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More Sweatshops for Africa?

Pilot Results from an Experimental Study of Industrial Labor in Ethiopia

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Abstract:

Factory jobs pay steady wages and could offer higher incomes than low-productivity agriculture and self-employment. For some, the question is not whether industrial labor improves well-being but how much. Others are more pessimistic. Classical economists from Smith to Marx viewed industrial labor as dull and deadening. Marx was especially concerned that it would crowd out the worker's ability to seek better employment and "noble" self-labor. This emphasis on self-employment is shared by most states and aid agencies, and poverty alleviation programs emphasize micro-enterprise growth and smallholder farm productivity rather than industrial expansion. Factories generate growth, goes this view, but are not "pro-poor". We test this view with an unusual experiment, one that randomly assigns applicants to industrial jobs to either the job, a self-employment program (of skills training and a cash grant), or neither. We are working with several factories and commercial farms in Ethiopia who agreed to randomize jobs among eligible applicants. This paper reports preliminary results from the first two hiring cohorts at a single firm, a water bottling plant near the capital. This initial pilot group is small—fewer than 100 assigned to either the factory job or the control group—and so the results are not statistically significant. The results are also short-term, looking at well-being just one year after the experiment. Full results on all firms and on the self-employment intervention will be available in 2013. The preliminary results, however, are most consistent with the optimists view. We see substantial reductions in poverty and income risk, and increases in health and subjective well-being. At a minimum, we read the results as no evidence that factory jobs systematically worsen their conditions of life.

"the misery of being exploited by capitalists is nothing compared to the misery of not being exploited at all."
– Joan Robinson (1962/2006, p.45)

1. Introduction

In the world's poorest countries, do workers profit from low-skill and low-wage industrial jobs, or do factories exploit their vulnerability and compromise their well-being? Some observers are optimists. They look at the long lines of applicants for every handful of factory jobs and see revealed preference for steadier formal labor, especially compared to the unpredictability, stress and drudgery of self-employment and agriculture. Many people, goes this view, hold an overly romantic view of self-employment and agriculture, and overlook the fact that most people are unhappy entrepreneurs who actually value steady work and wages. Factories also bring organization, skills and specialization and increase the marginal productivity of workers. To the extent that workers accumulate valuable skills and experience over time, and become less interchangeable with the nearly unlimited supply of unskilled and underemployed labor, they will share in these productivity gains and see their wages rise.

Skeptics rightly point out that workers need not share in these gains. If unskilled and interchangeable, the poor will be paid their (low) reservation wage and firms will capture the gains from productivity, increasing inequality. The scholars writing during England's own Industrial Revolution, from Adam Smith to Karl Marx, feared that specialized, industrial labor would stultify and stupefy workers (e.g. Marx 1891/2004). Exhaustive drudgery would crowd out the time or ability to seek out more productive opportunities and "noble" forms of self-labor—work for oneself and for one's own sustenance. Hence factory labor could present a poverty trap, especially for those compelled to seek factory jobs because of adverse circumstances, such as a bad shock, or because of social, economic and political forces that drive them from traditional livelihoods. Such people may see their well-being fall, and it is in this sense that capitalism and industrial labor was deemed exploitative.

Modern economic theory offers other reasons factory jobs may reduce worker welfare in the long run. Workers might be poorly informed about long term costs (such as health risks) or value short-run wages over investments in human capital that would raise lifetime incomes. Hence imperfect information, impatience, or absence of foresight could reduce their long term well-being—frailties that could be exploited by firms, purposefully or not.

Of course, as Joan Robinson implied, both alternatives could be miserable, with exploitation by capitalism merely less deadening and unprofitable than unproductive, tradition-bound rural life. We regard this not as a maxim but a hypothesis, one still awaiting a rigorous test. What is the effect of obtaining an industrial job on the level, growth, and variability of income? Are industrial jobs stultifying and stupefying and unhealthy, or do higher and less risky incomes lower stress and raise health and happiness?

To examine these questions, we conduct a randomized control trial of factory labor in Ethiopia. We are presently collaborating with ten medium-size firms in different sectors and regions, urban and rural, and are expanding to three more in 2012. Each firm received many times more applicants to low-skill, low-wage positions than it could hire, and the firms agreed to select its new hires randomly from the pool of qualified applicants. We assess the one-year impacts on a variety of measures of well-being. Such one-year "pilot" results are available from two separate hiring waves at one of these firms, a water bottling plant outside the capital Addis Ababa, and presented here. Full results on all firms will be available in 2013.

Unsuccessful job applicants offer a useful, but not entirely fair, comparison. While eligible applicants might represent the most ambitious and able, they could also be the most desperate, or the ones most deprived of traditional livelihoods. Marx's alternative to exploitation by capitalism was not the absence of exploitation, but the opportunity to pursue "noble" self-labor. Industrial jobs might be better than nothing, but are they better than the opportunity to start or expand one's own enterprise?

Thus, our experiment not only asks how industrial jobs compare to their absence, but also how they compare to self-employment. The pool of eligible applicants is large enough that, in addition to a pure control group, we also have a second random treatment group who are offered intensive business skills training and a cash grant of $290[3] to start or grow an enterprise of their choosing—something akin to Marx's noble self-labor. These microenterprise comparison results will be available with the full factory results in 2013.

How important are the answers to the above questions? The bias towards self-employment and away from industrial labor is no idle ideological stance, held by aging and outdated political theorist. One could argue that the idea of noble self-labor pervades thought and practice in development and foreign aid, and shapes the world's approach to poverty alleviation. Indeed, the promotion of smallholder agriculture and self-employment through skills training, capital injections, and other programs is a dominant feature of state and foreign development strategies.

This focus on self-labor comes, moreover, to the exclusion of large firms and industrialization. For instance, take the Millennium Development Goals, or MDGs—the guiding objectives for nearly every major development institution. United Nations strategic plans for achieving the MDGs, for instance, make little or mention of the role of firms or industry (UN Millennium Project 2005; MDG Africa Steering Group 2009). National development strategies often adopt the same bias. Ethiopia is a prime example,

[3] This sum is equivalent to more than 12 weeks of earnings at baseline.

where a repeated series of five-year plans has placed the commercialization of smallholder agriculture at the center of its growth strategy and deliberately put urban industrial growth to the side.[4]

One reason for this bias may be the worry among some policymakers and activists that large firms create poorly paid, low quality, even exploitative jobs. Certainly there are instances of truly exploitative "sweatshops" and other firms, reports of which have made factory labor a scourge of public opinion and led to powerful advocacy movements (e.g. the Clean Clothes Campaign in Europe, or US Students against Sweatshops). But suspicions seem to extend to low-skill, low-wage industrial jobs in general.

Another reason may be the pervasiveness of self-employment and the potential for aid to expand it. Most of the world's poor are engaged in smallholder agriculture or informal self-employment, and so it is natural for policymakers to focus on improving the scale and productivity of these activities. Thus, the World Bank has put agricultural productivity at the center of poverty alleviation (World Bank 2008) and scholars like Hernando De Soto (1989) hail micro-entrepreneurs as the key source of growth. A growing body of microeconomic theory and program evaluations also demonstrates the importance of capital, credit, and skills in igniting microenterprise growth (Banerjee and Duflo 2011). These are tools easily and directly wielded by governments and aid agencies, further enhancing their appeal as development instruments.

We worry this focus on agriculture and microenterprise is rooted in an overly optimistic and romantic view of self-employment and underestimation of the potential for industry to relieve poverty, even among firms offering low-skill and low-wage jobs. Indeed, we contend that smallholder agriculture and microenterprises are risky, stressful, and often undesirable, and that most workers prefer, and benefit from, formal employment and steady wages.

To a large extent, the results bear out the more optimistic view. We first examine the effect of an employment offer on a variety of measures of well-being, from poverty and income levels, to income volatility, productivity and leisure, health, and social status and integration. This quantity is useful, but not the only relevant quantity for our purposes. Since 33% in the control group obtain formal sector employment elsewhere, and 14% of the treatment group refuse the offer, these intent-to-treat estimates do not tell us the effect of factory or formal employment on those who could not get employment in the absence of the offer—that is, the marginal impact of more factory work on eligible people who would take up jobs if offered them. We estimate this quantity by using assignment to treatment as an instrument for having obtained factory or formal sector employment in the past year.

