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The impacts of industrial and entrepreneurial work on income and health: Experimental evidence from Ethiopia*

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Abstract

Working with five Ethiopian firms, we randomized applicants to an industrial job offer, an "entrepreneurship" program of $300 plus business training, or control status. Industrial jobs offered more and steadier hours but low wages and risky conditions. The job offer doubled exposure to industrial work but, since most quit within months, had no impact on employment or income after a year. Applicants largely took industrial work to cope with adverse shocks. This exposure, meanwhile, significantly increased health problems. The entrepreneurship program raised earnings 33% and provided steadier hours. When barriers to self-employment were relieved, applicants preferred entrepreneurial to industrial labor.

JEL codes: J24, O14, F16, J81, O17

Keywords: wage labor, factories, employment, entrepreneurship, cash transfers, field experiment

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1 Introduction

"The misery of being exploited by capitalists" the economist Joan Robinson famously remarked, "is nothing compared to the misery of not being exploited at all" (1962, p.45). This reflects a common view that even low-wage industrial jobs provide poor people with higher and more stable earnings than their alternatives.[1]

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 minimum wages, labor codes or union bargaining.[2] 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.[3]

Empirically, a large body of observational evidence suggests that formal firms pay premium wages, especially large, foreign-owned, or exporting firms.[4] 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, and the health of children.[5] More anecdotally, it is also common to see long queues of applicants for factory jobs. By revealed preference, it would seem that workers who line up prefer these so-called sweatshops to their alternatives.[6]

Industrial jobs may be attractive only compared to poor people's largely informal alternatives. Summing up the case for sweatshops, Paul Krugman (1997) wrote, "the wages of those workers are shockingly low but nonetheless represent a vast improvement on their previous, less visible rural poverty." Even some opponents concede the point: a Scholars Against Sweatshop Labor statement (2001) admits that "after allowing for the frequent low wages and poor working conditions in these jobs, they are still generally superior to 'informal'

[1] We use the term "industrial" jobs to capture work in formal firms in manufacturing and high-intensity commercial agriculture, focusing on formal firms with 10 or more employees (i.e. excluding very small enterprises and workshops).

[2] e.g. (Katz et al., 1989; Akerlof and Yellen, 1986) on efficiency wages and (Card, 1996) on unions.

[3] Lewis (1954); Harris and Todaro (1970); Fields (1975).

[4] For Mexico see Bernard et al. (2010); Verhoogen (2008) and for Africa see Söderbom and Teal (2004); El Badaoui et al. (2008). Plant-level data show that large and foreign firms pay higher wages than smaller or domestically-owned ones (see Aitken et al. (1996) for Venezuela and Mexico, Lipsey and Sjöholm (2004) for Indonesia, and Strobl and Thornton (2004) for five African countries). The correlation tends to remain but decrease when firm and worker characteristics are taken into account.

[5] Kabeer (2002) and Hewett and Amin (2000) provide evidence that working in textiles factories is associated with higher female status and quality of life. Atkin (2009) finds that Mexican women in export manufacturing increase their incomes, have more household bargaining power, and have taller children. Getahun and Villanger (2016) find that flower farm work for poor rural women in Ethiopia increases their bargaining power.

[6] "Sweatshop" jobs usually imply low wages, long hours, and unpleasant or hazardous working conditions. The firms in this paper arguably fit this broad definition, but since this is a subjective judgment we refer to them as low-skill industrial jobs.

employment in, for example, much of agriculture or urban street vending."

It is not clear, however, that wage premiums exist in general, or that steady employment makes up for the disamenities of industrial work. Some would argue that factory jobs, much like fast food positions, are no better or worse than other low-skill alternatives, such as agriculture or street vending. In standard theories of competitive markets, factor prices are equalized across sectors and firms. If unskilled and interchangeable, poor workers will be paid their low reservation wage. In this case, any queues outside factories simply reflect normal search activities, and any formal sector wage premiums in the observational data reflect unobserved heterogeneity.[7]

A more pessimistic view is that workers make poorly-informed or present-biased decisions that sacrifice health or schooling, reducing long-run earnings potential. Looking at the rise of Mexican maquiladoras, for instance, Atkin (2012) observed that as firms and wage opportunities grew, young adults dropped out of high school to take the job, sacrificing a large amount of long-run earnings—a sacrifice inconsistent with even fairly high discount rates. New industries may also pose health risks that neither the firm nor worker fully understands.

In the long run, a rising industrial sector should raise demand for labor and thus improve wages and working conditions across the economy. But at the early stages of industrialization (before firms face tighter labor supply and must compete for qualified workers) it is unclear what opportunities and risks industrial jobs offer to workers relative to their informal opportunities.

This paper uses a case study and small-scale experiment in Ethiopia to investigate a set of questions: What are the relative qualities of informal and industrial work at this early stage of industrialization? Are there benefits or risks to the choice of one occupation over the other? And how does the quality of self-employment options affect this occupational choice?

We expected industrial jobs to be desirable and provide at minimum more steady incomes, and possibly even some form of rents. Instead, we found that jobs in these firms look much more like fast food positions, and are not that desirable. If anything, they are undesirable and risky to people's health. Meanwhile, informal opportunities over time proved to be much better than we anticipated.

We follow a panel of nearly 1000 applicants to entry-level jobs in five industrial firms in different parts of Ethiopia, reporting results over the first year of the study. We randomly assigned roughly a third of the applicants to a job offer, a third to a control group, and a third to

[7]For instance, firms may screen, select, and retain suitable workers that, to researchers, appear observationally equivalent (Rosenzweig, 1988).

an entrepreneurship program intended to improve their informal employment opportunities. A wave of "entrepreneurship" and "social protection" programs have increased poor people's incomes by giving them cash, livestock, skills, and other inputs into self employment. These inputs relieve binding constraints and help the poor expand informal self-employment and raise their earnings (Banerjee and Duflo, 2011; Blattman and Ralston, 2015). Our program provide five days of business training and planning, followed by $300 grant (about $1030 in purchasing power parity, or PPP).[8]

Ethiopia is a growing export hub in horticulture, textiles, and leather, and although the economy is 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 (e.g. NPR, 2014). We approached hundreds of firms about the experiment. A majority were willing to participate in the study, but only a handful were eligible in the sense of hiring dozens of low-skilled workers at one time. In the end we worked with five firms: a water bottling plant, a vegetable farm, a flower farm, a shoe manufacturer, and a textile and garment factory.

The people applying to these low-skill, entry-level jobs were generally unemployed but educated young adults, mainly women, who lived nearby the firms. Most had had no work in at least a month, and outside the factories most local work opportunities were informal.

The comparison between the job offer and control arms indicates how the industrial job compares to the workers' typical alternatives. The entrepreneurship arm, meanwhile, was designed to allow us to compare industrial work to the income trajectory, health, and other dimensions of better informal employment.[9] The point is not to juxtapose a grant against a job (since a grant is unlikely to be a worker's counterfactual) but rather simulate self-employment opportunities under reduced capital constraints.

There are limits to what we can learn from five firms. Yet the same is true of any single program evaluation. Our results speak to low-skill light industry in contexts where workers are effectively disposable to firms. This is a common feature of light manufacturing and agribusiness in Ethiopia, and we discuss how our results are consistent with patterns in more representative panels of young, unskilled Ethiopian workers and job searchers, plus (to some extent) early and middle industrialization in the US, Europe, and Asia.

Besides a randomized design, this situation also offers the opportunity to measure a range of outcomes over time, including physical and mental health. Previous studies have had larger samples but have relied on a narrow set of outcomes. Hence the health and other non-pecuniary consequences of different occupations is not known.

[8]This conversion is based on 2011 International Comparison Program data from the World Bank.

[9]Industry could have broader impacts that this design will not capture, such incentives for prospective employees to gain education. Heath and Mobarak (2014) show that growth in the Bangladeshi textile industry increases girls' age at marriage and educational attainment.

Beforehand, we expected the offer of an industrial job not only to end a potentially lengthy unemployment spell, but also increase the probability these young people took permanent employment in the industrial sector. We also expected that unsuccessful applicants could still get work in industry, of course, but to the extent these jobs were scarce, or to the extent other informal opportunities could arise before the next industrial job opportunity, we expected the randomized job offer to shift people's employment trajectories.[10]

Instead, over the following year, the results painted a different picture: one where these 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, at least when people had access to capital. More worryingly, this industrial safety net seems to have come with serious health risks.

First, looking at the cross-sectional and panel data, we see no evidence of an industrial wage premium in these firms. A simple non-experimental wage comparison suggests that industrial jobs seemed to pay almost a quarter lower wages than informal opportunities.

Second, industrial work was associated with more stable employment hours, though only modestly. Whereas the sample had an average of 7.5 hours of work a week in the month before baseline, most were able to find full-time informal work by the time of the endline. Informal work also tended to pay higher wages than the industrial forms, 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.

Third, turnover in the industrial jobs was high, in part because higher-wage informal opportunities arose over time. A third of people offered a job quit the study firm in the first month, and 77% 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.

Another reason people said they quit was that they found industrial work unpleasant and hazardous to their health. Those who quit described the jobs as difficult, poorly paid, and rigid. They also described a number of hazards, including chemical exposure.

Turning to the experimental impacts, our results suggest that, a year after job offers, the economic gains from industrial work were negligible and the health risks substantial. But the entrepreneurship program raised incomes by a third without significant health costs.

First, the main impact of the job offer is to increase someone's *exposure* to industrial work during the year: from 3 months in the control group to 6 months in job group. But because

[10]This push to formal employment, for instance, could not only reduce the uncertainty and volatility in their incomes and employment, but potentially improve their long-term employment and earnings potential through the experience and other human capital acquired in formal firms, or the potential to earn efficiency wages and other wage premiums.

of the high quit rates, the job offer only had a modest effect on long-term employment at the end of the year. At endline only 32% of the job arm was employed in any factory or commercial farm, compared to 20% of the control group. Partly because of the high quit rates, but also because industrial jobs offered more hours at lower wages, we see no evidence that the job offer increased incomes on net.

Receiving the entrepreneurship program, however, deterred people from joining an industrial firm and raised their earnings. Those who received capital and training invested a large proportion of the grant and shifted from casual labor and industrial work to their own farms and petty business. Only 9% were in an industrial job at the end of the year, compared to 20% in the control group. Relieving constraints on self-employment reduced industrial labor supply. After a year, earnings in the entrepreneurship arm had risen by a third compared to the control group, without working significantly more hours. This was only a gain of $1 a week, but an important since they only earned $3 otherwise.

Turning to health impacts, industrial work appears to have doubled self-reported health problems. The number of people reporting "great difficulty" performing simple daily activities (such as lifting heavy items or walking a short distance) increased from 4% among controls to 7.3% in the industrial job offer arm. Qualitatively, workers reported repetitive stress, kidney, and respiratory issues. These health impacts are large and striking, especially considering that most respondents had quit the industrial job or the sector altogether by the end of the year. These impacts come from spending just three additional months that year in industry. For every additional month of industrial work induced by the job offer, people were 1.1 percentage points more likely to report a disability of some form.

Why did workers apply and take poorly paid, hazardous jobs? In qualitative interviews many of the applicants said they used industrial jobs as temporary employment to cope with adverse shocks and unemployment spells. Also, to some extent, as youth with little formal sector experience, they were uncertain about the nature of these jobs or their aptitude. They learned mainly through experience. Finally, we also see weak evidence that workers who have baseline characteristics associated with poor earning and employment aspects do see relative income gains from the job offer. Thus workers with poor aptitude for informal work and self-employment might choose riskier work for higher compensation. This is clearly not the case for higher aptitude workers, however.

Naturally, workers were probably not perfectly informed about job risks and quality of these jobs, and there is some evidence that they underestimated the risk. Nonetheless, our data suggest that workers understood the health risks, at least in part, did not update their assessment of the risks as a result of spending more time in industry, and that they were willing to bear these risks to cope with temporary unemployment spells. It is possible

that the short term earnings, and ending of a temporary unemployment spell, were the compensating differential for risk. The degree to which workers understand health risks and how they seek compensation is an important one for future research.

Otherwise, the search process for industrial work in Ethiopia seems to be relatively fast, with little hint of constraints on mobility or barriers to learning once in the positions. There is no indication that the opportunity cost of time in trying out one of these jobs is high for these disposable jobs. If not for the worrisome health effects of these jobs, this would look like a competitive, frictionless, integrated labor market.

2 Setting

Firms exporting from Asia are looking to Ethiopia as a new industrial hub (The Economist, 2014). The country has 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. Yet the country also remains poor, as 78% live under $2 a day in purchasing power parity (PPP) terms, and agriculture employs 85% of the workforce.

Ethiopia has a long history of manufacturing, especially shoemaking, with some firms in existence since the 1930s. Until the 1990s, however, Ethiopia was a command economy with few private firms. In the last 20 years, the state has encouraged entry by private firms, with relatively limited controls. This has resulted in significant domestic investment in agribusiness and light manufacturing, as well as a significant foreign direct investment from Europe and Asia. In the years prior to our study, 2000–08, national income and industrial output both grew about 10% per year, with the number of medium and large manufacturers doubling in number (CSA, 2011). Ethiopia represents one of the continent's few committed attempts to attract labor-intensive industries.

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. The period around our study, 2005–15, saw rises in manufacturing value added between 8.6 and 16.9 percent per year in constant 2010 US dollars (World Bank, 2015).[11]

Over the last two decades, there has also 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

[11]Manufacturing value added was substantially higher as a share of national income in the 1990s, after the end of the war and new market reforms, but this mainly reflects the low level of national income at the time rather than the size of the manufacturing base.

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. Appendix A elaborates.

3 Experimental design and procedures

We worked in five firms, described in Table 1, with details in Appendix B. The firms were from five sectors and four regions. Two firms hired more than one cohort over the study period, 2010–13, for a total of eight cohorts. While this is a small number, the differences in timing, region, and sectors reduced the influence of idiosyncratic shocks. Three firms engaged in light manufacturing (textiles and garments, shoes, and beverages) and two in commercial agriculture (flowers and vegetables). With the exception of the import-competing beverage producer, all were export-oriented. Only one of the five was foreign-owned.

The positions required no experience, although the three manufacturers (not the two farms) required applicants to have completed grade 8 or 10. The jobs involved working on production lines—bottling water, picking and packing produce and flowers, cutting fabric, or sewing shoes. They could involve heavy machinery or simple tools.

The positions required people to work 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 healthcare 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.

Table 1: 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.

3.1 Site selection

We approached more than 300 private firms over two years, roughly half of all private industrial firms with 50 or more employees.[12] We identified these firms through applications for investment certificates, public business listings, industry associations, and personal contacts.[13] To be eligible for the study, a firm had to be in a manufacturing or commercial farm sector, expect to hire a batch of at least 15 low-skill, full-time workers, and be willing to randomly assign job offers within a pool of applicants the firm pre-screened.

More than three-quarters of the firms we approached were open to the study.[14] Few firms, however, had plans to open a new production line and hire a large batch of workers at once. One reason is that sector growth was slowing in this period.[15] 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. Most of all, few firms planned to hire their employees in bulk at one time, but rather planned to hire people piecemeal, to accommodate more gradual growth and cope with regular attrition.

Randomizing jobs proved uncontroversial in the labor-intensive industries that make up most Ethiopian industry. 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, human resource managers described entry-level hiring as ad hoc. For instance, we commonly witnessed 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

[12] 2009 census data suggest just over 500 private firms with more than 50 employees in the manufacturing sector and less than 100 export-oriented commercial farms (CSA, 2011).

[13] These firms are not a representative sample of all firms, but rather were our attempt to contact all firms in textiles, leather, horticulture, and other manufacturing, especially firms that were expanding operations. We contacted most firms via phone or walk-in.

[14] 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.

[15] 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.

profits, two to three times as many production employees, and lower-skilled employees.[16]

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.

3.2 Stimulating self-employment through grants and training

Starting with the second cohort, the entrepreneurship treatment arm offered people five days of business training and planning, followed by an unconditional cash grant of nearly 5000 Birr, or roughly $300 at the time it was given (see Table 1).[17] We chose the amount based on our qualitative assessment of the costs required to set up a part-time enterprise, plus the limits of what we could afford.

Professional skills trainers led classes of about 20, and each person also received individual mentoring during those five days.[18] 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. Subjects had to complete at least three days of the training, however, to receive the grant.

3.3 Experimental procedures

We generally followed each firm's standard procedures for hiring batches of new employees to staff new production lines: the firms advertised jobs through a posting on the front gate,

[16] 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% of the workforce in manufacturing in general and more than 60% in textiles or footwear (CSA, 2011). The wages of the workers in our sample fall in these ranges. See Appendix C for data and analysis.

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

[18] Cohorts 2 and 3 also received a follow-up visit by the trainer after three months for additional advice. Grantees did not see this service as helpful, and given the cost it was discontinued after cohort 3.

word of mouth, and local job boards.[19]

Applicants were instructed to gather on a specific day. Firm managers would then screen written or verbal applications, typically based on firm- or job-specific minimum education and health requirements. Some firms also preferred specific age ranges or genders for different positions. Across the study cohorts, between 75 and 95% 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 random assignment to the job, and informed them for the first time about the entrepreneurship program. Thus all were aware of the possibility of a grant. Nearly all agreed to enter the study and were invited to complete a baseline survey and enter the lottery, stratified by gender. 304 were assigned to receive a job offer, 285 to receive the entrepreneurship program, and 358 to a control group. The firm posted the names of people receiving the job offer at the factory site and a member of the IPA/EDRI research team contacted all those assigned to the job or entrepreneurship program. Job offers began within a few days. 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 Study participants and balance

Table 2 reports characteristics of the eligible applicants at baseline, from self-reported surveys, and tests of balance.[20] 80% 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% had worked in a large, formal firm before, and only 19% 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

[19] 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 distance, since all live nearby.)

[20] 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.

offered at the time of hiring.

The experimental sample was imbalanced across the treatment arms at baseline. Of the 34 covariates across two treatments, 8 of the 68 mean differences (12%) 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 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 and attrition after one year

We ran endline surveys 11 and 13 months after job offers, for two rounds of data on low-autocorrelation outcomes (such as weekly employment hours and earnings), thus increasing statistical precision in a fixed sample (McKenzie, 2012).[21] At 11 months we also attempted to interview the household head for household labor allocations, wealth, and attitudes. Our sample frequently moved between survey rounds. We tracked 88% of individuals after 11 months, 85% after 13 months, and also interviewed 90% of their households.

Note that 5.3% were not interviewed because they traveled to the Middle East for temporary domestic work and construction. Based on qualitative interviews with emigrants, these jobs can pay significantly higher wages than domestic factories, but may have fixed costs of entry and potentially severe disamenities, such as extremely long hours, no days off, infrequent family contact, restricted freedoms, and sometimes abuse. We know whether someone emigrated, but we do not have survey outcomes. Based on household interviews, we estimate 4% of the control group emigrated, and emigration is 2.1 percentage points higher among those assigned to a job and 2.8 points higher among those assigned to a grant (both effects have a standard error of .017 and so are not statistically significant). Since there is a small treatment differential, this could mean that the estimated income treatment effects are too low, and estimated impacts on quality of life measures (such as mental health) are too low. We bound treatment effects for different attrition scenarios in Section 5.6 below, and see no reason migration should affect our core conclusions.

Overall, across the two surveys, response rates were 87.4% in the job arm, 87.4% in the entrepreneurship arm, and 84.2% in the control arm. When controlling for baseline covariates, attrition (including emigration) is 2.3 percentage points higher in the control

[21] See Appendix D.6 for a comparison of results from just one round of data collection versus two, to demonstrate the value added of adding the second round. The point estimates are slightly higher with just one round of data, but the precision is much lower.

Table 2: Baseline summary means and test of randomization balance

Baseline covariate (n=947)	Control mean (n=358) (1)	Balance test (OLS) Job–Control Diff (2)	Job–Control p-value (3)	Entrepreneur–Control Diff (4)	Entrepreneur–Control p-value (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.

group than the treatments, but this is not statistically significant.[22] Attrition is, however, associated with covariates: it is higher among women and those who were poorer, had less formal sector experience, and were less conscientious at baseline.[23] This is a further reason to control for baseline covariates when calculating treatment effects.

3.6 Qualitative data

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.

4 Descriptive analysis

Some of the first insights into industrial work come from collecting and analyzing panel data on largely unemployed industrial job seekers. In addition to reporting take-up of the experimental treatments, this section discusses what summary statistics and observational analysis tell us about the treatment arms and employment opportunities in their absence.

4.1 Job take-up, retention, and turnover

First, we see that large numbers of people applying for industrial jobs turned down a job offer or quit quickly. Within a few months of the offer, most quit the job and the industrial sector as a whole. Table 3 reports take-up and turnover rates treatment arm.

If offered the job, 10% did not show up the first day. A further 20% quit within a month. After a year, 77% had quit the study firm (there were very few firings or involuntary separations). Meanwhile, 13% of the control group was hired by a study firm during the year. Most of these people quit as well. Controlling for covariates, assignment to a job increased the chances an applicant held that job for at least a month by 57 percentage points relative to the control group, but assignment to a job offer increased the probability they held that job at endline by just 17 percentage points.

