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Impacts of Industrial and Entrepreneurial Jobs on Youth: 5-year Experimental Evidence on Factory Job Offers and Cash Grants in Ethiopia

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ABSTRACT

We study two interventions for underemployed youth across five Ethiopian sites: a $300 grant to spur self-employment, and a job offer to an industrial firm. Despite significant impacts on occupational choice, income, and health in the first year, after five years we see nearly complete convergence across all groups and outcomes. Shortrun increases in productivity and earnings from the grant dissipate as recipients exit their micro-enterprises. Adverse effects of factory work on health found after one year also appear to be temporary. These results suggest that one-time and one-dimensional interventions may struggle to overcome barriers to wage- or self-employment.

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1 Introduction

In developing countries, such as Ethiopia, low-skilled youth often spend long periods of time underemployed or not employed at all. Young women often face steeper challenges than men and exhibit lower rates of labour force participation and higher rates of unemployment.

This paper follows a panel of young, mostly women, low-skilled job-seekers in Ethiopia over a period of five years. Our aim is to assess whether young people in this context face barriers to entry into occupations for which they would be well suited. We use two experiments to analyze whether one-time interventions can overcome impediments to entering two of the most common types of work: (i) low-skill wage work, especially in factories; and (ii) self-employment in petty business and other "microenterprises".

Between 2010 and 2013, we identified nearly 1000 people interested in an industrial job at one of five firms. The firms were in different sectors and regions, each one hiring a large batch of workers for a line expansion, sometimes multiple cohorts over time. Most of the eligible applicants were healthy but unemployed women in their early 20s, with no formal work experience and little self-employment. We introduced two experimental interventions: a cash grant intended to stimulate self-employment, and a factory job offer intended to reduce barriers to entry in wage employment. Thus, the job-seekers were assigned to either a start-up grant, the job offer, or a control group. After a baseline survey, we re-interviewed the sample after roughly 1 and 5 years, finding 85–90%.

The first intervention was a cash grant worth roughly $300, equal to about one year's income at prevailing wages. It was framed as a business start-up grant and came with a few days of training and consulting on microenterprises.[1] The grant was designed to help these young people overcome one of the most commonly cited barriers to self-employment: a lack of capital or access to credit.[2] Unemployed youth in developing countries may have opportunities in "entrepreneurial" self-employment, in agriculture or petty trades, but lack the initial capital required to start such small businesses. They may also face uncertainty about their own ability to run an enterprise and not be able to take the risk of experimenting with this kind of work.

In these contexts, cash or in-kind grants could spur investment and increase long-run earnings. A number of recent studies have found that cash or other capital injections reduce poverty over horizons of mostly 1–4 years.[3] These results have bolstered a view that a lack of

[1] We repeatedly stressed to participants, the grant was unconditional.

[2] See Banerjee and Duflo (2011), Blattman and Ralston (2015), Brudevold-Newman, Honorati, Jakiela, and Ozier (2017), McKenzie (2017).

[3] In northern Uganda, a program giving women $150 grants, basic training and follow-up led to large income gains after 18 months (Blattman, Annan, Green, Lehmann, and Jamison (2016)). Grants of cash and in-kind capital to less poor, existing entrepreneurs in Sri Lanka, Ghana and India led to sustained

capital and imperfect financial markets hold many poor people below their potential. There is relatively little long term evidence, however, and we do not know the longevity of these high returns to capital. Moreover, theory suggests that, unless people are hampered by extreme constraints and frictions, over time they should be able to earn and save enough to make the same microenterprise investments.

In the second experiment, we worked with the industrial firms to randomize offers to entry-level jobs. We observed queues of young people, mostly women, applying for industrial jobs around Ethiopia, most of whom were turned away because of insufficient new openings.[4] Following months of qualitative observation and interviews around dozens of factories, we hypothesized that these industrial jobs were widely desired but scarce. There were some indications the firms offered unusually stable employment levels and paid wage premiums compared to self-employment. Theoretically, there are several reasons why industrial work would pay a wage premium compared to informal work. Firms may pay efficiency wages or there may also be institutional and legislative sources, such as minimum wages, labor codes, or union bargaining (Katz, Summers, Hall, Schultze, & Topel, 1989; Akerlof & Yellen, 1986; Card, 1996). If so, the result is a dual or segmented labor market, in which those gaining industrial jobs earn rents while informal workers queue for those jobs (Lewis, 1954; Harris & Todaro, 1970; Fields, 1975).[5] Even in the absence of wage premiums, industrial jobs could also set young workers on a job ladder that leads to scarce skills, better jobs, and higher wages over time, especially young women.[6] But workers may exhibit considerable hetero-

increases in earnings 1–5 years later (Mel, McKenzie, and Woodruff (2012), Fafchamps, McKenzie, Quinn, and Woodruff (2014), Hussam, Rigol, and Roth (2017)). Cash grants to poor farmers in Mali raised farm inputs and incomes after 1 and 2 years (Beaman, Karlan, Thuysbaert, and Udry (2014)). Unconditional cash transfers in Kenyan villages led to sustained increases in assets between treatment and control villages, but no consumption impacts after 9 months and 3 years (Haushofer and Shapiro (2016, 2018)). There is also some evidence from a Mexican national program that cash relieves important financial constraints and leads to higher income after 1–2 years (Gertler, Martinez, and Rubio (2012), Bianchi and Bobba (2013)). Across 7 countries, programs that give grants of livestock with basic training and temporary income support show sustained increases on the incomes and consumption of the poorest rural households four years after grants (Banerjee, Karlan, and Zinman (2015), Bandiera et al. (2013, 2017)). The effects of capital injections are not universally positive, however. Fiala (2018) fails to find income effects from cash grants to existing businesses in Uganda. A cash grant programs to young men living on the streets of Monrovia and engaged in petty crime also had very short-lived impacts (less than one year) on enterprise and earnings, potentially due to the unusual instability and risk of their existence (Blattman, Jamison, and Sheridan (2017)). Karlan, Osei, Osei-Akoto, and Udry (2014) find that cash grants to Ghanaian farmers had no effect without insurance, also because of the constraints from imperfect insurance.

[4]Ethiopia has been a growing export hub in horticulture, textiles, and leather. As countries like Ethiopia enter the early stages of industrialization, the number of low-skill industrial job opportunities have grown.

[5]Empirically, a large body of observational evidence suggests that formal firms pay premium wages, especially large, foreign-owned, or exporting firms (Bernard, Robertson, & Schott, 2010; Verhoogen, 2008; Söderbom & Teal, 2004; El Badaoui, Strobl, & Walsh, 2008).

[6]Women are commonly employed in low-skill firms, and there is observational evidence that working in textile factories or other export manufacturers raises women's status in the household, their quality of life,

geneity in their suitability for factory work. And a growing body of literature suggests that hiring among firms in developing countries is prone to considerable frictions and information assymetries, which may prevent firms from selecting the workers who are most likely to flourish in careers in these occupations (Abebe et al., 2018; Bassi & Nansamba, 2017; Abel, Burger, & Piraino, 2017).

Even absent any scarcity or rents in industrial jobs, a randomized job offer might change occupational choice and earnings paths in the long run. Most of our sample was unemployed for at least a month before they entered our sample, either due to adverse shocks or because they are entering the labor market for the first time or after a spell of unemployment. A range of research in more developed labor markets suggests that market conditions and opportunities for such people matter a great deal for long term labor market prospects (Arulampalam, Gregg, & Gregory, 2001; Kroft, Lange, & Notowidigdo, 2013). Young people and women in particular in Ethiopia may also face uncertainty about their own proclivities and abilities for industrial work, be unaware of health and other risks, or face other search and matching frictions. Because so many low-skilled people apply to the still small number of industrial firms, even well-matched individuals may never get the opportunity to enter into these occupations.

For the most part, we do not find support for the hypotheses that start-up grants lead to sustained income changes, or that industrial job offers affect long-run well-being. Over five years, we see that these young and mostly unemployed women found relatively full-time employment in a variety of wage work and microenterprises, even without the opportunity for a grant or an initial job offer. Medium-run equilibrium labor market outcomes seem unaffected by the interventions. After 5 years we see almost complete convergence in earnings, employment, and health.

These medium-run results diverge from some of the short-run results in important ways. For instance, in Blattman and Dercon (2018), we saw evidence that the start-up grant dramatically raised short-run productivity. While those offered the grant weren't working many more hours than the control or job offer groups, their earnings were a third higher. This corresponded to nearly PPP$1 a day greater income—a huge amount considering that counterfactual earnings were only about PPP$3 (or about $1 a day at 2010 price levels and market exchange rates). After 5 years, we find that the new microenterprises and productivity boost are not sustained and that the control and start-up grant groups converge within a few years. At best, the cash acted as like a temporary boost to earnings with short-term consumption impacts rather than a permanent lift out of poverty.

