	House Quality
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Differential effect of Gikuriro at benchmarked cost	-0.18**	-0.016	-0.040	0.80***	1.18***	-0.23	-0.44**	-0.29***	-0.79***	-0.015	-0.0055
	(0.082)	(0.13)	(0.20)	(0.28)	(0.34)	(0.33)	(0.22)	(0.099)	(0.26)	(0.052)	(0.15)
GD cost equivalent impact (dummy for 'either treatment')	0.082	0.19	0.011	-0.73**	-0.094	0.15	0.16	0.30***	0.40**	-0.0051	-0.19
	(0.090)	(0.14)	(0.21)	(0.31)	(0.32)	(0.32)	(0.22)	(0.10)	(0.20)	(0.060)	(0.13)
Additional effect of each $100 from GD	0.056**	0.077**	0.075	0.092	0.17**	-0.024	-0.021	0.11***	0.12**	0.046***	0.098**
	(0.022)	(0.031)	(0.064)	(0.074)	(0.074)	(0.097)	(0.066)	(0.025)	(0.056)	(0.014)	(0.045)
Baseline Control Mean	10.4	4.16	12.7	5.96	5.18	0.19	-0.23	11.2	8.72	13.6	0.018
Observations	1744	1744	1745	1745	1744	1751	1751	1744	1744	1638	1750
R-squared	0.12	0.18	0.23	0.12	0.16	0.036	0.064	0.29	0.35	0.33	0.099

All regressions are ANCOVA with lagged dependent variables as controls, run on the panel sample. First row is a dummy for Gikuriro treatment, and reflects the differential effect of Gikuriro treatment over GD at equivalent cost. Second row is a dummy for either treatment. The third row includes the dollar-value deviation (in hundreds of dollars) of the GD village-level transfer amount from the cost of Gikuriro, meaning that the first row serves as an intercept and measures impact at equivalent cost. Regressions weighted so as to be representative of eligible households in study villages. Standard errors are clustered at the village level to reflect the design effect. Outcomes marked with a † are analyzed as inverse hyperbolic sine, meaning that impacts should be interepreted as percent changes in the outcome. *** p<0.01, ** p<0.05, * p<0.1

Panel B: Individual-level Outcomes:

	Primary Outcomes:					Secondary Outcomes:							
VARIABLES	Height-for-Age	Weight-for-Age	Mid-Upper Arm Circumference	Anemia, Children	Anemia, Mothers	Child Mortality	Pregnancy	Live Birth	Births in Facilities	Vaccinations (any in past year)	Vaccinations complete	Disease Burden	Diarrheal Prevalence
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Differential effect of Gikuriro at benchmarked cost	0.061	0.024	0.019	-0.018	-0.019	-0.0023	0.0023	0.024	-0.11**	0.019	0.022	0.012	-0.0034
	(0.040)	(0.039)	(0.062)	(0.026)	(0.021)	(0.0049)	(0.027)	(0.067)	(0.051)	(0.032)	(0.036)	(0.033)	(0.014)
GD cost equivalent impact (dummy for 'either treatment')	-0.0098	0.014	0.0031	0.022	-0.0046	-0.0038	-0.034	0.081	0.057	-0.0087	-0.010	-0.031	-0.00011
	(0.038)	(0.032)	(0.063)	(0.025)	(0.028)	(0.0061)	(0.030)	(0.070)	(0.050)	(0.031)	(0.034)	(0.029)	(0.015)
Additional effect of each $100 from GD	0.022**	0.013	0.030	-0.0076	-0.0043	-0.0014	0.0070	-0.035**	-0.028	-0.00033	0.0029	0.0042	-0.0019
	(0.0096)	(0.0086)	(0.019)	(0.0093)	(0.0055)	(0.0012)	(0.0063)	(0.017)	(0.021)	(0.0088)	(0.0085)	(0.0079)	(0.0034)
Endline Control Mean	-1.92	-1.06	-0.72	0.22	0.12	0.0056	0.20	0.68	0.84	0.72	0.58	0.54	0.090
Observations	2125	2104	1629	2372	1581	2687	2552	411	293	1291	1291	2680	2680
R-squared	0.71	0.68	0.51	0.071	0.11	0.0098	0.082	0.13	0.16	0.26	0.17	0.053	0.043
ANCOVA	Y	Y	Y	N	N	N	N	N	N	N	N	N	N
Sample	Under 6 Anthro panel			Under 6	Moms	Under 6	Women	Pregnancies	Births	Under 3		Under 6	

Regressions with both baseline and endline outcome measurement are ANCOVA with lagged dependent variables as controls, run on the panel sample. Anemia data was only collected at endline, and regresions on pregnancy outcomes and mortality are endline cross-sectional regressions. Regressions are weighted so as to be representative of all eligible households in study villages. Standard errors are clustered at the village level to reflect the design effect. *** p<0.01, ** p<0.05, * p<0.1

Table 8: Total Causal Effects on Whole Village

Panel A: Household-level Outcomes:

	Primary Outcomes:			**Secondary Outcomes:**							
VARIABLES	Per Capita Monthly Consumption†	Dietary Diversity	Total Household Wealth†	Borrowing†	Saving†	Health Knowledge	Sanitation Practices	Productive Assets†	Consumption Assets†	House Value†	House Quality
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Gikuriro Treatment	-0.14	0.12	-0.19	0.14	-0.21	1.44***	0.17	-0.056	0.10	0.0065	-0.17
	(0.10)	(0.13)	(0.24)	(0.41)	(0.37)	(0.39)	(0.20)	(0.14)	(0.23)	(0.074)	(0.17)
GiveDirectly Small transfers	-0.13	0.0010	-0.32	-0.39	-0.40	0.68*	-0.27	-0.15	0.021	0.031	-0.0081
	(0.11)	(0.13)	(0.28)	(0.39)	(0.40)	(0.41)	(0.23)	(0.14)	(0.24)	(0.066)	(0.16)
GiveDirectly Large transfer	-0.090	-0.24	-0.47	-0.26	-0.75**	-0.40	0.30	0.25	0.35	0.083	-0.019
	(0.18)	(0.18)	(0.30)	(0.43)	(0.38)	(0.54)	(0.35)	(0.17)	(0.37)	(0.077)	(0.19)
Baseline Control Mean	10.6	4.29	13.9	5.35	6.03	-0.12	0.29	12.0	9.85	14.0	0.20
F-test: GD benefit ratio = cost ratio	0.26	0.68	0.40	0.35	0.50	0.048	0.14	0.14	0.83	0.82	0.98
Observations	2710	2710	2710	2708	2710	2718	2718	2710	2709	2494	2716
R-squared	0.11	0.22	0.28	0.11	0.15	0.055	0.068	0.31	0.31	0.40	0.16