People did not simply quit the factory job: they tended to leave the sector altogether. Between baseline and endline, 69% of the control group held a formal sector job of any form

[22] Appendix D.1 reports response rates by round and treatment, and the correlates of attrition.

[23] The p-value on a joint test of significance of baseline covariates is <.001. Conscientiousness a personality trait that indicates lower perseverance and discipline

Table 3: Take-up of treatments and turnover

	Proportion who take-up by treatment assignment				Take-up differences (OLS)			
					Job–Control		Entrepreneur–Control	
Dependent variable	Job	Entrepreneur	Control	Obs	Coeff.	Std. Err.	Coeff.	Std. Err.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Employment in a study firm:								
Was directly informed of a job offer†	0.99	0.00	0.04	947	0.964	[.020]***	-0.027	[.017]
Worked at least a day†	0.89	0.07	0.14	947	0.753	[.039]***	-0.081	[.041]*
Worked at least a month†	0.69	0.07	0.13	947	0.569	[.044]***	-0.071	[.032]*
Was working in study firm at endline	0.21	0.01	0.03	1841	0.172	[.017]***	-0.016	[.012]
Employment in formal or industrial sectors:								
Worked at least a month in formal sector	0.91	0.53	0.69	1628	0.225	[.032]***	-0.158	[.041]***
Worked at least a month in any industrial firm	0.83	0.26	0.43	835	0.408	[.057]***	-0.153	[.020]***
# of months worked in any industrial firm	5.98	1.43	3.16	835	2.943	[.387]***	-1.496	[.267]***
Was working in any industrial firm at endline	0.32	0.09	0.20	1587	0.107	[.023]***	-0.118	[.016]***
Entrepreneur & training intervention:								
Offered grant & training†	0.00	0.97	0.00	947			0.970	[.019]***
Received grant & training†	0.00	0.94	0.00	947			0.938	[.029]***

Notes: A study firm is one of our five firms. The formal sector includes any formal firm with 10 or more employees in any sector. By "industrial firm" we mean any manufacturer or commercial farm. Indicators for working at least a month in a firm since baseline are equal to 1 if it was reported in the 11- or 13-month endline. The # of months worked in a firm since baseline is calculated up to the 13-month endline, where those data are available. The indicator for working in any firm at endline equals one if they reported such work at either endline. The estimates in Columns (5) to (8) come from an OLS regressions of the dependent variable on assignment to job and assignment to entrepreneurship program with cohort dummies and the baseline control vector listed in Table 2.

†Data drawn from project administrative data rather than the endline survey data. Directly informed of a job offer means one of the study staff reached them in phone or person.

*** p<0.01, ** p<0.05, * p<0.1 (not adjusted)

for a least a month, and 43% worked in some kind of industrial firm. Being assigned a job offer to one of our study firms increases the probability of working a month or more in an industrial firm by 41 percentage points. Being assigned to the entrepreneurship program, meanwhile, decreases this probability by 15 percentage points.[24]

After a year, only 32% of those assigned to an industrial job still worked in the sector in the month before the survey, versus 20% in the control group. In exit interviews, many reported that they quit despite having no outside opportunity at that time.

Thus the real treatment here is the amount of time working in the industrial sector. The control group worked 3.16 months in the sector, compared to 5.98 months in the treatment group and 1.43 months in the grants group. As a consequence, below we will examine both intent-to-treat effects as well as complier average causal effects using assignment to treatment as an instrument for length of time in the industrial sector.

Who stayed versus quit? Table 4 reports the correlates of industrial job retention among those assigned to the industrial job offer or the control group (results are similar regardless of the sample we use). Those who stayed were older and had less formal work experience, lower executive function[25], lower conscientiousness, greater physical health (measured by a self-reported disability, discussed below), and weakly greater future orientation (based on self-reported time preferences plus play in incentivized games).[26] One interpretation is that people with observable human capital (education, experience) had good outside options, while those with less observable skills and productivity (future orientation, or productivity reflected in their initial outside earnings) were less likely to leave.

4.2 Grant use

We do not have precise investment figures, but we returned to all grant recipients 6–8 weeks after the grant and asked them to describe how they had used the cash by allocating 30 tokens across 12 pictures representing different expenditures. They indicated that 55% of the grant went to business materials or investments, 35% to savings or cash on hand, and 10% to consumption or transfers. Money is fungible (among other weaknesses of these data) but nonetheless the exercise implies the that young people sought to smooth the income shock and put at least $165 of the $300 into enterprises.

[24] Of those assigned to the entrepreneurship program, 3% could not be reached and 2% declined the training and cash or dropped out midway, typically (they said) due to illness or a preference for full time work.

[25] Also known as working memory, it is used to perform activities such as planning, organizing, strategizing, paying attention to details, and managing time and space. We measure it through a combination of forward and backward digit recall tests, as well as a modified Stroop test that tests inhibitory control.

[26] We have survey questions related to patience and self-control in everyday situations. We also play games involving trade-offs of real money over time, which we use to calculate a discount rate. We average all measures and standardize the index.

Table 4: Baseline correlates of industrial job retention, job and control arms only (N=518)

	# of months worked in any industrial firm since baseline		Working in any industrial firm at endline	
Baseline covariate	Coefficient	Std. Err.	Coefficient	Std. Err.
	(1)	(2)	(3)	(4)
Age	0.167	[.096]*	0.017	[.008]*
Female	-0.039	[.924]	0.101	[.084]
Unmarried	-0.032	[.719]	0.013	[.066]
Household head	-0.611	[.705]	-0.065	[.063]
Disability indicator	-3.749	[1.706]**	-0.431	[.134]***
Total years of education and training	-0.129	[.109]	-0.003	[.010]
Cognitive ability, z-score	-0.621	[.288]**	-0.074	[.028]***
Mental health, z-score	-0.420	[.291]	-0.029	[.027]
Conscientiousness index, z-score	-0.359	[.343]	-0.060	[.032]*
Risk aversion, z-score	-0.189	[.285]	-0.036	[.028]
Future orientation, z-score	0.409	[.294]	0.048	[.026]*
Income and wealth, z-score	-0.110	[.342]	0.005	[.032]
Years experience in formal work	-0.559	[.233]**	-0.044	[.024]*
Ever worked in industrial firm	0.801	[.832]	0.010	[.084]
Prospects for employment in next month (0–1)	0.004	[1.263]	0.027	[.118]
Mean of dependent variable	5.038		0.320	
R-squared (including fixed effects)	0.106		0.129	
F-test of joint significance (p-value)	0.029		0.001	

Notes: All regressions use OLS. pooling 11- and 13-month surveys, and include cohort-gender fixed effects. Standard errors are robust and clustered by participant.

4.3 Employment and earnings in and out of industrial work

At baseline, most applicants had little work or earnings. So why quit a permanent job? First, the panel data suggest that applicants were in a temporary employment slump, perhaps because they had recently lost a job, were new entrants, or were re-entering the labor market after a period of no employment. Hence baseline earnings and employment levels did not reflect most people's options over a longer horizon.

In the control group, the share of participants with no employment hours fell from 68% at baseline to 36% at endline, and average hours of work per week rose from 7.5 to 26.[27] As a result, average weekly earnings in the control group more than tripled from baseline to endline, from 9 to 33 Birr (all values in 2010 prices). Most of their work was in the informal sector: farm or casual labor, petty business or salaried work in an informal firm. This is unlikely to come from a common labor demand shock. The eight cohorts of hires began at different points in time and space over two years, with varied macroeconomic conditions.

[27]The largest increases in hours came from factory and farm wage labor, casual non-farm labor (e.g. construction), and salaried labor in non-industrial organizations (such as shops, offices, etc.).

Table 5: Industrial employment, earnings, and wage premiums at endline

	Dependent variable			
	Unemployed (<5 hours work per week)	Weekly work hours	Weekly cash earnings (Birr)	Earnings per hour (Birr)
	(1)	(2)	(3)	(4)
Mean, people with no industrial work (including unemployed)	0.50	20.66	27.92	1.59
Mean increase among people with any industrial work (with baseline covariates)	-0.47	23.45	26.04	-0.38
	[.026]***	[1.468]***	[5.408]***	[.175]**
Mean increase among people with any industrial work (without baseline covariates)	-0.48	23.88	27.08	-0.39
	[.025]***	[1.421]***	[5.446]***	[.177]**
Observations	1087	1087	1088	681
Mean, people with no industrial work (excluding unemployed)		41.50	54.71	1.45
Mean increase among people with any industrial work (with baseline covariates)		4.44	2.67	-0.24
		[1.492]***	[6.731]	[.165]
Mean increase among people with any industrial work (without baseline covariates)		4.17	2.70	-0.25
		[1.492]***	[6.685]	[.165]
Observations		671	671	671

Notes: 11- and 13-month survey responses are pooled. Industrial work is an indicator for positive hours in factory work any time in the two weeks prior to the 11 or 13-month survey. "Unemployed" mean less than 5 hours of work per week, on average, across the weeks of work reported in the 11- and 13-month surveys. The "Mean increase among people with any industrial work" is the coefficient on an indicator for industrial work in an OLS regression of each dependent variable on an indicator for having any factory or commercial farm work at endline, plus baseline covariates and cohort fixed effects. Earnings per hour are undefined if there were zero work hours reported. Standard errors are robust and clustered by individual..

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

Second, while receiving an industrial job increased the hours and earnings available, and reduced the risk of an unemployment spell, industrial work paid wages that were similar or lower than most people's non-industrial options. Table 5 reports employment and earnings for those with and without any industrial work at endline. Our estimate of the premium from industrial work comes from an OLS regression of each outcome on an indicator for any employment in a factory or farm at endline plus select baseline covariates and cohort fixed effects.[28] We look at this premium with and without the "unemployed" (which for the purposes of this table we redefine as those with fewer than five hours of work per week). We can also look with and without baseline controls to observe selection effects.

At endline, 39% of the sample worked fewer than 5 hours per week. Including these "unemployed" (in the top panel), people outside factory work had only about 20.7 hours of work a week and 28.0 Birr in weekly earnings. Conditional on having 5 or more hours of work a week (bottom panel), the non-industrial opportunities were better: 41.5 hours a week, and 54.7 Birr in earnings.

Overall, the data suggest that an industrial job increased hours and the potential for total earnings because (if people stayed in the job) it limited unemployment spells that came with the unpredictable and often temporary nature of mostly informal work. Compared to people employed in other sectors, there was an increase in hours and total earnings from working in an industrial firm, but only the rise in hours is statistically significant.

More striking, the coefficient on earnings per hour in an industrial job is actually negative and statistically significant at the 5% level. It appears that, if anything, industrial work is more poorly remunerated than non-industrial work (when such work is available).

5 Experimental analysis

To estimate program impacts on outcome Y, we calculate the intent-to-treat (ITT) estimate of an offer of a job or entrepreneurship program via OLS:

$$Y_{irj} = \alpha_j + \gamma_{r=13} + \theta_J Job_{ij} + \theta_E Entrepreneur_{ij} + \beta X_{ij} + \epsilon_{ij}$$

where *Job* and *Entrepreneur* are indicators for random assignment to the treatment arms.

[28] To the extent that these covariates capture the endogenous determinants of occupational choice and matching with an industrial firm, the coefficient on the factory/farm work indicator can be interpreted as the causal effect of industrial jobs. This assumption of conditional unconfoundedness is probably too strong, and the coefficient is undoubtedly biased, but it nonetheless gives, to a first approximation, a sense of the returns to industrial work. We cannot use assignment to an industrial job as an instrument since it would violate the exclusion restriction; assignment to a factory job could affect current earnings even if a year later the individual is no longer in the job.

We control for the baseline covariates, X, listed in Table 2, as well as gender-cohort fixed effects, α_j. We pool both 11- and 13-month survey rounds, r, and we include a fixed effect for the 13-month round and cluster (robust) standard errors by individual. In principle, the request not to hire the control group members for a short while is a violation of the stable unit treatment values assumption (SUTVA) but given that this request was short term and (as we will see) mainly ignored, it is unlikely to affect our estimates of impact.

Our tables also report $\theta_J - \theta_E$, but except for the artificial environment of this research project, the grant is not a meaningful counterfactual for the job offer, and so the difference should be taken with caution. It is useful for comparing whether the risks or benefits of industrial work are echoed in entrepreneurial work.

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.

Complier average causal effects Finally, given the high quit rates, we also calculate a complier average treatment effect (CATE), where we use *Job* as an instrument for the number of months since baseline the person worked in a formal firm with 10 or more employees. The CATE approximates the effect of staying longer in industrial work. Those in the control group worked an average of 3.2 months in a formal job, and this roughly doubles with a job offer.[29]

Transitory versus permanent impacts To the extent that study subjects are in a temporary unemployment spell at baseline, the job offer will lead to an immediate increase in employment and income relative to control group subjects, who remain in an unemployment spell. We did not measure these short term, largely mechanical increases in income and employment. Rather, we measure income and employment 11 and 13 months later. We anticipated that any transitory shocks leading subjects to apply to the factory at baseline would be resolved by this time. As a result, our research design was intended to capture the

[29]There are reasons to treat this CATE with caution. In principle a job offer could affect outcomes through some mechanism other than weeks worked (e.g. people who learn and quit after a day). Also, using the number of months in industrial work as the endogenous variable assumes a linear effect size, but if the effect is not linear then the random job offer will not necessarily be a valid instrument (since it will also affect number of weeks squared and other nonlinear terms). As an approximation, however, these CATE results give us a sense of the consequences of longer time in industrial work. Alternatively, we could use "had any industrial work" as the endogenous variable. Since almost 90% work at least a day when offered a job, however, the results would not be materially different than a simple ITT, and any other indicator (such as "worked at least a month in an industrial job") would not pass the exclusion restriction.

more permanent effects on income, employment, and other aspects of well being. That said, if unemployment spells tend to be long in the absence of the job offer, there is a risk that any treatment effect on income or employment reflect the persistence of transitory shocks. This is especially true for permanent income measures, such as consumption or assets.

Multiple comparisons This experiment predated the development of the social science registry, but we documented our theoretical priors in a short policy note discussing "pilot results" from the very first hiring cohort.[30] To reduce the number of hypothesis tests we follow Kling et al. (2007) and collect major outcomes into family indexes—additive, standardized indexes of related outcomes. Four are directly affected by industrial work: employment, income, physical health, and mental health. Four are indirect, in that changes are likely to be a result of "first-stage" changes in one of the direct outcome: attitudes to industrial work/firms, social integration, autonomy, and non-cognitive skills.[31] Finally, the paper reports treatment effects on all outcomes measured in the survey. We collected a small number of measures on time use, school enrollment, emigration, or workplace conditions for largely descriptive or exploratory purposes, and these do not easily fit into the eight major families above. We discuss impacts on these other outcomes in the text, but report treatment effects in the appendix.

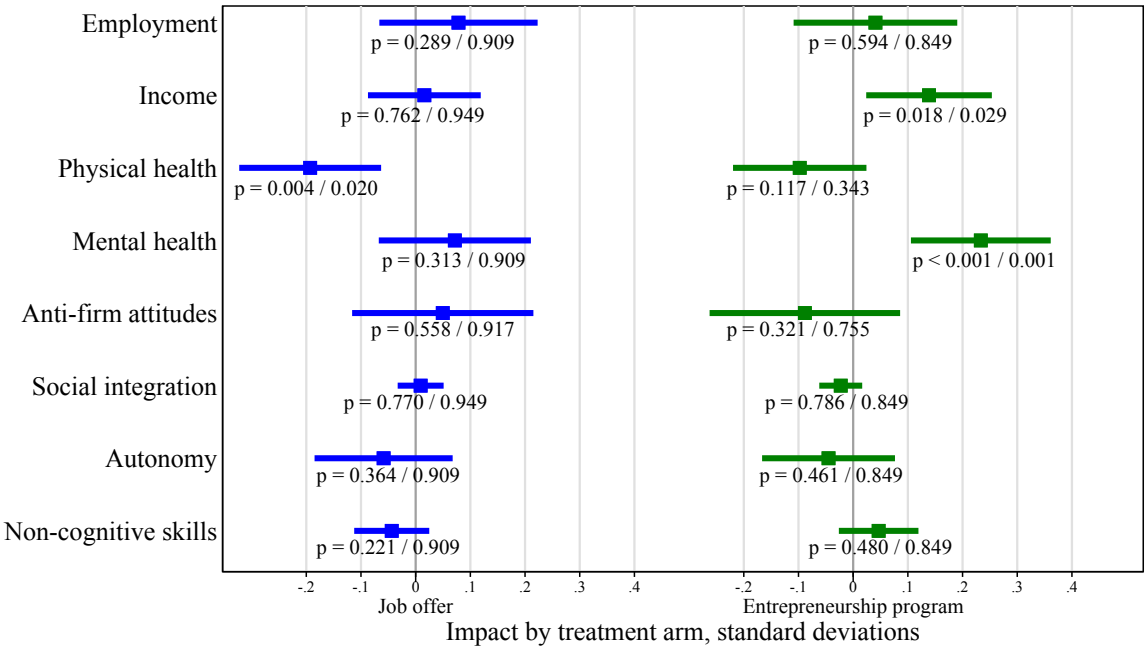
In addition to variable reduction, we also consider more conservative p-values to account for the fact that we are testing multiple hypotheses. Below we present unadjusted standard errors as well as p-values adjusted for multiple comparisons, so that readers can use the threshold appropriate to their question and preferences. We use the Westfall and Young (1993) free step-down resampling method for the family-wise error rate (FWER), the probability that at least one of the true null hypotheses will be falsely rejected, using randomization inference.[32] All results are robust to fairly conservative adjustments, such as for all eight family indexes (direct and indirect). Figure 1 summarizes the one-year ITT estimates of the effect of each treatment on standardized indexes for the eight direct and indirect outcome families, including both unadjusted and adjusted p-values. As seen in Appendix D.3, we draw the same conclusions if we adjust for both treatment arms (16 comparisons).

[30] http://www.theigc.org/publication/more-sweatshops-for-africa-pilot-results-from-an-experimental-study-of-industrial-labor-in-ethiopia-policy-brief/

[31] The direct outcomes, plus social integration, were the focus of the published policy note, prior to endline data collection on 7 of the 8 cohorts, illustrating that this division and emphasis is not simply post hoc.

[32] Other papers taking this approach include Kling et al. (2007); Casey et al. (2012); Anderson (2008). Using the Westfall-Young bootstrap and the Holm-adjusted Bonferroni methods yields similar results.

Figure 1: Program impacts on standardized family indexes, with 95% confidence intervals and unadjusted/adjusted p-values



Notes: The figure reports the effect of each treatment arm, averaging the 11 and 13 month survey outcomes. It also reports the difference between the two treatment arms. Treatment effects are estimated via OLS controlling for baseline covariates and block fixed effects. Each summary index is the standardized mean of composite outcomes. Standard errors are heteroskedastic-robust. *** p<0.01, ** p<0.05, * p<0.1

5.1 Impacts on employment/occupational choice

To measure occupational and earnings impacts, at each endline we asked respondents whether they had engaged in 22 occupations, from farming to petty business, trades, and formal jobs. For each one, we collected self-reported hours and net earnings in both the last week and the week prior. With two endline surveys this gives us four weeks of employment data per person. People reported 0 to 3 occupations, with an average of 0.75 (1.13 among those reporting any). We calculate total hours and earnings across all occupations each week, and estimate treatment effects on the average of the two weeks of data.

Table 6 reports ITT estimates on the overall family index, the components of the index, as well as a set of other economic outcomes measures in the survey that are of interest but do not fit conceptually in employment/occupational choice and income. We adjust p-values for family indexes but do not adjust p-values for the components of indexes, or for other outcomes, and so those results must be taken with some caution, and be viewed as more suggestive and descriptive in nature. We report on them mainly to help understand the

Table 6: Impacts of the job offer and entrepreneurship program on employment and income

		ITT estimate (N = 1587)							
	Control	Job offer			Entrepreneurship program			Job – Entrepreneur	
Outcome	mean	Coeff.	Std. Err.	Adj. p-val.	Coeff.	Std. Err.	Adj. p-val.	Coeff.	Std. Err.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Employment & occupational choice, z-score	-0.04	0.078	[.074]	0.909	0.040	[.076]	0.849	0.038	[.079]
Hours work/week, past two weeks	26.39	0.997	[1.895]		3.506	[1.892]*		-2.509	[2.010]
Factory labor	7.46	3.017	[1.380]**		-4.104	[1.169]***		7.122	[1.287]***
Farm wage labor	3.07	1.817	[.914]**		-1.480	[.744]**		3.297	[.865]***
Smallholder farming	0.82	-0.258	[.323]		1.480	[.398]***		-1.738	[.430]***
Petty business	4.04	-0.878	[.978]		5.381	[1.379]***		-6.259	[1.353]***
Skilled trades	1.59	-0.736	[.449]		-0.576	[.483]		-0.160	[.408]
Casual non-farm labor	2.18	-0.954	[.568]*		0.746	[.770]		-1.700	[.662]**
Low-skill salaried labor	4.19	0.064	[1.095]		-0.412	[.956]		0.476	[.984]
Medium skill salaried labor	1.21	-0.415	[.420]		1.604	[.590]***		-2.018	[.545]***
Other work	2.27	-0.085	[.694]		0.489	[.738]		-0.574	[.800]
Unemployed in past two weeks	0.34	-0.013	[.033]		-0.082	[.032]**		0.068	[.034]**
Std. dev. of hours/week	16.44	-1.306	[1.342]		3.949	[1.476]***		-5.254	[1.458]***
Income, z-score	-0.01	0.014	[.052]	0.949	0.150	[.058]**	0.029	-0.135	[.057]**
Weekly earnings, 2010 Birr	34.23	3.049	[4.479]		12.005	[5.463]**		-8.956	[5.426]*
Earnings per hour, 2010 Birr	1.46	-0.020	[.186]		0.153	[.200]		-0.173	[.200]
Std. dev. of weekly earnings	56.01	4.107	[7.600]		3.769	[8.263]		-0.079	[.067]
Household-level durable consumption assets, z-score†	0.07	-0.071	[.069]		0.009	[.067]		-56.289	[33.861]*
Household-level non-durable consumption, 2010 Birr	664.46	20.548	[34.653]		76.837	[35.492]**		-0.282	[.074]***
Household-level durable productive assets, z-score†	-0.12	0.049	[.068]		0.331	[.077]***		-0.282	[.074]***

Notes: Columns (2) to (5) report the results of an OLS regression of each outcome on treatment indicators, baseline covariates, and cohort-gender fixed effects. 11- and 13-month survey responses are pooled. Standard errors are robust and clustered by respondent. P-values are adjusted for family outcomes using the Westfall-Young approach described in section 5.1. Some outcomes contain fewer observations than the listed number of observations because a very small number of respondents were not asked certain questions.

