Are Small Firms Labor Constrained? Experimental Evidence from Ghana

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Abstract

Small firms in developing countries are typically modeled as facing a frictionless market for workers, characterized by low search costs, full information, and a lack of regulation. We report the results of a field experiment documenting that firms find it costly to hire workers on the open market, that the marginal revenue product of labor is positive and quite large in small firms, and that there is substantial heterogeneity in these returns as a function of (unobserved) worker ability. We study the impact of a program that randomly placed unemployed young people as apprentices with small firms in Ghana. The program provided a novel worker screening technology to firms (in addition to simply reducing search costs), as (voluntary) participation included non-monetary costs for unemployed young people applying to the program. We find that firms that were offered apprentices by the program hired and retained them for at least six months (the end of our study window). Secondly, treatment firms experience increases in revenues and profits of about seven to ten percent per assigned apprentice. Together, these findings suggest the presence of economically significant search costs in our context. Moreover, revenue and profit gains are particularly large for firms treated with high cognitive ability apprentices. This result highlights the importance of worker screening in firms' hiring decisions, and echoes the widespread use of a sophisticated bond posting mechanism to hire apprentices in our baseline labor market. A simple model in which productivity differences associated with worker ability necessitate costly screening can predict the impacts of our program. In sum, our findings have implications for our basic understanding of labor markets in low-income settings and in particular suggest that high youth unemployment in developing economies is the result, at least in part, of substantial labor market frictions.

JEL Classifications: D22, D61, J23, J38, J46, M51, M53, O12, O14, O15

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1 Introduction

Two of the most ubiquitous features of economic activity in poor countries are an abundance of very small firms and high rates of youth unemployment.[1] Conventional wisdom argues that small firms face a frictionless market for workers, characterized by a lack of regulation (Rauch (1991)) and community networks that limit information constraints and prevent coordination failures (Zenou (2008)). On the other side of the market, it is often argued that unemployed youth lack the skills to be productively employed (Johanson and Adams (2004)), yet have free entry into small firm employment (Harris and Todaro (1970)). Empirical research on small firm growth has focused primarily on credit constraints (e.g. De Mel, McKenzie and Woodruff (2008)) and managerial skill deficits (e.g. Bloom and Reenan (2007))[2]. However, there is little empirical evidence to substantiate assumptions that small firms are unconstrained by labor market frictions. In fact, anecdotal evidence suggests that small firms face high labor market search costs. For instance, firms in our baseline labor market require potential apprentices to post a monetary bond to buy into a job, and firm owners in our baseline survey cite difficulty finding and hiring good workers as a major constraint to growth.

In this paper, we study a national-scale government-initiated and -implemented worker placement program. The program recruited unemployed young people interested in apprenticeships and placed them with small firms in Ghana. It included no subsidy to firms (or workers) beyond in-kind recruitment services, and wages paid by firms to program apprentices are equivalent on average to those paid to non-program apprentices within sample firms. We interpret the intervention primar-

[1] The *World Bank Enterprise Surveys*, firm-level data from 135 countries which include primarily formal firms and only those with five or more employees, nonetheless show a strikingly higher density of small firms in poorer countries and poorer regions. In Ghana, the National Industrial Census (NIC) attempts to capture at least some proportion of informal manufacturing firms and shows 94% of manufacturing firms have fewer than twenty workers and these account for 48% of manufacturing employment (in 2000). Both the Enterprise Surveys and the NIC have been used to argue that firms in Sub-Saharan Africa start small and do not grow over time, in contrast to surviving firms in other regions (Iacovone, Ramachandran and Schmidt (2014), Sandefur (2010)). Hsieh and Olken (2014) present more comprehensive data of both formal and informal firms of all sizes (which is generally unavailable for countries in Sub-Saharan Africa) from India, Indonesia, and Mexico, where 98%, 97%, and 92% of firms have fewer than 10 employees, and 65%, 54%, and 22% of the labor force work in firms with fewer than 10 employees, respectively.

International Labor Organization measures put youth (age 15-24) unemployment at 11.8% in Sub-Saharan Africa and 12.6% in Ghana in 2012 (ILO (2013)). The unemployment rate may also understate the difficulties young people face in the labor market, as many are classified as employed but working only a few hours in agriculture or petty trade. Inactivity rates are also quite high, reaching 50% in some countries, and at least 20% in a majority of Sub-Saharan Africa countries with data, even among young men (Garcia and Fares (2008)).

[2] See also e.g. Anagol and Udry (2006), Bloom et al. (2013), Karlan, Knight and Udry (2012), and Kremer et al. (2013)

ily as providing firms with a non-monetary screening mechanism to identify high-quality workers. In our empirical setting, workers pay this "sweat equity" bond by attending several meetings, interviews, and surveys, and continuing to show interest in the apprenticeship despite a long lag in program roll-out.

Unemployed young people targeted by the program were chosen before any firm recruitment, which then centered around occupational trades preferred by program apprentices and geographic areas with high concentrations of program apprentices. Chosen apprentices and firm owners interested in hiring apprentices through the program were required to attend one of over a hundred district and trade level meetings. At these meetings, firm owners introduced themselves and apprentices were given the opportunity to list the firms with which they would be willing and able to work, based on geographic feasibility and general interest. These listed preferences generated apprentice-specific firm sets.

Within these apprentice-specific firm sets each apprentice was randomly assigned to one of his or her listed firms. Each randomization was independent and apprentices had equal probability of being assigned to each of their listed firms. Firms, consequently, were assigned a random number of apprentices (of differing ability levels at baseline) conditional on non-random apprentice interest. 383 firms were assigned zero apprentices. The remaining 700 firms were assigned between one and six apprentices, with 411 firms assigned one apprentice, 187 firms assigned two apprentices, and 102 firms assigned three or more. In our preferred specification, we control for non-random apprentice interest by including firm-level lottery fixed effects, within which each firm faces an equal probability of being assigned each of the multi-valued treatment assignments. Functionally, we measure the impact of a marginal apprentice across firms with similar levels of apprentice interest.

In addition, apprentices participated in a series of cognitive tests, including a Ravens matrices test, a short math test, an oral English vocabulary test, and a Digit Span Recall test. This detailed data on worker cognitive ability (unobservable to the firm) allows us to estimate experimental impacts of sub-treatments defined by splitting the apprentice sample into two groups. We split apprentices into those who perform above and below the median on each of the cognitive tests, and estimate differential treatment effects by (unobserved) worker cognitive ability (in the sample of firms that were listed by both above and below median workers). We are also able to compare these findings to differential treatment effects in sub-experiments defined by a largely observable

measure of cognitive ability, namely the completion of Junior Secondary School (the end of free and compulsory education in Ghana).

We study a labor market in which firm owners, in the absence of the intervention, make use of a sophisticated bond-posting mechanism to hire inexperienced workers, and nearly universally cite a desire to screen workers as the impetus for the bond[3]. Under the program intervention, firm owners do not charge a monetary fee to begin an apprenticeship, yet screening via a non-monetary mechanism is executed by the government program. The non-monetary screening mechanism echoes the monetary bond-posting requirement. We develop a stylized model to formalize this insight. Workers, who vary by both ability type and wealth, know their type. Firms, however, have no useful signals about worker type. In the absence of any affordable screening technology, large lump sum search costs cause the market to collapse completely and small firms employ no workers (every firm is size one, the owner). In the market equilibrium we observe before intervention, firm owners screen out the lowest quality workers by requiring new apprentices to post a bond in order to begin an apprenticeship. Wages are paid as a proportion of revenues, which depend on ability. Consequently, only those workers whose ability is above a certain minimum level can expect a wage large enough to compensate them for the payment of the up-front bond. Missing credit markets cause a market failure in that workers whose ability exceeds fixed hiring costs remain unemployed if they cannot afford to post the bond.

We then model the worker recruitment and job placement program as a government-financed alternative (non-monetary) screening technology. Workers pay a "sweat equity" bond to signal ability. The model predicts an increase in employment as high ability workers who were previously unable to buy into jobs become employed. If we additionally model the program as paying (all or part of) the fixed costs of vacancy posting and search, employment would increase further as it becomes profitable (or at least zero profit in expectation) to employ lower ability workers.

Our first main result is that firm size increased in proportion to treatment assignment. Like most job training and placement programs, apprentice take-up was less than perfect. However, firms complied with the program design and did not reject assigned apprentices. We show a strong and linearly increasing relationship between total firm size and treatment assignment. Measured using

[3]A market of this type is highly unusual, but the intuition behind it fits a large literature on the bonding critique to efficiency wage models, starting with Becker and Stigler (1974).

lottery fixed effects, firm size increased by about half a worker for each assigned apprentice. These results imply two things. First, firms assigned one or more apprentices did not substitute away from other employment by firing existing workers, and second, firms assigned zero apprentices through the program failed to hire apprentices through some other means six months after apprentice placement. This suggests that though the program included no subsidy, the search and screening costs necessary to hire new apprentices are both a meaningful channel for policy intervention and potentially economically prohibitive for individual firms.

In the second main result of the paper, we show that apprentice labor inputs increased both reported revenues and reported profits, by about seven to ten percent over two rounds of firm-level follow-up data in the Intention To Treat (ITT) specification. We also estimate heterogeneity in revenue and profit effects by occupational trade group, gender, and baseline firm size. We find that effects do not vary by occupational trade group, but may vary by gender (with large and negative, but insignificant point estimates on the interaction term). Our most robust heterogeneity finding is that treatment effects are larger for firms that are smaller at baseline, suggesting that these firms are indeed facing higher search costs. It is worth noting that estimated increases in profits represent a lower bound for the fixed cost of search. We find no evidence that treatment firms invest in capital to complement the additional labor available for production.

Leveraging variation in worker cognitive ability and educational background at baseline, we show that above median cognitive ability apprentices generate larger treatment effects on revenues and profits. This third main result underlies the potential importance of adverse selection in the labor market for inexperienced workers, even in the context of high unemployment and largely unregulated small firms. In the presence of fixed costs to post a vacancy, identify potential workers, and train new hires, firm owners require a screening mechanism to ensure that these costs are recouped in expectation by worker output. Imperfect or missing screening technologies (and in general high search costs) can generate inefficiently low hiring in equilibrium. The ability metrics we use to show that high ability apprentices generate larger treatment effects are not immediately available to firm owners seeking to hire a worker. Signals that are available, like evidence of having completed Junior Secondary School, have no predictive power over the size of treatment effects.

This paper's findings have potentially important implications for theory and policy. The closest paper to ours is De Mel, McKenzie and Woodruff (2013), the first experimental study to our

knowledge of a labor market intervention with small firms in a developing country context. They offered a wage subsidy to a sample of firms in Sri Lanka which was taken up by only about 20% of the firms in the sample, and found no effects on revenues or profits. The program required firm owners to find, screen, and hire their own workers in order to qualify for the subsidy. We should note that in our screening model, a reasonably sized wage subsidy would not increase employment. This is because in our model the binding labor market constraint comes from lump sum search costs and asymmetric information over worker quality, rather than minimum wage restrictions.

We also add to a classic literature on the dual economy and dual labor markets, pioneered by Lewis (1954) and implicit in influential theoretical work on rural/urban migration (Harris and Todaro (1970)). These models argue that in a dual sector labor market, small firms in the informal sector hire mostly family members and thus suffer from fewer coordination failures (Zenou (2008)). In our sample, while family and other socially connected individuals make up a sizable portion of the existing workforce, apprentices previously unknown to the firm owner are common. Recent macro models of informality have started to consider search costs in the informal sector, but direct empirical evidence is still missing (Ulyssea (2010), Meghir, Narita and Robin (2012))[4].

Finally, apprenticeship training is widespread in Ghana and West Africa, and a common employment arrangement by which small firms can access low wage labor inputs and apprentices can gain both training and work experience. Recent non-experimental research has found that apprenticeship training has positive labor market impacts on earnings for completed apprentices (Frazer (2006), Monk, Sandefur and Teal (2008)). This paper is the first evidence on the impact of apprentice labor on firm output and suggests that apprentice placement programs like the one studied here could generate benefits not only for unemployed young people but also for small firms in similar contexts.

The remainder of the paper proceeds as follows. Section 2 describes the setting. Section 3 develops our stylized conceptual framework. Section 4 presents the experimental design, describing our data, the randomization, the program details, and estimation. Section 5 presents our first two

[4]Besley and Burgess (2004) do provide empirical evidence on the topic, but consistent with older literature find that stronger labor regulation in Indian states pushes workers and firms into the (less productive) informal sector. As Rauch (1991) notes, firm size and firm formality are empirically distinct ways to characterize the firm landscape. The majority of both the theoretical and empirical literature focuses on the formal/informal distinction and/or on minimum wage and other direct regulatory restrictions. Our study in contrast focuses on small firms, regardless of formality status, and on search costs inherent in the functioning of the labor market (rather than imposed by government regulation).

main results, and Section 6 discusses our results on worker ability. Section 7 concludes.

2 Setting

2.1 Apprenticeships in Ghana

Employment in informal sector Ghana is heavily influenced by the apprenticeship system. The emergence and prevalence of apprentices as workers in West Africa is documented in Frazer (2006)[5]. Though the apprenticeship institution has a long history throughout West Africa, it is arguably increasing rather than decreasing in importance[6]. The National Industrial Census reports that in 1984, 18% of wage employees in manufacturing were apprentices, while in 2000, 34% of wage employees in manufacturing were apprentices (Sandefur (2010)). These figures are likely understated for small firms, where the vast majority of workers are apprentices. Additionally, while historically the institution tended to function within extended families, modern apprentices are most often hired from outside the extended family.

Although the system has no centralized rules or regulations, it is characterized by a few widely practiced customs. Most firm owners and their apprentices (or apprentices' families) enter into verbal or written employment and training contracts with a duration that varies but is typically three years. These agreements generally require the posting of a bond to start the apprenticeship and the payment of "chop money" or wages throughout the apprenticeship. These wages tend to be quite low, but increase with seniority. At the completion of the apprenticeship, which is marked by the end of the fixed contract duration, by the discretion of the firm owner, or by the apprentice passing an external craftsmanship exam, the apprentice becomes a "master" of their craft. "Master" workers then transition into one of several roles. They may be retained and receive a sharp increase in wages commensurate with their new title. They may be retained and receive only a slight increase in wages under the title "senior apprentice". Most commonly, they may leave the firm, to start their own shop elsewhere, to work as a "master" worker at another firm, or to

[5]The significance of the institution is documented as well in Bas (1989), Boehm (1997), and Birks et al. (1994). Callaway (1964) and King (1977) put apprenticeship in historical context. Mazumdar and Mazaheri (2003) report on survey data from seven countries in Sub-Saharan Africa, where they find that in Ghana and Cote d'Ivoire, over half of manufacturing sector entrepreneurs have completed apprenticeship training.

