

Final report

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Evidence from a unique  
experiment in Ghana

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March 2016

When citing this paper, please  
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reference number:  
F-33202-GHA-1

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**Final Report**  
**IGC Ghana Project 1-VCC-VGHA-VXXXX-33202**

Do Apprentices Alleviate Firms' Labor Constraints?  
Evidence from a Unique Experiment in Ghana

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March 3, 2016

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PRELIMINARY ANALYSIS, PLEASE DO NOT CITE OR CIRCULATE. Morgan Hardy is an Assistant Professor at New York University-Abu Dhabi. Email: mlhardy@gmail.com. Isaac Mbiti is an Assistant Professor at the Frank Batten School of Leadership and Public Policy at the University of Virginia. Email: isaacmbiti@gmail.com. Jamie McCasland is an Assistant Professor at the Vancouver School of Economics at the University of British Columbia. Email: jamie.mccasland@gmail.com. This project received funding from 3ie, USAID, J-PAL, and the Private Enterprise Development in Low-Income Countries (PEDL) initiative. We thank Naana Amonoo-Neizer, Lois Aryee, Maham Farhad, Robert Obenya, Charles Sefenu, and Yani Tyskerud for excellent research assistance in the field, and Innovations for Poverty Action-Ghana for hosting the study.

# 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. In Ghana, apprenticeships make up a large share of first job experiences and a large majority of employment in small firms. For example, in a representative household survey of urban Ghana, Monk, Sandefur and Teal (2008) find that 26.7% of working age adults worked as an apprentice at some point early in their careers. The National Industrial Census reports that in 2000, 34% of wage employees in formal manufacturing were apprentices (Sandefur (2010)). Meanwhile, in our baseline survey of primarily informal firms, the average firm had 2.5 apprentices, compared with 0.6 paid workers.

Despite the importance of apprenticeships in West Africa, relatively little is known about their impacts on the firms that employ them. In fact, empirical research on small firm growth has focused primarily on credit constraints and managerial skill deficits (De Mel, McKenzie and Woodruff, 2008; Bloom and Reenan, 2007; Anagol and Udry, 2006; Bloom et al., 2013; Karlan, Knight and Udry, 2012; Kremer et al., 2013), while conventional wisdom argues that small firms face a frictionless market for hiring workers (including apprentices) (Rauch, 1991; Zenou, 2008). In this study, we seek to understand the effects of access to apprentices on firm size and firm profits, by exploiting exogenous variation generated by an apprentice placement program conducted in collaboration with the Government of Ghana.

## 2 Study Design

### 2.1 Umbrella RCT

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 the government apprentice placement program. Applicants to the apprenticeship program were recruited by local government officials in 32 districts, in all 10 regions of the country. Approximately 60% of these applicants were then randomly offered access to the program. While recruitment and applicant baseline surveys began in August of 2012, a long lag in program roll-out meant that firm recruitment and apprentice placement did not begin until nearly a year later. Of the 2,360

treatment apprentices across 32 districts, about 50% (1,178 people or 30% of the original applicant sample of 3,948) participated in the 2013 matching meetings and entered the apprentice sample used in this paper. Long-term follow-up surveys with the apprentice sample will take place in 2017, about 5 years after recruitment and 4 years after initial program placement.

## **2.2 Sample Recruitment**

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 targeted recipients of the program. 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. 1,833 firm owners attended a matching meeting, and 1,087 of these received interest from at least one apprentice. It is these 1,087 that compose our base sample in this paper.

## **2.3 Placement Meetings**

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.

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 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.

Apprentice placement began shortly after all matching meetings were completed, in October of 2013, and some apprentices did not report to their assignments until early 2014.

