

## The Long-Term Impacts of Grants on Poverty: Nine-Year Evidence from Uganda’s Youth Opportunities Program<sup>†</sup>

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*In 2008, Uganda gave \$400 per person to thousands of young people to help them start skilled trades, work more, and raise incomes. Four years on, an experimental evaluation found grants raised work by 17 percent and earnings by 38 percent (Blattman, Fiala, Martinez 2014). After nine years, we find these gains have dissipated. Grantees’ investment leveled off; controls eventually increased their incomes through business and casual labor; and so both groups converged in employment, earnings, and consumption levels. We see little effect on mortality, fertility, or family health and education. However, grants had lasting impacts on durable asset stocks and skilled work. (JEL H53, I32, I38, O15, O22)*

What is the effect of one-time grants to the poor? Governments and nonprofits commonly give cash, livestock, or equipment to poor people who propose starting basic businesses. These programs vary in what they grant, to whom, and with what conditions or other services. Nonetheless, similar assumptions underlie most such programs: that the poor have high returns to capital but face frictions and constraints on their ability to borrow, save, or mitigate risk (Banerjee and Duflo 2011, Cho and Honorati 2014, Blattman and Ralston 2015). If this is true, then a one-time grant of capital may help the poor overcome financial imperfections, start microenterprises, and raise their incomes.

Broadly speaking, evaluations one to four years after such programs show that recipients raised their incomes compared to randomized control groups (de Mel, McKenzie, and Woodruff 2012; Banerjee et al. 2015; Blattman et al. 2016; see

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online Appendix A). Results vary, and some programs show no impact at all. But on the whole, these short- and medium-term results have bolstered the view that poverty, combined with start-up costs and imperfect financial markets, holds many poor people below their potential.

This includes the Youth Opportunities Program (YOP) in Uganda. YOP is emblematic of group-based employment programs supported by the World Bank and other organizations. YOP gave one-time cash grants to small groups that had proposed to set their members up as independent craftspeople. Uganda's government and the World Bank wanted to reduce underemployment and help young people move up the job ladder. But their primary aim was to help these mostly rural youth raise their earnings and climb out of poverty.

In an earlier two- and four-year follow-up, we found that YOP dramatically raised skilled work, work hours, incomes, consumption, and durable assets (Blattman, Fiala, and Martinez 2014). Though we could not test mechanisms directly, the results were consistent with the theory of imperfect financial markets and initial start-up costs that underlay the whole program. This paper reassesses impacts and implications nine years after the grants. Long-run follow-ups of field experiments are rare, but as we will see, they can have important theoretical and policy implications.

Some journalists and policymakers have looked at the body of short- and medium-term evidence and concluded that grants of cash and other capital can offer people a lift out of poverty. Theory, however, suggests we should be more cautious. When poor people face financial market imperfections, one-time grants could have either temporary or lasting effects. It largely depends on how serious the constraints and frictions that hold people back are, as well as on the returns to other labor market opportunities. For example, if credit-constrained poor people increase their work hours, or have the means to save and invest some portion of their earnings over time, eventually they will be able to start their own microenterprises and will converge to the same level of income and investment as a grant recipient. This is an intuitive point and one consistent with simple models of occupational choice and investment (Buera, Kaboski, and Shin 2014, 2015).<sup>1</sup> Nonetheless, it is a point most of the literature overlooks, including our earlier paper.

As a result, a key question long-run program evaluations can answer is, "how long before the control group converges?" Convergence will slow down if wage work is scarce or poorly paid, if saving is costly, if there are especially high start-up costs, if people are impatient or myopic, or if there are other market failures or social constraints that limit people's earnings and investment (Buera, Kaboski, and Shin 2014, 2015; Blattman, Fiala, and Martinez 2014). Convergence will be especially slow if the returns to capital are such that an early head start confers a sustained advantage. With extreme enough frictions or constraints, it is conceivable that poor people stagnate without grants—a kind of poverty trap.<sup>2</sup>

Overall, we find little sustained effect on work hours or income flows after nine years, largely because the control group eventually found other kinds of work with

<sup>1</sup>In Blattman, Fiala, and Martinez (2014) we used a Ramsey model of investment with occupational choice to predict effects of YOP.

<sup>2</sup>The empirical development literature has failed to find many instances of household-level poverty traps (Banerjee and Duflo 2011, Kraay and McKenzie 2014).

similar levels of productivity and earnings. However, YOP did have a lasting effect on people's asset stocks and moved a subset permanently up the job ladder into skilled trades. We also find limited evidence of long-run benefits on people's own human capital or that of their children.

With YOP, the Government of Uganda invited groups of about 20 poor young people from small towns and villages to submit grant applications for roughly \$400 per person—an amount roughly similar to their annual incomes. Groups applied to use the grants to hire trainers, buy tools and materials, and set themselves up as individual tradespeople. If selected, they received a one-time grant of roughly \$8,000 into a group bank account, free of further supervision or follow-up.

We worked with the government to set up YOP as a randomized trial, with more than 12,000 people in 535 eligible groups, 265 of which were funded. We attempted to follow a random subsample of five people in all 535 groups after two, four, and (in this paper) nine years. These nine years were a period of modest but steady per capita income growth in Uganda, one where the program region broadly began to converge to the rest of the country.

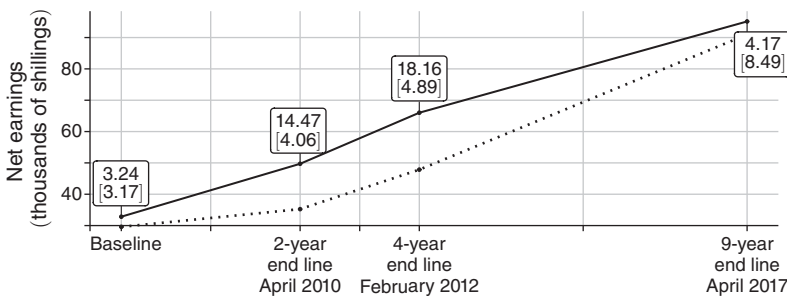
Nine years after the grants were delivered, we see convergence in the income and employment levels of the treatment and control groups. Figure 1 shows the progression of three income measures over time. We plot control group incomes as well as the added impact of treatment at baseline and each end line. Figure 2 does the same for three measures of employment levels and occupational choice. The figures indicate some baseline imbalance in earnings and assets, and our treatment effects account for baseline covariates.