†denotes outcome variables that were measured during only one of the endline surveys.

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

substantive nature of the indexes and interpret what changes may mean.

There was no change in total hours worked per week among those offered an industrial job, but occupations shift. Factory and commercial farm labor increased while petty business, casual labor, and other activities decreased. Those assigned to the entrepreneurship program, meanwhile, increased total hours worked by 3.5 per week (significant at the 10% level only) and shifted their time towards smallholder farming and petty business, as well as medium skilled salaried labor (e.g. white collar jobs).[33] The difference between the employment and occupational choice index in the entrepreneurship arm and in the job offer arm is less than 0.04 standard deviations and statistically not significant.[34]

5.2 Impacts on income

We use three measures of income. One is the sum of weekly cash earnings across the 22 occupations.[35] Earnings are seasonal and do not reflect home production, so we also consider two measures of permanent income reported by the household: an index of 32 durable consumption assets (e.g. housing quality, furniture) and the value of non-durable consumption in the previous 4 weeks via an abbreviated consumption module of 82 items.[36]

On balance the factory job offer seems to have no significant effect on income by any of the three measures, and a family index of the three increases only 0.014 standard deviations.[37] These income measures tend to be skewed and highly variable, however, and so our estimates are imprecise. Thus while the average effect is close to zero, the confidence interval on income includes moderate increases and decreases in income from the industrial job offer.

Even if the income effect after a year is negligible, the job offer could have still had an important impact on annual income by ending lengthy unemployment spells. Several results

[33] A family index of hours worked, unemployment, and the standard deviation of hours does not rise significantly because the increase in the standard deviation of hours enters negatively into the index. If we exclude or reverse the direction of this volatility measure, the rise in employment is highly significant. In Appendix D.2, we display histograms of average weekly hours and estimated earnings in the past month for the job offer and control groups. They are broadly similar.

[34] The employment family index does not include information on general time use or school enrollment. We measured these in the survey for exploratory purposes, however, and report impacts in Appendix D.4. Not surprisingly, assignment to industrial work or the entrepreneurship program had little impact on time use, whether commute time (about 4 hours a week) or chores (20 hours on average per week). Those assigned to the entrepreneurship program report 3.6 fewer leisure hours per week compared to the control group (significant at the 5% level). School enrollment is 3.2 percentage points lower among those assigned to a job, about half the control mean (though significant at only the 10% level).

[35] As a check against this weekly and activity-based measure, we also ask respondents to estimate their total cash earnings in the past four weeks. The treatment effects are similar.

[36] See Beegle et al. (2012) for this approach. This abbreviated measure likely understates total consumption by excluding durable asset use and less common purchases. We do not have price data for valuing durables.

[37] We also ask about savings flows in the past month and see no evidence of an increase (see Appendix D.4). Including this in the income index has not effect on our conclusions.

indicate this was not the case. One is that two of our three income measures (consumption and assets) are measures of permanent income, and so we would expect them to reflect large recent shocks, even if temporary. Moreover, if we use the survey data to estimate total annual income, we do not see any evidence of an increase.

The entrepreneurship program has a sizable effect on income, however, an impact that seems to be driven by the small business rather than a direct wealth effect of the grant, as the impact shows up in earnings more than assets and consumption. Weekly earnings in the entrepreneurship arm are 12 Birr higher than the control group. In absolute terms this is roughly $1 per week ($3.4 PPP), and in relative terms this is a large effect—a one third increase in earnings for otherwise very poor young people.

If we assume this earnings gain lasted the whole year, those assigned to the entrepreneurship program earned about $52 more since baseline—about 16% of the full grant, or 32% of the amount they said they initially invested. If we deduct compensation for added hours worked, these figures fall to 7% and 13%.[38] These are not necessarily high returns, especially when we consider that the recipients also received training in addition to the capital, though the returns are higher when using our estimated measures of annual hours worked and total income: about 16% of the grant after accounting for added hours worked.

Looking at the income family index, the increase in income in the entrepreneurship arm is significant at the 5% level after adjusting for eight comparisons. Note that it is only significant at the 10% level when we adjust for 16 comparisons (across outcomes and treatment arms). Furthermore, Table 6 shows that the job arm earns 0.14 standard deviations less than the entrepreneurship arm, significant at the 10% level.

A job offer does not lead to less volatile earnings or works hours. If we take the standard deviation of weekly work hours or weekly earnings for the four weeks of data available, we see no evidence of a significant decrease from a job offer (indeed the point estimate on the standard deviation of earnings is positive).[39] The cash grant, meanwhile, increases the standard deviation of weekly work hours and weekly earnings.

Heterogeneous employment and earnings impacts

The zero average treatment effect on earnings conceals some heterogeneity, reported in Table 7. We divide the sample into two groups—those with high and low outside earnings potential—constructed using an index of baseline characteristics that predicts endline earn-

[38] Entrepreneurship recipients work 3.5 more hours per week, valued at 5 Birr per week at the average wage, from Table 6. We subtract this from the 12 Birr per week.

[39] See Appendix D.2 for histograms of average weekly hours and estimated earnings in the past month for the job offer and control groups. They are broadly similar.

Table 7: Heterogeneity in treatment effects by baseline earnings potential

Outcome	Dependent variable (N=1587)	
	Working in industrial firm	Weekly earnings
	(1)	(2)
Assigned to job offer (a)	0.103	10.532
	[.038]***	[5.375]*
Job × Above median earnings potential (b)	0.012	-11.493
	[.053]	[8.558]
Assigned to entrepreneurship program	-0.151	10.393
	[.031]***	[6.053]*
Entrepreneur × Above median earnings potential	0.061	4.355
	[.044]	[9.526]
Impact of job if above median earnings potential (a + b)	0.115	-0.962
	[.037]***	[6.448]
Impact of entrepreneurship program if above median earnings potential (a + b)	-0.090	14.748
	[.032]***	[7.693]*

Notes: See footnote 40 for construction of the indicator of above median earnings potential. 11- and 13-month survey responses are pooled. Standard errors are robust and clustered at the individual level.
*** p<0.01, ** p<0.05, * p<0.1 (not adjusted)

ings in the absence of treatment.[40] We interact the indicator for above median earnings potential with treatment. As a result, the coefficient on assignment to the job offer can be interpreted as the treatment effect on applicants with characteristics associated with below median earnings opportunities. The sum of this coefficient and the coefficient on the interaction (reported at the base of the table) can be interpreted as the effect of a job offer on applicants with characteristics associated with above median earnings. This heterogeneity analysis was not prespecified and so must be taken with some caution, but also happens to be the only heterogeneity we analyzed.

Regardless of their earnings prospects, those assigned to job offer are about 10 percentage points more likely to be working in an industrial firm at endline (Column 1). The job offer weakly increases earnings for applicants with low predicted outside earning potential, but has no apparent effect on applicants with high outside earning potential (Column 2). The difference (the interaction term) is not statistically significant. Substantively, however, the

[40]We regress the control group's endline earnings on baseline covariates and predict an outcome. To avoid endogenous stratification, we do this for each control group member individually, excluding their own observation from the fitted model (see Abadie et al. (2014) for a description of this "leave-one-out" method). We then use the estimated coefficients from the full control group to predict earnings potential for the treatment group. We use these predictions to create an indicator for above- and below-median earnings potential, and interact this indicator with indicators for assignment to treatments.

figures are large. At endline, control group members with below median earnings potential earned just 11.6 Birr per week on average (compared to 34 Birr per week for the full control group). Thus the 10.5 Birr treatment effect on low types is nearly a doubling in earnings. This comes almost entirely for an increase in the hours of work available, not the wage.

While the wide standard errors suggest caution, these patterns imply that people with poor self-employment or wage prospects can increase their earnings in the industrial sector because it offers them more regular employment. For people with stronger self-employment and wage prospects, however, accepting a job can have persistent effects on occupational choice a year later, even if the quality of the match is poor.[41]

Interestingly, the entrepreneurship program seems to increase earnings among both high- and low- predicted outside earning potential. This could imply that low outside earning potential is driven by liquidity and credit constraints rather than innate ability.

5.3 Impacts on physical health

Table 8 reports impacts on our measures of health and well-being. One of the main measures of physical health is self-reported ability to perform five activities of daily life, or ADLs: walk 2 km, work outside on your feet for a full day, carry a 20 liter carton of water 20 meters, do usual daily activities, and standing at workbench working for 6-8 hours.[42] Each is measured on a 0–3 scale running from unable (0), great difficulty (1), slight difficulty (2) and easily (3). ADL measures are widely used in studies of labor supply or health and economic development in developed and developing countries, including in Africa, and they have been tested for consistency across tests, interviewers, and skills.[43]

We also code an indicator for a "disability", which we define as reporting "great difficulty" or "unable" on at least two ADLs. 4% of the control group report such a disability at endline. Otherwise, most people in our sample are young and in excellent health, and so report nearly perfect ADLs (14 of a potential score of 15 on average).

Finally, we collect a subjective measure of health using Cantril's Self-Anchoring scale,

[41] One cautionary note: it is possible that the "high" types who stay, and have lower earnings than their peers in the control group, are actually low ability in some unobserved way, and that their low earnings reflect their low marginal product. While possible, it is worth noting we have an unusually rich set of baseline covariates going far beyond the usual Mincerian regressions, including time preferences, personality traits, work histories, mental abilities (such as executive function), and other covariates that are typically strong determinants of lifetime earnings potential.

[42] We adapted the five questions from existing ADL instruments to the context in Ethiopia. We are not aware of a standardized or validated ADL instrument for either Ethiopia or sub-Saharan Africa.

[43] See for example Ware et al. (1980); Strauss and Thomas (2007). ADLs are commonly used in household surveys and program evaluations because they are more common than morbidity and mortality in small or short-run samples, and because measures such as days ill or away from work may be endogenous to labor supply decisions.

Table 8: Impacts of the job offer and entrepreneurship program on physical and mental health

		ITT estimate (N=1587)							
	Control	Job offer			Entrepreneurship program			Job - Entrepreneur	
Outcome	mean	Coeff.	Std. Err.	Adj. p-val.	Coeff.	Std. Err.	Adj. p-val.	Coeff.	Std. Err.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Physical health, z-score	0.06	-0.193	[.066]***	0.020	-0.098	[.062]	0.343	-0.097	[.070]
Ability to do activities of daily life (0–15)	14.07	-0.274	[.125]**		-0.240	[.128]*		-0.039	[.136]
Disability	0.04	0.033	[.015]**		0.017	[.014]		0.017	[.015]
Subjective health assessment (0–10)	8.91	-0.233	[.104]**		0.001	[.104]		-0.236	[.111]**
Subjective health assessment, 5 years from now (0–10)	9.67	-0.181	[.060]***		0.000	[.056]		-0.182	[.062]***
Mental health & subjective well being, z-score	-0.11	0.072	[.071]	0.909	0.228	[.065]***	0.001	-0.155	[.068]**
Depression symptoms (0–27)	2.54	-0.088	[.219]		-0.278	[.211]		0.189	[.210]
Aggressive & anti-social behaviors (0–27)	1.92	-0.058	[.127]		-0.071	[.123]		0.013	[.126]
Anxiety symptoms (0–21)	2.03	0.053	[.197]		-0.279	[.183]		0.332	[.189]*
Level of financial anxiety (0–12)	1.59	-0.071	[.136]		-0.297	[.128]**		0.226	[.133]*
Subjective well being (0–10)	4.14	0.193	[.111]*		0.377	[.108]***		-0.182	[.109]*
Subjective well being, 5 years from now (0–10)	7.57	0.093	[.122]		0.340	[.120]***		-0.247	[.120]**

Notes: Columns (2) to (5) report the results of an OLS regression of each outcome on treatment indicators, baseline covariates, and cohort-gender fixed effects. 11- and 13-month survey responses are pooled. Standard errors are robust and clustered by respondent. P-values are adjusted for family outcomes using the Westfall-Young approach described in section 5.1. Some outcomes contain fewer observations than the listed number of observations because a very small number of respondents were not asked certain questions.

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

a life evaluation approach commonly used in Gallup polls and social science studies (e.g. Kahneman and Deaton, 2010). We gave respondents a picture of three ladders, with ten rungs each, where the top rung represented the best health and the lowest the worst health.

An industrial job is associated with a 0.27 decrease in the ADL index and a 0.23 decline in subjective health evaluations. While these declines are small relative to the means, they seem to be driven by a few people reporting serious disabilities. The disability rate rises 3.3 percentage points among those offered an industrial job, nearly doubling the risk of injury. A family index of the health measures suggests job recipients report a 0.19 standard deviation decline in health, and the effect is significant at the 1% level using conventional p-value cutoffs and at the 5% level after adjusting for eight multiple comparisons (Figure 1).

Our qualitative interviews suggest a number of common ailments, especially repetitive stress injuries from standing or repeated tasks, or respiratory problems due (people report) to dust, particulates, or chemical fumes. We do not know if these represent chronic or temporary problems, but there is some suggestion they may be permanent problems. We asked people to rate their subjective health five years from now on the same ladder. The control mean rose, but the treatment effect is similar to the subjective health today.

These results are also robust to different ways of coding "disabilities" (Appendix D.5). They are also robust to excluding any one firm from the estimates, although disabilities seem to be highest in the horticultural firm and shoe factory (Appendix D.8).

Table 9 reports the CATEs for main outcomes. The clearest impact is on health. Each additional week in an industrial firm is associated with about a 1.1 percentage point increase in a serious difficulty performing two or more standard activities of daily life.

We have to take these results with some caution. First, only a small absolute number of people report "great difficulty" at an ADL: 10 in the control group and 15 in the job offer arm. Second, these are self-reported data and in principle measurement error could be correlated with treatment.[44] Unfortunately, we do not have objective tests of health or disability. Finally, note that the difference between the average health of the job offer arm and the health of the entrepreneurship arm is not statistically significant, as the entrepreneurship arm also experiences a small, statistically insignificant reduction in health. The general magnitude of the job treatment effect remains, but it loses its statistical significance. This

[44]Self-perceptions of health might be affected endogenously by work experiences, for example. Our disability treatment effect, for instance, becomes smaller and no longer statistically significant if we exclude either the "working outdoors for a full day" or the "standing all day at a workbench" components. These are the components with the largest and most statistically significant. While this may be worrisome, it is worth noting that if we use the "some difficulty" threshold for a disability indicator, the largest and most significant components are "carrying 20 liters" and "performing daily activities", and the overall impact of the job offer on disability is statistically significant and robust to excluding "working outdoors for a full day" or the "standing all day at a workbench" components. See Appendix D.5 for this sensitivity analysis.

Table 9: IV estimates of the impacts of an extra month in an industrial job

		Months in an industrial firm since baseline (N=1587)	
Outcome	Control mean	CATE	Std. Err.
	(1)	(2)	(3)
Hours work/week, past month	26.39	0.389	[.702]
Weekly earnings, 2010 Birr	34.23	1.186	[1.6745]
Earnings per hour, 2010 Birr	1.46	-0.007	[.058]
HH consumption durable assets, z-score†	0.07	-0.024	[.022]
HH non-durable consumption, z-score	665.05	7.889	[13.057]
Activities of daily life (0–15)	14.07	-0.106	[.049]**
Disability	0.04	0.013	[.006]**

Notes: Columns (2) and (3) report the results of two-stage least squares regression of outcomes on months employed in an industrial job since baseline, an entrepreneurship program assignment indicator, baseline covariates, and cohort-gender fixed effects, instrumented using random assignment to the factory job. Coefficients on the grant and all covariates are omitted. Table 3 displays first-stage results. 11- and 13-month survey responses are pooled. Standard errors are robust and clustered by respondent.
†denotes outcome variables that were measured during only endline survey.
*** p<0.01, ** p<0.05, * p<0.1

may be because self-employment carries its own occupational risks, or because (as we mention above) the results are based on a small number of extreme injuries.

Nonetheless, the results are consistent with worker's descriptions of factory workplace conditions. We asked survey respondents to describe the amenities and disamenities of their endline workplace, largely for descriptive purposes. Table 10 reports summary statistics, treatment effects, and correlations with endline employment status.

14% of the control group reported serious health risks at their place of work, and this increased 7.8 percentage points in the job offer arm. 27% reported a need for greater safety equipment in their place of work, and this increased 11 percentage points in the job offer arm. These health concerns do not appear to be associated with air quality or general workplace comfort, since we see little effect of treatment on these measures. We see a weak treatment effect on complaints of chemical smells. We see no other statistically significant treatment effects of the job offer on other workplace conditions other than these health-related ones.

These ITT estimates likely understate the disamenities of industrial work, given that most people assigned to industrial jobs quit the sector. Columns 6 to 9 of Table 10 report the results of a simple OLS regression of workplace conditions on an indicator for endline industrial work, controlling for baseline covariates, first on the full sample and then limiting the sample to those with non-zero hours of work. Working in an industrial job is associated with at least a 28 percentage point increase in perceived health risks (especially chemicals

Table 10: Self-reported workplace conditions: treatment effects and correlates of endline workplace

		ITT estimate (N = 1587)				OLS coeff. on indicator for industrial work at endline			
	Control	Job Offer		Entrepreneurship		Full sample (N=829)		Non-zero work hours (N=532)	
Outcome	mean	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Serious health risks at workplace (0-1)	0.14	0.078	[.033]**	0.005	[.032]	0.279	[.040]***	0.297	[.054]***
Need more safety equipment at work (0-1)	0.27	0.111	[.040]***	-0.031	[.038]	0.423	[.043]***	0.407	[.059]***
Poor air quality (0-5)	0.47	0.025	[.047]	-0.066	[.045]	0.313	[.056]***	0.299	[.090]***
Very bad smells	0.19	0.029	[.034]	-0.051	[.034]	0.250	[.039]***	0.139	[.057]**
Dust	0.35	-0.030	[.041]	-0.031	[.041]	0.044	[.045]	0.043	[.064]
Smoke from fire	0.12	-0.005	[.028]	0.042	[.030]	-0.096	[.025]***	-0.078	[.045]*
Smoke from machine	0.08	-0.014	[.020]	-0.034	[.020]*	0.092	[.026]***	0.051	[.034]
Other chemicals or bad air quality	0.14	0.061	[.031]*	-0.044	[.030]	0.374	[.038]***	0.355	[.052]***
Workplace conditions, (0-8)	5.23	-0.043	[.143]	0.357	[.147]**	-0.804	[.155]***	-0.763	[.211]***
Good physical comfort	0.84	-0.047	[.035]	-0.033	[.034]	-0.062	[.038]	-0.093	[.054]*
Temperature is comfortable	0.76	-0.050	[.038]	0.043	[.037]	-0.266	[.043]***	-0.296	[.058]***
Work hour control	0.69	0.024	[.040]	0.078	[.040]*	-0.183	[.044]***	-0.090	[.070]
Easy to get time off	0.73	-0.049	[.038]	0.069	[.039]*	-0.260	[.043]***	-0.185	[.064]***
Opportunity to learn skills	0.42	0.054	[.042]	0.068	[.044]	0.212	[.044]***	0.256	[.067]***
Has enough space	0.62	0.032	[.041]	0.030	[.044]	-0.044	[.045]	-0.099	[.068]
Can take short breaks	0.64	0.020	[.042]	0.027	[.042]	-0.160	[.044]***	-0.165	[.067]**
Work is interesting	0.52	-0.026	[.044]	0.085	[.045]*	-0.046	[.046]	-0.095	[.070]

Notes: Treatment effects in Columns 1 to 5 are calculated as in Tables 6 and 8. The estimates in Columns 6 to 9 come from an OLS regression of each outcome on an indicator for working in industrial employment in the past month for both the entire sample and for those with non-zero hours of work. In columns 8 and 9, we also include an indicator working more than 15 hours in non-industrial work in the past week.

