Access-to-demand Frictions and Firm Growth: Experimental Evidence from Liberia*

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December 2, 2019

PRELIMINARY AND INCOMPLETE: DO NOT CITE

Abstract

We hypothesize that many productive firms in poor countries stagnate due to informational barriers to accessing existing demand. To investigate, we gave a randomly chosen subset of Liberian firms the opportunity to participate in a seven day-long training program. The program exclusively teaches how to bid on contracts from large buyers that are awarded through a formal procurement process. Overall, the program increased the number of bids firms submit; the total number and quality of contracts won; and the number of contracts won through other channels than a formal bidding process. We then show via a regularization procedure that, relative to otherwise similar firms, the impact of the program is especially large for firms that use the Internet at baseline. We interpret these results through a simple theoretical framework in which a "keys-to-the-door" training program facilitates firms' growth by boosting their ability to win contracts they bid on, and firms that face lower costs of finding and selecting appropriate contracts to bid on—for example those that use the Internet—benefit more. This interpretation is supported by the way in which the differential impact of the program for firms that use the Internet varies with the *share* of tenders for contracts published around the time of treatment that are published online. In sum this paper's findings suggest that, to grow, firms need both knowledge of how to win contracts and the technology necessary to cost-effectively access demand.

*hjort@columbia.edu, vinayak.iyer@columbia.edu,gd2382@columbia.edu We thank the IGC for financial support, IPA and Q&A Liberia for great field-work, seminar participants at several universities and conferences for helpful comments, and Building Markets for data-sharing and excellent collaboration. The project was registered in the AEA RCT Registry, ID 0002264.

1 Introduction

Economists have traditionally focused on firms' *production* capabilities, but goods and services also need to be *sold*. When otherwise stagnant firms in poor countries are awarded contracts through randomized-demand-allocation programs that side-step variation in ability to find and appeal to suitable buyers, many successfully deliver (Ferraz *et al.* , 2016; Atkin *et al.* , 2017; Carrillo *et al.* , 2019).

Nevertheless, small and medium-sized enterprises (SMEs) rarely win valuable contracts in business-as-usual markets. Information frictions help explain why (Allen, 2014; Cai & Szeidl, 2017; Campante & Yanagizawa-Drott, 2018; Startz, 2018; Hansman *et al.* , 2019; Hjort & Poulsen, 2019). However, the *largest* buyers—multinationals and government entities that SMEs especially benefit from supplying (Abebe *et al.* , 2017; Lee, 2017; Alfaro-Urena *et al.* , 2019)—are by nature highly visible. The reasons why capable and trustworthy suppliers fail to secure contracts with such right-tail buyers presumably go beyond pure search and contracting frictions. The literature on job-finding by *workers*—in particular, evidence that enhancing job-seekers' ability to convey their qualifications to employers can dramatically increase job-finding rates (Hardy & McCasland, 2017; Abebe *et al.* , 2019; Bassi & Nansamba, 2019; Carranza *et al.* , 2019)—points towards another constraint. Might productive firms' ability to secure contracts from desirable buyers be constrained by informational barriers not only to finding, but also to *appealing*, to such buyers?

In this paper we show that Liberian firms face stark and multi-dimensional access-to-demand frictions. Our analysis has four parts. First we estimate the average impact of a training program that simply teaches firms how to write good bids on contracts from large buyers. Next we lay out a simple theoretical framework that highlights how and why we expect productive firms' de facto market access to depend on the interaction between their *contract-winning skills* and their access to *tender-information*. We then show empirical support for this model by showing that the impact of the training program varies with a measure of tender-information: firms' use of the Internet. Finally we take advantage of quasi-random variation in the number of online- and offline-tenders a given firm may be able to access during the experiment. These demand shocks help us unpack the nature of Liberia's access-to-demand frictions and to show *why* firms need both information and "appeal" to grow.

If simply taught how to bid on contracts from large buyers, do small and medium-sized firms in a low-income country do so? Do large buyers award contracts to such firms if they bid? Are technological means of finding and selecting appropriate contracts to bid

on necessary for firms to benefit from enhanced ability to make good bids? These questions start where our still modest understanding of the forces that constrain productive firms in developing countries ends. To begin to answer them,

The firms in our sample are by local standards medium-sized, but have little experience supplying large buyers. The week-long training we evaluate is run by a non-profit. It teaches firms how to bid on tenders from large firms and government entities that are awarded through a formal procurement process. The training is not sector-specific and focuses exclusively on how to bid. The non-profit provided regular training sessions throughout the period of the experiment. Firms in the treatment group were visited at the beginning of the experiment and given a free voucher to attend the training. They were also informed that the training appears to have been beneficial for firms similar to theirs who took the training in the past.

Our primary results are as follows. The encouragement (voucher+information) successfully persuaded many firms to take the training. The firms who took the training bid on more contracts; are more likely to win contracts; and the contracts they win are of higher quality. For example, treated firms are more likely to win a contract lasting more than six months.

To guide our interpretation of these results we use a simple conceptual framework of contests with asymmetric firms. In the framework, a "keys-to-the-door" training program like the one we study increases firms' ability to win contracts they bid on, and firms that face lower costs of finding and selecting appropriate contracts to bid on—for example those that use the Internet—benefit more.

Next we provide support for this interpretation by taking advantage of different publication methods of tenders for contracts. This allows us to test whether firms with better tender-information are benefitting more from the training. We hypothesize that firms who regularly use Internet for business purposes will have better information about tenders published online. We show that the firms that benefit the most from the training are those who make use of information and communication technology: the impact of treatment on the number of bids submitted and contracts won is considerably bigger for firms that use the Internet for business purposes at baseline. This result holds also when we additionally interact the treatment with a range of correlates of baseline Internet use; a LASSO regularization procedure suggests that baseline Internet use is in fact the strongest predictor of treatment effect heterogeneity.

Finally, we exploit variation in the number of tenders for contracts that are published online over time. We find that, relative to otherwise similar treated firms, those that use the Internet benefit differentially more from the training if they are exposed to a greater

number of online tenders after encouragement.

In sum this paper documents that Liberian firms face severe informational access-to-demand frictions. The implied inequality of opportunity across firms of similar productivity might help explain the slow average growth of firms in developing countries' distorted private sectors (Hsieh & Klenow, 2014). Our results suggest that firms need both knowledge of how to market their products and the technology necessary to cost-effectively find contracts to bid on. It may thus be possible to enhance growth through comparatively simple informational policy tools.

We contribute to two related strands of the literature on the forces that constrain firms in developing countries. First, this paper is to our knowledge the first to experimentally estimate the consequences of reducing access-to-buyers frictions. The literature on infrastructure and other trade barrier improvements makes clear that market access is important for firms' growth (see e.g. Donaldson & Hornbeck, 2016). Building on this insight, recent work takes a more microeconomic approach by *directly* giving firms contracts (Ferraz *et al.* , 2016; Atkin *et al.* , 2017), hence zeroing in on the importance of demand itself. We view our approach of giving firms "the key to the door" as complementary to studies in which contracts are randomly allocated. In business-as-usual markets, while policy can lower *access* barriers, firms themselves must *win* contracts to survive and grow. In contrast, policymakers can directly control who wins contracts in "managed" markets such as public procurement.

Second, this paper begins to unpack the nature of the information frictions that can plague markets in developing countries. The *importance* of information frictions is well-established (Allen, 2014; Startz, 2018; Hjort & Poulsen, 2019; Mitra *et al.* , 2018; Hansman *et al.* , 2019; Jensen & Miller, forthcoming). However, the *nature* of information frictions, and the forces that constrain intrinsically productive firms in business-as-usual developing economies, remains poorly understood.[1] Accordingly, interventions aimed at solving these information frictions have had mixed results (McKenzie & Woodruff, 2014). We document an intervention that tackles a particular form of knowledge, how to access demand from large buyers.

The rest of the paper is organized as follows. Section 2 presents the context and the experiment. Section 3 investigates the average effect of the treatment for all firms. Section 4 presents the conceptual framework and Section 5 shows corresponding empirical evidence on heterogeneity in the impact of the training. Section 6 concludes.

[1] An important exception is Bloom *et al.* (2013)'s evidence that larger firms in developing countries lack knowledge of how to manage operations well.

2 Context and Experimental Design

In this section we describe the Liberian private sector, and the experiment we ran.

2.1 Liberia's private sector

Firms in Liberia In 2013, a national economic census was conducted aimed at counting any business with a solid physical structure in Liberia. The census reports data on 21,500 firms and confirms common stylized facts observed in other low-income countries. In particular, most firms in Liberia are small. While the average firm has 7.3 employees, 63 percent have less than three employees and 98.5 percent have less than 50.

Procurement by large buyers The non-profit we work with attempts to record all formal tenders in Liberia, and notifies all firms in its directory operating within the relevant sector of all tenders and contracts captured in its database. In 2016, they recorded 1,381 tenders. Summary statistics of these tenders are shown in Table 1. A little more than half the tenders were from public sector buyers; a small minority (about 2 percent) from private companies; and the remainder from international organizations.[2] Most tenders were posted publicly: 57 percent in newspapers and another 31 percent online. About 12 percent of tenders were publicized only through word-of-mouth, requiring that firms hear about the tender through the buyer itself, another supplier, or the non-profit, to be able to bid.[3] Most government entities report that they have a hard time finding SMEs that meet the requirements to fulfill their contracts.

2.2 Sample

The firms in our sample were recruited through the non-profit's directory of active firms in Liberia. The organization collects information abp goal is to connect local small and medium-sized firms to supply chains and contracts. Among other services, they publish an online directory of local firms similar to the Yellow Pages in advanced countries. The

[2]Big public sector buyers included multiple central government authorities, while international buyers included well-known international organizations. A policy goal of the Government of Liberia is to help small businesses grow. In 2014, they passed a law entitled the "Small Business Empowerment Act", which mandates all government entities to allocate at least 25 percent of their total procurement budget towards Liberian-owned small and medium-sized firms. However, since the act was passed, very few government entities have met the threshold.

[3]A majority of tenders advertised by Liberian entities were published in newspapers while about half of tenders published by international organizations were published online.

directory covers more than 2,000 Liberian firms. Four different criteria were used to define our sampling frame. To be included, firms had to:

- Be listed on the non-profit's business directory
- Have not already taken the access-to-buyers training
- Have at least one employee in addition to the owner
- Be located in Monrovia, the capital city

In addition, since a lot of firms closed down after the 2014-2016 West African Ebola outbreak, only firms that had been in contact with the non-profit after April 2015 (when the outbreak subsided) were included in the sampling frame.

The non-profit keeps track of the firms in its directory via periodical surveys, and the data they collect allows us to describe the sample of firms in our experiment before the treatment started. Table 2 shows summary statistics of firms in our sample. The sample represents various sectors, with about one fourth of firms in the "Construction and Renovation" sector. The vast majority of firms have at least one Liberian owner, while five percent have at least one Lebanese owner. 34 percent of the managers speak at least one local language—we categorize these firms as run by native Liberians. 21 percent of firms applied to at least one tender in the six months before being interviewed, and 12 percent were successful in winning at least one contract through a bid. The average success rate—the number of contracts won through a bid divided by the number of bids submitted—is 32 percent.