All regressions are ANCOVA with lagged dependent variables as controls, run on the panel sample. Regressions include both 'eligible' and 'ineligible' households and are weighted so as to be representative of all households in study villages. Standard errors are clustered at the village level to reflect the design effect. Outcomes marked with a † are analyzed as inverse hyperbolic sine, meaning that impacts should be interepreted as percent changes in the outcome. *** p<0.01, ** p<0.05, * p<0.1

Panel B: Individual-level Outcomes:

	Primary Outcomes:			**Secondary Outcomes:**							
VARIABLES	Height-for-Age	Weight-for-Age	Mid-Upper Arm Circumference	Child Mortality	Pregnancy	Live Birth	Births in Facilities	Vaccinations (any in past year)	Vaccinations complete	Disease Burden	Diarrheal Prevalence
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Gikuriro Treatment	-0.015	-0.066	-0.072	0.0042	0.020	0.0070	-0.027	0.11*	0.20***	0.018	0.0070
	(0.054)	(0.049)	(0.069)	(0.0057)	(0.022)	(0.077)	(0.070)	(0.059)	(0.064)	(0.052)	(0.018)
GiveDirectly Small transfers	-0.012	-0.015	-0.024	-0.0014	0.0017	-0.043	0.048	0.074	0.079	-0.028	0.033
	(0.054)	(0.039)	(0.069)	(0.0021)	(0.020)	(0.091)	(0.061)	(0.057)	(0.064)	(0.047)	(0.022)
GiveDirectly Large transfer	0.049	0.020	0.062	-0.0030	-0.0072	-0.044	-0.0060	0.16**	0.21***	-0.0026	0.030
	(0.058)	(0.051)	(0.086)	(0.0023)	(0.023)	(0.093)	(0.088)	(0.062)	(0.064)	(0.055)	(0.025)
Endline Control Mean	-1.75	-0.87	-0.61	0.0059	0.12	0.70	0.90	0.73	0.48	0.50	0.068
F-test: GD benefit ratio = cost ratio	0.65	0.61	0.58	0.69	0.86	0.69	0.35	0.39	0.52	0.52	0.19
Observations	2618	2594	1981	3373	4137	594	416	1479	1479	3366	3366
R-squared	0.74	0.74	0.57	0.020	0.11	0.13	0.16	0.31	0.17	0.064	0.053
ANCOVA	Y	Y	Y	N	N	N	N	N	N	N	N
Sample	Under 6 Anthro panel			Under 6	Women	Pregnancies	Births	Under 3		Under 6	

Regressions with both baseline and endline outcome measurement are ANCOVA with lagged dependent variables as controls, run on the panel sample. Anemia data was only collected at endline, and regresions on pregnancy outcomes and mortality are endline cross-sectional regressions. Regressions include both 'eligible' and 'ineligible' households arnd are weighted to be representative of all households in study villages. *** p<0.01, ** p<0.05, * p<0.1

Table 9: Cost Benchmarked Total Causal Effects

Panel A: Household-level Outcomes:

	Primary Outcomes:			**Secondary Outcomes:**							
VARIABLES	Per Capita Monthly Consumption†	Dietary Diversity	Total Household Wealth†	Borrowing†	Saving†	Health Knowledge	Sanitation Practices	Productive Assets†	Consumption Assets†	House Value†	House Quality
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Differential effect of Gikuriro at benchmarked cost	-0.033	0.27*	0.22	0.48	0.43	1.49***	0.057	-0.18	-0.13	-0.057	-0.15
	(0.13)	(0.15)	(0.24)	(0.37)	(0.35)	(0.43)	(0.25)	(0.14)	(0.28)	(0.074)	(0.15)
GD cost equivalent impact (dummy for 'either treatment')	-0.11	-0.14	-0.40	-0.33	-0.64*	-0.043	0.11	0.12	0.23	0.063	-0.021
	(0.13)	(0.14)	(0.25)	(0.37)	(0.33)	(0.43)	(0.26)	(0.14)	(0.28)	(0.063)	(0.16)
Additional effect of each $100 from GD	0.00046	-0.0025	-0.0011	0.00047	-0.0046	-0.014**	0.0070	0.0052**	0.0038	0.00058	-0.00035
	(0.0022)	(0.0022)	(0.0043)	(0.0051)	(0.0051)	(0.0068)	(0.0045)	(0.0020)	(0.0045)	(0.00099)	(0.0021)
Baseline Control Mean	10.6	4.29	13.9	5.35	6.03	-0.12	0.29	12.0	9.85	14.0	0.20
Observations	2710	2710	2710	2708	2710	2718	2718	2710	2709	2494	2716
R-squared	0.11	0.21	0.28	0.11	0.15	0.056	0.068	0.31	0.31	0.40	0.16

All regressions are ANCOVA with lagged dependent variables as controls, run on the panel sample. First row is a dummy for Gikuriro treatment, and reflects the differential effect of Gikuriro treatment over GD at equivalent cost. Second row is a dummy for either treatment. The third row includes the dollar-value deviation (in hundreds of dollars) of the GD village-level transfer amount from the cost of Gikuriro, meaning that the first row serves as an intercept and measures impact at equivalent cost. Regressions weighted so as to be representative of eligible households in study villages. Standard errors are clustered at the village level to reflect the design effect. Outcomes marked with a † are analyzed as inverse hyperbolic sine, meaning that impacts should be interepreted as percent changes in the outcome. *** p<0.01, ** p<0.05, * p<0.1

Panel B: Individual-level Outcomes:

	Primary Outcomes:					**Secondary Outcomes:**							
VARIABLES	Height-for-Age	Weight-for-Age	Mid-Upper Arm Circumference	Anemia, Children	Anemia, Mothers	Child Mortality	Pregnancy	Live Birth	Births in Facilities	Vaccinations (any in past year)	Vaccinations complete	Disease Burden	Diarrheal Prevalence
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Differential effect of Gikuriro at benchmarked cost	-0.043	-0.074	-0.098	0.0022	-0.0075	0.0068	0.025	0.049	-0.039	-0.013	0.035	0.033	-0.024
	(0.049)	(0.050)	(0.072)	(0.028)	(0.022)	(0.0059)	(0.020)	(0.083)	(0.066)	(0.045)	(0.054)	(0.045)	(0.020)
Village receives either Treatment (GD effect)	0.027	0.0076	0.026	0.0019	-0.016	-0.0024	-0.0046	-0.042	0.012	0.13**	0.16***	-0.015	0.031
	(0.050)	(0.041)	(0.069)	(0.028)	(0.028)	(0.0018)	(0.019)	(0.076)	(0.073)	(0.056)	(0.058)	(0.047)	(0.021)
Additional effect of each $100 from GD	0.00073	0.00041	0.00084	-0.00043	-0.00023	-0.000023	-0.00013	0.000071	-0.00070	0.00095	0.0016**	0.00017	-0.000067
	(0.00071)	(0.00065)	(0.0011)	(0.00053)	(0.00031)	(0.000030)	(0.00028)	(0.0014)	(0.00088)	(0.00059)	(0.00077)	(0.00062)	(0.00034)
Endline Control Mean	-1.75	-0.87	-0.61	0.22	0.12	0.0059	0.12	0.70	0.90	0.73	0.48	0.50	0.068
Observations	2618	2594	1981	2372	1581	3373	4137	594	416	1479	1479	3366	3366
R-squared	0.74	0.74	0.57	0.071	0.11	0.019	0.11	0.13	0.16	0.31	0.17	0.064	0.053
ANCOVA	Y	Y	Y	N	N	N	N	N	N	N	N	N	N
Sample	Under 6 Anthro panel			Under 6	Moms	Under 6	Women	Pregnancies	Births	Under 3		Under 6	

Regressions with both baseline and endline outcome measurement are ANCOVA with lagged dependent variables as controls, run on the panel sample. Anemia data was only collected at endline, and regresions on pregnancy outcomes and mortality are endline cross-sectional regressions. Regressions are weighted so as to be representative of all eligible households in study villages. Standard errors are clustered at the village level to reflect the design effect. *** p<0.01, ** p<0.05, * p<0.1

Table 10: Comparison of Lump Sum and Flow Transfers

Panel A: Household-level Outcomes:

	Primary Outcomes:			Secondary Outcomes:							
VARIABLES	Per Capita Monthly Consumption†	Dietary Diversity	Total Household Wealth†	Borrowing†	Saving†	Health Knowledge	Sanitation Practices	Productive Assets†	Consumption Assets†	House Value†	House Quality
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Flow GD Treatment in Small GD arm	0.082	0.048	-0.036	-1.03***	-0.47	0.29	0.055	0.17	0.33	0.036	-0.31
	(0.097)	(0.16)	(0.27)	(0.32)	(0.38)	(0.37)	(0.31)	(0.13)	(0.24)	(0.065)	(0.22)
Additional Impact of Lump Sum in Small	-0.026	0.065	-0.21	0.85*	0.50	-0.041	0.46	0.12	0.47	-0.16**	0.26
	(0.13)	(0.18)	(0.47)	(0.44)	(0.45)	(0.49)	(0.52)	(0.30)	(0.34)	(0.066)	(0.44)
Flow GD Treatment in Large GD arm	0.30***	0.57***	0.12	-0.041	0.55	0.18	0.28	0.75***	0.67**	0.18*	0.21
	(0.10)	(0.14)	(0.38)	(0.40)	(0.41)	(0.55)	(0.28)	(0.13)	(0.32)	(0.093)	(0.21)
Additional Impact of Lump Sum in Large	0.077	-0.19	0.85*	-0.15	-0.021	0.19	-0.089	0.18	1.03**	0.022	0.22
	(0.12)	(0.24)	(0.47)	(0.81)	(0.79)	(0.60)	(0.58)	(0.25)	(0.52)	(0.12)	(0.25)
Control Outcome	10.4	4.16	12.7	5.96	5.18	0.19	-0.23	11.2	8.72	13.6	0.018
Observations	1131	1131	1131	1131	1131	1132	1132	1131	1130	1064	1131
R-squared	0.13	0.19	0.26	0.15	0.17	0.049	0.073	0.29	0.38	0.34	0.11

All regressions are ANCOVA with lagged dependent variables as controls, run on the panel sample. Regressions use only the GD (non-choice) arm and control arm, and analyze the individual-level experiment in which households were randomly assigned to receive Flow transfers (58% of sample), Lump-Sum transfers (29% of sample), or the Choice between the two (13% of sample). We also include a dummy for the village-level experiment in giving Huge GD transfer amounts. The F-test at the bottom of the table compares the outcome in the Choice arm to the average of the outcome in the LS and Flow arms, weighted by the fraction of the choice group that chose each. Regressions weighted so as to be representative of eligible households in study villages. Standard errors are clustered at the village level to reflect the design effect. Outcomes marked with a † are analyzed as inverse hyperbolic sine, meaning that impacts should be interepreted as percent changes in the outcome. *** p<0.01, ** p<0.05, * p<0.1

Panel B: Individual-level Outcomes:

	Primary Outcomes:					Secondary Outcomes:							
VARIABLES	Height-for-Age	Weight-for-Age	Mid-Upper Arm Circumference	Anemia, Children	Anemia, Mothers	Child Mortality	Pregnancy	Live Birth	Births in Facilities	Vaccinations (any in past year)	Vaccinations complete	Disease Burden	Diarrheal Prevalence
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Flow GD Treatment in Small GD arm	-0.018	0.0080	-0.0015	0.027	0.000084	-0.0031	-0.033	0.075	0.084	-0.017	-0.015	-0.028	0.0032
	(0.037)	(0.033)	(0.065)	(0.025)	(0.029)	(0.0064)	(0.032)	(0.071)	(0.051)	(0.032)	(0.034)	(0.029)	(0.016)
Additional Impact of Lump Sum in Small	-0.021	-0.11	-0.20	-0.016	0.23	-0.014*	-0.20***	0	0	0.22	0.25	-0.13	-0.096***
	(0.19)	(0.13)	(0.34)	(0.20)	(0.25)	(0.0074)	(0.046)	(.)	(.)	(0.23)	(0.21)	(0.20)	(0.025)
Flow GD Treatment in Large GD arm	0.088**	0.061	0.16**	0.00021	-0.021	-0.0074*	-0.0040	-0.078	-0.020	-0.00091	0.021	-0.0073	-0.00093
	(0.044)	(0.037)	(0.072)	(0.036)	(0.030)	(0.0040)	(0.027)	(0.078)	(0.078)	(0.036)	(0.036)	(0.033)	(0.015)
Additional Impact of Lump Sum in Large	-0.071	0.0054	-0.40**	-0.091	0.022	-0.0056	0.068	-0.047	-0.79***	-0.10	-0.11	-0.37***	-0.0071
	(0.071)	(0.088)	(0.18)	(0.13)	(0.16)	(0.0064)	(0.088)	(0.18)	(0.15)	(0.091)	(0.091)	(0.11)	(0.035)
Control Outcome	-1.92	-1.06	-0.72	0.22	0.12	0.0056	0.20	0.68	0.84	0.72	0.58	0.54	0.090
Observations	1474	1461	1132	1644	1103	1867	1767	299	205	907	907	1861	1861
R-squared	0.74	0.70	0.52	0.073	0.12	0.018	0.084	0.20	0.26	0.27	0.20	0.060	0.052
ANCOVA	Y	Y	Y	N	N	N	N	N	N	N	N	N	N
Sample		Under 6 Anthro panel		Under 6	Moms	Under 6	Women	Pregnancies	Births	Under 3		Under 6	

Regressions with both baseline and endline outcome measurement are ANCOVA with lagged dependent variables as controls, run on the panel sample. Anemia data was only collected at endline, and regresions on pregnancy outcomes and mortality are endline cross-sectional regressions. Regressions are weighted so as to be representative of all eligible households in study villages. Standard errors are clustered at the village level to reflect the design effect. *** p<0.01, ** p<0.05, * p<0.1

Table 11: Analysis of Choice in Transfer Modality

Panel A: Household-level Outcomes:

	Primary Outcomes:			Secondary Outcomes:							
VARIABLES	Per Capita Monthly Consumption†	Dietary Diversity	Total Household Wealth†	Borrowing†	Saving†	Health Knowledge	Sanitation Practices	Productive Assets†	Consumption Assets†	House Value†	House Quality
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Lump Sum GD Treatment	0.082	0.17	-0.070	-0.20	0.34	0.050	0.39	0.35	0.76***	-0.11*	0.15
	(0.097)	(0.15)	(0.34)	(0.38)	(0.40)	(0.42)	(0.33)	(0.22)	(0.28)	(0.067)	(0.33)
Flow GD Treatment	0.075	0.28**	-0.28	-0.77**	0.37	-0.21	0.099	0.32**	0.0033	-0.037	0.084
	(0.095)	(0.14)	(0.26)	(0.36)	(0.38)	(0.42)	(0.26)	(0.14)	(0.27)	(0.056)	(0.25)
Choice between Lump Sum & Flow GD	-0.044	0.54**	0.39	-1.14	0.96**	-1.24**	0.34	0.64***	0.15	-0.054	0.022
	(0.17)	(0.22)	(0.40)	(0.70)	(0.47)	(0.51)	(0.40)	(0.19)	(0.36)	(0.098)	(0.24)
GiveDirectly Large transfer amount	0.24***	0.27**	0.41	0.39	0.48	0.12	-0.035	0.47***	0.67***	0.24***	0.31
	(0.086)	(0.12)	(0.26)	(0.35)	(0.34)	(0.41)	(0.27)	(0.12)	(0.25)	(0.061)	(0.21)
F-test: Choice = weighted sum of LS & Flow	0.44	0.087	0.29	0.29	0.17	0.016	0.96	0.20	0.21	0.63	0.54
Observations	1221	1221	1221	1221	1221	1222	1222	1221	1220	1148	1221
R-squared	0.14	0.19	0.26	0.14	0.17	0.052	0.070	0.30	0.37	0.35	0.11

All regressions are ANCOVA with lagged dependent variables as controls, run on the panel sample. Regressions use only the GD arm and control arm, and analyze the individual-level experiment in which households were randomly assigned to receive Flow transfers (58% of sample), Lump-Sum transfers (29% of sample), or the Choice between the two (13% of sample). We also include a dummy for the village-level experiment in giving Huge GD transfer amounts. The F-test at the bottom of the table compares the outcome in the Choice arm to the average of the outcome in the LS and Flow arms, weighted by the fraction of the choice group that chose each. Regressions weighted so as to be representative of eligible households in study villages. Standard errors are clustered at the village level to reflect the design effect. Outcomes marked with a † are analyzed as inverse hyperbolic sine, meaning that impacts should be interepreted as percent changes in the outcome. *** p<0.01, ** p<0.05, * p<0.1

Panel B: Individual-level Outcomes:

	Primary Outcomes:					Secondary Outcomes:							
VARIABLES	Height-for-Age	Weight-for-Age	Mid-Upper Arm Circumference	Anemia, Children	Anemia, Mothers	Child Mortality	Pregnancy	Live Birth	Births in Facilities	Vaccinations (any in past year)	Vaccinations complete	Disease Burden	Diarrheal Prevalence
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Lump Sum GD Treatment	-0.053	-0.068	-0.34*	-0.039	0.16	-0.012**	-0.063	0.0017	-0.92***	0.15	0.16	-0.24*	-0.057**
	(0.11)	(0.084)	(0.19)	(0.12)	(0.17)	(0.0058)	(0.074)	(0.21)	(0.079)	(0.21)	(0.20)	(0.13)	(0.029)
Flow GD Treatment	-0.071	-0.15	-0.43	0.062	0.28	-0.012**	-0.011	0.21***	-0.40**	-0.26*	-0.50***	0.034	-0.029
	(0.13)	(0.15)	(0.26)	(0.10)	(0.17)	(0.0054)	(0.077)	(0.074)	(0.16)	(0.13)	(0.12)	(0.13)	(0.037)
Choice between Lump Sum & Flow GD	-0.20	0.015	-0.037	-0.093	-0.19**	-0.016*	-0.11	0.49***	0.64**	0.079	0.095	0.11	-0.075***
	(0.18)	(0.17)	(0.26)	(0.14)	(0.081)	(0.0085)	(0.095)	(0.17)	(0.26)	(0.13)	(0.14)	(0.18)	(0.021)
GiveDirectly Large transfer amount	0.098**	0.061*	0.17**	-0.018	-0.030	-0.0054	0.016	-0.13	-0.0071	0.0083	0.029	0.0046	-0.0012
	(0.039)	(0.032)	(0.068)	(0.036)	(0.024)	(0.0035)	(0.024)	(0.077)	(0.048)	(0.033)	(0.032)	(0.031)	(0.013)
F-test: Choice = weighted sum of LS & Flow	0.43	0.57	0.19	0.63	0.014	0.44	0.59	0.045	0.00000024	0.90	0.66	0.11	0.36
Observations	1483	1470	1141	1654	1107	1877	1774	300	206	910	910	1871	1871
R-squared	0.74	0.70	0.53	0.072	0.13	0.018	0.082	0.20	0.29	0.27	0.21	0.060	0.052
ANCOVA	Y	Y	Y	N	N	N	N	N	N	N	N	N	N
Sample	Under 6 Anthro panel			Under 6	Moms	Under 6	Women	Pregnancies	Births	Under 3		Under 6	

Regressions with both baseline and endline outcome measurement are ANCOVA with lagged dependent variables as controls, run on the panel sample. Anemia data was only collected at endline, and regresions on pregnancy outcomes and mortality are endline cross-sectional regressions. Regressions are weighted so as to be representative of all eligible households in study villages. Standard errors are clustered at the village level to reflect the design effect.

Table 12: Heterogeneity by Baseline Malnutrition

	Anthropometric Outcomes:		
VARIABLES	Height-for-Age	Weight-for-Age	Mid-Upper Arm Circumference
	(1)	(2)	(3)
Baseline Outcome * Gikuriro Treatment	-0.041	-0.035	0.085
	(0.044)	(0.062)	(0.056)
Baseline Outcome * GiveDirectly Treatment	-0.025	-0.066	0.077
	(0.046)	(0.044)	(0.065)
Baseline Outcome * GiveDirectly Large transfer	0.023	0.007	0.081
	(0.043)	(0.046)	(0.060)
Gikuriro Treatment	0.043	0.032	0.025
	(0.043)	(0.036)	(0.056)
GiveDirectly Treatment	-0.025	0.002	-0.005
	(0.040)	(0.036)	(0.065)
GiveDirectly Large transfer	0.093*	0.063	0.135*
	(0.052)	(0.039)	(0.079)
Baseline Outcome	0.768***	0.749***	0.600***
	(0.034)	(0.035)	(0.043)
Baseline Control Mean	-1.92	-1.06	-0.72
Observations	2125	2104	1629
R-squared	0.696	0.673	0.507
Sample	Anthro panel under 6 at endline		

Regressions with both baseline and endline outcome measurement are ANCOVA with lagged dependent variables as controls, run on the panel sample. Regressions include fixed effects for the randomization blocks, and are weighted to be representative of all households in study villages. Anthropometric outcomes are demeaned prior to interaction so that the uninteracted treatment terms provide impact at average level of baseline anthro measure.

*** p<0.01, ** p<0.05, * p<0.1

Table 13: Heterogeneity by Child Age

	Interaction with 'First 1000 Days' dummy			Interaction with 'Newborn' dummy		
VARIABLES	Height-for-Age	Weight-for-Age	Mid-Upper Arm Circumference	Height-for-Age	Weight-for-Age	Mid-Upper Arm Circumference
	(1)	(2)	(3)	(4)	(5)	(6)
Indicator * Gikuriro Treatment	-0.014	-0.026	0.112	0.595	0.249	0.28
	(0.138)	(0.113)	(0.109)	(0.644)	(0.505)	(0.489)
Indicator * GiveDirectly Treatment	-0.307**	-0.158	0.156	0.38	0.592	0.666
	(0.138)	(0.115)	(0.104)	(0.525)	(0.498)	(0.506)
Indicator * GiveDirectly Large transfer	-0.125	-0.025	0.157	0.408	0.73	0.307
	(0.139)	(0.123)	(0.145)	(0.399)	(0.472)	(0.280)
Gikuriro Treatment	0.012	0.013	-0.08	0.004	0.002	-0.027
	(0.106)	(0.080)	(0.082)	(0.083)	(0.063)	(0.069)
GiveDirectly Treatment	0.117	0.08	-0.107	-0.018	0.009	-0.035
	(0.119)	(0.083)	(0.091)	(0.099)	(0.068)	(0.073)
GiveDirectly Large transfer	0.248**	0.194**	0.076	0.193**	0.183***	0.156**
	(0.105)	(0.081)	(0.111)	(0.085)	(0.066)	(0.078)
Indicator uninteracted	0.147	0.13	-0.007	-0.01	0.023	0.178
	(0.149)	(0.118)	(0.141)	(0.255)	(0.298)	(0.270)
Baseline Control Mean	-1.92	-1.06	-0.72	-1.92	-1.06	-0.72
Observations	2360	2347	2020	2360	2347	2020
R-squared	0.071	0.035	0.072	0.068	0.035	0.073
Sample	Kids under 6 at endline					