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

but also smoke), a more than 40 percentage point increase in the perception of a need for safety equipment, and at least a 35 percentage point increase in complaints of chemicals. In general, air quality and workplace comfort is moderately lower in industrial as opposed to other forms of work, even excluding the unemployed.

5.4 Impacts on mental health and happiness

Table 8 also considers a number of self-reported facets of mental well being.[45] In general people report very few symptoms of depression or anxiety. We also asked people four questions about severe financial worries. These are also relatively rare, perhaps because many are young people living at home with lower middle class families, and generally have enough to eat. Finally, to measure subjective well being (happiness) we used the same life evaluation ladder as with health, but where the top rung represented the best possible life and the lowest the worst possible.

We see no evidence of an effect of the job offer on mental health, and weak evidence of an increase in subjective well-being now (but not in five years). The entrepreneurship arm, however, led to small and statistically significant decreases in financial anxiety, as well as a rise in subjective well-being. There were also small declines in depression and anxiety symptoms. Overall a family index of all these measures increase 0.23 standard deviations (significant at the 1% level using conventional thresholds and at the 5% level after we adjust for eight outcomes).

5.5 Impacts on indirect outcome families

In Table 11, we see no statistically significant changes in our secondary measures, which include firm attitudes (e.g. whether firms pay fair wages, or respect worker rights); social integration (including family, social, and community connectedness and trust); autonomy (including financial independence and freedom from coercion in the household); or non-cognitive skills (including conscientiousness, self-esteem, and punctuality).

Appendix D.7 describes these measures in more detail, where none of the individual measures making up these family indices have a statistically significant treatment effect. We measured them in anticipation of low turnover and substantial effects of the job offer on occupations and earnings. Given the absence of a large direct effect of factory job offers on

[45] We piloted several standard depression and anxiety questionnaires to determine what appeared to work best in our population, in part by ease of respondent understanding and in part by high levels of within-scale intercorrelations. For depression we use the Patient Health Questionnaire (PHQ-9), a nine-item scale incorporating depression diagnostic criteria (Martin et al., 2006). To evaluate clinical anxiety and stress, we use the Generalized Anxiety Disorder-7 (GAD-7), a seven-item scale (Spitzer et al., 2006).

well-being, the absence of indirect effects is unsurprising. It is worth noting, however, that despite the relatively large effects of a entrepreneurship program on occupational choice and earnings we see little effect on social integration or empowerment.

5.6 Robustness and sensitivity analysis

Table 12 reports sensitivity analysis for key outcomes. We report: pooled endlines using randomization inference (Column 2), difference-in-difference effects (Column 3), ITT effects averaging the 11- and 13-month outcomes (Column 4) and conservative attrition scenarios (Columns 5 to 7).[46] We estimate attrition bounds by imputing outcomes for unfound individuals at different points of the observed outcome distribution, focusing on the cases that reduce program impacts. For positive outcomes we impute the observed mean plus x standard deviations of the distribution for the control group, and for the treatment group we impute the observed treatment mean minus x standard deviations of the distribution., for $x = 0.1$, 0.25, and 0.5. In general treatment effects are robust to all specifications. The increase in disability risk among the job arm, and the increase in income in the entrepreneurship arm, are smaller and less precise under extreme attrition scenarios. The direction of these effects and qualitative findings are unchanged, even in the worst case scenarios.[47]

6 Discussion and conclusions

Overall, the results suggest that that industrial firms in Ethiopia paid no better than worker's informal alternatives, so that most workers were at best indifferent between these forms of work. This suggests a formal and informal labor market that was more fluid and competitive than we expected, at least for the young, unskilled, and capital-poor. When these young people's constraints on self-employment were lifted, however, they avoided industrial work and tended to start small but sustainable microenterprises that had raised their meager incomes by a third a year later. We see no evidence that entrepreneurship is undesirable or increases income risk—two of the most common charges levied against self-employment. The existence of competitive labor markets and subsistence-level industrial jobs would seem innocuous if not for evidence of adverse health effects of industrial work.

[46] The results are also highly robust to clustering standard errors by factory, though this is an unnecessary adjustment because of the individual-level randomization, and a potentially problematic one given the small number of clusters.

[47] Appendix D.8 also reports firm-by-firm treatment effects and illustrates robustness to omitting one firm at a time. It also shows that effects are similar by gender.

Table 11: Treatment effects on secondary outcomes

Outcome	Control mean	ITT estimate (N=1587)							
		Job offer			Entrepreneurship program			Job - Entrepreneur	
		Coeff.	Std. Err.	Adj. p-val.	Coeff.	Std. Err.	Adj. p-val.	Coeff.	Std. Err.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Anti-firm/pro-union attitudes, z-score	0.00	0.05	[.085]	0.917	-0.09	[.089]	0.755	0.14	[.090]
Firms good for workers & country (0-24)	10.81	0.10	[.248]		-0.41	[.257]		0.51	[.259]*
Foreign firms good for country (0-24)	9.81	0.39	[.258]		-0.32	[.268]		0.72	[.268]***
Workers rights protected (0-24)	14.13	-0.17	[.252]		-0.04	[.269]		-0.13	[.267]
Pro-unions (0-24)	12.49	-0.07	[.294]		0.35	[.320]		-0.42	[.315]
Social integration, z-score	0.04	0.01	[.018]	0.949	0.00	[.018]	0.849	0.00	[.018]
Family support (0-15)	12.31	-0.01	[.210]		-0.02	[.223]		0.01	[.223]
Social support (0-15)	2.41	-0.08	[.072]		0.00	[.069]		-0.08	[.076]
Community participation (0-8)	9.55	0.17	[.233]		-0.13	[.229]		0.30	[.230]
Trust (0-12)	3.40	-0.15	[.151]		0.07	[.149]		-0.22	[.152]
Autonomy, z-score	5.72	0.25	[.195]	0.909	0.08	[.204]	0.849	0.18	[.206]
Independent decision-making (0-36)	0.05	-0.06	[.064]		-0.05	[.062]		-0.01	[.060]
Involvement in spending decisions (0-10)	5.43	0.16	[.265]		0.29	[.262]		-0.13	[.269]
Money freedom (0-12)	4.25	-0.02	[.209]		-0.07	[.217]		0.06	[.224]
Abuse received (0-21)	1.00	-0.12	[.150]		-0.17	[.136]		0.06	[.131]
Non-cognitive skills, z-score	0.08	-0.04	[.033]	0.909	0.03	[.035]	0.849	-0.07	[.036]*
Locus of control (0-24)	17.09	-0.05	[.238]		0.22	[.254]		-0.27	[.255]
Self esteem (0-24)	16.47	-0.39	[.258]		-0.11	[.276]		-0.28	[.275]
Patience (0-14)	2.91	-0.06	[.073]		0.08	[.078]		-0.15	[.077]*
Risk aversion (0-24)	1.79	0.03	[.055]		-0.02	[.058]		0.05	[.058]
Self control (0-24)	2.35	-0.02	[.054]		0.02	[.056]		-0.04	[.059]
Punctuality (0-15)	11.04	-0.24	[.168]		-0.08	[.171]		-0.16	[.172]

Notes: Columns (2) to (5) report the results of an OLS regression of each outcome on treatment indicators, baseline covariates, and cohort-gender fixed effects. 11- and 13-month survey responses are pooled with robust standard errors clustered by individual. P-values are adjusted for family outcomes using the Westfall-Young approach described in section 5.1

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

Table 12: Sensitivity analysis of treatment effects to alternate models and missing data scenarios

	ITT estimate under alternative models				Sensitivity of ITT to attrition		
					Impute missing dependent variable with mean + (-) X SD for missing control (treatment) individuals		
Outcome	Result from Tables 6 and 8	Randomization inference	Diff-in-diff estimate	Average 11- and 13-month responses	0.1 SD	0.25 SD	0.5 SD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment: Industrial job offer							
Working in any industrial firm at endline	0.108	0.108	0.101	0.108	0.092	0.081	0.065
	[.034]***	[.033]***	[.027]***	[.035]***	[.033]***	[.033]**	[.033]**
Weekly earnings, 2010 Birr (lower bound)	3.049	3.049	1.299	3.351	2.164	0.181	-3.126
	[4.479]	[5.317]	[4.525]	[4.615]	[4.152]	[4.163]	[4.203]
Weekly earnings, 2010 Birr (upper bound)					-2.628	-0.928	1.905
					[4.954]	[4.968]	[5.009]
Disability	0.033	0.033	0.044	0.033	0.030	0.024	0.018
	[.015]**	[.015]**	[.016]***	[.015]**	[.014]**	[.014]*	[.014]
Treatment: Entrepreneur							
Working in any industrial firm at endline	-0.121	-0.121	-0.136	-0.121	-0.106	-0.095	-0.078
	[.028]***	[.032]***	[.024]***	[.029]***	[.027]***	[.027]***	[.027]***
Weekly earnings, 2010 Birr (lower bound)	12.005	12.005	11.931	12.164	10.090	8.202	5.055
	[5.463]**	[5.385]**	[5.445]**	[5.600]**	[5.050]**	[5.060]	[5.093]
Subjective well being, now compared to one year ago	0.239	0.239	0.111	0.234	0.206	0.176	0.127
	[.071]***	[.076]***	[.094]	[.073]***	[.065]***	[.065]***	[.066]*
Observations	1587	1587	1587	835	1841	1841	1841

Notes: Column 1 replicates the main tables. Column 2 uses randomization inference. Column 3 calculates difference-in-difference effects where baseline data are available. Column 4 averages instead of pooling the 11- and 13-month survey outcomes, so that there is one observation per person and down weights respondents only found in one endline. Columns 5–7 impute the mean of the control (treatment) group plus (minus) "X" standard deviations of the group's distribution (SD), for X = 0.1, 0.25, 0.5. All regressions include a vector of baseline covariates and gender-cohort fixed effects. Some outcomes contain fewer observations than the listed number of observations because a very small number of respondents were not asked certain questions. Standard errors are robust. *** p<0.01, ** p<0.05, * p<0.1

6.1 Why did people take poorly paid, hazardous jobs?

There are only a small number of possible explanations. One is that the severity of the health risks were not apparent to applicants, or even to active industrial workers themselves, and so they did not demand a compensating wage differential. A second possibility is a learning and matching story, where industrial jobs pay a compensating differential (or are otherwise worthwhile) for some, but that workers need to learn about their own nature or the nature of the work through the process of working (i.e. learning and matching). A third possibility is that firms screen and match workers only after they join the firm, promoting good matches and terminating bad ones. A final possibility is a smoothing shocks story, where industrial jobs do not pay a compensating differential when informal work is available, but are available during informal unemployment spells and other shocks. All but the last one require some kind of imperfect information.

We do not have the data or research design to test these alternatives formally. Eight cohorts in five firms do not offer enough variation or power to conduct a formal analysis, as the relevant number of firm and environmental factors exceeds the number of cohorts. Nonetheless, a combination of manager and worker interviews, patterns of treatment effects, and analysis of worker reports suggest that firms seldom screened, and workers generally understood the health risks, but that they took jobs anyways, often temporarily, to smooth consumption or to learn their fit with the position.

Imperfect information about health risks We do not have baseline data on expectations of health risks but, as reported in Table 10, we asked the sample to describe their workplace conditions at endline. Factory and commercial farm workers were significantly more likely to see their workplace as risky and in need of greater safety protection than provided.

We see no evidence that people updated their beliefs about the harmfulness of factory work as a result of the treatment, however. The "attitudes to firms" index in Table 11 includes one health-related attitudinal question: whether "factories provide an environment that will not cause injuries or longer term health problems". On a 0 to 4 scale, the control group reported 1.99 on average. The ITT coefficient (and standard deviation) on the job offer was just 0.16 (0.10), and the CATE for months in formal work was 0.05 (.034). If workers learned of risks on the job, we would have expected a larger and more robust estimate.

Screening and matching We can eliminate the screening and matching explanation simply based on how the firms appear to function. In models by Jovanovic (1979) and others, labor turnover is driven by imperfect information, as firms only learn about workers and

their match quality (productivity) by hiring them. Workers apply where they may be a poor match, and firms hire more workers than needed. Employers reward good matches with higher wages or promotions and fire the others. If there are regulatory or other barriers to firing, then poor matches could be offered lower wages or unpleasant working conditions in the hopes they quit. One prediction would be a tenure premium: a steep earnings-experience relationship, not due to any direct causality in the form of experience leading to higher productivity and earnings, but rather from a correlation as those who are observed to stay in the firm for longer are the better, more productive matches for these industrial jobs.

We see little evidence in Ethiopia to suggest this screening mechanism explains our findings. In the five firms we worked with (and the many other firms we interviewed as candidates) none of the management described screening employees in this way. There was typically limited room for advancement, especially for women who made up the majority of our sample. There were also few regulatory barriers to firing workers. In general, firm managers were puzzled and dismayed by the high levels of turnover, and would have preferred to see all of the applicants stay.

The managers we interviewed also made efforts to add non-wage benefits, such as offering free transport, free clinics, and in some cases a free meal at a canteen. They appeared less likely to raise wages, however, and indeed often failed to keep pace with inflation and keep real wages constant. The reasons for not paying higher wages were hard to discern, and we gained a range of impressions from the many firms we interviewed: a belief that higher wages would not be profit-maximizing; a belief they might be but cash flow problems at the firm level made a higher wage bill impossible; and a tendency to money illusion, or thinking about wages in nominal terms and failing to keep pace with inflation.[48]

We do see a slight rising wage profile over time among the workers who stayed in industrial jobs, but the gains are not dramatic. In Appendix D.9 we regress endline earnings on a Mincerian-style set of baseline covariates, including of age, gender, health, education, and work experience. In no instance is prior industrial experience associated with higher earnings, and tenure with the firm is only weakly associated with a rise in earnings. Given that we do not observe any screening or involuntary separations in practice, this modestly rising wage profile likely reflects self-selection and rising productivity more than screening.

Shocks The patterns we observe could reflect attempts to smooth consumption in response to shocks or changing needs by changing labor allocation, such as in models of added worker effects and other labor market responses to shocks (Morduch, 1995).

[48]Indeed, even the authors succumbed to this money illusion, initially failing to keep survey enumerator wages in pace with inflation until a correction was made after high quit rates and complaints.

It is worth noting that nearly all of the applicants we interviewed described industrial jobs as temporary in the sense that they did not expect to work there for more than a few years. For example, women commonly explained that they only planned to work in the formal sector for only a few years, until they had children and took on child-rearing, household occupations, and more flexible part-time self-employment. Many young people, perhaps even a majority, also expressed a preference for one day running their own businesses. Formal sector jobs were temporary jobs while they accumulated enough savings to start an enterprise. Among those who did see a career in the formal sector, they commonly saw the factory as a stepping stone to white-collar or other non-industrial employment. These patterns are largely consistent with recent labor market studies and ethnographic work in Ethiopia, which find that higher paid white collar jobs are preferred and that roughly half of factory workers are dissatisfied with their job (Mains, 2012; Franklin, 2014). Recent panel studies of young adults with some secondary education suggest that young people typically take temporary, often unsatisfactory work, in different places, varying from week to week. Few can afford to be completely unemployed for long stretches, and rely on family networks for short periods before returning to some temporary work, all while searching for higher quality permanent jobs. Both this survey and ethnographies of youth and work in Ethiopia stress how longer-term, contractual jobs are prized, especially white collar jobs in the private and public sector (Mains, 2012). Industrial jobs of the kind we studied appear to belong in the undesirable rather than the desirable category.

This does not explain the often very short tenure of most of the people offered a job in a study firm, however, especially since quitters often exited the industrial sector entirely. Unfortunately we do not have data on pre-application shocks, nor do we have sufficiently detailed employment histories to distinguish new entrants from recently or long term unemployed. Even if we did, unobserved selection into the experimental sample would cloud any test.[49] Hence sharp tests of the shocks hypothesis are not possible. Nonetheless, there is some evidence consistent with industrial jobs as a response to temporary shocks.

First, in interviews, some applicants described the factory jobs as undesirable and short-term occupations while they found more interesting, respectable, or easier work for better wages. In these accounts, they seemingly understood the poor conditions, difficulty, and low pay of industrial work, but endured it for a few weeks or months between other, better jobs.

Similarly, a 2012 urban labor market survey found that young workers, especially the higher-ability and more ambitious ones, churn through multiple jobs, treating them as temporary places to earn money while they search for something better (Franklin, 2014, 2015).

[49]For instance, we cannot easily distinguish between an adverse shock due to chance or unobserved characteristics of the worker, such as motivation and other factors that affect outside options.

That is, seasonal or short-term but higher-paying informal work is regularly available, and less desirable jobs (such as industrial work) are used in the interim.

Indeed, as described in Section 5, the average job applicant had other, largely informal employment opportunities that paid similar wages with fewer disamenities. And when the grant was offered, giving people marginally more earnings in informal self-employment, the percentage of people working in an any industrial job at endline halved, from 20% in the control group to 9% in the entrepreneurship arm.

Not all the evidence is consistent with this view, however. If shocks are influential, we might expect applicants with more liquid wealth to leave more quickly. But we see no heterogeneity in length of stay in the industrial job by baseline household assets (see Appendix D.10). Moreover, people commonly quit despite having no other full-time work opportunity, instead entering a spell of unemployment. As young people who commonly lived with their parents or a husband, they did not necessarily need to work full time, and given the low wage and demanding work many said they preferred to stop working full time and do petty jobs while searching for better full time employment. This is a common employment search strategy in Ethiopia. Hence shocks are probably only a partial explanation for the short tenure we observe.

Learning and matching The final possibility is akin to the imperfect information and matching case of Jovanovic (1979), but in this case the workers are the ones learning about the job. That is, a job is an "experience good", in that its full characteristics are only revealed to the worker when taking it on. While workers know the wage in advance, they do not know the non-wage characteristics, including how hard they have to work, the working conditions and other features (Nelson, 1970). As our sample is young and has little formal labor market experience, and since the sector is fairly young, applicants many not know their affinity for the work or the risks or disamenities in advance, and so they learn by doing. They quit if they discover the job was a poor match for them given wage levels and their opportunity cost. Recent models include Antonovics and Golan (2012); Papageorgiou (2014); Gervais et al. (2014).

Again, due to unobserved selection into the sample, it is not possible to test this interpretation formally.[50] However the qualitative data, along with labor market surveys, are consistent with this theory. For 81% of applicants, this was their first industrial job (indeed, for most it was their first formal sector job). We do not have data on pre-application expectations of the posts. But in both exit and qualitative interviews, some people reported they

[50]For instance, it's not clear what we should predict if an applicant had prior industrial experience. Should they stay a shorter time because they learn more quickly? Or are they a good match for unobserved reasons and are likely to stay?

had little sense of the nature or difficulty of the work, or the hazards involved a priori.[51]

Other applicants did say they had some information about the quality of the jobs in advance, from friends and neighbors, and that they expected the jobs to be unpleasant. But they said they underestimated the disamenities, or how little the pay were once they factored in the inflexibility of the work, transport time and costs, or the physical demands and risks. In both cases they tried out the job but left if the workload and disamenities were not worth their expectations for such work. Similar points are echoed by subjects in a weekly panel survey of young job searchers in the capital over the course of a year (Franklin, 2015).

Finally, recall that quitters did not not simply switch industrial jobs. Rather, they tended to exit the industrial sector entirely, suggesting they realized their poor fit. Hence, there is suggestive evidence for learning and matching on the part of workers contributing to the high turnover rates observed.

6.2 Generalizability

If we want to extrapolate to the case of Ethiopia, it is important to note our five firms were largely domestically and foreign owned exporters, sought out low-skilled and inexperienced employees, and paid below average wages, but not wages that were exceptionally low for for firms in these particular labor-intensive sectors. In this light, at best we have estimated local average treatment effects on young, entry-level workers to firms with large number of lower-skill jobs where employees are, in practice, interchangeable, and the labor market is not evidently segmented. They are in firms in sectors attracting considerable FDI, and seen as a future for this country and its labor force.

Clearly there are limits to what we can learn from five firms. It's conceivable that industrial labor markets in other countries, or higher-skill industries, are more segmented. Even so, it's worth noting that the patterns we observe—low wages and high rates of turnover—have been relatively common features of industrial jobs from modern day higher-skill Chinese manufacturing to industrialization in the US or UK as recently as a century ago. Hence our firms and worker experiences may not be so exceptional.

For instance, one study of turnover at an Apple supplier in eastern China found that 92% of workers leave within six months of hire, and weekly rates of exit average about 7% (Cohen et al., 2015).[52] Another example comes from Jordan, where Groh et al. (2014) not

[51] Applicants with prior industrial experience should have some sense of the job. Yet in Appendix D.9 we see that prior industrial experience was associated with lower endline earnings and higher turnover. Rather than being better matches, these could be people who were poor matches with industrial jobs who took the work because of adverse shocks. It is such a self-selected sample, we hesitate to infer much from the patterns.

[52] A poll of Chinese workers and firms found that over 40% of employees stayed at their previous companies for just one to two years, and only 38% of polled firms had prepared strategies to retain their workers even

only show that it is difficult to create matches between employers and workers searching for work, but that a majority of workers quit their "successful matches" within a few months. It's not clear whether these represent churn across sectors or within the industrial sector, and within-sector churn is undoubtedly part of the activity.