Four years after the grants, YOP recipients reported 17 percent greater work hours, 38 percent greater earnings, and 11 percent greater consumption than controls. After nine years, work hours are nearly identical, earnings are less than 5 percent greater, and consumption is less than 2 percent greater than controls. None of these differences in income flows or employment levels are statistically significant.

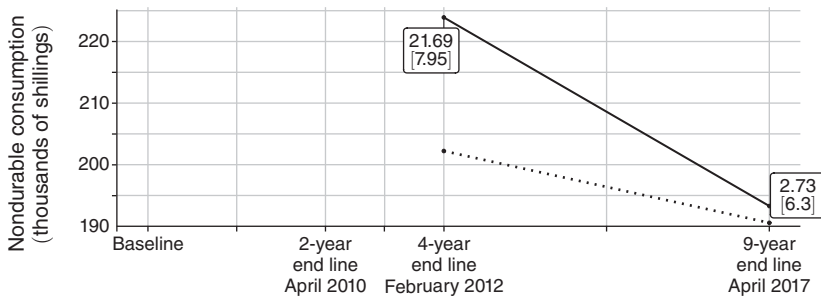
We do, however, see two sustained effects. First, the fraction of people who are able to find full-time work in a skilled trade increases from about 3 to 6 percent of the sample. Even if we do not see evidence of higher earnings or well-being in this group, YOP could have important positive externalities on the local economy through the growth of a skilled sector. Second, we see sustained effects on durable assets, totaling more than a 0.14 standard deviation increase. Because of the baseline imbalance in asset stocks, these asset impacts are somewhat fragile. Nonetheless, it seems plausible that YOP's temporary effect on earnings would be smoothed through a rise in durable assets. This is an important form of consumption and precautionary savings, and so YOP arguably had some long-term effect on poverty and insurance against shocks.

Our interpretation is that, against a backdrop of economic stability and moderate growth, YOP start-up grants helped youth with capital to test their skills and luck in microentrepreneurship and accelerated the pace at which underemployed young people could reach their long-run income and employment levels. It also influenced their occupational choice. In the absence of this start-up capital, however, control group members eventually found other equally profitable sources of work, especially wage labor. They also saved and accumulated enterprise capital. As a result, control earnings and consumption converged to the treatment group over time. These results are consistent with recent long-run evidence from Ethiopia. Blattman, Dercon, and

Panel A. Net monthly earnings: control means and intent to treat (ITT)



Panel B. Nondurable consumption: control means and ITT



Panel C. Index of durable assets: control means and ITT

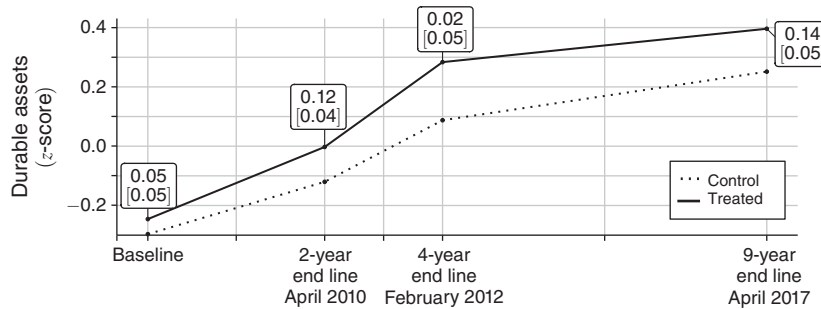


FIGURE 1. PROGRESSION OF EARNINGS, CONSUMPTION, AND ASSETS ACROSS TIME

Notes: For each outcome, we plot the mean value for the control group as well as the sum of the control mean and the ITT estimate of program impact. Each figure examines an alternative measure of self-reported income over time (when available): panel A—net monthly earnings (the sum of wages and business profits); panel B—nondurable consumption (from an abbreviated consumption survey module); and panel C—a normalized index of durable assets, mainly home quality and household furnishings and items. Earnings and consumption are in thousands of 2008 Ugandan shillings. The market exchange rate in 2008 was 1,720 shillings to \$1, and the PPP exchange rate is 862 shillings to \$1. See online Appendix B.5 for additional measurement details. Not all outcomes were measured in the briefer baseline and two-year surveys.

Franklin (2018) found that cash grants of \$300 plus basic business consulting raised incomes by a third in the first year but that employment and earnings converged within five years.<sup>3</sup>

<sup>3</sup>Another example in Bangladesh is more ambiguous (Bandiera et al. 2017).