2.3 Treatment

We randomly assigned the firms in the sample to treatment (772 firms) and control (420 firms) status. The randomization was stratified on number-of-employees bins, sector, and geographical zone within Monrovia. The treatment and control groups are balanced, as shown in the first two columns of Table 3.

Firms in the treatment group were visited in June 2016 and offered a free voucher to attend the non-profit's bid trainings. The voucher allowed one person from the firm to attend the training sessions for free.[4] The firms were also asked to answer a survey and given information about the training. This information included the training's content, as well as statistics on how participation correlated with behavior and various measures of

[4]The voucher did not have an expiration date and could be used when the firms desired.

success for firms like theirs in the past, as measured in data from the non-profit's periodical surveys.[5]

The first bid training session lasts one week and is referred to as "General Procurement" training. This training teaches firms the fundamentals of the bidding process such as how to find tenders and how to bid, but also features commonly required in submitted bids. Understanding these requirements is an important aspect of the training because, to institutions posting the tenders, they very often seem like easy-to-meet standards when in fact they refer to concepts that are often unclear to firms. An example of such requirement is environmental awareness: a lot of small and medium firms in Liberia de facto use very little energy and would be considered "green businesses", but they might fail to mention this in their application. Accordingly, the first week of training has modules on environmental awareness, ethics, cultural differences and sensitivity to persons with disabilities.

This first week of training is a requirement for the second training session, called "Bid Compilation" training. This second session lasts two days and offers a hands-on toolkit for producing bids. During this session, participants are asked to go through four different exercise, in which they examine a mock tender, prepare a bid proposal, learn to communicate with procurement officers, undergo evaluation of their bid and prepare a contract. The key part of the second session of the training is that it teaches how to engage with institutions at the different stages of the tender process – bidding to contracting.

The non-profit offered two to three training sessions per month, depending on demand, and a total of eight sessions. The treatment group firms who took the training as part of the experiment did so from June 2016 to January 2017. Training was in groups and not sector-specific: on average training sessions gathered 32 attendees from 11 different sectors. Nothing in the syllabus of the training is aimed at changing the productivity of the firm, and in particular there is no mention of management practices, financial planning or product ideas. Figure 1 shows the timeline of the experiment.

2.4 Data collection

The analysis in this paper is based on three datasets: pre-baseline, baseline, and endline.

The first dataset was collected by the non-profit. Firms on the non-profit's online directory are asked to answer a phone survey every three to six months. The data collected through these phone surveys has been made available to the research team and is what

[5] As specified in the pre-analysis plan, several different sub-treatments were used to encourage firms to attend the training. However, we do not observe differences in effects of these sub-treatments in either take-up or effect of the treatment. In the analysis presented here, all sub-treatments are combined.

we refer to as pre-baseline data.

The second dataset was collected by the researchers in June 2016, when firms in the treatment group were visited and given the voucher. Firms in the treatment group were also asked to answer a survey, the data from which we refer to as baseline data. By construction, this dataset only contains information on firms in the treatment group.

Lastly, the research team attempted to re-interview all firms in the sample for an endline survey conducted from March to June 2017. Out of the 1,192 firms in the sample, we successfully (re-) surveyed 831 firms: 295 firms in the control group, and 533 firms in the treatment group.

Figure 2 presents an overview of firms in each step of the process. Not all firms in the sample answered the endline survey, and firms who answered the endline survey are slightly different from firms who did not.[6] However, the last two columns of Table 3 shows that the treatment and control firms in the restricted sample who answered the endline survey are balanced.

The following section looks at the effect of the encouragement on firm behavior and firm outcomes at endline.

3 Average Impact of Gaining Access to Large Buyers

In this section, we explore the average effect of the treatment on firm behavior and outcomes. We expect firms who received a free voucher to be more likely to attend bid training and be more successful at winning contracts. In later sections, we explore heterogeneity in the impact.

We show results from both Intent-to-treat (ITT) regressions of the outcomes of interest on treatment status and treatment-on-the-treated (TOT) regressions like the following:

$$y_i = \beta_0 + \beta_1 \text{BidTraining}_i + \gamma X_i + \epsilon_i \tag{1}$$

Here y_i is a measure of firm i's endline behavior, expectations, or performance, and X_i is a set of controls. BidTraining_i is an indicator variable equal to one for firms that participated in the bid training, and β_1 is the coefficient of interest. We focus primarily on TOT regressions in which we instrument for BidTraining_i with firms' randomly assigned treatment status.

[6] Table A.1 of the appendix shows attrition rates. Table A.2 shows summary statistics for firms in the restricted sample who answered the endline interview.

3.1 Take-up of training and firms' expectations

Firms in the treatment group are significantly more likely to attend the training. Firms in the control group were not encouraged to attend the training, but had the possibility of paying to do so. Only four firms in the control group decided to attend. Table 4 shows estimates from regressing a dummy for training attendance on assignment to treatment. Columns (1) and (2) show that the encouragement treatment increased the probability of attending the training by 19-20 percentage points.[7]

Table 4 is based on the non-profit's training attendance sheet. Appendix Table A.3 looks at the effect of treatment on self-reported training attendance. This includes *any* type of training, including the non-profit's bid training. The encouragement significantly increased the probability of the firm attending any training by 14 percent, and the number of training sessions attended by 0.35—an increase of 50 percent compared to the control group.

Participating in the access-to-demand training made firms more optimistic about their own future effort and outcomes. The estimates in Table 5 shows that the training induced firms to believe that they would bid on about one—or 50 percent—more contracts in the coming six months, and that they would also win about 50 percent more of the contracts they bid on.

3.2 Impact on how many contracts firms bid on and win

We find that the treatment significantly increased the number of contracts won. We show this in Table 6. Treatment-on-the-treated (TOT) estimates are in the top panel, and Intent-to-treat (ITT) estimates in the bottom panel. As expected, the ITT estimates are smaller than the TOT estimates, but both are statistically significant.

First note that, while bidding on contracts is relatively rare for the firms in our sample—in the control group, firms bid on 0.35 contracts in the past six months on average—winning a contract through a formal bidding process is even more rare. Control group firms won an average of 0.16 contracts through a bidding process in the past six months,

[7] These numbers are for the first part of the access-to-buyers training, the General Procurement session. Panel A of Appendix Table A.3 shows the same coefficient for the second session, on Bid Compilation. Given that almost all the firms who attended the first session also attended the second session (85 percent), the coefficients for the Bid Compilation training are similar to the coefficient for the General Procurement training: treatment increased the probability of attending the second session by 17 percent. In the results that follow, we will estimate the local average treatment effects on firms who took the General Procurement training. Note also that, following the recommendations of Abadie *et al.* (2017), we present robust standard errors as there are neither *sampling design* nor *experimental design* reasons for clustering in our context. However, our results are robust to clustering at the sector level.

as shown in Column (6) of Table 6.

The ITT estimates show that the encouragement significantly increased the number of bids submitted by 0.16, an increase of nearly 50 percent compared to the control group. Treated firms are also more successful at winning contracts through formal bidding processes; the ITT estimates show an increase of 53 percent compared to the control group. The magnitude of the TOT estimate is larger, as expected, and significant at the 10 percent level.

The effect of the training goes beyond helping firms win contracts through formal bidding processes. To see this, we look at the impact of training on contracts that do not require a bid, i.e., the number of contracts won without formal tenders. Treatment increased the number of such contracts won by firms in the treatment group by 0.2, an increase of 60 percent compared to the control group. Column (4) in Panel A of Table 6 shows a positive impact of treatment on the overall number of contracts won, with firms who took the training winning more than one extra contract—an increase of more than 200 percent.

The last two columns of Table 6 look at the effect of the treatment on the value of contracts won. The results suggest that learning how to access demand from big buyers raised the total value of contracts won by around USD 10,000, or about 200 percent. Although remarkably large, these estimates are not significant, perhaps because—as is common in firm surveys—many managers were unwilling to answer questions about the value or sources of their contracts.[8]

3.3 Impact on the quality of contracts firms bid on and win

In Appendix Table A.4 we show that firms that learn how to access big buyers bid on and win a greater proportion of all formal contracts advertised for goods and services the firms specialize in. The outcome variables are now the number of contracts a firm bids on or wins as a proportion of the total number advertised within the primary sector the firm operates in. The total number of contracts in a sector is captured in the non-profit's database of tenders. The estimates show that the firms that were induced to take the bid training by the encouragement chose to bid on a more than 500 percent higher proportion of all same-sector contracts, and won a 600 percent higher proportion of such contracts, than firms in the control group.

Learning how to access demand from big buyers also impacted the quality of con-

[8]We treat such missing values as zeroes. Appendix Table A.5 shows that firms in the treatment group were weakly more likely not to answer value-of-contracts-won questions, suggesting that we may be underestimating the impact on value of contracts won.

tracts won. To see this, Table 7 shows the impact of treatment on whether firms had a contract lasting longer than six months. The proportion of firms who won such contracts increased by 40 percent in the treatment group: treated firms who took the training more than doubled their chances of winning long-lasting contracts. Appendix Table A.5 looks at a second measure of contract quality: whether the contract was from an international client. We find that the bid training doubled a firm's chances of winning contracts from international clients. These results indicate that the treatment not only increased the quantity of contracts but also the quality of contracts won.

In sum, giving Liberian firms the opportunity to learn how to access big buyers had a remarkably strong impact on firms' expectations about their own future outcomes; the number of bids for contracts submitted; the number of contracts won; and the quality of those contracts.

These results contrast with the mixed results of the business training literature (McKenzie & Woodruff, 2014; Grimm & Paffhausen, 2015). They show that a business training teaching a specific and easy-to-apply set of skills focused on access-to-demand has a stronger effect on firm outcomes, compared to trainings usually evaluated in the literature which focus on increasing firm productivity in the long term, such as financial planning or accounting.

3.4 Do gains come at the expense of other firms?

The training increased the possibility for small firms to win contracts with big buyers. One possibility is that gains to the treated firms come at the expense of other firms which may be worse off as a result of the experiment. If this is the case then the assumption that control firms are not affected by the treatment is violated. This does not put into question the main result – that giving firms the opportunity to learn how to access big buyers had a remarkably strong impact on firms' outcomes – but raises questions about internal and external validity and has implications for both the scalability of the experiment and policy implications. We want to raise three points on this question.

First, under the assumption that the total number of tenders published is constant, the effect of training on contracts won has to come at the expense of other firms (control or out of sample). However, trained firms also submit more bids which, unlike contracts, are not excludable. This means that while we cannot claim that the experiment increased the total number of *contracts*, the experiment did increase the overall number of *bids*.

Second, these new bids submitted by small and medium firms are competitive bids and, sometimes, winning bids. As a result, the reallocation of contracts to trained firms

from either control or out-of-sample firms shows that the training increased the pool of potential suppliers the buyers considered. This in itself is a gain for the buyer, and also shows that we would expect overall welfare implications to be positive.

