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OCCUPATIONAL CHOICE IN EARLY INDUSTRIALIZING SOCIETIES: EXPERIMENTAL EVIDENCE ON THE INCOME AND HEALTH EFFECTS OF INDUSTRIAL AND ENTREPRENEURIAL WORK

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Working Paper 22683
http://www.nber.org/papers/w22683

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
September 2016

We thank the participating firms plus the Ethiopian Development Research Institute and Innovations for Poverty Action for field research. For comments we thank David Atkin, Abhijit Banerjee, Konrad Burchardi, Stefano Caria, Brandon de la Cuesta, Simon Franklin, Tim Harford, Macartan Humphreys, Supreet Kaur, Thomas Leavitt, Jeremy Magruder, Suresh Naidu, Paul Niehaus, Cristian Pop-Eleches, Dani Rodrik, Antoinette Schoar, Georgiy Syunyaev, Eric Verhoogen, Chris Woodruff, and seminar participants at Princeton, Harvard Business School, Stanford, Georgetown, Johns Hopkins SAIS, Oxford, CU Denver, CGD, DFID, and the 2015 NBER Summer Institute, 29th BREAD, 2015 CAPERS, and 2012 IGC Growth Week conferences. For funding we thank the Aspen ANDE group, the International Growth Center, the U.K. Department for International Development, the Templeton Foundation, and a Vanguard Charitable Trust. Natalie Carlson, Felipe Dizon, Courtney Han, Dawit Kebede, Sana Khan, Benjamin Morse, Richard Peck, Patryk Perkowski, Katherine Rodrigues, Joe St. Clair, and Nynne Warring provided research assistance. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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NBER Working Paper No. 22683
September 2016
JEL No. F16,J24,J81,O14,O17

ABSTRACT

As low-income countries industrialize, workers choose between informal self-employment and low-skill manufacturing. What do workers trade off, and what are the long run impacts of this occupational choice? Self-employment is thought to be volatile and risky, but to provide autonomy and flexibility. Industrial firms are criticized for poor wages and working conditions, but they could offer steady hours among other advantages. We worked with five Ethiopian industrial firms to randomize entry-level applicants to one of three treatment arms: an industrial job offer; a control group; or an "entrepreneurship" program of $300 plus business training. We followed the sample over a year. Industrial jobs offered more hours than the control group's informal opportunities, but had little impact on incomes due to lower wages. Most applicants quit the sector quickly, finding industrial jobs unpleasant and risky. Indeed, serious health problems rose one percentage point for every month of industrial work. Applicants seem to understand the risks, but took the industrial work temporarily while searching for better work. Meanwhile, the entrepreneurship program stimulated self-employment, raised earnings by 33%, provided steady work hours, and halved the likelihood of taking an industrial job in future. Overall, when the barriers to self-employment were relieved, applicants appear to have preferred entrepreneurial to industrial labor.

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A online appendix is available at http://www.nber.org/data-appendix/w22683

1 Introduction

Most of the world's poor work informally for themselves or others. A wave of "entrepreneurship", "social protection", and other anti-poverty programs have tried to increase poor people's incomes by giving them cash, livestock, skills, and other inputs into self employment. Broadly speaking, evaluations of these programs suggest that these inputs relieve binding constraints and help a wide array of people, from the very poorest to small business owners, expand informal self-employment and raise their earnings.[1]

The larger process of development and structural change happening in many countries, however, means that a great deal of future employment growth will be in the formal sector with large firms. In recent years, for example, the world's least developed countries have experienced high growth in low-skill, labor-intensive industries such as agroindustry, textiles and apparel. Governments and development organizations generally see large-scale job creation as one of the chief advantages of such low-skill industrialization (UNIDO, 2013).

In the long run, rising industrial demand for labor should increase wages and improve 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 raises the broad question: How pro-poor are industrial jobs, and how do they compare to so-called "entrepreneurial" self-employment?