Regressions are endline cross-sections, run on the panel sample, and do not include the lagged outcome variable so as to be able to consider children who are newborns in R2. Regressions include fixed effects for the randomization blocks, and are weighted to be representative of eligible households in study villages. *** p<0.01, ** p<0.05, * p<0.1

Figures

Figure 1: Actual and Assigned Treatment Amounts

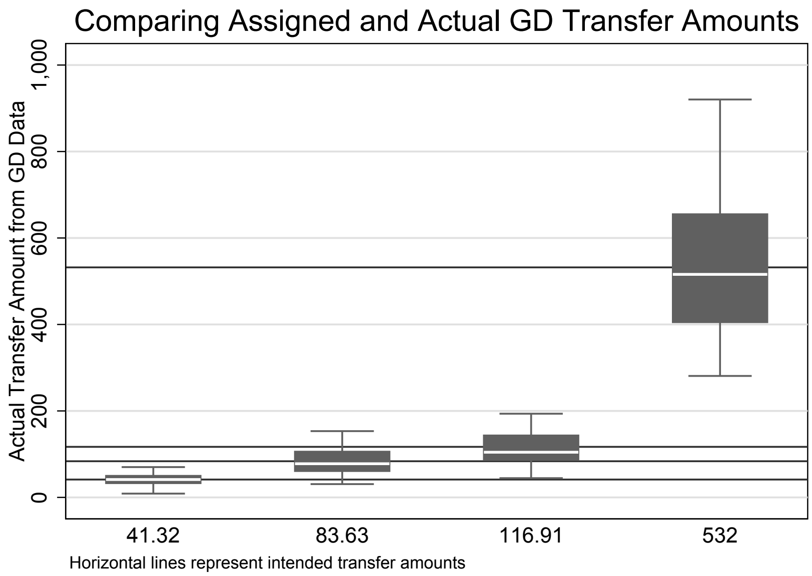


Figure 2: Plotting Outcomes by Program Cost

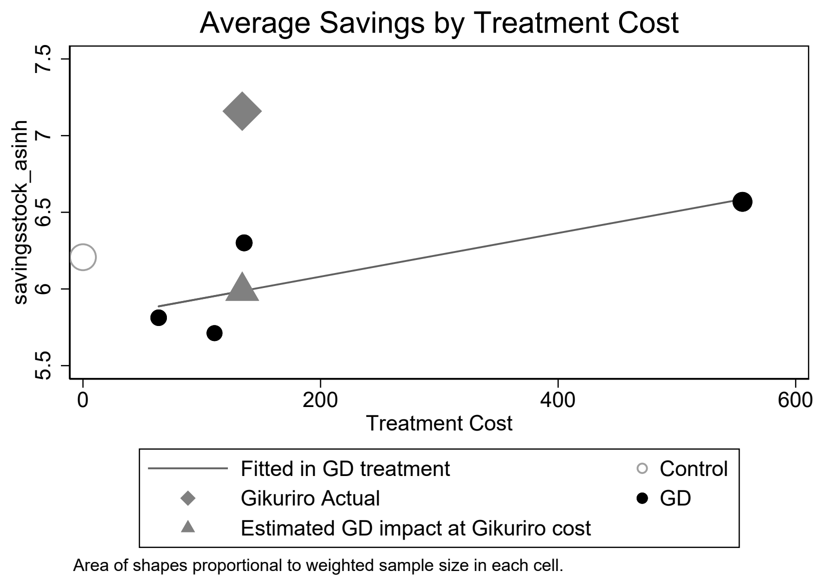


Figure 3: Fan Regression Impacts by Age

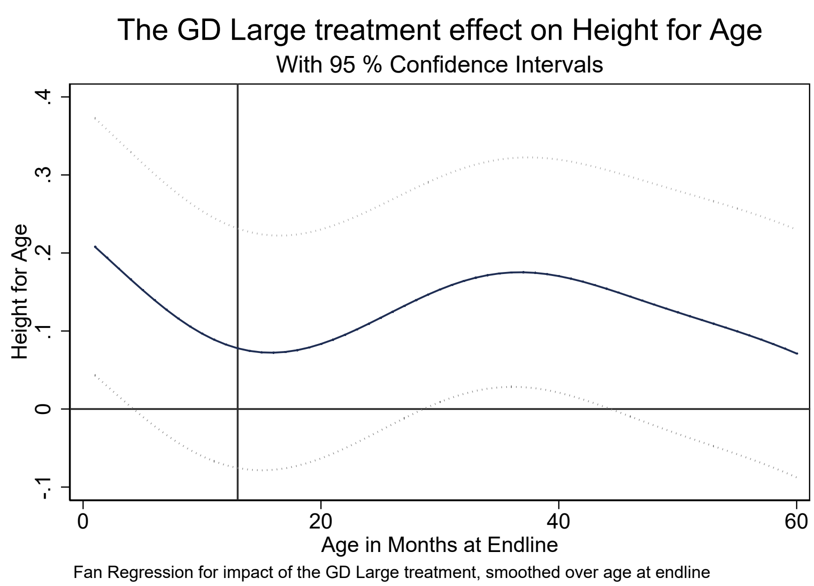
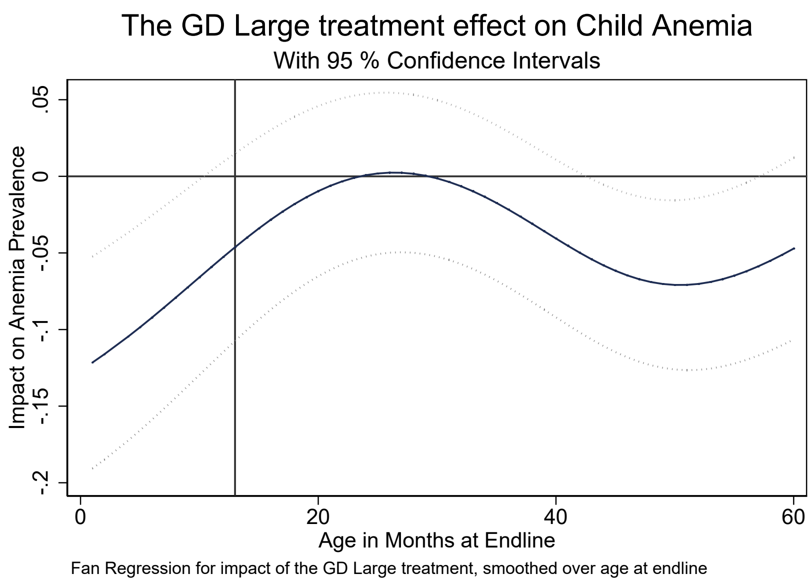


Figure 4: Fan Regression Impacts by Age



Appendix A GiveDirectly Nutritional Handout

GDID: ________________________ **PAYMENT METHOD:** ______________________

RECIPIENT INFORMATION REGARDING NUTRITION AND HYGIENE

GiveDirectly's program is supported by made possible by the generous support of the American people through USAID. The information below is approved by the Rwanda Ministry of Health.