In general we see a modest decrease in poverty levels, especially as measured by consumption in the household, and by the level of assets and other wealth (including housing quality and durable goods). We

[4] These plans include the Sustainable development and Poverty Reduction Program (SDPRP), the Plan for Accelerated and Sustainable Development to End Poverty (PASDEP), and the new Growth and Transformation Plan (GTP).

see less evidence of an increase in income levels, though income is sufficiently volatile and difficult-to-measure that we place more stock in the consumption poverty and asset measures. Income volatility decreases dramatically, however, as the formal sector job provides steady wages.

The evidence suggest that external conditions matter a great deal, however, as these poverty improvements are much more dramatic in the first cohort than the second, who are offered a job in the same factory a year later, when the labor market was tighter and the factory wages were failing to keep up with rising general wages. Nevertheless, the net impact on poverty is substantial, and is echoed by sizable increases in subjective well-being improvement over the past year and anticipated well-being in the near and long-term among the formal sector employed.

These income increases come in spite of the fact that there is no change in hours worked. Formal sector work appears to provide intense but full employment and higher worker productivity as a result. Our findings do not suggest that work conditions and hours are so onerous that it exhausts and dulls the worker, and precludes finding more productive employment. Indeed, we find that formal sector work improves the depth of relationships with others in the community and increases the incidence of community leadership, a sign that the factory employed may have more leisure time.

These results, based on two small cohorts in a single factory in just two different years, by no means provide a comprehensive view of formal sector employment in Ethiopia, let alone Africa. The expanded sample and firms will broaden the base and the external validity, at least within Ethiopia, and provide some of the sole evidence of industrial work on workers in all of Africa.

2. Background and literature

The literature on industrial development sees industry as unambiguously good (indeed, necessary) for aggregate economic growth. The limited evidence we have on the microeconomic impacts, especially on workers, is ambiguous and mixed.

Agricultural interventions can indeed enhance smallholder productivity, reduce poverty, and improve nutrition and health, yet the agricultural sector in general is an unlikely engine of economic growth. Agriculture needs strong demand for its produce to drive innovation and productivity growth (Dercon 2009). The agricultural sector, while important, typically does not produce enough value added per worker to drive long term growth; labor productivity growth, essential for poverty reduction, is also lagging in agriculture.

Likewise, the informal sector and self-employment has limited potential to generate growth and steadily reduce poverty. A host of new, often experimental evidence shows that many of the poor have high returns to capital and, to some extent, skills (de Mel, McKenzie et al. 2008; Banerjee, Duflo et al. 2010; Banerjee and Duflo 2011; Blattman, Fiala et al. 2011). For the most part, however, these poverty gains are small in absolute terms and there is little evidence so far that access to human and physical capital has changed rates of income growth. Woodruff (2006) reviews the performance of the informal sector across developing countries, concluding that its dynamism tends to be overstated. Beck et al. (2003) find no evidence that the size of the small enterprise sector contributes significantly to either growth or poverty reduction across countries. Rather, large informal sector often emerge as a response to poor economic performance and regulation, rather than being a sign of an emerging dynamic economy (Lloyza and Serven 2009). Banerjee and Duflo (2008) examine survey data from more than a dozen poor nations and find that, more than any other activity, wage labor rather than self-employment sets the non-poor apart from the poor.

Evidence from Africa strengthens the economic case for large firms as engines of growth. Using a large comparative data panel data set on manufacturing firms across seven African countries, Bigsten et al. (2004) argue that large firms are more likely to export, and show faster productivity growth. Van Biesebroeck (2005) uses the same data to show that bigger firms actually grow faster in terms of employment than smaller firms. Contrary to evidence from richer economies, there is no evidence that small firms ever become large, at least using careful analysis of census data in Ghana: large firms are large to start with (Sandefur 2010).

Medium and large industry, then, drives aggregate growth. But is this growth pro-poor? The question is unresolved: is industrial development the kind of growth and job creation that favors the poor, or are these jobs poor quality or poorly paid jobs? Are jobs in the informal sector or agriculture superior at poverty reduction? Or, even if less profitable, are better embedded in local social relations, and hence less exploitative? In short, what are the welfare consequences of jobs in these large firms?

The empirical literature on the impacts of industrial labor is thin. In Africa, a simple comparison of wages across different size firms shows a large wage premium for those employed in large formal firms compared to those in small formal firms or those in the informal or self-employed sector (Soderbom and Teal 2004; El Badaoui, Strobl et al. 2008). The same premium is observed in Latin America (Verhoogen 2008). But these correlations may be misleading; higher ability employees may self-select into the large firm jobs, or be carefully screened by employers. Attempts to account for this bias tends to reduce this wage gap or even make it disappear (Pratap and Quintin 2006). The question whether large firms would pay the *same worker* more remains unresolved.

Not only do wages tend to be higher in larger firms, large firms also favor female workers. While the employment of large numbers of seemingly less organized or unionized female workers is viewed by some as a further sign of exploitation, the welfare effects could be positive overall. A large economic literature suggests that female employment improves the bargaining power of women in the household (Strauss and Thomas 1995). Women also appear to have stronger preference for spending on children than men, and thus increasing female income increases child education and health outcomes (Thomas 1997; Duflo 2003). If true, female employment should increase empowerment and child investments.

Evidence from Mexico and Bangladesh bolsters the view that factory jobs boost female bargaining power and investments in children. In Bangladesh, textile firms employ young rural women who have very few other extra-household employment opportunities. Two studies, by Kabeer (2002) and Hewett and Amin (2000), provide suggestive evidence that working in textiles factories is associated with higher female status and better quality of life measures. Atkin (2010) finds that Mexican women in export manufacturing jobs see increased incomes, and have better bargaining power in the household and significantly taller children.

Evidence from Indonesia, however, tells another story. Federman and Levine (2005) look at district-level data on manufacturing employment and adolescent school enrolment. Higher manufacturing growth is associated with higher enrollment and lower labor force participation among adolescents. For male youth, this effect was largely associated with rising adult male employment. At the household level, however, the employment of adult females in manufacturing is associated with lower enrollments, higher labor force participation, and more household responsibilities for young female teens.

What's more, the impact of factory jobs may not be wholly positive. For instance, lifetime incomes for men and women in Maquiladoras are lower than those who never had the opportunity to join a large firm. Atkin (2009) suggests that, since the export sector in Mexico pays higher wages than other sectors, yet does not require high skill level (e.g. a high school certificate), school drop-out rates increase with the arrival of new export jobs. Thus workers induced to enter export manufacturing eventually earn less than they would have earned had the jobs never appeared and they stayed in school.

Such results illustrate the potentially complex effects of industrial employment, some positive, some negative. None of the above studies are able to provide a complete picture, however, typically because of the absence of data on all outcomes of interest, or because they measure at a regional level rather than an individual one.

All of the above studies, moreover, use observational data at the district or household level. This imposes two important limitations. First, the results are often very sensitive to small changes in the statistical treatment of the data. Second, they cannot account for selection bias in who is and who is not employed in firms. For instance, if firms hire more able men and women, we would expect these studies to overestimate the value of industrial employment. If more able people become successful entrepreneurs, on the other hand, we might underestimate the impact of industrial jobs. Hence the direction of causation is not clear, and estimates are likely biased in unknown direction.

Random assignment of workers to jobs, as in the current study, can settle the evidence on any earnings gap conclusively. One other study has attempted this before—as it happens, a study of flower farm contract laborers in Ethiopia (Hjort and Villanger 2011). The study is limited, however, to domestic abuse and female empowerment, and does not speak to the broader economic and health impacts. Their results may give pause to the industrial job optimists, as the authors find a 13 percent increase in physical violence and a 34 percent increase in emotional abuse when women get employed.

3. Intervention and Experimental Procedures

Intervention

We work with ten medium-size firms in Ethiopia who agreed to hire new employees randomly from the pool of applicants who meet certain minimum criteria. The type of job, qualifications, and applicant pool varies from firm to firm. All are low-skill positions which typically require a minimum grade 8 education and good health. Qualified applicants generally outnumber the jobs available, and each the firms agreed to allow us to select its low-skill new hires on its behalf.