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? Workers may not understand many of these occupational tradeoffs, especially in a relatively new sector. The limited literature offers ambiguous predictions.

On the one hand, industrial work could offer steady employment, valuable training and experience, and in some cases wage premiums.[2] Even some opponents concede the point: a Scholars Against Sweatshop Labor statement (2001) admits that "after allowing for the

[1] e.g. Banerjee and Duflo, 2011; McKenzie, 2012; Bianchi and Bobba, 2013; Grimm and Paffhausen, 2015; Blattman and Ralston, 2015; Banerjee et al., 2015.

[2] There are several reasons why industrial work would pay a wage premium compared to informal work. Firms may pay efficiency wages (Katz et al., 1989; Akerlof and Yellen, 1986) or there may also be institutional and legislative sources, such minimum wages, labor codes or union bargaining (Card, 1996). If so, the result is a dual or segmented labor market, in which those gaining industrial jobs earn rents while informal workers queue for those jobs (e.g. Lewis, 1954; Harris and Todaro, 1970; Fields, 1975). Empirically, a large body of observational evidence suggests that formal firms pay premium wages, especially large, foreign-owned, or exporting firms. For Mexico see Bernard et al. (2010); Verhoogen (2008) and for Africa see Söderbom and Teal (2004); El Badaoui et al. (2008).

frequent low wages and poor working conditions in these jobs, they are still generally superior to 'informal' employment in, for example, much of agriculture or urban street vending." This reflects a common view that informal entrepreneurship is inefficient, risky, and relatively unprofitable, especially the petty trading that is so commonplace. Some evidence also suggests that industrial jobs could have important social impacts, such as empowering young women or enhancing the health of their children.[3] As the economist Joan Robinson remarked, "The misery of being exploited by capitalists is nothing compared to the misery of not being exploited at all" (1962, p.45).

On the other hand, industrial jobs could entail difficult-to-foresee costs and risks. One concern is that workers could 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. Industrial firms may also pose health risks that neither the firm nor worker fully understands, especially when the sector or technology is relatively new.

To investigate the relative risks and benefits of industrial and informal work, we follow a panel of nearly 1000 applicants to entry-level jobs in five industrial firms in different parts of Ethiopia. They were self-selected to have at least some interest in an industrial job, and screened by firms to have some secondary schooling. We randomly assigned roughly a third of the applicants to a job offer, a third to a control group, and a third to an entrepreneurship program intended to relieve key constraints on their informal employment opportunities: five days of business training and planning followed by $300 grant (about $1030 in purchasing power parity, or PPP).[4] This paper reports results over the first year of the study.

Ethiopia is a growing export hub in horticulture, textiles, and leather products, 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 garment 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, due to, for example,the opening of a new manufacturing line. In the end we worked with cohorts of applicants in five firms: a water bottling plant, a vegetable farm, a flower farm, a shoe manufacturer, and a textile and garment factory.

[3]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.

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

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: casual labor, home enterprises, or agricultural work.

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 greater informal employment rather than unemployment.[5] Our reasoning was based on growing evidence that poor, young, credit-constrained adults may be able start or expand a profitable microenterprise with capital and some basic advice.[6] 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 fewer constraints.

There are obvious limits to what we can learn from five firms in one country. The same is true of any impact evaluation, where one program is usually evaluated in one setting. Studies comparing formal to informal workers typically use country-wide surveys of firms and workers. These country-wide studies have the advantage of comparing informal workers to formal workers across a great many more firms, but these comparisons typically rest on the assumption that informal and formal workers are similar after accounting for a handful of demographic characteristics. These studies also typically have data on only a narrow set of outcomes, such as earnings. Hence the health and other non-pecuniary consequences of different occupations is not known.

What this case study and experiment offers, besides a randomized design, is the opportunity to measure a range of outcomes over time, including physical and mental health. Of course we need to be cautious about generalizing from five firms. At best our results speak to low-skill light manufacturing and agribusiness, in contexts where workers are essentially disposable from the perspective of firms. This is a common feature of light manufacturing and agribusiness in Ethiopia, and indeed 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 U.S., Europe, and Asia.

