Identifying and Spurring High-Growth Entrepreneurship:

Experimental Evidence from a Business Plan Competition[*]

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Abstract

Almost all firms in developing countries have fewer than 10 workers, with a modal size of one. Are there potential high-growth entrepreneurs, and can public policy help identify them and facilitate their growth? A large-scale national business plan competition in Nigeria provides evidence on these questions. Random assignment of US$36 million in grants provided each winner with approximately US$50,000. Surveys tracking applicants over three years show that winning leads to greater firm entry, more survival, higher profits and sales, and higher employment, including increases of over 20 percentage points in the likelihood of a firm having 10 or more workers.

Keywords: Business plan competition; entrepreneurship; high-growth

JEL codes: O12, M13, L26, C93.

1. Introduction

The modal firm size in most developing countries is one worker, consisting of only the owner of the firm. Among the firms that do hire additional workers, most hire fewer than 10. Hsieh and Olken (2014, p.93) report that in India and Indonesia "the fraction of firms with less than 10 workers is almost visually indistinguishable from 100 percent." Likewise in Nigeria, survey data indicate that 99.6 percent of firms have fewer than 10 workers.[1] This is in sharp contrast to the United States, where the modal manufacturing firm has 45 workers (Hsieh and Klenow, 2014). This move away from self-employment towards wage employment in firms of larger sizes is a key aspect of the development process (e.g. Gollin, 2002). Are there constrained entrepreneurs in developing countries with the ability to grow a firm beyond this 10-worker threshold? If so, this raises the questions of whether such individuals can be identified in advance, and of whether public policy can help them overcome these constraints to firm growth?

I investigate these questions through the context of an evaluation of the impact of a national business plan competition in Nigeria. The YouWiN! competition was launched in late 2011 by the President of Nigeria, and in its first year attracted almost 24,000 applications aiming to start a new business or expand an existing one. The top 6,000 applications were selected for a 4-day business plan training course, and then winners were chosen to receive awards averaging US$50,000 each. The top-scoring plans overall and within region were chosen as winners automatically, and then 729 additional winners were randomly selected from a group of 1,841 semi-finalists, providing experimental variation from US$36 million in grants that enables causal estimation of the program's impact. Three annual follow-up surveys enable tracking the trajectory of impacts, with the last survey occurring 27 months after winners received their first grant payment and 12 to 18 months after the last payment.

I find that winning this competition has large positive impacts on both applicants looking to start new firms as well as those aiming to expand existing firms. Three years after applying, new firm applicant winners were 37 percentage points more likely than the control group to be operating a business and 23 percentage points more likely to have a firm with 10 or more workers (relative to a control mean of 11 percent), while existing firm winners were 20 percentage points more likely

to have survived, and 21 percentage points more likely to have a firm with 10 or more workers (relative to a control mean of 17 percent). Together the 1,200 winners are estimated to have generated 7,000 more jobs than the control group, are innovating more, and are earning higher sales and profits. Examining the channels of impact, I find the grants enabling firms to purchase more capital and hire more labor, with no changes in business networks, mentors, self-efficacy, or uses of other sources of finance. The results show there is a group of constrained entrepreneurs with the ability to grow their businesses beyond a small scale, and that the business plan competition was successful in both attracting such individuals, and in helping them overcome their constraints. Nevertheless, conditional on getting to the stage of submitting a full business plan, we find that personal, business, and business plan characteristics have low predictive power for identifying which entrepreneurs will grow faster, and which will respond best to treatment.

This paper contributes to three main literatures. The first addresses the sources and constraints to job growth, which is one of the most fundamental concerns of policy makers globally. An increasing body of work examines which types of firms create more jobs, with recent evidence highlighting the importance of business start-ups and young firms in job creation.[2] Related work has found that a small number of firms, variously termed "gazelles", "high-growth entrepreneurs" or "high-impact firms" represent a tiny fraction of the overall firm population, but make a disproportionally large contribution to job growth.[3] New job creation by firms is of particular importance in Sub-Saharan Africa given that it currently has the lowest share of the labor force in wage work of any region in the world, and that demographic forces result in the working-age population growing 2.8 percent per year (ILO, 2012). A recent survey of African entrepreneurs which concluded that "The culture of entrepreneurship in Africa is largely defined by necessity driven entrepreneurship; that is, entrepreneurship as a means of survival. Entrepreneurship is viewed as a last resort, as opposed to the pursuit of an opportunity or aspiration" (Omidyar Network, 2013, p. 18). While the study felt these attitudes were changing, it concurred with a long literature that suggests a perceived lack of high-growth entrepreneurship in many African countries (e.g. Elkan, 1988) may be in part due to multiple market failures that prevent individuals with good

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[2] See, for example, Haltiwanger et al. (2013) for the U.S.A,, Lawless (2014) on Ireland, and Ayyagari et al. (2014) for evidence from a range of developing countries.

[3] For example, Morris (2011) finds that 4 percent of entrepreneurs in the Global Entrepreneurship Monitor surveys account for close to 40 percent of all jobs created; see also reviews by Henrekson and Johannsson (2010) and the OECD (2010).

ideas from funding them. This paper provides empirical evidence that there are such potential high growth entrepreneurs who can grow rapidly if funded.

Business plan competitions are starting to become one popular tool that seeks to foster high growth entrepreneurship. They seek to identify individuals with promising ideas and the aspiration to grow, help these individuals formalize these ideas through getting them to develop a detailed business plan, and then spur the development of some of these potentially high growth firms through providing financing to the winners. The first such competition for MBA students was held in 1984 at the University of Texas at Austin, and has now become a global competition;[4] now such competitions are held in more than 50 American business schools, as well as by a variety of other organizations (Kolodny, 2009). Business plan competitions are also increasingly being launched in developing countries. Examples include the MENA 100 business plan competition in 14 countries in the Middle East and North Africa since 2009,[5] the GIST/I-Dare business plan competition for individuals from 28 developing countries, launched in 2011;[6] and business plan competitions run by Technoserve throughout Central America since 2002 (Klinger and Schündeln, 2011).[7] They are also starting to be used by the World Bank in several Sub-Saharan African countries, including Côte d'Ivoire, Gabon, Guinea-Bissau, Rwanda, Senegal, Somalia, South Sudan, Tanzania and Uganda.

However, there is very little evidence as to the effectiveness of these programs. Many of the programs are small in nature, awarding fewer than 10 prizes in any given year, limiting the sample size for evaluation. Programs explicitly try to select entrepreneurs with the best growth prospects, so that any comparison of winners and losers is likely to overstate the effects of the program due to selection bias. Klinger and Schündeln (2011) and Fafchamps and Quinn (2015) attempt to address these issues by pooling together competitions run in multiple countries to get larger sample sizes of winners (although still fewer than 40 cash winners in either study), and use a regression-discontinuity approach to compare winners to those who just finished below them in rank. Both studies find impacts on business start-up or expansion, but do not look at job creation outcomes or

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[4] http://www.mccombs.utexas.edu/Centers/Texas-Venture-Labs/Investment-Competition/About [accessed March 18, 2014].

[5] http://www.egypt-business.com/Paper/details/1151-xg-MENA-100-Business-Plan-Competition/3061 [accessed March 18, 2014].

[6] http://gist.crdfglobal.org/gist-programs/business-plan-competition [accessed March 18, 2014].

[7] See also Fafchamps and Quinn (2015) for a list of other examples.

longer-term growth. In the one experimental study we are aware of, Fafchamps and Woodruff (2014) run a small business plan competition in Ghana, in which winners are selected to receive individualized training, but not cash. They find no significant impact of this individualized training on firm growth. This paper builds on this literature through experimental evaluation of a much larger sample, with substantial prizes, detailed surveys, and a longer time period for tracking winners.

Finally this paper adds to a literature on how to generate entrepreneurship in developing countries, and in particular the role of capital in alleviating constraints.[8] The vast majority of this work has focused on microenterprises, typically with no employees, with the emphasis on starting such businesses and growing the incomes of their owners. This is the case with work testing small grants of US\$100 to US\$200 to existing businesses (e.g. De Mel et al, 2008; McKenzie and Woodruff, 2008; Fafchamps et al, 2014; Karlan et al, 2014), ultra-poor programs providing grants and training to get very poor people to start businesses (e.g. Banerjee et al, 2011; Bandiera et al, 2013), and business training programs for microenterprises (reviewed in McKenzie and Woodruff, 2014). Blattman et al. (2014) considers a program in Uganda where groups received grants of approximately US\$382 per member, and finds this generates increases in skilled self-employment and in incomes for the recipients, but only minor increases in employment in these firms.[9] The grants studied here of almost US\$50,000 per recipient are more than 100 times as large as the grants in these earlier studies. In the absence of market failures and frictions on the use of this capital, theory would predict that these grants would merely supplement the incomes of the business owners, without changing their production decisions. Even with credit constraints, there is a question as to whether diminishing returns to capital results in grants of this size being far more than is needed to move businesses to their efficient size. The results in this paper show that the business plan competition is successful in identifying entrepreneurs with the potential to use these large amounts of capital, and that with this capital, they are able to generate enterprises that hire employees and exhibit rapid growth.

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[8] Capital has also been found to be an important constraint to innovation for firms in developed countries (see Howell, 2015).

[9] The grants to the 39 winners in the business plan competition of Fafchamps and Quinn (2015) were US\$1000, and over a 6 month horizon the winners were more likely to start firms and had higher profits than the runners-up and third place getters.

The paper also contributes to a related debate in the literature as to whether a lack of capital constrains entrepreneurs from growing and hiring employees, or whether it is other factors such as labor regulations, management ability (Bloom et al, 2012) and the possibility that "transformational" entrepreneurs who aim to create large, vibrant businesses that grow and hire workers are fundamentally different from owners of microenterprises (Schoar, 2010). The evidence here shows that there are individuals for whom capital is the key constraint.

The remainder of the paper is as follows. Section 2 describes the details of the business plan competition, the selection process, and the randomization. Section 3 provides details on data collection. Section 4 provides some theoretical predictions of how the program may affect businesses, and the estimation strategy, while Section 5 provides the main results of the paper, estimating the impact of winning the program on business outcomes. Section 6 discusses robustness to some concerns with such evaluations, Section 7 discusses targeting, and Section 8 concludes.

2. The Business Plan Competition

The *Youth Enterprise With Innovation in Nigeria* (YouWiN!) program is a business plan competition for young entrepreneurs in Nigeria.[10] It is a collaboration between the Ministry of Finance, the Ministry of Communication Technology, and the Ministry of Youth Development with support from DFID and the World Bank. It has the stated objective of encouraging innovation and job creation through the creation of new businesses and expansion of existing businesses. It was formally launched on October 11, 2011 by President Goodluck Jonathan in a ceremony aired live over the National Television network, and advertised through a variety of media events, roadshows, and SME outreach events. Appendix 1 details the timeline of the project.

The program provides a four-day training course on preparation of a business plan to applicants who make it through a first stage, and then grants to the winning 1,200 submissions, with each winner eligible for an amount up to 10 million Naira (approximately US$64,000), with the amount any winner getting varying between 1 and 10 million Naira depending on the funding needs identified in their business plan and the assessment of independent consultants of what the actual

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[10] The business plan competition was run in three consecutive years. My analysis uses the first year of the program, which was the only year a randomized experiment was fully implemented.

needs are. Winners also receive ongoing monitoring as the grant is paid in four tranches, coupled with some potential mentoring and two additional two-day group training events.[11]

Figure 1 provides a schematic of the different phases of the competition. I summarize the key aspects of these in the text, with additional details provided in Appendix 2.

2.1 Eligibility and the First Stage Application

To be eligible for the program, applicants had to be Nigerian citizens aged 40 or younger, proposing the creation of a new or expansion of an existing business venture within Nigeria that would employ Nigerians, and which did not involve the production or distribution of weapons, alcohol, tobacco or gambling. They then needed to register on a website and submit an application by November 25, 2011. This application included basic personal information, proof of age and nationality, the proposed location of the business, along with information about their business idea (appendix 2 provides further details). Applicants were divided into six regional pools on the basis of the geo-political zone in which they wanted to do business, with training also taking place in these different regional locations. 23,844 applications were received, of which 3,614 (15%) were for existing business expansions and 20,230 were for new businesses. A minority of applicants were female: 18.5 percent of new business applicants, and 14.9 percent of existing business applicants were female.

2.2 Application Scoring

Applications were scored by the Enterprise Development Center (EDC) of the Pan-African University, a sister institution of the Lagos Business School. Names and other identifying information were removed before scoring to increase the impartiality of this process. Marks were given for the quality and viability of the business idea, the amount of job creation likely, the abilities and market understanding of the applicant, and the passion and commitment displayed in the application, and the likelihood of the proposed business succeeding. This first stage marking was a rapid assessment (taking approximately 10 minutes per plan) given the vast number of applications and the short window for assessment before training was to start. There was a

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[11] A pool of volunteer mentors were available, although as we will see, the winners appear to have received no more mentoring than the control group. The additional training consisted of two 2-day bootcamps run by the UK school for start-ups covering cashflow management, sales, purchasing, and team management.

conscious decision to favor existing businesses over new businesses throughout the competition given the assumption that they had at least made the first step of establishing a business. An algorithm was then used to select 6,000 candidates for the business plan training on the basis of location, type of business (new or existing), and score.

2.3 Business Plan Training

Business plan training took place between December 6 and December 20, 2011. Each of the six regions had a local training location. Of the 6,000 individuals selected to attend training, 4,873 attended (81%). The rapid roll-out of the program and short amount of time (1-2 weeks) between notification of selection into the 4-day training session and it taking place is a likely reason for non-attendance. Biometric data (iris scans and fingerprints) were collected on all attendees to enable accurate identification of individuals. Candidates who did not attend training were not eligible to submit a business plan. Appendix 2 provides detail on the content of the course.

2.4 Business Plan Submission and Business Plan Scoring

Applicants who had attended the training had until January 22, 2012 to submit the business plan. The business plan required much more detail than the first round concept note. In total 4,510 business plan applications were received, which is 92.5 percent of those who attended the training. The business plans were then scored by a joint EDC/PriceWaterCoopers team, with a team of 20 markers split evenly between the two organizations. Marking criteria were developed which focused on the market, management skills, business experience, articulating the risks and financing needs of the business, and job creation and cash flow prospects. A typical plan took 30-45 minutes to mark.

2.5 Selection of Winners

The business plans were narrowed down to 2,400 semi-finalists by first selecting the highest scoring 450 existing businesses and highest scoring 150 new businesses nationwide, and then selecting the top scoring 300 plans per zone after taking out these national top scores. Then from these 2,400 semi-finalists, the following groups of winners were selected:

- **300 National Merit Winners:** the 225 top-scoring existing business plans and 75 top-scoring new business plans nation-wide were selected as national winners. This was done to ensure the highest-scoring proposals on pure merit were selected.

- **180 Zonal Merit Winners (30 per region):** after removing the 300 national merit winners, the highest-scoring 45 existing business plans and 15 new business plans per zone were selected as zonal finalists. Their proposals were reviewed by panelists of local entrepreneurs and local business leaders in each zone, who identified the best 30 out of the 60 zonal finalists to be zonal merit award winners. The zonal finalists who were not selected were then placed back into the pool, and were eligible to be selected as ordinary merit winners.

- **720 Ordinary Merit Winners (120 per region):** 120 ordinary winners per region were randomly selected from among the remaining 1920 semi-finalists using the lottery procedure described below.

2.6 Randomization Algorithm

Random selection of the ordinary winners was done for two main reasons. First, from an operational point of view, a competition run at this scale is not feasible to do with presentations in front of judges, and it was recognized that there would be inherent uncertainty in the scoring as to which applicants were best amongst the semi-finalists. Moreover, in many developing countries there are concerns that programs get captured by individuals with certain political or ethnic ties, and it was felt randomizing gives people a fair chance and is less subject to this perception risk. Second, random assignment enabled rigorous measurement of the program's impacts.

The randomization procedure was designed as follows. First, among the semi-finalists, all those with business plan total scores below 30 were dropped, to maintain a minimum standard. This reduced the pool from 1,920 to 1,841 firms. Then a two-step stratified randomization was conducted in STATA to choose the ordinary winners:

1. First, within each region, half of the existing firm business plans were randomly chosen to be ordinary winners, with this selection stratified by gender. This reflected the preference of the government to again oversample existing business plans.

2. Then the number of slots for new firm business plans was calculated per zone as 120 minus the number of existing business ordinary winners. This was subdivided into a number of slots for new businesses operated by men and new businesses operated by women in proportion to the share of women among new businesses in the pool in each region. Then

the number of new firms dictated by the number of remaining slots was randomly chosen from the number of new firm semi-finalists in each region and gender strata.

This gives an ordinary winner treatment group of 720 firms and an ordinary winner control group of 1,121 firms. After quality checking which disqualified some winners, and redrawing of replacements (see appendix 2), of the 1,841 firms in the ordinary winner pool, we have a treatment group of 729 randomly assigned to treatment (of which 13 were disqualified). The control group consists of the 1,112 firms randomly assigned to the control group (after the random replacement), of which 9 were non-randomly selected to actually receive treatment. In terms of the impact evaluation, this will be handled through assignment to treat analysis.

2.7 Award Amounts and Payment

The amounts awarded were payable in four tranche payments. The first payment was typically around 10 percent of the total amount, with a second tranche payment averaging 45 percent of the total then payable for the purpose of physical capital acquisition and working capital. The third and four tranche payments were made conditional on employment and sales turnover triggers that were set individually for each firm based on their business plan projections. These triggers were deliberately set to be quite low, and were intended to ensure the business was set up and producing. The third tranche payment required sales turnover to have reached 25 percent of the first-year target annualized turnover set out in the business plan, with firm-specific employment triggers that averaged 3.7 workers. The fourth tranche payment required sales turnover to have reached 40 percent of the first-year annualized turnover goal, with firm-specific employment triggers that averaged 5.5 workers.

Appendix 3 summarizes the award amounts received by the winners. The mean (median) winner received a total award of US$49,000 (US$57,000). Only 6 of the 1,200 winners received no payment (due to being disqualified, or in one case, withdrawing). 1168 winners received all four tranche payments, but as appendix Table 1 shows, the length of time taken to receive all four tranches varied across firms: 748 of the firms received their last tranche payment in June or July 2013, while the last 33 recipients got their last tranche in January 2014.

3. Verification of Random Assignment and Data Collection

3.1 Verification of Randomization and Characteristics of Winning Firms

Table 1 presents summary statistics for the treatment and control groups according to assignment to treatment status for new and existing firms, as well as the characteristics of the non-experimental winners. The goal of the randomization is to ensure that the treatment and control groups are similar in terms of average characteristics. We test this formally through an F-test of joint orthogonality, which tests whether the observable characteristics in Table 1 are jointly unrelated to treatment status. We cannot reject this null hypothesis, confirming that randomization has delivered balanced groups. In contrast we reject that the observable characteristics of the national and zonal winners are similar to those of the experimental sample, even when we exclude the business plan scores used to separate the groups.

Table 1 is also useful for showing some basic characteristics of the experimental sample. We see the average existing firm owner is male, aged 32, with 4 years of business experience and running a business with a median of 5 workers. Half are married, and 10 percent have previously worked or lived abroad, and 65 percent have university education. Only 7 percent have ever had a formal loan before for their business. New applicants are slightly younger, with an average age of 29, 70 percent have university education, and they have higher business plan scores than the existing firm applicants on average, reflecting the preference for existing firm applicants at each stage. We see they come from relatively well-off households, with 85 percent having a computer at home and two-thirds having a satellite dish. The most common business sectors are manufacturing and crop and animal production – sector is looked at in more detail below.

The mean (median) annual turnover for existing businesses in the experimental sample is 5.6 million Naira (1.5 million Naira). The maximum grant of 10 million Naira thus represents two years of turnover for the mean firm and more than 6 years for the median firm.

Appendix 4 summarizes the main sectors that YouWiN!! winners propose to operate their businesses in, based on the self-classification of applicants at the time of submitting their business plans. Agricultural production activities is the largest sector, accounting for one-third of winning new business applications and 20 percent of existing business applications. Typical examples include plans to set up or expand poultry or catfish farms, and production of cashews, palm oil, and cassava. The second most common sector for existing firms is IT and computer services,

comprising 17 percent of existing firm winners, but only 7 percent of new firm winners. Typical examples include software and website development, repairs and installation, and computer training courses. Manufacturing is the second most common sector for new firms, comprising 13 percent, and third most common for existing firms, comprising 14 percent of winners. The products being manufactured are very heterogeneous, and include processed food products, books and media, metal products, chemicals and detergents, and a range of other products. Other industries includes a variety of different activities such as advertising, teaching, event management, photography and film-making, as well as some activities that were a mix of other categories.

3.2 Data Collection

Our follow-up surveys targeted a total of 3,139 individuals comprised of four groups who had applied to the first round of the YouWiN! competition:

- 475 individuals selected as national or zonal winners of the competition, based on having the highest absolute scores on their business plans, or highest scores for their region. These are referred to as the *national and zonal winner sample.* Note this consists of all national and zonal winners except for the 5 disqualified plans.

- 729 individuals who were selected as ordinary winners of the competition through a random draw among individuals with the next highest scores. These are referred to as the *experimental treatment sample.* This does include individuals originally allocated to treatment who were subsequently disqualified.