## **2.4 Program Intervention**

The National Apprenticeship Program was originally envisioned to mirror the traditional apprenticeship system in that firm owners would be paid at the start of the apprenticeship (about 150GHc) and toolkits would be provided to program apprentices. It was intended to depart from traditional apprenticeships in that the program period would be one year and the curriculum would be more neatly tailored to the skills qualification system being redesigned by the Government of Ghana. In the end, however, the program rather departed from the traditional apprenticeship system in that firm owners were not paid by the government and apprentices did not receive toolkits, but fits fairly neatly the traditional apprenticeship system in that most apprentices who reported to their posts appear to still be in training at two years after training began (the timing of the fourth follow up survey). We cannot experimentally test the effect of these departures both from the traditional

apprenticeship system and from the original vision of the program, but anecdotally these changes do not appear to have had a significant effect on firm owner willingness to train and employ NAP apprentices or on the basic structure of apprenticeship training and employment in these firms.

## 2.5 Data

Data for this study come from four sources: (1) firm baseline surveys, (2) apprentice baseline surveys, (3) apprentice-specific firm sets, and (4) four firm level follow-up surveys. IGC funding co-funded the third and fourth of these firm level follow up surveys. Survey attrition across rounds is relatively minimal, with 99% of core sample firms participating in the baseline survey, and participation rates in rounds one through four of 94%, 92%, 92% and 85% respectively. Further analysis of attrition patterns is left for a future version of this working paper. Follow-up surveys took place in January 2014, April 2014, February 2015, and November 2015, spanning approximately 3 months to two years after program placement.

Follow-up survey data collection includes revenues, profits, detail on program apprentices, other labor inputs, measures of capital stock, and information on subsequent hiring. All survey questions and strategies were extensively piloted<sup>1</sup>. Following De Mel, McKenzie and Woodruff (2009), 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?”*

Apprentice cognitive tests, a proxy of ability, 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 surveyors read out a number or series of numbers and respondents repeat the numbers. The number of digits increases over

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<sup>1</sup>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.

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. The cognitive ability index is the sum of the normalized scores on the four individual tests<sup>2</sup>.

## 2.6 Randomization and Estimation

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  $T_i$  that takes values between zero and eight apprentices assigned to the firm. We control for dummy variables for being listed by each apprentice in the sample, which function similar to strata fixed effects as a control for the probability distribution of the treatment value. The potential outcomes are independent of the treatment assignment conditional on these apprentice preference dummies. In earlier versions of this analysis, we focused on a lottery fixed effect specification which pooled the probability distribution of treatment assignments across apprentices rather than controlling for individual apprentice preferences (Hardy and McCasland, 2015) .

Our primary outcome families of interest are (1) labor inputs and firms size and (2) revenues and profits. Following McKenzie (2012), our main specification stacks data from the four follow-up rounds, controls for the baseline value of the outcome variable, and includes round fixed effects ( $\eta_r$ ) and apprentices dummies ( $\varphi_a$ ), as follows:

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

The coefficient  $\beta$  estimates the Intent-to-Treat effect and is identified from within-round, within

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<sup>2</sup>The apprentice baseline survey attempted a fifth cognitive test in reading. Unfortunately, a majority of respondents opted out of the reading test, making it a poor measure for ability across apprentices.

apprentice-specific randomization variation.  $\beta$  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 firm level across rounds.

To measure treatment effects across rounds, we estimate:

$$Y_{it} = \alpha + \beta_1 T_i * \eta_1 + \beta_2 T_i * \eta_2 + \beta_3 T_i * \eta_3 + \beta_4 T_i * \eta_4 + \gamma Y_{i0} + \eta_r + \varphi_a + \epsilon_{it} \quad (2)$$

In additional specifications we define treatment separately as  $T_{i,abovemedian}$  and  $T_{i,belowmedian}$  to measure separate experimental effects associated with access to higher or lower ability apprentices as measured by our apprentice cognitive tests.

## 3 Results

### 3.1 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 entry fee that is traditionally 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. Table 1 reproduces some of these questions, and the most common responses.

### 3.2 Summary Statistics

In our nationwide sample of 1,087 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 about 21 Ghana Cedis, which at the time of baseline surveys was about 10 US dollars.

Column 1 of Table 2 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).

Raw tests of covariate balance reveal that the larger firms received more interest from apprentices and were thus assigned more workers. However, tests of covariate balance that control for our apprentice preference/strata fixed effects show balance along observables conditional on these randomization controls.