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

[5]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.

[6]Growing evidence suggests that the average poor person has high returns to capital but is credit-constrained, and injections of cash or capital increases hours worked and earnings in self-employment (Udry and Anagol, 2006; de Mel et al., 2008; Haushofer and Shapiro, 2013; Blattman et al., 2014, 2015). Cash transfers can also relieve the risk that someone takes an unwanted or unsafe job to cope with shocks.

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.[7] Since the sample self-selected into applying for industrial work, moreover, it was not clear that they were natural entrepreneurs and would start businesses, even with a grant.

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. Even short spells in industry resulted in significant increases in serious health problems a year later.

Some of these insights come from post-treatment qualitative observation and analysis of the cross-sectional and panel data. First, we see no evidence of an industrial wage premium in these five firms. A simple (non-experimental) wage comparison adjusting for baseline characteristics 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. Thus, while earnings from industrial work were less volatile over short periods, such as a week, earnings in the industrial sector were not necessarily more stable over the horizon of a month or a year.

Third, turnover in the industrial jobs was extremely high, in part because higher-wage informal opportunities arose over time. Almost a third of people offered a job quit the study firm in the first month, and 77% had quit within the year. People generally quit the industrial sector altogether, rather than simply switch firms. They frequently quit before they had found alternative work. The firms reported that they found the high levels of turnover inconvenient, but were generally able to fill the positions with other low-skill workers since, given the style of production, these workers required virtually no training. The firms were reluctant to raise wages in order to reduce turnover.

Another reason people said they quit was that they found industrial work unpleasant

[7]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.

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 one-year experimental impacts, our results suggest the income gains from industrial work were meager and the health risks substantial compared to controls, while the entrepreneurship program raised incomes by a third without significant health costs.

First, because of the high quit rates, the job offer only had a modest effect on long-term employment. After a year, 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, significantly deterred people from joining an industrial firm and raised their earnings. Those who received the capital and training invested a large proportion of the grant and shifted occupations 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, suggesting that 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 gain given 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? Our data and research design cannot say for sure, but 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 qualitative evidence that they underestimated the unpleasantness and 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. This is somewhat speculative, but 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's no indication that the opportunity cost of time in trying out one of these jobs is high for the firm or worker, at least for these largely disposable jobs. If not for the worrisome health effects of these jobs, this would look like a relatively competitive, frictionless, integrated labor market.

In Section 2, we describe the context in more detail, including the institutional features of the labor market. Section 3 presents the experimental design and procedures used. Section 4 and 5 offer descriptive and experimental results. Section 6 offers a discussion of the results.

2 Setting

Firms exporting from Asia are looking to Ethiopia as a potential new industrial hub (The Economist, 2014). The country has many 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 quite poor, as 78% live under $2 a day in purchasing power parity (PPP) terms, and agriculture employs about 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 15 years, the state has encouraged entry by private firms, with relatively limited controls. 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.[8] In 2009 there were 186,799 temporary and permanent

[8]Industry has grown in absolute rather than relative terms, however, and the 13% share of industry in

employees in manufacturing firms with more than 10 employees (CSA, 2011). The country has also attracted increasing levels of Foreign Direct Investment (FDI), mainly from Turkey, China, and India. It is concentrated in labor-intensive manufacturing, with three quarters accounted for by garments and textiles and leather and footwear, followed by commercial agriculture (Chen et al., 2015; World Bank, 2015).

This expansion is still small compared to levels of manufacturing FDI and industrial jobs in countries such as Bangladesh, but is nonetheless large by African standards, and reflects one of the continent's few committed attempts to attract labor-intensive industries.

The beginning of the study, when firms were being recruited, was first a boom time followed by a mild slow down. The manufacturing census data for 2010/11 confirm that after manufacturing employment growth between 2007/08 and 2009/10 of almost 50%, employment contracted between 2009/10 and 2010/11 by 6% (CSA, 2011). There was significant sector variation, with industries such as textiles contracting considerably (more than 30% in terms of employment), while footwear and food and beverages expanded.[9] 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.