Another example is the effects of industrial work on health. Our 1-year results showed

and the health of children (Kabeer, 2002; Hewett & Amin, 2000; Atkin, 2009; Getahun & Villanger, 2016).

that those who took the factory job were more likely to report substantial health problems a year after the job offer, even though the average employee quit after a few weeks or months. Were these health problems temporary or lasting? We returned with a battery of expanded measures, and pre-specified health as our second primary outcome after incomes. After 5 years, we see no evidence of long term adverse effects from the industrial work. Those assigned to the industrial job offer have 3 months more cumulative experience in factories than the control group, and neither reduced-form treatment effects or an instrumental variables approach suggest that this added factory work reduced health in the long run.

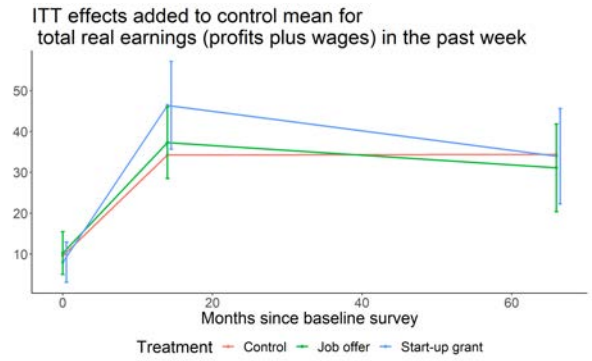
Such longer-term results are useful for a few reasons. First, they allow us to rule out sustained or transformative impacts of the cash grants. There are few studies on the effects of cash transfers in early industrializing areas, let alone long term follow ups. Our findings are consistent with a 9-year evaluation of group cash transfers in Uganda, however (Blattman, Fiala, & Martinez, 2018). Second, as more societies industrialize, it is also important to know whether industrial work can lead to large, chronic, and unanticipated health problems. At least in this instance, the adverse health effects seem to have been temporary.

Finally, we wanted to assess longer run impacts of the industrial job offer on income and employment. We might not expect earnings gains in industrial work after one year, because entry-level wages are so low. Even so, by fostering experimentation with a new sector, new matches, and allowing 'good' types to get a foot in the door, we may see their wages rise with tenure and from climbing up the job ladder (even if this is a minority of those assigned to the job).

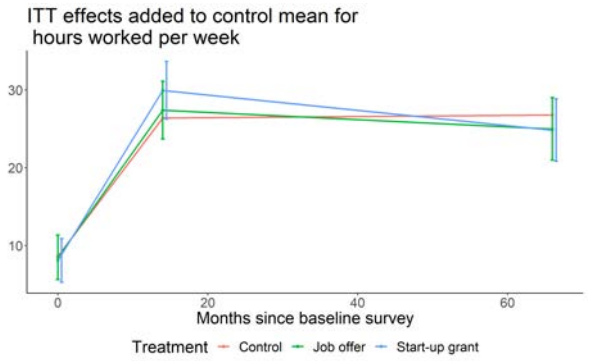
Figures 1a and 1b summarize the paths of income and employment in all three arms over the 5 years, with all results deflated to 2010 Birr. The jump in weekly earnings and employment in all three groups in the first year suggests that people self-selected into the sample of job applicants because they were new labor market entrants or had suffered a shock. Likely this is both a life-cycle effect from youth increasing hours and earnings over time and perhaps rapid recovery from any adverse shocks to employment that propelled people into the sample.

Figure 1: Progression of income and hours worked across time by treatment status

(a) Mean income (all profits and wages) in past week across time for all cohorts



(b) Mean hours worked across time for all cohorts



Note that the figure makes it seem that job offer and control group incomes are stagnating from years 1 to 4, and that the start-up grant group is converging down to their level. This is possible, and indeed we see evidence of microenterprise exit. But this is also a period of high inflation, and nominally earnings and consumption are rising. At the same time, this is a period of high inflation and the national inflation index (the only available measurement) could exaggerate the flatness of poor young people's incomes. In any case, we see no sustained effect of cash on income or consumption.

In the end, only one in six people remain in factory work, even among those randomly offered the job. From our data and qualitative work, it seems that people in all three arms used factory jobs as a safety net to smooth income temporarily until other less rigid and less risky work could be found. Altogether, these medium-run results are consistent with the simple alternative hypothesis that industrial jobs are not higher-quality and "rare" jobs, but rather are average to low quality jobs where labor markets function normally.

In the end, a main takeaway from this experiment is that two of the interventions that some schools of thought think should have large lasting effects on poverty simply do not have these impacts, at least in this setting and with young women. One interpretation is that one-off interventions and nudges addressing a single or small number of market failures may not be lasting. Powerful forces may push individuals back towards equilibrium outcomes irrespective of interventions. This is largely speculative, but if more interventions in more places fail to find long run effects of initially-successful poverty or employment interventions, it may challenge the widespread marginalist view of poverty alleviation. This could push interventions to be more multifaceted addressing multiple barriers at once, and perhaps addressing constraints at the local or macro level as well.

2 Setting

Ethiopia is the second most populous nation in Africa after Nigeria, and also one of the poorest. 27 percent live under $2 a day in purchasing power parity (PPP) terms, and agriculture employs 85 percent of the workforce. Like many African countries, the underdeveloped private sector has offered few formal sector jobs. Youth unemployment and underemployment are high. Most young people are engaged in informal wage work or self-employment.

At the same time, Ethiopia is also one of the fastest growing economies in the region, with GDP growth of roughly 10% per year from 2006–16. In particular, Ethiopia has become a growing export hub in horticulture, textiles, and leather. Although the economy has been moving in fits and starts through the early stages of of industrialization, Ethiopia has been touted as one of China's successors in light manufacturing (The Economist, 2014). The country has several advantages from a manufacturer's point of view: low wages, a politically stable and foreign investment-friendly regime, a domestic market of 94 million people, and proximity to Europe.

Over the last two decades, there has been a transformation in Ethiopia's urban labor markets. They have become more flexible, with rising importance of private sector work, no obvious skill premiums between the private and public sector, and lower (but still considerable) urban unemployment. In all the firms in our study, and in general across the private sector, employers can set wages without any legal restriction or reference to union deals. The governing labor law makes it also relatively straightforward to fire an employee.

In the years prior to our study, 2000–08, national income and industrial output both grew about 10 percent per year, with the number of medium and large manufacturers doubling in number (CSA, 2011). The beginning of the study period was first a boom time followed by a mild slow down. Even so, during this period new foreign firms were entering the market and starting small plants, and some domestic firms were continuing to invest and expand. Growth picked up later in the study period, and again Ethiopia is now one of the fastest growing economies in the region.

3 Experimental sample, procedures, and data

3.1 Experimental sample

Our sample of young people comes from job applicants to 5 industrial firms, in 5 different sectors and 4 regions of the country, both urban and rural. Two firms hired more than one cohort over the study period, 2010–13, for a total of eight cohorts. Table 2 describes the firms and cohorts. Three firms engaged in light manufacturing (textiles and garments,