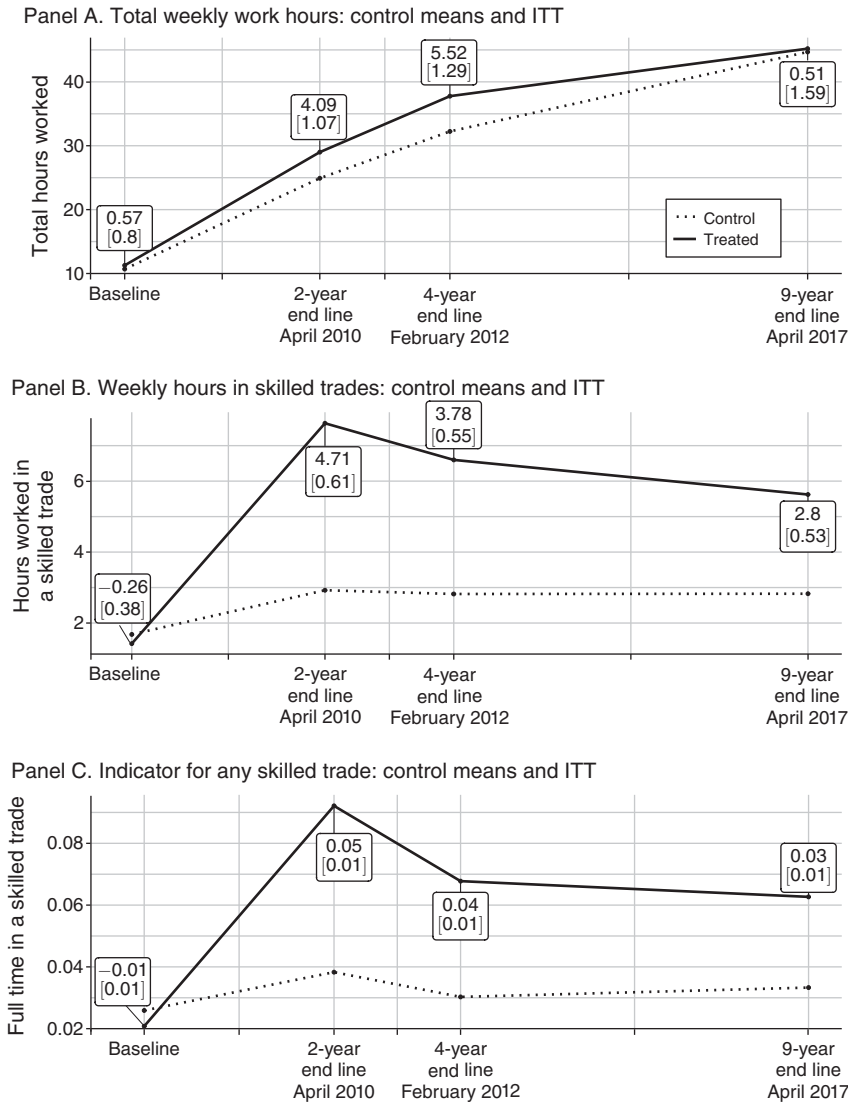


FIGURE 2. PROGRESSION OF OCCUPATIONAL CHOICE ACROSS TIME

Notes: For each outcome, we plot the mean value for the control group as well as the sum of the control mean and the ITT estimate of program impact. We asked respondents their average weekly work hours in the previous month for more than 30 income-generating activities. Each figure examines an alternative measure of self-reported employment and occupation: panel A—total average work hours per week, summing over all activities; panel B—total average hours per week in skilled trades only (mainly those trades that YOP encouraged, such as carpentry); and panel C—an indicator for primarily working in a skilled trade, which we define as at least 30 hours per week in the past month. See online Appendix B.5 for additional measurement details.

Long-run results also speak to another important empirical question: whether higher incomes have positive effects within the household over time, such as increasing children’s health and education. When we examine investments in health or education, however, we see relatively limited impacts on the YOP recipients or their children.

## I. Intervention and Experimental Design

Uganda, a small landlocked country in East Africa, is poor but has had a stable and growing economy. By the mid-2000s, however, most of that growth had been concentrated in the country's capital and southwest due to distance from trade routes, low public investment, and moderate insecurity: a low-level insurgency destabilized districts in North-Central Uganda; instability in neighboring Sudan; and banditry in Uganda's northeast. By 2006, peace came to Uganda and its neighbors, and the government increased efforts to develop the North. There is no subnational growth data, but we speculate that real per capita incomes grew 1–4 percent per year over the 9-year period of this study.

The government's northern development strategy focused on one-time capital injections, in part because of an almost complete lack of formal and informal credit. The Northern Uganda Social Action Fund (NUSAF) was Uganda's second largest development program. Communities and groups could apply for cash grants for three purposes: community infrastructure construction; livestock; or YOP, for skilled-employment generation.

NUSAF and YOP are examples of the community-driven development (CDD) approach. In 2018, the World Bank reported 199 active CDD projects in 78 countries totaling \$19.7 billion (World Bank 2018). YOP-like employment programs that transfer cash for enterprise start-up are common components of CDD programs.

### A. Intervention

YOP invited groups of young adults aged roughly 16 to 35 to apply for cash grants in order to start a skilled trade such as metal fabrication, carpentry, or tailoring. YOP was targeted to poor, unemployed young people with at least some education and prospects for starting a skilled trade, not the "ultra poor."

People applied to YOP in groups. Group disbursements were mainly an administrative convenience. Groups ranged from 10 to 40 people, averaging 22, mostly from the same parish (a collection of villages). Half the groups existed already as farm cooperatives, or sports or microfinance clubs. Most groups mixed genders, with about one-third female.

Groups submitted written proposals. Group members typically proposed to set themselves up as independent businesses, though sometimes they shared expensive tools. Groups could request up to \$10,000 for training and asset purchases. They identified their own trainers, typically a local artisan or small institute under ten kilometers away.

Several levels of government bureaucracy screened and selected proposals: the village, county, and district, then finally the national NUSAF office. Districts said they prioritized early applications and disqualified incomplete ones, but unobserved quality and political calculation could have played a role.

Successful proposals received a lump sum bank transfer in the names of the management committee, with no monitoring thereafter. The average grant was 12.9 million shillings, or \$7,497 (all figures in the paper are quoted in 2008 Ugandan shillings and US dollars, and conversions use market exchange

rates).<sup>4</sup> Per capita grant size varied across groups, but 80 percent were between \$200 and \$600 per member, averaging \$382. Online Appendix B.1 describes intervention details.

### *B. Experimental Sample and Design*

YOP was vastly oversubscribed. In 2008, two years after YOP began, the government had funds remaining for 265 proposals. They screened and selected 535 eligible proposals, containing nearly 12,000 group members from 14 districts. We randomly assigned 265 groups to YOP, stratified by district. There were no funds remaining for the four most war-affected districts, so there is no direct “postconflict” subsample. Spillovers between study villages were unlikely, as the 535 groups were spread across 454 communities in a population of more than five million.