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, with no obvious skill premiums between the private and public sector, and with lower (but still considerable) urban unemployment.[10] 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.[11] The governing labor law makes it also relatively straightforward to fire an employee.

national income (and less than 5% for manufacturing) remained more or less constant throughout this period, given high growth rates across all sectors of the economy(Söderbom, 2012). Nonetheless, manufactured exports per capita rose by 10% per year over this period. This pace of growth in manufacturing, industrial output and the general economy has continued since then (World Bank, 2015). In 2009 there were 739 manufacturing firms with more than 50 employees, including 138 state-owned firms (CSA, 2011).

[9]Although no new census data have been released yet, Chen et al. (2015) have access to later data as part of a World Bank study and report expansion in all these sectors in subsequent years.

[10]See Bigsten et al. (2013) and Appendix A for a brief history of the Ethiopian labor market.

[11]The private formal labor market is relatively flexible and unregulated. Ethiopia has no legal minimum wage, but there are rights to unionization or collective bargaining. Unionization and collective bargaining are most common in the public sector and state-owned enterprises. Unions are present in some private enterprises (including in four of the five firms in our study). Still, trade union density remains limited. Shortly before our study began, a national survey showed only 12.9 percent of wage and salary earners were unionized (ILO, 2011). Still, they are largely focused on workers rights, benefits, and labor conditions, and in none of the firms studied were they involved in wage setting.

3 Experimental design and procedures

3.1 Study firms and jobs

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 (due to dust particles or chemical fumes); discomfort and fainting from standing or lack of breaks or water; and occasional safety hazards such as wet floors, sharp instruments, and so forth. In interviews, workers who worked with chemicals—typically cleaning solvents, pesticides, dyes or glues—sometimes reported fainting from inhalation.

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

3.2 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 con-

[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).

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 3.5.

tacts.[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.

To our surprise, more than three-quarters of the firms we approached were open to the study.[14] Relatively few existing firms, however, had plans to open a new production line (or other major expansion) and thus hire a large batch of workers at once. There are several apparent reasons for this. One is that sector growth was slowing in this period (though not contracting, with the exception of the textile industry). Also, some of the sector growth was coming from new firms, often but not always foreign-owned, who were reluctant to participate because their start-up activities were already complicated and uncertain enough. Thus these new entrants did not join our study; rather, we dealt with firms that tend to have several years of operation already. Finally, not all existing firms planned to hire their employees in bulk at one time, but rather planned to hire people more piecemeal, to accommodate more gradual growth as well as cope with regular attrition. In the end, five existing firms (one foreign-owned) had large expansion plans and followed through. At least two other firms intended to participate, but suffered prolonged delays and did not open their new line during the study period.[15]

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 the five study firms a case of? Our firms were not a representative sample of industrial firms in their sector, but broadly-speaking, 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

[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 (and hence requiring an investment certificate). We contacted most firms via phone or walk-in, though some came from personal contacts.

[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.

and heavier or more capital-intensive manufacturing.

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.

Compared to a representative sample of industrial firms in 2014 in the capital Addis Ababa, our five firms had higher revenues, lower profits, two to three times as many production employees, and lower-skilled employees.[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.[17]

3.3 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).[18] 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.[19] 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.

[16] See Appendix C for data and analysis.

[17] 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.

[18] 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.

[19] 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.

3.4 Experimental procedures

We 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, word of mouth, and local job boards.[20]

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.)

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.

The research team then debriefed eligible applicants on the random assignment to the job, and informed them for the first time about the grant. Nearly all agreed to enter the study and were invited to complete a baseline survey. Afterwards we randomized people to each treatment group via computer, stratified by gender. The firm posted the names of people receiving the job offer at the factory site and the research team contacted all those assigned to the job or grant. 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.