Firms were commonly open to randomized hiring. Low-skill positions are typically filled in an ad hoc and first-come, first-hire manner, with no interview process and minimum oversight from senior management. Firms seemed indifferent to the extra administrative burden of screening additional applicants and conducting a randomization, and typically expressed interest in participation 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, the other opportunities available to their employees, and the association between qualifications and performance.

We contacted approximately 40 small, medium, and large firms in Ethiopia, in a variety of sectors from textiles to light manufacturing, food and beverages, and commercial livestock and agriculture[5]. To each firm, we proposed a randomized hiring design in their next expansion or new firm opening, with a research team conducting a survey and randomization after a basic screening by the employer. Approximately 85% of the firms we approached expressed interest, and until March 2012, three had new hiring waves that suited the study.

This paper reports preliminary one-year results from the two earliest hiring waves, both in a water bottling plant. The first wave hired 15 randomly selected persons from a sample of 53 eligible candidates in April 2010. The second hiring wave hired 19 persons randomly selected from a sample of 46 eligible candidates. We refer to these samples as Cohort 1 and Cohort 2 respectively. One-year results from all ten firms, with a total of 450 new hires, will be completed in 2013.

[5] Firms were approached through personal contacts, word of mouth, presentations to industry associations, and by contacting firms who had applied for an investment certificate to the Ethiopian government in the previous year.

The Real Water (TGMD) bottling plant

The Real Water (TGMD) bottling plant is a located in Burayu town, Oromia Region, around 20 km outside Ethiopia's capital Addis Ababa. The plant is more than a decade old, has approximately 150 employees in total, and shares the site with three non-beverage firms owned by the same parent company. The plant manufactures bottled spring water as well as flavored water in various bottle sizes.

In the first wave of hiring, the firm sought 15 workers to work on an expansion project for producing 5L bottled water and expressed a preference for a specific number of females and males. In the second wave of hiring, the firm sought 19 workers to work on a further expansion project to produce flavored water and expressed a preference for seven females and 12 males. Females are typically preferred for assembly line works such as packing, labeling and quality checking, while males are typically preferred for jobs that require physical strength such as loading and unloading products.

Table 1 lists summary statistics for Cohorts 1 and 2. Overall, the average age of applicants was 23 years, 47 % were male and only 13% report that they are married. They had an average of 11.2 years of education. Counting the four weeks prior to the application, applicants reported earnings of $23 (314 ETB) and had an average of 12.3 hours of employment per week p. 31% of the eligible applicants had prior experience working in a factory for at least a month over the past year. Levels of employment and earnings were greater in the second wave than the first wave, in part because of the larger pool of males, but also because of improving local economic conditions, especially a construction boom and increasing scarcity of unskilled labor. These changing economic conditions will prove important and we return to them below.

Experimental procedures

Screening, Surveys and Randomization

Applicants typically submitted a *curriculum vitae* (CV) and other supporting documents proving their educational qualification and work experience to human resource department of the factory.

In the first hiring wave in 2010, 288 applications were received for the 15 vacancies. Real Water human resource staff screened applicants based on these documents, and the screening revealed a preference for individuals with at least eight years of education and who live in the town where the firm is located (since the firm pays transport costs to and from work). Based on administrative data which compare eligible to ineligible applicants, prior factory experience and age did not influence eligibility. The staff identified 53 eligible applicants for this hiring.

In the second hiring wave in 2011, 101 applications were received for the 19 vacancies. Factory staff screened the applicants according to the weighted criteria: applicant's knowledge of factory working conditions and shift work (40%), prior work experience (25%), education (20%), salary expectations (10%), and proximity to Real Water factory site (5%). Consequently, 68 eligible applicants were identified.

Factory staff then passed eligible applicants to a research team, who explained the random selection process and asked permission to administer an optional questionnaire and incentivized games and cognitive tests (which would not affect their chances of receiving the job). All accepted. The lottery followed data collection. For the first hiring wave in 2010, randomization was done via a public draw; for the second wave in 2011, the randomization was done by a computer program, with results posted at the factory site and successful applicants contacted by phone or in person. Successful applicants typically began their work at Real Water within one to two weeks of the survey and randomization.

We assess balance between the treatment and control groups in Table 1 across pre-program outcome measures and basic demographic characteristics. The results suggest balance between treatment and control, with few exceptions. To account for pre-program imbalances, we include lagged dependent variables in all outcome regressions.

Treatment Compliance

In addition to looking at simple intent-to-treat effects of being *Offered a job*, we consider two different forms of treatment compliance, each of which provides a different theoretical and policy insight.

First, we consider whether the *Job offer was taken*, which we define as having worked at least one month in the assigned firm. In our current sample, 10 of 34 people offered the position left the firm within one month. Of these, four left the firm within a week, and one person turned down the job offer without returning. A treatment effect based on the one month cutoff tells us the effect of the job offer on compliers—those who accept a job offer and indicate an interest in continued employment.

Second, we consider whether the individual *Held any full-time formal sector job in the past 12 months*. We code this variable based on an employment history collected at endline.[6] This is an extremely useful treatment effect, as we are mainly interested in the general effect of a formal sector industrial job on welfare - not necessarily the effect of specific firms in our sample. Attainment of an industrial job is endogenous, and our experiment randomly increases the probability of attaining an industrial job.

Average treatment effect (ATE) estimation

We focus on two measures of impact. First, we look at the effect of being offered a position at the factory on outcomes—an intention-to-treat (ITT) estimate. This properly estimates the marginal effect of

[6] We also measure treatment as had a full-time factory job for at least 1 month, where full-time is assumed at least 8-hours per day using self-reported average hours per day worked.

the offer on well-being, but probably underestimates the effect of factory employment on those interested in and able to obtain such employment. It includes those who find factory work unsuitable and leave quickly, but more importantly it also includes in the control group the substantial number of people who get a factory job elsewhere. We might also be interested in an estimate of how factory work affects the well-being of those who would not have otherwise gotten a position unless offered one, and accept when given the opportunity (i.e. compliers). To estimate this, we assess an instrumental variable (IV) estimate of the ATE of factory employment. We define being "treated" as obtaining formal sector employment (an endogenous outcome) and use our experimental assignment as an instrument.

We estimate the ITT, θ_{ITT}, as follows:

$$Y_{1ij} = \theta_{ITT}A_{ij} + \beta X_{ij} + \alpha_{ij} + \alpha_{ij}A_{ij} + \varepsilon_{ij} \qquad (1)$$

where Y_{1ij} denotes the outcome variable, A_{ij} is an indicator for assignment to treatment. This approach (the ANCOVA estimate) is more efficient than a difference-in-difference estimator (Frison and Pocock 1992; McKenzie 2011). X_{ij} is a pre-specified (optional) set of baseline covariates (principally used to correct for covariate imbalance after random assignment), α_j is a hiring wave fixed effect, $\alpha_{ij}A_{ij}$ is an interaction between hiring wave and assignment treatment, and ε_{ij} denotes the error term.[7]

For the IV estimate, θ_{IV}, we use A_{ij} as an instrument for treatment T_{ij}:

$$Y_{1ij} = \theta T_{ij} + \beta X_{ij} + \alpha_{ij} + \alpha_{ij}A_{ij} + \varepsilon_{ij} \qquad (2a)$$

$$T_{ij} = \pi A_{ij} + \delta X_{ij} + \alpha_{ij} + \alpha_{ij}A_{ij} + \varepsilon_{ij} \qquad (2b)$$

Estimations of (1), (2a-b) are run with and without the interaction term.

To gauge the size of the treatment effect, we also show control group means and calculate the ATE as a percentage of the control group mean. This gives us a measure of the magnitude of the effect.

[7] The vector of controls includes age, squared age, religion dummy, gender dummy, household dependency ratio, civil status dummy, wealth index, education, literacy, math cognitive skills, risk aversion index and a time preference index.

4. Data and Measurement

Data

Survey data

Baseline surveys were conducted with all eligible applicants. The survey investigates the applicant's education, employment history, demographics and health, cash and savings, wealth and consumption and social networks, among other topics.