Third, we hypothesize that the small and medium firms in our sample are winning contracts that were previously allocated to out-of-sample bigger firms. In fact, small and medium-size firms are over-represented in our sample. Figure 3 shows that compared to the distribution of firm sizes in the census and in a sample of benchmark[9] firms, our sample includes fewer firms from higher size categories.

A policy goal of the Government of Liberia is to reallocate procurement contracts towards smaller firms. In 2014, they passed a law entitled the "Small Business Empowerment Act", which mandates all government entities to allocate at least 25 percent of their total procurement budget to Liberian-owned small and medium-sized firms. The rationale behind such a policy is that while while procurements contracts may not represent a big sale for large suppliers, they can represent a "foot-in-the-door" for small businesses. However, since the act was passed, very few government entities have met the threshold. Helping small and medium businesses to submit more competitive bids plays an important role in reaching this goal.

4 Theoretical Framework

This section introduces a simple theoretical framework which will guide our investigation of how and why gaining access to large buyers benefits firms.

Consider a simple contest model with two firms where each firm exerts effort e_i to win a prize of value V. Firm i wins the contest with probability $p_i(e_1, e_2)$, which depends on its own effort and the effort exerted by the other firm. Specifically, the probability of Firm i winning the contest is :

$$p_i(e_1, e_2) = \frac{\alpha_i e_i}{\alpha_i e_i + \alpha_j e_j}$$

where α_i captures the overall ability of Firm i to win the contest, conditional on *bidding* i.e. it captures the *contract-winning abilities* of Firm i. To see this, if $e_i = e_j$, then the firm with the higher α_i wins the contest with a higher probability.

To increase their probability of winning the contest, firms have to pay the cost of effort

[9]Benchmark firms include firms who took part in the experiment as well as firms which were in the registry of firms of the non-profit we work with, and which could have been part of the experiment had they not taken the training in the past.

$\frac{t_i}{1+t_i}e_i$ where $\frac{t_i}{1+t_i}$ represents the marginal cost of an extra unit of effort. In this stylized setting, t_i captures the firms' *access to tenders* with a higher value of t_i implying lower access to tenders. Given that some tenders are only published online, a firm with access to the internet will experience lower t_i, and thus lower costs of effort.

Given the model setup, the profit of both firms is given by :

$$\Pi_1 = \frac{\alpha_1 e_1}{\alpha_1 e_1 + \alpha_2 e_2} V - \frac{t_1}{1+t_1} e_1$$
$$\Pi_2 = \frac{\alpha_1 e_1}{\alpha_1 e_1 + \alpha_2 e_2} V - \frac{t_2}{1+t_2} e_2$$

The first order conditions with respect to e_1 and e_2 are given by :

$$V\left[\frac{\alpha_1 \alpha_2 e_2}{(\alpha_1 e_1 + \alpha_2 e_2)^2}\right] - \frac{t_1}{1+t_1} = 0$$
$$V\left[\frac{\alpha_1 \alpha_2 e_1}{(\alpha_1 e_1 + \alpha_2 e_2)^2}\right] - \frac{t_2}{1+t_2} = 0$$

Which yields the equilibrium effort levels e_1^* and e_2^* :

$$e_1^* = \frac{\alpha_1 \alpha_2 t_2 (1+t_1)^2 (1+t_2) V}{(\alpha_1 t_2 (1+t_1) + \alpha_2 t_1 (1+t_2))^2} \tag{2}$$
$$e_2^* = \frac{\alpha_1 \alpha_2 t_1 (1+t_2)^2 (1+t_1) V}{(\alpha_1 t_2 (1+t_1) + \alpha_2 t_1 (1+t_2))^2} \tag{3}$$

And the equilibrium probabilities of winning the contest p_1^* and p_2^* :

$$p_1^*(e_1^*, e_2^*) = \frac{\alpha_1 t_2 (1+t_1)}{\alpha_1 t_2 (1+t_1) + \alpha_2 t_1 (1+t_2)} \tag{4}$$
$$p_2^*(e_1^*, e_2^*) = \frac{\alpha_2 t_1 (1+t_2)}{\alpha_1 t_2 (1+t_1) + \alpha_2 t_1 (1+t_2)} \tag{5}$$

4.1 Impact of better access to tenders and better ability on probability of winning

We are interested in finding how effort e_i^* and probability of winning the contest p_i^* changes with improved *access to tenders* and an increase in the *contract winning abilities* of a firm. In the context of this framework, this corresponds to an decrease in the cost of accessing tenders t_i, and an increase in the overall ability of the firm given by α_i.

Proposition 1. *Under the assumption that $t_i, t_j \geq 1$ and $2\alpha_i < \alpha_j$, then Firms that (i) have better access to tenders (t_i decreases) and (ii) have higher ability to win tenders (α_i increases), exert a higher level of effort and have a higher probability of winning the contest.*

Proof. From Equations 2 and 4, we see that for firm 1:

$$\frac{\partial e_1^*}{\partial \alpha_1} = \frac{V\alpha_2 \left(1+t_1\right)^2 t_2 \left(1+t_2\right) \left(\alpha_2 \left(1+t_2\right) t_1 - \alpha_1 \left(1+t_1\right) t_2\right)}{\left(\alpha_1 t_2(1+t_1) + \alpha_2 t_1(1+t_2)\right)^3} > 0$$
$$\frac{\partial p_1^*}{\partial \alpha_1} = \frac{V\alpha_2 t_1 t_2 \left(1+t_1\right) \left(1+t_2\right)}{\left(\alpha_1 t_2(1+t_1) + \alpha_2 t_1(1+t_2)\right)^2} > 0$$

The first term is positive under the assumption that $2\alpha_1 < \alpha_2$, and the second term is always positive, which implies that as α_1 increases e_1^* and p_1^* increase. [10] □

Proposition 2. *Suppose $t_i \geq 1$ and $4\alpha_1 < \alpha_2$, then the benefit of increasing the ability of firms to win contests is bigger form firms who have better access to tenders (lower t_i).*

Proof. The first derivative of the two expressions above with respect to t_1 is :

$$\frac{\partial^2 e_1^*}{\partial \alpha \partial t_1} = \frac{2V\alpha_2^2 \left(1+t_1\right) t_2 \left(1+t_2\right)^2 \left(2\alpha_1 \left(1+t_1\right) t_2 - \alpha_2 \left(1+t_2\right) t_1\right)}{\left(\alpha_1 t_2(1+t_1) + \alpha_2 t_1(1+t_2)\right)^4} < 0$$
$$\frac{\partial^2 p_1^*}{\partial \alpha \partial t_1} = \frac{V\alpha_2 \left(1+t_2\right) t_2 \left(\alpha_1 \left(1+t_1\right) t_2 - \alpha_2 \left(1+t_2\right) t_1\right)}{\left(\alpha_1 t_2(1+t_1) + \alpha_2 t_1(1+t_2)\right)^3} < 0$$

Both of these second order derivatives are negative under the assumption that $4\alpha_1 < \alpha_2$. □

Proposition (1) supports the results we saw in the previous section, that firms with higher *contract-winning abilities* provide higher effort by bidding on more contracts and their probability of winning contracts is higher. Proposition (2) suggests that increasing *contract-winning abilities* should benefit the firms who benefit from a better access to tenders. [11] The next section provides evidence that supports empirically Proposition (2).

[10] Note that the condition $2\alpha_1 < \alpha_2$ means that this works with lower ability than the average firm in the economy, which are firms targeted by the type of training we are studying in this paper.

[11] Note that Propositions (1) and (2) are entirely based on comparative static results. In a context where firms are bidding on a number of different tenders facing competition from high ability firms, these results show effects on average payoffs over time.

5 How the Impact of Gaining Access to Large Buyers Varies with Market Access

In this section we analyze heterogeneity in the impact across firms of different types to begin to understand how and why gaining access to large buyers benefits Liberian firms. Guided by the simple framework above, we explore whether firms with better technological means of finding appropriate contracts to bid on benefit more from bid training. We show that use of the Internet enhances the benefits of having the skills necessary to craft good bids.

In the baseline survey, firms were asked how often they use the Internet for business purposes. Respondents could choose between seven answers, ranging from "Every Day" to "Never". The distribution of answers is shown in Figure 4. There is a wide variation in usage, with about 45 percent of firms reporting that they use the Internet for business purposes daily and 30 percent that they never do so.

Appendix Table A.6 shows that, at baseline, firms that use the Internet for business purposes are typically larger, and apply to and win more contracts. Such firms also report to find it easier to access tenders, to have sufficient time to prepare bids, and to better understand the requirements in tender documents.

In the next sub-section we explore how the impact of bid training differs for firms that use the Internet, and in Sub-section 5.2, whether any differential impact of the training for such firms arises because of their access to better information and search technology.

5.1 Heterogeneity in impact on the number and quality of contracts firms bid on and win by Internet use

There is substantial heterogeneity in the effect of the treatment across firms that do and do not use the Internet. To show this, we estimate the following regression:

$$y_i = \beta_0 + \beta_1 \text{BidTraining}_i + \beta_2 \text{BidTraining}_i \times \text{Internet}_i + \gamma X_i + \epsilon_i \quad (6)$$

For easier interpretation, we normalize our measure of firms' Internet use to a unit scale. Thus, $\text{Internet}_i = 1$ if firm i at baseline reports to use the Internet for business purposes every day and $\text{Internet}_i = 0$ if the firm reports to never use the Internet for business purposes. β_2 is the coefficient of interest.[12]

Table 8 show that gaining access to large buyers induced firms that use the Internet

[12] Here X_i includes Internet_i so that β_2 captures the pure interaction effect.

daily to submit 0.7 more bids relative to firms that do not use the Internet for business purposes. "Internet firms" doubled the number of bids submitted in response to the treatment, while firms that do not use the Internet appear not to benefit from the bid training. A similar pattern holds if we replace the outcome variable with a dummy for having submitted any bids in the preceding six months.

We also find that gaining access to large buyers enables firms to *win* more contracts among firms that use the Internet, but not among those who do not. Columns 3 - 8 of Table 8 shows that the total number of contracts won; the number of contracts won through a tender; and the number of contracts won without a tender approximately doubled for firms that were induced to take the bid training by the encouragement. In contrast the estimates suggest that these outcomes were unaffected for treated firms that took the training but do not use the Internet for business purposes. In Appendix Figure A.1 we break down the estimated impact on contracts won by how intensively the firm uses the Internet. The figure shows that the more firms use Internet, the bigger the treatment effect on the number of contracts won through tenders. The extent to which the treatment effect on contracts won without tenders depends on Internet use is less clear, as expected.[13]

In the last column of Table 8 we estimate how the impact of the bid training on the value of contracts won varies with Internet usage. Treated firms that use the Internet for business purposes won contracts worth about USD 10,000 or 200 percent more than control firms, while the impact on contracts won is small and insignificant for firms that do not use the Internet. The average impact on value of contracts won is thus driven entirely by firms with access to a technology that helps them find appropriate contracts to bid on.