3.5 Study participants and balance

Table 2 reports characteristics of the eligible applicants at baseline, from self-reported surveys, and tests of balance.[21] 80% were women. The average applicant was 22 and had

[20] To fill vacancies on existing production lines, the hiring process was typically much different. Only one or two people might be needed at a time. Applicants would be interviewed on a first-come, first-serve basis. Sometimes word would go out through existing employees (referrals). But this was not the procedure followed for large hiring batches like the ones we studied.

[21] 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

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. They were inexperienced. 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 offered at the time of hiring. Work conditions and paths for progression were poorly understood.

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 0.02 for the entrepreneurship program. To minimize bias, we control for baseline covariates when estimating treatment effects.

3.6 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).[22] 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. These rates of attrition are comparable to or lower than other panels of young adults in Africa (e.g. Baird et al., 2015; Blattman et al., 2014). Note, however, that 5.3% were not interviewed because they left the country. Young Ethiopians commonly migrate 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

enumerator delivered surveys and the games verbally in the local language. The games remunerated the respondent with roughly a half days wages.

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

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 5.7 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. Controlling for baseline covariates, attrition (including emigration) is 2.3 percentage points higher in the control group than the treatments, but this is not statistically significant.[23] 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 (a personality trait that indicates lower perseverance and discipline).[24]This is a further reason to control for baseline covariates when calculating treatment effects. We bound these effects for attrition scenarios as well.

3.7 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, time trends, and simple observational analysis tell us about the treatments as well as employment opportunities in the absence of treatment.

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 both the job and the

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

[24]The p-value on a joint test of significance of baseline covariates is <.001.

industrial sector as a whole. Table 3 reports take-up and turnover rates, and differences by, 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 (or fail to find another job in the sector). Between baseline and endline, 69% of the control group held a formal sector job of any form 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.[25]

By the end of the year, only 32% of those assigned to an industrial job still worked in the sector in the month before the survey, compared to 20% in the control group. Thus very few of those who quit the study firm went to another industrial firm. In fact, in exit interviews, many firm employees reported that they quit despite having no outside opportunity at that time.

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[26], 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).[27] 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.

[25] 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.

[26] Also know 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.

[27] 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 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	0.11	947	0.776	[.047]***	-0.064	[.050]
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	1,841	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	835	0.225	[.034]***	-0.157	[.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	1,587	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	0.95	0.00	947			0.945	[.030]***

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)

Table 4: Baseline correlates of industrial job retention (job and control arms only)

	# 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]
Observations	518		518	
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.

4.2 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.

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, for example, the share of participants with no employment hours whatsoever fell from 68% at baseline to 36% at endline, and average hours of work per week rose from 7.5 to 26.[28] 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 the text are 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. Depending on the cohort, the endline was conducted at either a boom or mild slowdown.The eight cohorts of hires began at different points in time and space over two years, with highly varied macroeconomic conditions, and this employment rise is consistent across the cohorts.

Second, while receiving an industrial job increased the hours and earnings available (and while keeping a job 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 the employment levels, earnings, and average earnings per hour ("wages") for those with and without any industrial work at endline. Our estimate of the wage 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.[29] We look

[28]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.).

[29]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

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 on average. We can also look with and without baseline controls to observe the effect of selection on the "premium".

At endline, 39% of the sample worked fewer than 5 hours per week. Including these "unemployed" members of the sample (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 being employed 5 or more hours a week (bottom panel), however, 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.[30]

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

5.1 Empirical strategy

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. We control for the baseline covariates, X, listed in Table 2, as well as gender-cohort fixed

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.

[30] Likewise, Table 9 below looks at the complier average causal effect. The coefficient on earnings is positive and reasonably large, indicating that compliers earn more because of the larger aggregate number of hours. But this estimate is not statistically significant.