There is balance across a wide range of measures, but a handful show imbalance. They suggest higher levels of initial wealth among the treatment group. Our estimation strategy and robustness tests will test for sensitivity to this mild imbalance. Online Appendix B.2 describes sample selection and balance tests.

YOP group members were 25 years old on average and mostly rural, poor, credit constrained, and underemployed. Less than a quarter lived in a town, and most lived in villages of 100–2,000 households. A quarter did not finish primary school, but on average they reached eighth grade.

At baseline they reported 11 hours of work a week, half in low-skill labor and half in rudimentary agriculture. Half had zero employment in the past month. Cash earnings in the past month averaged a dollar a day. Savings were \$15 on average.

Although poor by any measure, these applicants were slightly wealthier and more educated than their peers. If we compare our sample to their age group and gender from a 2008 population-based household survey, our sample has on average 1.7 years more education and 0.15 standard deviations more wealth.

### *C. Data and Attrition*

We randomly sampled 5 people per group (2,677 people) for a baseline survey and attempted to track this sample over time. We conducted the baseline post-randomization due to government funding delays, and three groups (3 percent of the sample) could not be surveyed.

The government disbursed grants between July and September 2008. The two-year end line was conducted between August 2010 and March 2011, 24 to 30 months after disbursement; a four-year survey was conducted between April and June 2012, 44 to 47 months after disbursement; a nine-year end line was conducted between March and May 2017, 103 to 106 months after disbursement.

YOP applicants were young and mobile. While we found 97 percent of the experimental sample at baseline, up to 40 percent of respondents had moved or were away temporarily at each end line survey. To minimize selective attrition, we tracked in two phases. For example, in phase one of the nine-year survey we tracked and found

<sup>4</sup>The market exchange rate in 2008 was 1,720 shillings to \$1, and the purchasing power parity exchange rate was 862 shillings to \$1.

71 percent of the original sample. In phase two, we randomly sampled 36 percent of the unfound and invested heavily in tracking, finding 43 percent. Consequently, we interviewed 74 percent of the baseline sample. However, because we randomly sampled the difficult-to-find migrants, we can give them greater weight in ITT estimation using inverse sampling weights. This approach provides a reweighted “effective response rate” of 86 percent after 2 years, 82 percent after 4, and 87 percent after 9. See online Appendix B.3 for further details.

Attrition is slightly higher among controls, in part due to the fact that many of the unfound baseline groups were in the control group and never tracked (see online Appendix B.3). If these “never found” controls had particularly high potential outcomes, we would overstate the impact of the intervention. Our estimation strategy below corrects for attrition, and robustness tests demonstrate that our conclusions are not affected by relatively extreme attrition scenarios.

Overall, our attrition levels are similar to other panels of young adults in Africa (Baird et al. 2015, Friedman et al. 2011, Baird et al. 2016), though higher than panels of existing entrepreneurs, who are typically urban, less mobile, and screened for attrition before the experiment (de Mel, McKenzie, and Woodruff 2012).

#### D. Estimation

We follow the prior empirical strategy and estimate program impacts by calculating ITT estimates via the weighted least squares regression:

$$Y_{ij} = \beta_{ITT}T_{ij} + \gamma X_i + \alpha_d + \epsilon_{ij},$$

where  $Y$  denotes the outcome for person  $i$  in group  $j$ ,  $T$  is an indicator for assignment to treatment,  $X$  is the set of baseline covariates (the same as previous and listed in online Appendix B.2),  $\alpha$  are district fixed effects (required because the probability of assignment to treatment varies by strata), and  $\epsilon$  is an individual error term clustered by group. We weight observations by their inverse probability of selection into end line tracking and attrition probability. Several continuous outcomes have a long upper tail, and we top-code these at the ninety-ninth percentile.

As there is no administrative data on earnings or consumption in Uganda, all outcomes are based on survey self-reports. We will overestimate the impact if the treatment group overreports well-being due to social desirability bias or if the controls underreport outcomes in the hope it will increase their chance of future help. This is unlikely, partly because misreporting would have to be highly systematic across hundreds of questions and activities. Also, we do not observe treatment effects across many socially desirable measures. Misreporting would have to be confined to economic outcomes alone, in early years only, to bias our results.

Note that 11 percent of treatment groups did not receive YOP. Of the 265 treatment groups, 21 did not receive the grant due to administrative problems, and a further 8 said their funds were stolen. Our ITT estimates do not account for this treatment noncompliance. A complier average causal effect, using assignment to treatment as an instrument for receiving the grant, would increase all program impact estimates by a factor of roughly 1.1.



## II. Results

### A. Economic Impacts

*Initial Treatment Compliance and Investments.*—Most groups chose to invest their grants in business capital and materials rather than training. Panel A of Table 1 reports investments in the first two years, including control group means and ITT estimates. Two years after the grants, 68 percent of the treatment group had enrolled in vocational training compared to 15 percent of the control group. Treatment translated into 340 more hours (equal to 8.5 full-time weeks) of vocational training than controls. Among those who enrolled in any training, 38 percent trained in tailoring, 23 percent in carpentry, 13 percent in metalwork, 8 percent in hairstyling, and the remainder in miscellaneous other trades (Blattman, Fiala, and Martinez 2014). Skills training, however, was a minority of expenditures. Two years after the grant, our median group estimated that they spent just 11 percent on skills training compared to 52 percent on tools, 13 percent on materials, and 24 percent on other expenses.

How did group grants shape treatment compliance and initial behavior? It is difficult to say, but we collected some survey data and conducted qualitative interviews. First, despite the group structure, most people in the sample appear to have started individual trades. Generally speaking, these were not cooperatives or joint businesses. Second, most groups elected small committees to handle procurement, made their training and tool purchases in bulk, and then shared the training, tools, and materials among the group members. This probably ensured that the original investment plans were followed, but it did not necessarily affect the fact that most people set themselves up as individual enterprises. Most of the bulk-purchased tools and materials were distributed to individuals. About 90 percent of treated group members also said they felt the training and tools were equally shared. Even so, about half of respondents said they shared some tools with other group members in the first two years—usually more expensive tools that they could not afford on their own (such as a welding machine).