We also find that the impact of the bid training on contract *quality* as measured by contract length is considerably greater for firms that use the Internet. Such firms are 26 percentage points or around 100 percent more likely to win a contract lasting more than six months after learning how to bid on contracts from large buyers, as seen in Column 2 of Table 9. However, we do not find evidence that the increase in the probability of winning a contract from an international buyer (shown in Table 7) is greater for "Internet firms".

In sum the evidence in this sub-section makes clear that firms that use the Internet for business purposes benefit substantially more from learning how to bid on contracts from large buyers, consistent with the framework in Section 4.

[13]Internet is not the preferred method of advertising for contracts that do not require a formal bidding process. In qualitative interviews, firms reported that they were informed of contracts outside of the tender process not from the Internet but from either past clients, or other contacts. Additionally, all the contracts that are being advertised online that we accessed are formal tenders.

5.2 Understanding heterogeneity in impact on the number and quality of contracts firms bid on and win by Internet use

The results in Sub-section 5.1 leave open the question of whether firms with access to the Internet benefit more from gaining access to large buyers *because* they have access to the Internet. Alternatively, it may be that such firms (also) differ from other treated firms in other ways that ultimately drive the heterogeneity in the impact of the treatment we have established.

To investigate, we start by including—in addition to $\text{BidTraining}_i \times \text{Internet}_i$—interactions between the treatment indicator and a wide range of baseline firm characteristics that may correlate with use of the Internet. These firm characteristics—employment, counties of operation, gender of the owner, sectors the firm operates in, languages used for business, the geographical zone the firm is located in, and the number of submitted bids at baseline—collectively proxy for unobserved firm heterogeneity. The results in Table 10 suggest that Internet use in itself directly enhances the impact of the bid training on firm performance. As in Table 8 , we find that the increase in bids submitted, total number of contracts won, contracts won through a tender, and the total value of contracts won is significantly greater for firms that use the Internet. These estimates are in fact bigger in magnitude with the inclusion of interactions between the treatment indicator and additional firm characteristics, pointing towards access to information and search technology itself raising the gain from learning how to bid on contracts from large buyers.

We next show that Internet access is in fact the best predictor of firms' conditional average treatment effect (CATE) among the wide set of firm characteristics captured in the data we use.[14] The additional firm characteristics we interact with the treatment in Table 10 cover a wide range, but nevertheless represent a selection of such characteristics subjectively chosen by the authors to attempt to proxy for unobserved firm heterogeneity. We now take a different approach by simply estimating which firm characteristics best predict which firms benefit from access to large buyers.

Following Chernozhukov *et al.* (2018), we estimate the best linear predictor of the conditional average treatment effect on the number of bids submitted. The best linear predictor is based on a LASSO procedure that characterizes treatment effect heterogeneity by firm characteristics.

The algorithm we use is as follows:

1. We first split the full sample into two parts, the *auxiliary* sample and the *main* sample.

[14]The conditional average treatment effect (CATE) is the difference in expected outcome between treatmeant and control groups conditional on covariates.

The auxiliary sample is used as the training set, and the main sample as the hold-out set.

2. We then use a LASSO regression of number of bids on baseline observables estimated on the control group part of the *auxiliary* sample to predict the number of bids for the full auxiliary sample (control and treatment). A second LASSO regression of number of bids on (i) the predicted output of the first LASSO regression and (ii) the interaction of treatment and baseline observables selects variables which best predict the heterogeneity of the treatment effect observed.

3. We then test the predictive power of the heterogeneity variables selected in the *auxiliary* sample in step 2 on the *main* sample. Predicted number of bids is generated on the *main* sample using the variables selected in the step 2 with their associated coefficients from the *auxiliary* sample. The observed number of bids in the *main* sample is regressed on the predicted number of bids based on the *auxiliary* sample. This allows us to test whether variables selected in step 2 accurately describe the observed heterogeneity in treatment effects.[15]

4. Finally, we run a cross-validation preocedure wherein the main sample is used as the training set and the auxiliary sample as the hold-out set.

The result of this procedure depends on the random split of the sample. We thus "bootstrap" the procedure by repeating it 100 times. Given that we run the procedure 100 times, and that each procedure runs two estimations, the total number of LASSO estimations is 200. Out of these 200 estimations, 196 were validated by the test for the hold-out set heterogeneity variables as good predictors of heterogeneity. Table 11 shows how many times each variable was selected in the set of variables that best explain heterogeneity in treatment effects in the training set. Internet access is by far the variable selected the most times, 194.

In sum, the evidence in Tables 10 and 11 thus strongly suggests that Internet access itself enhances the impact of bid training on firm behavior and performance, as the framework in Section 4 predicts.

[15] A variable is said to accurately describe the observed heterogeneity if the p-value of its coefficient on the *main* sample is smaller than 0.01.

5.3 Firm awareness of the combined role of access to large buyers and Internet use

The evidence in sub-sections sub-sections 5.1 and 5.2 makes clear that knowledge of how to access demand from large buyers can significantly benefit Liberian firms, in particular those with access to the Internet. A question that has important implications for how we model and design policy that accounts for the complementarity between contract-winning skills and use of information technology is whether firms themselves are aware of and act on this complementarity. Two findings suggest that this may not be the case.

First, firms with Internet access were not more likely to choose to participate in the bid training. Appendix Table A.7 shows that while the encouragement increased take-up of the training, it did so to a similar extent for firms that do and do not use the Internet for business purposes. This suggests that firms did not anticipate that access to complementary technology was essential to benefit from the training.[16]

Second, while we saw in Table 5 that learning how to bid on contracts from large buyers improves firms' expectations about the future on average, Appendix Table A.8 shows that the impact on expectations is similar for firms that do and do not use the Internet. This suggests that, while firms need access to the Internet to benefit from increased contract-winning skills, managers themselves do not recognize this, even six months after the bid training.

In the next section we investigate *why* firms that make use of information technology benefit more from access to large buyers.

6 Why the Impact of Gaining Access to Large Buyers Varies with Market Access

When viewed through the lens of the framework in Section 4, the results in Section 5 suggest that Internet access enhances the benefits of learning how to bid on contracts from large buyers because firms can more easily find appropriate contracts to bid on once online. If this interpretation is correct we would expect the additional benefit of bid training for Internet firms to be concentrated in periods when a greater share of demand is "discoverable" online.

To do so we use detailed tender data collected by the non-profit we work with to construct firm-specific online demand shocks. The dataset includes the date each tender

[16]Note also that the possibility of using the Internet to access tenders was not mentioned in the encouragement.

was published, the medium it was published in (newspaper, online or other), and the sector(s) to which the relevant tender belongs. We investigate how the variation in the number of tenders being published online affects whether Internet firms benefit from the training.

Figure 5 shows the sector-aggregated variation in the number of tenders published during 2016. While about a third of tenders are published online overall, the figure shows that this varies a lot throughout the time period. We use this variation to construct a firm-level online demand shock, defined as the number of tenders published online in the time-period between a firm's training and the endline survey. The graph also marks the time frame when firms in our sample encouraged as well as the time period when Building Markets conducted the training sessions which the firms in our sample were encouraged to attend.

With this measure in hand, we estimate the following regression:

$$\begin{aligned} \mathrm{y}_i = \beta_0 + \beta_1 \mathrm{BidTraining}_i + \mathrm{OnlineDemand}_i + \\ + \beta_2 \mathrm{BidTraining}_i \times \mathrm{Internet}_i + \beta_3 \mathrm{BidTraining}_i \times \mathrm{OnlineDemand}_i \\ + \beta_4 \mathrm{BidTraining}_i \times \mathrm{Internet}_i \times \mathrm{OnlineDemand}_i + \gamma \mathrm{X}_i + \epsilon_i \end{aligned} \tag{7}$$

Where $OnlineDemand_i$ is the number of tenders published online in the period between training and November 31st, 2016. Since firms take the training at different times, the variable is a firm-specific demand shock. We expect online demand shock to have a strong sector-specific component (with some sectors being more prone to have tenders published online) and that this might be endogenously correlated with whether firms use Internet for business purposes. To account for this, the results presented in this section all include sector fixed effects. This means that the effect captured in this analysis is entirely derived from within-sector variation in time of training.[17]

To account for the fact that a high number of online tenders might reflect an overall high number of tenders (online and not online), we also control for the overall number of tenders published between the date of training and the endline survey.[18] Another way to cope with this would be to measure the ratio of online tenders on overall number of tenders (instead of the number of online tenders). Both yield similar results.[19]

Another concern with this regression is that firms endougenously decide at which date

[17] Results without sector fixed effects are available upon request and are coherent with our current analysis–sectors with a high number of online tenders during the period of the experiment are more likely to benefit from the training.

[18] Since this measure is also at the firm-level, it is not captured in sector fixed effects.

[19] Results available upon request.

to take the training. This would happen if for example firms anticipate that a high number of tenders are about to be published online, and decide to take the training at that time. To cope with this concern, we first show that our measure of online demand shock is uncorrelated with firm characteristics. Table 12 shows that, as expected, this measure of a firm's online demand shock is uncorrelated with the firm's baseline outcomes—that is, the measures of behavior and performance we focus on in sections 3 and 5, suggesting that these shocks are independent of unobserved firm characteristics. Since firm unobservables could still be correlated with date of training and firm performance and thus violate the exclusion restriction, we build an instrument for the online demand shock. While the date of training might be endogenous, we argue that the date of encouragement–chosen by the survey firm based on logistical constraints– is arguably exogenous. We thus predict a hypothetical training date based on the encouragement date, and use the–exogenous–number of tenders published online after this hypothetical training date as an instrument for the–endogenous–number of tenders published online after the actual training date. [20]

Note that in equation 7 above, we do not add the variable $OnlineDemand_i$ or its interaction with $Internet_i$. We do this for two reasons. First, since our instrumented measure of online demand shock is exogenous, adding these would keep the results unchanged. Second, our measure of online demand shock is the number of tenders published online *after training* which, by definition, can only be built for firms who took the training. This means that these variables would almost perfectly match the interaction of these variable with $BidTraining_i$.[21]

Results from this analysis are presented in Tables 13 and A.10. We find that a greater firm-specific online demand shock increased the extent to which firms that use the Internet benefited from the bid training relative to other firms. Internet firms who benefitted from an online demand shock are more likely to bid on tenders, and while their total number of contracts won is not significantly higher (column (2)), there is a shift in the type of contracts they win. Indeed, as a results of training, Internet firms win a higher number of contracts that do not require bids, regardless of their online demand shock. However, this effect is slightly lower if firms have an online demand shock, as seen in the negative coefficient of the last row of column (4). This is compensated by an increase in the number of contracts won through tenders for these firms, as seen in column (3).

This means that for firms to win a high number of tenders–which was the first-order goal of the training–firms need both access to the Internet and an online demand shock. Contracts that are won through tenders are of particular interest to these firms, compared

[20] The results are also robust if we instrument the demand shocks post training with the demand shocks post the encouragement date. The results are available on request.