Endline surveys were conducted after six months and again after one year for the first hiring wave. For the second wave, a single endline survey was conducted 11 months after baseline. To conduct the endline surveys, a research team tracked the applicants that had participated in the baselines. Upon successful tracking, a set of two of surveys, one concerning the individual applicant and one concerning the applicant's household, were administered to the applicant and the head of his/her household, respectively. These endline surveys include questions on employment, health, consumption and expenditure patterns, economic activities of household members, community participation and psycho-social well-being, among other topics.

Attrition

Out of our total applicant pool of 99, 21 individuals were not surveyed at the endline, leading to an attrition rate of 21%. Of these, nine had moved abroad, ten refused to participate in the endline survey and two could not be located. For the household survey, 16 households were not interviewed at the time of endline, making the household attrition 16%. The household attrition is lower than the individual attrition since for some individuals who had moved away and could not be surveyed, the household were still found and was willing to participate in the survey.

The individuals in our sample are highly mobile. In addition to the nine people who moved abroad, 18 people had moved within the country, so that a total of 27% of the sample had moved between the baseline survey and endline survey one year later. The reasons for moving to a different area are diverse, but are typically related to looking for better paying jobs, family needs or emergencies. The individuals who moved abroad have in most cases gone to work as domestic maids in Middle Eastern countries. We do not have full survey data on these emigrants, but telephone conversations with a subset suggest the payment received for this kind of job is between $90 and $120 per month ($3 to $4 per day). The others who moved to different places inside the country work as day laborers in construction industry (which pays around $2.10 per day), or in commercial farms. Some have joined a government-sponsored initiative where they prepare and shape stones for a cobble stone road project. They are paid on piece rate and their total payment depends on the number of stones they prepare in a day. Most of them get $3 per day on average.

In order to mitigate the attrition, the research team collected thorough contact information during the baseline survey and requested the applicants' willingness to participate in the endline survey. In cases where the individuals nevertheless cannot be found, an absentee survey is conducted with their household heads or other household members to obtain basic information about the individual's economic activities, health, education and children.

Key measures

We focus on the level, growth, and variability of employment and income, subjective well-being, and health as our primary measures of welfare. Our outcomes are summarized in Table 1.

Economic outcomes

Level of employment. We first consider employment in the industrial sector using the variables *Job offer was taken* and *Held any full-time formal sector job in the past 12 months,* as described above. Notably, 32% of our control group held a full-time formal sector job in the past 12 months.

We next turn to *Hours on all economic activities in the past four weeks*, which includes formal and informal sector employment but excludes household work and chores. We additionally consider *Hours on all economic activities in the past year,* which we construct by multiplying the hours spent on each activity in an average month by the number of months the respondent reports doing each activity in the past 12 months, and summing across all activities.

Poverty levels. To measure levels of poverty, we construct *Aggregate household consumption* and *Aggregate household consumption per capita* from an extensive survey module on food and non-food household consumption. To reduce the influence of outliers, we also consider the log transformation of the two consumption variables. As an additional measure, we calculate an *Index of wealth* z-score using 7 measures of housing quality, 20 household and business assets, and 5 types of landholdings. The index is the score from the first principal component of these assets—shown to be a relatively reliable proxy for full consumption aggregates (Filmer and Scott 2008). Finally, we consider the self-reported *Number of times going to bed hungry in the past four weeks*.

Income and earnings. To calculate incomes, we ask respondents to estimate their profits from business activities and wages or earnings from other activities in the previous seven days by activity, and calculate *Total profts earned in past week*. An analogous calculation yields *Total cash earnings in past month*. For both measures, we consider the natural log transformation to reduce the influence of outliers.

An important mechanism by which a factory job may increase welfare is through reduced income risk. To assess income risk, we ask respondents to consider the next year under good and bad conditions ("If things are good/bad next year..."), and to report their projected income under each condition. *Highest minus lowest foreseeable income in next year* is calculated as our measure of income risk.

Subjective well-being and work place well-being. The provision of a steady and reliable source of income may also affect self-reported subjective well-being. We measure *Current well-being* by asking respondents to place themselves on 10-step ladder where the bottom rung represents the worst possible life for the respondent and the top run represents the best possible life for the respondent. We also consider the difference between current well-being and last year's reported well-being as the *Improvement in subjective well-being*. *Anticipated well-being improvement (near-term)* and *Anticipated well-being improvement (long-term)* are defined in an analogous manner using anticipated ladder positions in one and five years respectively. Lastly, we construct an *Index of work place well-being* from a series of questions on the quality of the workplace environment, including physical comfort, temperature, ability to control hours, ability to take time off for personal reasons, opportunities to learn new skills, sufficient physical space, ability to take short breaks, and workplace boredom.

Health outcomes

To assess health impacts, we construct an *Index of disability* from reported difficulty with everyday tasks: walk two kilometers, carry a 20-liter water jug, perform routine daily activities, work a full-day on feet, and stand at a workbench or assembly line for eight hours. We next turn to psychosocial well-being. First, we construct an index of psychological distress, *Index of depressive symptoms (PHQ),* using questions from the standard Patient Health Questionnaire (PHQ) index. Second, we examine an *Index of generalized anxiety disorder* using standard GAD questions. Lastly, we consider an *Index of Aggressive Behavior,* which we aggregate from the self-reported frequency of seven aggressive behaviors.

Community outcomes

Receiving a steady-wage factory job could affect community participation and family relations through increased perceived status, self-esteem or leisure time (as labor productivity increases). We examine several measures of empowerment in the community, including a binary indicator for *Holds leadership position in any group or Kebele* and the *Index of community participation*, an aggregate of several individual measures: group leadership, source of advice to others in community, attendance and participation in meetings, attendance at religious services and voting. We also consider the quality of family relations with our *Index of family support,* an aggregated index of questions on the frequency of family quarrels, the availability of help and advice from family members, and feelings of value within the family. The *Index of community support* is constructed analogously with respect to relations with friends and neighbors. Lastly, we turn to the respondent's *Index of pro female empowerment attitudes*, which aggregates seven agree/disagree responses to statements on female rights, as well as the *Index of involvement in spending decisions*, constructed as the aggregate of self-reported involvement in household spending decisions for school fees, household items and assets.

Attitudinal outcomes

Finally, we measure attitudes towards labor and firms through a series of Likert-scale responses to statements on labor unions, large scale farms, foreign firms and the state of worker rights in the industrial sector. From these we construct the following indices: *Index of positive attitudes toward labor unions*, *Index of positive attitudes toward large firms, Index of positive attitudes on the state of workers' rights,* and *Index of positive attitudes toward foreign firms*.

5. Results

Before turning to results, we emphasize two points. First, the initial sample is too small to provide statistically significant or conclusive results. Hence we should regard these initial results as preliminary and suggestive only. Nevertheless, they provide a general sense of the direction and magnitude of the program impacts, and many of the patterns are consistent across measures and survey rounds.

Second, labor market conditions were very different between hiring Wave 1 (2010) and hiring Wave 2 (2011). From 2010-11 economic opportunities outside the factory were relatively poor, while from 2011-12 economic opportunities outside the factory were relatively strong due in particular to a construction boom. The evidence support this trend: among Cohort 1, 43% of the control group reported full-time industrial sector employment at endline and 15% of the treatment group refuse the offer (presumably for a better outside offer), compared to 45% and 31% among Cohort 2, respectively. As discussed previously, treatment non-compliance will bias our ATE estimates downward. Accordingly, we place particular emphasis on our IV estimate, which by using treatment as an instrument for industrial sector employment better controls for treatment non-compliance.

Tables 2-7 display treatment effects for each outcome family, reporting the ITT impact of *Offered a job* and the IV estimate of *Held any full-time formal sector job in the past 12 months*. As we discuss below, treatment effects for Cohort 1 are large and positive, and more muted, or in some cases negative, among Cohort 2.