- **Infant Nutrition**
  - Infants less than 6 months old should be fed by breast only. During this period an infant receives only breast milk and no other liquids or solids, not even water, unless medically indicated. A non-breastfed baby is 14 times more likely to die than an exclusively breastfed baby in the first 6 months.
  - Infants 6 to 24 months old should continue to be fed by breast, but should also receive complementary feeding that includes animal-source foods (meats, fish, milk products, eggs) and fruits and vegetables that are rich in vitamin A (such as mango, papaya, oranges, yellow sweet potato and carrots). Guidelines are for kids 6-24 months to eat at least 4 food groups: fruits, vegetables and legumes, grains, meats, dairy.
    - Infants 6 to 8 months old should be fed complementary foods 2-3 times daily;
    - Infants 9 to 24 months old should be fed complementary foods 3-4 times daily, plus 1-2 snacks.
- **Reducing Illness**
  - If you or your children get diarrhoea, use *Oral Rehydration Salts* (ORS) to replace the nutrients being lost. Typical symptoms of diarrhoea include frequent, loose, watery stools, abdominal cramps, and/or abdominal pain. If ORS is not available, a simple solution can be prepared for drinking by mixing one liter of clean drinking water and mix it with ½ teaspoon of salt and 6 teaspoons of sugar.
  - The government has a 6-monthly deworming program and Vitamin A supplementation program. Ask your Community Health Worker for more information.
- **Dietary Diversity**
  - Anemia
    - Anemia is a health condition, commonly caused by nutritional deficiency of iron and other nutrients (folate or vitamin B12). Around 72% of 6-8 months-olds in Rwanda have Anemia. Anemia can be an underlying cause for maternal death and prenatal and perinatal infant loss. Anemia among children is associated with low mental performance and physical development.
    - Examples of iron-rich food: fish, meat, milk products, oranges, lemons, grapefruits, guavas, papayas, and green leafy vegetables. Breast milk for your child is an important source of iron, too.
  - Here are some other examples of food you can produce/buy/eat to cheaply increase nutrition:

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        - Breeding small, inexpensive animals such as hens, rabbits and guinea pigs can provide you and your children with important body building protein and other important nutrients.
        - Grow kitchen gardens if you have time. You can grow different vegetables for your family throughout the year, like amaranths, carrots, and dark-green leaves such as spinach and dodo, all of which are important sources of body protecting nutrients.
        - Consume soya beans, yogurt, avocados and dodo (which you could grow)
        - Eat orange-flesh rather than white-flesh sweet potatoes
- **Hygiene**
    - Handwashing with soap or wood ash can kill bacteria/viruses and prevents the spread of disease. Handwashing with soap at critical times is estimated to reduce diarrhoea by 47%. The most important times that hands should be washed with soap and water are:
        - After defecating
        - After cleaning a child who has defecated
        - Before eating or handling food
    - Recommended practices for personal hygiene further include:
        - Washing hair every week with shampoo
        - Washing the face every day after sleeping
        - Brushing teeth twice every day, in the morning and the night after eating
    - Safe disposal of waste means defecating into a latrine, disposing into a latrine, or burial. Inappropriate disposal of human feces, such as open defecation, facilitates the transmission of pathogens and disease.
- **Birth preparedness for delivery**
    - Early initiation of antenatal care (ANC) can reduce common maternal complications and maternal and perinatal mortality. Visit your nearest health facility early during pregnancy for medical tests and more information. The World Health Organization promotes four antenatal clinic visits, one in each trimester, during each pregnancy.

Appendix B Selection of control variables

In our pre-analysis plan, we state that control variables for the primary specification "will be selected on the basis of their ability to predict the primary outcomes". In doing so, we seek to build on recent developments that balance the challenge of using baseline data to select variables that will reduce residual variance in equation (1) with the danger that researcher freedom in the selection of control variables can lead to p-hacking, in which right-hand-side variables are selected specifically on the basis of the statistical significance of the coefficient of interest (Card and Krueger, 1995; Casey et al., 2012), thereby invalidating inference.

To balance these concerns, we follow the *post-double-selection* approach set forth in Belloni et al. (2014b). Those authors advocate a two-step procedure in which, first, Lasso is used to automate the selection of control variables, and second, the post-Lasso estimator (Belloni et al., 2012) is used to estimate the coefficients of primary interest in Equation (1), effectively using Lasso as a model selection device but *not* imposing the shrunken coefficients that results from the Lasso estimates directly. Belloni et al. (2014b) demonstrate that this approach not only reduces bias in estimated treatment effects better than alternative approaches—less a concern given the successful randomization in our experiment—but that it may improve power while retaining uniformly valid inference.

In the first stage, model selection is undertaken by retaining control variables from the union of those chosen either as predictive of the treatment assignment or of the outcome. This model selection stage can be undertaken after residualizing to account for a set of control variables that the authors have a priori determined belonw in the model, as in Belloni et al. (2014a); in our case, we retain block fixed effects, lagged values of the outcome, and lagged values of (the inverse hyperbolic sine of) household wealth in all specifications, per our pre-analysis plan. We modify the heteroskedasticity-robust Lasso estimator of Belloni et al. (2012) to incorporate sampling weights consistent with our design, using the Lasso penalty is chosen as a function of the sample size and the number of potential covariates, as in Belloni et al. (2014a).