[21]

to contracts that do not require bidding. These contracts are usually of higher value (as seen in the non-signficant but positive coefficient in column (5)), and allow the firm to start working with big suppliers they might otherwise never work with. Consistent with the results outlined in Section 5.3, we find in Table A.9 that firms experiencing larger demand shocks are not more likely to take-up the training implying that firms were unaware of the potential benefits of the training.[22]Moreover, the impact on expectations is similar for Internet using firms who experience large or small demand shocks. [23]

These results support the way we interpret the results in Section 5 from the vantage point of the framework in Section 4, namely that the bid training benefited firms with access to the Internet more because of a complementarity between firms' ability to find appropriate contracts to bid on and their ability to win such contracts conditional on bidding.

6.1 Kick-start effect of demand shocks

Internet firms who benefited from demand shocks at the moment of training are more likely to apply to and win contracts through tenders. In this subsection, we explore whether these demand shocks affect firms even after the demand shock expires.

We use dates reported by firms for applying to tenders and winning contracts to identify tenders and contracts in the period after the demand shock. Figure 6 shows the timeline of the demand shock and endline survey. As detailed in the previous section, yhe demand shock spans from the date of training to the end of November 2016. During the endline interview, firms are asked for the dates at which they applied to tenders and won contracts since the encouragement. In this subsection we restrict observations of tenders applied to and contracts won after February 1st, 2017. This ensures that the effect we are measuring is not purely mechanical. [24]

Table 13 presents the results from this restricted period. While magnitudes seem smaller, results remain mostly unchanged. Results show that Internet firms are more likely to benefit from an online demand shock, even after the demand shock has expired. This means that the demand shock at the moment of training has a "kick-start" effect on these

[22]For these results, the demand shocks for a firm are computed as the number of tenders specific to the sectors that the firm operates in which are published online between the date of encouragement and the date of training.

[23]Results obtained by considering not only *online* demand shocks, but *total* demand shocks are presented in the Appendix in Tables A.11, A.12 and A.14 are similar to the results using only online demand shocks.

[24]For a subset of tenders, we are able to observe the application opening and closing dates. 90% of our sample has an application period of less that 30 days, with a mean of 15 days. This ensures that the tenders in the demand shock -which ends on November 31st 2016- and that firms apply to in the period we are considering -February 1st 2017- do not overlap.

firms. Our interpretation of these results is that human capital has to be combined with a demand shock shortly after training.

7 Conclusion

Governments and market-making organizations often develop policies aiming at benefitting small and medium enterprises. However the factors that prevent these firms from growing are not well known. While other authors have shown that demand shocks are an important factor in the short term, information frictions that prevent firms from accessing bigger markets could play a more signficant role in the long term. This paper tests the effect of a training that teaches the tools needed to prepare a competitive bid for contracts with large buyers. We show that the training boosts firm growth, confirming that these firms where access constrained to begin with.

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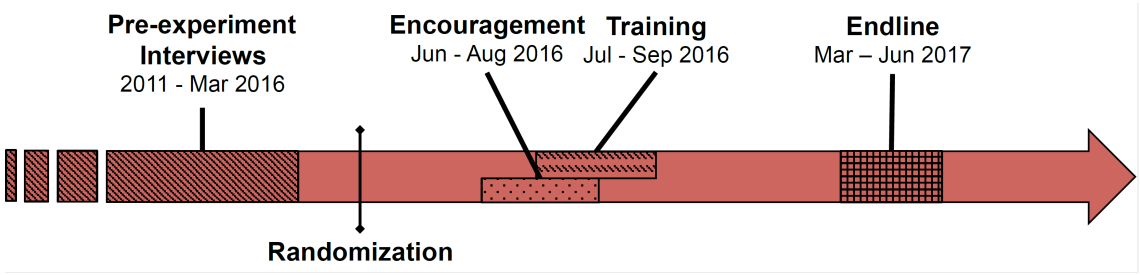
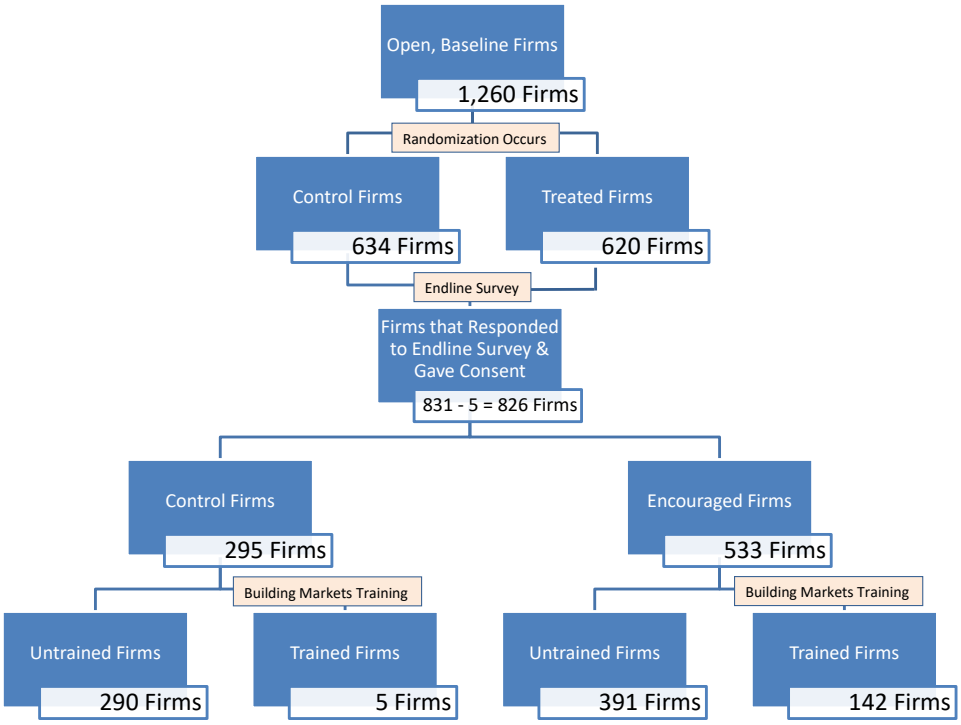
FIGURE 1: TIMELINE



This Figure shows the timeline of the experiment evaluated in this paper. The experiment spanned from June 2016 to June 2017, with some pre-baseline interviews conducted before April 2016 by an outside source.

FIGURE 2: RANDOMIZATION DESIGN



This figure shows the number of firms in the sample at every step of the experiment. Open baseline firms are the firms who were in Building Markets' directory, who never took the training, who have at least one employee and who are located in Monrovia. These firms were randomly selected for treatment or control. For the endline survey, the research team tried to reach these firms and was able to track down and interview only a subsample. Out of the 533 firms who interviewed at endline that were in the treatment group (the encouraged firms), 142 firms had taken the training.

FIGURE 3: SIZE OF FIRMS IN THE SAMPLE

Figure 3.A Comparison of Sample and Census Firms

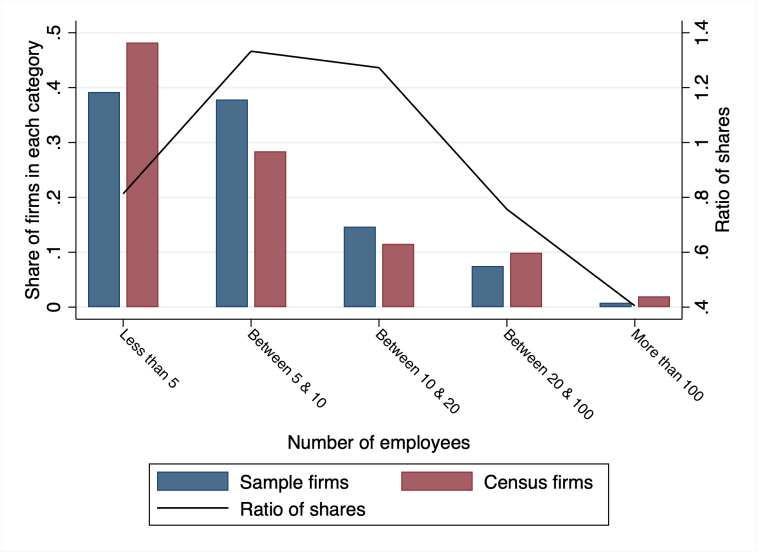
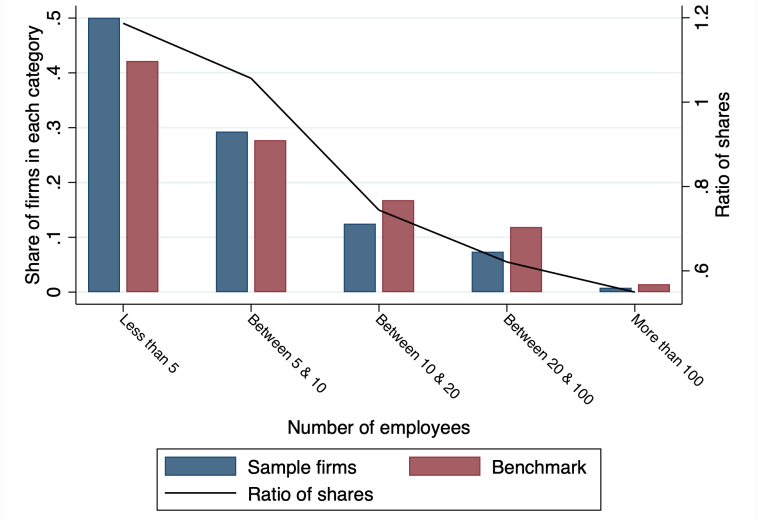
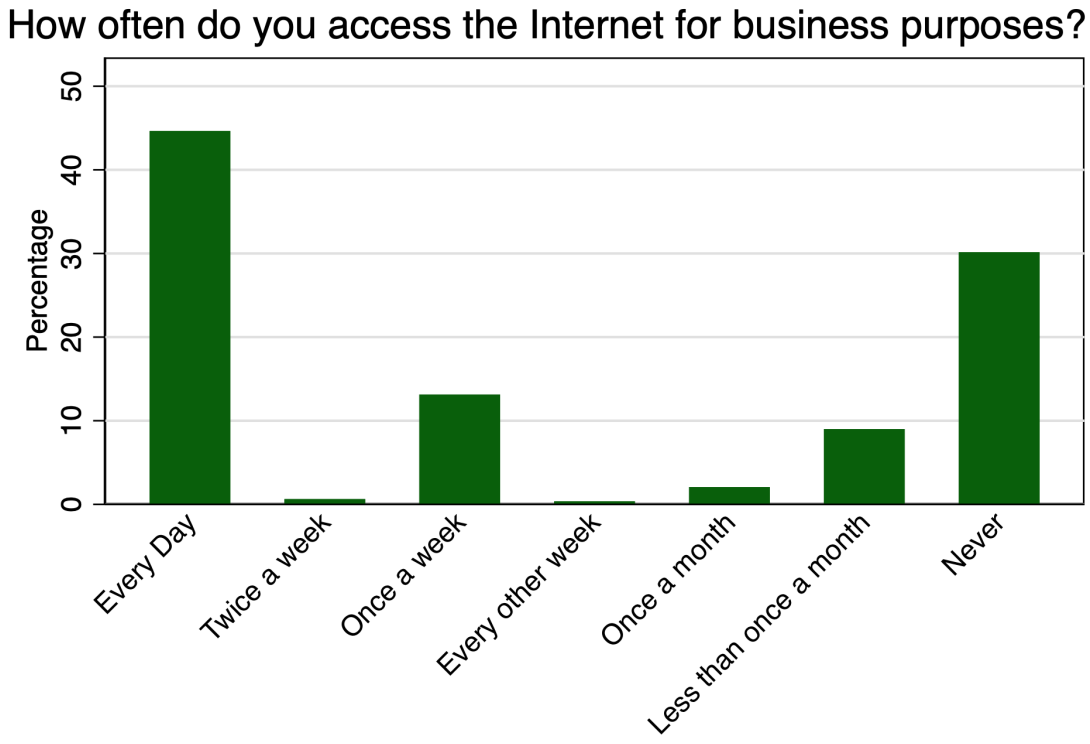