We can be fairly confident that our program impacts are principally due to YOP rather than any later impact YOP had on inclusion into or displacement out of other social programs. In panel A of Table 1, we see no evidence that the treatment group was more or less likely to have received other major government or nongovernmental organization programs in the nine years after YOP.

*Investment over Time.*—We already see signs of convergence in capital stocks in the first four years after grants. Panel A of Table 1 reports people's estimated value of capital stocks and ITTs at the two- and four-year end lines. Given survey length constraints, we did not collect these data for the nine-year survey (collecting detailed child outcomes instead). Also, these are self-reported assessments of the total value of raw materials, tools, and other capital goods for respondents' enterprises (and hence distinct from household durable assets, discussed later). Despite these limitations, we see some patterns that will be important in interpreting the impacts on income below.

After two years, the treatment group reported 377,685 shillings (130 percent) more business capital than the control group. After four years, however, we see

TABLE 1—PROGRAM IMPACTS ON COMPLIANCE, EARNINGS, AND EMPLOYMENT

| Dependent variable                                 | Control mean |        |        | Treatment effects   |                       |                     |                     |                   |
|--|--------------|--------|--------|---------------------|-----------------------|---------------------|---------------------|-------------------|
|  | 2-year       | 4-year | 9-year | 2-year              | Difference            | 4-year              | Difference          | 9-year            |
| Observations = 2,005 in 2-year                     | (1)          | (2)    | (3)    | (4)                 | (5)                   | (6)                 | (7)                 | (8)               |
| <i>Panel A. Compliance and initial investments</i> |              |        |        |                     |                       |                     |                     |                   |
| Enrolled in vocational training since baseline     | 0.15         |        |        | 0.532<br>[0.023]    |                       |                     |                     |                   |
| Returned to school since baseline                  | 0.10         |        |        | 0.026<br>[0.015]    |                       |                     |                     |                   |
| Hours of training received                         | 48.98        |        |        | 340.424<br>[22.478] |                       |                     |                     |                   |
| Business assets (thousands of shillings)           | 290.24       | 392.79 |        | 377.685<br>[78.273] | −154.500<br>[100.276] | 223.186<br>[62.679] |                     |                   |
| Reported a major non-YOP program since 2006        |              |        | 0.17   |                     |                       |                     |                     | −0.029<br>[0.018] |
| <i>Panel B. Income</i>                             |              |        |        |                     |                       |                     |                     |                   |
| Standardized income index                          |              | −0.08  | −0.02  |                     |                       | 0.224<br>[0.050]    | −0.146<br>[0.069]   | 0.078<br>[0.048]  |
| Monthly net earnings (thousands of shillings)      | 35.25        | 47.85  | 90.97  | 14.470<br>[4.064]   | 3.694<br>[6.356]      | 18.163<br>[4.887]   | −13.992<br>[9.797]  | 4.172<br>[8.491]  |
| Nondurable consumption (thousands of shillings)    |              | 202.22 | 190.56 |                     |                       | 21.688<br>[7.946]   | −18.961<br>[10.139] | 2.726<br>[6.298]  |
| Durable assets                                     | −0.12        | 0.09   | 0.25   | 0.118<br>[0.044]    | 0.078<br>[0.066]      | 0.196<br>[0.050]    | −0.051<br>[0.068]   | 0.145<br>[0.047]  |
| <i>Panel C. Employment</i>                         |              |        |        |                     |                       |                     |                     |                   |
| Average employment hrs/wk                          | 24.91        | 32.24  | 44.68  | 4.086<br>[1.071]    | 1.433<br>[1.674]      | 5.519<br>[1.286]    | −5.006<br>[2.048]   | 0.513<br>[1.593]  |
| Agricultural hrs/wk                                | 13.90        | 18.77  | 17.34  | −1.223<br>[0.755]   | 1.645<br>[1.210]      | 0.422<br>[0.945]    | −0.343<br>[1.276]   | 0.079<br>[0.856]  |
| Nonagricultural hrs/wk                             | 11.01        | 13.48  | 27.35  | 5.309<br>[0.865]    | −0.212<br>[1.322]     | 5.097<br>[0.999]    | −4.663<br>[1.793]   | 0.434<br>[1.488]  |
| Casual labor, low skill hrs/wk                     | 1.51         | 2.27   | 10.93  | 0.153<br>[0.360]    | −0.270<br>[0.539]     | −0.117<br>[0.401]   | −1.089<br>[1.068]   | −1.206<br>[0.990] |
| Petty business, low skill hrs/wk                   | 3.46         | 3.54   | 7.59   | 0.035<br>[0.559]    | 0.103<br>[0.815]      | 0.138<br>[0.593]    | −1.727<br>[1.222]   | −1.589<br>[1.069] |
| Skilled trades hrs/wk                              | 2.92         | 2.82   | 2.83   | 4.706<br>[0.612]    | −0.927<br>[0.821]     | 3.779<br>[0.548]    | −0.983<br>[0.762]   | 2.796<br>[0.529]  |
| High-skill wage labor hrs/wk                       | 1.24         | 1.84   | 2.93   | 0.659<br>[0.330]    | 0.239<br>[0.553]      | 0.898<br>[0.444]    | 0.008<br>[0.732]    | 0.906<br>[0.582]  |
| No employment hours in past month                  | 0.10         | 0.05   | 0.03   | −0.011<br>[0.015]   | −0.011<br>[0.017]     | −0.022<br>[0.009]   | 0.018<br>[0.012]    | −0.004<br>[0.008] |
| Works over 30 hrs/wk in skilled trade              | 0.04         | 0.03   | 0.03   | 0.054<br>[0.013]    | −0.016<br>[0.018]     | 0.037<br>[0.013]    | −0.008<br>[0.017]   | 0.029<br>[0.011]  |