### 3.3 Take Up

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. However, apprentice take-up was about 60% of those assigned a firm placement by the placement randomization. In addition, we had a number of treatment NAP apprentices who entered a sample firm (or another firm) after the placement randomization (having missed the placement meeting, but still wanting a place in the program), and a number of treatment apprentices who received a placement but ultimately reported to another firm either within our sample or elsewhere (i.e. did not comply with their firm placement treatment assignment). Nonetheless, as Table 3 shows, for each NAP apprentice assigned to a firm in our core sample, about 0.36 NAP apprentices are observed working over the course of the four rounds of follow-up data collection.

In columns (3) through (8) of Table 3, we see relatively little evidence of crowd-out, either of non-NAP apprentices or paid workers in sample firms. If anything, there appears to be evidence of crowd-in, whereby treatment apprentices led to additional employment of paid workers. We will continue to investigate this finding as we complete the analysis of our most recent IGC-funded rounds of data collection.

### 3.4 Revenues and Profits

Tables 4 and 5 present Intention to Treat (ITT) results on revenues and sales in both levels and logs. Raw levels are quite noisy, and insignificant in most specifications. Winterized levels at 5% by round are positive and significant for both profits and sales, as are log specifications which likewise address outliers. While log specifications help with power, levels specifications are probably most appropriate given the level treatment.

In general we find sizable effects on firm output associated with access to apprentices through the program. Our recent working paper puts forward a theory about the screening mechanism imbedded in the apprentice recruitment and the long-lag in program roll-out. In this report however, we will suffice to say that it appears the program has a large effect and interpretations of the effect will be left for future work.

### 3.5 Apprentice Cognitive Ability

In Tables 6 and 7 we split the apprentices in to those who perform above the median and below the median on our cognitive ability index, which is composed of the sum of the normalized scores on four individual tests (ravens, math, vocabulary, and digits forward). In previous work, we uncovered a pattern that suggests that treatment effects on revenues and profits are larger when considering access to above median cognitive ability apprentices. Across the four pooled rounds using new data, the pattern continues to be weakly visible in our data, though like in our earlier work, power makes it difficult to identify point estimates on above and below median cognitive ability apprentices that are statistically significantly different from each other.

## 4 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 premia 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, leading to large differences in firm size between control and treatment firms. In addition, we show that access to apprentices through the program had large ITT effects on revenues and profits. Analysis of the IGC-funded rounds 3 and 4 of the firm-level follow-up data is still underway. Round 4 in particular has ample data on subsequent hiring, which may be able to shed light on some of the questions left unanswered by this preliminary analysis.

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Table 1: **Descriptive Characterizations of the Labor Market for Small Firms.** Sample includes 1,070 core sample firms with a baseline survey.

Baseline Survey Question	Common Response
<b>Search and Hiring</b>	
<i>What are the three biggest barriers to the growth and success of your business?</i>	The three most common response categories are access to finance (68% of firms), access to labor (52% of firms), and infrastructure (32% of firms).
<i>Have you ever advertised or asked around for an apprentice?</i>	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.
<i>After how many months does a typical new apprentice begin to add to the profits of your business?</i>	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.
<b>Information about Worker Ability</b>	
<i>After how many months do you typically know if an apprentice is good or not very good?</i>	The median response is three months, with 93% of sample firms saying it takes at least one month.
<i>What is the main reason apprentices are normally required to make a payment at the start of an apprenticeship?</i>	By a landslide, the most common response (85% of firms) is some variant of ensuring that the apprentice is good and committed.
<i>Do you give more chop money/tips/wages to better performing apprentices?</i>	80% of firms said yes.
<b>Interest in Firm Growth</b>	
<i>Why are you interested in training NAP (program) apprentices?</i>	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”.
<i>Overall, when you think of the size of your business, would you prefer to have it be larger, the same, or smaller?</i>	96% of firms in the sample said they would like their business to be larger.
<i>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.</i>	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 2: Summary statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Female	0.66	0.47	0	1	1087
Garment Makers	0.42	0.49	0	1	1087
Hairdressers and Beauticians	0.33	0.47	0	1	1087
Construction	0.25	0.43	0	1	1087
Firm size	4.48	2.89	1	23	1071
Has any worker(s) besides owner	0.89	0.31	0	1	1087
Paid workers	0.53	1.2	0	10	1071
Apprentices	2.78	2.59	0	22	1074
Unpaid workers	0.16	0.59	0	7	1071
Proportion of workforce is family	0.15	0.27	0	1	949
Revenues (nominal GHC)	717.38	1408.11	0	30000	1065
Profits (nominal GHC)	336.96	615.39	0	9000	1066
Assets (nominal GHC)	7278.07	12206.9	0	180400	1074
Firm age	11.53	7.28	0	40	1072
Bank account	0.67	0.47	0	1	1072
Electricity connection	0.87	0.33	0	1	1014
Registered w/district assembly	0.34	0.48	0	1	1072
Registered w/registrar general	0.07	0.26	0	1	1071
Owner years schooling	8.95	3.39	0	21	1071
Owner digits span recall (of 14)	6.93	2.51	1	14	1073
Owner math correct (of 4)	2.62	0.89	0	4	1070