Economic Impacts

Poverty

Table 2 displays the ATE impacts for the poverty measures. Overall, we see signs of a small decrease in poverty. Household consumption per capita in the past month increases, though this increase is relatively small using the ITT estimate. The IV estimate is larger—a 29% increase in household consumption per capita, suggesting that successful job-seekers in the control group ("treatment non-compliance") biases the treatment effect downward. We also look at the natural log of aggregate consumption per capita, adding 1 for values of zero to avoid losing observations.[8] This transformation reduces the influence of outliers, which may be particularly influential given our small sample size. Here, the treatment coefficient is measured more precisely, is nearly significant, and is larger when using the IV approach.

Turning to household durable assets, we see larger effects—asset wealth in the treatment group is 0.13 standard deviations higher than the control group, while the IV estimated coefficient on treatment suggests a 0.34 standard deviation rise in asset wealth. Finally, we look at hunger as a measure of severe poverty. On average, members of the control group report going to bed hungry 0.3 times in the past four weeks, relatively low but still substantial. Treatment reduces this incidence of hunger by 49% relative to the control group mean. The positive treatment effects across all measures of poverty, suggest real decreases in the overall level of poverty as a result of treatment.

Consistent with our discussion of differential labor market conditions in 2010-2011 and 2011-2012, we observe considerable heterogeneity by Cohort. Across all measures, the sum of the treatment coefficient and the *Treatment X Cohort 2* interaction results in estimated treatment effects close to zero, while estimated Cohort 1 treatment effects indicate large reductions in poverty, often statistically significantly so. For example, treatment appears to have decreased consumption by 138% among Cohort 2 while increasing consumption by 85% among Cohort 1. This pattern also holds for household asset wealth. Cohort 2 treatment decreases household asset wealth by 0.4 standard deviations, and this effect increases in magnitude to -1.17 standard deviations using our IV estimate, suggesting the effect is real. By contrast, treatment increases household wealth among Cohort 1 by 0.66 standard deviations using the ITT estimate, and 1.49 standard deviations using the IV estimate. We return to a discussion of heterogeneous treatment effects below.

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[8] An alternative transformation with similar properties to the log, but defined at zero, is the inverse hyperbolic sine, or IHS. We compare results on all natural log transformations herein presented to those of the IHS transformation and find similar results.

Income

Does income rise as a result of industrial sector employment? Table 3 displays the ATE impacts for measures of income and income volatility. Overall, the impact on net income is ambiguous. The treatment effect on net income measured linearly is close to zero, while the log transformation of net income, which reduces the influence of outliers, indicates a small and positive effect (ITT estimate of 0.33). Treatment effects are larger when estimated using the IV estimator to reduce the influence of treatment non-compliance (IV estimate of 0.94). We emphasize results on the log transformation of income, which in addition to being less prone to influential outliers, are more consistent with the small decrease in aggregate poverty discussed above.

However, caution is in order when interpreting our net income results, as our measure of income, weekly profits, is quite volatile, and we find evidence of substantial measurement error: only 22% of those reporting a factory job report income in the past week. This appears to be the result of a bi-weekly or monthly pay schedule. Accordingly, we emphasize the positive treatment effects on consumption and asset wealth in Table 2 as more reliable indicators of the treatment's effect on income and poverty. Measurement of income will be revised in future survey rounds, namely by extending the recall period. Repeated surveys of the current sample, in addition to the larger sample size from additional cohorts, will further smooth this volatility.

Income volatility, measured as the *Highest foreseeable income minus lowest foreseeable income in next year,* diminishes dramatically as a result of the factory job. On average, the treatment group reports 19% less income volatility relative to the control group. The estimated reduces climbs in magnitude to 50% when using our IV estimate, which reinforces our confidence in this result.

As with poverty outcomes, we observe heterogeneous treatment effects by Cohort. The treatment coefficient on the natural log transformation of income, at 0.85, suggests that incomes rose among Cohort 1 as a result of treatment. The interaction of *Treatment X Cohort 2* is -1.08, suggesting little impact among Cohort 2. The estimated reduction of income volatility is also stronger and more precisely measured for Cohort 1 (ITT estimate of -22%) than for Cohort 2 (ITT estimate of -14%) using both the ITT and IV estimators.

Why such muted effects on income? In addition to measurement error, we believe ATE impact is actually the average of a small positive Cohort 1 impact and a small or negative Cohort 2 impact occurring as a result of differential labor market conditions at the time of the respective treatments. It is also possible that wages did not rise in this particular factory at the market rate.

Nonetheless, the general picture from both the income and poverty data suggest that the program did effect reductions in poverty and small increases in income. The ITT impact estimate is 10% on household consumption per capita, 0.1 standard deviation on wealth, -49% on hunger, 11% on income, and -19%on income volatility. That the impacts are larger across all measures when using the IV estimate reinforces this interpretation, and suggests that larger poverty gains are possible.

Employment

Table 4 presents results on employment as measured by houses worked in the past four weeks and the past year, employment at Access Capital, our partner firm, and employment in any factory job at endline. As expected, assignment to treatment is very strongly associated with employment at Access Capital, a relationship that is weakened only by the few cases of treatment non-compliance. The relationship between treatment and factory employment is much weaker, due to treatment non-compliance among the treatment group, as well as the fact that much of control group found factory.

Turning to employment, we see little evidence that employment, as measured by hours worked in the past four weeks and in the past year, have changed as a result of the program. With respect to hours on all economic activities in the past year, treatment effects are close to zero (1%), though measured with considerable error. Hours worked in the past four weeks decrease slightly, by 7%, but this does not appear to be a robust effect. The IV estimates of treatment, at 3% and -18% respectively, are similarly small. Given that employment hours are roughly constant while income, wealth and consumption rise (moderately), this implies an increase in productivity.

The evidence does not suggest, as Marx worried, that these factory jobs are sufficiently consuming or exhausting that they prevent workers from pursuing other opportunities. The average number of work hours per week in the treatment and control groups is 30 and 42, in the four weeks prior to the survey. Those in the treatment group currently and factory employed (N=22) work on average 35 hours per week and spend on average 7 hours per week working outside the factory on other income-generating activities. This suggests that these formal sector jobs provide close to "full" employment and allow (or, for some, necessitate) some additional home production and other self-employment or work, but do not consume all leisure or ability to search for other work.

Subjective well-being

These increases in material consumption result in greater anticipated well-being but no change in current well-being. Table 5 reports treatment effects on past and expected future improvements in subjective well-being. We find suggestive evidence of an increase in anticipated well-being and improvement in well-being over the past year. We measure well-being improvement as the difference between well-being today and well-being one year ago. ITT estimates indicate a treatment effect of 35% relative to the control group. The estimated treatment impact increases to 104% using the IV estimate. We also observe increased expectations of anticipated well-being improvement in the near and long-term, measured as the difference between well-being today and well-being in two and five years respectively. Here, estimates of near-term well-being improvement are 10% and 29% for the ITT and IV estimates respectively, and 15% and 44% respectively for long-term expected improvement.

The fact that changes in the past year seem to be larger than anticipated future changes suggest that the workers with a formal sector job are optimistic about their wage and employment prospects, but that the level effect of getting a formal sector job is greater than the perceived medium-term growth effect. Even if there is a growth effect, simple diminishing marginal utility of consumption could explain this pattern as well—going from unemployment to employment and income may bring far more valuable consumption and esteem and security than that provided by further wage increases or improvements in employment.

Workplace conditions, as measured through a series of questions on the quality of the workplace environment, display no significant change as a result of treatment status. A larger sample size and number of firms is necessary to interpret these impacts.

Health Impacts

Industrial employment, it is feared, carries risks to worker health—from industrial accidents, exposure to harmful materials or hazardous working conditions, or stressful and repetitive work. On the other hand, regular wage employment could also improve health in at least two ways. First, a reliable source of income can help in responding to or planning for a health shock. Second, working conditions may be better in the industrial sector relative to the informal or construction sector.

We present one-year health impacts in Table 6. The results point to a small decrease in disability, a slight increase in symptoms of anxiety, and no substantial effect on symptoms of depression or self-reported aggressive behavior. We measure disability as an index of reported difficulty with six daily tasks, such walking two kilometers. The mean level of disability in the control group is 0.85, suggesting very low levels of disability among our population. ITT Treatment impacts are small—a 20% decrease in disability with respect to the control group mean. The impact pattern is also similar to what we see among poverty impacts: improvements for the first cohort and more muted effects for the second cohort.