- 1,112 individuals who had similar business plan scores as the ordinary winners, but were not selected in the random draw. These are referred to as the *experimental control sample.* Note 9 of these actually received treatment.

- 823 individuals who applied for the YouWiN! competition and had first round application scores just on either side of the cut-offs used to select people to attend the 4-day business plan training course. This group is referred to as the *regression-discontinuity booster sample.* Appendix 5 discusses this sample in more detail, which is used to examine whether the business plan training by itself had any effect.

3.3 Follow-up Surveys and Attrition

Three follow-up surveys were taken, approximately at yearly intervals after individuals had applied for the program. Surveys took place nationwide via face-to-face interviews, supplemented by telephone interviews and web interviews when security concerns prevented travel, and to boost response rates. Surveys were conducted by TNS RMS Nigeria. In addition, an attempt was made through proxy interviews and two question interviews with refusals to ascertain whether individuals who could not be contacted or had refused were currently operating a business, and if so, the number of employees they had.

The first follow-up survey took place between November 2012 and May 2013. The survey took place approximately one year after individuals had first applied to the program, 8 months or more after the winners had been announced, and an average of 5 to 6 months since the winners received their first tranche payment. The median firm had received 4,000,000 Naira ($US25,000) by the time of this first survey. Overall 2,323 interviews were completed, representing an overall response rate of 74 percent, with data on whether the business was in operation available for 2,420 individuals. Appendix Table 6 summarizes the response rates by group, breaking these down also by new and existing firms. Response rates were highest for the YouWiN! winners, with 84.6 percent of the experimental treatment group and 83.4% of the national and zonal winners providing at least information on whether the business was operating or not. The response rate was 75 percent for the experimental control group, and only 69 percent for the regression discontinuity booster sample, reflecting that individuals who had not benefited as much from the YouWiN! program were more reluctant to participate. We use Lee (2009) bounds to show robustness to this differential attrition by treatment status.

The second follow-up survey took place between October 2013 and February 2014, approximately two years after application and just as firms had received their last tranche payments. This was an even more intensive effort in data collection, and succeeded in completing 2,712 interviews, and getting information on business ownership and employees for 2,777 individuals (88.5%). In particular, in this round we were able to greatly close the gap between the treatment and control

firms, with data on enterprise ownership available for 93.0 percent of the experimental winners and 90.6 percent of the experimental control group.[12]

The third and final follow-up survey took place between September 2014 and February 2015. This corresponds to three years after application, and between 12 and 18 months after firms had received their last tranche payment from the program. 2,618 interviews were completed (83.4%) with information on business ownership and employees for 2,661 individuals. Data on enterprise ownership are available for 88.5 percent of the experimental winners and 82.5 percent of the experimental control group.

4. Measuring the Impact of the Business Plan Competition: Theory and Empirical Approach

The main objective of the YouWiN! program was to generate jobs by encouraging and supporting aspiring entrepreneurial youth in Nigeria to develop and execute business ideas. We discuss first what theory suggests the likely impacts of the program will be, and then our empirical approach to measuring these impacts. Both the theory and empirical approach set out here were pre-specified in advance through a registered pre-analysis plan.[13]

4.1 Theory: How might participating in the YouWiN! program lead to more jobs and higher profits?

Consider a simple model where a firm's production Y is a function $f(.)$ of their productivity A, their capital stock K, the owner's entrepreneurial skill E, and outside labor L. The firm owner's problem is to choose K and L given A and E.

$$Y = f(A, K, E, L) \tag{1}$$

With complete markets the firm production decision will be separable from the household consumption decision and firms will choose capital and labor such that their marginal products are equal to the market interest rate and market wage rate respectively:

$$f_K(A, K^*, E, L^*) = r \tag{2}$$

$$f_L(A, K^*, E, L^*) = w \tag{3}$$

Case 1: Perfect markets, YouWiN! program is just a grant

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[12] Moreover, some of those not interviewed in round 2 were interviewed in round 1, so there are only 4 percent of the experimental sample for which we have no follow-up data in either of the first two rounds.

[13] See https://www.socialscienceregistry.org/docs/analysisplan/25/document

If firms are not credit-constrained and the program just changes the resources firm owners have available to them, then there is no change in the first-order conditions (2) and (3), and so no change in employment or output. The grant will merely make the owner richer, but not change their production decisions.

Case 2: Perfect markets, YouWiN! program is a conditional grant

The YouWiN! program does not make a single lump sum grant to firm owners, but instead is payable in tranches, conditional on the firm owner taking certain actions – with the first and second tranches typically paying for more working capital and investment, and the third and fourth tranches being triggered by reaching jobs and turnover triggers. This conditionality does not fundamentally change the equilibrium first-order conditions, but can be viewed as causing a temporary increase in the returns to capital and labor in the firm- therefore we would predict a short-term increase in capital and labor, which would then dissipate once all the tranche payments have been received. The grants then should crowd out other investments the owners would have otherwise made to start or expand firms, and once the final payments are received, we should see no difference between treatment and control firms as the treated firms reduce capital and labor to get back to the equilibrium levels.[14]

Case 3: YouWiN! program is more than just a grant

It is possible that participating in, and especially winning, the YouWiN! program may also have other impacts on the productivity of the firm (*A*), and the skills of the owner (*E*). Appendix 7 discusses several such channels including training increasing skills, network and mentoring effects, improvements in confidence and attitudes, and changes in interactions with government and family.

Case 4: YouWiN! program with capital and labor market constraints

If firms are credit-constrained, then they invest less in their firms than optimal according to (2). Winning the YouWiN! program could reduce credit constraints in three ways:

    (i)   Directly by providing a grant to the firm

    (ii)   Indirectly, through providing a signal of quality that leads to more bank lending

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[14] An exception to this prediction will occur if there is a flypaper effect, as in Fafchamps et al. (2014). In this case, firm owners may have sub-optimally low levels of capital invested in their business as a result of self-control problems or external pressure, and the program may overcome these issues by directing capital into mental accounts for business use. In this case there may be a long-term level effect on capital and profits.

(iii)   Indirectly, through providing co-financing and a signal of quality that leads to more outside investments from partners.

The impact of these channels will be to increase capital stock. This may increase or decrease labor depending on the shape of the production function – a heavily credit-constrained firm may have previously substituted capital for labor, and so reduce workers once it can buy machines to replace them. Conversely, if capital and labor are complements in production, more capital will enable the firm to hire more workers. Profits should increase in either case.

This simple model therefore offers both potential reasons why winning the program may have no impact on employment and profits, as well as potential reasons why it may. It highlights key intermediate channels affecting A and E, and access to K that we should examine in order to understand the mechanisms through which the program is or is not having an effect. This guides the empirical analysis below.

4.2 Experimental Estimation

The main approach used for evaluating the impact of the program is to use the randomized controlled trial (RCT) based on the random selection of ordinary winners from among the semi-finalists. This is done separately for the new and existing business applicants, and involves regressions of the following form:

$$Outcome_i = a + b * AssignTreat_i + c * Region * Gender_i + \varepsilon_i \quad (4)$$

Here *AssignTreat* denotes whether or not applicant i was randomly chosen as an ordinary winner from among the semi-finalist experimental pool, and *region*gender* controls for the randomization strata. Robust (Huber-White) standard errors are used. The coefficient b then gives the average effect of being assigned to receive a grant, the ITT estimate. Recall that 13 of the 729 winners were disqualified and so did not receive a grant, while 9 of the 1,112 control firms were non-randomly selected to replace them. Since these compliance rates with treatment are so high, the local average treatment effects of actually receiving the grant are similar to the intent-to-treat effects, and so we report just the ITTs. In order to estimate the trajectory of impacts, we estimate the treatment effects separately by year. In addition, to improve precision (McKenzie, 2012) for estimating the impact on financial outcomes, we also report results pooling together the second and third survey rounds to give an average effect after the full receipt of the grant.

Our main outcomes of interest are whether or not the business starts up and survives, the employment in this business, and its profitability. We then also examine the intermediate channels of changes in *A, E,* and access to *K*, as well as changes in business inputs, business practices, and innovation.

4.3 Non-experimental estimation

The experimental estimation gives the impact of winning the program (and being assigned to receive a large grant) for the semi-finalists in the experimental pool. To measure the impact of winning on the national and zonal winners, I employ propensity-score matching. I match this winner group to the experimental control group on the variables in Table 1 (apart from the first round application mark and business plan score), and restrict analysis to observations within the common support. The propensity score specification is contained in the pre-analysis plan and was pre-determined and pre-coded before observing any follow-up data. It matches on gender, age, marital status, education, international migration experience, risk attitude, household wealth, and type of sector the individual proposes having a business in. The conditions are relatively promising for propensity score matching to be reliable, since both the winners and the control group selected into the program at the same time, had already survived screening on the initial application, and have similar observable backgrounds. However, since these winners were judged to have better prospects than the experimental group, propensity score matching may deliver an upper bound on the effectiveness of the program for this group if they differ in unobservable determinants of success. Secondly, in appendix 5, I use regression-discontinuity analysis to test for an effect of participating in the 4-day business plan training program. This is estimated for the sample with initial application scores within 5 points on either side of the cut-off for selection. This analysis shows no significant impacts of the training on either the likelihood of starting a business, or on employment. This is consistent with other global evidence showing limited impacts of short business training courses (McKenzie and Woodruff, 2014).

5. Results of the Impacts of the Business Plan Competition

I start with impacts on the main outcomes of start-up, survival, employment, and profitability, before examining the intermediate channels of impact. Appendix 8 provides details on how key outcomes were measured.

5.1 Impacts on Start-up and Survival

Table 2 examines the impact of the competition on whether applicants for new enterprises set up a business and have it subsequently survive (panel A), and whether applicants with existing enterprises have these businesses survive (panel B). Consider first the impacts on new enterprises. 55 percent of those in the control group were operating a business at the time of the first follow-up survey, and 56.9 percent at the time of the second follow-up survey, and 54 percent in the last survey. The experimental estimate of the impact is 21.3 percentage points at the time of the first survey, 35.8 percentage points by the second survey, and 37.3 percentage points by the last survey. These effects are also large, positive, and remain statistically significant when bounding approaches are used to account for differential attrition. The impact for national and zonal winners, estimated via propensity score matching, is slightly larger in each survey round, although the confidence intervals overlap. Panel B shows that winning also increases the survival rates of existing firm owners. 87 percent of the control firms survived 1 year, 84.4 percent two years, and 75.9 percent three years. Winners had 8 percentage point higher survival rates in the first year, 13 percentage point higher survival rates in the second year, and 20 percentage point higher survival rates in the third year. The results are similar for the experimental and propensity score estimates.

Although treatment has large impacts on the likelihood the winners are operating a business relative to the controls, the observable characteristics of the winners operating a firm and the control group operating a firm are similar (appendix 9). In particular, winning does not appears to have resulted in a change in the sectors individuals operate in, nor in the education and initial wealth profiles of the entrepreneurs who operate businesses.

5.2 Impacts on Employment

Generating employment was one of the main goals of the program. Our main measures of employment come from survey reports. The surveys asked detailed questions on employment in different categories, and questions on the most recent workers hired. Appendix 10 compares these reports to administrative data on the winners that came from program reports at around the same time as the second survey. The employment reported to the program is larger than that reported to our surveys in the majority of cases, and appears that the administrative data suffers from over-

reporting biases. In contrast, comparison of our survey measures with physical observation where available suggests more confidence in the survey measures. I also return to this issue in further robustness checks in Section 6.

Table 3 reports the impact of winning on key employment outcomes, while Figure 2 shows the CDFs of total employment in round 3 by treatment status. These are unconditional estimates, which code employment as zero for individuals not operating firms. Appendix 11 shows estimates conditional on the firm being in operation, and also reports estimates on other measures of employment that were contained in the pre-analysis plan.

The first and fifth columns of Table 3 consider the impact on the employment of the owner for new firm applicants and existing firm applicants respectively. We see increases in these own employment rates, with the difference between these estimates and the impacts on start-up and survival in Table 2 reflecting that some individuals would have been employed as wage workers had they not started or continued with their firms. Columns 2 and 6 consider total employment in the firm, which is the sum of the owner's employment in the firm, the number of wage and salary workers, and the number of daily and casual workers in the firm. The average control group firm among new applicants has 3.7 workers by the time of the third survey, with the treatment effect of 5.2 workers more than doubling this average. The average control group firm has 5.6 workers by the time of the third survey, with the treatment effect of 4.4 workers representing an 80 percent increase. Impacts are larger in the second and third years once all the grant had been received than in the first year. Appendix 11 shows these increases in employment come from both more wage and salary workers, and from more casual and daily workers, with little change in unpaid workers.

Our survey data asks more detailed questions on the three most recently hired workers each year. Only 5 percent of these are related to the owner, 33 percent are female, 45 percent have post-secondary education, and their average wage is 22,400 Naira/month (approximately US$143). The third-round survey also asked what these workers were doing before getting hired. Only 36 percent were working in another job, with the rest either unemployed or students.

Columns 3 and 6 then examine the extent to which winning the competition has enabled firms to surpass the 10 worker threshold.[15] Amongst new firm applicants, we see that only 11 percent of the control group had reached this size three years after applying, with treatment increasing this by 22.9 percentage points. Among existing firms, 17 percent of the control group were at this size after 3 years, with the treatment taking a further 20.6 percent to this level. Appendix 11 shows these impacts are robust to the use of Lee bounds for attrition. Few firms have grown to the size of having 25 workers, but columns 4 and 8 of Table 3 show that by the third round the treatment has had a statistically significant 2.5-2.7 percentage point increase in this likelihood, relative to a control mean of only 1.4 percent.

The propensity score matching estimates show larger impacts on employment for the national and zonal winners, with an estimated impact of 7.0 workers per firm for new applicants, and 7.3 workers per firm for the existing applicants. Figure 2 shows a separation at the top of the distribution between the experimental winners and these other winners. However, we cannot say whether this larger impact reflects selection on un-observables (that the very top scoring proposals are from individuals who would have grown larger firms even without the intervention), or whether it reflects the intervention being more successful for these firms with the highest scores. We will examine this further when it comes to testing for heterogeneity in the treatment effects.

Table 4 combines our estimates of the total employment effects to calculate the overall difference in employment created between the winners and the control group. The second column shows the amount of employment reported to the program according to administrative records at the time of the second follow-up survey. This total, at 23,781, is about 10,000 workers higher than what firms report to us in the surveys. As the time of the first follow-up survey, there were a total of 9,027 workers (including the owner) employed in the winning firms. Of this, 2,495 workers is the additional impact of the program, whereas the other 6,532 would have been employed anyway. Total employment in the winning firms grew in the second year to 13,945, before falling slightly to 12,728 in the third year. The treatment effect is estimated to be 6,777 workers in the second

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[15] Note this variable was not included in the pre-analysis plan. It was motivated by the definition of 10 workers as a cutoff between micro and small businesses in many countries, and by the work of Hsieh and Olken (2014) noting this threshold as one few firms in developing countries exceed. The CDFs in Figure 2 show that the results would be similar if we instead chose another binary threshold under 15 or 20 workers.

year, and 7,027 in the third year, so that by the third year, approximately 55 percent of all employment in the firm is attributable to the program.

This comparison of the treatment and control groups reflects the causal impact of the program on the difference in employment between the two groups. In order for this to reflect the overall impact on the economy, we need to make the following further assumptions: i) any wage job a YouWiN! winner leaves or doesn't take up is filled by someone else. If not, we should use the impacts on employment rather than on business ownership for the applicants themselves, which would lower the estimated employment creation by around 100 jobs; ii) YouWiN! firms do not destroy or generate jobs in other firms outside the experimental sample. If the winners compete with other Nigerian firms and cause these firms to shut down or not expand as rapidly, the overall impact on employment is less. Conversely, if the firms provide complementary services that allow other firms to grow faster, the overall employment impact would be greater. I assume these two channels offset each other so that the first-order effect is zero here (but return to this issue in Section 6); and iii) the YouWiN! competition does not generate additional jobs through exciting non-winners to start businesses. It is possible the publicity and attention given to entrepreneurship motivates others to start businesses. Finally, note that this employment impact is the direct impact, and does not include any multiplier effects induced by the firms increasing demand for products of other firms, and by the firm owners and their employees increasing consumption of products made by other firms.

5.3 Cost Effectiveness for Employment Generation

The first round gave the winners 9,240 million Naira (US$58 million) in grants. The cost of administering the first round of program including scoring the applications, running the 4-day business plan training, and administering the tranche payments is estimated at $2 million. At the end of the third year, the estimate from Table 4 is that 7,027 jobs were directly generated as a result of the program. Thus the cost per job created is US$8,538. The average wages of the jobs created in these firms is US$143 per month according to our survey reports, so the cost per job is equivalent to approximately 60 months of employment.

By way of comparison, the White House Council of Economic Advisors estimates that the cost per job-year created for different types of fiscal stimulus in the United States range from $92,136 for government spending to $145,351 for tax cuts.[16] Aggregating the job impacts over the three years in Nigeria, we have a cost of $3606 per job-year. Scaling for per-capita GDP differences would take this to $64,000. Thus even under the extreme assumption that all of the jobs created completely disappeared after our surveys, this program would be more effective at creating jobs than fiscal stimulus in the U.S.

The cost per job created also compares favorably to many job creation policy efforts in developing countries, which have struggled to find significant effects on employment. Appendix 12 provides some examples. The estimated cost per job created by wage subsidy and vocational training programs ranges from $11,000 to $80,000, with many of these studies not finding statistically significant impacts. Most studies of finance and training programs for microenterprises have at best found impacts on generating self-employment, but often offset by a reduction in wage employment of the potential entrepreneur (McKenzie and Woodruff, 2014), with little discernable effect on paid employment (Grimm and Paffhausen, 2015). Management consulting services to larger firms have also often not led to jobs, with the one study that has found impacts only finding these over the medium-term with administrative data on formal employment (Bruhn et al, 2013). Blattman and Ralston (2015) also highlight the limited success of microfinance, insurance, and several other policies in creating employment. Moreover, the YouWiN! program has other important impacts other than just direct job creation. I turn next to examining these other impacts.

5.4 Impacts on Innovation and Business Practices

The other stated objective of the program was to encourage innovation. Appendix 13 examines whether YouWiN! winners are employing better business practices in their firms, and whether they are innovating more. There is a positive and significant impact on innovation, with a 22 percentage point increase in innovative activities for experimental winners among new firms, and 14 percentage point increase for existing firms. The new applicants are innovating more in multiple dimensions, introducing new products, processes, pricing methods, quality control systems, using the internet, and using new channels for selling goods. There is also a positive and significant

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[16] https://www.whitehouse.gov/administration/eop/cea/Estimate-of-Job-Creation/ [accessed July 29, 2015].

impact on the use of better business practices, but most of this effect appears to be coming through the extensive margin, with only minor improvements conditional on survival.

5.5 Impacts on Profits and Sales

Table 5 reports the estimated impacts of winning the business plan competition on several measures of sales and profitability.[17] Appendix 14 shows the CDFs of profits and sales, impacts conditional on the firm being in operation, and impacts on other pre-specified measures of sales and profitability.

The CDFs show a clear separation in the distributions of the treatment and control groups profits and sales over most of the distribution, indicating larger profits and sales for the treatment than the control at most quantiles. However, they also illustrate two important features of the data that make it more difficult to estimate the treatment effect. The first is the high dispersion of the data, with some large values. The second is the presence of many zeros, and even negative profits. We use three pre-specified approaches to deal with these issues. The first is to truncate monthly profits and sales at the 99th percentile to reduce the influence of outliers. As a second alternative approach to reducing the influence of the upper tail we also consider the inverse hyperbolic sine transformation of profits, which is similar to a log transformation, but allows for zeros and negative values. Third, we use an overall index of standardized z-scores which considers multiple profit and sales measures[18] and allows us to see whether there is an effect in a common direction.

Consider first the impacts for new firms, shown in the first four columns of Table 5. In the first year the impact on the inverse hyperbolic sine of profits is positive and significant, but the impact on our other measures is not statistically significant. The impacts are larger in the second and third year follow-ups, and when we pool together to provide an average effect over these two years, are statistically significant for all measures. The estimated increase in monthly sales of 185,000 Naira represents a 32 percent increase on the control mean, while the estimated increase in monthly

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[17] Nominal Naira were converted into real (November 2012) Naira using the Consumer CPI of the Central Bank of Nigeria.
[18] These include other measures like asking for annual sales, whether sales were higher than one year ago, and profits in the best month. In the first two rounds we also asked for the number of customers and details of sales of the main product. Firms found these questions difficult to answer, and they were dropped for the third survey round and are not included in the aggregate index here (but are in the appendix).

profits of 45,608 Naira represents a 23 percent increase on the control mean. The impacts are larger for lower quartiles in the CDF, so the inverse hyperbolic since transform shows a larger effect. Appendix 14 suggests that a large part of this impact is occurring through the extensive margin of allowing firms to operate and earn profits that otherwise would not exist.