Figure 3.B Comparison of Sample and Benchmark Firms



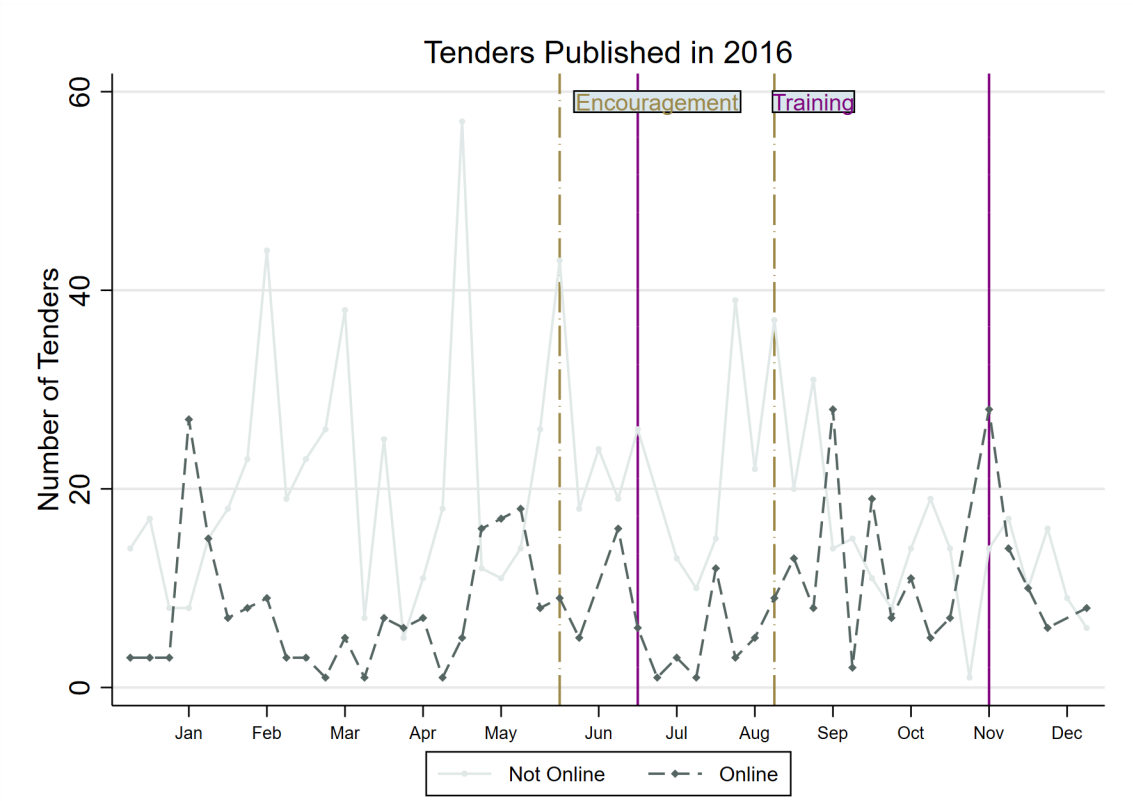
The two panels in this figure compare the size of firms in the sample with firms in the census (Panel A) and other Building Markets firms who have more than one employee and are located in Monrovia (Panel B). In both panels, the blue bars show the share of firms in our sample in each category, and the red bars show the share of the comparison sample. The solid line shows the ratio of shares in each category.

FIGURE 4: INTERNET ACCESS AT BASELINE



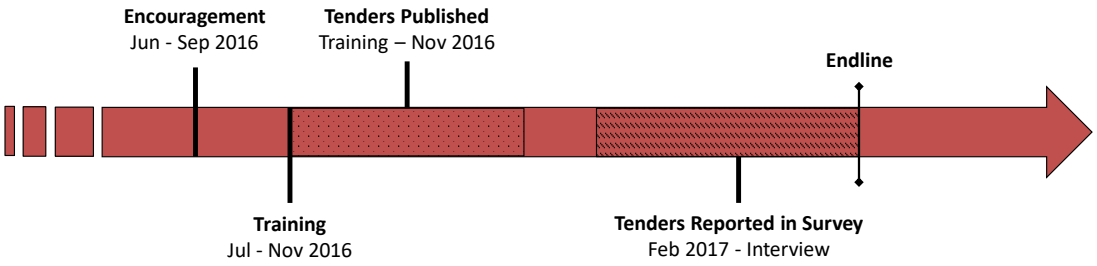
This graph plots the distribution of internet usage in the sample measured at baseline.

FIGURE 5: TIME SERIES OF TENDERS



This figure plots the time series tenders published in 2016 from the Building Markets database aggregated at a weekly level. The trend for tenders published online and offline are plotted separately. The brown lines represent the period of time when firms were encouraged to take the training while the purple lines mark the time period when Building Markets offered the General Procurement Training.

FIGURE 6: TIMELINE



This Figure shows the timeline for a particular firm as a motivating example.

TABLE 1: SUMMARY STATISTICS ON TENDERS

	Mean
Buyer	
Tender from Government	0.51
Tender from NGO	0.47
Source	
Newspaper	0.57
Website	0.31
Word of mouth	0.12
Sectors	
Construction and Renovation	0.23
Automotive	0.11
Business and Consulting	0.10
Printing and Copying	0.09

This table presents summary statistics of 1,381 tenders published in Liberia in 2016. The data is based on a database of tenders compiled by Building Markets, a Liberian local NGO.

TABLE 2: SUMMARY STATISTICS ON SAMPLE FIRMS

	Mean	SD	Observations
Sectors			
Construction and Renovation	0.23	(0.42)	1192
Food and Beverages	0.16	(0.36)	1192
Home Essentials	0.13	(0.33)	1192
Handicrafts and Artisans	0.11	(0.32)	1192
Business and Consulting Services	0.09	(0.29)	1192
Owner Nationality			
Liberian	0.90	(0.30)	1192
Lebanese	0.05	(0.21)	1192
Nigerian	0.02	(0.14)	1192
Indian	0.01	(0.11)	1192
Other			
Total Number of Employees	14.19	(42.62)	1187
Submitted a bid in the past 6 months (Y=1; N=0)	0.21	(0.40)	847
Number of bids submitted in the past 6 months	0.65	(1.62)	847
Won a contract through a bid in the past 6 months (Y=1; N=0)	0.12	(0.32)	876
Number of contracts won through bids in the past 6 months	0.30	(1.16)	876
Ever won a contract of six months or more	0.76	(0.43)	179
Proportion of bids won (conditional on applying)	0.32	(0.37)	174
Speaks at least one Liberian local language	0.34	(0.47)	1192

This table presents summary statistics of firms in the sample. The data is based on phone interviews conducted by Building Markets, a Liberian local NGO. A nationality of the business is determined if at least one of the owners has that particular nationality. The number of employees includes the owner or manager of the firm.

TABLE 3: BALANCE TABLE

	Full Sample		Restricted Sample	
	Diff. (T - C)	Std. Error	Diff. (T - C)	Std. Error
Sectors				
Construction and Renovation	-0.002	0.026	-0.016	0.032
Food and Beverages	-0.017	0.022	0.019	0.027
Home Essentials	0.003	0.020	0.022	0.025
Handicrafts and Artisans	0.003	0.019	0.001	0.024
Business and Consulting Services	0.005	0.018	0.023	0.022
Owner Nationality				
Liberian	-0.008	0.018	-0.013	0.020
Lebanese	-0.004	0.013	0.002	0.014
Nigerian	-0.002	0.009	-0.001	0.008
Indian	-0.003	0.007	-0.002	0.009
Other				
Total Number of Employees	-1.791	2.586	-0.663	2.726
Accessed Internet for business purposes (1=Every day ; 7= Never)	0.042	0.167	0.034	0.204
Submitted a bid in the past 6 months (Y=1; N=0)	-0.016	0.024	-0.035	0.031
Number of bids submitted in the past 6 months	-0.079	0.118	-0.131	0.147
Won a contract through a bid in the past 6 months (Y=1; N=0)	-0.007	0.023	-0.011	0.029
Number of contracts won through bids in the past 6 months	0.090	0.082	0.137	0.113
Proportion of tenders won (conditional on applying)	-0.023	0.059	-0.044	0.071
Ever won a contract of six months or more	-0.040	0.068	0.013	0.089
Speaks at least one Liberian local language	-0.006	0.028	-0.056	0.035

This table presents balance between firms of the treatment and control groups. "Full Sample" refers to the total sample at baseline, "Restricted Sample" refers to firms who responded to the endline survey. The data is based on phone interviews conducted by Building Markets, a Liberian local NGO. In cases where there are several owners, nationality is determined if at least one of the owners has that particular nationality. The number of employees includes the owner of the firm, temporary and permanent employees.

TABLE 4: EFFECT OF VOUCHER + ENCOURAGEMENT ON TRAINING TAKE-UP

	Took training	
	(1)	(2)
Voucher + Encouragement	0.187***	0.200***
	(0.0151)	(0.0169)
Controls		
Control Mean	0.010	0.010
Observations	1192	1143

Standard errors are in parentheses and are robust. This table presents coefficients of the regression of General Procurement training take-up as recorded by Building Markets on encouragement. Controls include employment, counties of operation, gender of the owner, sectors, languages used for business, geographical zone and the number of submitted bids. All controls are measured at baseline. The GP training teaches basic knowledge to find and apply to tenders. Column (1) includes controls.

TABLE 5: EFFECT OF VOUCHER + ENCOURAGEMENT ON EXPECTATIONS ABOUT THE FUTURE OF THE FIRM

	How many tenders do you expect your firm to bid on in the next 6 months?		Of these, how many do you expect you will win?	
	(1)	(2)	(3)	(4)
	TREATMENT-ON-THE-TREATED			
Took Training	0.850*	1.003**	0.860**	0.831**
	(0.463)	(0.438)	(0.395)	(0.380)
	INTENT-TO-TREAT			
Voucher + Encouragement	0.221*	0.276**	0.223**	0.228**
	(0.119)	(0.124)	(0.101)	(0.107)
Controls	NO	YES	NO	YES
Control Mean	2.037	2.037	1.510	1.510
Observations	788	753	788	753

Standard errors are in parentheses and are robust. The top panel presents the Treatment-on-the-Treated while the bottom panel presents the Intent to Treat estimates of the effect of training for firms who attended the General Procurement training. Controls include employment, counties of operation, gender of the owner, sectors, languages used for business, geographical zone and the number of submitted bids. All controls are measured at baseline. All outputs are referring to the period of 6 months preceding the interview. Columns (1) & (2) refer to the refer to the expectations of the manager about the future bids of the firms. Columns (3) & (4) refer to the expectations of the manager about the firm's future contracts won through tenders. Columns (2) & (4)of both panels include controls.