There is substantial historical evidence of cross-sector churn and industrial exit in the West. For instance, Beckert (2014), looking at early textile development, found high turnover and exit to be the norm from Barcelona to Liverpool to New England. For example, he writes: "The Dover Manufacturing Company in Dover, New Hampshire, had to employ a total of 342 workers in the period from August 1823 to Oct 1824 just to maintain an average workforce of approximately 140. ... Entering the factory for a few weeks, they would leave once they had made enough money to hold them over to the selling of their crops or when their labor was needed on the farm."

Similarly, Montgomery (1989) and Kaufman (2008) note how, between 1900 and 1920, US companies became aware of (and obsessed with) high turnover. In 1913 turnover at Ford Motor Company was 370%, and General Electric had turnover rates exceeding 100% in its plants. While the revolution in scientific management, or "Taylorism", is often associated with increasing productivity, reducing employee turnover was actually its initial focus. Increased screening was one of the innovations introduced.

The point is that high turnover (within and across sectors) is a historically common feature of manufacturing. The features of our Ethiopian case—low industrial wages and high sectoral turnover—could be influenced by the fact that the country is still at an early stage of industrialization, with relatively few manufacturers competing for workers. Low wages and high turnover could also be driven by poor personnel management. The story of US manufacturing above is one of innovation in management practice. Ethiopian firms may have yet to adopt and adapt these innovations.

To answer the question of generalizability requires replication, especially in countries at different stages of industrialization, or where the degree of industrialization and competition for labor varies over time. The openness of so many firms to participate in the study suggests replication elsewhere is possible.

6.3 Implications

However unattractive these jobs are to the majority of workers, we do not conclude that Africa needs fewer low-wage manufacturers. As we saw, these jobs paid more than the alternatives available to a substantial fraction of workers. Moreover, advocates such as Paul

though nearly all said they were aware of the negative impact of turnover (Hays PLC, 2012).

Krugman (1997) have argued that poor countries need more sweatshops not because they pay wage premiums, but rather because worker wages should rise as more and more firms begin to compete for a increasingly smaller pool of experienced workers, and as firms begin to adopt more technologically advanced production that is complementary to human capital. If so, wages would rise in general, in the informal as well as formal sector, and we would not expect an experiment such as ours to yield treatment effects on income.

Even so, the adverse health effects suggest an important role for information or regulation, especially if workers face these risks uninformed, or are time-inconsistent in their choices over short-term gains at long-term costs. For instance, the government or firms could begin to offer disability insurance, insist that firms communicate risks more clearly, or identify and limit the use of the most hazardous chemicals.

Regulation, of course, risks raising labor costs and benefiting insiders at the expense of outsiders. An important direction for more research is the costs and benefits of regulation. An important example is Harrison and Scorse (2010), who show that anti-sweatshop activists campaigns in Indonesia led to large real wage increases in targeted enterprises, with some costs in terms of reduced investment, falling profits, and increased probability of closure for smaller plants, but no adverse employment effects.

Understanding better why firms do not try to combat high turnover, for example through paying efficiency wages, may also lead to better and more efficient outcomes, especially if the low wages being paid are suboptimal. It is possible that formal and informal firms alike are poorly managed, or that the sector is constrained in unobserved ways. But it is also possible that, in very unskilled industries, high turnover at very low wages, where workers with the poorest outside options remain, is the profit-maximizing choice. The efficacy of management and the take-up and effectiveness of modern human resource practices in the firm is an important but unexplored area of research.

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Appendix for online publication

A A brief history of the Ethiopian labor market

Over the last three decades, Ethiopia has seen the transformation of urban employment dominated by public sector work, paying skills premiums with considerable queuing unemployment, to one that is more flexible, with a rising importance of private sector work, without obvious skill premiums between the private and public sector, and with lower (but still considerable) unemployment. The private sector labor market has limited unionization and no minimum wages, and firms face few restrictions in wage setting, hiring or firing. We draw these conclusions from a literature that draws on large sample cross-section and panel data surveys in urban areas, focusing on descriptive statistics, and regression analysis using participation selectivity corrections.

Before 1991, labor market data suggest a dual labor market in Ethiopia, as urban young people queued for public sector jobs, allocated in part by patronage. Ethiopia was a controlled (command) economy, with strong controls on labor mobility. There was centralized recruitment and deployment of civil service personnel and employees of publicly owned companies. Workers were not allowed to move without permission. The private sector was repressed, with very few larger private sector companies. By 1989, more than 75 percent of those aged 15-29 in formal wage work were working for government and state-run enterprises.[1] The public sector was paying better than the private sector. Krishnan (1996) estimated selectivity-corrected Mincerian earnings regressions to find returns to secondary education that were 65 percent higher for public sector employment than private sector wage work. She also found that connections (in the form of family background) strongly influenced whether a public sector job could be obtained.

Post-1991, these restrictions were gradually removed, with the end of central recruitment. Still, using data from 1994, Serneels (2007) found that the informal sector remained small and the urban labor market preserved the qualities of a dual sector with queuing for scarcer public jobs. Using data from a random sample across 7 urban areas, 80 percent of male adults in a wage job were employed in the public sector. The data suggest there was a large pay gap between public and private organizations—80 percent higher pay in the government sector for men (although that is not by skills). With a third of the labor force supposedly looking for work, there was also large unemployment, larger than what we usually see among urban workers in sub-Saharan Africa.[2] The urban informal sector was also surprisingly small throughout the 1990s, as there were about as many people in formal wage work (private and public) as in the informal sector.

Thus urban unemployment was most highly concentrated among very young men. In 1994, half of men between 15 and 30 reported they were unemployed, but this peaked at 19 years of age (Serneels, 2007). In 1989, only a third of the same group was unemployed, suggesting a rapid increase (Krishnan, 1996). Rather similar to 1989, however, in 1994 young men had a median duration in unemployment of nearly 4 years, and a majority of the unemployed had never held paid work in their lifetimes. Half the men aged 15 to 30 reported searching for a job in the public sector, implying job queues for this sector were far in excess of the employment opportunities. Of the remainder, half were indifferent between informal sector and private sector wage work.[3]

[1] Krishnan (1996), using data from the Survey of Adolescent Fertility, Reproductive Behavior and Employment Status of the Youth Population in Urban Ethiopia, 1990, representative of urban areas.

[2] See Kingdon et al. (2006) for a comparison with Ghana, Uganda and Tanzania.

[3] Education raised unemployment as well, and as returns to education remained higher in the public sector in this period (Krishnan et al., 1998), this is suggestive of a 'queueing' story. Similarly, having a father in the civil service

But a shift towards informality was already underway in the 1990s. Comparing panel data for the same group of young male adults in 1989 and 1994, among those working in 1989, 58% were in public sector wage work and only 20% in the informal sector. By 1994, this was 31% in public sector wage jobs and more than half in the informal sector. So while there was still queuing, probabilities of success were declining fast.

From 1994 to 2004, private sector jobs began growing quickly.[4] Not only were far fewer public sector jobs created in urban areas, but also central recruitment and other restrictions on the inter-sectoral movement of labor were removed, resulting in much more labor market flexibility. Private formal sector wage employment doubled in this period. Large public wage premiums remained, but the link between this premium and skills disappeared. There was also increasing mobility between these three sectors (private wage, public wage and informal). The increase in the rates of mobility, especially after 2000, across sectors of employment was accompanied by a small but persistent decline in the rate of open unemployment.

Franklin (2014) used data from the Urban Employment and Unemployment Survey to assess labor markets in 2012. Unemployment among those aged 15-29 was estimated now at about 25%. Many of them aspired still to government jobs. For very low levels of education, they still pay better, though there is no skill premium anymore for higher levels of education. But the labor market is much more flexible and the private sector plays an increasingly important role offering stable employment opportunities. Median unemployment spells for these young adults are 13 months, and there is much evidence of temporary wage work, mainly in the private or informal sector. Of those in work, a quarter were in public sector jobs, a third in private sector jobs and the rest in the informal sector including domestic work.

Labor hiring, firing and compensation is governed by the 2003 Labor Law (FDRE, 2003). It does not stipulate a minimum wage, but allows for trade union activity and collective bargaining, without requiring it. Active unions and collective bargaining, including over minimum wages, are very common in the public sector and state-owned enterprises, but not in the private sector (ILO, 2011). Unions are present in some private enterprises, but union activity is largely focused on workers' rights and benefits, and on specific labor conditions. Unionization remains limited, with less than 13% of workers on salaries or wages belonging to a union (ILO, 2011), In general, employers can set wages without frictions in the form of legal restrictions or reference to unions.

B Firm and cohort details

This section provides additional firm, job, and process details for each cohort. Table B.1 summarizes details of each cohort's recruitment, randomization, and grant implementation.

B.1 Beverage producer

The beverage plant is located in a town of roughly 200,000 people in the Oromia Region, around 20 km outside Ethiopia's capital Addis Ababa. The plant manufactures bottled spring water as well

raised unemployment—but duration of unemployment was lower the higher family wealth and connections (Serneels, 2007), similar to the Krishnan (1996) results: particular types have good reasons to queue, as they can be more successful to get the prized public sector jobs. This is not inconsistent with the early conclusion by Myrdal (1968) that unemployment in developing countries like Ethiopia is a 'bourgeois phenomenon'.

[4]See Bigsten et al. (2013) who use panel data covering 10 years. The data have some problems: this is a panel so we observe aging and attrition of older workers.

Table B.1: Cohort recruitment, randomization, and implementation details

Characteristic	Beverage producer		Horticulture farm	Flower farm	Shoe factory	Garment & Textile factory		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Job start date	5/4/2010	5/4/2010	3/10/2011	2/11/2011	28/1/2013	21/5/2012	11/5/2013	24/6/2013
Number of openings	15	19	30	50	50	30	45	65
Minimum education	8	8	n/a	n/a	n/a	8	6	6
Applicants	288	101	101	~170	230	114	210	263
Eligible applicants	60	68	90	152	190	90	197	226
Surveyed and randomized	53	68	89	152	158	89	140	188
Randomization strata	None	Gender	Gender	None (all female)	None	None	None (all female)	None (all female)
Assigned to job offer	15	23	30	50	59	30	45	60
Assigned to control	38	27	30	52	58	30	50	68
Assigned to grant	0	22	29	50	50	29	45	60
Grant tranches		2	2	2	1	1	1	1

as flavored water in various bottle sizes, mainly for domestic consumption. In 2010, the plant was more than a decade old, had approximately 150 employees in total, and shared the site with three non-beverage firms owned by the same parent company, a domestically owned investment firm. We learned of the firm and the opening through personal contacts of the authors.

The firm operated six days a week, 24 hours a day, with three 8-hour shifts, and workers rotate through shifts over time. The nightshift is unpopular. The firm had a reputation for being quite lenient with personal leave, personal breaks, or lateness, and some workers reported this is one reason they did not leave for other factories. About three-quarters of production staff are women. Women typically engaged in washing, labeling and bottling, whereas men performed more labor-intensive work such as packaging, loading, and operating heavy machinery.

Ownership changed several times between 2009 and 2014. In 2014, upon visiting the factory, we learned that it had filed for bankruptcy, had been closed for some months, but had just reopened.

Workers were organized in a local union, which mainly advocated for small changes to working conditions (e.g. reassigning pregnant women to physically undemanding tasks), advocating for benefits (such as maternity leave), and on rare occasion organizing walkouts in the event of late pay.

Compensation In April 2010, the plant offered starting salaries for 350 Birr a month to unskilled workers, with wages rising up to 600 Birr for more experienced workers. Managers and workers all agreed that pay is based mainly on seniority rather than productivity and ability, although ability and experience is one factor in promotion to more complex and higher paid tasks. Inexperienced workers begin with simpler jobs, in the bottle washing department for instance, but can graduate to higher skilled jobs in the firm. There are no incentive programs or bonus schemes. The cohort we study was not eligible for benefits, though in later years the firm introduced three months of maternity leave and transportation services for local commuters.

Firm managers were aware that turnover was high in part because other factories in Burayu were paying higher wages, in some cases twice the wages of Burayu, and because seasonal construction work also offered men higher wages. They increased starting wages several times from 2010–13, in part to keep pace with inflation and in part to reduce turnover, but their wages remained at the lower end of the scale compared to other factories in the town. When asked why wages were not increased further managers noted that they were uncertain whether this was profitable, or how much it would reduce turnover. One manager felt that turnover had only a modest impact on the firm because experienced workers were not required for the work, though it meant they seldom operated at full capacity as a result. Managers were also aware that the work flexibility and leniency allowed them to pay a lower wage (which some workers confirmed). They explained they were also cash flow constrained, and were facing increasing competition and falling real prices from new beverage producers, and so raising wages was not financially possible.

Health and hazards Most of the issues reported were concerning the chemicals used for washing bottles. In interviews, workers attributed respiratory issues, skin rashes, fainting, and in one case a lost pregnancy to these chemicals. From 2010–14, one serious accident could be recalled, when a new trainee lost three fingers in a machine. In general, moreover, the work could be physically demanding, especially the lifting and carrying of heavy water bottles and packages, and standing a full shift at bottling machines. Also, the factory was not heated and could become quite cold at night (especially if wet), to which many employees attributed common respiratory illnesses.

Recruitment and randomization, first cohort (March 2010) For the first cohort, the firm sought 15 workers to work on an expansion project for producing 5 liter bottled water. They also planned to move some excess workers out of other jobs to staff this new line in addition to the 15 new hires. Firm managers expressed a preference for a specific number of females and males.

We assisted the firm in advertising the factory jobs in the greater Burayu area in 5-6 different sites between Burayu town and the next nearest town. Approximately 100 advertisements were posted for a period of 4 days.

This resulted in more applicants than expected: 327. The firm screened and deemed eligible just 60 eligible partly on qualifications (at least eight years of education and who lived in the town where the firm is located, since the firm pays transport costs to and from work) and partly on a first come first served basis. Prior factory experience and age did not influence eligibility. 7 of the 60 could not be located for the baseline or randomization and were dropped from eligibility before randomization. There were 19 men and 34 women.

The randomization of the 53 to the job offer or pure control group was not stratified. We conducted the randomization by public draw, for transparency and buy-in by the sample.

Recruitment and randomization, second cohort (March 2011) In the second wave of hiring, the firm sought 19 workers to work on a further expansion project replace workers who had left other lines. They expressed a preference for seven females and 12 males. Females are typically preferred for assembly line works such as packing, labeling and quality checking, while males are typically preferred for jobs that require physical strength, such as loading and unloading products.

We assisted the firm in advertising the factory jobs in the greater Burayu area in 5–6 different sites between Burayu town and the next nearest town. Approximately 100 advertisements were posted for a period of 4 days.

101 applications were received. Factory staff screened the applicants according to the understanding of working conditions and shift work, prior work experience, education, salary expectations, and proximity to the site. Consequently, 68 eligible applicants were identified by the firm and we surveyed all of them.

The 101 was sufficient but lower than expected and so two enumerators conducted informal qualitative interviews to assess the relative lack of interest in the job compared to the previous year. Major reasons included low pay relative to construction and public works day labor, as well as higher wages in other Burayu firms. The factory notice also requested that each applicant have a personal reference; even though this was never used in screening, it may have deterred some applicants.

We randomized using computer algorithm, stratified by gender. 23 (not 19) were assigned to the job offer, anticipating some refusals. (This is the only time we offered excess jobs to the sample. Refusals in future firms/cohorts would be offered to people outside the study sample.)

Grant intervention No cash grant program was conducted for the first cohort. The second cohort was the first time the cash grant program was conducted. We could not find an Ethiopian non-profit organization willing to disburse cash grants with only basic training, and so we hired a private consultancy to disburse cash and provide training. The training commenced at the end of April 2011, roughly three weeks after the jobs began. We disbursed the grant in two tranches, roughly a month apart.

B.2 Vegetable farm

The horticultural farm is one of the main Ethiopian exporters of fruits and vegetables. Using open fields and greenhouses with modern irrigation technology, the firm produces various types of vegetables, fruits and flowers for domestic and foreign consumption. It is comprised of six farms located in different parts of the country, with headquarters in Addis Ababa. The firm is foreign owned and managed. We first established contact with the farm through a national horticultural association, via local research managers.

We worked in a one-year old site roughly 300 kilometers south of the capital, just outside a local capital city of roughly 200,000 people. It operated several greenhouses producing vegetables primarily for export. It employed nearly 250 production workers. There was one daily shift, and workers were required to work 8 hour days, 6 days a week. In practice shifts could last 10 or 12 hours, workers being required to complete their daily tasks, and there would not be paid for these extra hours. In busy seasons workers would work the seventh day of the week, typically for double pay, but receive no days off that month.

Eighty percent of workers were female, due to a preference of the company to hire women, and all were permanent rather than part-time or seasonal employees. Workers performed a range of activities including land preparation, harvesting, planting, greenhouse maintenance, transporting products, cutting, and chemical spraying. The majority of workers lived near the farm and had their own crops to cultivate. Work at home was a major source of absenteeism.

The study firm is the largest commercial farm in the area. Its competitors tend to hire short term labor rather than offer permanent contracts, but also pay slightly higher wages as a result.

Workers established a local labor union shortly after the study cohort began working (one did not exist before because the firm was so new). The union was semi-active, and tended to inform workers of their rights and responsibilities, intervene if there was a disagreement between workers and supervisors, advocate for higher wages, inquire as to the reasons workers were fired, and intervene if the reasons were deemed insufficient. A strike was threatened in 2014 for the first time.

Compensation In 2011-12, wages varied from 480 Birr to 600 Birr a month (in 2011 Birr) depending on position. Harvesting and crop culture paid lower salaries, and breeding and chemical spraying paid higher salaries. Chemical spraying wages were partly to compensate for unpleasantness and risk, so those in the chemical department earned higher wages than others. Wages increased annually by about 10-15% (inflation was roughly 10-25% over the period). Managers and workers generally agreed that pay was tied to seniority, absences, and ability. The highest performing workers were recognized twice annually with prizes, and workers can receive end-of-season bonuses based on performance and attendance. Bonuses can be as high as 280 Birr per month.

Shortly after the study cohort began working, the farm began offering some transportation services to workers. Workers also had access to a clinic free of charge. Women received three months of maternity leave, and all received two weeks of annual leave, plus time off for emergencies.

Despite relatively high turnover, it was not a major management concern, in part because they were easily replaced and skills and experience were not deemed essential. It did mean that the farm was always running slightly below capacity however, and this was the chief disadvantage cited.

Health and hazards The workers we interviewed described chemicals and dust in the workplace as a major health risk, though many didn't specify how they were directly affected by it, and

their responses overall didn't seem to indicate high anxiety about their occupational hazards in the workplace compared to other study firms. Some workers described having eye and skin irritation from contact with the chemicals sprayed on plants, and one woman attributed a lost pregnancy to the chemicals. Chemical spraying staff were equipped with masks and were tested for chemical toxicity in their blood every three months. Those with elevated toxicity were reassigned to another department for 6 months before returning to chemical spraying.

There is no piped water at the factory, thus workers reported they sometimes get sick when drinking from stagnant water jugs provided by the firm. Others complained of eye issues resulting from the intense reflection in the greenhouse. The region is malarial, leading to high illness, but it is not clear the risk is elevated in the farm.

Recruitment and randomization We worked with the firm to post 20 advertisements at the farm premises and the furthest distance the farm allowed workers to come from (based on walking distance and public transport). The firm's human resource office registered 101 eligible applicants.

The HR officer deemed 90 of the applicants eligible, in that they were (visually) in good physical condition and between the ages of roughly 18 and 35. There was no educational requirement, unlike manufacturing firms. the firm preferred to hire 90% women, so the majority of the sample was female. We found and surveyed 89 of the 90. Some Amharic enumerators used local translators to deliver the survey to respondents who spoke only a local language. In future surveys we made sure to have local-language trained enumerators, for all cohorts.

We randomized the 89 via a computer algorithm: 30 to receive the job offer, 29 the grant, and 30 the pure control group. Randomization was stratified by gender.

Grant intervention The training commenced roughly two weeks after the jobs began. Grants were disbursed in two tranches, roughly a month apart. the same private consultancy ran the grant intervention.

B.3 Flower farm

The flower farm is located roughly 100 km south of Addis Ababa in the Oromia region. It is foreign-owned and had been established almost a decade before. The farm grows two species of cut flowers for export to Europe, and employs anywhere from 700 to 3,000 workers, depending on the season. As of December 2011 the firm employed over 2,000 people, a majority of whom were permanent production workers. Field research managers established contact with the firm through the national horticultural association.

The firm operates seven days a week, and workers work 5.5 days a week, in one shift a day. Three quarters of production workers are women. Production workers perform several tasks including plant breeding, propagation, crop culture, harvesting, cooling, and "elite" (i.e. preparing the mother plants for propagation). Most workers are from the surrounding area, or have migrated from the south and southeast regions of Ethiopia. They typically live with family members or other factory workers to minimize the cost of living.

The firm has an unusually active labor union, which established a collective bargaining agreement with management. This agreement details the benefits, leave, bonus, materials and protective gear provided to workers, and is negotiated each year. Additionally, the union is responsible for informing

workers of their rights and responsibilities, chemical re-entry scheduling (i.e. determining when workers may re-enter the greenhouse after spraying), and intervening if workers are fired unjustly or have disagreements with management. Indeed, there have been a few instances in which workers who management had intended to fire were kept on after the labor union intervened. There have been three strikes at the company, in 2011, 2012 and 2013, over pay, national pension contributions, and the expiration of contracts, and all strikes resulted in many of the worker demands being met.