The last four columns of Table 5 show the impacts for existing firms. As with the new firms, the impacts are stronger in years 2 and 3 than in year 1, and are statistically significant for all four measures in the pooled specification. The estimated increase in monthly sales of 336,000 Naira represents a 63 percent increase on the control mean, while the estimated increase in monthly profits of 50,000 Naira represents a 25 percent increase. Again this impact appears to be coming to an important extent through the extensive margin, with appendix 14 showing no significant difference in profits and sales conditional on being in operation in the pooled specifications. The CDFs show the national and zonal winners appear to be more profitable and selling more than the experimental winners at many points of the distribution. Consistent with this, the propensity score matching estimates exceed the experimental estimates for both new and existing applicants.

5.6 Mechanisms

The results show that winning the competition resulted in sizeable increases in employment which persisted after all grant money had been received, and that this increase in employment was profitable for firms. This contrasts with cases 1 and 2 of section 4.1 which predicted that either there would be no employment effect, or that it would reverse once all the grant money had been received. I therefore turn to examining the impacts on intermediate channels to help understand the mechanisms behind this employment effect.

Appendix 15 examines how winning the program affects determinants of productivity (A), entrepreneurial skill (E), and access to capital (K). There is no significant change in entrepreneurial self-efficacy, which measures the owner's self-confidence in their ability to carry out 12 business-related actions such as "estimate customer demand for a new product", and "identify good employees who can help the business grow". This also helps rule out the concern that the control group has become less self-confident as a result of not winning. There is also no change in the likelihood the firm owner has a business mentor, or in the number of other firm owner's they

discuss business with. These suggest the main mechanisms are not through more confidence and connections.

Winning firms are found to be more likely to be formally registered, but not more likely to access other sources of finance: there is no significant change in the likelihood of receiving investments from outside equity partners, and a small, but statistically significant reduction in the likelihood of getting a formal loan in the year prior to the second round survey for existing firms. Thus the grant does not appear to have crowded in other sources of finance. Appendix 15 also provides suggestive evidence that winning firms were no more likely to have lowered the price of their main product than the control group, suggesting the mechanism for growth was not through price competition. In contrast, appendix 15 shows sizeable impacts on input use by firms. Owners are devoting more hours to their businesses, and we have already seen they use more labor in the form of employees. There is no more use of outside consultants.

Appendix 16 examines the impact of winning on the input use of existing firms. Column 1 shows that owners are devoting more hours to their businesses. Recall we have already seen firms are using more labor in the form of employees. They have higher inventory levels, are more likely to have made a purchase of business equipment, land or buildings of over 100,000 Naira, and have spent more on such purchases. Taken together, the aggregate outcome index shows treated firms are using more inputs.

Whilst suggestive, these results suggest that the main mechanism for firms growing appears to be firms using the grants provided by the competition to purchase more capital inputs and hire more labor. Table 6 estimates the implied return to capital from this, regressing monthly profits on capital stock instrumented by assignment to treatment. The estimated real return to capital is 1.4 to 1.6 percent per month, equivalent to a compounded annual return of 18 to 21 percent.[19] As a comparison, real interest rates for SME finance are around 20 percent per annum. So the return on capital is at the level needed to pay for a loan were credit available. However, such financing is

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[19] Note this includes both the return to capital and the return to the owner's own additional labor hours to the extent the owner does not pay him or herself a salary.

unlikely to be offered to most start-ups, and only 2 percent of the control group of new firms and 6 percent of the control group of existing firms received a loan in any given year.

6. Robustness to Common Concerns about Firm Experiments
There are several general issues that affect almost all firm experiments in developing countries: the use of self-reported data, the extent to which impacts arise from general equilibrium factors, and the extent to which the results generalize. I discuss each in turn.[20]

6.1 Self-reporting

While the use of self-reported data from surveys is standard for firm experiments in developing countries, readers may be concerned whether such data are reliable. A first point to note is that administrative data on firms are rare in most developing countries, and there is a growing body of evidence to show that firms systematically mis-report their revenues, costs, and wages to the authorities (Pomeranz, 2015; Carrillo et al, 2014; Kumler et al, 2015), so that it is not clear that one should prefer administrative data over survey data even if it were available. Indeed the analysis in appendix 10 suggests that the administrative data on employment for the business plan competition winners is unreliable in our specific case.

Second, the survey was designed and conducted in a manner to reduce reporting bias. It was conducted by a well-known independent market research company (TNS RMS Nigeria) and participants were told its "purpose is to help better understand the growth process of enterprises being run by Nigerian youth, as well as the development process of youths thinking about starting a business", with participants told the results would be kept confidential and used for research purposes only. The first round survey asked about a variety of government programs, and not just YouWin! Together these features are likely to make the survey responses less biased than if the data were collected in an official capacity by the government program implementers. Appendix 16 shows treatment does not have significant impacts on errors in reporting sales and profits.

Finally, although we need to rely on survey data for the majority of our analysis, there are a couple of checks of business size that we can do that are not based on self-reports. In the first and second round follow-ups, approximately 40 percent of the in-person surveys took place in the business

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[20] Note that these robustness checks were not included in the pre-analysis plan, but are a response to commonly occurring comments raised for almost all firm experiments.

(the rest taking place at the home or at a third location). In the third round we put more emphasis on trying to interview at the business premises, and 70 percent of the in-person surveys (and 57 percent of all surveys) took place at the business. For this subsample we asked the interviewers to physically count how many employees they observed at the place of business. This is an undercount of true employment because it misses workers who are working in another location (e.g. in the homes of clients, in other stores, travelling, etc.) or who are temporarily absent. Nevertheless, it does provide a check of whether observed employment is larger for the treated firms. Second, throughout the three rounds we collected information on whether the firms have a website, and collected the URL. On October 4, 2015 we tested to see whether each firm had a functioning URL. Having a functioning website is a proxy for being of a larger size three and a half years after winning.

Table 7 reports the results of these checks. YouWin! winners have significantly more observed workers and are more likely to have a functioning website for both new and existing applicants. New firms have approximately three times as many employees observed at the premises among the experimental winners than the control group, and existing firms have almost twice as many employees observed. While the absolute levels are smaller, these ratios are similar to those in Table 3. Both new and existing firms are twice as likely to have a functioning website among the experimental winners as in the control group. While limited, these checks do help provide assurance that there is a genuine increase in firm sizes for the treated firms.

6.2 Competition and Spillovers

A second potential concern may be whether the growth of the winners came at the expense of other firms. There are two elements of this concern. The first is a concern about internal validity: if the growth of the winners came at the expense of firms in the control group, the control group would no longer provide a valid counterfactual. This seems unlikely to be an important concern in our case, since the experimental sample is widely scattered over a country of 170 million people, and is not heavily concentrated in any single industry. As a check, Appendix 17 shows that there is no heterogeneity in treatment effect with the number of other firms selected as winners in the same state and industry. As a result the estimates should be internally valid, and are informative for showing the constraints facing individual businesses.

The second aspect of concern is then whether the gains to the winners came from taking business away from other firms in the economy, outside of those participating in the program. Table 8 examines whether the impacts of the program on firm survival, growth to 10 or more workers, and sales and profits vary with different measures of the level of competition firms face. Panel A focuses on existing firms, for which we have several measures of competition in the first follow-up: how many competitors exist within 2km of the business, how easily customers could replace them if they shut down, and what percent of revenues come from customers within 1km. Out of 9 interactions, only one is significant at the 10 percent level, and the coefficients vary in sign across outcomes and competition measures. We then consider for both existing and new firm applicants whether the treatment effect varies with the proportion of other semi-finalists in the competition that were in or proposed entering their industry, as a proxy for these being industries with more firms in general likely in the economy. Again we find no significant interactions. We therefore cannot reject that the impact of winning is similar for firms facing more competition as for firms facing less. Together with the evidence that firms are innovating more and introducing more products to market, and no more likely to lower prices, it may then be that the growth is coming from firms growing the overall market in an underdeveloped economy.

6.3 External Validity

A final critique that can be raised about any impact evaluation is the extent to which its results may generalize. The program studied here is a nationwide program to a large number of firms in Africa's largest country, and hence should be of intrinsic interest. Moreover, the competition has already been run two in two more rounds in Nigeria. Nevertheless, there are reasons to believe the findings may generalize beyond Nigeria. As noted in the introduction, these types of business plan competitions have become increasingly common, particularly in sub-Saharan Africa. While the amounts offered in Nigeria are high, they are far from unique – the average grant sizes in the Somalia business plan competition were $58,000 in round 1 and $67,000 in round 2, and awards of $25,000 were given in Côte d'Ivoire.   The key difference here is the large number of winners and randomized selection provide the ability to evaluate and learn from this competition, which has not been possible in the other cases. It seems likely that these competitions in other countries are also inducing applications from entrepreneurs with high growth potential. Whether such entrepreneurs can be identified in alternative ways to business plan competitions and/or be

supported through alternative policy instruments to grants are interesting questions for future research.

7. Targeting and Heterogeneity

The above results show that winning the YouWiN! competition results in higher employment, profits, and sales in the winning firms, with this impact appearing to be largely a result of the added capital. A key question for policy is then how to best target such programs. There are two aspects to this question. The first is the extent to which one can identify in advance those entrepreneurs who are most likely to grow their businesses. I examine this question in appendix 18. We have seen that the competition and initial screening process resulted in a set of firms whose owners are more educated, and which grow more than typical microenterprises. However, conditional on getting to the stage of submitting a business plan, the results suggest it is hard to determine which individuals will grow most. This is true for both the non-winners and the winners, and is consistent with the inherent risk of entrepreneurship (Hall and Woodward, 2010), and of the difficulty venture capitalists and investors have in knowing which ideas will work (Kerr et al, 2014).

The second aspect of interest for policy is the extent to which the impacts of the assistance offered by the program differ with key characteristics. Three key dimensions of heterogeneity of policy interest for the program are whether impacts are larger for new or existing firm applicants, how impacts vary with gender, and how impacts vary with the business plan score. Appendix 19 examines these. Although officials believed that supporting existing firms would generate more jobs than new firms, we find the reverse to be true. We find some heterogeneity with respect to gender, with the grants helping close a gender gap in the likelihood of starting a firm for new applicants, but no impact on closing the gender gap in the likelihood of reaching 10 workers or in sales and profits. For existing firms, female-owned existing firms are not less likely to survive than male-owned firms, and there is no significant gender treatment interaction for these firms. In the first year, the grants had more impact for new firms in the experimental group with lower business plan scores, again closing a gap that would otherwise exist. But there is no significant heterogeneity with respect to business plan score by the third round, or for existing firms. These results then also suggest that the cause of the higher estimated treatment effects for the national and zonal winners compared to the ordinary winners is likely selection, rather than winning having

more impact for them. That is, propensity-score matching, but matching only on observables, is not able to account for some of the growth that these firms would have experienced anyway.

8. Conclusions

The YouWiN! business plan competition has had large impacts on the rate of business start-up, survival of existing firms, employment, profits, and sales of winning firms. It has succeeded in generating more firms with 10 or more employees, and provides the first experimental evidence of how direct policy action can spur the growth of such firms. The impacts are not consistent with a model of no market failures in which the grants would be predicted to increase the incomes of the business owners without changing their production decisions. Examination of the intermediate channels suggests that a main effect of the program has been to enable firms to buy more capital and hire more workers, with little impact on business practices, mentoring or networking. The business plan competition seems an effective tool for identifying entrepreneurs with much greater scope for growth than the typical microenterprise. However, conditional on reaching the semi-finalist stage, the evidence also points to the difficulty of identifying who had the highest growth potential among them, and which individuals would benefit most from assistance, suggesting that these targeting issues are important areas for future research.

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Figure 1: Competition Stages and Treatment Assignment