TABLE 6: EFFECT OF VOUCHER + ENCOURAGEMENT ON TENDERS APPLIED TO AND CONTRACTS WON

	# of bids submitted		Total # of contracts		# of contracts won through a tender		# of contracts won w/o tender		Total value of contracts in USD	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	TREATMENT-ON-THE-TREATED									
Took Training	0.526*	0.593**	1.192***	1.009***	0.235	0.282*	0.954***	0.723**	11058.0	10362.3
	(0.304)	(0.263)	(0.378)	(0.351)	(0.172)	(0.159)	(0.311)	(0.294)	(7989.8)	(6473.5)
	INTENT-TO-TREAT									
Voucher + Encouragement	0.137*	0.163**	0.307***	0.276***	0.0609	0.0774*	0.246***	0.198**	2866.0	2843.4
	(0.0785)	(0.0745)	(0.0945)	(0.0980)	(0.0444)	(0.0451)	(0.0782)	(0.0829)	(2067.4)	(1852.8)
Controls	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
Control Mean	0.352	0.352	0.482	0.482	0.155	0.155	0.327	0.327	5322.458	5322.458
Observations	787	752	788	753	789	754	788	753	789	754

Standard errors are in parentheses and are robust. The top panel presents the Treatment-on-the-Treated while the bottom panel presents the Intent to Treat estimates of the effect of training for firms who attended the General Procurement training. Controls include employment, counties of operation, gender of the owner, sectors, languages used for business, geographical zone and the number of submitted bids. All controls are measured at baseline. All outputs are referring to the period of 6 months preceding the interview. Columns (1) & (2) refer to the number of bids submitted for tenders. Columns (3) to (8) refer to the number of contracts won overall, through a tender process and without a tender process. Columns (9) & (10) refer to the total value of contracts won by firms.

TABLE 7: EFFECT OF VOUCHER + ENCOURAGEMENT ON THE QUALITY OF CONTRACTS

	Ever had a contract of more than 6 months	
	(1)	(2)
	TREATMENT-ON-THE-TREATED	
Took Training	0.326**	0.299***
	(0.128)	(0.113)
	INTENT-TO-TREAT	
Voucher + Encouragement	0.0844***	0.0822***
	(0.0325)	(0.0317)
Controls	NO	YES
Control Mean	0.232	0.232
Observations	789	754

Standard errors are in parentheses and are robust. The top panel presents the Treatment-on-the-Treated while the bottom panel presents the Intent to Treat estimates of the effect of training for firms who attended the General Procurement training. Controls include employment, counties of operation, gender of the owner, sectors, languages used for business, geographical zone and the number of submitted bids. All controls are measured at baseline. All outputs are referring to the period of 6 months preceding the interview.The outcome variable is an indicator for whether or not the firm ever had a contract longer than 6 months.

TABLE 8: HOW THE TREATMENT EFFECT ON TENDERS AND CONTRACTS VARIES WITH INTERNET ACCESS

	# of bids submitted		Total # of contracts		# of contracts won through a tender		# of contracts won w/o tender		Total value of contracts in USD	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Voucher + Encouragement	-0.0557	-0.132*	0.0535	-0.0608	-0.0431	-0.0570	0.0967	-0.00382	-812.8	-1431.6
	(0.0665)	(0.0680)	(0.136)	(0.139)	(0.0418)	(0.0455)	(0.115)	(0.116)	(742.2)	(1121.2)
Voucher + Encouragement × Internet	0.486***	0.699***	0.484**	0.647***	0.238**	0.314***	0.244	0.330*	9917.2**	10523.5**
	(0.183)	(0.188)	(0.227)	(0.239)	(0.108)	(0.120)	(0.188)	(0.196)	(4454.0)	(4281.6)
Controls	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
Control Mean	0.352	0.352	0.482	0.482	0.155	0.155	0.327	0.327	5322.458	5322.458
Observations	739	710	740	711	741	712	740	711	741	712

Standard errors are in parentheses and are robust. Controls include employment, counties of operation, gender of the owner, sectors, languages used for business, geographical zone and the number of submitted bids. All controls are measured at baseline. Columns (1) & (2) refer to the number of bids submitted for tenders. Columns (3) to (8) refer to the number of contracts won overall, through a tender process and without a tender process. Columns (9) & (10) refer to the total value of contracts won by firms.

TABLE 9: HOW THE TREATMENT EFFECT ON CONTRACT QUALITY VARIES WITH INTERNET ACCESS

	Ever had a contract of more than 6 months	
	(1)	(2)
Voucher + Encouragement	-0.00830	-0.0349
	(0.0423)	(0.0434)
Voucher + Encouragement × Internet	0.215***	0.264***
	(0.0806)	(0.0798)
Controls	NO	YES
Control Mean	0.232	0.232
Observations	741	712

Standard errors are in parentheses and are robust. Controls include employment, counties of operation, gender of the owner, sectors, languages used for business, geographical zone and the number of submitted bids. All controls are measured at baseline. All outputs are referring to the period of 6 months preceding the interview.The outcome variable is an indicator for whether or not the firm ever had a contract longer than 6 months.

TABLE 10: HOW THE TREATMENT EFFECT ON TENDERS AND CONTRACTS VARIES WITH INTERNET INCLUDING ADDITIONAL INTERACTIONS WITH CONTROLS

	# of bids submitted	Total # of contracts	# of contracts won through a tender	# of contracts won w/o tender	Total value of contracts in USD
	(1)	(2)	(3)	(4)	(5)
Voucher + Encouragement	0.190	-0.178	0.0204	-0.196	-1711.2
	(0.215)	(0.313)	(0.124)	(0.274)	(4401.3)
Voucher + Encouragement × Internet	0.408*	0.517*	0.288***	0.229	9649.8***
	(0.209)	(0.263)	(0.109)	(0.224)	(3552.4)
Controls	YES	YES	YES	YES	YES
Controls Interacted w/ Treatement	YES	YES	YES	YES	YES
Control Mean	0.352	0.482	0.155	0.327	5322.458
Observations	739	740	741	740	741

Standard errors are in parentheses and are robust. Controls include employment, counties of operation, gender of the owner, sectors, languages used for business, geographical zone and the number of submitted bids. All controls are measured at baseline. The interaction of all controls with treatment are also included. Columns (1) & (2) refer to the number of bids submitted for tenders. Columns (3) to (8) refer to the number of contracts won overall, through a tender process and without a tender process. Columns (9) & (10) refer to the total value of contracts won by firms.

TABLE 11: RESULTS OF LASSO PROCEDURE

Variable	Times_Kept	Average_Coefficient
Of the total number of employees, how many are permanent employees?	1	0.00260
Of the total number of employee, how many are temporary employees?	1	0.00590
Of the total number of employees how many are family members of the owner(s)?	0	.
Have you ever responded to a tender or submitted a bid?	99	0.0310
At least one of the owners has an Americo-Liberian name	0	.
Have you responded to a tender or submitted a bid in the past 6 months?	67	0.0438
Number of tenders submitted in the past six months	55	0.0107
Number of contracts won through tenders in the past six months	78	0.0553
Do you import?	0	.
Have you ever had a contracts for the government?	0	.
Have you ever had a contracts for an NGO?	10	0.0487
Have you ever had a contracts for an international client?	46	0.0378
How often do you use the Internet for business purposes?	194	0.122

This table presents the results of the LASSO procedure developped in section 5.3.2. The LASSO estimation predicts what variables, interacted with treatment, best explain the heterogeneity if the effect observed. The first column shows the number of times each variable was kept out of the 200 LASSO estimations. The second column shows the average coefficient of each variables, accross LASSO procedures.

TABLE 12: BASELINE CORRELATION BETWEEN FIRM CHARACTERISTICS AND INTERNET-BASED DEMAND SHOCK

	Total # of employees	# of tenders applied to	# of contracts won through a tender	# of contracts won w/o tender	Ability to access tenders easily (1-10)
	(1)	(2)	(3)	(4)	(5)
Online Demand Shock	-0.0518	0.00915	0.00203	0.00328	0.0149
	(0.103)	(0.00688)	(0.00287)	(0.00726)	(0.0172)
Constant	11.96***	0.642***	0.195***	0.517***	6.898***
	(1.700)	(0.0618)	(0.0273)	(0.0484)	(0.183)
Observations	582	582	580	582	285

Standard errors are in parentheses and are robust. This table shows the correlation between the share of tenders published online in the week before the encouragement and baseline characteristics.

TABLE 13: HOW THE TREATMENT EFFECT VARIES WITH INTERNET ACCESS AND ONLINE DEMAND SHOCKS

	# of bids submitted	Total # of contracts	# of contracts won through a tender	# of contracts won w/o tender	Total value of contracts in USD
			TREATMENT TO ENDLINE		
Voucher + Encouragement	-0.214	-0.280	0.0101	-0.291*	19883.5
	(0.158)	(0.182)	(0.0777)	(0.157)	(16494.2)
Voucher + Encouragement	-0.0154	-0.00208	-0.0387	0.0387	130417.5
× Online Demand	(0.107)	(0.0615)	(0.0437)	(0.0584)	(148118.5)
Voucher + Encouragement	-0.132	0.546*	-0.123	0.662***	-90894.8
× Internet	(0.276)	(0.281)	(0.152)	(0.256)	(76452.5)
Voucher + Encouragement					
× Online Demand x Internet	0.184**	-0.0254	0.102***	-0.126**	12503.7
	(0.0810)	(0.0550)	(0.0396)	(0.0521)	(222739.4)
Control Mean	0.352	0.482	0.155	0.327	5322.458
			FEBRUARY TO ENDLINE		
Voucher + Encouragement	-0.0483	-0.0991	0.0227	-0.122	22084.9
	(0.122)	(0.119)	(0.0441)	(0.106)	(18680.8)
Voucher + Encouragement	-0.0104	-0.0174	-0.00939	-0.00801	135847.1
× Online Demand	(0.0880)	(0.0440)	(0.0272)	(0.0375)	(159539.5)
Voucher + Encouragement	-0.0573	0.396*	-0.0528	0.449**	-92032.3
× Internet	(0.267)	(0.205)	(0.106)	(0.179)	(78040.9)
Voucher + Encouragement					
× Online Demand x Internet	0.134*	0.0103	0.0435*	-0.0332	6770.1
	(0.0766)	(0.0416)	(0.0258)	(0.0359)	(225005.4)
Control Mean	0.169	0.239	0.070	0.169	4270.785
Observations	712	712	712	712	712

This table presents the heterogeneity of treatment effect with respect to Internet access at baseline and the online demand shock. Standard errors are in parentheses and are robust. Controls include employment, counties of operation, gender of the owner, sectors, languages used for business, geographical zone, the number of submitted bids, total number of tenders published in the sector from the predicted date of training, share of different types of tenders in the sector (tenders from local or international organizations, local or international governments, local or international private firms, local or international NGOs. All controls are measured at baseline. Columns (1) & (2) refer to the number of bids submitted for tenders. Columns (3) to (8) refer to the number of contracts won overall, through a tender process and without a tender process. Columns (9) & (10) refer to the total value of contracts won by firms.