Compensation In 2012, wages varied by department, from 422 ETB to 726 ETB ($24-41) a month. Those in the chemical department earned higher wages than others. Wages increased annually by 25% (inflation was roughly 10-25% over the period). There are 20 promotion levels, and each worker was promoted one level a year. Workers could also move up levels based on ability, according to supervisor evaluations. Workers generally agreed that pay was tied to both seniority and ability. There are no bonuses or incentive pay schemes.

Workers commonly work seven days a week or other overtime and receive 2.5 times hourly pay for these extra hours.

Shortly after the study cohort began working, the farm began offering some transportation services to workers. Workers also had access to a clinic free of charge. Women received three months of maternity leave, and all received two weeks of annual leave, plus time off for emergencies.

Management were aware of the high turnover issue, and felt that turnover negatively affects the firm because new workers must be trained for one month, thereby increasing costs and reducing productivity. Supervisors also agreed that turnover results in significant loses for the firm since new workers must be trained and remaining workers often need to work overtime to cover the increased work burden. In general this was one reason that the firm aimed to keep wages competitive with other farm jobs in the area, and also for the bonus scheme.

Health and hazards Workers complained of the excessive heat in the greenhouses, the chemicals being sprayed, and not being given sufficient water. Several workers complained the chemicals used cause skin irritation and respiratory issues. Workers are provided protective clothing, including masks, gowns, gloves, and closed-toed plastic shoes. The firm performs a chemical toxicity test on production workers every three months. If test results are below a certain threshold (which indicates one has been exposed to toxic chemicals), workers are moved to another department and tested monthly. When their levels return to normal, they are returned to their previous posts.

Regarding water, one jerry can is brought to each greenhouse in the morning. Everyone in the greenhouse drinks from this jerry can and once it's gone, workers are not permitted to drink additional water. Workers attributed kidney problems, urinary tract infections, swelling of the legs, and frequent fainting due to the excessive heat and inadequate water. The region is malarial, leading to high illness, but it is not clear the risk is elevated in the farm. The firm conducted an internal absenteeism study in 2012, which reported that 27% of workers interviewed self-reported that they became sick at least once as a result of the firm's working conditions.

Recruitment and randomization Advertisement by word of mouth was deemed sufficient to attract a large pool of applicants. The firm required applicants to be female and at least 18 years old. Registration and screening occurred concurrently. 190 applicants were registered, and a small but unknown number were turned away. The baseline survey and randomization took place the

following day, and 152 appeared for the survey. The firm preferred to limit randomization to these 152 since they displayed enough interest in the job to attend the survey.

We randomized the 152 via a computer algorithm: 50 to receive the job offer, 50 the grant, and 52 the pure control group.

Grant intervention The training commenced roughly two weeks after the jobs began. Grants were disbursed in two tranches, roughly a month apart. The same private consultancy ran the grant intervention.

B.4 Garment and textile factory

The firm has two main plants on the same site: one that produces and dyes textiles from raw materials, and another that produces garments, principally for export to the United States and Europe, including a large number of major retail brands. The firm is located just outside the capital of the Tigray region, Mekelle, about 775km north of the capital. It is domestically, owned, and had been in operation nearly a decade. In 2012 the firm employed over 1,400 people, a majority of whom were permanent production workers. Field research managers established contact with the firm through a firm visit.

The firm operates six days a week and workers work the full six days, eight hours a day. There were three overlapping shifts a day, with no night shift. 85% of production workers are women, and jobs are not officially segregated by gender. Production workers operated heavy machinery in the spinning and weaving and dyeing processes, and operated small machinery (such as sewing or cutting machines) in garment production.

The firm had an active labor union, which mainly advocated for small changes to working conditions and advocating for worker's rights and benefits. We were not aware of any organized labor actions or collective bargaining.

Compensation In 2012, the starting salary for production workers was 417 Birr per month, and management reported that after four to six months, depending on the job, workers could earn between 100 to 200 Birr more per month. The textile plant was typically staffed with the more experienced and higher paid workers, because of the skill required to operate the machinery. There were no other major textile or garment manufacturers in the area, and so most of this experienced was accumulated in-house. Workers could increase their pay principally by demonstrating ability and moving to more high skilled positions. Our study sample generally started in the garment manufacturing (lowest skilled) side of the business. There were no bonuses or incentive pay schemes during the study period, though one was introduced in 2014. Double overtime pay was offered for working a seventh day or holidays.

The firm offered some transportation services to workers. Workers also had access to a clinic free of charge. Women received three months of maternity leave, and all received three weeks of annual leave, plus time off for family events and emergencies.

Management were more concerned about turnover at this firm than the other study firms, in large part because the new workers must be trained. Losing workers diminished productivity and led them to incur direct training costs. Some managers attributed high turnover to the practice of

hiring workers with a 10th grade education or more, who would quit the firm to pursue higher-paying opportunities outside the manufacturing sector, or to continue their education. In 2014 the firm was considering lowering their minimum education level to sixth grade.

Health and hazards The most common health issue reported was respiratory infections (difficulty breathing, coughing, and congestion) from exposure to dust particles and chemicals in the factory. A couple of workers also maintained they had kidney infections from not being permitted to use the washroom or drink water throughout the day. One worker explained that there are only four or five toilets for over 1,000 workers; therefore, it is difficult to use the washroom in the allotted 15-minute breaks.

Recruitment and randomization (three cohorts in 2012 and 2013) This was a period of expansion for the firm, and workers needed to be replaced because of turnover, and so the firm sought to hire 30 people in May 2012, 45 in May 2013, and 60 in June 2013. All three cohorts proceeded similarly.

Jobs were advertised on the front gate, and we assisted the firm in posting advertisements within a few kilometers of the firm, including the capital city. Each hiring round advertised to a slightly wider area.

The firm required applicants to at least 18 years old within a specific education range: 8 to 12 years in the first cohort, and 6 to 12 in the second and third cohorts. Applicants with higher or lower qualifications were rejected. Staff then gave applicants basic physical tests: (i) for minimum height (to be able to reach necessary parts of the machinery); (ii) an eye exam; (iii) ability to walk back and forth rapidly; and (iv) a threading test (passing a thread through ten needles on a piece of wood). Ignoring applicants who did not meet the basic gender and education criteria (on whom we do not have data), 90 of 114 applicants passed the physical examination and were deemed eligible in the first cohort, 197 of 210 in the second, and 226 of 263 in the third cohort.

We randomized eligible applicants via a computer algorithm, only including those who re-appeared for a baseline survey. In the first cohort, a large number quit within the first few weeks, and the firm had trouble replacing them rapidly. Thus in the second and third cohorts we randomly selected about 40 applicants as a reserve list for the firm to draw from in the coming months, and excluded them from the study sample and initial job/grant randomization. In the first cohort, 30 received the job offer, 29 the grant offer, and 30 neither (no gender stratification). In the second cohort, 45 received the job offer, 45 the grant offer, and 50 neither (all were women). In the second cohort, 60 received the job offer, 60 the grant offer, and 68 neither (all were women). We did not survey or follow the reserve list individuals.

Grant intervention The training commenced roughly two weeks after the jobs began. Grants were disbursed in one tranche. This time we ran the grant intervention through a parastatal organization with the field research managers overseeing much of the activity, to reduce overall implementation costs (since the private firm was subject to taxation, which increased implementation cost by about a third).

B.5 Shoe factory

The firm has two manufacturing plants in Addis Ababa and 28 retail outlets, and manufactures leather shoes for both domestic and international markets. About 90% of sales are domestic, with the balance mainly to Italy and China. It was a parastatal in operation for more than 75 years and was privatized in 2011, being bought by an Ethiopian national. We worked in their largest plant, with over 700 production workers in 2013. We established contact with the firm through a survey enumerator's professional contacts.

The firm operates 5.5 days a week, in one shift a day, but managers and workers explained that there is effectively mandatory overtime evenings and weekends—about 1–2 hours a day when production demands it, plus Sunday. Production workers are both male and female and perform several tasks including leather cutter, sewer, sole adhering (gluing), stitching, packaging, and assistant or "helper" positions for several of these manufacturing tasks. The firm is centrally located in Addis Ababa and draws workers from around the city. Shoemaking is a longstanding and traditional enterprise in Ethiopia, and there are many large and small firms in the capital.

The firm has a labor union that bargains on behalf of workers and is involved when workers are fired (due to misbehavior and other issues). Additionally, shortly after the baseline survey, a few workers went on strike because they disagreed with their supervisors and asked that the supervisors be shifted to another section. They were partly successful.

Compensation This was the firm with the most sophisticated compensation scheme, and also the one firm where workers had the most upward mobility inside and outside the firm.

In 2013, compensation varied depending on the duties assigned, and appeared competitive for low-medium skilled labor in the area. The average monthly salary for helper positions (those our cohort was hired for) was around 715 Birr a month. Workers were promoted based on ability and experience (especially the operation of technical machinery and specialized tasks), and fast learners or those with previous experience may begin earning anywhere from 1,200 to 1,500 Birr a month within six months to a year of being hired. The firm would pay more skilled workers within a level a higher wage than others, even if this caused disgruntlement. There was a 300 Birr bonus for every employee after a year of work plus other bonus schemes contingent on the firm's profitability. Low-level workers could easily earn another 250 Birr per month in overtime pay per month.

The firm did not offer transportation assistance, but the salary was said to include a "transportation allowance". Workers also had access to a clinic free of charge. Women received three months of maternity leave, and all received two weeks of annual leave, plus time off for emergencies.

When asked to explain the low starting wage level, managers explained that new workers are typically unskilled and therefore require training. During this training period, the firm incurs additional expenses (e.g., in extra materials needed for training) and the trainee does not produce at full capacity. He further noted that because a regular employee must assist the newcomer, this experienced worker is also slowed down, thereby negatively impacting the firm's production and justifying the low wages new and unskilled laborers receive.

Unlike other study firms, turnover at the shoe manufacturer commonly came from (i) urban students taking temporary work until returning to school, and (ii) people going to work for other shoe firms. Because of the training investment, the firm viewed turnover as a major problem. This is one reason given for its complicated compensation scheme. Nonetheless, managers admitted that the cost of hiring and retraining a new unskilled worker was not that high.

Health and hazards According to a 2013 firm-led survey, 94 of the 103 workers polled (91%) believed working conditions at the factory were unfavorable to one's health. Specifically, workers cited a (i) lack of sanitation in the office and toilet, (ii) lack of safety equipment to mitigate the inhalation of fumes (such as glues), (iii) lack of skilled medical personnel on-site, iv) standing the entire day while at work, (v) high temperatures in the factory, and (vi) chemical fumes and dust particles. All of the factory jobs, with the exception of sewing, require workers to stand the entire day, causing health issues such as swelling in the legs. Because of the equipment, there were previous experiences with serious injuries to fingers and hands. Many workers also complained of kidney problems.

Recruitment and randomization Advertisement by word of mouth, and a notice on the plant was deemed sufficient to attract a large pool of applicants. There were no education or gender requirements, though the firm prioritized candidates aged 18 to 38 and disqualified applicants who suffered from a history of epilepsy, kidney, heart, or leg problems. Of 230 applicants, 190 were deemed eligible and were registered, and 158 appeared for the survey.

We randomized them via a computer algorithm: 50 to receive the job offer, 50 the grant, and 58 the pure control group.

Grant intervention The training commenced roughly two weeks after the jobs began. Grants were disbursed in one tranche. The intervention was handled by the parastatal partner.

B.6 Baseline characteristics by cohort

Table B.2 reports baseline summary statistics of cohorts by firm.

Table B.2: Summary Statistics by Firm

Baseline covariate	Beverage Producer (1)	Horticulture Farm (2)	Flower Farm (3)	Garment & Textile Factory (4)	Shoe Factory (5)
Age	22.60	22.61	22.05	20.90	23.68
Female	0.53	0.78	1.00	0.90	0.66
Unmarried	0.81	0.69	0.66	0.86	0.66
Muslim	0.06	0.10	0.13	0.00	0.06
Household size	3.66	6.45	3.51	4.40	4.06
Household head	0.34	0.18	0.24	0.22	0.21
Proportion household dependents	0.33	0.74	0.42	0.45	0.37
Total years of education and training	10.80	6.07	5.75	10.06	10.00
Executive function, z-score	0.24	-0.65	-0.38	0.17	0.10
Weekly cash earnings (2010 birr)	4.23	4.73	2.36	11.92	18.40
Durable assets, z-score	0.03	-0.67	-0.71	0.09	0.81
Ever worked in a large firm	0.55	0.36	0.28	0.12	0.32
Average weekly hours of work	3.67	7.52	4.59	7.76	14.05
No hours in the last X weeks	0.68	0.46	0.67	0.73	0.64
Highest - lowest earnings, past month	249.83	150.63	152.23	193.47	222.30
Could borrow 3000 birr	0.36	0.25	0.28	0.32	0.34
Ability to do activities of daily life (0-15)	14.54	14.39	14.45	14.50	13.82
Disabled	0.01	0.04	0.00	0.00	0.03
Risk aversion, z-score	0.07	-0.41	0.04	0.10	-0.12
Future orientation, z-score	0.77	0.27	0.14	-0.26	-0.17
Locus of control index, z-score	-0.03	-0.30	-0.24	0.22	-0.18
Self-esteem index, z-score	-0.08	0.02	-0.20	0.13	-0.12
Family relations index, z-score	-0.12	0.35	0.26	-0.10	-0.10
Friends and neighbors relations index	0.08	0.52	0.49	-0.20	-0.29
Change in subjective well being, past yr.	0.26	0.46	0.13	0.34	0.30
Symptoms of depression, z-score	0.15	-0.29	0.07	0.04	-0.14
Symptoms of anxiety, z-score	0.05	-0.18	0.07	0.03	-0.10
Aggressive or hostile behaviors, z-score	-0.54	-0.26	-0.14	0.20	0.14
Conscientiousness index, z-score	0.16	0.42	0.27	-0.05	-0.46
Years experience, private firm	0.87	0.56	0.53	0.14	0.60
Years experience, workshop	0.05	0.01	0.00	0.00	0.02
Years experience, government/NGO	0.13	0.15	0.03	0.04	0.18
Probability of better job, next month	0.70	0.62	0.70	0.70	0.60
Probability of full-time work, next month	0.50	0.54	0.58	0.58	0.60
Number of Participants	121	89	152	427	158

Notes: Medians are imputed for baseline variables with missing observations.

C Comparison of study firms to other firms

Firm data come from the 2014 Addis Ababa Large Employers Survey (Abebe et al., 2014), a representative sample of all small to large firms in the greater Addis area. We collaborated with this survey and supplemented it by surveying all five study firms, 8 manufacturers in Mekelle (in northern Ethiopia, the location of one of the study firms), and 9 flower and vegetable farms. We call this the supplemented sample of large manufacturing and farming firms.

In table C.1 below, we compare our firms to the supplemental sample, restricting the comparison to firms in the manufacturing and farming sectors with at least 50 employees. Our five study firms are significantly larger than the typical Ethiopian firm, with over four times as many employees as the typical firm in the sample. Workers in study firms are paid less than workers in other firms, but they are also less educated than workers at other firms in the sample.

Table C.1: Comparison to Large Manufacturing and Farm Firms

	Full Sample		Blattman-Dercon	
	Mean	Median	Mean	Percentile
Number of years the firm has been in business (as of 2014)	29.14	21.00	29.60	0.71
In Addis Ababa	0.86	-	0.40	-
Majority share government owned	0.04	-	0.00	-
Majority share domestically owned	0.72	-	0.60	-
Majority share foreign owned	0.18	-	0.40	-
Certified international quality	0.29	-	0.40	-
Average annual sales revenue (2004-2006), 000s ETB	148,675	46,063	206,987	0.79
Average annual profits (2004-2006), 000s ETB	22,083	3,998	4,345	0.52
Number of competitors within 15 minute walk	2.2	1	2	0.58
Number of persons employed	340	167	790	0.89
Number of persons employed in production	202	99	569	0.91
Average monthly starting salary of production workers (2010 ETB)	2,931	2,379	1,654	0.28
Average monthly starting salary of production workers, 12 months ago (2010 ETB)	2,649	1,983	1,426	0.25
Share of production workers who completed high school	0.62	0.69	0.44	0.30
Typical new production hire completed high school	0.64	-	0.40	-
Would participate in study using randomization	0.40	-	0.80	-
Observations	0.14		0.05	

Given that low wages are likely correlated with other worker characteristics or sector norms, we want to know if the study firms pay less, conditional on other firm and worker characteristics. In Table C.2, we report an OLS regression of each firm's reported production worker starting wages on an indicator for the five study firms, controlling for firm-reported characteristics. Columns (1) and (2) report this regression for all firms in the sample that report production salaries, including sectors such as finance, education, etc. Columns (3) and (4) report this regression for all manufacturing firms and commercial farms. Columns (5) and (6) report for all manufacturing firms and commercial farms with at least 50 employees. In each sample, the study firms pay production workers less than similar firms.

Table C.2: Conditional Wage Differences

	Full Sample		Manufacturing & Farms		Large Manufacturing & Farms	
	β	S.E.	β	S.E.	β	S.E.
	(1)	(2)	(3)	(4)	(5)	(6)
In Blattman-Dercon Sample	-585.5	[1177.786]	-658.4	[1200.902]	-885.9	[1044.924]
Number of years the firm has been in business (as of 2014)	6.8	[9.170]	10.0	[9.924]	16.0	[11.668]
In Addis Ababa	625.3	[1134.348]	460.7	[1154.741]	515.3	[997.437]
In Tigray region	1842.7	[1408.870]	1517.5	[1439.959]	1408.3	[1256.033]
Firm is in agricultural sector	-2044.1	[1366.665]	-1484.5	[1417.507]	-514.9	[930.847]
Produces garments, apparel, textiles, or shoes	-642.6	[562.204]	-526.1	[603.652]	-568.6	[535.276]
Produces food or beverages	-219.4	[543.227]	-31.8	[627.579]	-161.7	[544.277]
Majority share foreign owned	1119.0	[593.901]*	832.2	[635.163]	549.4	[598.531]
Certified international quality	-107.0	[380000]	-173.1	[404.597]	508.1	[442.250]
Log of average sales revenue	260.0	[99.116]***	140.6	[123.287]	200.6	[155.411]
Number of competitors within 15 minute walk	5.8	[15.410]	5.9	[18.513]	-36.9	[47.057]
Number of persons employed in production	-2.0	[.947]**	-1.5	[1.001]	-1.3	[.970]
Share of production workers who completed high school	380.1	[274.868]	1638.7	[604.445]***	806.6	[708.563]
Would participate in study using randomization	247.9	[293.750]	460.0	[337.477]	344.8	[417.753]
Constant	Sector Fixed Effects		875.4	[2087.804]	-521.7	[1912.004]
R-Squared	0.20		0.21		0.19	
Observations	310		246		122	

Note: Firms with <10 employees dropped. Firm age is imputed for 3 firms, revenue is imputed for 46 firms, number of competitors is imputed for 22 firms, share of workers who completed high school is imputed for 3 firms, and international certification is imputed for 1 firm.

D Supplementary analysis

D.1 Levels and correlates of attrition

Table D.2 reports response rates by round and study arm. Response rates were roughly 88% at the 11-month survey and 85% at the 13-month survey.[5] There is no association between the treatments and emigration. Household attrition is lower because individuals who were away temporarily or unavailable typically had household members available. Table D.1 reports correlates of attrition, via an OLS regression of attrition on select covariates (pooling the 11- and 13-month surveys).

D.2 Distribution of hours and earnings by treatment arm

Figure D.1 displays histograms of average weekly hours and estimated earnings in the past month for the job offer and control groups. They are broadly similar.

[5] Reasons for individual attrition include 32 who had moved and could not be found, 42 refusals, 54 people who moved abroad, typically to the Middle East for domestic or construction work, one who died and one who went to prison.