*Notes:* Each entry in columns 4, 6, and 8 is estimated from a weighted least squares regression of the dependent variable on an indicator for assignment to treatment, district fixed effects, and a vector of baseline covariates. Standard errors are clustered at the group level (of up to five people). We report the coefficient on treatment only. All regressions are weighted by inverse probabilities of attrition and selection into the end line tracking sample. Control means in columns 1, 2, and 3 are also calculated using these weights. Columns 5 and 7 refer to the difference between coefficients between end lines. *p*-values for differences are calculated using a simple *t*-test using the standard errors of coefficients.

evidence that the treatment and control groups converged: the treatment group had just 223,186 shillings (57 percent) greater capital stocks, partly because the control group grew their stocks by 38 percent while the treatment group decreased theirs by

19 percent (see online Appendix Table C.2). This decline in the treatment effect is significant at the 10 percent level. Furthermore, if we look at the 11 percent of YOP groups that did not receive a grant then, much like the control group, they are accumulating capital stocks rapidly in the 4 years after the grant (see online Appendix Table C.2).

Why did the treatment group disinvest on average? Some of the people who made initial investments appear to be dropping out of their new trades. Between the two- and four-year survey, we see a 72 percent fall in capital stock values among treatment group members who got the grant and took training but after 4 years said they no longer practice the trade. Meanwhile, those treatment group members who still practiced a trade after four years reported relatively stable assets, suggesting they are not continuing to invest retained earnings (see online Appendix Table C.2).

*Income.*—In line with our four-year analysis, we pre-specified an index of income as our primary outcome (Blattman, Fiala, and Martinez 2017). We combine multiple measures into one standardized family index to reduce comparisons. Panel B of Table 1 reports impacts on this standardized index of the three measures reported in Figure 1:

- Monthly net earnings: the sum of self-reported wages plus business profits in thousands of 2008 shillings. We measured this with a detailed employment and income module that collected information on all of the respondent's occupations.
- Nondurable consumption: collected using an abbreviated consumption module in thousands of 2008 shillings. The module captured the approximate value of food consumed in the past week and less frequent expenditures (such as clothing or entertainment) in the past month, but it did not include consumption from large durable assets infrequently purchased.
- Durable assets: constructed as the first principal component of a list of home quality measures and household furnishings.

We include each of these in a standardized index, where each outcome is standardized and weighted equally. Online Appendix B.5 includes additional measurement details.

After four years, people assigned to YOP had 0.22 standard deviations higher income.<sup>5</sup> This corresponds to a 38.0 percent increase in net monthly earnings, 10.7 percent increase in nondurable consumption, and 0.196 standard deviation increase in durable assets. Note that the two- and four-year figures are slightly different than those reported in Blattman, Fiala, and Martinez (2014) because of a minor change in the asset index (to include productive assets, such as sewing machines) and the inclusion of the nine-year data in the PCA calculation of the index, as well as to account for a minor error in baseline covariates. The qualitative conclusions do not change.

<sup>5</sup>Due to a minor error in the earlier use of baseline data, the estimates are slightly different than in Blattman, Fiala, and Martinez (2014).

After nine years, however, we no longer observe statistically significant impacts on income. The index of income is 0.078 standard deviations greater in the treatment group, which is not statistically significant. The decline from years four to nine is significant at the 5 percent level, however. Looking at the components of the income index, all treatment effects are positive, but the only large and significant sustained impact is on durable assets. The nine-year program impact on earnings is 4.6 percent of the control mean, and the impact on nondurable consumption is just 1.4 percent of the control mean. Neither nine-year estimate is statistically significant. The decline in both earnings and consumption between the four- and nine-year surveys is significant at the 10 percent level, however. Durable assets, meanwhile, are 0.145 standard deviations greater in the treatment group after nine years (significant at the 1 percent level).<sup>6</sup> This estimate is smaller than the four-year estimate but not statistically significantly so.

One caveat is that assets were somewhat imbalanced at baseline. Our treatment effects control for this difference. However, if we calculate a difference-in-difference treatment effect on assets, in some specifications the asset increase is statistically significant at the 10 percent level only, or not significant at all (see online Appendix D).

Nonetheless, our primary and pre-specified analysis suggests that YOP had a sustained effect on poverty by raising the accumulated stock of assets, though not by permanently raising income flows. This increase in durable assets (though fragile) is an important dimension of consumption and a likely source of precautionary savings and insurance against adverse events.<sup>7</sup>

Finally, altogether this temporary increase in income flows seems to have exceeded program costs.<sup>8</sup> In online Appendix Figure C.1, we estimate that the cumulative earnings gain over nine years is roughly \$665, or 1.8 times the size of the grant. This excludes program implementation costs and any local spillovers, both of which are unknown.

*Employment and Occupational Choice.*—Finally, panel C of Table 1 reports program impacts on employment. These estimates mirror Figure 2, showing a sustained increase in time spent doing skilled work but no sustained effects on total employment.

After four years, we saw a statistically significant increase in total time spent working of 5.5 hours, a 17 percent increase over the control group. This effect was largely due to increases in nonagricultural work, especially skilled trades (134 percent increase) and high-skilled wage labor (49 percent increase). None of these effects were sustained nine years after grant disbursement, save for time spent in a skilled trade. The treated sample spent twice as much time in a skilled trade as the control group and was twice as likely to be working primarily in skilled trades (defined as at least 30 hours per week in the past month).

<sup>6</sup>The asset index does not include savings, since we only have savings and loans information at the nine-year end line. We see no treatment effects on self-reported cash savings on hand, gross or net of all personal debts.

<sup>7</sup>The nine-year effects are similar for men and women (online Appendix Tables C.6 and C.8). We do not see major heterogeneity along other baseline characteristics (online Appendix Table C.9). Online Appendix Table C.3 examines impacts on business operations and migration. We see little evidence of impacts.