Table 1: Baseline summary means and test of randomization balance

Baseline covariate (N=947)	Control mean (N=358)	Balance test (OLS)			
		Job–Control		Entrepreneur–Control	
		Diff	p-value	Diff	p-value
	(1)	(2)	(3)	(4)	(5)
Female	0.80				
Age	22.02	-0.12	0.68	-0.14	0.63
Unmarried	0.81	-0.06	0.07	-0.04	0.25
Muslim	0.05	-0.00	0.90	0.00	0.98
Household size	4.35	-0.13	0.45	-0.14	0.45
Household head	0.23	0.04	0.25	-0.00	0.96
Proportion household dependents	0.43	-0.00	0.98	-0.00	0.96
Total years of education and training	9.31	-0.20	0.34	-0.02	0.92
Executive function, z-score	0.11	-0.18	0.01	-0.13	0.08
Weekly cash earnings (2010 birr)	9.57	0.59	0.81	-1.44	0.57
Durable assets, z-score	0.09	-0.11	0.13	-0.13	0.06
Ever worked in a large firm	0.27	-0.03	0.43	0.05	0.18
Average weekly hours of work	7.52	-0.09	0.94	-0.36	0.80
No work in past 4 weeks	0.68	0.01	0.86	-0.01	0.76
Highest - lowest earnings, past month	181.38	39.44	0.05	15.84	0.33
Could borrow 3000 birr	0.31	0.04	0.27	-0.00	0.98
Ability to do activities of daily life (0–15)	14.32	0.09	0.40	0.10	0.31
Disability (great difficulty at >1 ADL)	0.01	-0.01	0.26	-0.00	0.77
Risk aversion, z-score	-0.01	-0.05	0.55	0.10	0.20
Future orientation, z-score	0.10	-0.06	0.45	-0.03	0.73
Locus of control index, z-score	-0.04	0.04	0.62	0.13	0.12
Self-esteem index, z-score	-0.05	0.03	0.75	0.06	0.42
Family relations index, z-score	-0.05	-0.02	0.77	0.07	0.35
Friends and neighbors relations index	-0.01	-0.05	0.49	0.00	0.95
Change in subjective well being, past yr.	0.22	0.20	0.03	0.09	0.33
Symptoms of depression, z-score	-0.02	0.02	0.82	0.01	0.94
Symptoms of anxiety, z-score	-0.04	0.05	0.50	-0.01	0.92
Aggressive or hostile behaviors, z-score	0.04	-0.06	0.44	-0.13	0.11
Conscientiousness index, z-score	-0.00	0.01	0.89	0.04	0.65
Years experience, private firm	0.34	0.17	0.08	0.22	0.02
Years experience, workshop	0.01	0.00	0.73	0.01	0.27
Years experience, government/NGO	0.08	-0.02	0.67	0.02	0.73
Probability of better job, next month	0.68	-0.01	0.47	-0.01	0.72
Probability of full-time work, next month	0.55	0.01	0.74	0.03	0.17
p -value from F-test of joint significance		0.04		0.01	
Observations		662		643	

Notes: Medians are imputed for baseline variables with missing observations. Treatment and control group differences are calculated using an OLS regression of each covariate on treatment indicators plus block (cohort-gender) fixed effects. Balance tests for the female dummy are omitted because randomization was blocked on gender. Standard errors are heteroskedastic-robust.

Table 2: Characteristics of our study cohorts and interventions

	Firm and cohort						
Characteristic	Beverage producer		Horticulture farm	Flower farm	Shoe factory	Garment & Textile factory	
Site type	Peri-urban		Rural	Peri-urban	Urban	Peri-urban	
Region	Oromia		SNNP	Oromia	Addis	Tigray	
Approximate number of employees	150		250	2,000	1,400	700	
Foreign owner?	N		Y	Y	N	N	
Exporter	N		Y	Y	Y	Y	
Unionized?	Y		N	Y	Y	Y	
Start date (MM/YY)	04/10	03/11	10/11	11/11	01/13	05/12	05/13	06/13
Eligible sample	53	68	89	152	158	89	140	188
Jobs available	15	19	30	50	50	30	45	60
Monthly wage (current birr)	350	350	574	535	734	417	420	420
Monthly wage (2010 birr)	348	280	381	395	422	247	234	233
Weekly work hours	48	48	48	47	44	48	48	48
Grants (after tax)								
In current birr		4,872	5,016	4,969	5,773	5,124	5,849	5,884
In 2010 birr		3594	3330	3293	3278	3048	3266	3196
In USD		290	290	290	315	290	315	315
Tranches		2	2	2	1	1	1	1
Applicants								
Age	22	24	23	22	24	22	21	20
Female	64%	44%	77%	100%	66%	52%	100%	100%
Married	15%	22%	31%	34%	34%	10%	25%	8%
Education	11	11	6	6	12	10	11	9

Notes: Firm data come from firm manager interviews. Applicant data come from a baseline survey, described in Section 3.4.

shoes, and beverages) and two in commercial agriculture (flowers and vegetables). Four were export-oriented. Only one was foreign-owned.

Eligible job applicants were recruited and screened in the firms' standard fashion, described below. Only these screened applicants were eligible for one of the two treatments: an industrial job offer or the start-up grant package.

Table 1 reports baseline characteristics of these screened applicants, from self-reported surveys.[7] 80 percent were women. The average applicant was 22 and had completed grade 9. Most were unmarried. They had 7.5 hours of work per week, typically a portfolio of activities such as farming, casual labor, or petty business. They had earned little cash in the previous month. Only 27 percent had worked in a large, formal firm before, and only 19 percent in a factory. Based on qualitative interviews, most applicants had only a hazy idea of the type and difficulty of the work in advance, and often only learned the salary being offered at the time of hiring.

3.2 Interventions

3.2.1 "Start-up" grant and training

The core of the start-up treatment was an unconditional cash grant of nearly 5000 Birr, or roughly $300. (see Table 2).[8] We chose the $300 amount based on our qualitative assessment of the costs required to set up a small part-time enterprise. While we framed the cash grant as a business start-up fund, throughout the intervention we made clear that it was nonetheless an unconditional grant and grantees were free to use it as they saw fit—savings, consumption, or investment.

To encourage and enable business start-up, however, grantees initially received five days of business training and planning.[9] Professional skills trainers led classes of about 20, and each person also received individual mentoring during those five days.[10] Subjects had to

[7]Applicants completed a 90-minute baseline survey plus 45 minutes of interactive games, with real money, to measure time and risk preferences, and cognitive abilities such as executive function. An Ethiopian enumerator delivered surveys and the games verbally in the local language. The games remunerated the respondent with roughly a half days wages.

[8]The grant amount varied slightly from cohort to cohort because of inflation, currency devaluation, and tax issues. For cohorts 2 to 4, a for-profit firm ran the intervention and was required to withhold tax on the grants. To minimize the tax burden the cash was disbursed in two tranches several weeks apart. We used a for-profit firm because we could not find a non-profit organization willing to disburse cash without conditions at low cost. For cohorts 5 to 8, we ran the intervention through a parastatal research organization to avoid the tax burden. The amount of the grant was increased to maintain the rough purchasing power and disbursed in a single tranche to reduce implementation costs.

[9]Total implementation cost of training and grant was roughly $450 per person including the grant, training, and local program administration.

[10]Cohorts 2 and 3 also received a follow-up visit by the trainer after three months for additional advice.

complete at least three days of the training to receive the grant.

3.2.2 Industrial job offer

The jobs involved working on production lines where the workers bottled water, picked and packed produce and flowers, cut fabric, or sewed shoes. They could involve heavy machinery or simple tools.

In terms of eligibility, the positions required no previous work experience. Applicants had to be healthy and able-bodied. All firms also had a minimum education requirement —grade 6 in the two rural horticulture firms, and grade 8 or 10 in the more urban manufacturing ones. Most firms had separate jobs for men and women, and depending on the position they were hiring for, they would specify a gender.

The positions required 45 to 50 hours per week over 5 or 6 days. At the time of the baseline surveys, the jobs typically paid a wage of $1 to $1.50 per day at 2010 market exchange rates (where $1 = 13.5 birr in 2010). Some firms offered non-wage benefits such as on-site health care and bus transport.

The workplaces were professional and well-maintained, and firms never coerced employees. Nonetheless, health risks were common, especially: air quality (dust particles or chemical fumes); discomfort and fainting from standing or lack of breaks or water; and safety hazards such as wet floors, sharp instruments, and so forth. In interviews, workers who used cleaning solvents, pesticides, dyes and glues sometimes reported fainting from inhalation.

Most firms were unionized, but these were generally worker associations that mediated disputes but did not engage in collective bargaining. Occasionally, however, we did observe short strikes or walkouts in response to salary delays.

3.3 Recruitment

3.3.1 Firm recruitment and selection

We approached roughly 300 firms, about half of all private industrial firms in Ethiopia with 50 or more employees.[11] We contacted them by phone or walk-in. To be eligible for the study, a firm had to be in manufacturing or commercial farming, expect to hire a batch of at least 15 low-skill, full-time workers, and be willing to randomly assign job offers among screened applicants.

Grantees did not see this service as helpful, and given the cost it was discontinued after cohort 3.

[11] We identified these firms through applications for investment certificates, public business listings, industry associations, and personal contacts.

The limiting factor was whether a firm had imminent large expansion plans. Only a handful had plans to open a entire new production line and hire a large batch of workers at once.[12] Firms with more modest expansion plans were a poor fit for the study, as they planned to hire people piecemeal, to accommodate more gradual growth and cope with regular attrition.

Randomization was seldom an issue, and more than three-quarters of the firms we approached were open to the study.[13] While one might expect that firms want to select the best workers, low-skill entry-level positions were often filled without a substantive interview process. In most of the firms we approached, entry-level hiring was ad hoc in the sense firms filling low-skill positions on a first-come, first-hire manner, with little or no interview process.