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 "Average 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

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.[31] 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 $\theta_J - \theta_E$ for completeness. Except for the artificial environment of this research project, however, the grant is not a meaningful counterfactual for the job offer, and so the specific difference should be taken with caution. It is principally useful for comparing whether the risks or benefits of industrial work are echoed in the intensification of 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

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 gives us an approximate sense of the effect of staying longer in industrial work. From Table 3 below, those in the control group worked an average of 3.2 months in a formal job, and this roughly doubles with a job offer. Thus the ITT represents the impact of temporarily ending a person's search and increasing the probability they start and stay with an industrial job. The CATE estimates the effect of an additional month of industrial work.

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).[32] As an approximation, however, these CATE results give us a sense of the consequences of longer time in industrial work.[33]

[31] Given that randomization is at the individual level, there is little reason to cluster standard errors at a unit of analysis higher than the randomization, such as the cohort (Angrist and Pischke, 2008; Abadie et al., 2016). Given the small number of cohorts, moreover, the asymptotic requirements of clustering would not be met.

[32] 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 e.g. number of weeks squared and other nonlinear terms).

[33] 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

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 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 durable assets, due to income smoothing. Partly for this reason, we also plan for longer term follow up surveys in future.

Mitigating the risk of cherry-picking hypotheses

This experiment predated the development of the social science registry and the trend toward registering hypotheses and producing detailed pre-analysis plans. Thus our theoretical priors, estimation strategy, and outcomes were not formally pre-specified and archived.

In light of this, we try to limit concerns of cherry-picking in several ways. First, our theoretical priors, including expectations of an employment and wage premium, were documented in various funding proposals as well as a short policy note discussing "pilot results" from the very first hiring cohort.[34]

Second, to reduce the number of hypothesis tests and risk of "false positives", we follow Kling et al. (2007) and collect major outcomes into family indexes, which are additive, standardized indexes of related outcomes. We focus on eight families of 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.[35]

ITT, and any other indicator (such as "worked at least a month in an industrial job") would not pass the exclusion restriction.

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

[35] 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.

Third, the paper reports treatment effects on all outcomes measured in the endline 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 formal treatment effects in the appendix.

Multiple comparison adjustments

In addition to variable reduction, we can go further and 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.[36] All results are generally 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 illustrated in Appendix D.3, we draw the same qualitative conclusions if we adjust for both treatment arms (hence 16 comparisons).

5.2 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

[36]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.

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

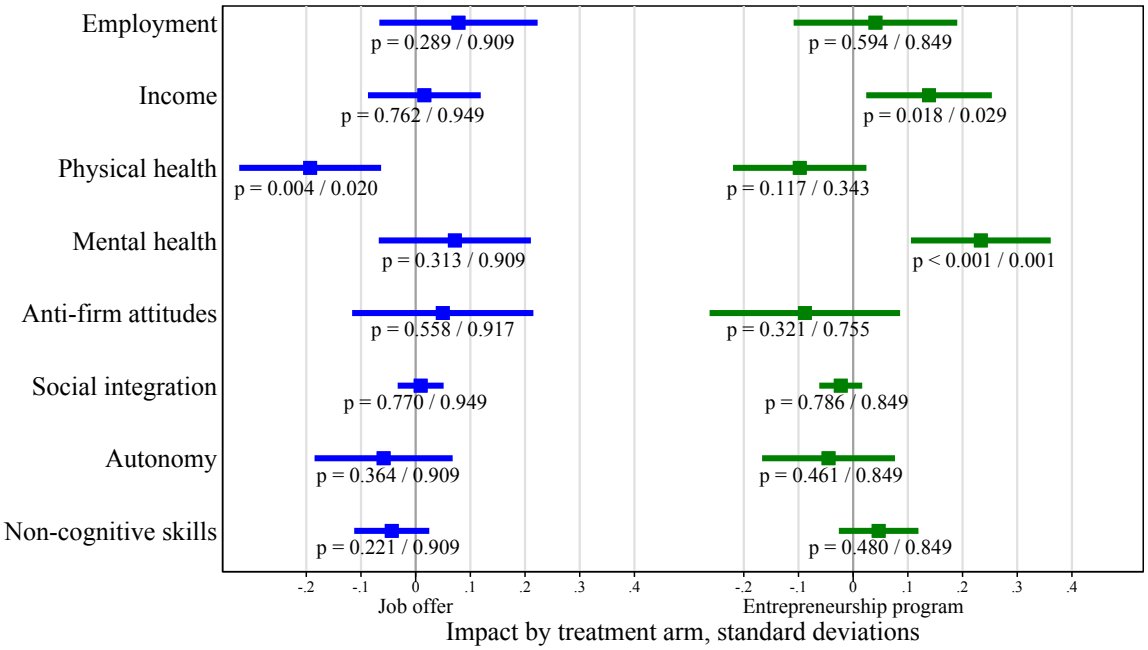
Outcome	Control mean	ITT estimate (N = 1587)							
		Job offer			Entrepreneurship program			Job – Entrepreneur	
		Coeff.	Std. Err.	Adj. p-val.	Coeff.	Std. Err.	Adj. p-val.	Coeff.	Std. Err.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Employment & occupational choice, z-score	-0.04	0.078	[.074]	0.909	0.040	[.076]	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	7.45	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	2.12	-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]		0.338	[8.074]
Household-level durable consumption assets, z-score†	0.07	-0.071	[.069]		0.009	[.067]		-0.079	[.067]
Household-level non-durable consumption, 2010 Birr	664.46	20.548	[34.653]		76.837	[35.492]**		-56.289	[33.861]*
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