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┌─────────────────────────────────────────────────────┐
│              First Round Application                  │
│   23,844 Firms (3,614 Existing, 20,230 New)           │
└─────────────────────────────────────────────────────┘
                          │
                          ▼
┌─────────────────────────────────────────────────────┐
│  Top 6,000 chosen for 4-day business plan training    │
│   with scoring cutoffs varying by region and          │
│   new/existing                                        │
│   6,000 Firms (1,254 Existing, 4,746 New)             │
│   4,873 attend training                               │
└─────────────────────────────────────────────────────┘
                          │
                          ▼
┌─────────────────────────────────────────────────────┐
│      Submit detailed business plan for scoring        │
│      4,510 Firms (981 Existing, 3,529 New)            │
└─────────────────────────────────────────────────────┘
                          │
                          ▼
┌─────────────────────────────────────────────────────┐
│  Semi-finalists chosen on basis of score, with        │
│   thresholds varying by region and new/existing       │
│   2,400 Firms (596 Existing, 1,804 New)               │
└─────────────────────────────────────────────────────┘
```

Non-experimental winners (selected on highest scores overall and within region, with preference for existing)	Experimental Sample (randomly selected within strata defined by region, gender, and new/existing)	Dropped if score <30
475 Firms	1841 Firms (541 Existing, 1300 New)	79 firms
357 Existing	729 Treatment (278 Existing, 451 New)	
118 New	1112 Control (263 Existing, 849 New)	Disqualified 5 firms

Follow-up Surveys	Follow-up Surveys
Follow-up 1: 396 Firms	Follow-up 1: 617 Treat, 836 Control
Follow-up 2: 439 Firms	Follow-up 2: 678 Treat, 1008 Control
Follow-up 3: 417 Firms	Follow-up 3: 645 Treat, 917 Control

Note: regression-discontinuity sample of firms around the cut-off for being chosen for business plan training not shown.

Figure 2: Cumulative Distribution Functions of Total Employment in Round 3 by Treatment Status
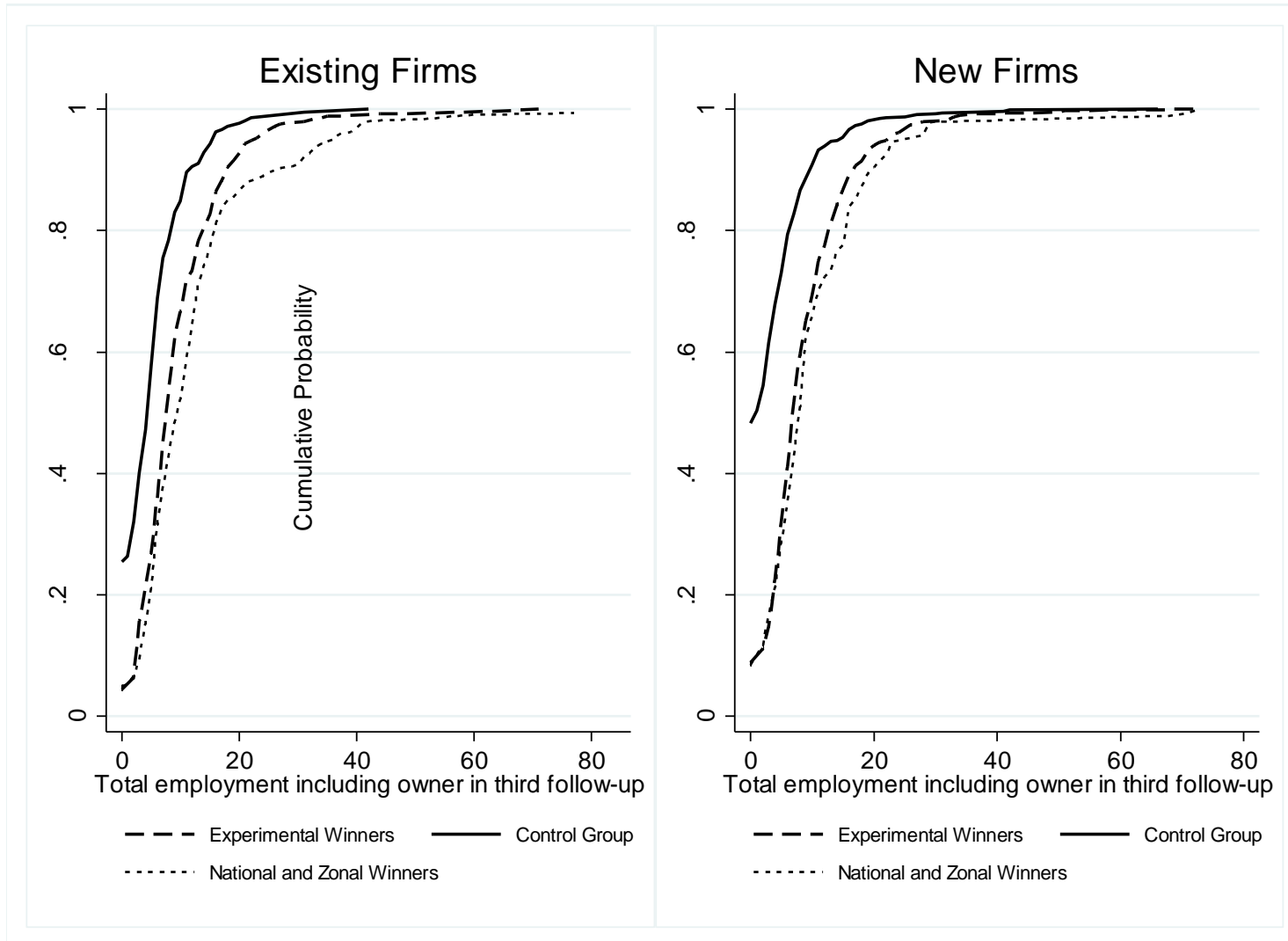
Table 1: Baseline Characteristics and Balance of Experimental Sample

	Existing Firms			New Firms		
	Non-Experimental Winners	Treatment Group	Control Group	Non-Experimental Winners	Treatment Group	Control Group
Applicant Characteristics						
Female	0.17	0.18	0.17	0.19	0.17	0.18
Age	32.5	32.0	31.8	30.1	29.3	29.6
Married	0.60	0.50	0.56	0.42	0.34	0.36
High School or Lower	0.10	0.13	0.12	0.06	0.11	0.10
University education	0.71	0.63	0.67	0.79	0.69	0.71
Postgraduate education	0.12	0.08	0.12	0.13	0.05	0.06
Lived Abroad	0.14	0.10	0.11	0.18	0.06	0.09
Choose Risky Option	0.59	0.56	0.52	0.63	0.57	0.55
Have Internet access at home	0.68	0.57	0.61	0.60	0.47	0.48
Own a Computer	0.94	0.87	0.88	0.92	0.84	0.86
Satelite Dish at home	0.74	0.67	0.71	0.64	0.68	0.64
Freezer at home	0.64	0.57	0.61	0.63	0.51	0.55
Business Characteristics						
Crop and Animal Sector	0.14	0.16	0.16	0.22	0.22	0.22
Manufacturing Sector	0.28	0.28	0.26	0.23	0.28	0.24
Trade Sector	0.05	0.06	0.05	0.06	0.04	0.04
IT Sector	0.14	0.15	0.14	0.04	0.07	0.06
First Round Application Score	59.0	57.2	56.6	59.9	59.9	59.9
Business Plan Score	61.7	45.8	45.4	74.4	53.7	55.5
Number of Workers	9.11	7.35	7.73			
Ever had Formal Loan	0.11	0.06	0.09			
Sample Size	357	278	263	118	451	849
Joint orthogonality test: treatment versus control		0.920			0.884	
Joint orthogonality test: non-experimental vs treatment	0.000			0.000		
Joint orthogonality test: non-experimental vs treatment (no score)	0.012			0.000		

Note: p-values shown for joint orthogonality tests. Test for comparison of treatment and control conditions on regional strata.

Table 2: Impact on Start-up and Survival

	Operates a firm at time of survey		
	Round 1	Round 2	Round 3
Panel A: New Firms			
Experimental Treatment Effect	0.213***	0.358***	0.373***
	(0.029)	(0.023)	(0.024)
Lee Bounds	[0.19,0.30]	[0.36, 0.38]	[0.37, 0.43]
Sample Size (Experiment)	1021	1181	1085
Control Mean	0.550	0.569	0.540
PSM Impact for National/Zonal winners	0.250***	0.414***	0.382***
	(0.040)	(0.023)	(0.035)
Sample Size (PSM)	717	838	764
Panel B: Existing Firms			
Experimental Treatment Effect	0.082***	0.130***	0.196***
	(0.027)	(0.025)	(0.031)
Lee Bounds	[0.07, 0.12]	[0.13, 0.15]	[0.19, 0.24]
Sample Size (Experiment)	432	505	477
Control Mean	0.871	0.844	0.759
PSM Impact for National/Zonal winners	0.097***	0.134***	0.200***
	(0.024)	(0.029)	(0.035)
Sample Size (PSM)	485	573	537

Notes: Robust standard errors in parentheses, *, **, *** indicate
significance at the 10, 5, and 1 percent levels respectively
PSM is propensity score matching comparing non-experimental winners
to experimental control group

Table 3: Impact on Employment

	New Firms				Existing Firms			
	Own Employment	Total Employment	Firm of 10+ workers	Firm of 25+ workers	Own Employment	Total Employment	Firm of 10+ workers	Firm of 25+ workers
Experimental impacts:								
First-Follow-up	0.074***	1.426*	0.024	0.007	0.046**	1.461*	0.055	0.007
	(0.025)	(0.732)	(0.020)	(0.008)	(0.019)	(0.808)	(0.041)	(0.019)
Second Follow-up	0.128***	6.012***	0.288***	0.022**	0.066***	2.521*	0.211***	0.008
	(0.017)	(0.412)	(0.026)	(0.009)	(0.018)	(1.366)	(0.041)	(0.018)
Third Follow-up	0.119***	5.227***	0.229***	0.025**	0.069***	4.391***	0.206***	0.027*
	(0.018)	(0.469)	(0.028)	(0.011)	(0.021)	(0.674)	(0.040)	(0.015)
PSM impacts for National/Zonal winners								
First-Follow-up	0.072**	2.707***	0.142***	0.011	0.043**	3.149***	0.102**	0.061***
	(0.036)	(0.683)	(0.042)	(0.016)	(0.021)	(0.844)	(0.050)	(0.020)
Second Follow-up	0.141***	11.162***	0.344***	0.097***	0.070***	5.739***	0.347***	0.100***
	(0.019)	(2.240)	(0.050)	(0.028)	(0.017)	(1.478)	(0.038)	(0.020)
Third Follow-up	0.137***	7.007***	0.261***	0.037*	0.072***	7.338***	0.349***	0.094***
	(0.024)	(1.277)	(0.053)	(0.022)	(0.023)	(0.784)	(0.042)	(0.018)
Control Mean: First follow-up	0.787	3.618	0.083	0.010	0.938	6.852	0.212	0.032
Control Mean: Second follow-up	0.841	3.305	0.088	0.009	0.922	8.134	0.231	0.038
Control Mean: Third follow-up	0.831	3.773	0.114	0.014	0.906	5.571	0.170	0.014
N(Experiment): First follow-up	1021	987	987	987	432	422	422	422
N(Experiment) Second follow-up	1181	1159	1159	1159	505	500	500	500
N(Experiment) Third follow-up	1085	1044	1044	1044	477	461	461	461
N(PSM): First follow-up	717	698	698	698	485	469	469	469
N(PSM) Second follow-up	838	822	822	822	573	568	568	568
N(PSM) Third follow-up	764	728	728	728	537	525	525	525

Notes: Robust standard errors in parentheses, *, **, *** indicate significance at the 10, 5, and 1 percent levels.
Experimental estimates are ITT estimates and control for randomization strata.
Propensity score matching (PSM) estimates compare non-experimental winners to experimental control group.
N(Experiment) and N(PSM) denotes the sample size for the experimental and PSM estimates respectively.

Table 4: Total Employment and Total Employment Impact in Winning Firms

	Number of Firms	Administrative Report of Employment	Total Employment in Winning Firms			Treatment Effect on Total Employment		
			Round 1	Round 2	Round 3	Round 1	Round 2	Round 3
Randomly selected winners	729	11940	4588	7183	6858	1051	3411	3579
New Firms	451	7487	2289	4209	4099	645	2711	2359
Existing Firms	278	4453	2299	2974	2759	406	701	1220
National and Zonal winners	475	11780	4439	6762	5870	1444	3366	3448
New Firms	118	3783	744	1712	1273	320	1317	827
Existing Firms	357	8009	3695	5050	4597	1125	2049	2620
All winners	1204	23781	9027	13945	12728	2495	6777	7027

Notes:

Treatment Effect are experimental estimates for randomly selected winners, and PSM estimates for other winners

Administrative Report was employment reported to the program at around the time of the Round 2 survey.

Table 5: Impacts on Business Sales and Profits

	New Firms				Existing Firms			
	Truncated Sales	Truncated Profits	Inverse Hyperbolic Sine Profits	Aggregate Index of Sales and Profits	Truncated Sales	Truncated Profits	Inverse Hyperbolic Sine Profits	Aggregate Index of Sales and Profits
Experimental impacts:								
First-Follow-up	36.160	-24.512	2.156***	0.016	50.805	0.074	0.972***	0.080
	(49.884)	(26.330)	(0.369)	(0.047)	(85.662)	(49.416)	(0.373)	(0.070)
Second Follow-up	297.783***	69.061***	4.154***	0.298***	346.304**	69.234*	2.183***	0.237***
	(56.494)	(15.150)	(0.326)	(0.036)	(134.728)	(35.420)	(0.401)	(0.060)
Third Follow-up	64.541	20.137	3.962***	0.167***	338.476**	32.035	2.580***	0.213***
	(92.338)	(21.635)	(0.346)	(0.042)	(142.812)	(40.956)	(0.464)	(0.070)
Pooled Second and Third Round Effect	185.142***	45.608***	4.062***	0.235***	336.931***	50.434*	2.357***	0.223***
	(60.642)	(14.550)	(0.264)	(0.032)	(110.685)	(30.255)	(0.338)	(0.052)
PSM impacts for National/Zonal winners								
First-Follow-up	-0.065	-55.926*	2.145***	-0.023	305.599***	67.872	1.232***	0.324***
	(69.685)	(32.838)	(0.630)	(0.058)	(96.901)	(70.883)	(0.411)	(0.091)
Second Follow-up	362.469***	152.708***	5.078***	0.442***	548.416***	119.593***	1.975***	0.333***
	(87.777)	(30.941)	(0.473)	(0.076)	(152.439)	(41.525)	(0.487)	(0.062)
Third Follow-up	279.080*	78.353**	4.617***	0.313***	654.925***	113.103**	2.346***	0.399***
	(169.253)	(38.705)	(0.578)	(0.085)	(142.289)	(51.802)	(0.584)	(0.105)
Control Mean: First follow-up	509.699	257.025	10.772	-0.045	509.699	257.025	10.772	-0.045
Control Mean: Second follow-up	660.535	206.305	9.646	-0.117	660.535	206.305	9.646	-0.117
Control Mean: Third follow-up	509.975	192.151	8.565	-0.108	509.975	192.151	8.565	-0.108
N(Experiment): First follow-up	995	995	995	995	423	423	423	423
N(Experiment) Second follow-up	1151	1150	1150	1152	497	497	497	497
N(Experiment) Third follow-up	1063	1063	1063	1063	470	469	469	470
N(PSM): First follow-up	702	702	702	702	474	474	474	474
N(PSM) Second follow-up	816	816	816	816	567	567	567	567
N(PSM) Third follow-up	749	749	749	749	530	530	530	530

Notes: Robust standard errors in parentheses, *, **, *** indicate significance at the 10, 5, and 1 percent levels.

Experimental estimates are ITT estimates and control for randomization strata.

Sales and Profits are in 1000s of real Naira per month.

Aggregate index of outcomes includes monthly sales, truncated monthly sales, annual sales, sales higher than one year ago, monthly profits, truncated monthly profits, profits in the best month, and inverse hyperbolic sine of profits.

Note pooled sample conditions on the firm being in operation in both follow-up 2 and 3.

Propensity score matching (PSM) estimates compare non-experimental winners to experimental control group.

N(Experiment) and N(PSM) denotes the sample size for the experimental and PSM estimates respectively.

Table 6: Estimated Monthly Real Return on Capital

Dependent Variable: Real Monthly Profits (truncated at the 99th percentile)

	New Firms			Existing Firms		
	Round 2	Round 3	Pooled	Round 2	Round 3	Pooled
Capital Stock (truncated at 99th percentile)	0.017***	0.015***	0.016***	0.023**	0.004	0.014*
	(0.003)	(0.005)	(0.003)	(0.010)	(0.010)	(0.008)
Sample Size	956	789	1745	381	327	708

Notes:

Robust standard errors in parentheses, clustered at the firm level for the pooled estimate which pools rounds 2 and 3 of data collection.

*, **, and *** indicate significance at the 10, 5, and 1 percent levels respectively.

Capital stock is instrumented by assignment to treatment

Table 7: Impacts on Non-Self-Reported Outcomes

	New Firms		Existing Firms	
	Employees Counted	Functioning Website	Employees Counted	Functioning Website
Assignment to Treatment	2.044***	0.040***	1.746***	0.081***
	(0.263)	(0.014)	(0.439)	(0.030)
Sample Size	860	1300	398	541
Control Mean	1.077	0.047	2.175	0.099

Notes: Robust standard errors in parentheses, *, **, *** indicate significance at the 10, 5, and 1 percent levels respectively

Table 8: Do Impacts Vary with the Level of Competition Firms Face?

	Outcomes in Third Follow-up		
	Operates a Firm	10+ workers	Sales & Profits Index
Panel A: Existing Firms			
Competition Measure 1: Only 0 or 1 competitors in 2km area of business			
Assignment to Treatment	0.123***	0.192***	0.212**
	(0.040)	(0.057)	(0.087)
Assignment to Treatment*0 or 1 competitors locally	0.138	-0.037	-0.201
	(0.087)	(0.110)	(0.188)
Sample Size	353	345	350
Mean of Interacting Variable	0.306	0.307	0.309
Competition Measure 2: Customers could not replace them in a week or less			
Assignment to Treatment	0.196***	0.157**	0.247***
	(0.052)	(0.067)	(0.088)
Assignment to Treatment*Not easily replaced	-0.071	0.059	-0.197
	(0.074)	(0.099)	(0.154)
Sample Size	353	345	350
Mean of Interacting Variable	0.496	0.490	0.494
Competition Measure 3: Percent of Revenues from customers within 1km			
Assignment to Treatment	0.157**	0.163*	-0.022
	(0.068)	(0.089)	(0.134)
Assignment to Treatment*Rely on Local Market	-0.000	0.001	0.005*
	(0.002)	(0.002)	(0.003)
Sample Size	351	343	348
Mean of Interacting Variable	35.7	36.0	35.9
Competition Measure 4: Proportion of Other Semi-finalists in same 2-digit industry			
Assignment to Treatment	0.179***	0.227***	0.225**
	(0.050)	(0.069)	(0.101)
Assignment to Treatment*Proportion in Same Industry	0.093	-0.193	-0.144
	(0.353)	(0.432)	(0.630)
Sample Size	477	461	470
Mean of Interacting Variable	0.122	0.122	0.121
Panel B: New Firms			
Competition Measure: Proportion of Other Semi-finalists in same 2-digit industry			
Assignment to Treatment	0.325***	0.187***	0.136*
	(0.041)	(0.047)	(0.076)
Assignment to Treatment*Proportion in Same Industry	0.336	0.296	0.222
	(0.233)	(0.272)	(0.433)
Sample Size	1085	1044	1063
Mean of Interacting Variable	0.142	0.143	0.142

Notes:

All regressions also include controls for randomization strata and for the level effect of the interacting variable. Robust standard errors in parentheses.

*, **, *** indicate significance at the 10, 5, and 1 percent levels respectively.

APPENDICES (ONLINE ONLY)

Appendix 1: Timeline

Appendix 1: Timeline of Project

	2011		2012												2013												2014												2015	
	N	D	J	F	M	A	M	J	J	A	S	O	N	D	J	F	M	A	M	J	J	A	S	O	N	D	J	F	M	A	M	J	J	A	S	O	N	D	J	F
Applications due	■																																							
Business Plan Training		■																																						
Business Plan Submitted			■																																					
Winners announced					■																																			
First Tranche payments								■	■	■	■	■	■																											
Second Tranche payments											■	■	■	■	■	■	■	■																						
Third Tranche payments															■	■	■	■	■	■	■	■	■	■																
Fourth Tranche payments																					■	■	■	■	■	■	■													
First follow-up survey											░	░	░	░																										
Second follow-up survey																								░	░	░	░	░	░											
Third follow-up survey																																			░	░	░	░	░	░

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Appendix 2: Additional Details on Business Plan Competition

A2.1 Launch and Outreach

The program was advertised throughout the country over different television and radio stations. Adverts were also published in the newspapers with the widest coverage. Road shows were organized by the Ministry of Youth Development and private vendors in major cities of each geo-political area of the country targeting areas with large numbers of youth eligible for the competition. Small and Medium Enterprise outreach events were also held in Lagos and Abuja.

A2.2 Information required in initial application

All applicants had to provide the following information:

- A statement as to why they want to be an entrepreneur, how they got their business idea, and why they will succeed.
- A description of their business idea, why it is innovative, what their market will be, why people will buy their products, who their competition is, how the business will make money, and the risks foreseen and how they will overcome them.

New applicants also had to provide:

- What the key steps needed to start the business are
- Description of their qualifications and experience
- How much money they need to start the business.

Existing business owners needed to provide information on:

- Years of operation, turnover, employment levels, and registration certificate number (firms did not need to be registered to apply, but if they won would need to register in order to be eligible to receive a grant).
- How much money they need to expand the business, how many more people they will employ if they do so, and what the projected annual turnover will be.

A2.3 Who Applied?

Nigeria has approximately 50 million youth aged 18 to 40. The almost 24,000 applications therefore represent only 0.05% of the overall youth population. Applicants are older on average, and more educated than the average Nigerian youth. Among the overall youth population, 5.5% have university education, compared to 52% of new business applicants and 54% of existing business applicants. Geographically we see that a higher proportion of youth applied from the North-Central region (where Abuja is located) and the South-Western region (where Lagos is located), while the North-Western region had the lowest proportion of youth.

A2.4 Content of the Four-Day Business Plan Training

Training was run by EDC with support from Plymouth Business School, and was a four day course. The goal was to provide tools and techniques that would help both in writing a business plan and in running the business. The course covered:

- The different sections of what should go into a business plan – and what sort of things funders would look for in each section
- How to find out more about the competition and competitive environment; understanding your competitors and how you can differentiate yourself
- Business plan financials – putting together a balance sheet, cash flow forecast, and profit and loss forecast; financial planning and breakeven analysis.
- Legal and regulatory matters: different forms of legal registration and how to register, different forms of business (e.g. sole proprietorship, partnership, different company types), taxation responsibilities.
- Introduction to marketing strategy – creating a marketing plan, different strategies for selling, marketing research, market segmentation.
- Establishing an online presence and engaging with customers through social media
- Presentation skills and developing a funding pitch and sales pitch
- Strategies for growth – the role of horizontal and vertical integration, of product diversification, and of Strategic Alliances.
- A quick introduction to the IFC-developed SME Toolkit available online, and all participants were also given a CD copy of this.

A2.5 Information Collected at Time of Submission of Business Plan