Table 3: **Take-Up.** Regressions include round fixed effects, apprentice preference fixed effects, and baseline values of the dependent variable, where applicable. Errors are clustered at the firm level.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	NAP	NAP	Non-NAP	Non-NAP	Paid	Paid	Total	Total
	Apprentices	Apprentices	Apprentices	Apprentices	Workers	Workers	Workers	Workers
Treatment Assignment	0.364*** (0.0409)		0.0956 (0.0880)		0.0963* (0.0500)		0.501*** (0.123)	
Treatment Assignment - Round 1		0.372*** (0.0487)		0.0487 (0.103)		0.113* (0.0589)		0.512*** (0.140)
Treatment Assignment - Round 2		0.437*** (0.0446)		0.101 (0.101)		0.0998* (0.0561)		0.551*** (0.131)
Treatment Assignment - Round 3		0.359*** (0.0482)		0.0376 (0.0985)		0.111** (0.0559)		0.473*** (0.139)
Treatment Assignment - Round 4		0.281*** (0.0484)		0.204* (0.116)		0.0585 (0.0572)		0.465*** (0.144)
Observations	3770	3770	3778	3778	3768	3768	3768	3768

Standard errors in parentheses

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



Table 4: **Profits.** Regressions include round fixed effects, apprentice preference fixed effects, and baseline values of the dependent variable, where applicable. Trimmed profits are winterized at 5%. Errors are clustered at the firm level.

	(1)	(2)	(3)	(4)	(5)	(6)
	Raw Profits	Raw Profits	Trimmed Profits	Trimmed Profits	Log Profits	Log Profits
Treatment Assignment	19.55 (35.69)		38.14*** (14.01)		0.120*** (0.0399)	
Treatment Assignment - Round 1		4.612 (39.18)		32.16* (17.11)		0.0831* (0.0459)
Treatment Assignment - Round 2		23.26 (37.15)		41.99*** (15.32)		0.137*** (0.0442)
Treatment Assignment - Round 3		24.98 (37.39)		38.98** (16.78)		0.125*** (0.0439)
Treatment Assignment - Round 4		25.29 (41.26)		39.39** (16.13)		0.134*** (0.0480)
Observations	3635	3635	3635	3635	3459	3459

Standard errors in parentheses

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

Table 5: **Sales.** Regressions include round fixed effects, apprentice preference fixed effects, and baseline values of the dependent variable, where applicable. Trimmed sales are winterized at 5%. Errors are clustered at the firm level.