<sup>8</sup>Note that the program grant was intended to be spent on capital investment (Table C.2 shows this was the case) and should not be thought of as income directly, so the grant is not included as a cost, despite being directly transferred to the recipient.

The control group has been increasing their overall hours worked considerably, from just under 11 hours per week at baseline to over 44 hours per week nine years later (online Appendix Table C.1). This growth in employment is due to not increases in skilled or agricultural work—both of which have been stagnant over the nine years—but to increases in nonagricultural wage work and petty business. This reflects both general economic improvements in northern Uganda during the time period studied as well as the fact that the study population at baseline was both young and highly likely to be unemployed.

*Robustness and Sensitivity Analysis.*—These conclusions are robust to different models and measurement decisions. Online Appendix D reports a range of sensitivity analyses, including difference-in-difference estimates, estimates that exclude baseline controls, estimates that omit the inverse probability weights for attrition, estimates that do not top-code continuous variables (such as earnings) at the ninety-ninth percentile, and estimates where we limit our analysis to the sample present for both the four-year and nine-year surveys. Each of these specifications provides qualitatively similar conclusions.

Furthermore, we model sensitivity to attrition by bounding treatment effects. In the spirit of Manski bounds, we recalculate treatment effects making fairly extreme assumptions about the unbound. For instance, we ask how our conclusions would differ if the unbound treatment group members were highly successful (with incomes 0.25 standard deviations above the mean) and unbound control members were highly unsuccessful (with incomes 0.25 standard deviations below the mean). Within these highly implausible attrition scenarios, we can obtain a statistically significant treatment effect. Even then, we show that these estimates are not economically significant. Thus, even if unobserved selective attrition is present, it is unlikely to affect our basic conclusions.

### B. Human Capital Impacts

At baseline, YOP recipients were 25 and had 1.5 children on average. They had 1.6 children in the four years after YOP, when income gains were highest. We hypothesized that they would invest more in their children at a young age and that the children would have better outcomes after nine years, consistent with a wide literature on antipoverty programs (e.g., Paxson and Schady 2010, Aizer et al. 2016). We only have these data for the nine-year end line. Table 2 reports group means and ITT estimates.

*Own Health.*—As a young population, mortality rates were just 0.55 percent over 9 years. However, looking at panel A of Table 2, YOP reduced mortality by 0.4 percentage points, an 80 percent improvement. This gain is not statistically significant, however.

Panel A also reports a number of self-reported measures on physical health (including specific physical functions, disabilities, and work interruptions) and mental health (including depression, prosociality, and hostility). We combine these secondary outcomes into two family indexes to reduce hypothesis tests. We see no substantively large or statistically significant differences between the treatment and control group in the two indexes or their components. The estimated treatment

TABLE 2—NINE-YEAR PROGRAM IMPACTS ON OWN HEALTH, CHILD MORTALITY, AND FERTILITY

| Dependent variable   | Mean           |                |                   |
|--|----------------|----------------|-------------------|
|  | Control<br>(1) | Treated<br>(2) | ITT<br>(3)        |
| Observations = 2,086   |                |                |                   |
| Nine-year end line   |                |                |                   |
| <i>Panel A. Own health outcomes</i>                                      |                |                |                   |
| Respondent passed away   | 0.01           | 0.00           | −0.004<br>[0.006] |
| Physical health index (z-score)  | −0.03          | −0.02          | −0.028<br>[0.047] |
| Mental health index (z-score)  | 0.01           | 0.00           | −0.056<br>[0.047] |
| <i>Panel B. Fertility, household size, and child expenditures</i>        |                |                |                   |
| Number of pregnancies 2007 or later                                      | 2.47           | 2.71           | 0.097<br>[0.101]  |
| Percent of births that were live 2007 or later                           | 0.92           | 0.93           | 0.013<br>[0.010]  |
| Percent of pregnancies 2007 or later where child still living            | 0.89           | 0.89           | 0.009<br>[0.012]  |
| Percent of successful pregnancies 2007 or later where child still living | 0.96           | 0.95           | −0.006<br>[0.006] |
| Number of biological children alive born 2007 or later                   | 2.14           | 2.31           | 0.075<br>[0.083]  |
| Size of household  | 5.86           | 6.03           | −0.127<br>[0.162] |
| Mean age of children (0–15)  | 7.50           | 7.53           | 0.014<br>[0.138]  |
| Mean age of biological children (0–15)                                   | 7.08           | 7.19           | 0.095<br>[0.147]  |
| <i>Panel C. Child educational outcomes</i>                               |                |                |                   |
| Child age-adjusted educational attainment (6–24)                         | 0.01           | −0.00          | −0.012<br>[0.037] |
| Child age-adjusted educational attainment (6–24), biological             | 0.09           | 0.05           | −0.046<br>[0.045] |
| Mean of child enrollment   | 0.91           | 0.86           | −0.016<br>[0.013] |
| Mean of child enrollment, biological                                     | 0.91           | 0.86           | −0.018<br>[0.013] |
| Current child expenditures (clothes and school)                          | 42.14          | 39.73          | 0.411<br>[2.784]  |
| Current child expenditures per child                                     | 14.00          | 12.92          | 0.502<br>[1.071]  |
| <i>Panel D. Child health outcomes</i>                                    |                |                |                   |
| Mean health index per child, ages 3–9, family average                    | −0.03          | 0.07           | 0.078<br>[0.043]  |
| Mean parent-reported health score per child, ages 3–9, family average    | 0.00           | 0.09           | 0.071<br>[0.047]  |
| Mean malaria cases in past year, ages 3–9, family average                | 2.96           | 2.74           | −0.125<br>[0.087] |
| Mean normalized ADL score per child, ages 3–9, family average            | 0.01           | 0.03           | 0.045<br>[0.041]  |

*Notes:* Each entry in columns 4, 6, and 8 is estimated from a weighted least squares regression of the dependent variable on an indicator for assignment to treatment, district fixed effects, and a vector of baseline covariates. Standard errors are clustered at the group level (of up to five people). We report the coefficient on treatment only. All regressions are weighted by inverse probabilities of attrition and selection into the end line tracking sample. Control means in columns 1, 2, and 3 are also calculated using these weights. Maximum number of observations across all regressions is equal to 2,086 rather than 1,981 as in other tables because of the variable *Has died*. We code someone as having died if we found out directly from a friend or relative that someone had passed away when searching for our respondent. Consequently, our sample also includes refusals and people we were unable to locate.

effects on the family indexes are less than 0.06 standard deviations. Online Appendix Table C.5 reports treatment effects on component measures.