The business plan collected information on the business profile including the product, its customer base, pricing, the experience and qualifications of the owner, a detailed description of physical capital, premises, form of business organization, cash flow and projected income statements for the first year, financing strategy including other sources of capital, use of e-commerce, marketing plans, and the perceived increase in employment from getting a grant from the YouWiN!! Project. In addition to this business plan, a baseline data sheet was collected which asked about previous business courses taken, current financing, demographic characteristics of the owner, time spent abroad, risk attitudes, reasons for wanting to own a business rather than work in salary work, self-assessed entrepreneurial efficacy, household asset ownership, and follow-up information to enable re-surveying in the future.

A2.6 Quality Checking and Finalization of Winners

After the 1,200 provisional winners were selected, a DFID-procured firm (Growbridge Advisors, supported by Nigerian consultants) reviewed all winning business plans to validate whether the

award amount asked for was reasonable given the proposal, and to propose business milestones and targets, along with a disbursement schedule. As a result of this process, 18 of the original 1,200 winners (3 national, 2 zonal and 13 ordinary merit winners) were disqualified based on an assessment that they required significantly more than 10 million Naira for their business, or that their financial projections were unrealistic. These 18 disqualified proposals were replaced with 18 businesses from the ordinary winner control group. 9 of these replacements were randomly chosen from the same regions and new/existing business status as the firms they were replacing. However, given the rapid finalization of the winners in time for an official announcement and the short time frame for assessing disqualifications, there was a need for 9 further replacements during a day in which the author was on an airplane. These other 9 replacements were chosen as the highest scoring ordinary winner control group in the zones that they were replacing.

Appendix 3: Amounts Received by Winners and Timing Relative to Surveys

Appendix 3: Summary of Amounts Received by Winners

	Mean	S.D.	10th	Median	90th	Max
Amount Received as First Tranche						
Naira	1079023	768276	400000	1000000	2268452	5000000
USD	6873	4893	2548	6369	14449	31847
Amount Received by First Follow-up Survey						
Naira	3591152	1820305	750000	4000000	5000000	8000000
USD	22874	11594	4777	25478	31847	50955
Total Amount Received over all Tranches						
Naira	7691604	2754758	3400000	9000000	10000000	10000000
USD	48991	17546	21656	57325	63694	63694
Months since First Tranche (Follow-up 1)	4.8	1.9	3	5	7	10
Months since First Tranche (Follow-up 2)	15.4	1.6	13	15	17	20
Months since First Tranche (Follow-up 3)	27.1	1.4	25	27	29	31
Months since Last Tranche (Follow-up 3)	14.4	1.5	12	14	16	18

Note: Exchange rate of 157 Naira = 1 USD used. Data for the 1200 winners.

This table shows the amounts awarded for the pooled group of 1200 winners. The experimental winners had a mean grant of $46,627 and the non-experimental winners a mean grant of $52,952. Among the experimental winners, the average grant amount was similar for the new firms and existing firms ($46,632 vs $46,619).

Appendix 4: Business Sectors Proposed by Winners

Appendix 4: Business Sectors Proposed by YouWin! Winners

	Existing Firms				New Firms			
	National Winners	Zonal Winners	Ordinary Winners	All Winners	National Winners	Zonal Winners	Ordinary Winners	All Winners
Retail trade	4.0	4.5	4.2	4.3	4.1	0	4.9	4.4
Food preparation or restaurant	4.0	3.0	2.5	3.2	2.7	6.8	7.2	6.5
Personal services	1.4	4.5	5.1	3.6	1.4	0	5.3	4.4
Tailoring/dressmaking/shows	4.5	7.5	5.4	5.5	5.4	2.3	3.1	3.4
Furniture manufacturing	0.5	0.8	1.8	1.1	0	0	0.7	0.5
Crafts (masks, jewellery, etc.)	0.5	0.8	2.5	1.4	0	2.3	1.1	1.1
Other manufacturing	17.5	11.2	13.4	14.4	14.9	15.9	13	13.4
Repair services	1.0	0.8	2.1	1.4	1.4	0	0.9	0.9
IT and Computer services	18.8	14.9	17.3	17.4	5.4	0	7.8	6.9
Accounting, legal, and medical services	3.6	1.5	1.4	2.2	5.4	0	2.5	2.7
Other professional services	13.5	7.5	7.6	9.6	10.8	13.6	6.5	7.6
Transportation	0.9	0.8	1.8	1.3	6.8	0	1.4	1.9
Construction work	2.3	6.7	4.8	4.3	1.4	2.3	2.7	2.5
Agricultural production	17.0	25.4	20.2	20.2	27	40.9	32.4	32.3
Other industries	10.8	9.7	9.4	9.9	13.5	15.9	10.5	11.3

Appendix 5: Regression Discontinuity Details

The median first stage application score for both new and existing businesses was 38 out of 100. There were few concept notes scoring above 60 – even among the 6,000 selected for training, 78% had application scores between 50 and 60. Among the unsuccessful applicants, only a small minority had scores which would place them close to being selected for training – the main exception being new business applicants in Lagos, where 566 applicants had scores over 50 or above but were not selected for training due to the desire for regional balance and preference for existing firms.

Regression-discontinuity analysis will be used to test for an effect of participating in the 4-day business training program on firm outcomes. This is done by surveying individuals who had first-round application scores just above or just below the thresholds for being invited to training. Appendix Figure 1 below show how the likelihood of being invited to training jumps from 0 to 100% around a score of 52 for new business applicants, and a score of 50 for existing business applicants.

Appendix Figure 1: Discontinuities in the Likelihood of Being Invited to Training According to Initial Application Score

Discontinuity for Existing Firms
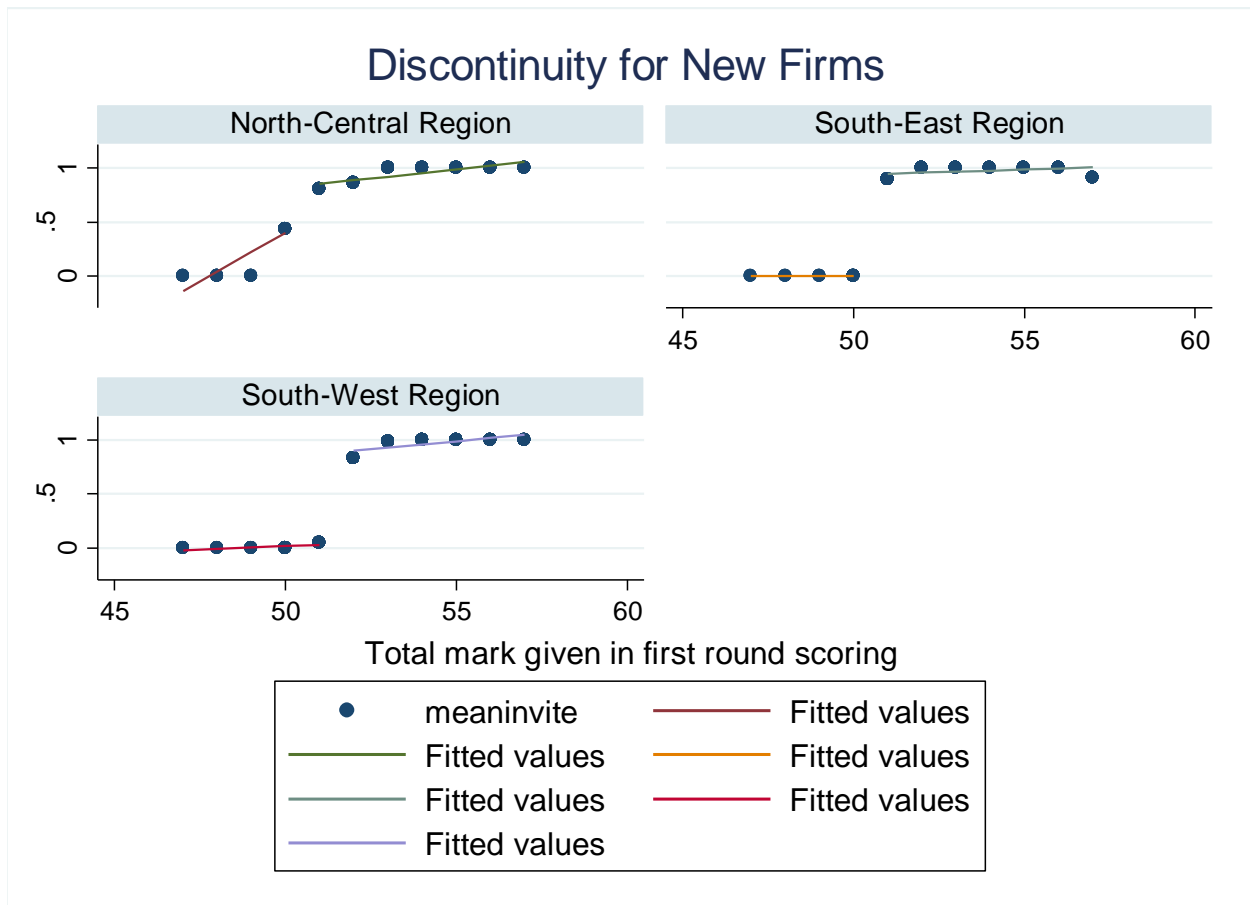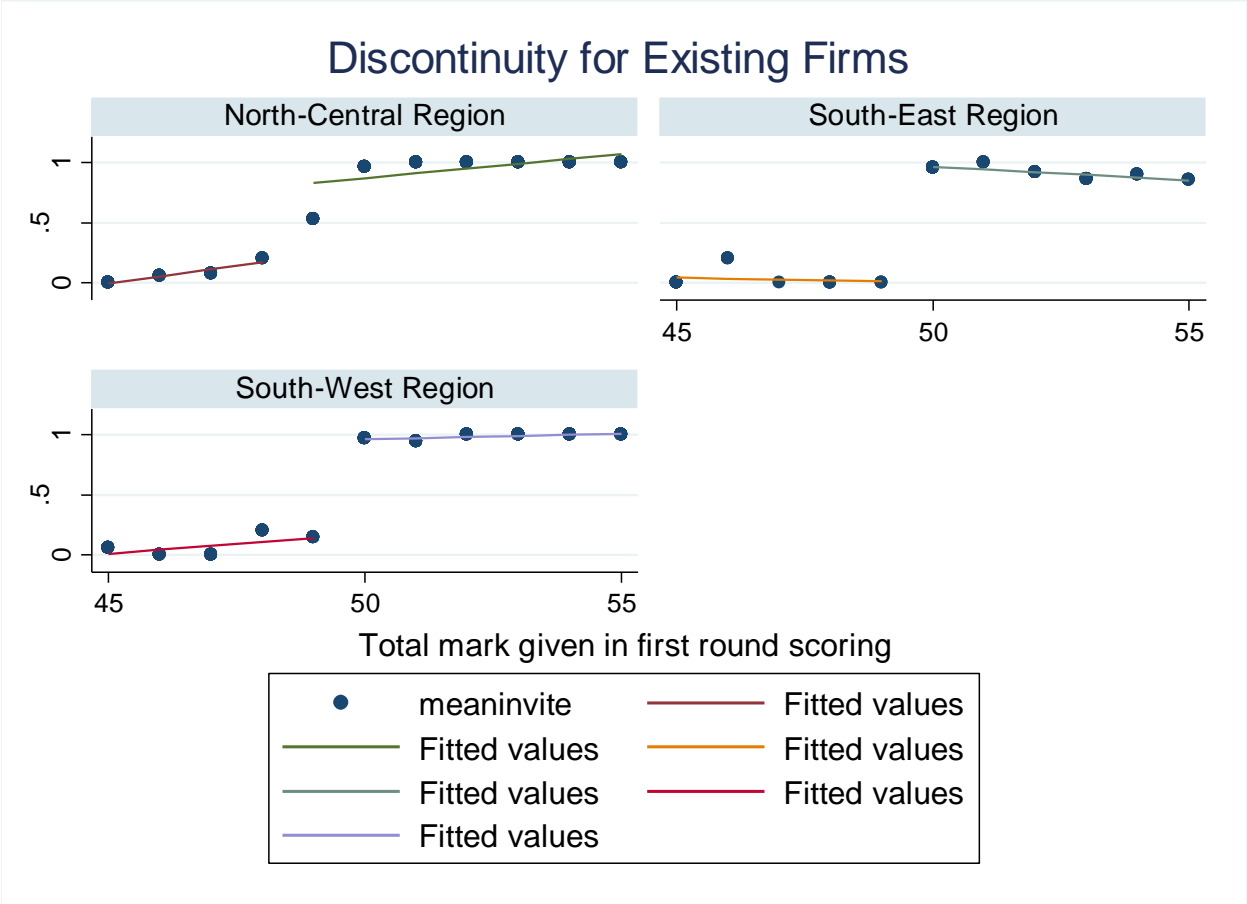
The regression-discontinuity sample for analysis was then constructed as follows. First, we restricted this group to applicant firms in the North-Central, South-Eastern, and South-Western regions, since the other regions have few firms close to the cutoffs. In total there are 4008 new enterprises and 652 existing enterprises that are within 5 points either side of the cutoff for being selected for business plan training. 770 of these firms (329 existing, 441 new) are already included in the 2316 firms in our experimental plus winner sample. This leaves up to 3890 firms that we could have added to the survey. Given budget constraints, we chose to add all 323 existing firms, and then a random sample of 500 of the new firms with scores around the threshold.

As a further complication, some of those who just made the cut-off for the training program then went on to be selected as winners, receiving the large capital grants. I exclude these firms, assuming they are selected at random (which some of them were). Then using the sample of non-winners within 5 points on either side of the cut-off I use instrumental variables to estimate the following regression:

$$Outcome_i = a + b * InvitedtoTraining_i + c * Region_i + d * mark_i + \varepsilon_i \qquad (A1)$$

Where *InvitedtoTraining* is instrumented with being above the scoring threshold. Since we are only looking within a very narrow window of the score around the threshold, I estimate equation (A1) with and without a linear control in the initial application mark. The results are shown in appendix table 5 below. The first column shows having a score above the threshold is a strong and significant predictor of being invited to the training course. However, the remaining columns show no significant impacts of the training on the likelihood of operating a firm or of employment. The one exception is with the round 3 data for existing firms, where adding a linear control in the application mark does result in a significant point estimate, although the coefficient is close to zero and not statistically significant when no linear control is added. Even in this case we see no significant impact on employment.

Appendix 5: Regression Discontinuity Estimates of the Impact of the 4-day Business Training Course

	Invited to Training Course	Operate a Firm			Employment		
		Round 1	Round 2	Round 3	Round 1	Round 2	Round 3
Panel A: New Firms							
Score above threshold	0.881***						
	(0.019)						
Invited to training		0.064	-0.013	0.002	0.450	0.626	0.342
		(0.054)	(0.049)	(0.050)	(0.434)	(0.455)	(0.544)
Score above threshold with linear control	0.742***						
	(0.038)						
Invited to training with linear control		0.089	0.039	0.040	0.412	0.947	1.135
		(0.095)	(0.086)	(0.095)	(0.758)	(0.861)	(0.906)
Sample Size	772	509	641	628	493	634	594
Control Mean	0.000	0.488	0.565	0.505	2.224	2.503	2.901
Panel B: Existing Firms							
Score above threshold	0.902***						
	(0.020)						
Invited to training		-0.004	0.016	0.014	-1.085	-1.655	-0.908
		(0.039)	(0.046)	(0.051)	(1.053)	(1.457)	(1.464)
Score above threshold with linear control	0.772***						
	(0.059)						
Invited to training with linear control		-0.007	0.114	0.240**	-2.791	-1.723	-5.043
		(0.100)	(0.101)	(0.112)	(2.591)	(3.266)	(3.554)
Sample Size	433	305	358	369	293	353	334
Control Mean	0.000	0.893	0.801	0.748	6.933	7.121	7.376

Notes: robust standard errors in parentheses, *, **, *** indicate significance at the 10, 5, and 1 percent levels respectively.

RD estimation is estimated within a window of +/- 5 points on either side of the threshold, and controls for region fixed effects.

Appendix 6: Survey Response Rates by Survey Round, Firm Type, and Treatment Status

Appendix 6: Response Rates by Group

	Sample Size	All Firms			New Firms			Existing Firms		
		First Survey	Second Survey	Third Survey	First Survey	Second Survey	Third Survey	First Survey	Second Survey	Third Survey
Panel A: Information available on whether or not they operate a firm										
Experimental Winners	729	0.846	0.930	0.885	0.840	0.922	0.869	0.856	0.942	0.910
Control Group	1112	0.752	0.906	0.825	0.756	0.901	0.816	0.738	0.924	0.852
Experimental Sample	1841	0.789	0.916	0.848	0.785	0.908	0.835	0.799	0.933	0.882
National and Zonal Winners	475	0.834	0.924	0.878	0.864	0.873	0.831	0.824	0.941	0.894
RD Booster Sample	823	0.694	0.792	0.829	0.678	0.788	0.822	0.718	0.799	0.839
Panel B: Responded to the Survey										
Experimental Winners	729	0.812	0.915	0.870	0.796	0.900	0.847	0.838	0.939	0.906
Control Group	1112	0.719	0.872	0.805	0.723	0.863	0.800	0.703	0.901	0.821
Experimental Sample	1841	0.756	0.889	0.831	0.748	0.876	0.816	0.773	0.921	0.865
National and Zonal Winners	475	0.806	0.918	0.869	0.831	0.856	0.805	0.798	0.938	0.891
RD Booster Sample	823	0.667	0.776	0.821	0.650	0.776	0.816	0.693	0.777	0.830

Appendix 7: Potential Channels Through Which YouWiN! may increase A and E

Potential channels by which the program could increase the entrepreneurial skills of the owner or productivity of the firm include:

(i)   Training increasing skills: the 4-day business plan training, and one day workshops provided to winners, along with online materials provided may increase the entrepreneurial skills of the owner. Assuming these are complementary with other inputs, we should have $dK^*/dE>0$ and $dL^*/dE>0$ so that both capital and labor increase, and profits and employment will both increase.

(ii)   Networks increase productivity or entrepreneurial skills: participating in the program may cause the firm owners to meet other successful business owners. This could increase their own productivity and skills if they learn from these owners, or can use these networks to obtain better business deals.

(iii)   Improvements in confidence and attitudes: entrepreneurial self-confidence could directly impact on productivity, or the program, by declaring the owner a winner, may spur their self-belief in the business and cause them to work harder.[21] In addition, the signal provided by winning the competition could cause firm owners who are uncertain about their entrepreneurial type to update their priors and thus change their output levels if they underinvest because they are unsure of whether they have the skills to make it at a larger scale.

(iv)   Mentoring increases *A* and *E*. The YouWIN program in principle provides some very basic mentoring services, which could increase *A* and *E*.

(v)   Reputation effects: *A* could increase if winning the competition increases the businesses' reputation, signaling quality to customers and therefore allowing it to gain more customers.

(vi)   Change in the interaction with government: winning the competition could give the firm some protection against government officials asking for bribes or otherwise inhibiting firm productivity, since now the firm is seen as a favored firm which should not be touched; or conversely winning the competition may make the firm be targeted by rent-seeking officials therefore reducing productivity.

(vii)   Changes in family demands: winning the competition may cause the firm owner be targeted for more requests for money and or free goods by extended family members. This could lower firm productivity, or conversely cause the firm owner to invest more in the firm if money in the firm is less subject to capture than money held at home.

Appendix 8: Measurement of Key Outcomes

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[21] Conversely there is the potential that not winning may dent entrepreneurial self-confidence for the control group. We show empirically that there appears to be no such impact.

The survey instruments and data are available in the World Bank's open data library (the replication files will be added once a final version of the paper is accepted):

http://microdata.worldbank.org/index.php/catalog/2329

Key outcomes are measured as follows:

Operates a firm: measuring by directly asking the owner if they currently operate a business. When the owner could not be found, this information was obtained by direct verification and through asking friends, relatives, and neighbors.

Own employment: a binary variable taking the value one if the owner owns a business, or reports working in the last month for pay in any other occupation.

Total employment: the number of paid workers in the firm, including the owner. This is the sum of whether the owner operates a firm, the number of wage and salary employees, and the number of casual and daily laborers. Unpaid workers are not included. Coded as zero if the business does not exist.

Firm of 10+ workers: a binary variable taking the value one if total employment is 10 or more.

Business practices: the proportion of the following 22 business practices employed, taken from McKenzie and Woodruff (2015):

Marketing Practices: coded as 1 for each of the following that the business has done in the last 3 months:

- M1: Visited at least one of its competitor's businesses to see what prices its competitors are charging
- M2: Visited at least one of its competitor's businesses to see what products its competitors have available for sale
- M3: Asked existing customers whether there are any other products the customers would like the business to sell or produce
- M4: Talked with at least one former customer to find out why former customers have stopped buying from this business
- M5: Asked a supplier about which products are selling well in this business' industry
- M6: Attracted customers with a special offer
- M7: Advertised in any form (last 6 months)

    Note: M1 and M2 are coded as zero if the firm says it has no competitors. M4 is coded as zero if the firm says it has no former customers.

Buying and Stock Control Practices: coded as 1 for each of the following:

- B1: Attempted to negotiate with a supplier for a lower price on raw material

- B2: Compared the prices or quality offered by alternate suppliers or sources of raw materials to the business' current suppliers or sources of raw material
- B3: The business does not run out of stock monthly or more (coded as one if the business has no stock)

Costing and Record-Keeping Practices: coded as 1 for each of the following that the business does:

- R1: Keeps written business records
- R2: Records every purchase and sale made by the business
- R3: Able to use records to see how much cash the business has on hand at any point in time
- R4: Uses records regularly to know whether sales of a particular product are increasing or decreasing from one month to another
- R5: Works out the cost to the business of each main product it sells
- R6: Knows which goods you make the most profit per item selling
- R7: Has a written budget, which states how much is owed each month for rent, electricity, equipment maintenance, transport, advertising, and other indirect costs to business
- R8: Has records documenting that there exists enough money each month after paying business expenses to repay a loan in the hypothetical situation that this business wants a bank loan

Financial Planning Practices: coded as 1 for each of the following:

- F1: Review the financial performance of their business and analyze where there are areas for improvement at least monthly
- F2: Has a target set for sales over the next year
- F3: Compares their sales achieved to their target at least monthly
- F4: Has a budget of the likely costs their business will have to face over the next year

Innovation Index: an average of standardized z-scores for the following 12 different measures of innovative activities:

- Introduced a new product in the past year
- Improved an existing product or service in the past year
- Introduced a new or improved process in the past year
- Introduced a new design or packaging in the past year
- Introduced a new channel for selling goods in the past year
- Introduced a new method of pricing in the past year
- Introduced a new method of advertising in the past year
- Changed the way work is organized in the firm in the past year
- Introduced new quality control standards in the past year
- Licensed a new technology in the past year
- Obtained new quality certification in the past year
- Uses the internet

Truncated sales: total sales reported in the last month, truncated at the 99th percentile to reduce the influence of outliers.

Truncated profits: total profits of the business in the last month, truncated at the 99th percentile. Profits are measured following the recommendations of de Mel et al. (2009) by means of a direct question asking "What was the total income the business earned during the last month after paying all expenses including wages of employees, but not including any income you paid yourself". They therefore include the return to the entrepreneur's own labor.

Inverse hyperbolic sine of profits: the transform $\log(y+(y^2+1)^{1/2})$ which is similar to the log transformation but which allows for zero and negative values of profits.