What are these five study firms a case of? Our data suggest the jobs are similar to other labor-intensive, low-skill, entry-level positions in the large textile, garment, footwear, beverage, and commercial farming sectors, and thus different from positions in higher-skill and heavier or more capital-intensive manufacturing. Compared to a representative sample of industrial firms in 2014 in the capital Addis Ababa, our five firms had higher revenues, lower profits, two to three times as many production employees, and lower-skilled employees.[14]

It is reasonable to worry that firms willing to randomize employment were poorly managed or had unusual turnover. While possible, qualitatively we saw little difference between our firms and the others we approached. On the contrary, all were expanding employment, suggesting they had more credit and higher returns to investment than others.

[12]One reason is that sector growth was slowing in this period. 2010 to 2012 in Ethiopia was a period of moderate government financial repression and pre-election uncertainty. Despite a growing economy and a boom in some sectors, such as construction, many of the existing firms we approached were temporarily holding off on growth plans. Other common sources of delay included difficulty in obtaining licenses, foreign exchange, importing equipment, and obtaining parts. At least two other firms intended to participate, but suffered prolonged delays and did not open their new line during the study period. Also, some sector growth was coming from new firms, often foreign-owned, who were reluctant to participate because their start-up was already complicated enough.

[13]They typically expressed interest in participation in the study for several reasons: curiosity in the answer; the opportunity to bring some structure to relatively unstructured hiring processes; and an interest in learning more about their applicant pool and the other opportunities available to their employees.

[14]Given the low-skill nature of the work and the entry-level positions, starting salaries were lower than the manufacturing average—at roughly the 25th percentile of manufacturers in the capital. Since most of our firms are outside the capital, the purchasing power of their wages is greater, probably putting them between the 25th and 50th percentile in terms of wages. Moreover, comparing wages to the distribution quoted in the 2009/10 census of manufacturing firms suggest that they were not at all uncommon for the specific sectors involved. The modal workers in the census earned between 400-600 birr in 2010 prices, with the second most common interval 200-400 birr, jointly making up 40 percent of the workforce in manufacturing in general and more than 60 percent in textiles or footwear (CSA, 2011). The wages of the workers in our sample fall in these ranges.

3.3.2 Sample recruitment and selection

We followed each firms' normal procedures for hiring batches of new employees to staff new production lines. The firms advertised jobs through a posting on the front gate, word of mouth, and local job boards.[15] Applicants were instructed to gather on a specific day. Firm managers would then screen written or verbal applications, typically based on job-specific gender, education, and health requirements. Across all 8 cohorts, between 75 and 95 percent of applicants passed these criteria and thus entered the study sample. We do not have data on ineligible applicants.

A research team from Innovations for Poverty Action (IPA) and the Ethiopian Development Research Institute (EDRI) then debriefed eligible applicants on the study, the start-up start-up arm arm, and the survey and randomization procedures. Nearly all agreed to enter the study, completed a baseline survey, and entered the lottery.

Following randomization, the firm posted the names of people receiving the job offer at the factory site and the IPA/EDRI research team contacted all those assigned to two treatment arms. Job offers began within a few days and the start-up training and grants within a few weeks.

We gave each firm a list of unsuccessful applicants and asked the firm not to hire them for at least 1–2 months. In practice, however, the firms kept poor records and within a few days or weeks of the randomization could have hired control group members.

3.4 Randomization and balance

We randomized by cohort, stratified by gender, using a uniform random variable generator. 304 were assigned to the job offer, 285 to the start-up grant arm, and 358 to the control arm.

Table 2 reports tests of randomization balance, where we regress each covariate on treatment indicators plus randomization block (cohort-gender) fixed effects. This sample is somewhat imbalanced across the treatment arms at baseline. As Table 1 shows, of the 34 covariates across two treatments, 8 of the 68 mean differences (12 percent) have $p < .1$. Those assigned to jobs are less likely to be married and have slightly lower executive functions and education compared to the control group. Those assigned to the entrepreneurship program

[15] In order to ensure sufficient applicants, we only made one change to standard procedures: we assisted the firm in posting more notices within a wider radius than usual (usually no more than a few kilometers). Since the firm typically drew employees from this radius, we expected this to generate an applicant pool very similar to the usual one. It is possible, however, that the experimental pool of applicants is further outside the family/friend network, and lives slightly further from the factory, than would otherwise have been the case. That said, most applicants live within a few miles of the firm, and so are extremely local by any measure. (The firms, who were reluctant to hire people who lived far away, reported that they did not think the distance would make a material difference, since all live nearby.)

have lower assets and more firm experience. A test of joint significance of all covariates has a p-value of 0.04 for the job offer and 0.01 for the entrepreneurship program. To minimize bias, we control for baseline covariates when estimating treatment effects.

3.5 Outcomes

3.5.1 1- and 5-year endline data

We conducted follow-up surveys roughly 1 and 5 years after assignment to the two treatments. At each of these two endlines, we attempted to reach each respondent twice, roughly 2–3 months apart, to measure our main outcomes twice. We did so to improve statistical precision with highly-variable outcomes such as earnings or consumption (McKenzie, 2012).[16]

During site visits to several dozen factories and commercial farms, we conducted informal interviews with workers and managers. At each study firm we systematically interviewed managers at every level from senior management to line managers. Research assistants also interviewed 138 workers and microenterprise owners, both in and out of the sample. They also conducted 60 exit interviews by phone with sample members who quit the study firms.

3.5.2 Attrition

Our sample frequently moved between survey rounds. We were able to track down 88% after 1 year and 84.3% after five.[17] Appendix Table A.1 reports the correlates of attrition after 5 years, from a simple regression of an attrition indicator on baseline covariates.

After 1 year, all treatment arms were roughly as likely to be found. After 5 years, those assigned to the job offer were no more likely to be found after five years than the control group. But those assigned to the start-up arm were roughly 5 percentage points more likely to be found and interviewed. Controlling for baseline covariates, this difference is not statistically significant, but it is potentially substantively important. Thus, below, we will consider the robustness of our estimates to alternative attrition scenarios and sensitivity analysis.

Otherwise, attrition is mostly uncorrelated with baseline characteristics. The exception is marriage, as unmarried individuals at baseline are 6.5 percentage points less likely to be found after five years. Women commonly move to become married, and this may account for their loss.

[16]For the 1-year survey, we also attempted to interview the household head once, since the sample member may have been a dependent and unaware of household labor allocations, wealth, and attitudes. At the 5-year survey, given that the sample is considerably older, we did not interview household heads.

[17]For discussion of the rates and pattern of attrition for the 1-year endline survey, see Blattman and Dercon (2018).

3.5.3 Primary outcomes and dealing with multiple outcomes

Based on the 1-year findings, our pre-analysis plan for the 5-year endline pre-specified two primary outcomes of interest: income and physical health. As secondary outcomes we specified an interest in occupational choice and quality. We designated all other outcomes as exploratory.[18]

We divided outcomes into primary and secondary to minimize the number of hypotheses tested. To further minimize comparisons, we assembled our various measures into a family index of income and a family index of health. Our tables report treatment effects on the components of these indexes as well, but those comparisons should be regarded as exploratory. At present we have not adjusted our p-values for multiple comparisons across the two primary measures or within these indexes.

3.6 Estimation

To estimate program impacts on outcome Y, we calculate the intent-to-treat (ITT) estimate of the job offer and start-up arms via OLS:

$$Y_{irj} = \alpha_j + \gamma_{r=13} + \theta_J Job_{ij} + \theta_E Startup_{ij} + \beta \mathbf{X}_{ij} + \epsilon_{ij} \quad (1)$$

where *Job* and *Startup* are indicators for random assignment to the treatment arms. To account for observed baseline imbalance and endline attrition, we control for the baseline covariates, X, listed in Table 1, as well as gender-cohort fixed effects, α_j. Recall that at each endline we surveyed respondents in two different rounds r, collecting the same outcome two times. Each round enters the regression as a separate observation, and we cluster standard errors by individual and include a fixed effect, $\gamma_{r=2}$, forthe second round.

Note that all outcomes are self-reported, and each treatment arm was aware of their assignment and the existence of other arms. Thus, there is the potential for self-reported outcomes to vary with treatment status. As with most low-income countries, there are no administrative data on earnings. And as with most countries, there are no systematic and available administrative data on health or informal earnings.