[6]Apprentices as a proportion of the manufacturing workforce increased dramatically in Ghana in the last thirty years, following liberalization in the eighties and massive expansion in the number of informal sector firms.

leave the craft entirely.

Apprenticeship training is concentrated in small-scale manufacturing and services, where young people can learn a craft, such as masonry, carpentry, or garment-making. Large firms do, however, employ apprentices and often employ "master" workers who completed apprenticeships at smaller firms. Gender segregation by occupation is nearly universal, though garment-making, the most common trade, is done by both men and women. Training often includes basic literacy and numeracy as well as craft skills, and apprentices begin working on actual customer orders almost immediately.

2.2 Labor Market for Apprentices

We began our study with a series of informal interviews with small firms owners in Accra and in rural areas around the country. These discussions highlighted several key features of the labor market for apprentices. First, small firms owners want to hire more high quality apprentices and consider them profitable inputs in the business. Secondly, difficulty finding high quality apprentices and the risk associated with hiring low quality apprentices are widely cited as reasons to avoid hiring at all. Third, the bond posting required to begin an apprenticeship is nearly universally motivated by a desire to force apprentices to signal investment in the apprenticeship, and willingness and ability to learn.

Firm-level baseline surveys included a series of questions meant to quantify, in part, the qualitative observations we gleaned from these interviews and survey piloting. The evidence largely validates our early anecdotal conclusions. Appendix Table 1 reproduces some of these questions, and the most common responses.

3 Conceptual Framework

In this section, we present a stylized model to formalize the insight that, in the presence of search costs and asymmetric information over worker ability, unemployment arises from firm owners' inefficient solution to screening workers. In the model, firms decide whether to hire an individual apprentice and workers decide whether or not to work given an equilibrium wage contract. The model makes a series of simplifications for convenience. Firms are modeled as perfectly competitive,

an assumption that is unlikely to hold in reality. Workers are modeled as having discrete ability types, though in reality ability is continuous. The model is single-period, and ignores training inputs and their potential effects on productivity. Instead, it focuses on the individual decision of a firm-owner to hire or not hire an individual apprentice, which implicitly assumes constant returns to scale over labor inputs.

The first goal of the simple model is to describe the market failure that limits employment without the intervention. The customary apprenticeship bond is modeled as a screening mechanism designed to attract only higher productivity workers. High ability workers expect to gain a return on their bond through wages commensurate with firm revenues, modeled as a share of their contribution to the firm. This solution successfully screens out the lowest ability workers, who would garner negative profits for the firm. However, in the absence of credit markets, it also excludes higher ability workers who cannot afford to post the bond.

Secondly, we use the model to formalize how the intervention affects the market for workers. The program intervention can be modeled in one of two ways. First, it could be the case that the intervention reduced search costs enough to induce the employment of lower ability workers. Second, the program intervention can be seen as providing a non-monetary screening mechanism, which allowed high ability workers unable to afford the bond an entry into employment. We favor the second interpretation, which finds support in the fact that program apprentices earn wages equivalent to non-program apprentices, on average. Modeled as a non-monetary screening mechanism, competitive bidding up of the share of revenues paid as wages is limited by the fixed, government-imposed non-monetary screening mechanism and firms' continued desire to screen out the lowest ability workers. This constraint generates positive profits in equilibrium.

Finally, and most importantly, the model predicts that the program intervention should increase employment. It is worth noting that a wage subsidy equivalent to the market wage would not increase employment in our model.

3.1 Model Set-up

Workers are either high ability θ_H or low ability θ_L. A worker's contribution to a firm is $Y(\theta) = \theta$. Hiring a worker costs $c > 0$, where $0 \leq \theta_L < c < \theta_H$. Therefore, it is unprofitable for a firm to hire workers with ability θ_L and potentially profitable for a firm to hire workers with ability θ_H. Firm

owners do not observe ability and make hiring decisions using expected ability $\hat{\theta}$. For simplicity, we assume that $\hat{\theta} < c$ for all workers[7].

A worker is willing to work if the offered compensation $r_w(\theta) > r_o(\theta)$, the worker's outside option. For simplicity, we assume that the outside option for any ability worker is $r_o(\theta) = 0$ and that workers weakly prefer their outside option, meaning that all workers want to work for any compensation package $r_w(\theta) > 0$. Additionally, workers have an initial wealth endowment of $\gamma \geq 0$ and there is no access to credit. Wealth γ is continuously distributed across workers with some cumulative distribution function F_g.

3.2 Market Equilibrium

If all firms had perfect information about all worker types, then θ_L workers would not work and θ_H workers would work for $w_H = \theta_H - c$. However, if firms are unable to observe worker type prior to incurring c and unable to screen workers, then there is no effective wage $w > 0$ such that expected profits $\hat{\pi} = \hat{\theta} - w - c \geq 0$. Therefore, without some form of screening, no hiring will occur.

Now suppose that firms offer a contract with a negative initial wage $\overline{w}$, but positive revenue sharing ($s \in [0, 1]$), in an attempt to differentiate between low and high type workers. Expected profits are:

$$\hat{\pi} = (1 - s)(\hat{\theta}|s, \overline{w}) + \overline{w} - c$$

where $\overline{w}$ is the bond posted by the worker to buy into the job.

If $\overline{w}$ and s are set such that $s\theta_L \leq \overline{w}$, then the firm can effectively screen out low ability workers and expected profits become:

$$\hat{\pi} = (1 - s)\theta_H + \overline{w} - c$$

where high types are willing to post a bond up to $\overline{w} < s\theta_H$.

In the perfectly competitive equilibrium, firms raise s and lower $\overline{w}$ until $\hat{\pi} = (1-s)\theta_H + \overline{w} - c = 0$

[7]These assumptions apply primarily in the anonymous market for non-family workers. Empirically, family members are rarely required to post a bond, and even close acquaintances or neighbors may also be exempt from the requirement. In these cases, we would presume a few key differences with our model. First, the search and screening costs for family members are likely lower. Secondly the firm owner likely has better information about the ability of the worker he/she knows and can therefore choose to employ or not employe him/her on the basis of that information. Finally, some potential intrahousehold transfers could be enclosed in the employment relationship between family members. Wages in the case of family members would be a function of ability, whether the firm can afford to post the bond elsewhere, whether the worker has connections to multiple firms, and intrahousehold transfers paid as wages. In our data, family members are paid more than non-family members, which we interpret to be the result primarily of intrahousehold transfers paid as wages.

or $s\theta_L = \overline{w}$. Because $\overline{w}$ is unbounded (can take negative values), both of these conditions will hold in equilibrium. Plugging $s\theta_L = \overline{w}$ into $\hat{\pi} = (1-s)\hat{\theta} + \overline{w} - c = 0$ we find:

$$(1-s^*)\theta_H + s^*\theta_L - c = 0$$
$$\implies s^* = \tfrac{\theta_H - c}{\theta_H - \theta_L}$$
$$\text{and } \overline{w}^* = s^*\theta_L = (\tfrac{\theta_H - c}{\theta_H - \theta_L})\theta_L$$

High ability workers whose type is unknown will work if $\gamma > \overline{w}^*$.

3.3 Government Intervention

In our preferred interpretation of the government program, the recruitment process required workers to pay a non-monetary "sweat equity" bond, which allowed for the screening out of low ability workers without the use of a monetary bond. In our empirical setting, the "sweat equity" bond consists of attending several meetings, interviews, and surveys; and continuing to show interest in the apprenticeship despite a long lag in program roll-out. We call this non-monetary screening cost $\overline{u}$ and assume $\overline{u} < (1 - \frac{c}{\theta_H})\theta_L$.

In the model, firms still seek to screen out workers with ability θ_L, such that $\overline{u} \geq s'\theta_L$, where s' is the share of revenues paid to program apprentices. However, unlike $\overline{w}$, $\overline{u}$ is fixed by the program and does not adjust until profits are zero. In equilibrium, $s'\theta_L = \overline{u}$ and firms earn positive profits:

$$\hat{\pi} = (1-s')(\hat{\theta}|\overline{u}, s) - c >$$
$$(1 - \tfrac{\overline{u}}{\theta_L})\theta_H - c >$$
$$(1 - \tfrac{\theta_L(1-\frac{c'}{\theta_H})}{\theta_L})\theta_H - c = c - c = 0$$

Allowing workers to post the bond in a non-monetary way draws out of unemployment that segment of the workforce where $\overline{u} < s^*\theta_H$ but personal savings $\gamma < \overline{w}^*$. This solves the market failure generated by the combination of the bond-posting screening mechanism and missing credit markets to finance that bond. The model also predicts that workers from poorer households would be employed through the program. Though we do not have data on the household wealth of the existing workforce, we have anecdotal evidence from program apprentices that the cost of the monetary bond posting kept them from becoming apprentices in local firms prior to the implementation

of the program.

Of course in reality, there is a continuum of types. In our empirical work we will rely on variation in ability within high types employed through the program to estimate whether worker ability directly affects firm revenues and profits. In that case, the reader should interpret the findings as a comparison of high ability workers to "medium" (or marginal) ability workers (who barely meet the fixed cost cut off).

3.4 Search Costs and Wage Subsidies

An alternative (or additional) modeling of the program could argue that program recruitment of workers lowered the cost of hiring c to c', where $0 \leq c' < \theta_L < \theta_H$. In this case, the competitive equilibrium would result in the employment of all workers at wages $w_H = \theta_H - c'$ and $w_L = \theta_L - c'$. It would also imply that the average worker employed by the program is lower ability than the average existing worker. Though we do not have the same detailed cognitive ability data for existing workers as we do for program apprentices, mean years of schooling are similar between program apprentices and existing apprentices in sample firms.

It is worthwhile to note that in our market equilibrium, a wage subsidy equal to $s\theta_H$ would not predict an increase in employment unless it exceeded the difference between c and θ_L. Where $\theta_L + s\theta_H < c$, firm owners would still seek to charge a bond to begin an apprenticeship in order to screen out the lowest ability workers, and the market failure caused by credit constraints would remain.

4 Experimental Design

4.1 Sample Recruitment

Our study sample comes from 32 districts around Ghana, randomly drawn from the 100 districts slated to participate in the second year of a national scale apprentice placement program[8] . The

[8]The National Apprenticeship Program (NAP) is the flagship program of the Council for Technical and Vocational Education and Training (COTVET), a relatively new national body that acts under the office of the president (rather than a particular ministry) to coordinate technical training across ministries. COTVET has no operational presence outside of the capital city, however, and the majority of the implementation of the program was carried out by district-level officials of the Ghana Education Service (the operational arm of the Ministry of Education).

districts include Accra and Kumasi, the two largest cities in Ghana, as well as rural districts in all ten regions. Figure 1 shows the selected districts.

Firms in the sample were recruited by local government officials and craft-specific trade associations to hire and train the unemployed young people who were the real targeted recipients of the program from the perspective of the government[9]. Recruitment of firms took place independently of apprentice recruitment and after the apprentice recipients were chosen, though it was targeted in the sense that local government officials and trade association leadership sought firms that broadly matched the location and trade preference of program apprentices. The program targeted three main trade groups: garment-making, hair/beauty/cosmetology, and construction. In our sample, garment-making includes both men and women, hair and beauty is nearly all women, and construction is nearly all men, both among firm owners and apprentices. In general, firms were approached directly and asked if they would be interested in hiring apprentices through the government program. Interested firms were then invited to attend one of 149 district and trade group level meetings. It was at these meetings that the research team first enrolled firms in the study, and at these meetings that firm owners participated in the baseline survey[10].

Apprentices were likewise recruited by local government officials, via advertisements publicly posted at the district office and elsewhere in town centers and via visits to churches and community meetings. The program intended to target economically disadvantaged young people, but did nothing to enforce an income requirement. Apprentices participating in the program were required to submit a formal application to the local government office and attend a short interview with local government officials (generally the district technical training coordinator, another education official, and someone from the local district assembly). About 5-10% of applicants were excluded

[9]The experiment on which we report in this paper was enclosed in a larger randomized controlled trial, which randomized over unemployed young people applying to become apprentices targeted by this government program. That randomization took place before any firms were recruited. We do not report on apprentice outcomes in this paper, though labor market impacts of apprenticeship training will be the subject of future work.

[10]The NAP was originally conceived as a subsidy which more closely mirrors the standard apprenticeship, including a bond payment at the start of the apprenticeship and a gift of a toolset to program apprentices. The bond payment was a subject of contentious negotiation between trade association leadership and COTVET during the government's program design period. At the time of this writing, no bond payments have been made and firm owners widely recognize that they are unlikely to receive a monetary benefit from the government. Toolsets for apprentices (to use and keep) have been procured and at the time of this writing are beginning to be distributed many months after apprentices began. Despite the dispute, firm owners continued to be interested in hiring through the program, and the dispute does not appear to have affected training and employment of NAP apprentices. It is possible, however, that interest in interacting formally with the government and/or hope of future government benefits or subsidies motivated, in part, firm owner interest in the program.

via this process, generally because they were believed to be a poor fit for the program (e.g. had already completed an apprenticeship, didn't actually want to do an apprenticeship but had been sent by a relative). Apprentices were later also required to attend the same district and trade group level meetings that interested firms attended.

4.2 Placement Intervention

The timeline of program activities is detailed in Figure 2. The program began in August 2012 with the recruitment of apprentices, at which time they participated in a baseline survey. There was then a long lag in the roll-out of the program as the national government agency that initiated and designed the program failed to move forward with activities or to instruct district level education officials on the same[11]. Starting in May 2013 firm recruitment and district and trade group meetings began. At these meetings firm owners were briefed on the program in more detail. In particular, conditional on geographic feasibility and apprentice willingness, apprentices would be randomly allocated. This protocol was acceptable in part because the assignment of apprentices to firms was seen by firm owners as a government benefit, so random placement allowed for arguably fair distribution of that benefit. In addition, firm owners would not have the opportunity to reject program apprentices (because the design sought to ensure a placement for every apprentice). Information on capacity constraints was also collected, though due to a relatively disperse sample across districts and trades capacity constraints were never binding (i.e. no firm owner was randomly assigned more apprentices than he or she was willing to accept). Firm owners still interested in hiring apprentices through the program then introduced themselves to the gathered group of apprentices, and stated the precise location of their businesses[12].