Symptoms of generalized anxiety increase 11% or by as much as 32% using the IV estimator, but do not appear to be accompanied with concomitant increases in depression (ITT impact estimate of 3%).

Granted, any sort-term health gains could be offset by longer-term health risks that do not appear in the space of a year. Health risks are also specific to firms or industries, and water bottling may be particularly risk-free.

Community and Family Engagement Impacts

Industrial sector employment may also affect family and community relationships. Increases in labor productivity may result in greater leisure time with which to engage with family and the community. Stable employment may instill a source of pride and self-esteem, or may increase status in the community and family. Higher and less volatile income may likewise affect community relations and engagement.

We turn to community engagement and family support impacts in Table 7. We see large positive effects on leadership and community support. The ITT estimated impact on leadership is 0.06, or 67% with respect to the mean in the control group, and increases to 170% using the IV estimator. In a related effect, community support increases 15% (ITT) to 53% (IV). We see no substantial change in family support or other forms of community participation. The overall picture points tentatively toward greater community engagement. Speculatively, the fact that hat we observe little increase in leisure time may indicate that this effect is driven by increases in self-esteem or status rather than greater leisure time in the community.

Turning to attitudes toward female empowerment, we do not see substantial changes, as treatment impacts are small (2%). Finally, we observe tentative evidence that treatment decreases involvement in household spending decisions. The ITT estimate of the treatment effect with respect to the control group mean is -10%, while the IV estimate is -47%.

Attitudes toward capitalism impacts

Finally, we briefly examine how formal employment affects attitudes toward unions, corporations, foreign firms, and worker's rights affected by industrial employment. Table 8 presents treatment impacts on labor market attitudes. We see a slight increase in support for labor unions (ITT impact of 8%, IV impact of 23%) and disappointment with the state of worker's rights in the industrial sector (ITT impact of -10%, IV impact of -31%). We cannot rule out that treatment effects are driven by a greater salience of worker rights issues among industrial sector employees. Nonetheless, one may speculate that the empowerment associated with industrial employment may interact with disappointment at the state of worker's rights to generate a more politically engaged, and potentially restive, political class.

6. Conclusions

We must be modest in our conclusions most of all because of the small sample sizes and the absence of statistical significance. They are also just for one factory at two points in time in a single city.

Nevertheless, we provide some of the first internally valid estimates of the poverty and health and well-being effects of formal sector work in the world, and especially in Africa. These results point to a more optimistic view of formal sector employment, as we observe modest decreases in poverty along with an increase in productivity and leisure and a great improvement in subjective well-being. In the short-term, at least, workers seem to feel themselves far better off with rather than without firm employment.

Another conclusion we may draw is that, in boom times, factories may not be as transformative. Timing and circumstances matter to the extent that they change counterfactual opportunities. Results taken from a small number of factories and time periods should therefore be treated with caution. Even with a larger sample of firms hiring across time-periods, we remain vulnerable to specific country trends. Replication across countries is therefore an important consideration for future researchers.

Overall, however, the results suggest that large firms can be pro-poor, even when wages do not keep pace with improvements in the general economy. Large firms also provide stability and productivity, and the worker can share in those gains with increased material consumption and leisure.

Governments and aid agencies may have more direct and effective tools for boosting self-employment than they do for fostering large firms. Nonetheless, the results suggest that firm development and industrial policy are congruent with poverty reduction strategies, and expand the case for industrial development. Whether that support is indirect—improving the general investment climate, access to finance, and quality of infrastructure—or direct though some form of active industrial policy is largely an empirical question, one still to be answered. These results, however, suggest that the answers should be pursued with more vigor and broader support from the larger aid and development community.

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Table 1: Summary Statistics and Baseline Test of Balance

	Pooled Sample Mean	Pooled Sample N	Treatment Mean	Control Mean	Treatment-Control Diff in Means	Treatment-Pure Control p-value
Economic Outcomes:						
Employment						
Hours on all economic activities in the past year	1,143.71	99	990.75	1,231.11	-240.36	0.32
Hours on all economic activities in the past four	49.10	99	29.97	60.03	-30.06	0.12
Worked in factory at least one month in past year	0.31	99	0.22	0.37	-0.14	0.14
Subjective well-being						
Current well-being (10-point ladder)	3.38	99	3.58	3.27	0.31	0.32
Improvement in subjective well-being (10-point ladder)	0.21	98	0.34	0.14	0.20	0.45
Anticipated well-being improvement (near-term, 10-point ladder)	1.48	99	1.25	1.62	-0.37	0.05
Anticipated well-being improvement (long-term, 10-point ladder)	3.07	99	2.86	3.19	-0.33	0.43
Poverty						
Number of times going to bed hungry in the past four weeks	0.26	99	0.28	0.25	0.02	0.87
Index of wealth	0.01	99	0.02	0.00	0.01	0.94
Amount spent on small expenditures for self in last month	99.09	99	73.83	113.52	-39.69	0.04
Income						
Total cash earned in past week	314.13	99	302.61	320.71	-18.10	0.86
Total cash savings in past month	108.47	99	94.56	116.43	-21.87	0.70
Average hourly wage across all activities in past 7 days	0.04	99	0.04	0.04	0.00	0.99
Total profit earned from activities in past 7 days	12.12	99	5.56	15.87	-10.32	0.47
Health						
Index of disability (0-15)	0.51	99	0.50	0.51	-0.01	0.98
Community Measures						
Index of family support (0-15)	12.41	99	13.03	12.06	0.96	0.07
Index of community support (0-15)	9.86	99	10.78	9.33	1.44	0.01
Number of community groups involved in	1.29	99	1.00	1.46	-0.46	0.08
Holds leadership position in any group or Kebele	0.09	99	0.00	0.14	-0.14	0.02
Index of community participation (0-8)	3.87	99	3.83	3.89	-0.06	0.88
Education Measures						
Total years of education and training	11.19	99	10.76	11.44	-0.68	0.16
Literate	0.99	99	0.97	1.00	-0.03	0.19
Total number of math questions correct (out of 11)	7.22	99	7.08	7.30	-0.22	0.59
Demographic Measures						
Age in years	23.36	99	24.03	22.98	1.04	0.58
Male	0.45	99	0.50	0.43	0.07	0.50
Head of the household	0.33	99	0.36	0.32	0.04	0.66
Number of household dependants (aged, disabled, ill, or under 15 years)	0.89	99	1.11	0.76	0.35	0.26
Civil status: married	0.13	99	0.22	0.08	0.14	0.04
Belongs to Orthodox denomination	0.69	99	0.61	0.73	-0.12	0.22

Table 2: Impacts on poverty

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Household consumption		Natural Log of household consumption		Household consumption per capita		Natural Log of household consumption per capita		Index of wealth		Number of times going to bed hungry in the past 4 weeks	
Treatment	-17.94	756.32	0.15	0.59	66.1	160.91	0.21	0.46	0.13	0.66	-0.16	-0.31
	[475.76]	[552.48]	[0.19]	[0.24]**	[102.03]	[173.07]	[0.15]	[0.21]**	[0.25]	[0.34]*	[0.12]	[0.19]
Treatment X Cohort 2		-1,634.77		-0.94		-193.87		-0.52		-1.06		0.31
		[804.33]**		[0.40]**		[289.71]		[0.33]		[0.43]**		[0.21]
Cohort 2	388.85	876.66	0.12	0.4	140.22	196.76	0.28	0.44	-0.16	0.15	-0.08	-0.18
	[438.31]	[580.40]	[0.24]	[0.31]	[155.80]	[216.68]	[0.19]	[0.24]*	[0.24]	[0.28]	[0.08]	[0.11]
Observations	82	82	82	82	82	82	82	82	82	82	78	78
R-squared	0.08	0.12	0.05	0.11	0.09	0.1	0.1	0.12	0.11	0.16	0.44	0.46
Mean in Control Group	1860	1860	7.151	7.151	636.4	636.4	6.13	6.13	-0.13	-0.13	0.32	0.32
ATE as a % of control group mean												
ATE/Cohort 1	-1%	41%			10%	25%					-49%	-97%
Cohort 2		-47%				-5%						0%
	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)
Any factory job	-46.6	1,585.64	0.38	1.28	171.92	366.76	0.54	1.05	0.34	1.49	-0.46	-0.72
	[1,144.14]	[1,366.19]	[0.47]	[0.69]*	[249.82]	[381.38]	[0.39]	[0.57]*	[0.62]	[0.98]	[0.36]	[0.47]
Any Factory Job X Cohort 2		-4,153.06		-2.3		-457.78		-1.21		-2.66		0.73
		[2,176.77]*		[1.10]**		[695.91]		[0.86]		[1.28]**		[0.57]
Cohort 2	389.76	2,334.25	0.11	1.19	136.83	348.37	0.27	0.83	-0.17	1.07	-0.05	-0.43
	[414.73]	[1,320.86]*	[0.23]	[0.67]*	[150.01]	[424.09]	[0.19]	[0.49]*	[0.22]	[0.70]	[0.09]	[0.32]
Observations	82	82	82	82	82	82	82	82	82	82	78	78
R-squared	0.08		0		0.06	0.04			0.05		0.32	0.27
Mean in Control Group	1860	1860	7.151	7.151	636.4	636.4	6.13	6.13	-0.13	-0.13	0.32	0.32
ATE as a % of control group mean												
ATE/Cohort 1	-3%	85%			27%	58%					-146%	-226%
Cohort 2		-138%				-14%						3%