Table 3: Impacts on income and employment

Outcome	1-year Endline				5-year Endline			
	Control mean	N	ITT Estimate		Control mean	N	ITT Estimate	
			Job offer	Start-up			Job offer	Start-up
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Income and consumption, z-score	-0.005	1,587	0.016	0.139**	0.002	1,390	-0.042	-0.038
			[0.053]	[0.059]			[0.066]	[0.068]
Weekly earnings, 2010 Birr	34.227	1,586	3.036	12.156**	34.405	1,390	-3.308	-0.470
			[4.476]	[5.466]			[5.484]	[5.940]
Earnings per hour, 2010 Birr	1.562	1,019	0.073	0.126	1.451	763	-0.109	0.266
			[0.267]	[0.266]			[0.375]	[0.342]
SD of weekly earnings	58.150	1,587	6.472	4.653	56.412	1,390	1.131	3.876
			[7.673]	[8.139]			[8.950]	[9.986]
Household nondurable consumption, 2010 birr	665.049	1,584	20.363	59.320*	1,737.076	1,390	-33.448	-66.041
			[35.300]	[35.845]			[93.031]	[88.097]
Employment and occupational choice, z-score	-0.038	1,587	0.078	0.041	0.084	1,390	-0.080	-0.083
			[0.074]	[0.076]			[0.079]	[0.079]
Hours work/week, past 2 weeks	26.394	1,585	0.995	3.535*	26.773	1,390	-1.797	-1.960
			[1.894]	[1.892]			[2.048]	[2.042]
Factory labor	7.463	1,581	3.017**	-4.114***	6.132	1,390	0.560	-3.534***
			[1.380]	[1.168]			[1.423]	[1.220]
Farm wage labor	3.074	1,584	1.816**	-1.469**	0.440	1,390	1.037**	0.129
			[0.914]	[0.744]			[0.452]	[0.356]
Smallhoder farming	0.821	1,584	-0.258	1.480***	0.219	1,390	-0.001	0.130
			[0.323]	[0.398]			[0.116]	[0.143]
Petty business	4.037	1,586	-0.877	5.378***	5.969	1,390	-1.705	-0.563
			[0.977]	[1.378]			[1.318]	[1.297]
Skilled trades	1.592	1,583	-0.737	-0.570	3.134	1,390	-1.483**	-1.403*
			[0.449]	[0.483]			[0.633]	[0.779]
Casual nonfarm labor	2.180	1,586	-0.952*	0.726	0.812	1,390	0.626	0.872
			[0.568]	[0.770]			[0.467]	[0.546]
Low-skill salaried labor	4.187	1,586	0.064	-0.410	3.761	1,390	-0.395	-0.164
			[1.095]	[0.955]			[0.932]	[0.945]
Hrs Medium-skill salaried labor	1.209	1,586	-0.415	1.610***	3.253	1,390	0.972	3.331***
			[0.419]	[0.590]			[0.863]	[0.980]
Other work	2.268	1,439	-0.094	0.493	2.257	1,390	-0.886	-0.784
			[0.693]	[0.737]			[0.577]	[0.593]
No work in past two weeks	0.343	1,586	-0.013	-0.082**	0.405	1,390	0.001	-0.033
			[0.033]	[0.032]			[0.040]	[0.039]
SD of hours/week	16.444	1,586	-1.307	3.956***	11.841	1,390	1.378	2.517
			[1.342]	[1.476]			[1.515]	[1.594]

* indicates $p < .1$, ** indicates $p < .05$, and *** indicates $p < .01$. Regression estimates are calculated with district and cohort fixed effects Standard errors are clustered at the level of the respondent due to having two observations per person. All observations are weighted by the inverse of their sampling probability and the inverse of their predicted probability of attrition using a Leave-One-Out logistic predictive method. Control means are also calculated using these weights.

4 Results

4.1 Economic impacts

Table 3 reports 1- and 5-year ITT estimates for measures of income and employment levels. Figures 1a and 1b in the introduction trace the earnings and employment results over time, and this table expands the range of measures and components.

Income and employment levels Our primary economic outcome is income. We have two measures of income, pre-specified, that we combine into an income family index. One is the sum of weekly cash earnings across 22 different occupations. Earnings are seasonal and do not reflect home production, so we also consider a measure of non-durable consumption in the previous 4 weeks via an abbreviated consumption module of 82 items.[19]

In the start-up grant arm, we see a striking short-run divergence followed by convergence within 5 years. Income increases 0.16 standard deviations after one year (including a one third increase in reported earnings and a nearly 10% increase in consumption), but there is virtually no income effect after 5 years. This temporary income effect from the grant was driven mainly by increased hours of work, almost all through the channel of self-employment in retail trades. We see little difference in earnings per hour reported worked. This increase in employment also disappeared after 5 years.

In contrast, we see no evidence of an effect of a factory job offer on income or total hours of work after 1 or 5 years.

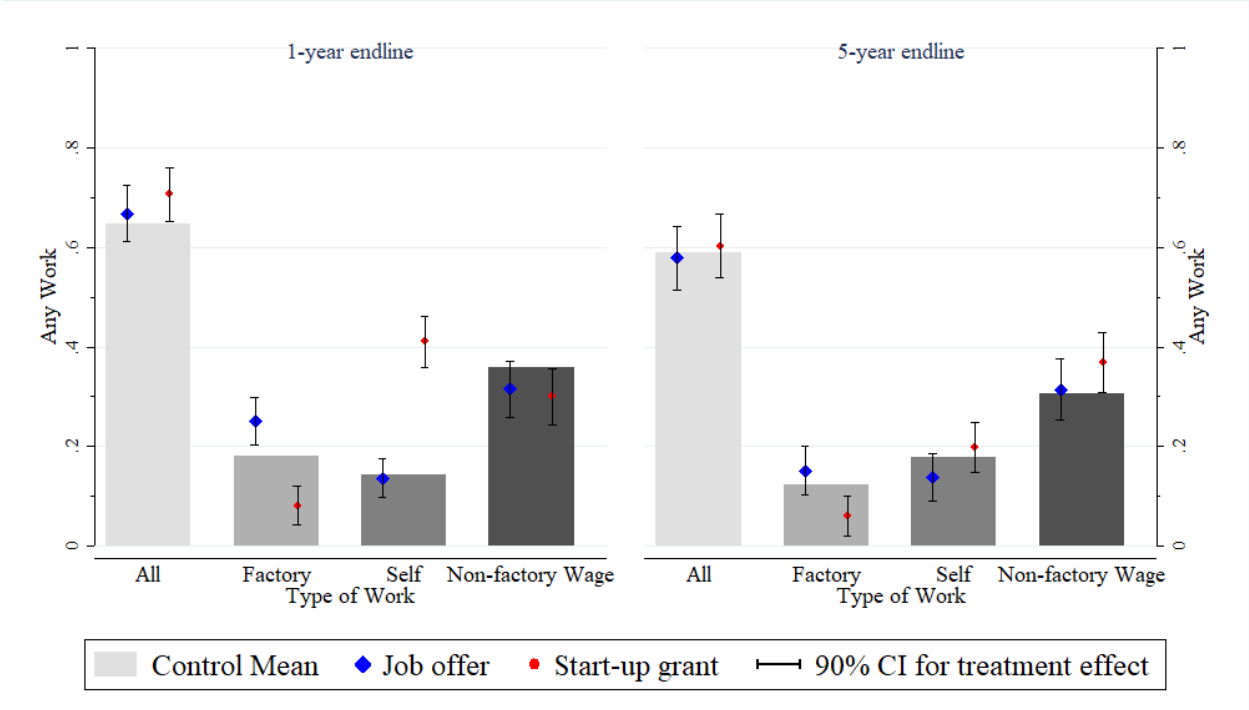
Unemployment and occupational choice Recall from Figures 1a and 1b above that both incomes and hours of employment rose steeply in the first year. They stagnated or fell, however, in the subsequent 4 years. Figure 2 looks at employment and occupational choice on the extensive margin (pre-specified as secondary outcomes of interest). The probability of being employed fell slightly between the 1 and 4 year endline. One reason is that a few young women exited the labour force. Others who are unemployed report looking for employment.

Examining trends in the control group gives insight to the life-cycle effects of occupational choice in our sample. While slightly more individuals entered self employment between the 1 and 5 year surveys, this increase in employment was counteracted by exit from factory and non-factory wage jobs.