Apprentices, for their part, were then given the opportunity to provide a list of firms with which they would be willing and able to work and train. The instruction was to provide information on firms within their craft of interest that were close enough to their homes that they could reach

[11] Recruitment of apprentices began in August 2012, group meetings took place in mid-2013, and program placement did not begin until October 2013. In general, logistical challenges on the part of the implementing government partners led to significant delays in all districts, and the start of apprenticeships in three phases. 21 districts, 657 apprentices, and 684 firms made up Phase 1, starting training in October and November 2013; 7 districts, 388 apprentices, and 280 firms made up Phase 2, starting training in December 2013 and January 2014; and 4 districts, 152 apprentices, and 123 firms made up Phase 3, starting training in February and March 2014. Phase 3 apprentices and firms were excluded from the first January 2014 follow-up survey.

[12] The formal meeting activities were heavily monitored, though unmonitored communication between participants was also common.

them without incurring large transport costs. However, detailed GPS or other information on firm location and apprentice home location was not available at the time so district officials and research field teams had no ability to enforce that instruction. Consequently, the apprentice-specific firm sets include both geographic feasibility (walkability, generally) and idiosyncratic preference. No minimum or maximum was placed on the number of firms listed and apprentices who listed only one firm were assigned that firm. However, the majority of apprentices listed at least two firms, with a mean of 2.2 firms. Anecdotally, we believe the firm sets to be an honest revelation of preferences, where apprentices who listed multiple firms were willing to work at all of the listed firms.

The application process, including the formal application, interview, attendance at group meetings, and the long lag in program roll-out function empirically as the non-monetary screening emphasized in the conceptual framework. In general it required a non-trivial investment of time and energy from potential apprentices.

4.3 Data

Data come from four sources: (1) firm baseline surveys, (2) apprentice baseline surveys, (3) apprentice-specific firm sets, and (4) two firm-level follow-up surveys conducted at approximately 3 and 6 months after the start of employment. 1,070 of 1,083 sample firms participated in a baseline survey which included personal background, digit span recall, four math questions, capital stock, detailed labor inputs, revenues and profits, managerial aptitude questions, and information on apprenticeship training experiences. 1,136 of 1,168 sample apprentices participated in a baseline survey which included education, training and work background, and a series of cognitive tests, including digit span recall, four math questions, Ravens matrices group B, and a fifteen word oral English vocabulary definition/recognition test. 1,062 of 1,083 sample firms participated in one or both of the follow-up surveys, with no differential survey attrition by treatment assignment. Follow-up surveys included revenues, profits, detail on program apprentices, and updates on non-program apprentices labor inputs. The second follow-up also included updated capital stock measures. The use of two follow-ups was intended to increase power for the key outcome variables, as profits and sales for microenterprises are both extremely noisy and have relatively low auto-correlation over time (McKenzie (2012)).

All survey questions and strategies were extensively piloted. Because Ghana has eleven government-

sponsored languages and the sample spans 32 districts and all 10 regions, the surveys were printed in English and translated on the spot. Surveyors had with them simple dictionaries developed specifically to assist in the correct translation of important questions/words. Following De Mel, McKenzie and Woodruff (2009*a*), the revenues and profits questions in each firm survey were as follows:

"What were the TOTAL SALES from your business LAST MONTH?"

"What was the total INCOME the business earned LAST MONTH after paying all expenses including wages of employees, but not including any INCOME you paid yourself. That is, what were the PROFITS of your business LAST MONTH?"

Labor inputs in the firm baseline were captured by category ("master" worker, apprentice, unpaid worker), and included detail on the sex, age, hours, wages, and training experience of each worker. Capital stock data was collected in seven categories: land, building(s), furniture, machinery and equipment, tools, inventory, and any other assets, only the last five of which were included in the second follow-up. Craft-specific pictorial aids were used to assist survey respondents in including capital stock by category.

Apprentice cognitive tests include the Ravens matrices group B, a commonly used measure of abstract cognitive ability. It is a series of 12 patterns, each with a missing piece. The respondent chooses from six options which piece fits the pattern for each of the 12 patterns. The Digit Span Recall test is essentially a memory test, in which surveyor read out number and respondents repeat the numbers. The number of digits increases over time so that later questions are more difficult than earlier ones. The oral English vocabulary test includes fifteen English words and possible synonyms for those words, and asks respondents to choose the synonym. We created the math test ourselves via survey piloting, and it consists of four word problems that require critical thinking and the use of simple arithmetic.

Seasonal variation in economic activity at these firms is important. The firm baseline surveys were completed between May and November 2013, with all surveys within a district completed around the same time. The first follow-up survey was completed in January 2014, and thus refers to revenues and profits from December, the heaviest month for both garment-makers and beauticians,

particularly in the Christian south of Ghana. The second follow-up survey was completed in April 2014 and refers to economic activity from March 2014. It is important to note that Ghana suffered from high rates of inflation over the course of the study. At present all specifications include nominal Ghana Cedis.

4.4 Randomization

Randomization was done on the individual apprentice level. Given the firm set of each apprentice, a random firm was chosen using a computer generated random number. No re-randomization or stratification beyond individual apprentice was done, and each randomization was independent. If the apprentice only listed a single firm as both geographically feasible and desirable generally, he or she was assigned to that firm.

Consequently, our identifying exogenous variation is conditional on non-random apprentice interest in each firm, and generates a multi-valued treatment assignment. Specifically, because each apprentice-specific randomization is independent, the probability distribution function for the treatment value of a given firm is conditional both on the number of apprentices who listed that firm and the number of other firms each of those apprentices listed.

As an example, consider a district and trade in which there is only a single apprentice. Suppose that apprentice listed three firms. In this case, each of the three firms would be in our sample and the apprentice would have a 1/3 probability of being assigned to any of the three firms. The randomization would assign the apprentice to one of the three firms, which would become the treatment firm and the remaining two would become control firms. Each of the three firms would have a 1/3 probability of being assigned one apprentice, a 2/3 probability of being assigned no apprentices, and zero probability of two or more. And each of the three firms could be compared to each other as members of the same lottery.

Most districts and trades, however, had more than one apprentice. Suppose, for example, there are two apprentices (and still three firms). The first apprentice lists each of the three firms as before, but now the second apprentice lists two of the three. Now the first firm has a 1/3 chance of being assigned one apprentice, a 2/3 probability of being assigned no apprentices, and zero probability of two or more. However, the second and third firms have a (2/3*1/2) + (1/3*1/2) = 1/2 chance of being assigned one apprentice, a (2/3*1/2) = 1/3 chance of being assigned zero apprentices, a

(1/3*1/2) =1/6 chance of being assigned two apprentices, and zero probability of three or more. Now the second and third firms retain the same probability of each treatment assignment and remain in the same lottery, but can no longer be strictly compared to the first firm.

In practice, though there are many more than one or two apprentices in each district and trade, relatively small numbers like this were common because of the geographic dispersion of the sample. The randomization resulted in firm treatment assignment taking values between zero and six apprentices.[13] Figure 3 shows the distribution of treatment assignment by firm, underlining the fact that the vast majority of firms were assigned zero, one, or two apprentices.

In order to control for differences across firms in apprentice interest and for different probability distributions of the treatment value, we execute a fixed effects specification akin to strata or school-choice lottery fixed effects. Our main estimation strategy includes these lottery fixed effects (φ_l) within which each firm faces an equal probability of being assigned each of the multi-valued treatment assignments.

An alternative way of articulating the same, is to recognize that we implement an approximation to an exact propensity score match across a multi-valued treatment assignment. First, note that the randomization ensures that:

$$Y(t) \perp T \mid X \text{ for all } t \in T$$

where X is the full set of all apprentice-specific firm sets, T is the number of apprentices assigned to the firm, and Y is the potential outcome associated with the treatment value. Following

Hirano and Imbens (2004), it follows that:

$$Y(t) \perp T \mid f_{T|X}(t \mid x)$$

This states that the potential outcomes are independent of the treatment assignment conditional on the lottery fixed effects. One important consideration is that the lottery fixed effects cut the data quite thin (we have over 200 unique lottery fixed effects for a sample of 1,083 firms). We note, however, that over half of the firms in the sample fall into one of the 15 most common lottery fixed effects. These most common lottery fixed effects include firms listed by relatively few apprentices, usually one, two, or three. Our findings are qualitatively robust to controlling for the randomization

[13]Four firms of 1,087 were assigned seven or eight apprentices because of unusual circumstances in the particular neighborhoods where those firms reside. No other firms share their Lottery Fixed Effect, so they would not contribute to the estimation strategy discussed below. Consequently, they have been dropped from the analysis.

in other ways. We will display OLS in the main tables, but the findings are also robust to instead controlling for moments of the probability distribution of the treatment assignment, or probabilities of each treatment assignment (similar to propensity score regression adjustment). We considered these more parameterized alternative specifications, but prefer lottery fixed effects as they control directly for the probability of treatment.

4.5 Estimation

We have three main outcome groups of interest: (1) labor inputs and firm size, (2) revenues and profits, and (2) complementary other inputs.[14] Following McKenzie (2012), our main specification stacks data from the two follow-up rounds, controls for the baseline value of the outcome variable, and includes a follow-up round 2 fixed effect (η_2), as follows:

$$Y_{it} = \alpha + \beta T_i + \gamma Y_{i0} + \eta_2 + \varphi_l + \epsilon_{it} \tag{1}$$

The coefficient β estimates the Intent-to-Treat effect and is identified from within-round, within lottery variation. β can be interpreted as the average effect of each assigned apprentice across follow-up rounds, where the effect of each apprentice enters the function linearly. Standard errors are clustered at the district level.

To measure treatment effects across rounds, we estimate:

$$Y_{it} = \alpha + \beta_1 T_i * \eta 1 + \beta_2 T_i * \eta 2 + \gamma Y_{i0} + \eta_2 + \varphi_l + \epsilon_{it} \tag{2}$$

In additional specifications, we interact treatment assignment with baseline characteristics of the firm (gender of the firm owner, firm trade, baseline firm size) to explore heterogeneous treatment effects. We also run Local Average Treatment Effect specifications, instrumenting for firm size with treatment assignment.

[14]This project was registered with the American Economics Association Randomized Controlled Trial Registry, complete with a Pre-Analysis Plan (PAP). The PAP was intended to coalesce ideas on the direction of analysis, and limit both the risks and perception of data mining or specification search. The estimation procedures described in the PAP did not properly control for non-random apprentice interest and were thus abandoned. The main hypotheses, however, laid out in the PAP, are tested in this paper. These include that treatment firms would increase in size, that treatment firms would see increases in revenues and profits associated with apprentice labor inputs, and that apprentice cognitive ability would matter for these outcomes.

Finally, we define treatment assignments $T_{abovemedian}$ and $T_{belowmedian}$ partitioned from the total value of T_i, where $T_{abovemedian}$ is the number of apprentices assigned who performed better than the median among all program apprentices on the noted cognitive test and $T_{belowmedian}$ is the number of apprentices assigned who did not. In the education tables, T_{JSS} is apprentices who finished Junior Secondary School and T_{noJSS} is those who did not. We then run variations of the following as our main specification in the ability analysis:

$$Y_{it} = \alpha + \beta T_{abovemedian-i} + \gamma Y_{i0} + \eta_2 + \varphi_{abovemedian-l} + \epsilon_{it} \tag{3}$$

The randomization over these partitioned treatment assignments is exactly the larger randomization, but again is conditional on non-random apprentice interest. Consequently, the firm samples in each of the partitioned experiments differ slightly, as we discuss below. Partitioned treatment assignment specific lottery fixed effects (e.g. $\varphi_{abovemedian-l}$) are also generated.

4.6 Summary Statistics

In our nationwide sample of 1,083 small firms, apprentices comprise the vast majority of the workforce. In the 962 firms who have any workers besides the owner at baseline, 80% of the 3,695 workers are apprentices. 46% of the workforce was previously unknown to the firm owner, underlying that modern apprenticeship is largely an anonymous market activity. The mean monthly wage for an apprentice during his/her first year of work in our baseline sample is eighteen Ghana Cedis, which at the time of baseline surveys was about nine US dollars.

Column 1 of Table 1 displays the summary statistics for a range of other variables at baseline. We see that garment-makers are the most common trade, that we have more female firm owners than male firm owners in the sample, and that only about 7% of the sample is registered with the Registrar General (to pay taxes). Our sample firms are slightly larger than those in many studies of microenterprises, though broadly still classified as small firms or microenterprises in the terminology of the discipline.

4.7 Balance Along Observables

Columns 2 through 11 of Table 1 test for raw balance along observable firm characteristics across the most common treatment assignments. Control are firms assigned zero apprentices, T=1 are firms assigned one apprentice, T=2 are firms assigned two apprentices, and T=3 are firms assigned three apprentices. Columns labeled mean give the mean value for each of these groups, in order. Columns 4-5, 7-8, and 10-11 show the difference between the mean in the control group and the three most common treatment groups (one apprentice, two apprentices, three apprentices), with the corresponding p-value on the test of equality.

The reader will notice that several variables reveal imbalance across individual treatment assignment groups in the raw data which does not control for non-random apprentice interest. In particular, baseline firm size is unbalanced without lottery fixed effects controls. This reveals that firms with larger baseline firm size received more apprentice interest and consequently, on average, a higher treatment assignment.

Next we test whether this imbalance across treatment and control groups with respect to random treatment assignment persists when we control for non-random apprentice interest. We regress firm baseline characteristics on treatment assignment, controlling for lottery fixed effects to confirm that treatment does not predict baseline characteristics. Each cell in Table 2 comes from a separate regression of the following form:

$$Baseline_i = \alpha + \beta T_i + \varphi_l + \epsilon_i \tag{4}$$

with lottery fixed effects (φ_d). What we would want to see in this table is that each coefficient is precisely and exactly zero. Though the point estimates are not exactly zero, we note that only one is significant, implying that imbalance across baseline firm characteristics nearly disappears when we control for lottery fixed effects[15]. Accordingly, the randomization procedure achieved conditional balance across treatment assignments.