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

ATE robust standard errors in brackets,

ATE regressions include lagged dependent variables and pre-program individual-level control variables, not displayed in this table

Table 3: Impacts on Income

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
VARIABLES	Total profit earned in past week		NATL LOG: total profit earned in past week plus one		Total cash earnings in past month		NATL LOG: Total cash earnings in past month plus one		Highest foreseeable income minus lowest foreseeable income in next year	
Treatment	2.06	3.13	0.33	0.85	-99.31	-166	0.04	-0.44	-379.85	-459.25
	[7.30]	[9.80]	[0.37]	[0.53]	[83.22]	[114.27]	[0.69]	[0.88]	[440.17]	[641.71]
Treatment X Cohort 2		-2.2		-1.08		137.88		0.98		164.38
		[18.78]		[0.86]		[193.95]		[1.40]		[1168.66]
Cohort 2	-7.21	-6.53	-0.14	0.2	-15.15	-60.13	-0.46	-0.77	1081.17	1024.61
	[11.37]	[14.85]	[0.34]	[0.44]	[108.21]	[139.99]	[0.78]	[0.92]	[499.12]**	[668.24]
Observations	78	78	78	78	78	78	78	78	78	78
R-squared	0.4	0.4	0.22	0.25	0.26	0.26	0.18	0.18	0.18	0.18
Mean in Control Group	19.29	19.29	0.643	0.643	396.4	396.4	4.312	4.312	2034	2034
ATE as a % of control group mean										
ATE/Cohort 1	11%	16.20%			-25.10%	-41.90%			-18.68%	-22.58%
Cohort 2		5%				-7%				-14%
	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)
Any factory job	6.05	7.49	0.94	1.94	-290.63	-396.38	0.1	-0.89	-1,105.47	-1,149.10
	[20.13]	[19.96]	[1.07]	[1.20]	[253.20]	[285.63]	[1.82]	[1.91]	[1,124.47]	[1,386.12]
Any Factory Job X Cohort 2		-4.36		-2.82		312.35		2.94		125.22
		[54.38]		[2.33]		[611.37]		[3.96]		[2,773.32]
Cohort 2	-7.64	-5.29	-0.18	1.3	5.4	-165.43	-0.47	-2.08	1236.45	980.45
	[10.16]	[34.69]	[0.33]	[1.28]	[117.60]	[382.08]	[0.73]	[2.38]	[585.75]**	[1,600.67]
Observations	78	78	78	78	78	78	78	78	78	78
R-squared	0.4	0.41	0.15	0.25	0.09	0.11	0.18	0.13	0.07	0.07
Mean in Control Group	19.29	19.29	0.643	0.643	396.4	396.4	4.312	4.312	2034	2034
ATE as a % of control group mean										
ATE/Cohort 1	31%	39%			-73%	-100%			-54.35%	-56.49%
Cohort 2		16%				-21%				-50%

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

ATE robust standard errors in brackets,

ATE regressions include lagged dependent variables and pre-program individual-level control variables, not displayed in this table

Table 4: Impacts on Employment

	(1)	(2)	(3)	(4)	First Stage Regressions			
VARIABLES	Hours on all economic activities in the past year		Hours on all economic activities in the past four weeks		Job offer was taken		Held any full-time formal sector job in the past 12 months	
Treatment	12.34	116.39	-10.54	-13.97	0.84	0.91	0.33	0.32
	[197.96]	[285.85]	[25.54]	[33.41]	[0.05]***	[0.07]***	[0.11]***	[0.14]**
Treatment X Cohort 2		-215.26		7.35	-0.1	-0.04	0.2	0.2
		[454.11]		[52.14]	[0.05]*	[0.06]	[0.11]*	[0.13]
Cohort 2	245.42	308.54	-7.96	-10.3		-0.17		0.01
	[262.46]	[324.43]	[29.64]	[34.73]		[0.11]		[0.22]
Observations	78	78	78	78	99	99	99	99
R-squared	0.26	0.27	0.29	0.29	0.78	0.79	0.2	0.2
Mean in Control Group	1362	1362	155.5	155.5				
ATE as a % of control group mean								
ATE/Cohort 1	1%	9%	-7%	-9%				
Cohort 2		-7%		-4%				

	(5)	(6)	(7)	(8)
Any factory job	35.59	254.74	-28.53	-33.64
	[521.92]	[593.52]	[63.98]	[72.30]
Any Factory Job X Cohort 2		-650.54		14.76
		[1,330.50]		[137.67]
Cohort 2	242.56	574.47	-6.46	-14.21
	[240.30]	[771.49]	[28.88]	[80.32]
Observations	78	78	78	78
R-squared	0.28	0.29	0.24	0.24
Mean in Control Group	1362	1362	155.5	155.5
ATE as a % of control group mean				
ATE/Cohort 1	3%	19%	-18%	-22%
Cohort 2		-29%		-12%

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

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Table 5: Subjective Well being

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
VARIABLES	Current well-being		Improvement in subjective well-being		Anticipated well-being improvement (near-term)		Anticipated well-being improvement (long-term)		Index of work place well-being	
Treatment	-0.27	0.03	0.19	-0.19	0.14	0.15	0.43	0.67	0.58	1.83
	[0.33]	[0.47]	[0.23]	[0.33]	[0.24]	[0.28]	[0.33]	[0.46]	[1.11]	[1.40]
Treatment X Cohort 2		-0.6		0.78		-0.02		-0.48		-2.54
		[0.71]		[0.55]		[0.53]		[0.71]		[2.11]
Cohort 2	-0.53	-0.35	-0.46	-0.7	0.04	0.05	0.26	0.41	-0.55	0.24
	[0.36]	[0.45]	[0.30]	[0.39]*	[0.24]	[0.28]	[0.36]	[0.40]	[0.96]	[1.22]
Observations	78	78	78	78	78	78	78	78	78	78
R-squared	0.14	0.15	0.09	0.12	0.07	0.07	0.11	0.11	0.06	0.08
Mean in Control Group	4.102	4.102	0.549	0.549	1.329	1.329	2.854	2.854	15.65	15.65
ATE/Cohort 1	-6%	1%	35%	-35%	10%	11%	15%	23%	4%	12%
Cohort 2		-14%		107%		10%		7%		-5%
	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)
Any factory job	-0.77	-0.06	0.57	-0.27	0.39	0.36	1.25	1.52	1.65	4.03
	[0.90]	[1.02]	[0.61]	[0.76]	[0.62]	[0.58]	[0.94]	[0.89]*	[3.02]	[3.25]
Any Factory Job X Cohort 2		-2.02		2.44		0.08		-0.76		-6.65
		[2.15]		[1.75]		[1.53]		[1.86]		[6.07]
Cohort 2	-0.48	0.57	-0.5	-1.79	0.01	-0.03	0.15	0.54	-0.68	2.81
	[0.38]	[1.32]	[0.28]*	[1.08]*	[0.22]	[0.82]	[0.33]	[0.99]	[0.98]	[3.54]
Observations	78	78	78	78	78	78	78	78	78	78
R-squared	0.11		0.11		0.07	0.07	0.01	0.05		
Mean in Control Group	4.102	4.102	0.549	0.549	1.329	1.329	2.854	2.854	15.65	15.65
ATE/Cohort 1	-19%	-1%	104%	-49%	29%	27%	44%	53%	11%	26%
Cohort 2		-51%		395%		33%		27%		-17%