Aggregate Index of Sales and Profits: an average of standardized z-scores for the outcomes monthly sales, truncated monthly sales, annual sales, sales higher than one year ago, monthly profits, truncated monthly profits, profits in the best month, and inverse hyperbolic sine of profits.

Entrepreneurial Self-Efficacy: Measured as the number of 9 business activities that the owner rates themselves as "very confident" in their ability to do. This is coded as 1 for each item if the owner answers 4 = very confident, and 0 if they answer 1 through 3, or 9 (not applicable or refuse). The activities are:
  - Come up with an idea for a new business product or service
  - Estimate accurately the costs of a new business venture
  - Estimate customer demand for a new product or service
  - Sell a product or service to a customer you are meeting for the first time
  - Identify good employees who can help the business grow
  - Inspire, encourage, and motivate employees
  - Find suppliers who will sell you raw materials at the best price
  - Persuade a bank to lend you money to finance a business venture
  - Correctly value a business if you were to buy an existing business from someone else.

Has a Mentor: The firm reports have a business mentor in response to a direct question.

Number in Business Network: number of other business owners the individual discusses business matters with, truncated at the 99th percentile.

Firm is formal: the firm reports that it has a registered business name, a municipal license, and pays income tax

Took a formal loan: received a loan from a bank, microfinance organization, or NGO in the past year

Received equity investment: received a new investment in the form of equity in the past year

Hours of consulting services: number of hours of consulting services used in the past year, truncated at the 99th percentile.

Value of inventories: current value reported of inventories and raw materials, top-coded at the 99[th] percentile.

Made a large capital purchase: reports making a capital purchase of more than 100,000 Naira in the past year

Value of capital purchases: total value of capital purchases over 100,000 Naira, truncated at the 99[th] percentile.

Capital stock: current value of inventories plus the sum of the value of capital purchases made in each of the survey rounds since the program began, truncated at the 99[th] percentile.

Functioning website: has a website with a functioning URL as of October 4, 2015.

Appendix 9: Does Treatment Change the Observable Characteristics of Who Operates a Firm?

Appendix 9: Comparison of characteristics of those operating firms in round 2 by treatment status

	Existing Firms			New Firms		
	Treatment	Control	p-value	Treatment	Control	p-value
Applicant Characteristics						
Female	0.169	0.190	0.930	0.167	0.143	0.588
Age	32.0	31.8	0.6	29.5	30.1	0.079
Married	0.502	0.576	0.125	0.350	0.400	0.131
High School or Lower	0.129	0.127	0.958	0.117	0.115	0.484
University education	0.627	0.644	0.734	0.687	0.683	0.277
Postgraduate education	0.071	0.112	0.138	0.042	0.044	0.756
Lived Abroad	0.086	0.107	0.503	0.063	0.080	0.615
Choose Risky Option	0.557	0.498	0.182	0.564	0.526	0.126
Have Internet access at home	0.553	0.605	0.347	0.462	0.520	0.438
Own a Computer	0.871	0.878	0.872	0.836	0.876	0.292
Satelite Dish at home	0.659	0.693	0.419	0.674	0.651	0.831
Freezer at home	0.569	0.639	0.166	0.514	0.561	0.300
Business Characteristics						
Crop and Animal Sector	0.161	0.156	0.993	0.227	0.225	0.817
Manufacturing Sector	0.271	0.263	0.840	0.279	0.228	0.147
Trade Sector	0.059	0.044	0.468	0.039	0.046	0.868
IT Sector	0.153	0.161	0.661	0.068	0.064	0.640
First Round Application Score	57.278	56.049	0.077	60.065	59.779	0.544
Business Plan Score	45.675	45.112	0.397	53.637	55.621	0.991
Number of Workers	5.502	6.663	0.061			
Ever had Formal Loan	0.059	0.063	0.907			
Joint orthogonality test p-value	0.585			0.678		

Treatment does not change the observable characteristics of who operates a firm, despite having a large impact on the proportion of individuals operating a firm.

Appendix 10: Observed vs Reported Employment

Since employment creation was a goal of the program, and firms were meant to show progress in growing their firms before receiving their last tranche payments, one may be concerned that firm owners are over-reporting employment. Around the time of the second round survey, firm owners were asked to report how many permanent employees, and how many casual or temporary employees they had employed since they won the grant. As shown in Table 4, the total of 23,781 employees is almost twice the total employment of 13,945 reported to us in our surveys.

Appendix Figure 2 plots our survey measure of total employment against the total reported by firms to the program. We see many reports lie close to the 45 degree line, but there are numbers of firms with relatively few workers in the survey who report more to the program, as well as one firm reporting around 200 workers in the survey versus 1000 in the program. The bottom panel of this figure plots the distribution of differences: 75 percent of firms report more employees to the program than they do in the survey, 11.5 percent report the same to both, and only 13.5 percent report more in the survey than in the firm.

While firm owners may have an incentive to over-report employment to the program to ensure they reach the job triggers needed for their third and fourth tranche payments, these triggers were set very low and the amounts reported in the administrative data greatly exceed these triggers (appendix Figure 3), with the median firm having 9 more workers in the administrative data than needed for their fourth tranche payment to have been made.

Firm owners have an incentive to over-report employment to the program, whereas this incentive is much less in the survey which was conducted by a survey research firm (TNS Gallup) and where the questions came as part of a much more detailed set of questions about the business. I therefore view the survey measures as more reliable. As an added check on this, survey enumerators were asked to record how many employees they physically observed at the enterprise while they were conducting the interview. This misses workers who are sick, those whose hours don't correspond with those of the interview, and those who are working in another location. Furthermore, it is not available for individuals who were interviewed at their house, or over the phone. Nevertheless, it provides a useful check which we discuss in the text.

Appendix Figure 2: Comparing Administrative to Survey Reports on Employment

Reporting to Survey vs Reporting to Program

Employment Reported by Owner in Second Round Survey

Distribution of Difference in Reporting of Employees to Program vs Survey

75% report more employees to the program than they do to the survey
11.5% report the same to both, only 13.5% report more in the survey

Additional number of employees reported to program vs to survey

Appendix Figure 3: Employment Reported in Administrative Data Greatly Exceeds the Job Trigger Needed for the Fourth Tranche Payment for Most Firms
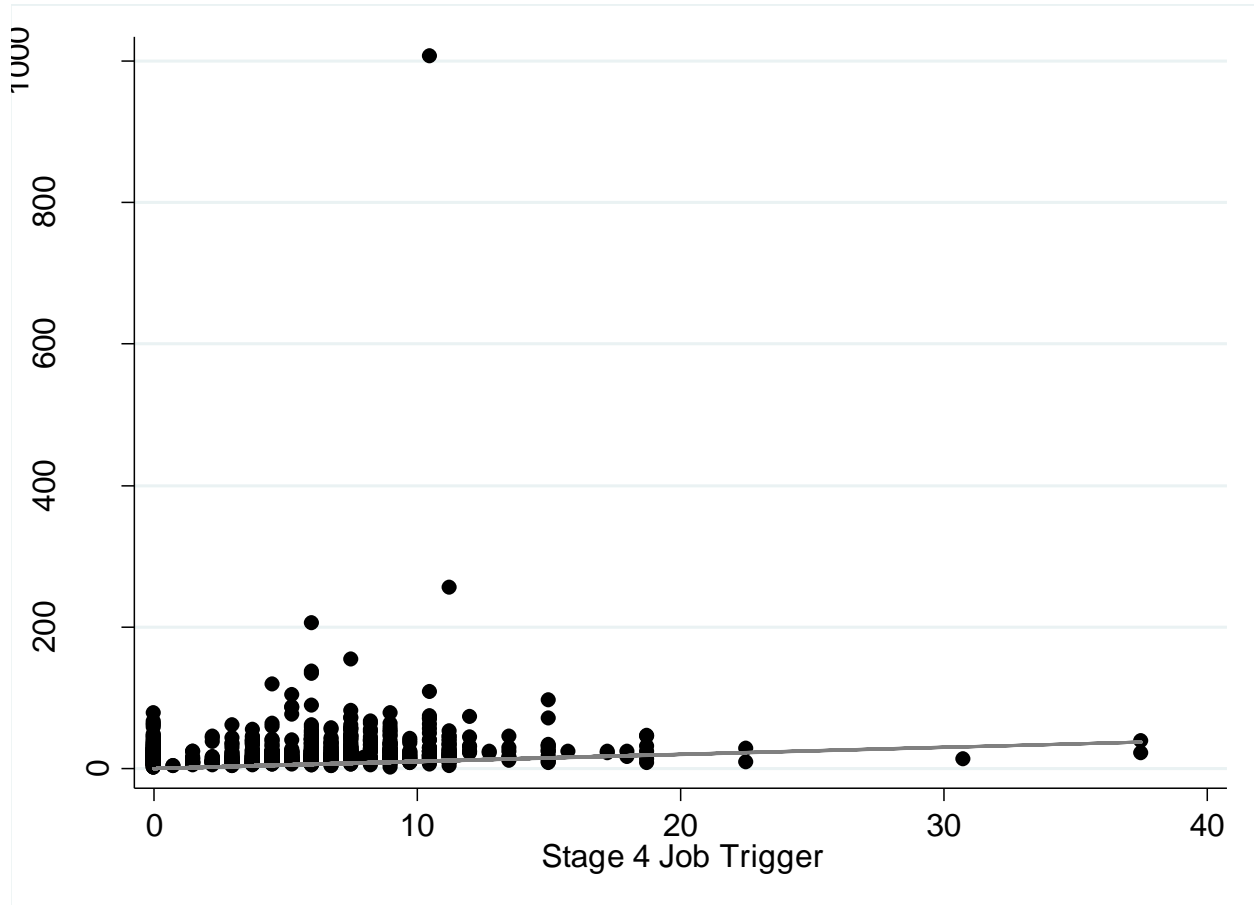
As a second check, there are 203 existing firms and 258 new firms in the experimental sample that are in business, and that have both a survey report of employment as well as the interviewers observation of the number of employees. I test whether there is any differential reporting effect by treatment status on this sub-sample by estimating:

$$Survey\ report_i - InterviewerObservation_i = a + bAssignTreat_i + c * Region * Gender_i + \varepsilon_i$$

The coefficient b is 0.91 (p=0.143) for the new enterprises, and 0.60 (p=0.466) for the existing enterprises. We can therefore not reject the null of no added difference in reporting with treatment group status. Although the point estimates are positive, they account for only 15 percent of the estimated treatment effect for new enterprises and 20 percent of the estimated treatment effect for existing enterprises. Thus even if selective over-reporting of employment by the treated is occurring in the survey, it only accounts for a small share of the overall treatment effect estimated. Finally note that any incentives to over-report employment should be lower in the third round,

which occurs 12 to 18 months after individuals have received all funding from the program and yet we still see our treatment effects persist with this data.

Appendix 11: Additional Employment Impacts

The table below shows the impacts on employment conditional on firm survival. Since these are conditioning on an outcome which is itself is affected by treatment, they should be considered as descriptive only. They show that even though the YouWiN! program has generated many more firms, the average number of employees per firm is still larger.

Appendix Table 11a: Impacts on Employment Conditional on Survival

	New Firms			Existing Firms		
	Round 1	Round 2	Round 3	Round 1	Round 2	Round 3
Experimental Treatment Effect	0.189	4.459***	2.211***	0.907	3.229***	2.481***
	(1.109)	(0.472)	(0.570)	(0.824)	(0.791)	(0.757)
Sample Size	608	712	550	386	412	333
Control Mean	6.702	5.519	4.931	7.896	7.681	9.325

Notes: Robust standard errors in parentheses, *, **, *** indicate significance at the 10, 5, and 1 percent levels. Experimental estimates are ITT estimates and control for randomization strata.

Appendix table 11b shows impacts on other pre-specified employment measures. Treatment results in firms hiring more wage and salary workers and more casual and daily workers, but little change in unpaid workers. There is both more hiring and more firing of workers.

Appendix Table 11b: Impacts on Other Pre-Specified Employment Outcomes

	Wage & Salary Workers	Casual & Daily Workers	Unpaid Workers	Workers hired in last year	Workers fired in last year	Aggregate Employment Index
Panel A: New Firms						
First Follow-up	0.791***	0.423	-0.047	1.687***	0.190	0.237***
	(0.180)	(0.704)	(0.070)	(0.263)	(0.168)	(0.039)
Second Follow-up	3.627***	2.068***	0.115*	3.937***	1.339***	0.549***
	(0.231)	(0.303)	(0.059)	(0.429)	(0.292)	(0.035)
Third Follow-up	3.246***	1.555***	0.070	n.a.	n.a.	0.439***
	(0.296)	(0.319)	(0.072)			(0.034)
Control Mean: First follow-up	1.536	1.547	0.206	1.006	0.246	-0.075
Control Mean: Second follow-up	1.793	0.943	0.170	0.894	0.149	-0.180
Control Mean: Third follow-up	2.368	0.965	0.150			-0.149
Obs: First follow-up	992	982	980	995	995	1021
Obs: Second follow-up	1153	1149	1150	1153	1087	1181
Obs: Third follow-up	1007	1068	1067			1085
Panel B: Existing Firms						
First Follow-up	0.489	0.998*	0.222*	1.021**	0.064	0.201***
	(0.494)	(0.595)	(0.128)	(0.433)	(0.200)	(0.054)
Second Follow-up	2.656***	-0.111	0.030	2.258***	0.640**	0.267***
	(0.582)	(1.188)	(0.110)	(0.482)	(0.283)	(0.049)
Third Follow-up	2.961***	1.225***	-0.067	n.a.	n.a.	0.308***
	(0.509)	(0.417)	(0.103)			(0.053)
Control Mean: First follow-up	4.026	1.968	0.219	2.321	0.532	-0.105
Control Mean: Second follow-up	3.802	3.370	0.265	2.126	0.552	-0.136
Control Mean: Third follow-up	3.716	1.200	0.169			-0.160
Obs: First follow-up	422	418	418	423	423	432
Obs: Second follow-up	499	498	496	501	460	505
Obs: Third follow-up	450	472	470			477

Notes:

n.a. denotes question not asked in this survey round.

Robust standard errors in parentheses, *, **, *** indicate significance at the 10, 5, and 1 percent levels.

Experimental estimates are ITT estimates and control for randomization strata.

Aggregate Employment Index is average of standardized z-scores of the owners' employment, the firm operating status, number of wage and salary workers, number of casual and daily workers, number of unpaid workers, and number of workers hired in the past year.

Appendix Table A11c shows that the second and third round impacts on the likelihood a firm has 10 or more workers are robust to using Lee bounds to account for potential selective differential attrition between the treatment and control groups

Appendix 11c: Lee Bounds for Impact on 10 or more workers

	New Firms			Existing Firms		
	First Round	Second Round	Third Round	First Round	Second Round	Third Round
Experimental Estimate (Table 3)	0.024	0.288***	0.229***	0.055	0.211***	0.206***
	(0.020)	(0.026)	(0.028)	(0.041)	(0.041)	(0.040)
Lower Bound	-0.081***	0.275***	0.185***	-0.061	0.204***	0.164***
	(0.012)	(0.026)	(0.028)	(0.038)	(0.041)	(0.040)
Upper Bound	0.038*	0.297***	0.252***	0.100**	0.222***	0.234***
	(0.022)	(0.027)	(0.029)	(0.043)	(0.041)	(0.041)
Sample Size	949	1150	1020	389	495	445

Notes: Dependent variable is firm having 10+ workers.

Robust standard errors in parentheses. *, **, and *** indicate significance at the 10, 5, and 1 percent levels respectively.

Appendix 12: Cost per Job Generated in Other Studies

Appendix Table 12 gives examples of the cost per job created from other impact evaluations in developing countries, for studies in which cost and employment impact data were available. Many studies do not provide cost data, limiting the set of studies for which this comparison can be done. For the vocational training and wage subsidy studies, the studies look at whether the program increases the employment likelihood of the person receiving the training or subsidy. For the management consulting, small grants to microenterprises, and business training interventions, the studies also include whether the firm hires paid employees. Note that the absence of an impact on paid employment does not necessarily mean these interventions are ineffective: the may also have impacts on the earnings of workers and firms, on a shift to formal employment, and on other outcomes of policy interest.

Appendix 12: Examples of Cost Per Job Generated for Other Employment and Firm Programs in Developing Countries.

Intervention Type	Study	Country	Employment Impact	Cost Per Treated Unit (USD)	Cost Per Job Created (USD)
Vocational Training	Hirshleifer et al. (2015)	Turkey	0.02 (n.s.)	1619	80950 (n.s.)
	Attanasio et al. (2011)	Colombia	0.068 (females),	750	11029 (females),
			0.013 (males, n.s.)		57692 (males, n.s.)
Wage Subsidies	Groh et al. (2014)	Jordan	0.015 (n.s.)	571	38100 (n.s.)
Management consulting	Bloom et al. (2013)	India	-1.28 (n.s.)	75,000	no creation
	Karlan et al. (2014)	Ghana	0.047 (n.s.)	1125	23936 (n.s.)
	Bruhn et al. (2013)	Mexico	0.52 (one year, n.s.);	11856	2676 (long-run),
			4.43 (admin long-run)		22800 (one year, n.s.)
Small grants to microenterprises	De Mel et al. (2012)	Sri Lanka	-0.03 (n.s.)	100-200	no creation
	Karlan et al. (2014)	Ghana	-0.169 (n.s.)	133	no creation
Business training	Karlan and Valdivia	Peru	0.017 (n.s.)	n.a.	n.a.
	Valdivia	Peru	-0.06 (n.s.)	337	no creation
	Drexler et al. (2012)	Dominican Republic	-0.02 (rule of thumb, n.s.),	21	no creation,
			0.05 (n.s. standard		420 (standard training, n.s.)
			training)		

Notes:

n.s. denotes not statistically significant, n.a. denotes cost data not available. Most studies costs are direct costs only, and do not include program implementation costs. Bruhn et al. (2013)'s long-run estimates are only for the subsample of firms they could match to administrative data, and captures formal employment only.

Appendix 13: Impacts on Innovation and Business Practices

Business practices are measured as the proportion of 22 business practices employed, following McKenzie and Woodruff (2015). Innovation is an index of 12 different types of innovation, including product, process, marketing, pricing, quality control, and use of the internet. The first four columns of Appendix Table 13a reports the unconditional estimates, where we see positive increases in both business practices and innovation for both new and existing firms. The point estimates show a 14 to 22 percentage point increase in innovative activities for experimental winners. However, some of this reflects that firms need to be in business in order to innovate, and that the program had large impacts on start-up and survival. Therefore in the last four columns we look descriptively at the treatment-control difference conditional on the business operating. We see that the winning firms among new applicants are doing more innovation and using better business practices on average than the control firms, and also some more innovation in the first two rounds for the existing firm winners. Appendix Table 13b examines the types of innovation occurring, and shows the new applicants are innovating more in multiple dimensions, introducing new products, processes, pricing methods, quality control systems, using the internet, and using new channels for selling goods.

Appendix 13a: Impacts on Business Practices and Innovation

	Unconditional Impacts				Impacts Conditional on Operating Firm			
	New Firms		**Existing Firms**		**New Firms**		**Existing Firms**	
	Business Practices	Innovation Index	Business Practices	Innovation Index	Business Practices	Innovation Index	Business Practices	Innovation Index
Experimental impacts:								
First-Follow-up	0.152***	0.099***	0.081***	0.105***	-0.013	0.019	0.015	0.072**
	(0.025)	(0.019)	(0.027)	(0.029)	(0.017)	(0.023)	(0.018)	(0.029)
Second Follow-up	0.339***	0.270***	0.133***	0.126***	0.050***	0.134***	0.017	0.058**
	(0.021)	(0.018)	(0.026)	(0.028)	(0.012)	(0.019)	(0.014)	(0.027)
Third Follow-up	0.358***	0.219***	0.183***	0.141***	0.060***	0.072***	0.003	0.029
	(0.022)	(0.019)	(0.032)	(0.029)	(0.014)	(0.022)	(0.019)	(0.029)
PSM impacts for National/Zonal winners								
First-Follow-up	0.128***	0.145***	0.070**	0.111***	-0.073***	0.052	-0.008	0.068***
	(0.038)	(0.032)	(0.029)	(0.029)	(0.027)	(0.042)	(0.017)	(0.021)
Second Follow-up	0.388***	0.262***	0.150***	0.166***	0.045***	0.083***	0.029**	0.093***
	(0.030)	(0.029)	(0.028)	(0.025)	(0.016)	(0.030)	(0.012)	(0.027)
Third Follow-up	0.391***	0.293***	0.189***	0.138***	0.089***	0.143***	0.006	0.027
	(0.032)	(0.036)	(0.025)	(0.028)	(0.020)	(0.041)	(0.020)	(0.034)
Control Mean: First follow-up	0.406	0.225	0.687	0.390	0.750	0.414	0.791	0.449
Control Mean: Second follow-up	0.409	0.214	0.676	0.407	0.770	0.404	0.816	0.492
Control Mean: Third follow-up	0.341	0.181	0.559	0.341	0.721	0.382	0.778	0.474
Obs: First follow-up	995	995	423	423	616	616	387	387
Obs: Second follow-up	1071	1071	458	458	708	708	413	413
Obs: Third follow-up	927	927	409	409	574	574	344	344

Notes: Robust standard errors in parentheses, *, **, *** indicate significance at the 10, 5, and 1 percent levels.

Experimental estimates are ITT estimates and control for randomization strata.

Propensity score matching (PSM) estimates compare non-experimental winners to experimental control group.