Table D.1: Correlates of attrition, selected covariates

	Unfound at endline	
Baseline covariate	Coeff.	Std. Err.
Assigned to job	-0.020	[.025]
Assigned to grant	-0.024	[.026]
Age	-0.001	[.003]
Female	0.271	[.087]***
Unmarried	-0.002	[.027]
Household head	0.003	[.026]
Disability indicator	0.052	[.095]
Total years of education and training	0.002	[.004]
Cognitive ability, z-score	0.011	[.010]
Mental health, z-score	0.014	[.011]
Conscientiousness index, z-score	-0.031	[.013]**
Risk aversion, z-score	0.008	[.011]
Future orientation, z-score	0.003	[.011]
Income and wealth, z-score	-0.024	[.010]**
Years experience in formal work	-0.026	[.006]***
Ever worked in industrial firm	0.045	[.029]
Prospects for employment in next month (0-1)	0.067	[.050]
Dependent variable mean	0.138	
p-value from F-test of joint significance	0.000	
Observations	1841	

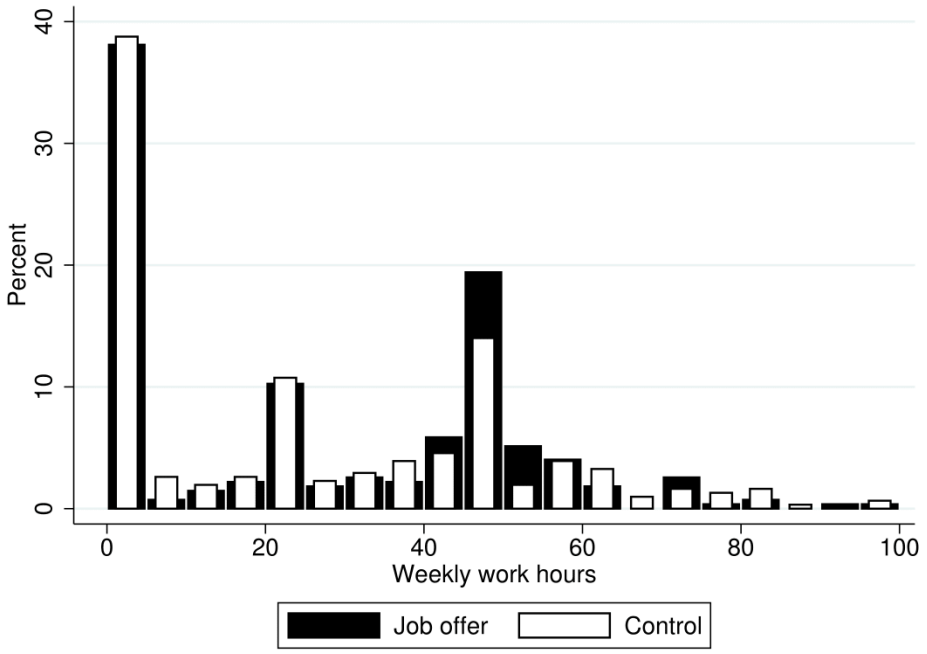
Notes: This table reports results of an OLS regression of an indicator for not being found at endline on baseline covariates as well as a dummy for the 13 month endline and gender-cohort fixed effects (not displayed). 11- and 13-month endline data are pooled in this regression with robust standard errors clustered by individual.

Table D.2: Survey response and attrition

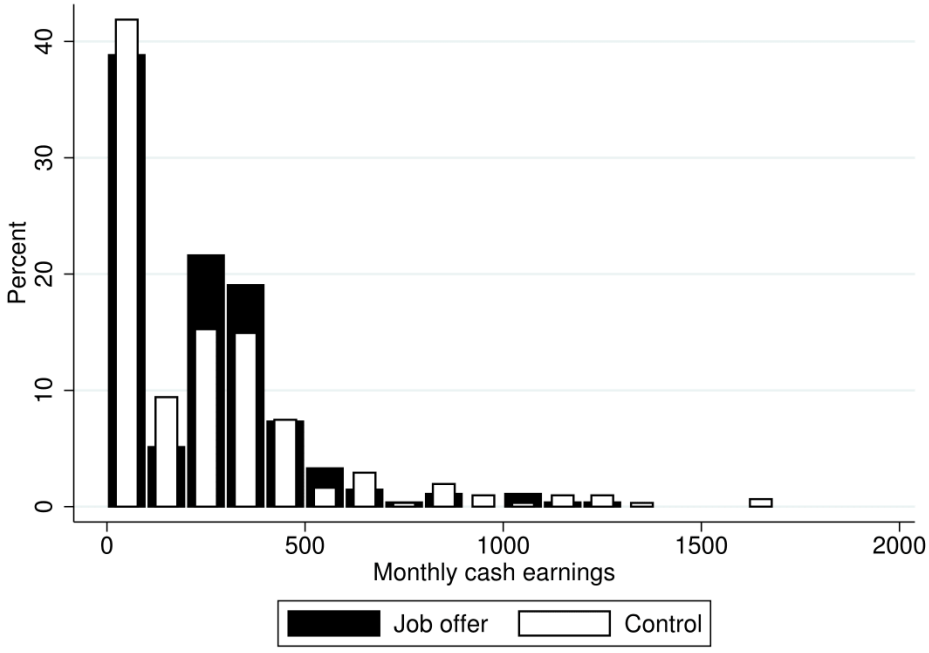
		Response rates			By treatment group			Job–Control		Grant–Control	
Respondent	Round	Sought	Respond	%	Job	Grant	Control	Diff.	p-value	Diff.	p-value
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Baseline	947									
Applicant	11 mo.	947	832	87.9%	90.1%	87.7%	86.0%	3.58%	0.157	0.39%	0.883
	13 mo.	894	755	84.5%	84.4%	87.0%	82.2%	1.80%	0.552	4.68%	0.106
Household head	11 mo.	947	856	90.4%	91.8%	90.9%	88.8%	2.66%	0.256	1.46%	0.551

Notes: This table pools people in all eight cohorts with available endline data. Columns (7) to (10) reports the coefficient on assignment to treatment from an OLS regression of the response rate on the treatment indicator and block fixed effects, with heteroskedastic-robust standard errors.

Figure D.1: Outcome distributions, by treatment assignment



(a) Weekly hours of work



(b) Monthly cash earnings

D.3 Adjusting across treatment arms

In table D.3 we adjust p-values of our family indices for 16 comparisons (eight outcomes and two treatments). The results are qualitatively similar to when we adjust for 8 comparisons (eight outcomes and one treatment). The effects of the job on physical health and of the entrepreneurship program on mental health are still significant after these adjustments., while the effect of the entrepreneurship program on income is now only marginally significant ($p = 0.095$).

D.4 Other outcomes

In Table D.4 we report other outcomes measured in the survey we did not include in the main family indexes.

D.5 Robustness of disability results

Our measure of disability is based on answers to 5 questions about difficulty in daily activities. For each activity (walking 2 kilometers, carrying 20 liters, performing daily activities, working outdoors for a full day, standing at a workbench for a full day) respondents are asked if they can complete the activity easily, with some difficulty, with great difficulty, or not at all. Based on this, we create an additive index of the 5 difficulty scores as well as a disability indicator. In our preferred disability indicator, we define disabled as having great difficulty performing more than one of the 5 tasks. However, in Table D.5, we report ITT effect estimates for alternative constructions of the indicator, as well as for responses to individual questions. Overall, the pattern of effects is consistent with the preferred disability indicator that we report in the main text, but the reader should beware that there are some alternate constructions that reduce the magnitude and statistical significance of our results, as well as alternative constructions that strengthen our results.

D.6 Two rounds of data collection versus one

Our estimation strategies pools together data from two rounds of collection. In table D.6, we display the results if we only collected one round of data. In general, the point estimates weakly decrease with two rounds of data, but the precision of the estimates increases pretty substantially, especially for our income family index. The one exception is that the point estimate of a job offer on physical health gets larger with both rounds of data collection.

D.7 Treatment effects on indirect outcomes

Here we elaborate on the secondary measures. Most were conceived as possible indirect effects of income gains or socialization as a result of becoming an industrial worker. Since we have a weak "first stage" relationship between the offer and factory employment, in general we see little ITT effect of the offer on these outcomes.

All endline survey questions are represented in these eight families, save for a set of exploratory political behavior questions not relevant for this study.

Table D.3: Program impacts on standardized family indexes, with p-values adjusted for 16 comparisons

		ITT estimate (N = 1587)					
	Control	Job offer			Entrepreneurship program		
Outcome (z-score)	mean	Coeff.	Std. Err.	Adj. p	Coeff.	Std. Err.	Adj. p
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Employment/occupational choice	-0.04	0.078	[.074]	0.985	0.040	[.076]	0.997
Income	-0.01	0.014	[.052]	0.997	0.150	[.058]**	0.095
Physical health	0.06	-0.193	[.066]***	0.037	-0.098	[.062]	0.820
Mental health/Subj. well being	-0.12	0.072	[.071]	0.987	0.228	[.065]***	0.003
Anti-firm/pro-union attitudes	0.00	0.05	[.085]	0.997	-0.09	[.089]	0.987
Social integration	0.10	0.01	[.018]	0.997	0.00	[.018]	0.997
Autonomy	0.04	-0.06	[.064]	0.990	-0.05	[.062]	0.997
Non-cognitive skills	0.10	-0.04	[.033]	0.952	0.03	[.035]	0.997

Notes: Columns (2) to (5) report the results of an OLS regression of each outcome on treatment indicators, baseline covariates, and cohort-gender fixed effects. 11- and 13-month survey responses are pooled. Standard errors are robust and clustered by respondent. P-values are adjusted for 16 comparisons (8 family outcomes and 2 treatment arms) using the Westfall-Young approach described in section 5.1. Some outcomes contain fewer observations than the listed number of observations because a very small number of respondents were not asked certain questions.

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

Table D.4: Economic impacts of the job offer and entrepreneurship program

		ITT estimate (N = 1587)			
	Control	Job offer		Entrepreneurship program	
Outcome	mean	Coeff	Std. Err.	Coeff.	Std. Err.
	(1)	(2)	(3)	(4)	(5)
Time use and enrollment					
Commuting, hours/week†	3.94	0.269	[.429]	0.015	[.432]
Leisure, hours/week†	21.15	-0.620	[1.550]	-3.580	[1.473]**
Chores, hours/week†	20.10	0.115	[1.197]	0.214	[1.224]
Returned to school, past year	0.16	-0.012	[.019]	0.024	[.020]
Currently enrolled†	0.07	-0.032	[.019]*	-0.015	[.021]
Annualized employment and earnings					
Estimated total annual hours worked	1,167.94	289.581	[71.197]***	91.052	[66.881]
Estimated total annual earnings, 2010 Birr	2,365.14	164.454	[162.789]	824.141	[192.509]***
Savings					
Savings in past month, 2010 Birr	45.68	-3.410	[6.212]	35.365	[8.505]***

Notes: Column (1) reports the control group mean. Columns (2) to (5) report the results of an OLS regression of each outcome on treatment indicators, baseline covariates, and cohort-gender fixed effects. 11- and 13-month survey responses are pooled. Standard errors are robust and clustered by participant. Observation counts displayed in the table report the maximum observation count for both types of outcome variables. Some outcomes contain fewer observations than these counts as a very small number of respondents were not asked certain questions.

†denotes outcome variables that were measured during only one of the endline surveys.

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

Table D.5: ITT estimates for different health outcomes

		ITT estimate (N=1587)			
	Control	Job offer		Grant	
Outcome	mean	β	S.E.	β	S.E.
	(1)	(2)	(3)	(4)	(5)
Ability to do activities of daily life (0-15)	14.07	-0.289	[.123]**	-0.228	[.127]*
Workplace has serious health risks (0-1)	0.14	0.084	[.032]***	0.004	[.032]
Disabled (great difficulty doing >=1 activity)	0.11	0.052	[.022]**	0.034	[.022]
Disabled (great difficulty doing >1 activity)	0.04	0.036	[.015]**	0.016	[.014]
Has great difficulty walking 2 kilometers	0.01	0.007	[.005]	0.018	[.007]***
Has great difficulty carrying 20 liters	0.04	0.023	[.015]	0.025	[.014]*
Has great difficulty performing daily activities	0.02	-0.006	[.009]	0.001	[.008]
Has great difficulty working outdoors for a full day	0.06	0.040	[.016]**	0.016	[.016]
Has great difficulty working on feet at bench for a full day	0.04	0.036	[.016]**	0.014	[.015]
Disabled (some difficulty doing >=1 activity)	0.04	0.023	[.015]	0.025	[.014]*
Disabled (some difficulty doing >1 activity)	0.02	-0.006	[.009]	0.001	[.008]
Has some difficulty walking 2 kilometers	0.01	0.007	[.005]	0.018	[.007]***
Has some difficulty carrying 20 liters	0.04	0.036	[.016]**	0.015	[.015]
Has some difficulty performing daily activities	0.06	0.041	[.016]**	0.018	[.016]
Has some difficulty working outdoors for a full day	0.32	0.046	[.028]	0.033	[.029]
Has some difficulty working on feet at bench for a full day	0.21	0.044	[.025]*	0.035	[.026]
Disabled (great difficulty doing >1 activity, excluding work bench)	0.04	0.020	[.014]	0.002	[.013]

Notes: Column (1) reports the control group mean. Columns (2) to (5) report the results of an OLS regression of each outcome on treatment indicators, baseline covariates, and cohort-gender fixed effects. 11- and 13-month survey responses are pooled. Standard errors are robust and clustered by individual. Some outcomes contain fewer observations than the reported sample size as a very small number of respondents were not asked certain questions. *** p<0.01, ** p<0.05, * p<0.1

Table D.6: Two rounds of data collection versus one

	Pooled results			
	Job offer		Grant	
Family index	11 & 13 month	11 month only	11 & 13 month	11 month only
	(1)	(2)	(3)	(4)
Employment	0.08	0.09	0.04	0.03
	[.074]	[.087]	[.076]	[.092]
Income	0.01	-0.06	0.15	0.17
	[.052]	[.085]	[.058]**	[.093]*
Physical health	-0.19	-0.16	-0.10	-0.13
	[.066]***	[.076]**	[.062]	[.075]*
Mental health	0.07	0.05	0.23	0.27
	[.071]	[.097]	[.065]***	[.087]***
Observations	1587	832	1587	832

Notes: Columns (1) and (3) pool the 11- and 13- month surveys so that there are two observations per person and clusters standard errors by individual. Columns (2) and (4) use only the 11-month survey and do not cluster standard errors.

Table D.7: Treatment effects on secondary (indirect) outcomes

Outcome	Control mean (1)	ITT estimate (N = 1587) Job offer Coeff. (2)	Job offer Std. Err. (3)	Entrepreneurship Coeff. (4)	Entrepreneurship Std. Err. (5)
Anti-firm/pro-union attitudes, z-score	0.00	0.05	[.085]	-0.09	[.089]
Firms good for workers & country (0-24)	10.81	0.10	[.248]	-0.41	[.257]
Large scale farms detrimental to land fertility (0–4) (-)	2.30	0.01	[.099]	0.09	[.104]
Factories take away land from farmers (0–4) (-)	1.64	-0.01	[.103]	0.02	[.102]
Large factories are harmful to environment (0–4) (-)	1.15	0.15	[.093]	0.00	[.093]
Large factories are good for overall economic growth (0–4)	3.25	-0.05	[.071]	0.04	[.076]
Factories pay decent salaries (0–4)	1.86	-0.08	[.106]	0.21	[.105]**
Factories provide job opportunities that can lift people out of poverty (0–4)	2.98	-0.13	[.092]	0.06	[.089]
Foreign firms good for country (0–24)	9.81	0.39	[.258]	-0.32	[.268]
Foreign businessmen send profits to other countries (0–4)	2.24	0.14	[.093]	-0.05	[.094]
Foreigners have too much influence over government (0–4)	1.69	0.06	[.093]	-0.11	[.095]
Foreign-owned firms take land that belongs to Ethiopians (0–4)	2.22	0.04	[.097]	0.01	[.102]
Foreign-owned firms develop good infrastructure (0–4)	1.43	0.05	[.092]	0.01	[.091]
Foreign firms procudes products that benefit Ethiopia (0–4)	1.28	0.10	[.085]	-0.10	[.088]
Foreign-owned firms have a better work culture (0–4)	0.96	0.02	[.075]	-0.08	[.075]
Pro-unions (0–24)	14.13	-0.17	[.252]	-0.04	[.269]
Labor unions damage the employer-employee relationships (0–4)	2.43	-0.01	[.074]	0.01	[.076]
Labor unions are unable to bargain for safe workplace (0–4)	2.30	-0.12	[.073]	0.04	[.077]
Labor unions are effective in bargaining for better pay and benefits (0–4)	2.37	-0.16	[.076]**	-0.05	[.078]
Without labor unions, employers would take advantage of employees (0–4)	2.27	0.09	[.080]	0.00	[.081]
I would prefer to work in a place that has a labor union (0–4)	2.75	0.07	[.074]	-0.03	[.077]
Labor unions are independent and uninfluenced by government (0–4)	2.01	-0.02	[.077]	0.00	[.082]

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Table D.7 (continued)

Outcome	Control mean (1)	ITT estimate (N = 1587) Job offer Coeff. (2)	Job offer Std. Err. (3)	Entrepreneurship Coeff. (4)	Entrepreneurship Std. Err. (5)
Workers' rights protected (0–24)	12.49	-0.07	[.294]	0.35	[.320]
Factories provide workers with an environment that will not cause longer term health problems (0–4)	1.99	0.16	[.100]	0.17	[.103]*
Factories allow workers to take leaves of absence (0–4)	2.52	-0.04	[.094]	-0.06	[.092]
Factories allow their workers to join or form labor unions (0–4)	2.20	0.04	[.079]	0.02	[.083]
Factories pay workers the same if they do the same work (0–4)	2.06	-0.09	[.108]	-0.04	[.110]
Factories do not terminate employment unless there is a valid reason (0–4)	2.26	-0.06	[.096]	0.10	[.103]
Factories pay workers enough to provide for family's basic needs (0–4)	1.46	-0.05	[.103]	0.16	[.105]
Social integration, z-score	0.04	0.01	[.018]	0.00	[.018]
Family support (0–15)	12.31	-0.01	[.210]	-0.02	[.223]
How often family is caring and supporting (0–3)	2.65	0.00	[.060]	0.02	[.062]
How often can turn to family for advice (0–3)	2.42	-0.05	[.067]	-0.05	[.069]
How often can rely on family for help (0–3)	2.31	0.00	[.078]	-0.07	[.083]
How often feels like valued family member (0–3)	2.54	0.11	[.060]*	0.06	[.063]
How often has angry quarrels with family (0–3)	2.41	-0.08	[.072]	0.00	[.069]
Community support (0–15)	9.55	0.17	[.233]	-0.13	[.229]
How often friends and neighbors are caring and supporting (0–3)	1.89	0.06	[.076]	0.00	[.078]
How often can turn to friends and neighbors for advice (0–3)	1.78	-0.06	[.074]	-0.10	[.074]
How often can rely on friends and neighbors for help (0–3)	1.28	0.09	[.080]	0.05	[.082]
How often has angry quarrels with friends and neighbors (0–3)	2.64	-0.01	[.057]	-0.11	[.063]*
How often feels like valued community member (0–3)	1.97	0.09	[.078]	0.04	[.079]
Degree of community participation (0–8)	3.40	-0.15	[.151]	0.07	[.149]
Has official position in the Kebele (0–1)	0.01	0.01	[.010]	-0.01	[.009]
Is leader in other groups, community, or church (0–1)	0.05	0.01	[.018]	0.00	[.018]

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Table D.7 (continued)

Outcome	Control mean (1)	ITT estimate (N = 1587) Job offer Coeff. (2)	Job offer Std. Err. (3)	Entrepreneurship program Coeff. (4)	Entrepreneurship program Std. Err. (5)
People in community come to you for advice (0–1)	0.42	-0.01	[.043]	0.05	[.044]
Usually speaks up at meetings (0–1)	0.40	-0.04	[.040]	0.00	[.040]
Usually participating in community meetings (0–1)	0.45	-0.07	[.040]*	0.04	[.043]
Usually votes in elections (0–1)	0.69	-0.01	[.038]	0.01	[.038]
Voted in the 2002 national elections (0–1)	0.63	-0.02	[.038]	-0.02	[.038]
Attends religious services regularly (0–1)	0.74	-0.01	[.038]	-0.01	[.039]
Trust (0–12)	5.72	0.25	[.195]	0.08	[.204]
Most people are basically honest (0–3)	1.13	0.06	[.072]	-0.02	[.078]
Most people can be trusted (0–3)	1.14	0.05	[.068]	-0.02	[.074]
Government officials can be trusted to do the job (0–3)	1.37	0.11	[.070]	0.08	[.073]
Neighbors can be trusted to watch the house (0–3)	2.07	0.04	[.073]	0.04	[.075]
Autonomy	0.05	-0.06	[.064]	-0.05	[.062]
Independent decision-making (0–36)	27.32	-0.53	[.368]	-0.14	[.415]
Wife has right to express her opinion when she disagrees with husband (0–3)	2.59	-0.01	[.054]	0.00	[.058]
Wife has right to buy clothes for herself (0–3)	1.59	0.01	[.083]	0.13	[.087]
Wife is justified in refusing sex with husband if he sleeps with other women (0–3)	2.13	0.02	[.090]	-0.03	[.095]
Wife can refuse to have more kids even if husband wants more (0–3)	1.96	-0.15	[.082]*	-0.06	[.086]
Woman equally capable of taking managerial positions (0–3)	2.62	-0.05	[.053]	-0.06	[.056]
Women with same qualifications should have equal pau (0–3)	2.63	0.06	[.055]	-0.01	[.061]
Wife doesn't have right to buy/sell things (0–3)	1.79	-0.09	[.085]	-0.10	[.088]
Wife not justified to request condom even if she knows husband has STD (0–3)	2.58	-0.10	[.061]*	0.01	[.061]
Appropriate for husband to beat wife if she refuses to have sex with him (0–3)	2.65	-0.03	[.054]	0.03	[.058]
Wife shouldn't divorce husband even if he has a mistress (0–3)	2.36	0.04	[.069]	0.09	[.070]
Men have more right to jobs than women when they are scarce (0–3)	2.29	-0.15	[.077]**	-0.13	[.080]
Wife should not be encouraged to work if she has young kids (0–3)	2.13	-0.08	[.077]	-0.01	[.079]