[15]Note that regressions of this form that exclude lottery fixed effects do produce significant coefficient estimates.

5 Results

5.1 Take Up and Other Production Inputs

Take-up requires both that the firm owner accept to train and employ apprentices and that apprentices report to their employment assignments. To our knowledge, only one firm in the study refused to train and employ the apprentice(s) assigned to their firm. Of the 1,168 apprentices assigned training and employment via the random match process, 767 (66%) reported to their assigned firm, 77 (6%) reported to a firm in the study other than their assigned firm, 305 (26%) did not report to any firm in the study, and 19 (2%) were not confirmed as their assigned firms attrited from the study.

Table 3 shows the results of estimating a standard OLS specification as well as Equations 1 and 2 on treatment assignment. Without lottery fixed effects, each additional assigned apprentices increases firm size by about .8 workers. Some of this effect is driven by the fact that apprentices preferred larger firms. Estimating the same using the lottery fixed effects, we find that each assigned apprentice increases firm size by about .5 workers. The median firm had 4 people (including the owner) at baseline, so half a worker increases the size of the firm about 10%. Figure 4 displays this result graphically.

In Appendix Table 2, we investigate the impact of the treatment program on other production inputs, including capital stock, firm owner hours worked, and reported hours of instruction given by the firm owner to apprentices. Note that capital stock and instruction hours were only captured in Round 2 of data collection. We show no complementary investment in other inputs. One possible explanation is credit constraints, such that though firms may prefer to invest in additional capital, they are unable to do so. Another possible explanation is that firm owners may incorporate the largely temporary nature of apprentice labor inputs, as the majority of apprentices outside this program leave the firm rather than graduate to master workers within the firm (at the time of this writing, program apprentices are still working and training in their assigned firms).

5.2 Treatment Effects on Revenues and Profits

Our second main set of results is presented in tables 4, 5, A3 and A4. The ITT effect of each treatment apprentice is an increase in revenues of seven percent and a Treatment on the Treated

(TOT)/Local Average Treatment Effect (LATE) of twelve percent. Profits increase in similar magnitudes, with an ITT estimate of eleven percent and a TOT estimate of eighteen percent. Tables 4 and 5 also show that raw revenues and profits estimates are qualitatively similar but insignificant. In Appendix tables A3 and A4, we investigate whether these power differences are driven by functional form or outliers in our revenues and profits data by running quantile regressions on both raw revenues and profits and log revenues and profits. We find that, while both may be important, outliers are the most likely cause of the loss of power in regressions on raw data.

This finding shows that profitable employment relationships were created by the search and screening intervention that did not occur in the absence of intervention. Mean monthly wages paid to program apprentices in Round 1 are 23 GHC, with a median of 15 GHC. In Round 2, this number increased to a mean of 25 GHC and a median of 20 GHC, likely due to both increasing worker productivity and inflation over the months between follow-up rounds. These numbers are broadly consistent with total additional revenues less wages and other expenses equaling total additional profits. The profit effects are essentially a lower bound to the fixed cost of search, and interpreted as such, are extremely high.

These findings also independently imply that unemployed young people with relatively little formal schooling and limited skills have a positive and quite large marginal revenue product, which is a meaningful finding in itself.

5.3 Heterogeneity in Treatment Effects

Table 6 explores heterogeneous treatment effects by firm characteristics at baseline. We find no evidence that treatment varies by trade group. Though not significant, point estimates suggest that female-owned firms in garment-making (the only major trade with large numbers of both men and women) benefit far less than male-owned firms in the same trade for each assigned program apprentice. This finding relates to De Mel, McKenzie and Woodruff (2009*b*) which finds that female-owned small firms benefit less from infusions of capital. Fafchamps et al. (2013) finds that female-owned firms benefit more from in-kind capital grants than cash, suggesting that the liquid nature of cash could be vulnerable to leakage into the household and/or greater levels of impatience among female owners. Labor is essentially an in-kind production input and therefore should not be subject to leakage into the household or into consumption. Our finding on heterogeneity in returns

by firm owner gender is not significant however, and thus only contributes suggestive evidence to the puzzle of productivity differences between male and female-owned firms.

The most robust heterogeneity result is that the smaller firms at baseline benefit most from the treatment. Appendix Table 5 attempts to explore this finding further. Though consistent with simple decreasing returns to additional labor inputs, these robust findings also suggest that the smallest subsistence firms may represent a special case.

6 Worker Ability

Next we turn to an attempt to characterize the nature of the labor market friction identified in Section 5. The coincidence of high youth unemployment and evidence of high search costs is puzzling at first glance. We present evidence that worker ability impacts the marginal revenue product of labor in our setting, and that missing signals of ability may make it difficult for firm-owners to screen directly. In particular, revenues and profit effects respond to cognitive ability as measured by the researcher but not to cognitive ability as measured by educational outcomes. These findings underlie the difficulty many small firm owners have identifying apprentices who can add to the profits of their firms.

6.1 Cognitive Ability

Econometrically, this analysis separates the treatment into two sub-experiments. We use the same manner to control for randomization and non-random apprentice interest as we do in the main analysis; however, in this case, we split the apprentices into two groups. So one experiment takes all apprentices who are above median performance on a particular cognitive test and another experiment takes all apprentices who are below median performance on the same test. We then estimate equation 3 on firms who received interest from both above median and below median apprentices. This limits our sample but ensures that we are studying firms of a similar type and that differences are not driven by unobservable differences across firms. In addition, new lotteries apply to the above median and below median experiments and require us to estimate them separately.

Panel A of Table 7 shows the results of estimating equation 3 for above median ability treatment, as measured using the Digit Span Recall Test. We see that, even in this small sample, higher ability

apprentices have quite large ITT effects on revenues and profits. Panel B of Table 7 shows the results of estimating equation 3 for below median ability treatment, as measured by performance on the Digit Span Recall Test. Here, the point estimates are slightly negative and not significant. Though the difference between above median and below median point estimates is not significant, these findings are nonetheless striking in this small sample of overlapping firms.

The pattern persists in our other measures of cognitive ability. Appendix Tables 5 through 8 show similar estimates for sub-experiments using the Ravens test, the math test, and the English vocabulary test. Though these measures are correlated, their relationship is very far from perfectly overlapping. As four independent measures of cognitive ability, each adds to the argument that high cognitive ability apprentices generate larger treatment effects on revenues and profits. Firms treated with apprentices who scored above the median (in our apprentice sample) on each of our cognitive tests experience much higher revenue and profit effects. Firms treated with apprentices who scored below the median are near break-even.

These findings are robust to running the specifications on the full sample in each sub-experiment (rather than restricting the sample to overlapping firms), in which case we have similar point estimates and more statistical significance. In addition, alternative specifications that control for non-random apprentice interest less rigorously and include treatment variables for both above median and below median cognitive ability apprentices have qualitatively similar findings.

6.2 Education

Tables 8 tells a different story. Panel A of Table 8 estimates equation 3 for apprentices who completed Junior Secondary School (the end of free and compulsory education in Ghana). Panel B of Table 8 estimates equation 3 for apprentices who did not complete Junior Secondary School. Here we see that again coefficients on the two experimental treatments are not significantly different. However, if anything the evidence suggests that less well educated apprentices benefit firm outcomes more. This is despite the fact that education is positively correlated with each of our four measures of cognitive ability.

The fact that we find no evidence that more years of schooling predict treatment effects on revenues and profits underlies the lack of useful signals of ability available to hiring firms.

7 Conclusion

Previous models of small firms in developing countries have largely assumed they face a frictionless market for workers. The justification for modeling firms in this way comes primarily from the idea that larger firms are subject to more stringent regulations and wage premiums and therefore face much higher hiring costs. This line of thinking, however, misses the fact that large firms have the ability and capacity to put significant resources into recruitment and screening of potential workers. Consequently, they have access to both a larger pool and a more complex mechanism by which to screen workers. Small firms, on the other hand, while they may have more private information about local young people, have very limited ability and resources to devote to complicated screening on ability, motivation, and other potentially productivity-enhancing worker characteristics.

This paper argues that small firms in Ghana face high labor market search costs, and in particular that screening over ability is both difficult and costly. Using the results from a field experiment which randomly gave firms access to worker recruitment services, we show that small firms offered workers through the program chose to hire them. Further, control firms not offered workers through the program failed to hire workers through other means by six months after the program began.

In addition, we show that the marginal revenue product of labor (even when that labor is unemployed young people not productively employed elsewhere) is positive and quite large. It appears that there is substantial room for small firms to grow in terms of employment and retain profitability. This finding is important because it stands in contrast to an oft-cited argument in development economics that small firms are low-productivity subsistence enterprises.

Finally, we present evidence that cognitive ability matters in the degree to which workers contribute to firm revenues and profits. Understanding how worker characteristics interact with productivity is of broad interest in economics, and meaningful in our context because it argues that there is substantial (largely unobservable) heterogeneity in the pool of unemployed young people in Sub-Saharan Africa. Signals that are available to firms (both large and small) do not appear to affect productivity as we might expect.

More work remains to be done to better understand small firms and labor markets in developing countries. This paper attempts to test of the type of labor market friction constraining employment in small firms, but its limitations leave further empirical tests as future work. In addition, the

findings in this paper suggest that labor market institutions in Ghana in this portion of the labor market are either missing or poorly functioning. Studying these institutions and policy options to address their failings is an important research agenda going forward.

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Tables and Figures

Figure 1: **Sample Districts.** The map highlights the 32 sample districts included in the study, which include Kumasi Metropolitan and Accra Metropolitan, the two largest urban centers. The sample also includes many very rural (and poor) districts. The government program was slated to take place in about half of the districts in Ghana, and the evaluation districts are a random subset of those.

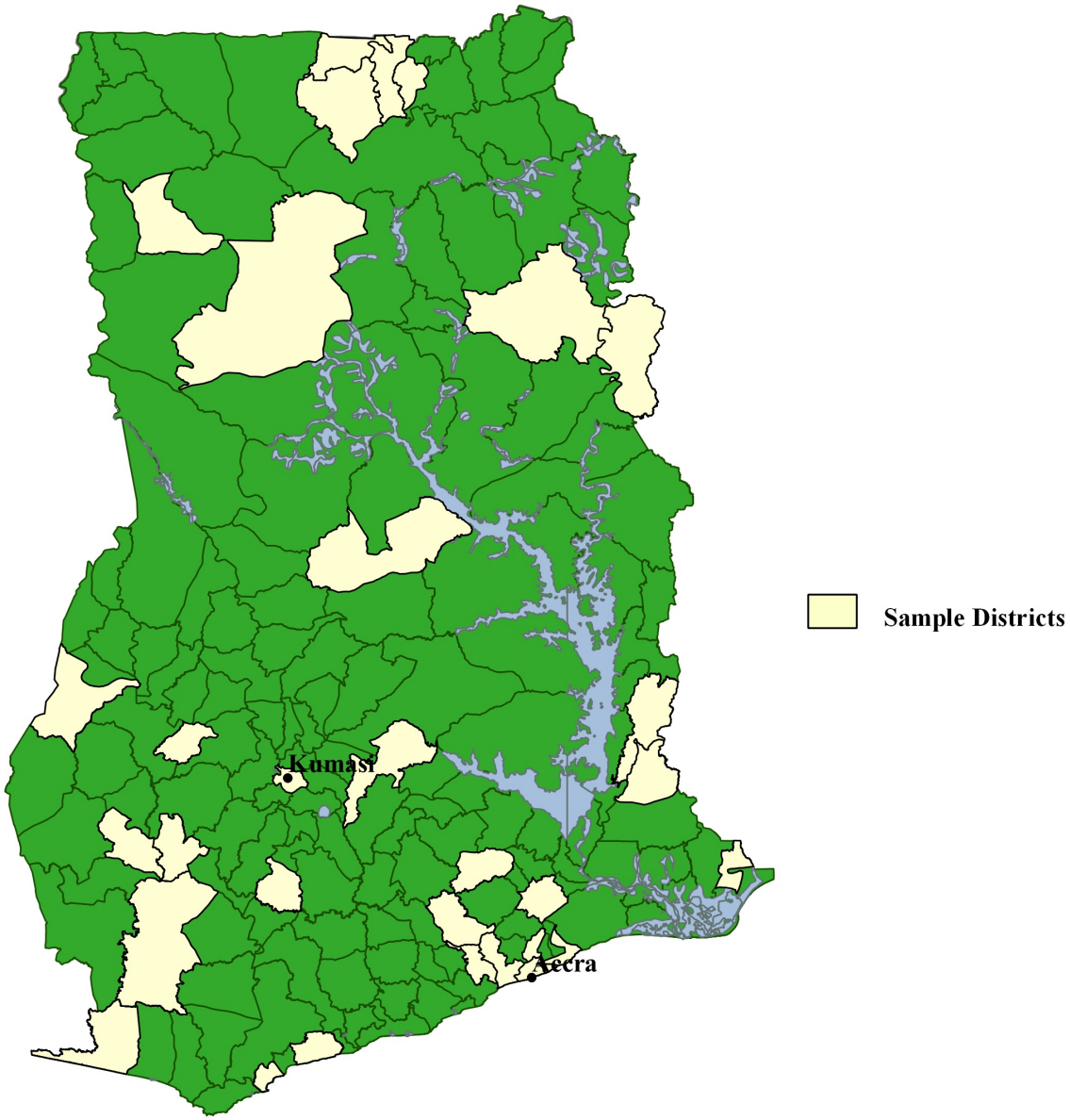


Figure 2: **Project Timeline.** The project timeline below describes the order of apprentice recruitment, firm recruitment, and data collection. Randomization took place just before placement, which signifies that apprentices received instruction to report to their assigned firms and firms received information about who they were assigned, if anyone.