[18] See `https://www.socialscienceregistry.org/trials/2198`

[19] See Beegle, De Weerdt, Friedman, and Gibson (2012) for this approach. This abbreviated measure likely understates total consumption by excluding durable asset use and less common purchases. Note that we also pre-specified a third measure of income, an index of almost two dozen durable assets. Due to a survey programming error, however, these durables data were mistakenly not collected in the 5-year endline.

Figure 2: Employment status and program impacts for work in past 2 weeks after 1 and 5 years



The start-up grant initially had a strong effect on self employment. After 1 year, 41% of this arm were engaged in self-employment. In fact, 78% of the start-up arm attempted self-employment at any time over five years, and most of this experimentation took place within the first year of receiving the treatment (Table 4). There are some lingering effects of this after 5 years. The start-up grant arm are still slightly more likely to be self-employed (not statistically significant). But the fall from 1-year levels of self-employment is precipitous, suggesting that even this occupational choice impact may be converging.

This finding runs counter to the notion that individuals in our sample would be successful in entrepreneurial work if they could only overcome barriers to starting a small business. The start-up arm induces significantly higher levels of experimentation with self-employment employment. But the large number of people who were induced to start small enterprises do not appear to have stuck with them five years later.

One of the other patterns we see across the five years of the study is rapid exit from factory work. To understand the rapid rate of exit from factory jobs, we begin with evidence from Blattman and Dercon (2018), where we analyzed the first year of qualitative data and patterns in the panel. A few findings stand out:

- We saw no evidence of an industrial wage premium in our five firms. A simple non-experimental wage comparison suggested that industrial jobs seemed to pay almost a

Table 4: Effect on time spent in factory and self-employment work since baseline

Outcome	Control mean (1)	N (2)	ITT Estimate Job offer (3)	ITT Estimate Start-up (4)
Any factory work since baseline	0.413	725	0.284***	-0.212***
			[0.044]	[0.043]
Months of factory work since baseline	8.794	725	2.898**	-5.046***
			[1.474]	[1.263]
Months of factory work by 1-year follow up	1.884	672	1.680***	-0.987***
			[0.346]	[0.276]
Months of factory work in 3-years between surveys	5.145	725	1.558	-2.958***
			[1.108]	[0.922]
Months of factory work in year before 5-year follow-up	1.566	725	0.172	-0.891***
			[0.373]	[0.326]
Any self-employment since baseline	0.439	725	0.000	0.344***
			[0.046]	[0.042]
Months of self-employment since baseline	8.242	725	-2.788**	4.056***
			[1.376]	[1.499]
Months of self-employment by 1-year follow up	1.094	691	-0.542**	2.428***
			[0.263]	[0.394]
Months of self-employment in 3 years between surveys	5.103	691	-1.734	2.223*
			[1.065]	[1.169]
Months of self-employment in year before 5-year follow up	2.273	725	-0.661	-0.426
			[0.430]	[0.409]

* indicates $p < .1$, ** indicates $p < .05$, and *** indicates $p < .01$. Regression estimates are calculated with district and cohort fixed effects Standard errors are clustered at the level of the respondent due to having two observations per person. All observations are weighted by the inverse of their sampling probability and the inverse of their predicted probability of attrition using a Leave-One-Out logistic predictive method. Control means are also calculated using these weights.

quarter lower wages than informal opportunities.

- Industrial work came with more stable employment hours, though only modestly so. Most members of the control arm were able to find full-time informal work by the time of the 1-year endline. Informal work also tended to pay higher wages than the industrial firms, but it typically came with the risk of short unemployment spells. Over the horizon of a month or a year, however, earnings in the industrial sector were no more stable than the alternatives.

- A third of people offered an industrial job quit the study firm in the first month, and 77 percent quit within the year. People generally quit the sector altogether, rather than simply switch firms. Firm managers said they found the high levels of turnover inconvenient, but were generally able to fill the positions with other low-skill workers.

- Qualitatively, our interviews suggested that young people used low-skill industrial jobs more as a safety net than a long-term job, and where self-employment and informal work were typically preferred to, and more profitable than, industrial jobs.

What do we see in the subsequent four years? First, looking again at Figure 2, participation in factory work has declined in the control group between the 1- and 5-year endlines, falling from 18% to below 12% at the extensive margin. In other words, the high rate of exit from factory jobs continued after year 1, such that the job-offer individuals are not significantly more likely to be in factory jobs five years later.

Even though few are employed in factories at the 5-year endline, the control group continued to experiment with factory work at a similar pace as they did in the first year of the study. (As Table 4 shows, the control group had on average 2 months of factory work by the one year follow up, and 5 months by the five-year follow up.)

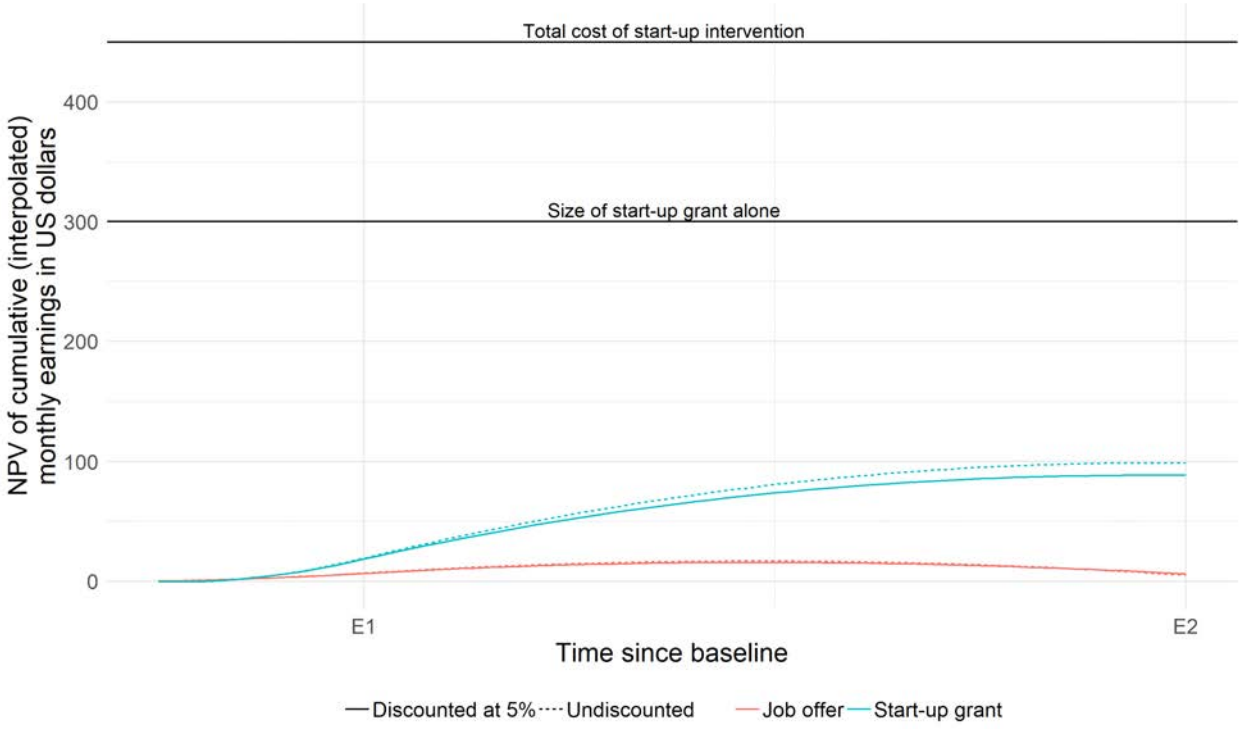
Even after 5 years, the job offer results in nearly 3 additional months of lifetime experience in factories – a one third increase over the control group mean. About half of this gain comes from the first year after the offer. About half comes in the subsequent 3 years (not statistically significant). The small but steady exposure of the control group to factory work bolsters the earlier interpretation that these are unpleasant jobs that our sample used as a last resort.

We also see that the start-up grant deters people from sampling factory work, to some extent. After 5 years, the start-up arm are significantly less likely to be engaged in factory work, and have only a third as many months of cumulative factory experience. We can also interpret this as consistent with unemployed workers seeking factory work as a last resort in

times of need. The start-up grant had higher incomes after 1 year, allowing them to avoid factory work for a time.

Finally, we see no evidence that these spells of factory employment have effects on long term incomes or hours of work. Appendix Table A.3 reports the complier average causal effect of assignment to the job offer or start-up grant, where we use assignment to treatment as an instrument for length of time in the industrial sector. That is, we use the estimates in Table 4 as a "first stage". This instrumental variables (IV) estimate is useful for understanding whether a longer spell of past employment in the industrial sector has long term effects on outcomes such as income and employment. For instance, the spell could lead to experience, social networks, or shocks that improve or hinder future employment prospects. The instrument is weak by the 5-year endline, since the cumulative effect on months of employment has fallen to 3 months (a 33% increase). This contributes to noisy IV estimates. Nonetheless, we see nothing to suggest that employment or income prospects improve with longer spells of factory employment. If anything there is a small but noisy adverse effect on incomes and employment levels.