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

suggestive and descriptive in nature. We report on them mainly to help understand the 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).[37]

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

[37]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.

significant.

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).

5.3 Impacts on income

We use three measures of income. One is the sum of weekly cash earnings across the 22 occupations.[38] 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.[39]

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.[40]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 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. We do not have data on hours and wages in the firms at the time of hire to calculate exactly, however.

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

[38]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.

[39]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.

[40]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.

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%.[41] 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.

Note that a job offer does not lead to less volatile earnings or works hours compared to the alternatives. 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).[42] 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 earnings in the absence of treatment.[43] We interact the indicator for above median earnings potential with treatment. As a result, the coefficient on assignment to the job offer can be

[41] 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.

[42] 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.

[43] For members of the control group, we regress endline earnings on all baseline covariates and use the estimated coefficients to 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.

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)	0.093	10.570
	[.048]*	[5.753]*
Job × Above median earnings potential (b)	0.031	-13.271
	[.067]	[9.174]
Assigned to entrepreneurship program	-0.160	10.591
	[.040]***	[6.793]
Entrepreneur × Above median earnings potential	0.066	4.537
	[.055]	[10.855]
Impact of job if above median earnings potential (a + b)	0.125	-2.702
	[.048]***	[6.947]
Impact of entrepreneurship program if above median earnings potential (a + b)	-0.093	15.128
	[.040]**	[8.648]*

Notes: See footnote 43 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)

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 9 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 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 10.6 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, overall these patterns imply that people with poor self-employment or wage prospects may be able to increase their earnings in the

industrial sector because it offers them more regular employment than they could otherwise find. 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.[44]

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.

5.4 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.[45] 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.[46]

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, 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

[44]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.

[45]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.

[46]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	-0.12	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)	4.13	0.193	[.111]*		0.377	[.108]***		-0.182	[.109]*
Subjective well being, 5 years from now (0–10)	7.56	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

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	0.388	[.702]
Weekly earnings, 2010 Birr	34.23	1.181	[1.674]
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	7.818	[13.292]
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

to be driven by a few people reporting more serious disabilities at the tail of the distribution. 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.[47] 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 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.

Adverse health impacts are consistent with self-reported workplace conditions

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 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

[47] 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 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

general, air quality and workplace comfort is moderately lower in industrial as opposed to other forms of work, even excluding the unemployed.