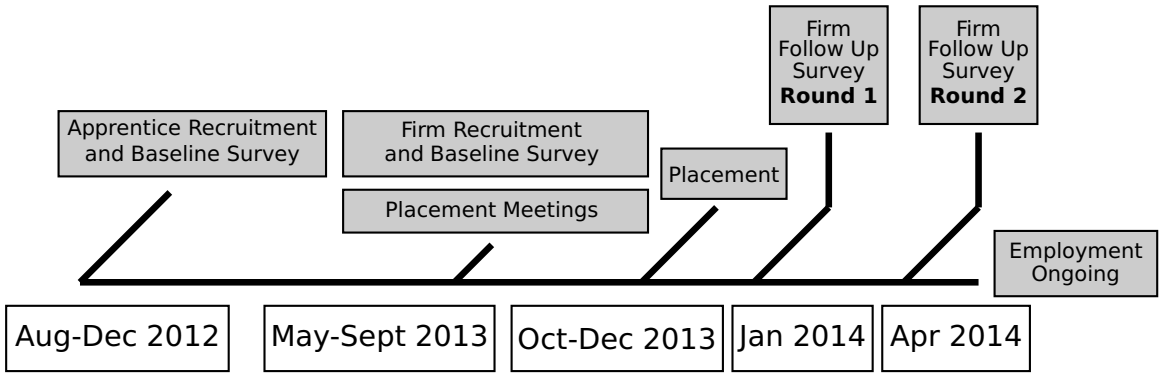


Figure 3: **Distribution of Treatment Assignments.** The vast majority of our sample firms were assigned zero, one, or two apprentices via the randomization. These numbers are a function of the lottery and the relatively small numbers of apprentices interested in each sample firm. Note that firms assigned larger numbers of workers were also listed by more interested apprentices, though these differences are controlled for by including lottery fixed effects.

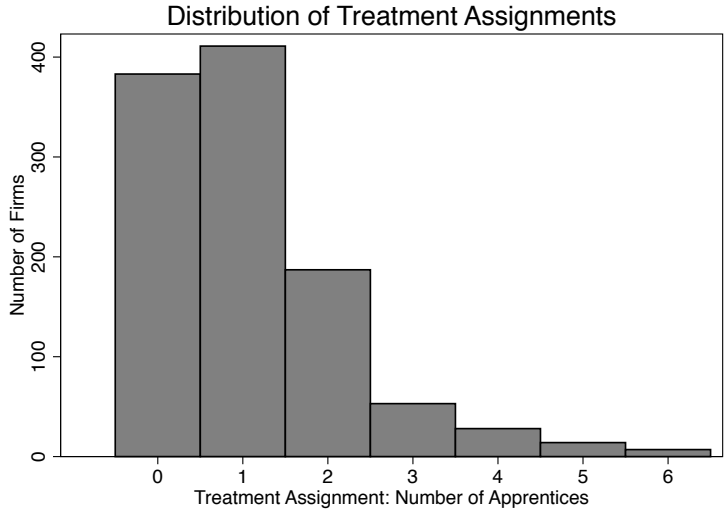


Figure 4: **Firm Size And Labor Market Constraints.** This figure plots raw firm size (including the firm owner) at baseline (time 0), first follow-up (time 1), and second follow-up (time 2). First follow-up took place approximately three months after placement, and second follow-up approximately six months after placement. Note that these raw data do not control for lottery fixed effects, which is why we observe imbalance in firm size at time 0 in this figure. The figure shows two striking patterns: (1) control firms fail to hire outside the program, and (2) firm size increases roughly in proportion to treatment.

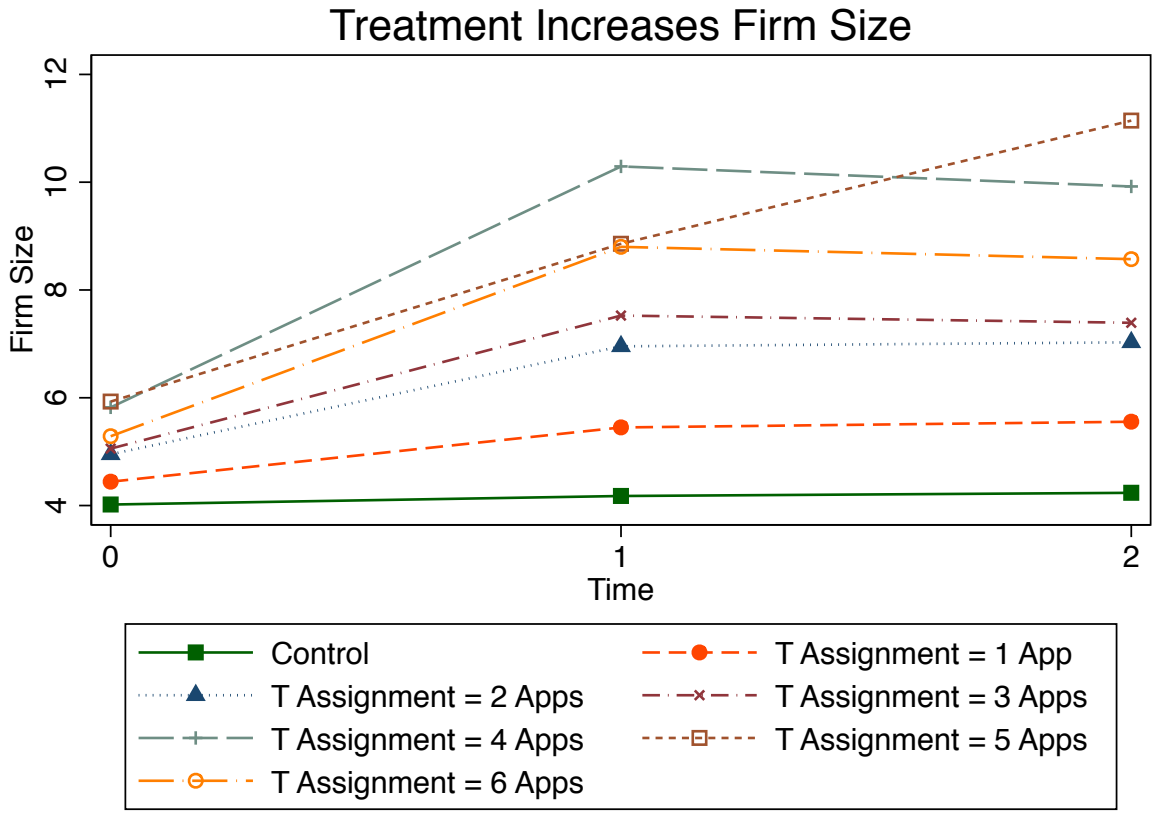


Table 1: **Summary statistics and raw covariate balance.** Columns labeled mean give the mean value for all firms in our sample, control firms, firms assigned one apprentice, firms assigned two apprentices, and firms assigned three apprentices, in that order. Columns 4-5, 7-8, and 10-11 show the difference between the mean in the control group and the three most common treatment groups (one apprentice, two apprentices, three apprentices), with the corresponding p-value on the test of equality.

	All Firms	Control	T=1	C-T1		T=2	C-T2		T=3	C-T3	
	mean	*mean*	*mean*	*diff*	*p-val*	*mean*	*diff*	*p-val*	*mean*	*diff*	*p-val*
Female Owner	0.66	0.69	0.63	0.06	0.08	0.65	0.04	0.34	0.83	-0.14	0.03
Garment Makers	0.42	0.44	0.41	0.03	0.32	0.39	0.05	0.25	0.45	-0.01	0.87
Hairdressers and Beauticians	0.33	0.34	0.31	0.03	0.32	0.35	-0.01	0.75	0.38	-0.04	0.59
Construction	0.25	0.22	0.29	-0.07	0.03	0.26	-0.04	0.32	0.17	0.05	0.41
Firm Size	4.47	4.02	4.44	-0.42	0.03	4.95	-0.93	0.00	5.06	-1.04	0.01
Has any worker(s) besides owner	0.89	0.86	0.90	-0.04	0.07	0.90	-0.04	0.13	0.91	-0.05	0.35
Paid Workers	0.53	0.55	0.51	0.04	0.63	0.56	-0.02	0.88	0.42	0.13	0.43
Apprentices	2.77	2.33	2.74	-0.40	0.02	3.19	-0.86	0.00	3.51	-1.18	0.00
Unpaid Workers	0.16	0.13	0.20	-0.07	0.13	0.16	-0.03	0.53	0.13	-0.00	0.99
Proportion of workforce is family	0.15	0.16	0.15	0.01	0.48	0.15	0.02	0.50	0.07	0.09	0.03
Revenues (nominal GHC)	717	626	700	-75	0.31	868	-243	0.03	530	95	0.53
Profits (nominal GHC)	337	298	354	-56	0.20	399	-101	0.07	285	13	0.87
Assets (nominal GHC)	7181	7822	7002	820	0.31	6178	1644	0.15	6549	1273	0.46
Assets excl building (nominal GHC)	4223	4450	3979	471	0.35	4256	194	0.81	3144	1307	0.20
Firm Age	11.5	12.07	11.56	0.51	0.35	10.59	1.47	0.02	11.04	1.03	0.34
Bank Account	0.67	0.64	0.67	-0.03	0.33	0.71	-0.07	0.11	0.68	-0.04	0.59
Electricity connection	0.87	0.91	0.86	0.05	0.05	0.84	0.07	0.02	0.84	0.06	0.15
Registered w/district assembly	0.34	0.34	0.34	0.00	0.96	0.37	-0.04	0.41	0.36	-0.02	0.77
Registered w/registrar general	0.07	0.08	0.08	0.00	0.88	0.05	0.03	0.14	0.04	0.05	0.25
Management Practices (of 5)	2.45	2.56	2.42	0.15	0.20	2.40	0.17	0.24	2.60	-0.04	0.87
Owner years schooling	8.95	9.03	9.14	-0.11	0.65	8.41	0.63	0.04	8.96	0.07	0.88
Owner digits span recall (of 14)	6.92	6.96	7.14	-0.18	0.31	6.62	0.34	0.13	6.34	0.62	0.09
Owner math correct (of 4)	2.62	2.61	2.63	-0.02	0.78	2.56	0.05	0.57	2.74	-0.13	0.36
Number of Firms	1083	383	411			187			53		

Table 2: **Covariate Balance with Lottery Fixed Effects.** In this table we test for balance in covariates across treatment groups, controlling for lottery fixed effects. Each coefficient is from a separate regression of the number value of the treatment assignment (zero, one, two, etc.) on the baseline firm-level covariate. Note that though not all point estimates are exactly zero, only one is statistically significant, and based on the means values in Table 1, none are economically significant. This suggests that the conditional on lottery fixed effects, the randomization resulted in balance along observed covariates across treatment groups.

	(1) Female Owner	(2) Garment Makers	(3) Hairdressers & Beauticians	(4) Construction	(5) Firm size
Treatment Assignment	0.02 (0.03)	0.00 (0.03)	0.01 (0.03)	-0.01 (0.03)	0.06 (0.16)
Observations	1083	1083	1083	1083	1067
R^2	0.296	0.308	0.305	0.307	0.329
Lottery FEs	YES	YES	YES	YES	YES
	(6) Has any worker(s)	(7) Paid workers	(8) Apprentices	(9) Unpaid workers	(10) Prop of workers are family
Treatment Assignment	0.00 (0.02)	0.04 (0.07)	-0.04 (0.15)	0.06* (0.04)	-0.01 (0.02)
Observations	1083	1067	1070	1067	945
R^2	0.221	0.204	0.360	0.182	0.261
Lottery FEs	YES	YES	YES	YES	YES
	(11) Revenues (nom GHC)	(12) Profits (nom GHC)	(13) Assets (nom GHC)	(14) Assets excl build (nom GHC)	(15) Firm age
Treatment Assignment	-2.45 (80.81)	30.08 (37.95)	-681.34 (725.73)	-166.07 (504.79)	0.18 (0.43)
Observations	1061	1062	1070	1070	1068
R^2	0.336	0.233	0.230	0.238	0.288
Lottery FEs	YES	YES	YES	YES	YES
	(16) Bank account	(17) Electricity connection	(18) Reg w/ dist assemb	(19) Reg w/ reg general	(20) Mgmt Practices (of 5)
Treatment Assignment	0.00 (0.03)	-0.02 (0.02)	0.03 (0.03)	-0.02 (0.02)	0.07 (0.09)
Observations	1068	1010	1068	1067	1059
R^2	0.248	0.234	0.264	0.289	0.300
Lottery FEs	YES	YES	YES	YES	YES
	(21) Owner yrs schooling	(22) Digits span recall (of 14)	(23) Math correct (of 4)		
Treatment Assignment	0.21 (0.20)	0.06 (0.15)	0.04 (0.05)		
Observations	1067	1069	1066		
R^2	0.308	0.248	0.250		
Lottery FEs	YES	YES	YES		

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: **Take-Up.** Regressions include round fixed effects, lottery fixed effects, and baseline values of the dependent variable where applicable, with errors clustered at the district level. Controlling for lottery fixed effects, about half a program apprentice is found to be working at follow-up for each assigned program apprentice. The point estimate on total firm size is also about half a worker, implying that control firms did not grow without the program and treatment firms did not fire existing workers. Program apprentices work around forty hours per week, and accordingly we see total hours worked in sample firms increasing by about 20 hours for each assigned apprentice.

	Take Up: Program Apps Working			Firm Size: Total Number of Workers			Firm Size: Total Number of Hours		
	(1) OLS	(2) FE: Pooled	(3) FE: By round	(4) OLS	(5) FE: Pooled	(6) FE: By round	(7) OLS	(8) FE: Pooled	(9) FE: By round
Treatment Assignment	0.76***	0.47***		0.82***	0.48***		41.65***	22.42***	
	(0.05)	(0.05)		(0.07)	(0.12)		(3.91)	(7.28)	
Treatment Assignment - Round 1			0.42***			0.45***			21.40**
			(0.05)			(0.13)			(7.82)
Treatment Assignment - Round 2			0.51***			0.52***			23.38***
			(0.05)			(0.13)			(7.76)
Number of Firms	1051	1051	1051	1051	1051	1051	1051	1051	1051
Total Observations	1879	1879	1879	1877	1877	1877	1876	1876	1876
Mean of Dep Variable	0.81	0.81	0.81	5.54	5.54	5.54	282.17	282.17	282.17
R squared	0.59	0.80	0.80	0.59	0.71	0.71	0.60	0.70	0.70
Lottery FEs	NO	YES	YES	NO	YES	YES	NO	YES	YES

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: **Treatment Effects on Revenues.** Revenues here are self-reports of all sales in the reported month. All regressions control for baseline values of the dependent variable. All FE regressions include lottery fixed effects. Columns 1-4 include round fixed effects. Errors are clustered at the district level. Intention to Treat (ITT) effects reported in Panel A-Column 2 show increases in revenues of about seven percent per assigned apprentice across two rounds of follow-up data. Local Average Treatment Effect estimates reported in Panel A-Column 4 show increases in revenues of twelve percent per apprentice. See Appendix Table 3 for quantile regressions reported at the median. Level regressions with revenues trimmed at 5% are also significant (not reported). Together, these suggest that level specifications in Panel B are not significant due primarily to outliers in the data (as opposed to the shape of the relationship). Note that levels are nominal Ghana Cedis (at first follow-up in January 2014, one US dollar was equivalent to 2.35 GHC).