	(1)	(2)	(3)	(4)	(5)	(6)
	Raw Sales	Raw Sales	Trimmed Sales	Trimmed Sales	Log Sales	Log Sales
Treatment Assignment	-32.16 (124.2)		67.27** (28.19)		0.120*** (0.0377)	
Treatment Assignment - Round 1		-49.86 (127.3)		44.63 (32.74)		0.0806* (0.0427)
Treatment Assignment - Round 2		-23.07 (116.9)		73.98** (31.10)		0.142*** (0.0414)
Treatment Assignment - Round 3		-21.26 (126.8)		77.85** (33.73)		0.122*** (0.0405)
Treatment Assignment - Round 4		-35.86 (149.4)		72.23** (32.54)		0.135*** (0.0453)
Observations	3629	3629	3629	3629	3485	3485

Standard errors in parentheses

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

Table 6: **Apprentice Cognitive Ability and Profits.** Regressions include round fixed effects, apprentice preference fixed effects, and baseline values of the dependent variable, where applicable. Trimmed sales are winterized at 5%. Errors are clustered at the firm level.

	(1)	(2)	(3)	(4)	(5)	(6)
	Raw Profits	Raw Profits	Trimmed Profits	Trimmed Profits	Log Profits	Log Profits
Treatment Assignment: Cog Index Above Median	36.83 (46.70)		48.42** (20.84)		0.105* (0.0549)	
Treatment Assignment: Cog Index Below Median	-33.83 (66.31)		2.661 (22.55)		0.0886 (0.0658)	
Treat - Round 1: Cog Index Above Median		39.00 (56.35)		53.28** (27.01)		0.0904 (0.0645)
Treat - Round 2: Cog Index Above Median		35.41 (49.24)		60.12*** (22.59)		0.131** (0.0627)
Treat - Round 3: Cog Index Above Median		23.49 (54.81)		26.49 (25.55)		0.0442 (0.0636)
Treat - Round 4: Cog Index Above Median		50.87 (50.54)		54.26** (24.19)		0.156** (0.0669)
Treat - Round 1: Cog Index Below Median		-56.89 (69.16)		-14.80 (26.87)		0.0364 (0.0734)
Treat - Round 2: Cog Index Below Median		-26.26 (75.02)		5.225 (24.84)		0.102 (0.0729)
Treat - Round 3: Cog Index Below Median		-19.41 (68.37)		20.09 (27.54)		0.148** (0.0738)
Treat - Round 4: Cog Index Below Median		-33.35 (70.94)		-1.071 (25.52)		0.0655 (0.0771)
Observations	3635	3635	3635	3635	3459	3459

Standard errors in parentheses

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

Table 7: **Apprentice Cognitive Ability and Sales.** Regressions include round fixed effects, apprentice preference fixed effects, and baseline values of the dependent variable, where applicable. Trimmed sales are winterized at 5%. Errors are clustered at the firm level.

	(1)	(2)	(3)	(4)	(5)	(6)
	Raw Sales	Raw Sales	Trimmed Sales	Trimmed Sales	Log Sales	Log Sales
Treatment Assignment: Cog Index Above Median	62.64 (186.8)		91.38** (43.42)		0.109** (0.00547)	
Treatment Assignment: Cog Index Below Median	-244.4 (182.4)		-4.632 (44.46)		0.0840 (0.00636)	
Treat - Round 1: Cog Index Above Median		40.03 (201.4)		70.96 (51.08)		0.0769 (0.0624)
Treat - Round 2: Cog Index Above Median		72.96 (180.6)		109.7** (46.73)		0.147** (0.0604)
Treat - Round 3: Cog Index Above Median		67.07 (196.6)		79.55 (52.84)		0.0793 (0.0606)
Treat - Round 4: Cog Index Above Median		70.99 (201.4)		106.8** (50.27)		0.133** (0.0672)
Treat - Round 1: Cog Index Below Median		-270.6 (189.9)		-35.45 (51.03)		0.0315 (0.0703)
Treat - Round 2: Cog Index Below Median		-223.9 (181.4)		6.054 (50.33)		0.0988 (0.0705)
Treat - Round 3: Cog Index Below Median		-233.9 (189.5)		17.30 (54.62)		0.114 (0.0695)
Treat - Round 4: Cog Index Below Median		-250.2 (204.5)		-7.852 (50.27)		0.0897 (0.0723)
Observations	3629	3629	3629	3629	3485	3485

Standard errors in parentheses

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

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