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

ATE robust standard errors in brackets,

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Table 6: Health Impacts

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Index of disability		Index of depressive symptoms (PHQ)		Index of generalized anxiety disorder		Index of Aggressive Behavior	
Treatment	-0.17	-0.49	0.12	-0.66	0.28	-1.09	0.06	-0.06
	[0.29]	[0.53]	[0.66]	[0.78]	[0.70]	[0.66]	[0.41]	[0.68]
Treatment X Cohort 2		0.64		1.59		2.78		0.26
		[0.62]		[1.19]		[1.26]**		[0.90]
Cohort 2	-1.53	-1.73	0.54	0.04	1.4	0.54	0.44	0.36
	[0.29]***	[0.39]***	[0.80]	[0.96]	[0.83]*	[0.94]	[0.43]	[0.54]
Observations	78	78	78	78	78	78	78	78
R-squared	0.45	0.46	0.12	0.14	0.11	0.16	0.07	0.07
Mean in Control Group	0.854	0.854	3.049	3.049	2.476	2.476	2.51	2.51
ATE/Cohort 1	-20%	-58%	4%	-22%	11%	-44%	3%	-3%
Cohort 2		18%		31%		68%		8%
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Any factory job	-0.5	-1.09	0.34	-1.36	0.8	-2.19	0.18	-0.12
	[0.77]	[1.07]	[1.74]	[1.64]	[1.83]	[1.38]	[1.10]	[1.42]
Any Factory Job X Cohort 2		1.65		4.76		8.38		0.84
		[1.52]		[3.66]		[4.49]*		[2.33]
Cohort 2	-1.49	-2.35	0.51	-1.99	1.34	-3.05	0.43	-0.01
	[0.27]***	[0.88]***	[0.79]	[2.33]	[0.78]*	[2.73]	[0.41]	[1.33]
Observations	78	78	78	78	78	78	78	78
R-squared	0.45	0.43	0.13		0.11		0.07	0.04
Mean in Control Group	0.854	0.854	3.049	3.049	2.476	2.476	2.51	2.51
ATE/Cohort 1	-58%	-127%	11%	-45%	32%	-89%	7%	-5%
Cohort 2		66%		112%		250%		29%

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

ATE robust standard errors in brackets,

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Table 7: Community Participation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Holds leadership position in any group or in Kebele		Index of community participation		Index of family support		Index of community support		Index of pro female empowerment attitudes		Index of involvement in spending decisions	
Treatment	0.06	0.17	0.23	1.28	-0.1	0.56	1.44	1.7	-0.54	-0.02	-0.56	-0.77
	[0.10]	[0.15]	[0.61]	[0.93]	[0.46]	[0.66]	[0.61]**	[0.86]*	[1.05]	[1.38]	[0.85]	[1.23]
Treatment X Cohort 2		-0.23		-2.08		-1.35		-0.54		-1.05		0.42
		[0.19]		[1.26]		[1.40]		[1.26]		[2.08]		[1.83]
Cohort 2	-0.06	0.01	-0.33	0.21	-0.92	-0.5	0.12	0.29	-1.3	-0.97	0.31	0.18
	[0.08]	[0.09]	[0.63]	[0.67]	[0.46]*	[0.56]	[0.67]	[0.80]	[1.15]	[1.36]	[0.97]	[1.19]
Observations	78	78	78	78	78	78	78	78	78	78	78	78
R-squared	0.07	0.1	0.14	0.18	0.24	0.26	0.24	0.24	0.24	0.24	0.16	0.16
Mean in Control Group	0.0854	0.0854	4.451	4.451	12.45	12.45	9.354	9.354	24.93	24.93	5.768	5.768
ATE/Cohort 1	67%	195%	5%	29%	-1%	5%	15%	18%	-2%	0%	-10%	-13%
Cohort 2		-70%		-18%		-6%		12%		-4%		-6%
	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)
Any factory job	0.15	0.38	-0.38	1.6	0.96	2.29	4.91	5.47	-1.62	-0.23	-2.73	-2.33
	[0.27]	[0.35]	[1.70]	[2.14]	[1.44]	[1.68]	[2.16]**	[2.28]**	[3.01]	[3.17]	[2.34]	[2.57]
Any Factory Job X Cohort 2		-0.69		-5.74		-3.88		-1.63		-4.03		-1.16
		[0.48]		[4.57]		[3.52]		[4.05]		[6.43]		[5.05]
Cohort 2	-0.05	0.31	0.61	3.58	-0.65	1.36	-0.01	0.83	-1.25	0.84	-0.05	0.55
	[0.09]	[0.23]	[0.54]	[2.34]	[0.56]	[1.93]	[0.87]	[2.22]	[1.12]	[3.79]	[1.01]	[2.98]
Observations	78	78	78	78	78	78	78	78	78	78	78	78
R-squared	0.05		0.07		0.15	0.15			0.22	0.06	0.1	0.08
Mean in Control Group	0.0854	0.0854	4.451	4.451	12.45	12.45	9.354	9.354	24.93	24.93	5.768	5.768
ATE/Cohort 1	170%	449%	-9%	36%	8%	18%	53%	59%	-7%	-1%	-47%	-40%
Cohort 2		-363%		-93%		-13%		41%		-17%		-61%

*** p<0.01, ** p<0.05, * p<0.1
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Table 8: Impacts on Attitudes

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Index of positive attitudes toward labor unions		Index of positive attitudes toward large firms		Index of positive attitudes on the state of workers' rights		Index of positive attitudes toward foreign firms	
Treatment	1.15	1.69	0	-0.45	-1.31	-0.87	0.2	-0.11
	[0.70]	[0.98]*	[0.76]	[1.15]	[0.92]	[1.35]	[0.80]	[1.20]
Treatment X Cohort 2		-1.13		0.93		-0.91		0.65
		[1.60]		[1.56]		[2.19]		[1.74]
Cohort 2	-0.17	0.17	-0.96	-1.23	-3.85	-3.58	-1.88	-2.08
	[0.87]	[1.11]	[0.86]	[0.95]	[1.08]***	[1.19]***	[0.77]**	[0.92]**
Observations	78	78	78	78	78	78	78	78
R-squared	0.22	0.23	0.19	0.2	0.3	0.31	0.16	0.16
Mean in Control Group	15.15	15.15	13.21	13.21	12.67	12.67	14.51	14.51
ATE/Cohort 1	8%	11%	0%	-3%	-10%	-7%	1%	-1%
Cohort 2		4%		4%		-14%		4%
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Any factory job	3.41	4.14	-0.01	-0.89	-3.88	-2.61	0.59	-0.09
	[2.03]*	[2.06]**	[2.02]	[2.51]	[2.73]	[3.16]	[2.18]	[2.55]
Any Factory Job X Cohort 2		-2.13		2.57		-3.67		1.99
		[4.06]		[3.95]		[6.39]		[4.45]
Cohort 2	-0.44	0.66	-0.96	-2.28	-3.55	-1.64	-1.93	-2.96
	[0.83]	[2.38]	[0.80]	[2.13]	[1.16]***	[3.42]	[0.73]***	[2.43]
Observations	78	78	78	78	78	78	78	78
R-squared	0.14	0.16	0.19	0.11	0.13	0.07	0.12	0.09
Mean in Control Group	15.15	15.15	13.21	13.21	12.67	12.67	14.51	14.51
ATE/Cohort 1	23%	27%	0%	-7%	-31%	-21%	4%	-1%
Cohort 2		13%		13%		-50%		13%

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

ATE robust standard errors in brackets,

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