Figure 3: Net present value of intervention according to treatment effects on income



Cost-benefit analysis Figure 3 shows the net present value of both the job offer and the start-up grant interventions. In each survey round – baseline, 1-year, and 5-year – we report

the treatment effects of the interventions on earnings and linearly interpolate between them. We take the cumulative sum of such treatment effects across all 5 years and discount the sum at a 5% annualized rate. This gives us a relatively generous estimate of the net present value.

The figure shows how far the start-up grant intervention is from being a cost-effective program of poverty reduction, in comparison to its $300 grant amount and $450 total operating cost. As noted in Blattman and Dercon (2018), the effects on earnings of the start-up grant after 1 year amounted to only 16% of the total $300 grant amount.

Table 5: Impacts on health

	1-year Endline				5-year Endline			
	Control		ITT Estimate		Control		ITT Estimate	
Outcome	mean	N	Job offer	Start-up	mean	N	Job offer	Start-up
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Physical health, z-score	0.058	1,587	-0.193***	-0.098	0.011	1,390	-0.023	0.103
			[0.066]	[0.062]			[0.069]	[0.075]
Ability to do 5 core activities of daily life (0–15)	14.072	1,587	-0.274**	-0.240*	13.485	1,390	-0.201	-0.036
			[0.125]	[0.128]			[0.196]	[0.198]
Ability to do 15 activities of daily life (0-45)					39.212	1,390	-0.424	0.299
							[0.501]	[0.548]
Disability	0.040	1,587	0.033**	0.017	0.020	1,390	0.009	0.010
			[0.015]	[0.014]			[0.011]	[0.014]
Subjective health assessment (0–10)	8.909	1,586	-0.233**	0.001	8.774	1,387	0.089	0.136
			[0.104]	[0.104]			[0.106]	[0.102]
Subjective health assessment, 5 years from now (-10 to 10)	0.760	1,586	0.055	-0.001	0.096	1,387	-0.066	0.134
			[0.093]	[0.091]			[0.084]	[0.082]
General health (0-60)					56.809	1,390	0.186	0.464
							[0.407]	[0.359]
Physical sympton count (1-5)					0.204	1,390	-0.029	-0.009
							[0.033]	[0.033]
Abnormal Spirometry Reading (colour system)					0.025	532	0.019	-0.004
							[0.021]	[0.020]
Mental health and subjective well-being, z-score	-0.110	1,587	0.072	0.233***	0.016	1,390	-0.088	0.029
			[0.071]	[0.065]			[0.077]	[0.066]
Depression symptoms (0–27)	2.545	1,587	-0.088	-0.281	2.283	1,390	0.261	-0.191
			[0.219]	[0.211]			[0.283]	[0.248]
Generailized Anxiety index (0–27)	2.028	1,587	0.054	-0.284	1.933	1,390	0.313	-0.013
			[0.197]	[0.183]			[0.251]	[0.218]

* indicates $p < .1$, ** indicates $p < .05$, and *** indicates $p < .01$. Regression estimates are calculated with district and cohort fixed effects Standard errors are clustered at the level of the respondent due to having two observations per person. All observations are weighted by the inverse of their sampling probability and the inverse of their predicted probability of attrition using a Leave-One-Out logistic predictive method. Control means are also calculated using these weights.

4.2 Health impacts

One year after receiving the job offers, we found evidence of reduced health outcomes, as measured by self-reported ability to perform activities of daily life, among those who were assigned to the factory job. Did these persist?

For the long-run data collection, we collected an expanded set of health measures from the first endline survey. First, we used an expanded list of fifteen activities to improve

our measurement of ability to perform activities of daily activities, ranging from 0 to 45. Second, we conducted a comprehensive questionnaire covering a list of twenty symptoms of ill health. Here respondents could report the regularity with which they experienced these physical symptoms from zero (never) to three (often), without having to be diagnosed with a particular condition. Third, we asked about four specific health conditions that are particularly common among individuals working in industrial work, namely: asthma, respiratory problems, dermatitis, and carpal tunnel. Fourth, to more accurately measure respiratory health, and verify our self-reported measures, we conducted a spirometry procedure. Here we followed guidelines from the European Respiratory Society, reported in Moore (2012). For the main results we report on indicator for whether the spirometry test shows an abnormal reading, indicating a respiratory problem.

In the pre-analysis plan we committed to report the effect of the two treatments on these four aggregate measures. Table 5 shows these results. We find that these negative health effects do not persist. Health outcomes are not significantly different for either treatment group across a wide range of measures.

There are two possible interpretation of these findings. The first is that the maladies experienced one year after receiving the factory job offer were not chronic. The second is that the control group was exposed to hazardous factory work in the interim and have experienced the same health problems as a result. The evidence favors the first explanation.

First, in absolute terms, the sample is in good health, perhaps even slightly improved over time. Serious disability rates after 5 years are half of what they were after 1 year, suggesting many of the problems reported in the previous endline were temporary.

Second, we see no evidence that those who stayed longer in factory work have poorer health. Appendix Table A.4 reports complier average causal effects using assignment to treatment as an instrument for length of time in the industrial sector (see Table 4). After 1-year, these IV estimates showed serious adverse effects on health. After 5 years we see no such evidence. While it is true that the instrument is considerably weaker after 5 years, the IV estimate for physical health or serious disabilities is close to zero, and the confidence interval does not include the 1-year point estimate.

5 Discussion and conclusions

We report on the labor market trajectories of almost 1000 young and mostly unemployed women in Ethiopia. We also report on two interventions that many have reasonably proposed could have had large and lasting effects on long run labor market outcomes: among people appearing to queue for industrial employment, offering an entry-level factory job in a country

that is just beginning to industrialize; and offering approximately one year of factory wages as a cash grant plus some business training.

We learn both from the descriptive analysis of the panel and the experiments. We find, after five years, neither intervention has any effect on the likelihood to still work that sector after 5 years, compared to the control group (although the start-up grant group were less likely to experiment with factory employment). Earnings and consumption are also no different in any of these groups. The labour market choices and outcomes for our population have roughly the same structure and earnings that the treated would have had without the intervention.

This is doubly important because of the promising short-term evidence that the start-up grant increased productivity and earnings by a third. Within 5 years, however, these productivity gains seem to have dissipated.

There are reasons to believe these (mostly) young women did not have high sustained returns to self-employment relative to non-industrial wage work. The businesses may simply have failed or faded away gradually over time, as the women failed to reinvest earnings in the business. It is also possible that the business was simply a form of savings and consumption smoothing in an economy where the real interest rate from cash savings is as low as -15--20% due to high inflation and the high cost of local savings institutions (which offer negative nominal interest rates). Investing in a small enterprise may simply be the most efficient way of maximizing consumption of a grant. For instance, at a real interest rate of -15%, a 5000 birr grant could be consumed completely in 5 years by spending 54 birr per month. But at a real interest rate of 5% for example (supposing this is what a business or other savings vehicle could earn), a 5000 birr grant could provide a monthly payment of 94 per month over the same period, almost double.

Plausibly, the number of constraints on entrepreneurial success in this setting meant that lifting a single, marginal constraint such as investment capital is not enough to have a lasting return for this particular group of young mainly female workers. Given that many of the microenterprises folded, we suspect that low returns to capital given other barriers is a likely factor. Of course, it is also possible that our sample – drawn from a population queuing for a factory job – are per definition not terribly representative, nor full of entrepreneurial talent. Still, evidence points to a large number of them regularly engaging in some self-employment as a temporary alternative to wage work.

Our results also bolster a view that industrial work is not particularly high-quality, and not particularly skilled and high-paying (at least at this stage of development in Ethiopia). Wages were no better than in other low-paid sectors, jobs were unpleasant and seemingly hazardous, and (most worryingly) those that spent more months in factory work reported more

serious health problems after one year. Nevertheless, for some it offered another employment option seemingly worth taking, at least for some time during the five years studied.

Naturally there are limits to what we can learn from five sites and five firms. Yet the same is true of any single program evaluation. Our start-up program is comparable in many respects to a suite of anti-poverty programs that give youth start-up capital, and the industrial results speak to low-skill light industry in contexts where workers are effectively disposable to firms. This is also a reasonable description of early and middle-stage industrialization in the US, Europe, and Asia.