	Log Revenues					
	(1) OLS	(2) FE: Pooled	(3) FE: By round	(4) FE IV: Pooled	(5) FE IV: Round 1	(6) FE IV: Round 2
Treatment Assignment	0.05**	0.07***		0.12**		
	(0.02)	(0.03)		(0.05)		
Treatment Assignment - Round 1			0.04		0.08	
			(0.03)		(0.07)	
Treatment Assignment - Round 2			0.10***			0.15**
			(0.03)			(0.07)
Number of Firms	1018	1018	1018	1018	846	922
Total Observations	1768	1768	1768	1768	846	922
Mean of Dep Variable	6.17	6.17	6.17	6.17	6.33	6.02
R squared	0.40	0.49	0.49	0.49	0.57	0.51
First Stage F Stat				18.72	6.14	9.74
Lottery FEs	NO	YES	YES	YES	YES	YES
District FEs	YES	NO	NO	NO	NO	NO
	Revenues Per Month (Nominal GHC)					
	(1) OLS	(2) FE: Pooled	(3) FE: By round	(4) FE IV: Pooled	(5) FE IV: Round 1	(6) FE IV: Round 2
Treatment Assignment	15.09	71.05		117.24		
	(43.30)	(78.22)		(117.30)		
Treatment Assignment - Round 1			54.92		144.21	
			(97.44)		(245.38)	
Treatment Assignment - Round 2			86.49			95.59
			(67.98)			(59.42)
Number of Firms	1034	1034	1034	1034	875	948
Total Observations	1823	1823	1823	1823	875	948
Mean of Dep Variable	875.75	875.75	875.75	875.75	1037.62	726.35
R squared	0.11	0.24	0.24	0.27	0.34	0.26
First Stage F Stat				18.72	7.09	10.03
Lottery FEs	NO	YES	YES	YES	YES	YES
District FEs	YES	NO	NO	NO	NO	NO

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: **Treatment Effects on Profits.** Profits here are self-reports of all sales less all expenses (including the wage bill) in the reported month. All regressions control for baseline values of the dependent variable. All FE regressions include lottery fixed effects. Columns 1-4 include round fixed effects. Errors are clustered at the district level. Intention to Treat (ITT) effects reported in Panel A-Column 2 show increases in profits of about eleven percent per assigned apprentice across two rounds of follow-up data. Local Average Treatment Effect estimates reported in Panel A-Column 4 show increases in profits of eighteen percent per apprentice. See Appendix Table 4 for quantile regressions reported at the median. Level regressions with profits trimmed at 5% are also significant (not reported). Together, these suggest that level specifications in Panel B are not significant due primarily to outliers in the data (as opposed to the shape of the relationship). Note that levels are nominal Ghana Cedis (at first follow-up in January 2014, one US dollar was equivalent to 2.35 GHC).

	Log Profits					
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	FE: Pooled	FE: By round	FE IV: Pooled	FE IV: Round 1	FE IV: Round 2
Treatment Assignment	0.05*	0.11***		0.18***		
	(0.03)	(0.04)		(0.07)		
Treatment Assignment - Round 1			0.07*		0.17	
			(0.04)		(0.11)	
Treatment Assignment - Round 2			0.14***			0.18**
			(0.04)			(0.09)
Number of Firms	1014	1014	1014	1014	842	916
Total Observations	1758	1758	1758	1758	842	916
Mean of Dep Variable	5.61	5.61	5.61	5.61	5.74	5.49
R squared	0.30	0.39	0.39	0.32	0.41	0.39
First Stage F Stat				17.86	6.41	9.66
Lottery FEs	NO	YES	YES	YES	YES	YES
District FEs	YES	NO	NO	NO	NO	NO
	Profits Per Month (Nominal GHC)					
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	FE: Pooled	FE: By round	FE IV: Pooled	FE IV: Round 1	FE IV: Round 2
Treatment Assignment	6.74	18.38		31.60		
	(15.22)	(23.99)		(37.60)		
Treatment Assignment - Round 1			5.45		4.06	
			(30.03)		(81.93)	
Treatment Assignment - Round 2			30.78			56.99
			(21.60)			(43.25)
Number of Firms	1036	1036	1036	1036	877	949
Total Observations	1826	1826	1826	1826	877	949
Mean of Dep Variable	472.74	472.74	472.74	472.74	527.80	421.87
R squared	0.10	0.15	0.15	0.17	0.26	0.14
First Stage F Stat				16.38	8.11	9.23
Lottery FEs	NO	YES	YES	YES	YES	YES
District FEs	YES	NO	NO	NO	NO	NO

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: **Treatment Effect Heterogeneity.** Regressions include round fixed effects, lottery fixed effects, and baseline values of the dependent variable where applicable, with errors clustered at the district level. Columns 1 and 4 show that firms that were smaller at baseline benefit more from additional apprentice labor inputs in terms of percentage of profits and revenues, suggesting that small firms are perhaps more labor constrained than larger firms. Appendix Table 5 investigates this further and finds that heterogeneity in effect size by baseline firm size is robust to several alternative explanations. Columns 2 and 5 show that effects do not seem to vary by our three main trade classifications, where the excluded trade is garment-making. Columns 3 and 6 show insignificant but suggestive evidence that firms owned by women benefit less from the treatment. Note that these regressions are restricted to garment-makers, where there are large numbers of both men and women.

	Log Revenues			Log Profits		
	(1)	(2)	(3)	(4)	(5)	(6)
	FE: Pooled	FE: Pooled	FE: Pooled	FE: Pooled	FE: Pooled	FE: Pooled
Treatment Assignment	0.20***	0.07**	0.23**	0.23***	0.12***	0.24**
	(0.06)	(0.04)	(0.11)	(0.07)	(0.04)	(0.11)
Baseline Firm Size	0.07***	0.04***	0.02	0.08***	0.05***	0.02
	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)
Baseline Workers*Treatment	-0.03**			-0.03**		
	(0.01)			(0.01)		
Construction		0.52***			0.48***	
		(0.12)			(0.12)	
Beautician		0.10			0.03	
		(0.11)			(0.11)	
Construction*Treatment		0.00			-0.02	
		(0.08)			(0.07)	
Beautician*Treatment		-0.02			-0.04	
		(0.06)			(0.06)	
Female			0.03			-0.03
			(0.16)			(0.19)
Female*Treatment			-0.18			-0.14
			(0.14)			(0.15)
Number of Firms	1018	1018	432	1014	1014	433
Total Observations	1768	1768	764	1758	1758	765
Mean of Dep Variable	6.17	6.17	5.86	5.61	5.61	5.40
R squared	0.50	0.52	0.45	0.41	0.42	0.39
Lottery FEs	YES	YES	YES	YES	YES	YES

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: **Quality Treatment Effects - Digit Span Recall.** The firm sample in these regressions includes only firms that received apprentice interest from **both** above median cognitive ability apprentices and below median cognitive ability apprentices. Power in these regressions is limited by this restriction. Alternative specifications that include the entire sample for each sub-experiment have similar point estimates and more statistical significance. These two regressions for above median and below median cognitive ability had to be run separately in our preferred specification because above median and below median lottery fixed effects differ. Alternative specifications that control for non-random apprentice interest less rigorously and include treatment variables for both above median and below median cognitive ability apprentices have qualitatively similar findings. Digit Span Recall is a memory/cognitive function test. Similar tables using the Ravens test, the math test, and the English vocabulary test (correlated but different measures of apprentice cognitive ability) are broadly consistent and shown in Appendix Tables 6, 7 and 8. This table shows that take-up was higher for above median cognitive ability apprentices, as would be predicted by our model. However, differences in take-up do not fully account for differences in revenue and profit point estimates, which in Follow-up 2 are large and significant for above median cognitive ability apprentices and near zero for below median cognitive ability apprentices. Regressions include round fixed effects, lottery fixed effects, and baseline values of the dependent variable where applicable, with errors clustered at the district level.

(a) Above Median Cognitive Ability Apprentices

	Take-Up		Log Profits		Log Sales	
	(1)	(2)	(3)	(4)	(5)	(6)
	FE: Pooled	FE: By Round	FE: Pooled	FE: By Round	FE: Pooled	FE: By Round
T-above median	0.60***		0.09		0.05	
	(0.09)		(0.07)		(0.06)	
T-above median - Round 1		0.51***		0.01		-0.03
		(0.10)		(0.09)		(0.07)
T-above median - Round 2		0.67***		0.16**		0.12*
		(0.09)		(0.06)		(0.06)
Observations	795	795	747	747	754	754
Number of Firms	441	441	429	429	429	429
R squared	0.64	0.65	0.34	0.34	0.47	0.48
Lottery FEs	YES	YES	YES	YES	YES	YES

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

(b) Below Median Cognitive Ability Apprentices

	Take-Up		Log Profits		Log Sales	
	(1)	(2)	(3)	(4)	(5)	(6)
	FE: Pooled	FE: By Round	FE: Pooled	FE: By Round	FE: Pooled	FE: By Round
T-below median	0.35**		-0.03		-0.05	
	(0.17)		(0.10)		(0.05)	
T-below median - Round 1		0.33*		-0.04		-0.07
		(0.17)		(0.11)		(0.05)
T-below median - Round 2		0.36**		-0.01		-0.03
		(0.17)		(0.10)		(0.05)
Observations	795	795	747	747	754	754
Number of Firms	441	441	429	429	429	429
R squared	0.53	0.53	0.32	0.32	0.47	0.47
Lottery FEs	YES	YES	YES	YES	YES	YES

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8: **Quality Treatment Effects - Completed JSS.** The firm sample in these regressions includes only firms that received apprentice interest from **both** apprentices who had completed JSS and apprentices who had not completed JSS. Power in these regressions is limited by this restriction. Alternative specifications that include the entire sample for each sub-experiment have similar point estimates and more statistical significance. These two regressions for completed JSS and did not complete JSS had to be run separately in our preferred specification because the lottery fixed effects differ. Alternative specifications that control for non-random apprentice interest less rigorously and include treatment variables for both completed JSS and did not complete JSS apprentices have qualitatively similar findings. This table shows that the pattern we observe for cognitive ability is not present when we proxy for cognitive ability with educational outcomes. Though it is difficult to draw firm conclusions from these regressions, point estimates suggest that better educated apprentices benefit firms less, if anything. This is particularly striking because cognitive ability is positively correlated with educational outcomes. Regressions include round fixed effects, lottery fixed effects, and baseline values of the dependent variable where applicable, with errors clustered at the district level.

(a) Apprentices Who Completed Junior Secondary School (59% of the apprentice sample)

	Take-Up		Log Profits		Log Sales	
	(1)	(2)	(3)	(4)	(5)	(6)
	FE: Pooled	FE: By Round	FE: Pooled	FE: By Round	FE: Pooled	FE: By Round
T-JSS	0.48***		0.04		0.00	
	(0.09)		(0.09)		(0.06)	
T-JSS - Round 1		0.41***		0.03		-0.01
		(0.09)		(0.10)		(0.07)
T-JSS - Round 2		0.54***		0.05		0.02
		(0.10)		(0.09)		(0.06)
Observations	675	675	643	643	653	653
Number of Firms	372	372	361	361	361	361
R squared	0.65	0.65	0.42	0.42	0.55	0.55
Lottery FEs	YES	YES	YES	YES	YES	YES

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

(b) Apprentices Who Did Not Complete Junior Secondary School (41% of the apprentice sample)

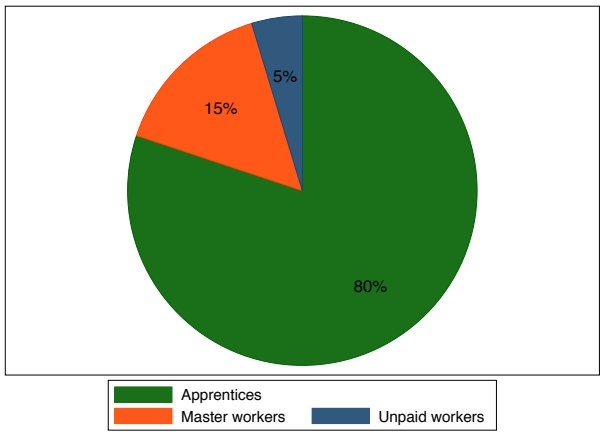
	Take-Up		Log Profits		Log Sales	
	(1)	(2)	(3)	(4)	(5)	(6)
	FE: Pooled	FE: By Round	FE: Pooled	FE: By Round	FE: Pooled	FE: By Round
T-not JSS	0.56***		0.04		0.10*	
	(0.13)		(0.07)		(0.05)	
T-not JSS - Round 1		0.49***		-0.02		0.06
		(0.13)		(0.09)		(0.07)
T-not JSS - Round 2		0.64***		0.11		0.13**
		(0.13)		(0.08)		(0.06)
Observations	675	675	643	643	653	653
Number of Firms	372	372	361	361	361	361
R squared	0.59	0.59	0.34	0.35	0.55	0.55
JSS Lottery FEs	YES	YES	YES	YES	YES	YES

Standard errors in parentheses

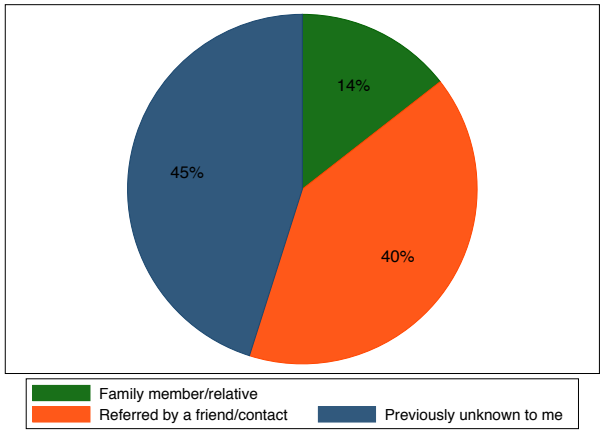
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix

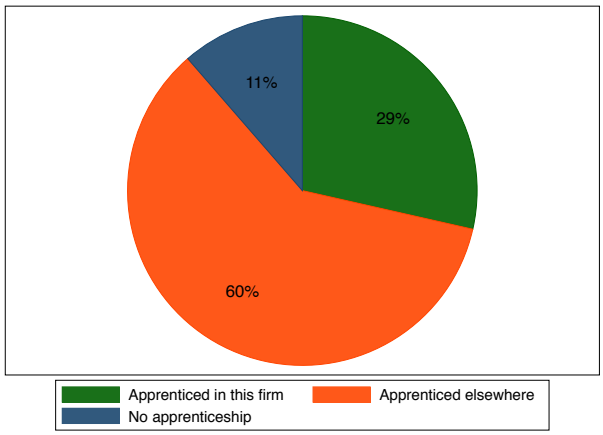
Figure A1: **Apprentices and Employment at Baseline.** Our baseline sample reflects the norm of small manufacturing and services firms in Ghana in composition of worker type, relationship to the owner, and characteristics of the typical workforce.