Business Practices is the proportion of 22 business practices employed; innovation index is an index of standardized z-scores for 12 different measures of innovative activities.

Obs is the number of observations in the experimental sample.

Appendix 13b: What types of innovation are they doing?

Impacts on Innovative Activities conditional on business operating

	New Firms			Existing Firms		
	Control Mean	Round 2	Round 3	Control Mean	Round 2	Round 3
Introduced a new product	0.377	0.183***	0.016	0.348	0.096**	0.056
		(0.037)	(0.041)		(0.049)	(0.055)
Improved existing product or service	0.582	0.078**	0.038	0.428	0.071	0.011
		(0.038)	(0.065)		(0.048)	(0.070)
Introduced new or improved process	0.508	0.131***	0.071*	0.406	0.079	0.023
		(0.037)	(0.038)		(0.049)	(0.054)
Introduced new design or packaging	0.563	0.164***	0.109***	0.497	0.069	0.063
		(0.038)	(0.041)		(0.048)	(0.055)
Introduced new channel for selling goods	0.525	0.145***	0.114***	0.473	0.086*	0.126**
		(0.038)	(0.041)		(0.049)	(0.054)
Introduced new method for pricing	0.612	0.147***	0.116***	0.535	0.061	0.009
		(0.037)	(0.040)		(0.048)	(0.053)
Introduced new method of advertising	0.656	0.173***	0.107***	0.543	0.033	-0.014
		(0.036)	(0.041)		(0.047)	(0.052)
Changed way work organized in firm	0.585	0.199***	0.065	0.428	0.071	0.042
		(0.037)	(0.040)		(0.048)	(0.055)
Introduced new quality control standards	0.481	0.106***	0.100**	0.358	0.002	0.032
		(0.037)	(0.041)		(0.050)	(0.053)
Licensed a new technology	0.186	0.070**	-0.003	0.126	0.043	-0.088*
		(0.029)	(0.026)		(0.040)	(0.045)
Obtained new quality certification	0.126	0.095***	-0.007	0.053	-0.020	0.023
		(0.024)	(0.021)		(0.032)	(0.028)
Uses internet	0.699	0.118***	0.105***	0.650	0.105**	0.050
		(0.034)	(0.037)		(0.042)	(0.045)

Notes: robust standard errors in parentheses, *, **, *** indicate significance at the 10, 5, and 1 percent levels respectively.

Each row contains the ITT estimate for the impact of winning the business plan competition on a particular measure of innovation, conditional on the business being in operation. Randomization strata are controlled for.

Appendix 14: Other Measures of Impacts on Sales and Profitability

Appendix Figures 4 and 5 show the CDFs of profits and sales at the time of the third round survey. We see the treatment CDF lies to the right of the control CDF for most of the distribution, except right at the upper tail. This means we should be more likely to see significant treatment effects for transformations which depend less on the upper tail. This is what we see in Table 5, where there is a large and statistically significant impact on the inverse hyperbolic sine of profits (essentially a log transformation), but noisier and insignificant positive impacts on the levels.

Appendix Table 14a examines the association of treatment with profits and sales, conditional on being in operation. Recall that treatment led to a large increase in entry and survivorship. In the first follow-up this lowers the average profits and sales of new firms, which is consistent with the additional new firms being started being slightly smaller than those that would have been started anyway. There is some evidence still for this in round 3, but the pooled round 2 and 3 association is not statistically significant. We see less in the way of significance for existing firms, and cannot reject that in the pooled round 2 and round 3 data that the treatment and control firms are earning similar profits and sales conditional on being in business – but there are a lot more treatment firms in business.

Appendix Table 14b examines the impacts on several other measures of profits and sales. Treated firms report having more customers (only significant for the second round for new firms), have higher annual sales, are more likely to say their sales are higher than one year ago, and have higher sales of their main product. Many of these additional measures were difficult for firms to answer, and were not asked in the third round survey.

Appendix Figure 4: Cumulative Distribution Functions for Profits in Round 3 by Treatment Status
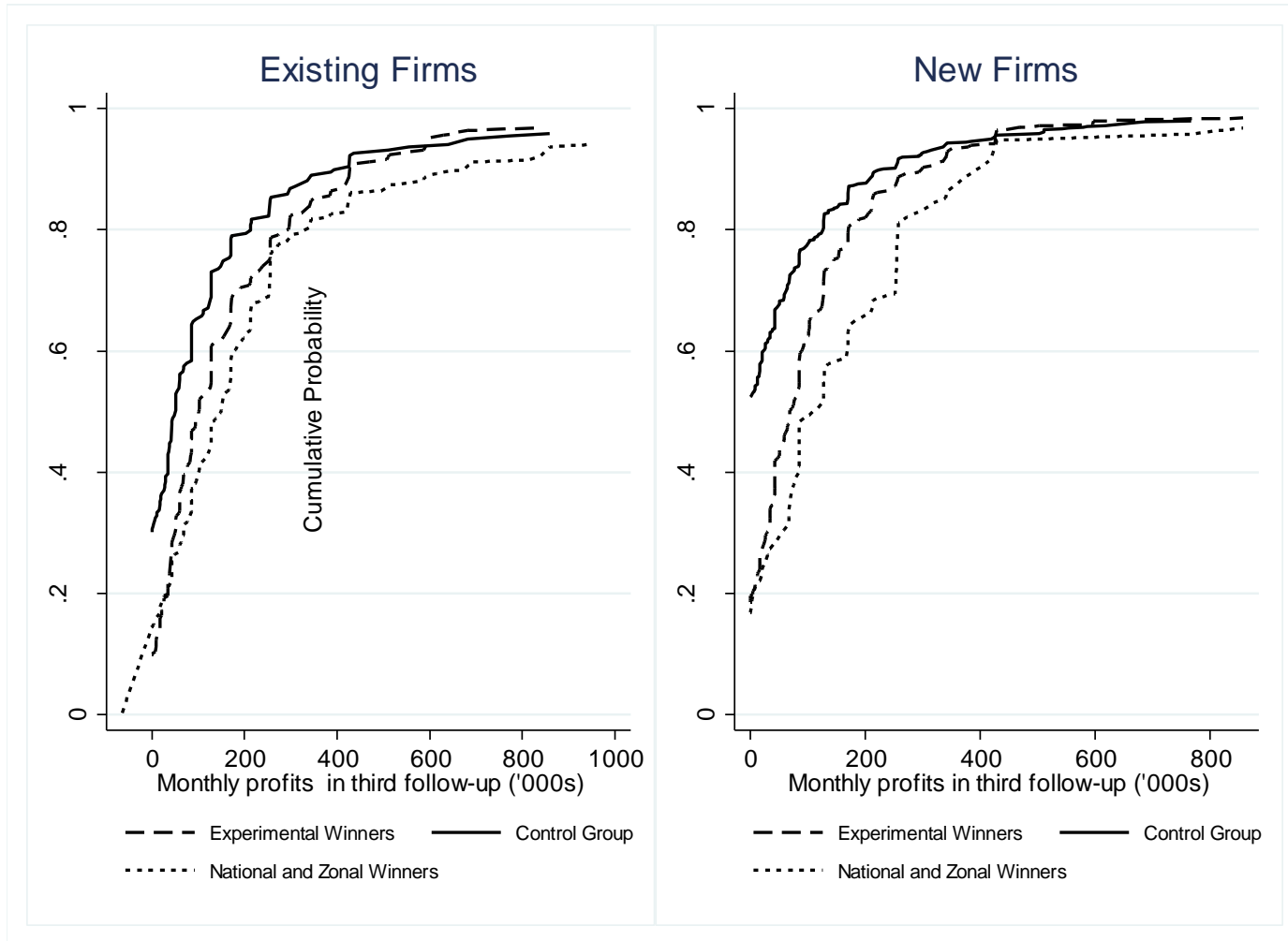
Appendix Figure 5: Cumulative Distribution Functions for Sales in Round 3 by Treatment Status
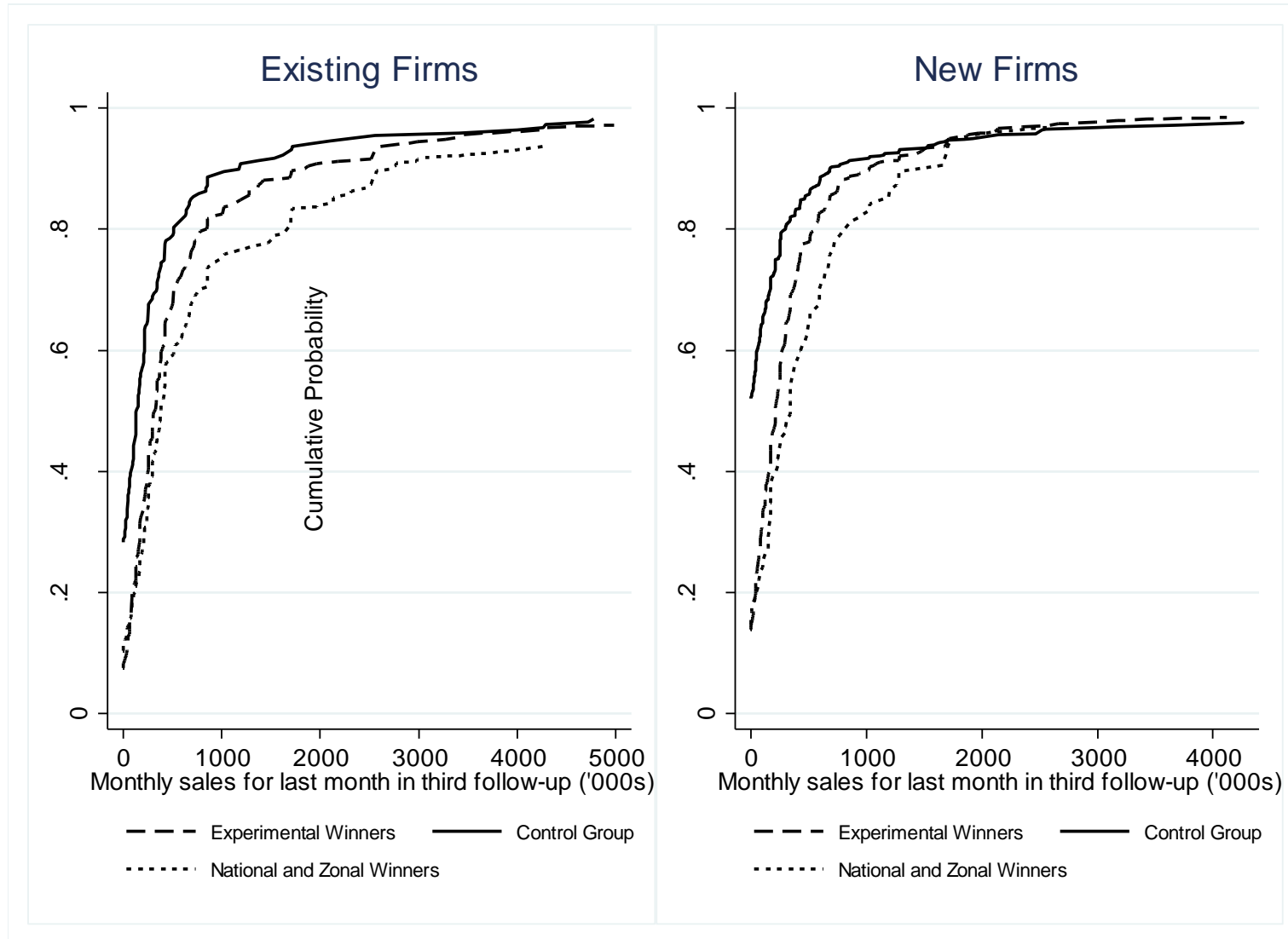
Appendix 14a: Conditional Impacts on Sales and Profits

	New Firms				Existing Firms			
	Truncated Sales	Truncated Profits	Inverse Hyperbolic Sine Profits	Aggregate Index of Sales and Profits	Truncated Sales	Truncated Profits	Inverse Hyperbolic Sine Profits	Aggregate Index of Sales and Profits
Experimental impacts:								
First-Follow-up	-90.919	-113.172***	-0.572***	-0.230***	1.540	-23.995	-0.092	-0.014
	(72.098)	(41.489)	(0.154)	(0.059)	(92.510)	(54.398)	(0.157)	(0.069)
Second Follow-up	127.726*	13.048	0.057	0.021	253.603*	36.646	0.657**	0.119*
	(75.035)	(19.769)	(0.257)	(0.045)	(147.999)	(38.556)	(0.301)	(0.062)
Third Follow-up	-257.996*	-65.799**	-0.158	-0.153***	212.215	-25.818	0.309	0.047
	(135.557)	(33.131)	(0.296)	(0.059)	(160.408)	(50.624)	(0.342)	(0.077)
Pooled Second and Third Round Effect	-79.933	-19.615	-0.121	-0.065	174.300	-14.779	0.307	0.049
	(110.593)	(23.383)	(0.248)	(0.051)	(136.167)	(38.963)	(0.250)	(0.061)
Control Mean: First follow-up	509.699	257.025	10.772	-0.045	509.699	257.025	10.772	-0.045
Control Mean: Second follow-up	660.535	206.305	9.646	-0.117	660.535	206.305	9.646	-0.117
Control Mean: Third follow-up	509.975	192.151	8.565	-0.108	509.975	192.151	8.565	-0.108
Obs: First follow-up	616	616	616	616	387	387	387	387
Obs: Second follow-up	788	787	787	789	452	452	452	452
Obs: Third follow-up	710	710	710	710	405	404	404	405

Notes: Robust standard errors in parentheses, *, **, *** indicate significance at the 10, 5, and 1 percent levels.

Experimental estimates are ITT estimates and control for randomization strata.

Obs is the number of observations in the experimental sample.

Aggregate index of outcomes includes monthly sales, truncated monthly sales, annual sales, sales higher than one year ago, monthly profits, truncated monthly profits, profits in the best month, and inverse hyperbolic sine of profits.

Note pooled sample conditions on the firm being in operation in both follow-up 2 and 3.

Appendix Table 14b: Unconditional Impacts on Other Pre-Specified Sales and Profits Outcomes

	Number of Customers in week	Untruncated Monthly Sales	Annual Sales	Sales are higher than year ago	Untruncated Monthly Profits	Profits in best month	Sales of main product	Mark-up profit on main product	Aggregate outcome index
Panel A: New Firms									
First Follow-up	9.748	119.381	-248.263	-0.053*	-49.163	-19.114	4555.891*	1292.952*	0.044
	(6.098)	(97.599)	(162.620)	(0.031)	(36.547)	(28.329)	(2763.075)	(714.581)	(0.042)
Second Follow-up	14.109**	148.377	1765.721***	0.205***	39.346	111.898***	121.812***	42.756***	0.266***
	(6.362)	(134.477)	(483.571)	(0.031)	(30.743)	(21.165)	(34.218)	(13.125)	(0.035)
Third Follow-up		-26.983	802.723**	0.203***	-50.874	83.643***			
		(129.783)	(381.820)	(0.031)	(59.576)	(31.141)			
Control Mean: First follow-up	27.965	277.280	1271.742	0.369	195.740	188.660	2680.548	775.056	-0.015
Control Mean: Second follow-up	32.600	502.419	2022.581	0.393	139.112	124.074	170.514	61.608	-0.087
Control Mean: Third follow-up		528.777	2197.340	0.341	174.143	154.308			
Obs: First follow-up	989	995	995	995	995	995	989	954	995
Obs: Second follow-up	1152	1151	1069	1151	1150	1071	1142	1141	1156
Obs: Third follow-up		1063	925	1063	1063	927			
Panel B: Existing Firms									
First Follow-up	5.992	40.778	286.407	0.082*	-10.975	24.431	-733.669	671.735	0.066
	(10.160)	(89.075)	(386.195)	(0.043)	(57.133)	(55.678)	(4823.938)	(1127.670)	(0.059)
Second Follow-up	20.329	302.230	1874.552*	0.172***	128.944*	131.023**	100.325	33.633	0.205***
	(12.766)	(401.365)	(1009.407)	(0.038)	(76.449)	(57.770)	(62.374)	(23.394)	(0.058)
Third Follow-up		439.487**	2068.898**	0.095**	70.043	74.256			
		(195.793)	(847.843)	(0.046)	(61.668)	(59.610)			
Control Mean: First follow-up	45.473	519.907	2697.286	0.684	271.504	337.512	9111.524	1773.691	-0.035
Control Mean: Second follow-up	42.167	982.920	4770.229	0.664	225.071	327.765	313.702	122.960	-0.103
Control Mean: Third follow-up		509.975	3367.593	0.516	196.047	296.349			
Obs: First follow-up	420	423	423	423	423	423	420	411	423
Obs: Second follow-up	500	497	458	497	497	458	496	496	501
Obs: Third follow-up		470	409	470	469	409			

Notes: Robust standard errors in parentheses, *, **, *** indicate significance at the 10, 5, and 1 percent levels.

Experimental estimates are ITT estimates and control for randomization strata.

Aggregate index of outcomes includes monthly sales, truncated monthly sales, annual sales, sales higher than one year ago, monthly profits, truncated monthly profits, profits in the best month, inverse hyperbolic sine of profits, number of customers, sales of main product, and mark-up profit.

Appendix 15: Examination of Mechanisms

Appendix Table 15a examines the impacts of winning on several mechanisms that potentially affect firm productivity (A), entrepreneurial skills (E), and access to capital (K). The first column examines entrepreneurial self-efficacy, a measure of the owner's self-confidence in their ability to carry out 12 business-related actions such as "estimate customer demand for a new product", and "identify good employees who can help the business grow". The mean owner is very confident in their ability to do 5 out of 12 tasks, and winning the competition has no impact on this measure. This measure captures a combination of actual skill and confidence, and suggests little change in E.

The second and third columns look at mentoring and use of business networks. Mentoring measures whether the business owner has a mentor they talk to about business matters. This is significant for existing firms in the unconditional regressions, but with one exception, not in the results conditional on operating a business. Network measures the number of other firm owners the business owner discusses business matters with, which is again not significant in the conditional regressions. This lack of change in self-efficacy, mentorship, or networking, suggests the impacts of winning are unlikely to reflect better business knowledge.

Column 4 examines formality, measured in terms of whether the firm has a registered business name, municipal license, and income tax registration. We see a large increase in this measure, which is consistent with the winners needing to register in some form to receive grant payments. The existing global evidence suggests that formality per se does not have measureable impacts on firm productivity or performance for most firms (Bruhn and McKenzie, 2014).

Finally, in terms of access to other sources of capital, there is no significant change in the likelihood of receiving investments from outside equity partners, and a small, but statistically significant reduction in the likelihood of getting a formal loan in the year prior to the second round survey for existing firms. Thus the grant does not appear to have crowded in other sources of finance.

The impact on the aggregate index is significant in the conditional regressions, but largely appears to be picking up the increase in formality.

Appendix Table 15a: Impact on A, E, and Access to K

	Entrepreneurial Self-Efficacy	Has a Mentor	Number in Business Network	Firm is Formal	Took a Formal Loan	Received Equity Investment	Aggregate Outcome Index
Panel A: New Firms							
Unconditional Experimental Impacts:							
First-Follow-up	0.226	0.206***	0.563***	0.084***	-0.003	-0.005	0.185***
	(0.179)	(0.032)	(0.216)	(0.021)	(0.006)	(0.010)	(0.035)
Second Follow-up	0.045	0.381***	1.709***	0.382***	0.003	0.026**	0.460***
	(0.175)	(0.028)	(0.210)	(0.028)	(0.009)	(0.012)	(0.035)
Third Follow-up	0.300*	0.334***	1.711***	0.368***	0.015	0.001	0.417***
	(0.180)	(0.032)	(0.235)	(0.032)	(0.012)	(0.010)	(0.042)
Impacts Conditional on Business in Operation							
First-Follow-up	0.071	0.044	-0.371	0.075**	-0.011	-0.022	0.031
	(0.218)	(0.033)	(0.293)	(0.030)	(0.010)	(0.016)	(0.042)
Second Follow-up	-0.033	0.063**	0.411*	0.323***	-0.008	0.017	0.188***
	(0.206)	(0.025)	(0.247)	(0.034)	(0.012)	(0.015)	(0.037)
Third Follow-up	0.204	0.041	0.438	0.295***	0.001	-0.013	0.205***
	(0.208)	(0.033)	(0.275)	(0.037)	(0.014)	(0.013)	(0.042)
Control Mean: First follow-up	4.765	0.429	2.029	0.067	0.011	0.029	-0.070
Control Mean: Second follow-up	5.523	0.445	1.879	0.114	0.018	0.017	-0.162
Control Mean: Third follow-up	5.513	0.443	1.964	0.126	0.022	0.020	-0.152
Obs: First follow-up	973	995	992	995	995	995	997
Obs: Second follow-up	997	1071	1071	1071	1071	1071	1071
Obs: Third follow-up	859	857	857	857	857	857	979
Panel B: Existing Firms							
Unconditional Experimental Impacts:							
First-Follow-up	0.003	0.114***	0.762**	0.082**	-0.025	0.026	0.111**
	(0.259)	(0.040)	(0.347)	(0.041)	(0.017)	(0.019)	(0.050)
Second Follow-up	0.015	0.150***	1.082***	0.310***	-0.039**	0.030	0.227***
	(0.257)	(0.038)	(0.337)	(0.043)	(0.020)	(0.023)	(0.050)
Third Follow-up	-0.245	0.085**	0.828**	0.242***	0.001	0.001	0.138**
	(0.238)	(0.043)	(0.340)	(0.050)	(0.025)	(0.018)	(0.064)
Impacts Conditional on Business in Operation							
First-Follow-up	0.006	0.046	0.483	0.065	-0.030	0.024	0.044
	(0.269)	(0.036)	(0.361)	(0.045)	(0.019)	(0.021)	(0.048)
Second Follow-up	-0.112	0.027	0.544	0.277***	-0.049**	0.023	0.100**
	(0.267)	(0.033)	(0.348)	(0.047)	(0.023)	(0.025)	(0.048)
Third Follow-up	-0.190	-0.013	0.380	0.204***	-0.007	-0.003	0.081
	(0.251)	(0.040)	(0.346)	(0.053)	(0.028)	(0.020)	(0.057)
Control Mean: First follow-up	5.135	0.726	3.095	0.184	0.042	0.026	-0.066
Control Mean: Second follow-up	5.507	0.719	3.059	0.226	0.063	0.045	-0.125
Control Mean: Third follow-up	6.034	0.736	3.356	0.307	0.061	0.031	-0.100
Obs: First follow-up	418	423	423	423	423	423	423
Obs: Second follow-up	448	458	458	458	458	458	458
Obs: Third follow-up	392	372	372	372	372	372	413

Notes: Control means shown are unconditional means

Aggregate outcome index is the average of standardized z-scores of the other columns.

Robust standard errors in parentheses, *, **, *** indicate significance at the 10, 5, and 1 percent levels.

Experimental estimates are ITT estimates and control for randomization strata.

Most firms in the study produce and sell a wide variety of products, making it difficult to measure changes in their prices. The first and second round surveys asked firms the price per unit of their main product. I use this to examine whether treated firms were more likely to lower prices between rounds 1 and 2, during the period in which they expanded their firm size and received most of the grant. This outcome was not included in the pre-analysis plan, and conditions on survival, so should be considered suggestive only. It shows that winners were no more likely to have lowered the price of their main product than the control group.

Appendix 15b: Treated firms are no likely to lower prices
Dependent variable: lowered price between round 1 and round 2

	Existing Firms	New Firms
Assignment to Treatment	0.005	-0.032
	(0.055)	(0.047)
Sample Size	341	469
Control Mean	0.436	0.517

Notes:

Price is price per unit of main product sold. Data only for sample in business in round 1 and round 2, and so conditions on start-up and survival.

Robust standard errors in parentheses, *, **, *** indicate significance at the 10, 5, and 1 percent levels respectively.

Appendix Table 15c examines the extent to which winning enabled the firm to increase its business inputs of capital and labor. We see owners are working more hours, have more inventories, and have purchased more capital. . Column 2 examines a final source of labor, which is hiring outside consultants. There is no significant impact on this margin for existing firms after conditioning on remaining in business, although perhaps a shift in timing of this use for new firms.

Appendix Table 15c: Impact on Business Inputs

	Owner's Labor Hours	Hours of Consulting Services	Value of Inventories	Made large K purchase	Value of Capital Purchases	Aggregate Outcome Index
Panel A: New Firms						
Unconditional Experimental Impacts:						
First-Follow-up	13.538***	1.874	349.447***	0.289***	1062.394***	0.380***
	(1.961)	(2.897)	(122.713)	(0.031)	(127.841)	(0.042)
Second Follow-up	21.699***	14.146***	1868.767***	0.404***	1542.570***	0.672***
	(1.704)	(2.259)	(349.657)	(0.029)	(142.963)	(0.041)
Third Follow-up	19.526***	-0.016	745.583***	0.113***	183.804*	0.303***
	(1.748)	(3.178)	(182.426)	(0.030)	(109.557)	(0.042)
Impacts Conditional on Business in Operation						
First-Follow-up	5.499***	-1.326	75.781	0.270***	1236.484***	0.295***
	(1.960)	(4.368)	(174.286)	(0.040)	(170.615)	(0.048)
Second Follow-up	4.649***	12.694***	1464.506***	0.299***	1586.015***	0.477***
	(1.697)	(2.632)	(426.952)	(0.036)	(164.168)	(0.044)
Third Follow-up	2.089	-22.658***	353.372	-0.050	-5.410	-0.069
	(1.843)	(4.895)	(240.923)	(0.040)	(160.420)	(0.050)
Control Mean: First follow-up	24.949	7.460	721.444	0.211	345.219	-0.139
Control Mean: Second follow-up	23.928	3.636	925.474	0.206	252.411	-0.240
Control Mean: Third follow-up	19.471	17.927	601.747	0.187	246.931	-0.099
Obs: First follow-up	993	994	991	995	991	995
Obs: Second follow-up	1071	1071	1013	1071	1013	1071
Obs: Third follow-up	927	818	872	927	872	927
Panel B: Existing Firms						
Unconditional Experimental Impacts:						
First-Follow-up	9.058***	7.564	729.308***	0.369***	1356.056***	0.425***
	(2.653)	(5.930)	(267.946)	(0.046)	(184.612)	(0.054)
Second Follow-up	8.643***	6.117**	1319.844**	0.242***	1018.391***	0.340***
	(2.552)	(2.901)	(578.671)	(0.045)	(202.343)	(0.051)
Third Follow-up	11.687***	1.767	1029.584**	0.152***	306.290	0.236***
	(2.492)	(4.449)	(440.225)	(0.048)	(304.020)	(0.058)
Impacts Conditional on Business in Operation						
First-Follow-up	4.819**	6.649	639.108**	0.349***	1350.441***	0.376***
	(2.428)	(6.346)	(288.353)	(0.048)	(195.908)	(0.052)
Second Follow-up	1.717	4.928	982.279	0.175***	961.396***	0.234***
	(2.438)	(3.265)	(665.083)	(0.048)	(222.787)	(0.051)
Third Follow-up	2.012	-4.411	695.914	0.060	145.969	0.060
	(2.310)	(5.612)	(516.818)	(0.055)	(374.417)	(0.063)
Control Mean: First follow-up	43.263	10.521	1222.716	0.358	536.734	-0.234
Control Mean: Second follow-up	40.869	7.941	2226.204	0.434	595.510	-0.183
Control Mean: Third follow-up	31.589	15.457	1360.244	0.307	552.070	-0.125
Obs: First follow-up	423	422	422	423	423	423
Obs: Second follow-up	458	458	453	458	453	458
Obs: Third follow-up	409	335	400	409	400	409

Notes: Control means shown are unconditional means

Aggregate outcome index is the average of standardized z-scores of the other columns.

Robust standard errors in parentheses, *, **, *** indicate significance at the 10, 5, and 1 percent levels.