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Table D.7 (continued)

Outcome	Control mean	ITT estimate (N = 1587)			
		Job offer		Entrepreneurship program	
		Coeff.	Std. Err.	Coeff.	Std. Err.
	(1)	(2)	(3)	(4)	(5)
Involvement in spending decisions (0–10)	5.43	0.16	[.265]	0.29	[.262]
Who decides to pay school fees for relatives? (0–2)	1.15	-0.02	[.057]	0.08	[.058]
Who decides to purchase small household items? (0–2)	1.10	0.04	[.057]	0.10	[.058]*
Who decides to purchase small household assets? (0–2)	1.07	0.05	[.057]	0.04	[.057]
Who decides to purchase expensive household items? (0–2)	1.07	0.05	[.056]	0.02	[.056]
Who decides to purchase expensive household assets? (0–2)	1.04	0.05	[.056]	0.04	[.056]
Money freedom (0–12)	4.25	-0.02	[.209]	-0.07	[.217]
Expresses opinion when disagrees with partner (0–3)	2.32	0.01	[.067]	0.00	[.074]
Refuses to give money to partner when earns it (0–3)	1.68	0.13	[.096]	0.20	[.096]**
Allowed to buy/sell in market without partner permission (0–3)	1.32	0.10	[.089]	-0.02	[.088]
Abuse received (0–21)	1.00	-0.12	[.150]	-0.17	[.136]
Partner threatens to hurt you (0–3)	0.09	-0.03	[.028]	-0.02	[.027]
Partner humiliates you in front of others (0–3)	0.06	-0.01	[.024]	-0.01	[.023]
Partner beats you badly (0–3)	0.03	-0.01	[.017]	-0.01	[.016]
Partner kicks or hits you (0–3)	0.07	0.02	[.029]	-0.01	[.024]
Partner has accused you of unfaithfulness (0–3)	0.03	0.00	[.015]	0.01	[.016]
Partner limits contact with family (0–3)	0.08	-0.01	[.030]	-0.03	[.024]
Partner insists on knowing where you are at all times (0–3)	0.64	-0.08	[.084]	-0.10	[.088]
Noncognitive skills	0.08	-0.04	[.033]	0.03	[.035]
Locus of control (0–24)	17.09	-0.05	[.238]	0.22	[.254]
You can determine your future through your own actions (0–3)	2.45	-0.02	[.055]	-0.03	[.055]
When you make plans, you are confident you can make them work (0–3)	2.24	0.03	[.061]	0.04	[.062]
You have little influence over the things that happen to you (0–3)	1.46	-0.03	[.072]	-0.06	[.075]
In the long run people get the respect they deserve (0–3)	2.10	-0.08	[.076]	0.02	[.075]
If you try hard, you can improve your situation (0–3)	2.73	-0.02	[.042]	0.02	[.042]
It is not always wise to plan too far ahead (0–3)	1.15	-0.05	[.078]	-0.07	[.079]

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Table D.7 (continued)

Outcome	Control mean	ITT estimate (N = 1587) Job offer Coeff.	Job offer Std. Err.	Entrepreneurship program Coeff.	Entrepreneurship program Std. Err.
	(1)	(2)	(3)	(4)	(5)
Many of the bad things in people's lives are partly due to bad luck (0–3)	1.11	0.00	[.075]	-0.01	[.076]
Becoming successful is a matter of hard work (0–3)	2.28	-0.02	[.068]	0.03	[.068]
Self esteem (0–24)	16.47	-0.39	[.258]	-0.11	[.276]
On the whole, you are satisfied with yourself (0–3)	2.29	-0.02	[.064]	0.04	[.066]
You feel useless at times (0–3)	0.61	0.06	[.072]	0.06	[.072]
You are inclined to feel that you are a failure (0–3)	0.55	0.02	[.065]	0.05	[.068]
You wish you could have more respect for yourself (0–3)	2.43	-0.03	[.064]	0.08	[.066]
You feel you are a person of worth (0–3)	2.42	-0.09	[.057]	0.00	[.057]
At times you think you are no good at all (0–3)	0.60	0.13	[.065]**	0.09	[.067]
You are able to do things well as most other people (0–3)	2.50	-0.11	[.055]*	0.02	[.053]
You feel you do not have much to be proud of (0–3)	1.54	0.02	[.082]	-0.09	[.082]
Patience (0–8)	2.91	-0.06	[.073]	0.08	[.078]
When you earn money, you save some for the future (0–1)	0.88	0.01	[.028]	-0.02	[.029]
Prefers 100 birr each week for 10 weeks over 1000 birr now (0-1)	0.27	0.03	[.038]	0.03	[.039]
Prefers 100 birr each week for 15 weeks over 1,000 birr next week (0–1)	0.48	-0.01	[.044]	0.01	[.044]
Prefers 100 birr each week for 12 weeks over 1000 birr now (0-1)	0.41	-0.02	[.043]	0.04	[.044]
Prefers doing hard task over easy task first (0–1)	0.62	-0.05	[.044]	-0.14	[.046]***
Spends money quickly (0–1)	0.00	0.00	[0000]	0.00	[0000]
Postpones most planned activities until later (0–1)	0.00	0.00	[0000]	0.00	[0000]
Prefers 700 birr per month indefinitely over 1000 birr per month that can end anytime (0–1)	0.93	-0.01	[.024]	-0.01	[.024]
Risk aversion (0–6)	1.79	0.03	[.055]	-0.02	[.058]
Likes to find out risks before doing an activity (0–1)	0.79	0.00	[.035]	-0.01	[.035]
Would rather have 400 birr for sure over flipping a coin for 1000 birr (0–1)	0.70	0.06	[.040]	0.03	[.041]

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Table D.7 (continued)

Outcome	Control mean	ITT estimate (N = 1587) Job offer Coeff.	Job offer Std. Err.	Entrepreneurship program Coeff.	Entrepreneurship program Std. Err.
	(1)	(2)	(3)	(4)	(5)
Gets anxious if uncertain about risks of job (0–1)	0.42	-0.05	[.044]	-0.03	[.044]
Gets disturbed when uncertain of effects of a decision (0–1)	0.41	0.01	[.044]	-0.07	[.044]
Prefers to reduce pain but not cure it over surgery to cure with death risk (0–1)	0.34	0.07	[.042]*	0.09	[.044]**
Prefers low, safe profits over high, risky profits (0–1)	0.83	-0.01	[.035]	-0.06	[.038]
More willing to take risks relative to friends (0–1)	0.74	-0.06	[.040]	0.04	[.039]
Does dangerous things for fun (0–1)	0.01	0.00	[.009]	0.01	[.010]
Self control index (0–6)	2.35	-0.02	[.054]	0.02	[.056]
Continues doing something after starting it and it becomes difficult (0–1)	0.75	-0.02	[.040]	-0.04	[.042]
Good at resisting temptation (0–1)	0.93	-0.01	[.022]	-0.01	[.025]
Often spends money on things and regrets it later (0–1)	0.08	0.05	[.028]	0.08	[.030]***
Wish you had more self-discipline (0–1)	0.92	-0.01	[.024]	-0.01	[.025]
Puts money into safe place to avoid spending it (0–1)	0.83	-0.01	[.034]	-0.01	[.035]
Prefers to wait and see instead of buying something you like right away (0–1)	0.88	-0.05	[.031]	-0.05	[.030]*
Has trouble getting started when having a job to do (0–1)	0.09	-0.02	[.026]	0.00	[.029]
Speaks often before thinking (0–1)	0.08	0.02	[.024]	-0.02	[.023]
Punctuality and time consciousness (0–12)	11.04	-0.24	[.168]	-0.08	[.171]
It is not okay to be late to appointments with friends (0–4)	2.10	0.00	[.129]	-0.01	[.131]
It is okay to leave work and go home even if not finished all tasks (0–4)	4.51	-0.18	[.075]**	-0.08	[.066]
It is okay to be late to work in the morning (0–4)	4.47	-0.05	[.075]	-0.05	[.074]

Notes: Columns (2) to (5) report the results of an OLS regression of each outcome on treatment indicators, baseline covariates, and cohort-gender fixed effects. 11- and 13-month survey responses are pooled with robust standard errors clustered by individual.

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

Labor and firm attitudes We hypothesized that long term participation in industrial work would change attitudes to firms, self-identification as a worker, and possibly tensions between the two. Naturally short term exposure could have some effect as well. Perhaps because of the weak first stage, however, we see little effect of either treatment on attitudes to labor rights, unions, or firms. Table D.7 reports treatment effects. We asked six questions per index, on a 0–4 Likert scale, for a 0–24 scale per index. We see little change in an index of pro-union attitudes (e.g. prefer to work in a place with labor unions, or unions protect workers from firms); of attitudes supportive

of large firms (e.g. they are good for growth, they pair fair salaries); are that workers rights are protected and respected (e.g. have safe environments, are free to quit jobs or join labor unions). We do see a weak decrease in whether large foreign firms are good for the country (e.g. they benefit Ethiopia, they pay taxes and invest in Ethiopia).

Social integration In Ethiopia, as in much of Africa, social standing is tied in part to being a net contributor to the household rather than a dependent. We hypothesized that large income increases would increase standing in the household and community. In general we see no effect on five measures of family connectedness and support (e.g. how often can the respondent turn to his/her family for help), five measures of concrete social support received from family and friends (e.g. how often can the respondent rely on neighbors for help), eight forms of community participation (e.g. attending community meetings or holding official an position in the Kebele), or 4 measures of in-group and out-group trust levels (e.g. believing most officials could be trusted).

Autonomy We hypothesized that large income increases and long term participation in industrial work could increase workers' autonomy. However, we observe no changes in eight measures of independent decision-making (e.g. whether someone has the right to buy or sell things in the market without asking for the permission of a spouse or family member); five measures of involvement in spending decisions (e.g. who decides to purchase expensive household items); three measures of freedom in money and market access (e.g. refusing to give earned money to partner to purchase alcohol); and seven measures of domestic abuse received (e.g. how often the respondent was kicked or punched by partner).

Non-cognitive skills We hypothesized that long term participation in industrial work could socialize workers to be more docile or orderly. We see little effect on self-reported psychometric scales that measure personality and character traits such as locus of control (e.g. believing that you can determine your own future through your own actions and choices), self-control (also known as conscientiousness, e.g. spending money on things now and regretting it later), or self-esteem (e.g. being satisfied with yourself). Each is measured using eight standard questions from personality scales, locally adapted to Ethiopia, using a Likert scale for a 0–24 index. We also measured economic preferences through self-reported patience (e.g. preferring 1000 birr now or 100 birr each week for the next 12 weeks) and risk aversion (e.g. having 400 birr for sure or flipping a coin for 1000 birr). We also assess self-reported punctuality (e.g. whether it is okay to be late to work in the morning).

D.8 Treatment effects by gender and firm

Impacts by gender Table D.8 reports ITT estimates by gender. The effects are broadly consistent across genders, with a few exceptions. Women are much more likely to stay employed in industrial work as a result of the job offer than their male counterparts. The ITT effects on disability are also concentrated in the female subsample. On the other hand, the point estimates for the effect of the grant on earnings is nearly 3 times as high for men as it is for women.

Impacts by firm Tables D.9 and D.10 investigate heterogeneity of job offer effects by firm in two ways. First, Table D.9 reports ITT estimates when we restrict the sample to individual firms. Second, Table D.10 reports the impact when individual firms are excluded from the sample. It is

Table D.8: Impacts of job offer and grant by gender, all firms

	Men (N=174)			Women (N=773)		
		ITT			ITT	
	Mean	Job	Grant	Mean	Job	Grant
	(1)	(2)	(3)	(4)	(5)	(6)
Worked at least a month in study firm	0.089	0.522	-0.016	0.155	0.549	-0.059
		[.074]***	[.057]		[.038]***	[.034]*
Worked at least a month in any industrial firm	0.348	0.389	-0.168	0.377	0.350	-0.157
		[.084]***	[.082]**		[.037]***	[.037]***
Was working in any industrial firm at endline	0.179	-0.014	-0.203	0.203	0.113	-0.121
		[.074]	[.068]***		[.038]***	[.033]***
Emigrated to Middle East	0.000	0.003	0.041	0.053	0.031	0.029
		[.011]	[.022]*		[.020]	[.021]
Weekly earnings, 2010 Birr	50.270	7.893	30.937	30.313	7.990	12.986
		[12.881]	[13.120]**		[4.694]*	[5.457]**
Ability to do activities of daily life (0–15)	14.491	0.170	0.285	13.969	-0.283	-0.232
		[.164]	[.141]**		[.142]**	[.139]*
Disabled	0.009	0.001	-0.015	0.048	0.029	0.013
		[.017]	[.015]		[.017]*	[.016]

Notes: Columns (2) to (5) report the results of an OLS regression of each outcome on treatment indicators, baseline covariates, and cohort fixed effects. 11- and 13-month survey responses are pooled with robust standard errors clustered by individual.

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

important to note that this study is underpowered for this firm level analysis. Consequently, the absence of statistical significance at the firm level should not be taken as fatal to the overall finding. Instead, we focus on patterns of point estimates across firms. Effects on earnings are mixed across firms, while positive effects on rates of disability are consistent among firms. The magnitudes of the health effects vary considerably, and in Table D.10 we can see that the negative health are concentrated in the industrial farms.

D.9 Correlates of earnings and industrial employment

Table D.11 regresses endline earnings and industrial employment on a Mincerian-style set of baseline covariates, including of age, gender, health, education, and work experience. The table examines three subgroups: those who (endogenously) are in an industrial job at endline, and those randomly assigned to the job or control group.

In no instance is prior industrial experience associated with higher earnings, nor is tenure with the firm. Moreover, if we take everyone in the sample working in a factory or commercial farm at endline and add to the regression months of tenure in that industrial job (Columns 1 and 2), the coefficients on baseline experience remain negative, and the coefficient on months in the firm is positive but is small and not statistically significant—equal to about a 3.5% increase in real wages per month worked (or about 12% in total given that the average person spent 3–4 months in the job). If we extrapolate to a worker who remained for a year, this amounts to a one third increase in the wage. But we need to remember that this estimate is endogenous, in that it reflects workers who choose (or were encouraged) to stay because they were a better fit.

D.10 Other heterogeneity analysis

Another interpretation of the high quit rates that we observe is that job applicants are viewing industrial jobs as temporary responses to negative income shocks. Indeed, incomes in both the control and treatment groups rise throughout the study period, so the Ashenfelter dip is a factor here. But if these job applications are a response to negative income shocks, then the better-off applicants should be in a better position to quit the job if the job is undesirable. In Table D.12 we report heterogeneity in treatment effects by baseline wealth (index of consumer durables). We do not see wealthier applicants responding differently to the job offer treatment.

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Table D.9: Impacts of job offer, by firm

Outcome	ITT, all firms	ITT for each firm alone: Beverage Producer	Horticulture Farm	Flower Farm	Garment & Textile Factory	Shoe Factory
	(1)	(2)	(3)	(4)	(5)	(6)
Worked at least a month in study firm	0.541	0.677	0.472	0.478	0.501	0.650
	[.034]***	[.124]***	[.143]***	[.101]***	[.054]***	[.076]***
Worked at least a month in any industrial firm	0.357	0.444	-0.106	0.192	0.443	0.458
	[.034]***	[.146]***	[.239]	[.102]*	[.046]***	[.096]***
Was working in any industrial firm at endline	0.107	-0.004	-0.073	0.185	0.074	-0.017
	[.034]***	[.096]	[.153]	[.103]*	[.050]	[.088]
Emigrated to Middle East	0.016	0.048		0.064	-0.015	0.101
	[.017]	[.088]		[.077]	[.018]	[.059]*
Weekly earnings, 2010 Birr	3.614	12.078	-15.503	13.030	4.847	-12.805
	[4.442]	[9.637]	[9.488]	[16.240]	[6.621]	[16.514]
Ability to do activities of daily life (0–15)	-0.289	-0.208	-1.939	-0.079	-0.234	0.191
	[.123]**	[.510]	[.603]***	[.400]	[.167]	[.413]
Disabled	0.036	0.000	0.056	0.008	0.014	0.045
	[.015]**	[.030]	[.071]	[.040]	[.016]	[.050]
Observations	1587	153	176	246	740	272

Notes: Columns (2) to (5) report the results of an OLS regression of each outcome on treatment indicators, baseline covariates, and cohort-gender fixed effects for each firm. 11- and 13-month survey responses are pooled with robust standard errors clustered by individual.

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

Table D.10: Impacts of job offer, excluding one firm at a time

Outcome	ITT, all firms	ITT estimate of job offer excluding: Beverage Producer	Horticulture Farm	Flower Farm	Garment & Textile Factory	Shoe Factory
	(1)	(2)	(3)	(4)	(5)	(6)
Worked $\geq$ 30d in a study firm	0.541	0.525	0.538	0.542	0.568	0.535
	[.034]***	[.037]***	[.037]***	[.037]***	[.046]***	[.038]***
Worked $\geq$ 30d in any factory or farm with $>$ 10 emp.	0.357	0.379	0.382	0.387	0.252	0.335
	[.034]***	[.036]***	[.036]***	[.037]***	[.051]***	[.038]***
Working in any factory or farm with $>$ 10 emp. at endline	0.107	0.111	0.104	0.095	0.119	0.101
	[.034]***	[.036]***	[.036]***	[.037]**	[.051]**	[.038]***
Emigrated (typically to Middle East)	0.016	0.019	0.018	0.008	0.040	0.007
	[.017]	[.017]	[.019]	[.017]	[.029]	[.018]
Mean weekly cash profits, 3/2010 Birr	3.614	2.493	4.205	1.216	5.070	4.388
	[4.442]	[4.810]	[4.991]	[4.557]	[6.203]	[4.770]
Ability to do activities of daily life, 0-15 scale	-0.289	-0.308	-0.121	-0.243	-0.382	-0.421
	[.123]**	[.134]**	[.128]	[.133]*	[.183]**	[.124]***
Disability	0.036	0.039	0.025	0.033	0.043	0.036
	[.015]**	[.016]**	[.015]*	[.016]**	[.024]*	[.014]***
	1587	1434	1411	1341	847	1315

Notes: Columns (2) to (5) report the results of an OLS regression of each outcome on treatment indicators, baseline covariates, and cohort-gender fixed effects where each firm is removed from the sample. 11- and 13-month survey responses are pooled with robust standard errors clustered by individual.

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

Table D.11: Correlates of endline earnings and industrial employment

	Dependent variable (and sample)									
	Weekly earnings, 2010 Birr						Employed in an industrial job at endline			
	In industrial job		Assigned to job		Assigned to control		Assigned to job		Assigned to control	
Baseline covariate	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Age	-3.067	[1.336]**	0.963	[1.152]	0.066	[0.790]	0.014	[0.008]*	-0.004	[0.005]
Female	-18.110	[13.302]	-17.843	[11.248]	-23.608	[11.165]**	0.120	[0.088]	0.020	[0.064]
Disabled	-31.482	[8.920]***	-3.010	[27.217]	11.993	[16.063]	-0.250	[0.107]**	-0.101	[0.101]
Education	0.554	[1.567]	1.091	[1.274]	1.276	[1.275]	-0.005	[0.010]	0.003	[0.008]
Formal, non-industrial work experience, years	-1.995	[6.752]	-5.278	[3.934]	8.273	[4.084]**	-0.075	[0.022]***	-0.028	[0.021]
Industrial work experience, years	-2.757	[3.596]	-3.634	[2.782]	-2.633	[2.665]	-0.051	[0.021]**	-0.009	[0.016]
Months of tenure in industrial job (endline)	1.313	[1.093]								
Mean of dependent variable	54.6		35.6		34.2		0.320		0.198	
Observations	321		517		571		518		571	

Notes: The entrepreneurship arm is omitted from this analysis. 11- and 13-month survey responses are pooled. Standard errors are robust and clustered by individual. Columns 1 and 2 include only people who are employed by a factory or commercial farm at endline.

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

Table D.12: Heterogeneity of treatment by initial wealth

	Dependent variable (N=1587)				
	Working in any factory or farm with > 10 emp. at endline	Mean weekly cash profits, 3/2010 Birr	Has major disability	ADL Index (0-15)	One year change in subjective well-being
	(1)	(2)	(3)	(4)	(5)
Assigned to job offer	0.107	3.446	0.037	-0.291	-0.057
	[.035]***	[4.370]	[.015]**	[.122]**	[.074]
Consumption assets ×	-0.015	-4.820	0.001	0.057	0.024
Assigned to job offer	[.035]	[4.613]	[.016]	[.132]	[.072]
Assigned to cash grant	-0.120	12.511	0.015	-0.231	0.239
	[.029]***	[5.414]**	[.013]	[.128]*	[.071]***
Consumption assets ×	0.000	5.877	-0.005	0.006	-0.030
Assigned to cash grant	[.028]	[6.859]	[.016]	[.136]	[.073]
Consumption assets	0.010	4.455	0.016	-0.106	-0.014
	[.023]	[3.831]	[.010]	[.092]	[.052]

Notes: Columns (1) to (5) report the results of an OLS regression of each outcome on treatment indicators, interactions of treatment and consumption assets, baseline covariates, and cohort-gender fixed effects. 11- and 13-month survey responses are pooled with robust standard errors clustered by individual.

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

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