*Child Health and Education.*—We first asked respondents to recall each pregnancy and followed these through to their current status to construct measures of number of pregnancies, percentage of births and children living, and the number of children. Men report pregnancies about which they were aware, with obvious sources of error (discussed below).

For all biological children, plus nonbiological children living in the household, we asked respondents to report grade attainment, enrollment, and current child clothing and education expenditures. We report results for children currently ages 6–15 (age 6 or less at the time of the grant). Conclusions are similar for children ages 6–24 (not shown).

Finally, we collected health information on biological children who were ages 3–9 at the time of the survey and hence were unborn at the time of the grant. Many respondents did not live with their children, and so we did not collect anthropometric data. Instead, we asked parents to report (i) measures of specific physical functioning, (ii) a subjective assessment of the child's healthiness, and (iii) reported malaria cases in the past year. We use these to construct an overall index of child health.

These recall measures are prone to significant error, especially among men. Noise and systematic underreporting of child health will bias us toward failing to reject the null. The main threat to identification is that measurement error is correlated with treatment status. For example, if treated men know their children's health better than control men, it could bias our estimates. We see no treatment effects on the number of children or their ages, however, and treatment men are no more or less likely to migrate away from their family (online Appendix Table C.3). This mitigates concerns about men's measurement error being correlated with their treatment status.

Looking at panels B through C of Table 2, we see little evidence of impacts on fertility, children's education, or their health. Most of the estimated coefficients are not statistically significant, and most 95 percent confidence intervals exclude improvements greater than 10 percent. The education coefficients generally point opposite the direction we would expect. Hence, even with less noisy measures, we would be unlikely to observe substantive impacts on children's well-being.

One exception is the health index among children alive today. The index is 0.08 standard deviations greater in the treatment group, significant at the 10 percent level.

*Robustness and Sensitivity Analysis.*—Online Appendix D shows additional analysis. First, we estimate separate effects for men and women. The pooled impact of the program on the health of children might be concentrated among women recipients, who are 0.17 standard deviations healthier, significant at the 5 percent level. However, the difference in effects between men and women is not statistically significant. Next, we conduct the same robustness tests as for the economic outcomes. Program estimates are virtually unchanged, with the exception of the average health of children; when we omit baseline covariates, the impact of YOP increases slightly and becomes significant at the 5 percent level. However, we do not adjust for multiple hypotheses across specifications, so this change may be spurious.

### *C. Political Behavior*

In Blattman, Emeriau, and Fiala (2018), we found that three years after YOP, during the 2011 national elections, YOP recipients reported that they were no more likely to engage in nonpartisan political actions but were more likely to support and vote for opposition candidates. We hypothesized that YOP earnings helped free recipients from clientelistic election tactics. Online Appendix E reports nine-year impacts. We see similar but less statistically significant patterns in the 2016 elections. YOP recipients were 0.08 standard deviations more likely to support the opposition party, significant at the 10 percent level only. Nonetheless, some of the component treatment effects are very large: actively working to get an opposition candidate elected rose 23 percent relative to the control mean, and “would vote for the opposition” rose 14 percent relative to the control mean, although both are insignificant. The fact that political behavior is more persistent than the income effects could mean that separation from clientelistic networks is not what drives the political behavior change. Changes in political behavior could be persistent if party affiliation in young adulthood is habit forming.

### **III. Discussion and Conclusions**

The main effect of giving groups of underemployed, poor young Ugandans large one-time grants appears to have been an increase in consumption that lasted at least four years and at most nine years. By smoothing this temporary income gain, YOP recipients increased their durable assets and precautionary savings. We do not have data on income volatility and job stability. Even so, at least 3 percent of the sample moved up the job ladder into full-time skilled trades. The level and trend of these occupational choice effects makes it unlikely that the control group is converging on this dimension.

These patterns are consistent with our simple model in Blattman, Fiala, and Martinez (2014) as well as more elaborate structural models and program simulations by Buera, Kaboski, and Shin (2015, 2014). In the absence of extreme frictions or poverty traps, these models all predict that the positive economic impacts of entrepreneurship capital grants can be short lived if the returns to capital diminish quickly, people can acquire capital in absence of the program, or labor productivity and wages are high in other sectors.

Several features of the study context may help explain the slowdown we observe. One is modest but healthy local economic growth. Second, YOP was designed to target the most promising demographic group for short-term impacts: capital-poor, high-ability, high-initiative young people (Hussam, Rigol, and Roth 2020). In effect, we observe undercapitalized youth in their mid-20s taking some time to find steady wage work or start a microenterprise. YOP appears to have sped up this process while also shaping occupational choice.

Such convergence is especially important to assess in higher-cost programs. Beneficiary targeting, physical asset delivery, and skills training can be expensive to deliver relative to the earnings potential of microenterprises. For example, programs to deliver livestock, training, and income support to ultrapoor households in six countries estimated that the three-year impacts would have to persist or grow for at



least a dozen more years in order for consumption gains to outweigh program costs (Banerjee et al. 2015). Persistent gains are plausible for the ultrapoor, but the speed of convergence will make or break cost-effectiveness in these cases.

Tentatively, one might conclude from the emerging body of evidence that *some* of the poor have high returns to capital and face financial market imperfections, and grants help them start successful microenterprises. The poverty gains are probably most sustained for the young and poorest in the places with the worst access to credit and fewest employment options.

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