Experimental estimates are ITT estimates and control for randomization strata.

Appendix 16: Does winning YouWiN! affect reporting of sales and profits?

A concern with any program involving some business training or improvements in record-keeping is that it may lead to changes in the accuracy of information being reported, even if the underlying business financial position does not change. If businesses systematically under- or over-state sales and profits, this will lead to a bias in the measured treatment effect. If this is the case, we might expect changes in the number of inconsistencies or errors in reporting of profits and sales. I consider four reporting errors: a) total sales in the last month exceed total sales in the year to date so far; b) profits in the last month exceed sales in the last month; c) profits in the best month of the year are less than profits in the last month; and d) revenues in the last month from the main product[22] (calculated as price per unit times number of units sold) exceed reported total revenues for the last month. The control group made about 0.8 errors on average in the first round, and 0.12 on average in the second round, with a large part of this drop reflecting better interviewer training on how to ask about the main product. We see that there is no differential treatment effect for existing firms in the number of errors made in either round. Among new enterprises who are in business, treated firms make 0.04 fewer errors than control firms, which is marginally significant at the 10 percent level in the second round. This difference is small in magnitude, and may reflect the selection occurring from the treatment causing more firms to start-up, since these results are conditional on being in operation. Therefore it does not appear that treatment is resulting in large differences in reporting behavior.

Appendix Table 16: Impact of Treatment on Sales and Profit Reporting Errors

| | Existing Firms in Operation | | New Firms in Operation | |
	First Round	Second Round	First Round	Second Round
Experimental Treatment Effect	0.018	0.004	-0.044	-0.048*
	(0.081)	(0.038)	(0.068)	(0.027)
Sample Size	384	413	610	706
Control Mean	0.780	0.115	0.853	0.142

Notes: robust standard errors in parentheses, *, **, *** indicate significance at the 10, 5, and 1 percent levels respectively
Dependent variable is the total number of reporting errors out of 4 made (monthly sales>annual sales, profits>sales, profits in best month<profits in last month, and sales of main product>total sales).
Note main product sales not asked in round 3.

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[22] This variable was not asked in round 3, so we focus only on the first two rounds.

Appendix 17: Heterogeneity in Impact with Respect to Number of Other Winners

This table examines whether the impact of treatment varies with the number of other winners in the same state and industry. We see no heterogeneity in treatment with respect to the number of other winners, suggesting that winners are not taking business from the control group or each other.

Appendix Table 17: No Heterogeneity With Respect to Number of Other Treated Firms

| | Outcomes in Third Follow-up | | | | | |
| | New Firms | | | Existing Firms | | |
	Operates a Firm	10+ workers	Sales & Profits Index	Operates a Firm	10+ workers	Sales & Profits Index
Assignment to Treatment	0.388***	0.230***	0.224***	0.202***	0.256***	0.264***
	(0.033)	(0.037)	(0.057)	(0.046)	(0.060)	(0.092)
assigntreat_numberotherwinners	-0.004	-0.000	-0.013	-0.002	-0.010	-0.010
	(0.005)	(0.005)	(0.009)	(0.007)	(0.009)	(0.012)
numberotherwinners	-0.003	0.000	0.001	-0.001	0.002	0.010
	(0.004)	(0.003)	(0.007)	(0.006)	(0.006)	(0.011)
Sample Size	1085	1044	1063	477	461	470

Notes:

All regressions also include controls for randomization strata. Robust standard errors in parentheses.

*, **, *** indicate significance at the 10, 5, and 1 percent levels respectively.

Appendix 18: Do Initial Characteristics Predict Subsequent Growth?

I examine this question for the sample which submitted business plans.[23] Recall my survey contains all the winners, the control group of semi-finalists, and a sample of other non-winners who were used for the discontinuity analysis. Because winning has a large impact on firm growth, I examine separately for the sample of non-winners and sample of winners the extent to which initial characteristics predict the outcomes of the firm three years later. In addition to the total business plan score (range of 2 to 73, s.d. of 11.2 for non-winners, range of 30 to 91, s.d. of 11.7 for winners), I also consider the subcomponent of the score which marked applicants on their perceived job creation impact (scored out of 25 points, mean of 8.9, s.d. of 5.7). I control for a range of personal characteristics including gender, age, education, experience abroad, risk attitude, ability (measured as a first principal component of the Raven test and Digitspan recall scores), the grit measure of Duckworth et al. (2007), a household wealth index (the first principal component of 20 durable assets), as well as indicators for proposing to operate an agricultural business, manufacturing business, and an IT business. I also control for which of the six regions the individual lives.

I use these characteristics to examine their predictive power for three key outcomes three years after applying: whether the individual operates a business, whether their business has 10 or more workers, and the standardized sales and profits measure. Probit estimation is used for the first two

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[23] The initial application was much less detailed and so I have fewer background characteristics for the subsample who did not make it to the 4-day business plan training.

outcomes, and regression for the third. I use a chi-squared and F-test respectively to test the null hypothesis that the coefficients are all jointly zero, that is, that they have no predictive power for these outcomes.

Appendix Table 18a shows the results for the non-winners. Without any controls, the total business plan score is not a significant predictor of any of these outcomes for new and for existing business applicants. After adding the full set of controls the pseudo-R^2 or R^2 is still 0.1 or less in each case. The job creation subcomponent of the score does have some predictive power for whether an existing firm gets to 10 or more workers – a one standard deviation increase in this score is associated with a 7.8 percentage point higher likelihood of reaching this size when no other controls are added, with this coefficient significant at the 1 percent level. However, for the specification shown, we cannot reject that the coefficients are all jointly zero for existing firms. In contrast for new firm applicants the characteristics do have some predictive power, with the key predictors being that female-operated firms are less likely to start and earn lower profits and sales, and that older entrepreneurs (among this sample of individuals up to age 40) are more likely to start.

Appendix Table 18b provides the same specifications for the winners. Among the winners the overall business plan score again is not a significant predictor for who will grow, but the score for job creation potential does significantly predict which firms get above 10 workers for both new and existing firms. Among new firms we again see that female-owned firms are less likely to grow large, while more able firm owners appear to be more likely to grow. Nevertheless, the overall predictive power is still very low, as indicated by the pseudo-R^2 or R^2.

Appendix 18a: How well can we predict outcomes three years later for non-winners?

	New firm applicants			Existing Firm Applicants		
	Operates a firm	Firm of 10+ workers	Sales & Profits Index	Operates a firm	Firm of 10+ workers	Sales & Profits Index
Business plan overall score	-0.000	0.017	0.004	0.003	-0.012	0.001
	(0.007)	(0.011)	(0.003)	(0.012)	(0.012)	(0.006)
Score for job creation potential	-0.005	0.012	-0.000	0.019	0.078***	-0.002
	(0.013)	(0.016)	(0.007)	(0.024)	(0.026)	(0.014)
Female	-0.469***	0.157	-0.154*	-0.317	-0.029	-0.030
	(0.152)	(0.187)	(0.088)	(0.275)	(0.292)	(0.173)
Age	0.034**	0.034**	0.010	0.033	0.019	0.011
	(0.013)	(0.017)	(0.006)	(0.025)	(0.032)	(0.011)
Postgraduate education	0.231	0.261	0.031	-0.479	0.155	-0.298
	(0.254)	(0.281)	(0.157)	(0.351)	(0.384)	(0.214)
Has lived or worked abroad	0.094	0.012	0.337	0.543	0.133	0.481
	(0.266)	(0.291)	(0.234)	(0.394)	(0.371)	(0.331)
Would choose risky gamble	-0.034	0.464***	0.026	-0.167	0.245	-0.121
	(0.119)	(0.167)	(0.069)	(0.203)	(0.225)	(0.110)
Ability	0.005	0.015	0.004	-0.045	-0.024	0.021
	(0.021)	(0.027)	(0.013)	(0.049)	(0.058)	(0.030)
Grit	0.059	-0.150	0.059	0.051	-0.110	-0.098
	(0.106)	(0.133)	(0.058)	(0.186)	(0.220)	(0.126)
Wealth index	0.104*	0.126*	0.049	0.141	0.145	0.055
	(0.056)	(0.073)	(0.031)	(0.112)	(0.113)	(0.068)
Agriculture	-0.078	0.032	-0.039	0.023	-0.421	-0.030
	(0.138)	(0.171)	(0.083)	(0.268)	(0.329)	(0.151)
Manufacturing	-0.048	-0.251	-0.100	0.588**	0.232	-0.031
	(0.156)	(0.223)	(0.083)	(0.299)	(0.308)	(0.143)
IT	0.080	-0.422	-0.190**	-0.093	-0.181	-0.045
	(0.216)	(0.279)	(0.088)	(0.311)	(0.365)	(0.190)
Pseudo-R2 or R2	0.040	0.081	0.053	0.089	0.105	0.068
Sample Size	520	498	512	196	184	193
Test coefficients all zero p-value	0.071	0.033	0.027	0.298	0.448	0.826

Notes: robust standard errors in parentheses, *, **, *** indicate significance at the 10, 5, and 1 percent levels respectively. Region fixed effects included but not shown.

Columns 1,2,4, and 5 report marginal effects from probit estimation, columns 3 and 6 regression coefficients. Ability is the first principal component of the digitspan and raven test scores; wealth index is the first principal component of household assets at application. Industries are those proposed on business plan.

Appendix 18b: How well can we predict outcomes three years later for winners?

	New firm applicants			Existing Firm Applicants		
	Operates a firm	Firm of 10+ workers	Sales & Profits Index	Operates a firm	Firm of 10+ workers	Sales & Profits Index
Business plan overall score	-0.002	-0.013	0.002	0.003	0.001	0.012
	(0.013)	(0.009)	(0.004)	(0.013)	(0.007)	(0.008)
Score for job creation potential	-0.021	0.038***	0.007	0.040	0.040***	-0.005
	(0.017)	(0.014)	(0.005)	(0.028)	(0.013)	(0.010)
Female	-0.021	-0.371*	-0.181***	0.185	-0.150	-0.205*
	(0.266)	(0.202)	(0.060)	(0.306)	(0.177)	(0.121)
Age	0.030	0.007	0.004	0.057**	0.010	0.017
	(0.024)	(0.016)	(0.007)	(0.026)	(0.014)	(0.011)
Postgraduate education	0.251	-0.002	-0.072	0.159	-0.190	0.161
	(0.489)	(0.312)	(0.101)	(0.299)	(0.209)	(0.187)
Has lived or worked abroad	0.022	-0.074	-0.048	-0.699**	-0.052	-0.215
	(0.354)	(0.261)	(0.093)	(0.292)	(0.196)	(0.143)
Would choose risky gamble	-0.060	-0.016	-0.011	0.032	-0.142	0.080
	(0.194)	(0.140)	(0.058)	(0.209)	(0.122)	(0.149)
Ability	-0.044	0.057**	-0.003	0.044	0.057**	0.055***
	(0.030)	(0.026)	(0.010)	(0.037)	(0.026)	(0.016)
Grit	-0.044	-0.104	0.002	0.014	-0.090	-0.103
	(0.164)	(0.125)	(0.056)	(0.189)	(0.108)	(0.110)
Wealth index	-0.042	-0.065	-0.011	-0.122	0.056	-0.052
	(0.096)	(0.068)	(0.028)	(0.094)	(0.061)	(0.055)
Agriculture	-0.039	0.120	-0.104	-0.124	-0.143	-0.161
	(0.225)	(0.165)	(0.071)	(0.275)	(0.162)	(0.123)
Manufacturing	-0.253	0.315	-0.195***	0.113	-0.109	0.137
	(0.243)	(0.196)	(0.073)	(0.307)	(0.159)	(0.178)
IT	-0.296	-0.021	-0.132	-0.004	-0.231	-0.266*
	(0.361)	(0.296)	(0.102)	(0.274)	(0.177)	(0.145)
Pseudo-R2 or R2	0.056	0.055	0.064	0.108	0.052	0.06
Sample Size	373	368	367	477	475	476
Test coefficients all zero p-value	0.324	0.097	0.049	0.011	0.021	0.002

Notes: robust standard errors in parentheses, *, **, *** indicate significance at the 10, 5, and 1 percent levels respectively. Region fixed effects included but not shown.

Columns 1,2,4, and 5 report marginal effects from probit estimation, columns 3 and 6 regression coefficients. Ability is the first principal component of the digitspan and raven test scores; wealth index is the first principal component of household assets at application. Industries are those proposed on business plan.

Appendix 19: Which Firms Have Larger Treatment Impacts?

Three key dimensions of heterogeneity of policy interest for the program are whether impacts are larger for new or existing firm applicants, how impacts vary with gender, and how impacts vary with the business plan score.

As noted, there was a preference given to existing over new firms in the design of the first year of the YouWiN! competition, based on the belief that firms that were already in existence would be more likely to survive and grow than new firms. However, tables 3 and 4 show that the job creation impacts over the first three years were larger for the new firm winners: the average new firm experimental winner created 12.7 job-years over the first three years, compared to 8.4 job-years for existing firms.

Gender is a second important dimension of heterogeneity for the program. Females constituted only 18 percent of applicants and 17.6 percent of winners for the first round of YouWiN!, leading to the second year of the program to be a call only for female business owners. We have seen among new firm applicants (appendix 18) that female applicants were less likely to grow their firms. Panel A of Table 19a examines the heterogeneity in treatment effects with respect to gender for new applicants. We see a positive and significant interaction effect on gender with respect to whether a firm is started up and operating for each of the three rounds. Here the control means for females are below those for males, and the treatment is closing this gap. In contrast, despite female-operated firms being less likely to exceed 10 workers, and having lower profits and sales, we do not see any significant treatment impact on closing these gaps. For existing firms, appendix 19b shows that female-owned existing firms are not less likely to survive than male-owned firms, and there is no significant gender treatment interaction for these firms.

Panel B of appendix tables 19a and 19b examines the heterogeneity of treatment effects by business plan score among the experimental sample. For new firms we see that control firms with higher business plan scores are more likely to have started a business than those with lower scores, and in the short-run are more profitable. The treatment interaction effects then show winning the competition has slightly smaller effects on start-up in the first year, and on profits and sales in the second year for those with higher scores. In contrast, none of the treatment interactions with the business plan score are statistically significant for the existing firms. These results then also suggest that the cause of the higher estimated treatment effects for the national and zonal winners compared to the ordinary winners is likely selection, rather than winning having more impact for them. That is, propensity-score matching, but matching only on observables, is not able to account for some of the growth that these firms would have experienced anyway.

Appendix Table 19a: Heterogeneity in Treatment Impacts for New Firm Applicants

	Operates a Firm			Total Employment			Profits and Sales Index		
	Round 1	Round 2	Round 3	Round 1	Round 2	Round 3	Round 1	Round 2	Round 3
Panel A: Heterogeneity by Gender									
Assigned to Treatment	0.185***	0.341***	0.354***	1.411	6.119***	4.976***	-0.002	0.282***	0.159***
	(0.032)	(0.025)	(0.026)	(0.860)	(0.471)	(0.508)	(0.054)	(0.041)	(0.048)
Assigned to Treat*Female	0.189**	0.104*	0.120*	0.100	-0.638	1.557	0.119	0.093	0.055
	(0.078)	(0.063)	(0.067)	(1.021)	(0.895)	(1.321)	(0.084)	(0.077)	(0.099)
Sample Size	1021	1181	1085	987	1159	1044	995	1152	1063
Control Mean Females	0.420	0.481	0.422	1.674	2.165	2.883	-0.233	-0.239	-0.144
Control Mean Males	0.574	0.586	0.562	3.964	3.539	3.937	0.035	-0.067	-0.032
Panel B: Heterogeneity by Business Plan Score									
Assigned to Treatment	0.677***	0.484***	0.371***	-0.305	3.462	2.456	0.451	0.723***	0.211
	(0.173)	(0.141)	(0.141)	(3.642)	(2.274)	(2.686)	(0.319)	(0.210)	(0.241)
Assigned to Treatment*Business Plan Score	-0.009***	-0.002	0.000	0.032	0.047	0.051	-0.008	-0.008**	-0.001
	(0.003)	(0.003)	(0.003)	(0.075)	(0.043)	(0.051)	(0.006)	(0.004)	(0.004)
Business Plan Score	0.004	0.001	0.001	0.053	0.054*	0.071*	0.012**	0.007**	0.005
	(0.003)	(0.003)	(0.003)	(0.037)	(0.032)	(0.039)	(0.005)	(0.003)	(0.004)
Sample Size	1021	1181	1085	987	1159	1044	995	1152	1063
Control Mean Bottom Quartile	0.513	0.548	0.563	3.497	3.263	4.372	-0.064	-0.131	-0.065
Control Mean Top Quartile	0.592	0.604	0.567	3.542	3.556	4.353	0.081	-0.034	-0.062

Notes:

Robust standard errors in parentheses, *, **, *** indicate significance at the 10, 5, and 1 percent levels.

Experimental estimates are ITT estimates and control for randomization strata.

Appendix Table 19b: Heterogeneity in Key Outcomes for Existing Firms

	Operates a Firm			Total Employment			Profits and Sales Index		
	Round 1	Round 2	Round 3	Round 1	Round 2	Round 3	Round 1	Round 2	Round 3
Panel A: Heterogeneity by Gender									
Assigned to Treatment	0.092***	0.139***	0.185***	1.553*	2.176	4.348***	0.073	0.242***	0.227***
	(0.032)	(0.029)	(0.035)	(0.864)	(1.628)	(0.685)	(0.076)	(0.066)	(0.076)
Assigned to Treat*Female	-0.060	-0.051	0.061	-0.267	2.182	0.477	0.049	-0.033	-0.089
	(0.045)	(0.059)	(0.083)	(2.211)	(2.571)	(2.293)	(0.191)	(0.162)	(0.194)
Sample Size	432	505	477	422	500	461	423	497	470
Control Mean Females	0.967	0.886	0.722	7.862	7.364	6.091	0.027	-0.048	-0.009
Control Mean Males	0.854	0.834	0.766	6.669	8.309	5.475	-0.058	-0.132	-0.126
Panel B: Heterogeneity by Business Plan Score									
Assigned to Treatment	-0.022	-0.041	-0.047	1.153	9.559*	6.290	0.477	0.461	-0.329
	(0.166)	(0.154)	(0.201)	(4.506)	(5.577)	(4.671)	(0.471)	(0.357)	(0.465)
Assigned to Treatment*Business Plan Score	0.002	0.004	0.005	0.008	-0.154	-0.040	-0.009	-0.005	0.012
	(0.004)	(0.003)	(0.004)	(0.101)	(0.140)	(0.099)	(0.011)	(0.008)	(0.011)
Business Plan Score	-0.004	-0.005	-0.004	-0.007	0.112	-0.025	0.003	-0.003	-0.006
	(0.003)	(0.003)	(0.004)	(0.083)	(0.117)	(0.063)	(0.009)	(0.006)	(0.007)
Sample Size	432	505	477	422	500	461	423	497	470
Control Mean Bottom Quartile	0.944	0.844	0.814	7.962	6.391	6.456	0.027	-0.167	0.026
Control Mean Top Quartile	0.852	0.849	0.778	7.698	10.873	5.417	0.072	-0.093	-0.092

Notes:

Robust standard errors in parentheses, *, **, *** indicate significance at the 10, 5, and 1 percent levels.

Experimental estimates are ITT estimates and control for randomization strata.

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