(a) **Worker Type Composition:** In our sample of over 1,000 firms, over 80% of the non-owner workforce are apprentices. Another 15% are "master" workers, who have completed apprenticeships either in the current firm or elsewhere and are paid much higher wages. The final 5% are unpaid workers, generally young family members. The workforce in the small firm sector throughout Ghana generally reflects the proportions in our sample.



(b) **Worker Relationship to Owner Composition:** Among apprentices, master workers, and unpaid workers in our sample (excluding firm owners), a full 46% were unknown to the firm owner before employment. It is among these unknown workers that the bond tends to be required and highest.



(c) **Master Worker Apprenticeship History:** Master workers almost always completed an apprenticeship (of between six months and three years) before beginning higher paid employment. They tend to join small firms with significant relevant occupational skills (in contrast to new apprentices, who learn on the job).

Figure A2: **Firm Size Effects - Program Apprentice Take-Up.** Mean of raw number of program apprentices working across two rounds, by treatment assignment. Note that there are few firms at the higher treatment assignments so the mean value is noisier.

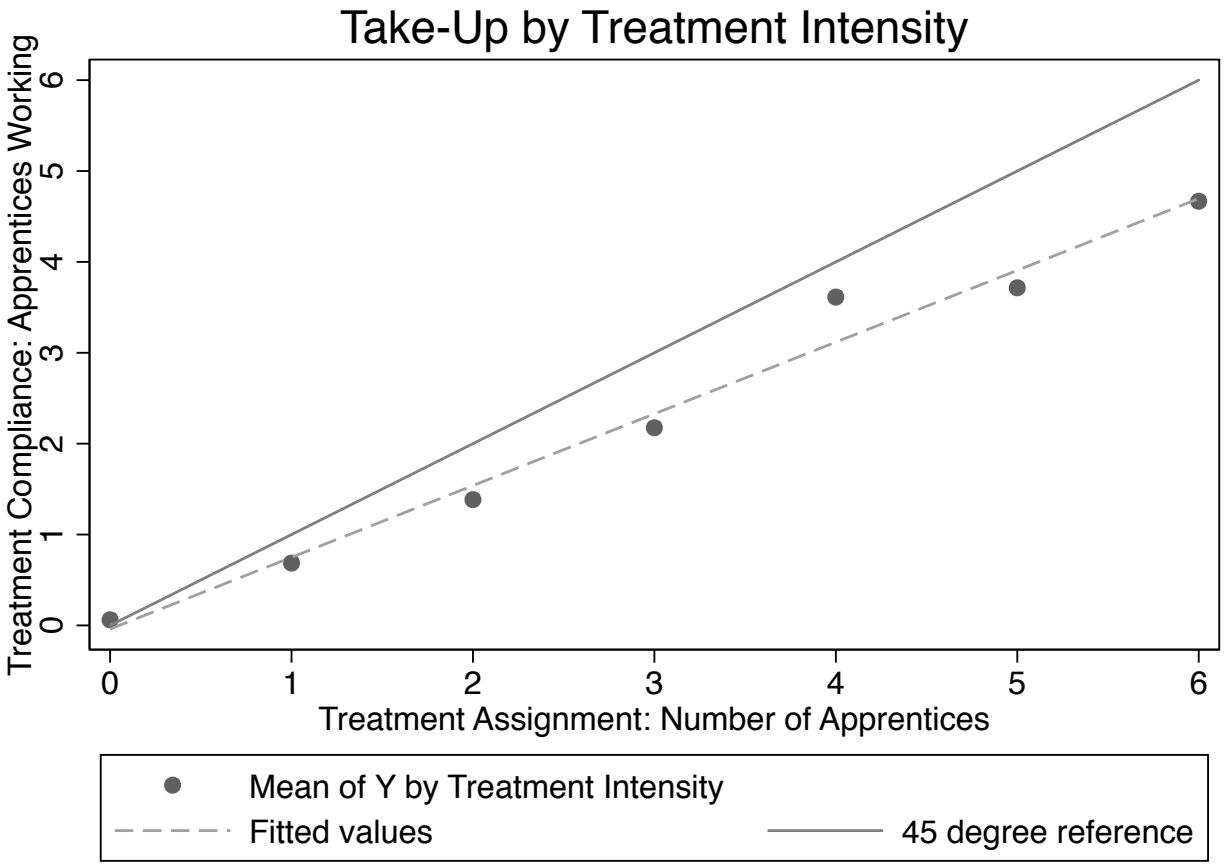


Figure A3: **Firm Size Effects - Total Firm Size Including Program Apprentices.** Mean of raw change in firm size across two rounds, by treatment assignment. Note that there are few firms at the higher treatment assignments so the mean value is noisier.

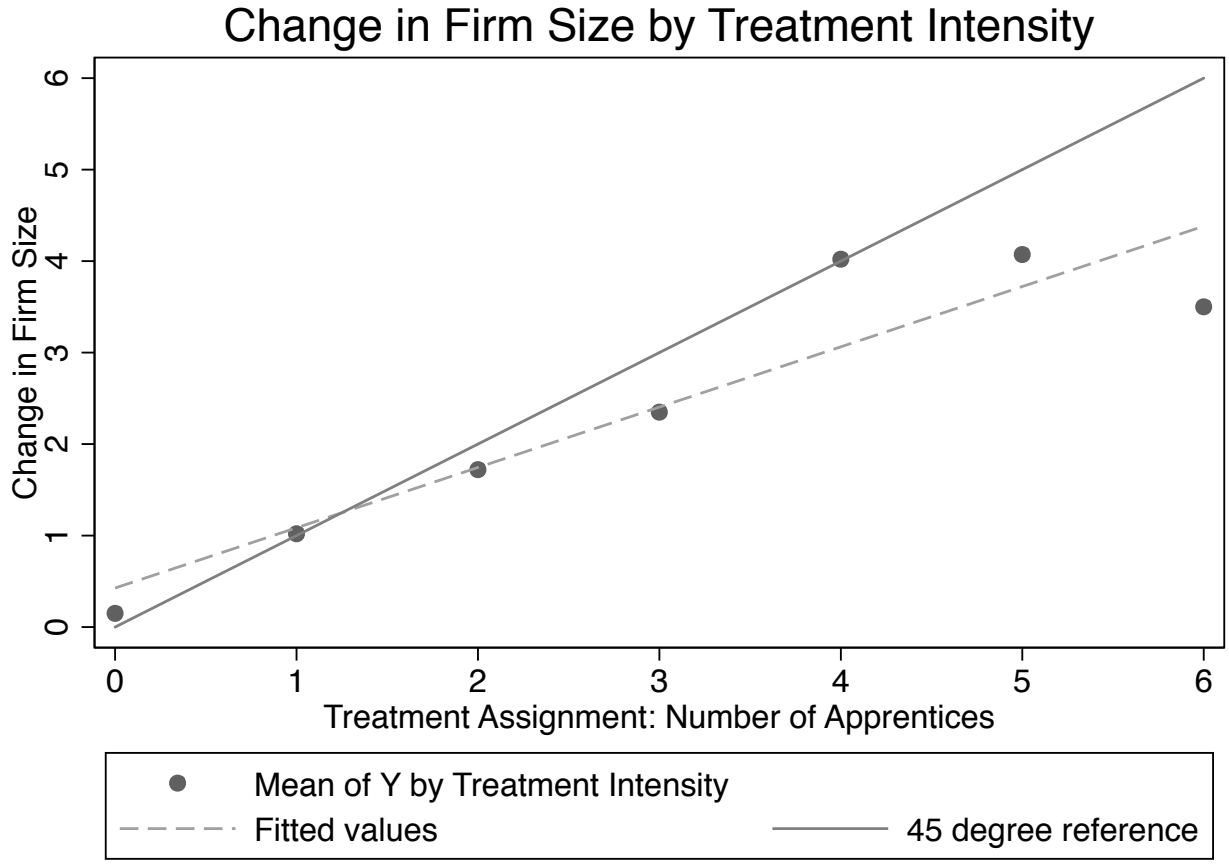


Figure A4: **Firm Size Effects - Total Firm Hours Including Program Apprentices.** Mean of raw change in total firm hours across two rounds, by treatment assignment. Note that there are few firms at the higher treatment assignments so the mean value is noisier.

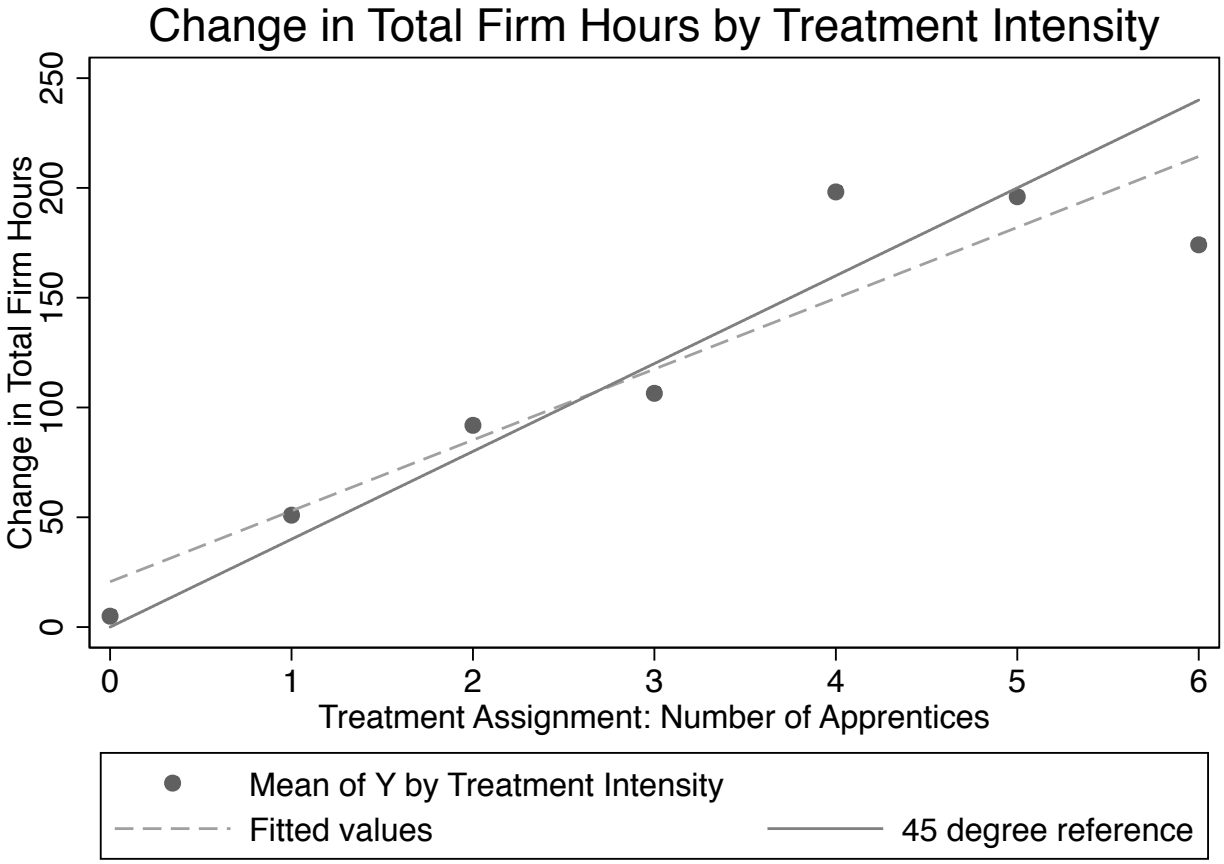


Table A1: **Descriptive Characterizations of the Labor Market for Small Firms.** The firm-level baseline survey included a series of questions meant to quantify, in part, the qualitative observations we gleaned from early piloting and focus groups. These focus groups were used prior to the design of the experiment to build a conceptual understanding of the apprentice labor market and the nature of labor constraints in our context, which were largely validated by the responses in the firm-level baseline survey (of about 1,000 firms) displayed below.