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Online appendix

Table A.1: Attrition

	Attrition and Non-Response			
	Only FE		All Covariates	
Dependent Variable: Never found at 5-year endline	Beta	Std. error	Beta	Std. error
Fraction of sample not found: .2	(1)	(2)	(3)	(4)
Job offer	0.003	0.032	0.014	0.034
Start-up	-0.054	0.031*	-0.047	0.032
Age			0.648	1.347
Age squared			-0.039	0.079
Age cubed			0.001	0.002
Age quartic			-0.000	0.000
Female			0.110	0.096
Unmarried			0.044	0.034
Muslim			0.231	0.071***
Household Size			0.009	0.008
Household head			0.055	0.039
Proportion of household dependents			-0.034	0.026
Numeracy score			-0.003	0.006
Total years of education and training			-0.001	0.008
Cognitive function score			-0.022	0.019
Executive function, z-score			0.039	0.018**
Completed secondary school			0.003	0.046
Weekly cash earnings, 2010 Birr			0.000	0.001
Durable assets, z-score			-0.017	0.020
Wealth index: Production Durables (Z-Score)			-0.036	0.015**
Total debt			0.000	0.000
Total savings			0.000	0.000*
Average weekly work hours (over past 2 weeks) :			0.003	0.005
Hrs work/wk, cas non-farm labor			0.002	0.003
Hrs work/wk, factory			-0.002	0.004
Hrs work/wk, petty business			0.001	0.002
Hrs work/wk, skilled trades			-0.002	0.004
Hrs work/wk, low skill sal labor			-0.002	0.002
Hrs work/wk, med skill sal labor			0.000	0.000
Hrs work/wk, other work			-0.001	0.003
Did no work in the past 2 weeks (formal and informal)			0.003	0.060
Ever worked in a large firm			0.006	0.040
Months experience: Agriculture			0.000	0.000
Months experience: Casual work			-0.002	0.001
Months experience: Factory labor			-0.001	0.002
Months experience: Petty business			-0.000	0.001
Months experience: Skilled trade			-0.002	0.001**
Months experience: Wage labor - low skill			0.000	0.001
Months experience: Wage labor - medium skill			0.003	0.006
Months experience: Other			0.000	0.002
Years experience, private firm			-0.015	0.014
Years experience, workshop			-0.105	0.084
Years experience working in state/parastatal org			0.022	0.035
Years experience working in NGO			-0.023	0.063
Self perception of health			-0.002	0.006
Probability of a better job, next month			-0.034	0.064
Probability of full-time work, next month			0.064	0.062
Predicted income uncertainty in next year			0.000	0.000
Predicted income uncertainty in next month			-0.000	0.000
Highest - lowest earnings, past month			-0.000	0.000
Could borrow 3000 birr			0.058	0.033*
Family relations index, z-score			0.025	0.015*
Friends and nieghbors relations index			-0.027	0.015*
Ability to do activities of daily life (0-15)			0.002	0.012
Disability: Great difficulty at more than 1 activities			0.219	0.190
Change in subjective well being, past yr.			-0.009	0.013
Symptoms of depression, z-score			-0.008	0.019
Symptoms of anxiety, z-score			0.015	0.020
Risk aversion from IBM games			0.004	0.015
Risk aversion score			0.008	0.011
Patience score from IBM games			-0.009	0.017
Time inonsistency score			0.013	0.022
Patience index			0.022	0.009**
Locus of control index			-0.008	0.015
Self esteem index, s-score			0.023	0.016
Self control index			-0.015	0.014
Agressive or hostile behaviors, z-score			0.016	0.014
Conscientiousness index, z-score			-0.025	0.016
P-value of F-test	0.1220		0.0011	
N	947		947	

* indicates $p < .1$, ** indicates $p < .05$, and *** indicates $p < .01$

Regression estimates use the full sample of baseline respondents with cohort and district fixed effects.

Table A.2: Impacts on child outcomes

Outcome	Control mean (1)	N (2)	ITT Estimate Job offer (3)	ITT Estimate Start-up (4)
Panel A: Fertility outcomes				
Number of pregnancies	0.409	1,390	-0.015	0.017
			[0.040]	[0.040]
Number of children alive	0.354	1,390	-0.005	-0.007
			[0.038]	[0.036]
Fraction of pregnancies ending in a live birth	0.864	336	0.015	-0.058
			[0.041]	[0.047]
Franction of pregnancies where the child is still alive	0.862	336	0.015	-0.058
			[0.041]	[0.047]
Panel B: Health outcomes				
Child age-normalized health index of child	-0.040	243	0.106	0.187
			[0.196]	[0.195]
Age-normalized subjective parent health assessment of child	-0.065	309	0.168	0.168
			[0.123]	[0.142]
Child age-normalized number of times with malaria in past year	-0.118	304	0.188	0.371**
			[0.141]	[0.180]
Child age-normalized Activities of Daily Life index	-0.064	244	0.146	0.265
			[0.196]	[0.196]
Ability to dress themselves	0.010	264	0.059	0.080
			[0.182]	[0.168]
Ability to feed themselves	-0.039	264	0.095	0.142
			[0.192]	[0.211]
Ability to use the toilet	0.012	256	-0.043	-0.177
			[0.180]	[0.201]
Ability to wash themselves	-0.039	278	0.115	0.155
			[0.156]	[0.164]
Aility to bend over	-0.042	286	0.132	0.196
			[0.157]	[0.170]
Ability to say their name	-0.143	244	0.117	0.411*
			[0.201]	[0.240]
Ability to walk	-0.014	268	0.105	0.206
			[0.179]	[0.180]
Ability to run	-0.022	246	0.030	0.274
			[0.184]	[0.187]
Panel C: Education outcomes				
Child age-normalized school and daycare enrollment	-0.066	250	0.057	0.484**
			[0.157]	[0.205]
Child age-normalized educational attainment	-0.017	226	-0.043	-0.126
			[0.151]	[0.180]

Table A.3: Instrumental variables approach to the effect of a month's factory work on income

F-statistics of first stage: 1-year Endline: 44 5-year Endline: 4	Months of factory work since baseline			
	1-year Endline		5-year Endline	
Outcome	Control mean	Effect	Control mean	Effect
	(1)	(2)	(3)	(4)
Income and consumption, z-score	-0.00	-0.017	0.00	-0.030
		(0.037)		(0.031)
Weekly earnings, 2010 Birr	34.23	-1.767	34.41	-3.442
		(2.335)		(2.906)
Earnings per hour, 2010 Birr	1.56	-0.100	1.45	-0.167
		(0.084)		(0.156)
SD of weekly earnings	58.15	1.851	56.41	0.233
		(3.345)		(2.935)
Household durable consumption assets, z-score	0.07	-0.028		
		(0.030)		
Household nondurable consumption, 2010 birr	665.05	18.085	1,737.08	-7.813
		(29.729)		(38.050)
Household durable productive assets, z-score	-0.12	0.018		
		(0.030)		

Table A.4: Instrumental variables approach to the effect of a month's factory work on health

F-statistics of first stage: 1-year Endline: 43 5-year Endline: 4 Outcome	Months of factory work since baseline 1-year Endline Control mean (1)	 Effect (2)	 5-year Endline Control mean (3)	 Effect (4)
Physical health, z-score	0.06	-0.082	0.01	-0.008
		(0.035)		(0.029)
Ability to do 5 core activities of daily life (0–15)	14.07	-0.125	13.49	-0.070
		(0.069)		(0.089)
Ability to do 15 activities of daily life (0-45)			39.21	-0.073
				(0.209)
Disability	0.04	0.012	0.02	0.003
		(0.008)		(0.004)
Subjective health assessment (0–10)	8.91	-0.090	8.77	0.029
		(0.061)		(0.043)
Subjective health assessment, 5 years from now (-10 to 10)	0.76	0.009	0.10	-0.013
		(0.053)		(0.035)
General health (0-60)			56.81	-0.033
				(0.203)
Physical sympton count (1-5)			0.20	-0.003
				(0.014)
Abnormal Spirometry Reading (colour system)			0.03	0.008
				(0.008)
Mental health and subjective well-being, z-score	-0.11	0.018	0.02	-0.034
		(0.043)		(0.037)
Depression symptoms (0–27)	2.54	-0.024	2.28	0.143
		(0.136)		(0.146)
Generailized Anxiety index (0–27)	2.03	0.125	1.93	0.085
		(0.124)		(0.114)