Resulting covariates selected for each of the primary and secondary outcomes, at household and individual level, are presented in Tables B.1 and B.2, respectively.

Table B.1: Covariates selected in Belloni et al. (2014) post-double-lasso selection procedure for household outcomes

Outcome	Control set
consumption_asinh	Baseline value of consumption_asinh, present in both rounds
	L.Lhh_wealth_asinh
	L.Fraction of village defined eligible by IPA
Household dietary diversity score	Baseline value of dietarydiversity, present in both rounds
	L.Lhh_wealth_asinh
	L.Fraction of village defined eligible by IPA
	Lsavingsstock_asinh3
	Lconsumpti_x_Ldietarydi
	Lconsumpti_x_Lproductiv
	Ldietarydi_x_Lassetscon
wealth_asinh	Baseline value of wealth_asinh, present in both rounds
	L.Lhh_wealth_asinh
	L.Fraction of village defined eligible by IPA
	L.Own dwelling
borrowingstock_asinh	Baseline value of borrowingstock_asinh, present in both rounds

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Table B.1 (continued)

Outcome	Control set
	L.Lhh_wealth_asinh
	L.Fraction of village defined eligible by IPA
savingsstock_asinh	Baseline value of savingsstock_asinh, present in both rounds
	L.Lhh_wealth_asinh
	L.Fraction of village defined eligible by IPA
	Lconsumpti_x_Lproductiv
	Lconsumpti_x_Lassetscon
Health Knowledge Index	Baseline value of health_knowledge, present in both rounds
	L.Lhh_wealth_asinh
	L.Fraction of village defined eligible by IPA
Sanitation Practices Index	Baseline value of sanitation_practices, present in both rounds
	L.Lhh_wealth_asinh
	L.Fraction of village defined eligible by IPA
	Lproductiv_x_Lassetscon
productiveassets_asinh	Baseline value of productiveassets_asinh, present in both rounds
	L.Lhh_wealth_asinh
	L.Fraction of village defined eligible by IPA
	Lconsumpti_x_Lassetscon
assetsconsumption_asinh	Baseline value of assetsconsumption_asinh, present in both rounds
	L.Lhh_wealth_asinh
	L.Fraction of village defined eligible by IPA
	L.Number of rooms
	L.Durables expenditure (12-month recall)
	Ldietarydi_x_Lassetscon
	Lproductiv_x_Lassetscon
selfcostdwell_asinh	Baseline value of selfcostdwell_asinh, present in both rounds
	L.Lhh_wealth_asinh
	L.Fraction of village defined eligible by IPA
	L.Number of rooms
	L.Durables expenditure (12-month recall)
Housing Quality Index	Baseline value of housing_quality, present in both rounds
	L.Lhh_wealth_asinh
	L.Fraction of village defined eligible by IPA
	L.Number of rooms

Note: block fixed effects and lag of the relevant outcome included in all specifications. Specifications that include both eligible and ineligible households include an indicator for eligibility status.

Table B.2: Covariates selected in Belloni et al. (2014) post-double-lasso selection procedure for individual outcomes

Outcome	Sample	Control set
haz06, Winsorized fraction .005, high only	Under 5s	L.haz06, Winsorized fraction .005, high only
		female
		agemonths
		agemonths_sq
		agemonths_cu
		L.Lhh_wealth_asinh
		L.Food expenditure (weekly recall)
		L.Food consumption-value own production (weekly recall)
		L.waz06, Winsorized fraction .005, high only
		Lconsumpti_x_Lselfcostd
waz06, Winsorized fraction .005, high only	Under 5s	L.waz06, Winsorized fraction .005, high only
		female
		agemonths
		agemonths_sq
		agemonths_cu
		L.Lhh_wealth_asinh
		L.Food expenditure (weekly recall)
		L.Food consumption-value own production (weekly recall)
		Lconsumpti_x_Lproductiv
muacz, Winsorized fraction .01	Under 5s	L.muacz, Winsorized fraction .01
		female
		agemonths
		agemonths_sq
		agemonths_cu
		L.Lhh_wealth_asinh
		L.waz06, Winsorized fraction .005, high only
		Lconsumpti_x_Lproductiv
anemia_dummy	Under 5s	female
		agemonths
		agemonths_sq
		agemonths_cu
		L.Lhh_wealth_asinh
anemia_dummy	Pregnant/lactating women	agemonths
		agemonths_sq
		agemonths_cu
		L.Lhh_wealth_asinh
mortality	All	female
		agemonths
		agemonths_sq
		agemonths_cu
		L.Lhh_wealth_asinh
Was this women pregnant at any point in the past 12 months	Pregnant/lactating women	agemonths
		agemonths_sq
		agemonths_cu
		L.Lhh_wealth_asinh
		L.Lwealth_asinh

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Table B.2 (continued)

Outcome	Sample	Control set
Did pregnancy conclude in live birth	Pregnant/lactating women	agemonths
		agemonths_sq
		agemonths_cu
		L.Lhh_wealth_asinh
		L.Food expenditure (weekly recall)
		L.Food consumption-value own production (weekly recall)
		Lconsumpti_x_Lwealth_as
facility_birth	Pregnant/lactating women	agemonths
		agemonths_sq
		agemonths_cu
		L.Lhh_wealth_asinh
anthro_vacc_year	Under 3s	female
		agemonths
		agemonths_sq
		agemonths_cu
		L.Lhh_wealth_asinh
		Lconsumpti_x_Lproductiv
anthro_vacc_complete	Under 3s	female
		agemonths
		agemonths_sq
		agemonths_cu
		L.Lhh_wealth_asinh
Any fever, diarrhea, or coughing blood at individual/round level	Under 5s	female
		agemonths
		agemonths_sq
		agemonths_cu
		L.Lhh_wealth_asinh
		L.Food consumption-value own production (weekly recall)
Individual reported with diarrhea/vomiting/fever now	Under 5s	female
		agemonths
		agemonths_sq
		agemonths_cu
		L.Lhh_wealth_asinh

Note: block fixed effects and lag of the relevant outcome included in all specifications. Specifications that include both eligible and ineligible households include an indicator for eligibility status.