Baseline Survey Question	Common Response
Search and Hiring	
What are the three biggest barriers to the growth and success of your business?	The three most common response categories are access to finance (68% of firms), access to labor (52% of firms), and infrastructure (32% of firms).
Have you ever advertised or asked around for an apprentice?	Only 35% of firms said yes. We interpret this as evidence that simply posting a vacancy is unlikely to garner a suitable new apprentice, and that institutional centers for vacancy posting are lacking.
After how many months does a typical new apprentice begin to add to the profits of your business?	The median response is four months, though 30% of the sample firms said one month or less. About 14% of the firm owners think it takes a year or more for a typical new apprentice to add to the profits of the business.
Information about Worker Ability	
After how many months do you typically know if an apprentice is good or not very good?	The median response is three months, with 93% of sample firms saying it takes at least one month.
What is the main reason apprentices are normally required to make a payment at the start of an apprenticeship?	By a landslide, the most common response (85% of firms) is some variant of ensuring that the apprentice is good and committed.
Do you give more chop money/tips/wages to better performing apprentices?	80% of firms said yes.
Interest in Firm Growth	
Why are you interested in training NAP (program) apprentices?	27% of firms chose "It will be profitable for my business", while 21% of firms chose "I have many customers and need help". The most common response was "I want to help young people".
Overall, when you think of the size of your business, would you prefer to have it be larger, the same, or smaller?	96% of firms in the sample said they would like their business to be larger.
How important is the following reason in your choice to work in self-employment rather than a wage job? The potential for my business to grow much bigger in the future.	63% of firm owners said this reason was "very important", and another 31% said it was "important" in their decision to become self-employed.

Table A2: **Other Inputs.** Regressions include round fixed effects, lottery fixed effects, and baseline values of the dependent variable where applicable, with errors clustered at the district level. Capital stock is reported in nominal Ghana Cedis. We find no effect of the treatment on capital stock, log capital stock, or firm owner hours per week, suggesting that the availability of additional apprentice labor inputs did not lead to investments in complementary capital or managerial inputs. We do, however, find that firm owners report spending about half an hour per apprentice per day on instruction/training.

	Capital Stock		Log Capital Stock		Firm Owner Hours/Week			Firm Owner Instruction Hrs/Day	
	(1) OLS	(2) FE: Round 2	(3) OLS	(4) FE: Round 2	(5) OLS	(6) FE: Pooled	(7) FE: By round	(8) OLS	(9) FE: Round 2
Treatment Assignment	-85.45	-46.87	0.03	0.05	0.53	0.06		0.44***	0.44***
	(103.24)	(258.92)	(0.02)	(0.05)	(0.32)	(0.86)		(0.04)	(0.08)
Treatment Assignment - Round 1							0.16		
							(0.83)		
Treatment Assignment - Round 2							-0.03		
							(0.94)		
Number of Firms	958	958	958	958	1047	1047	1047	987	987
Total Observations	958	958	958	958	1868	1868	1868	987	987
Mean of Dep Variable	3305.98	3305.98	7.61	7.61	54.51	54.51	54.51	0.83	0.83
R squared	0.17	0.33	0.36	0.51	0.06	0.21	0.21	0.19	0.46
Lottery FEs	NO	YES	NO	YES	NO	YES	YES	NO	YES

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A3: **Quantile Regression Treatment Effects on Revenues.** Regressions include round fixed effects, lottery fixed effects, and baseline values of the dependent variable, with robust standard errors. Quantile regressions estimated at the median. Quantile regressions are an alternative to log transformations in dealing with noisy data. Though it may well be that the relationship between revenues and labor is concave, this table suggests that regressions using a log transformation in our main tables is significant while levels are not primarily due to power issues that come from outliers in the data.

	Revenues Per Month		Log Revenues	
	(1)	(2)	(3)	(4)
	FE QREG: Pooled	FE QREG: By Round	FE QREG: Pooled	FE QREG: By Round
Treatment Assignment	24.44***		0.04***	
	(0.00)		(0.00)	
Treatment Assignment - Round 1		13.24***		0.12***
		(0.00)		(0.00)
Treatment Assignment - Round 2		42.65***		0.03***
		(0.00)		(0.00)
Number of Firms	1034	1034	1018	1018
Total Observations	1823	1823	1768	1768
Mean of Dep Variable	875.75	875.75	6.17	6.17
Lottery FEs	YES	YES	YES	YES

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4: **Quantile Regression Treatment Effects on Profits.** Regressions include round fixed effects, lottery fixed effects, and baseline values of the dependent variable, with robust standard errors. Quantile regressions estimated at the median. Quantile regressions are an alternative to log transformations in dealing with noisy data. Though it may well be that the relationship between profits and labor is concave, this table suggests that regressions using a log transformation in our main tables is significant while levels are not primarily due to power issues that come from outliers in the data.

	Profits Per Month		Log Profits	
	(1)	(2)	(3)	(4)
	FE QREG: Pooled	FE QREG: By Round	FE QREG: Pooled	FE QREG: By Round
Treatment Assignment	25.45***		0.08***	
	(0.00)		(0.00)	
Treatment Assignment - Round 1		16.24***		0.13***
		(0.00)		(0.00)
Treatment Assignment - Round 2		40.52***		0.04***
		(0.00)		(0.00)
Number of Firms	1036	1036	1014	1014
Total Observations	1826	1826	1758	1758
Mean of Dep Variable	472.74	472.74	5.61	5.61
Lottery FEs	YES	YES	YES	YES

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A5: **Heterogeneous Effects for the Smallest Firms.** Regressions include round fixed effects, lottery fixed effects, and baseline values of the dependent variable where applicable, with errors clustered at the district level. Column 1 shows that take-up did not differ by baseline firm size and thus cannot explain the heterogeneous treatment effects by firm size. Columns 2 and 5 show that we do not find evidence of decreasing returns to labor inputs within our treatment. This is not surprising as the vast majority of firms were assigned zero, one, or two apprentices. Columns 3 and 6 investigate whether heterogeneous effects by firm size are explained wholly by firms with no workers besides the owner, as one might think these firms are a special case in which lack of management experience is a barrier to growth. These findings differ little from Table 6 and present no strong evidence that management is driving these effects. Columns 4 and 7 control for other characteristics which could explain our heterogeneity by firm size findings (un-interacted baseline characteristics are included in the regression but not reported in the table for space reasons). We find that our heterogeneity by firm size effects are robust.

	Take-Up	Log Revenues			Log Profits		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	FE: Pooled	FE: Pooled	FE: Pooled	FE: Pooled	FE: Pooled	FE: Pooled	FE: Pooled
Treatment Assignment	0.43***	0.07	0.14**	0.28**	0.14**	0.24***	0.27*
	(0.07)	(0.05)	(0.06)	(0.13)	(0.06)	(0.09)	(0.15)
Baseline Firm Size	-0.00			0.06***			0.08***
	(0.01)			(0.02)			(0.02)
Baseline Workers*Treatment	0.01			-0.02**			-0.03**
	(0.01)			(0.01)			(0.01)
Treatment*Treatment		-0.00			-0.01		
		(0.02)			(0.02)		
No workers besides owner			-0.15			-0.25*	
			(0.11)			(0.13)	
No workers besides owner*Treatment			0.07			0.15	
			(0.07)			(0.09)	
Female*Treatment				-0.03			0.03
				(0.10)			(0.10)
Construction*Treatment				0.02			0.02
				(0.11)			(0.12)
Beautician*Treatment				-0.00			-0.03
				(0.07)			(0.07)
Owner Years Sch*Treatment				-0.00			-0.00
				(0.01)			(0.01)
Assets*Treatment				-0.00			0.00
				(0.00)			(0.00)
Owner Digits High*Treatment				0.05			0.01
				(0.05)			(0.05)
Owner Math High*Treatment				-0.04			-0.04
				(0.06)			(0.07)
Mgmt Score High*Treatment				-0.03			-0.03
				(0.06)			(0.07)
Number of Firms	1047	1018	1018	1003	1014	1014	1000
Total Observations	1871	1768	1768	1744	1758	1758	1736
R squared	0.80	0.49	0.49	0.53	0.39	0.39	0.43
Lottery FEs	YES	YES	YES	YES	YES	YES	YES

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A6: **Quality Treatment Effects - Ravens.** The firm sample in these regressions includes only firms that received apprentice interest from **both** above median cognitive ability apprentices and below median cognitive ability apprentices. Power in these regressions is limited by this restriction. Alternative specifications that include the entire sample for each sub-experiment have similar point estimates and more statistical significance. These two regressions for above median and below median cognitive ability had to be run separately in our preferred specification because above median and below median lottery fixed effects differ. Alternative specifications that control for non-random apprentice interest less rigorously and include treatment variables for both above median and below median cognitive ability apprentices have qualitatively similar findings. Regressions include round fixed effects, lottery fixed effects, and baseline values of the dependent variable where applicable, with errors clustered at the district level.

(a) Above Median Cognitive Ability Apprentices

	Take-Up		Log Profits		Log Sales	
	(1)	(2)	(3)	(4)	(5)	(6)
	FE: Pooled	FE: By Round	FE: Pooled	FE: By Round	FE: Pooled	FE: By Round
T-above median	0.64***		0.13*		0.10	
	(0.10)		(0.07)		(0.06)	
T-above median - Round 1		0.53***		0.11		0.08
		(0.10)		(0.08)		(0.07)
T-above median - Round 2		0.74***		0.15*		0.13*
		(0.10)		(0.08)		(0.07)
Observations	762	762	723	723	729	729
Number of Firms	423	423	411	411	411	411
R squared	0.59	0.59	0.36	0.36	0.49	0.49
Lottery FEs	YES	YES	YES	YES	YES	YES

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

(b) Below Median Cognitive Ability Apprentices

	Take-Up		Log Profits		Log Sales	
	(1)	(2)	(3)	(4)	(5)	(6)
	FE: Pooled	FE: By Round	FE: Pooled	FE: By Round	FE: Pooled	FE: By Round
T-below median	0.51***		0.02		0.00	
	(0.16)		(0.09)		(0.05)	
T-below median - Round 1		0.49***		-0.03		-0.04
		(0.16)		(0.11)		(0.07)
T-below median - Round 2		0.54***		0.07		0.04
		(0.16)		(0.10)		(0.06)
Observations	762	762	723	723	729	729
Number of Firms	423	423	411	411	411	411
R squared	0.57	0.57	0.33	0.34	0.48	0.49
Lottery FEs	YES	YES	YES	YES	YES	YES

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A7: **Quality Treatment Effects - Math.** The firm sample in these regressions includes only firms that received apprentice interest from **both** above median cognitive ability apprentices and below median cognitive ability apprentices. Power in these regressions is limited by this restriction. Alternative specifications that include the entire sample for each sub-experiment have similar point estimates and more statistical significance. These two regressions for above median and below median cognitive ability had to be run separately in our preferred specification because above median and below median lottery fixed effects differ. Alternative specifications that control for non-random apprentice interest less rigorously and include treatment variables for both above median and below median cognitive ability apprentices have qualitatively similar findings. Regressions include round fixed effects, lottery fixed effects, and baseline values of the dependent variable where applicable, with errors clustered at the district level.

(a) Above Median Cognitive Ability Apprentices

	Take-Up		Log Profits		Log Sales	
	(1)	(2)	(3)	(4)	(5)	(6)
	FE: Pooled	FE: By Round	FE: Pooled	FE: By Round	FE: Pooled	FE: By Round
T-above median	0.50***		0.11		0.07	
	(0.10)		(0.07)		(0.06)	
T-above median - Round 1		0.44***		0.04		-0.01
		(0.11)		(0.09)		(0.08)
T-above median - Round 2		0.55***		0.16**		0.13**
		(0.10)		(0.06)		(0.05)
Observations	730	730	686	686	694	694
Number of Firms	404	404	393	393	393	393
R squared	0.55	0.55	0.33	0.33	0.42	0.43
Lottery FEs	YES	YES	YES	YES	YES	YES

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

(b) Below Median Cognitive Ability Apprentices

	Take-Up		Log Profits		Log Sales	
	(1)	(2)	(3)	(4)	(5)	(6)
	FE: Pooled	FE: By Round	FE: Pooled	FE: By Round	FE: Pooled	FE: By Round
T-below median	0.31*		0.02		0.01	
	(0.18)		(0.09)		(0.05)	
T-below median - Round 1		0.25		-0.06		-0.04
		(0.17)		(0.10)		(0.05)
T-below median - Round 2		0.36*		0.09		0.06
		(0.18)		(0.09)		(0.07)
Observations	730	730	686	686	694	694
Number of Firms	404	404	393	393	393	393
R squared	0.55	0.55	0.36	0.37	0.42	0.43
Lottery FEs	YES	YES	YES	YES	YES	YES

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A8: **Quality Treatment Effects - English Vocabulary.** The firm sample in these regressions includes only firms that received apprentice interest from **both** above median cognitive ability apprentices and below median cognitive ability apprentices. Power in these regressions is limited by this restriction. Alternative specifications that include the entire sample for each sub-experiment have similar point estimates and more statistical significance. These two regressions for above median and below median cognitive ability had to be run separately in our preferred specification because above median and below median lottery fixed effects differ. Alternative specifications that control for non-random apprentice interest less rigorously and include treatment variables for both above median and below median cognitive ability apprentices have qualitatively similar findings. Regressions include round fixed effects, lottery fixed effects, and baseline values of the dependent variable where applicable, with errors clustered at the district level.

(a) Above Median Cognitive Ability Apprentices

	Take-Up		Log Profits		Log Sales	
	(1)	(2)	(3)	(4)	(5)	(6)
	FE: Pooled	FE: By Round	FE: Pooled	FE: By Round	FE: Pooled	FE: By Round
T-above median	0.44***		0.09		0.02	
	(0.11)		(0.06)		(0.05)	
T-above median - Round 1		0.37***		0.04		-0.05
		(0.12)		(0.07)		(0.07)
T-above median - Round 2		0.50***		0.15**		0.09*
		(0.11)		(0.06)		(0.05)
Observations	801	801	758	758	763	763
Number of Firms	444	444	432	432	432	432
R squared	0.56	0.56	0.36	0.36	0.48	0.48
Lottery FEs	YES	YES	YES	YES	YES	YES

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

(b) Below Median Cognitive Ability Apprentices

	Take-Up		Log Profits		Log Sales	
	(1)	(2)	(3)	(4)	(5)	(6)
	FE: Pooled	FE: By Round	FE: Pooled	FE: By Round	FE: Pooled	FE: By Round
T-below median	0.40***		0.01		0.08*	
	(0.11)		(0.07)		(0.04)	
T-below median - Round 1		0.34***		-0.03		0.06
		(0.12)		(0.09)		(0.05)
T-below median - Round 2		0.45***		0.05		0.09*
		(0.11)		(0.07)		(0.05)
Observations	801	801	758	758	763	763
Number of Firms	444	444	432	432	432	432
R squared	0.60	0.61	0.37	0.37	0.48	0.48
Lottery FEs	YES	YES	YES	YES	YES	YES

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$