5.5 Impacts on mental health and happiness

Table 8 also considers a number of self-reported facets of mental health and well being. First, we used pre-existing, validated questionnaires on depression and anxiety symptoms, and examine additive indices of the symptoms and their severity.[48] In general people report very few symptoms. We also asked people four questions about their financial worries, including whether they worried about meeting expenses for education, health expenses, and other essential costs, as well as worrying about being able to afford less than others (each on a 0–3 scale of severity). 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.

In general 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 program, however, led to small but statistically significant decreases in self-reported financial anxiety, as well as a rise in subjective well-being. There were also small declines in depression and anxiety symptoms, and while these were not individually significant, 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). Furthermore, Figure 1 shows that the improved well-being from the entrepreneurship program is also (statistically) significantly greater than the modest, insignificant benefits of the job offer.

5.6 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

[48] 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).

(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 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.

5.7 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).[49] 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. These imply large systematic differences between missing treatment and control members.

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 generally smaller and less precise under extreme attrition scenarios. The direction of these effects and the qualitative findings are unchanged, however, even in the worst case scenarios.

Finally, Appendix D.8 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.

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

[49]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.

Table 11: Treatment effects on secondary outcomes

		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)
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)	10.83	0.10	[.248]		-0.41	[.257]		0.51	[.259]*
Foreign firms good for country (0-24)	9.87	0.39	[.258]		-0.32	[.268]		0.72	[.268]***
Workers rights protected (0-24)	14.09	-0.17	[.252]		-0.04	[.269]		-0.13	[.267]
Pro-unions (0-24)	12.40	-0.07	[.294]		0.35	[.320]		-0.42	[.315]
Social integration, z-score	0.10	0.01	[.018]	0.949	0.00	[.018]	0.849	0.00	[.018]
Family support (0-15)	12.27	-0.01	[.210]		-0.02	[.223]		0.01	[.223]
Social support (0-15)	9.55	0.17	[.233]		-0.13	[.229]		0.30	[.230]
Community participation (0-8)	3.39	-0.15	[.151]		0.07	[.149]		-0.22	[.152]
Trust (0-12)	5.72	0.25	[.195]		0.08	[.204]		0.18	[.206]
Autonomy, z-score	0.04	-0.06	[.064]	0.909	-0.05	[.062]	0.849	-0.01	[.060]
Independent decision-making (0-36)	27.24	-0.53	[.368]		-0.14	[.415]		-0.39	[.405]
Involvement in spending decisions (0-10)	5.36	0.16	[.265]		0.29	[.262]		-0.13	[.269]
Money freedom (0-12)	4.24	-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	0.10	-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)	16.48	-0.39	[.258]		-0.11	[.276]		-0.28	[.275]
Patience (0-14)	2.90	-0.06	[.073]		0.08	[.078]		-0.15	[.077]*
Risk aversion (0-24)	1.78	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)	11.01	-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)	3.036	3.036	1.299	3.036	2.164	0.181	-3.126
	[4.476]	[5.395]	[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)	12.156	12.156	11.931	12.156	10.090	8.202	5.055
	[5.466]**	[5.457]**	[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	0.239	0.206	0.176	0.127
	[.071]***	[.077]***	[.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

than 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.

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 statistical power to conduct a formal analysis, especially when 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 above, 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.[50]

[50]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.

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(Lundberg, 1985; Morduch, 1995).

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 unem-

ployed. Even if we did, unobserved selection into the experimental sample would cloud any test.[51] 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). 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

[51] 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.

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 of this type include Antonovics and Golan (2012); Papageorgiou (2014); Gervais et al. (2014).

Again, due to unobserved selection into the experimental sample, it is not possible to test this interpretation rigorously.[52] However, much of the qualitative evidence, 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 had little sense of the nature or difficulty of the work, or the hazards involved a priori.[53]

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 at not exceptional levels for firms in these particular labor-intensive sectors. In this light, at best we have estimated local average

[52] 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?

[53] 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.

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).[54] Another example comes from Jordan, where Groh et al. (2014) not 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

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

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 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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