A Appendix

TABLE A.1: DIFFERENTIAL ATTRITION

	Not Interviewed	Interviewed	Diff.	P-Val.
Sectors				
Construction and Renovation	0.20	0.25	-0.0537**	0.0431
Food and Beverages	0.16	0.16	0.0008	0.9722
Home Essentials	0.12	0.13	-0.0083	0.6901
Handicrafts and Artisans	0.11	0.12	-0.0112	0.5765
Business and Consulting Services	0.08	0.10	-0.0221	0.2248
Owner Nationality				
Liberian	0.87	0.92	-0.0418**	0.0248
Lebanese	0.05	0.04	0.0139	0.2889
Nigerian	0.04	0.01	0.0224**	0.0111
Indian	0.01	0.01	-0.0090	0.1844
Other				
Total Number of Employees	12.08	15.11	-3.0330	0.2592
Submitted a bid in the past 6 months (Y=1; N=0)	0.17	0.22	-0.0467	0.1212
Number of bids submitted in the past 6 months	0.55	0.70	-0.1429	0.2352
Won a contract through a bid in the past 6 months (Y=1; N=0)	0.09	0.13	-0.0431*	0.0706
Number of contracts won through bids in the past 6 months	0.19	0.35	-0.1555*	0.0679
Ever won a contract of six months or more	0.82	0.73	0.0856	0.2080
Proportion of bids won (conditional on applying)	0.29	0.33	-0.0363	0.5717
Speaks at least one Liberian local language	0.30	0.36	-0.0538*	0.0718
Treatment Group	0.67	0.64	0.0287	0.3399

This table presents differential attrition between firms who responded to endline interviews and firms who did not. The data is based on phone interviews conducted by Building Markets, a Liberian local NGO. A nationality of the business is determined if at least one of the owners has that particular nationality. The number of employees includes the owner or manager of the firm.

TABLE A.2: SUMMARY STATISTICS OF RESTRICTED SAMPLE

	Mean	SD	Observations
Sectors			
Construction and Renovation	0.25	(0.43)	828
Food and Beverages	0.16	(0.36)	828
Home Essentials	0.13	(0.34)	828
Handicrafts and Artisans	0.12	(0.32)	828
Business and Consulting Services	0.10	(0.30)	828
Owner Nationality			
Liberian	0.92	(0.28)	828
Lebanese	0.04	(0.20)	828
Nigerian	0.01	(0.11)	828
Indian	0.01	(0.12)	828
Other			
Total Number of Employees	15.11	(45.69)	825
Submitted a bid in the past 6 months (Y=1; N=0)	0.22	(0.41)	587
Number of bids submitted in the past 6 months	0.70	(1.65)	587
Won a contract through a bid in the past 6 months (Y=1; N=0)	0.13	(0.34)	609
Number of contracts won through bids in the past 6 months	0.35	(1.31)	609
Ever won a contract of six months or more	0.73	(0.45)	119
Proportion of bids won (conditional on applying)	0.33	(0.37)	129
Speaks at least one Liberian local language	0.36	(0.48)	828

This table presents summary statistics of firms who responded to the endline survey. The data is based on phone interviews conducted by Building Markets, a Liberian local NGO. A nationality of the business is determined if at least one of the owners has that particular nationality. The number of employees includes the owner or manager of the firm.

TABLE A.3: EFFECT OF VOUCHER AND ENCOURAGEMENT ON TRAINING TAKE-UP

PANEL A : BASED ON ATTENDANCE				
	General Procurement Training (Part 1/2)		Bid Compilation Training (Part 2/2)	
	(1)	(2)	(3)	(4)
Voucher + Encouragement	0.187***	0.200***	0.161***	0.175***
	(0.0151)	(0.0169)	(0.0144)	(0.0156)
Controls				
Control Mean	0.010	0.010	0.010	0.010
Observations	1192	1143	1192	1143
PANEL B : SELF-REPORTED				
	Did Your Firm Go Through Training		How Many Training Sessions did Your Firm Attend	
	(1)	(2)	(3)	(4)
Voucher + Encouragement	0.140***	0.161***	0.330***	0.356***
	(0.0365)	(0.0374)	(0.106)	(0.107)
Controls				
Control Mean	0.507	0.507	0.680	0.680
Observations	789	754	789	754

Standard errors are in parentheses and are robust. This table presents coefficients of the regression of training take-up as recorded by Building Markets on encouragement. Controls include employment, counties of operation, gender of the owner, sectors, languages used for business, geographical zone and the number of submitted bids. All controls are measured at baseline. Panel A measures training attendance using data collected by the NGO Building Markets, while Panel B measures attendance based on self-reported attendance by the Firms. Columns (1) to (4) refer to the two parts of the training delivered by Building Market. Columns (1) and (2) refer to the "General Procurement" training. This training teaches basic knowledge to find and apply to tenders. Column (3) and (4) refer to the "Bid Compilation" training. During this training attendees are presented a fake tender and asked to prepare a bid. The Global Procurement training is a requirement for the Bid Compilation training. Panel B refers to training take-up as reported by the firm. This includes all types of training, not only the training studied in the scope of this paper. Columns (2) and (4) of both panels include controls.

TABLE A.4: EFFECT OF VOUCHER AND ENCOURAGEMENT ON TENDERS APPLIED TO AND CONTRACTS WON, ADDITIONAL OUTPUTS

	Submitted bids in the past 6 months		Proportion of tenders applied to (over relevant tenders)		Proportion of tenders won (over relevant tenders)	
	(1)	(2)	(3)	(4)	(5)	(6)
	TREATMENT-ON-THE-TREATED					
Took Training	0.0916	0.127	0.0480**	0.0468**	0.0223*	0.0229**
	(0.109)	(0.0981)	(0.0204)	(0.0191)	(0.0116)	(0.0110)
	INTENT-TO-TREAT					
Voucher + Encouragement	0.0238	0.0350	0.0125**	0.0129**	0.00578*	0.00629**
	(0.0284)	(0.0282)	(0.00527)	(0.00539)	(0.00298)	(0.00311)
Controls	NO	YES	NO	YES	NO	YES
Control Mean	0.169	0.169	0.007	0.007	0.003	0.003
Observations	787	752	787	752	789	754

Standard errors are in parentheses and are robust. The top panel presents the Treated on the Treated Estimate while the bottom panel presents the Intent to Treat estimate. Controls include employment, counties of operation, gender of the owner, sectors, languages used for business, geographical zone and the number of submitted bids. All controls are measured at baseline. All outputs are referring to the period of 6 months preceding the interview. Columns (1) & (2) refer to whether the firm submitted bids for tenders. Columns (3) & (4) refer to the proportion of tenders the firm applied to over total number of tenders in its sector(s). Columns (5) & (6) refer to the proportion of tenders the firm won over total number of tenders in its sector(s). Columns (2), (4) & (6) include controls.

TABLE A.5: EFFECT OF VOUCHER AND ENCOURAGEMENT ON THE QUALITY OF CONTRACTS WON, ADDITIONAL OUTPUTS

	Missing value of contracts		Ever worked with international clients	
	(1)	(2)	(3)	(4)
	TREATMENT-ON-THE-TREATED			
Took Training	0.214*	0.150	0.305**	0.290**
	(0.117)	(0.111)	(0.135)	(0.124)
	INTENT-TO-TREAT			
Voucher + Encouragement	0.0554*	0.0411	0.0791**	0.0796**
	(0.0302)	(0.0318)	(0.0343)	(0.0347)
Controls	NO	YES	NO	YES
Control Mean	0.190	0.190	0.285	0.285
Observations	789	754	789	754

Standard errors are in parentheses and are robust. The treatment-on-the-treated estimate the effect of training for firms who attended the General Procurement training (Part 1/2). Controls include employment, counties of operation, gender of the owner, sectors, languages used for business, geographical zone and the number of submitted bids. All controls are measured at baseline. Columns (1) & (2) refer to whether the firm has missing values for all its contracts (either because the firm did not want to communicate values, or because the firm won no contracts). Columns (3) & (4) refer to whether the firm ever worked with international clients. Columns (2) & (4) include controls.

TABLE A.6: BASELINE CORRELATION BETWEEN FIRM CHARACTERISTICS AND INTERNET ACCESS

	Total # of employees	# of tenders applied to	# of contracts won through a tender	# of contracts won w/o tender	Ability to access tenders easily (1-10)
	(1)	(2)	(3)	(4)	(5)
Internet	14.05***	1.062***	0.319***	0.0818	1.934***
	(3.917)	(0.143)	(0.0602)	(0.122)	(0.535)
Constant	5.092***	0.128**	0.0426*	0.500***	5.706***
	(1.038)	(0.0555)	(0.0217)	(0.0776)	(0.387)
Observations	546	546	544	546	265

Standard errors are in parentheses and are robust. This table looks at the correlation of baseline Internet usage with firm characteristics correlated with unobservable firm quality. All variables are measured at baseline.

TABLE A.7: HOW THE EFFECT ON TRAINING TAKE-UP VARIES WITH INTERNET ACCESS

PANEL A : BASED ON ATTENDANCE				
	General Procurement Training (Part 1/2)		Bid Compilation Training (Part 2/2)	
	(1)	(2)	(3)	(4)
Voucher + Encouragement	0.177***	0.178***	0.152***	0.147***
	(0.0242)	(0.0261)	(0.0228)	(0.0244)
Voucher + Encouragement × Internet	0.0247	0.0454	0.0250	0.0651
	(0.0409)	(0.0458)	(0.0388)	(0.0415)
Controls	NO	YES	NO	YES
Control Mean	0.010	0.010	0.010	0.010
Observations	1118	1077	1118	1077

Standard errors are in parentheses and are robust. Controls include employment, counties of operation, gender of the owner, sectors, languages used for business, geographical zone and the number of submitted bids. All controls are measured at baseline. This table looks at the heterogeneity of the take-up of training with respect to Internet usage by firms at baseline. Panel A measures training attendance using data collected by the NGO Building Markets, while Panel B measures attendance based on self-reported attendance by the Firms.

TABLE A.8: HOW THE EFFECT ON FIRM EXPECTATIONS VARIES WITH INTERNET ACCESS

	How many tenders do you expect your firm to bid on in the next in the next 6 months?		Of the tenders your firm will bid on in the next 6 months, how many do you expect you will win?	
	(1)	(2)	(3)	(4)
Voucher + Encouragement	0.395**	0.355*	0.318*	0.290*
	(0.191)	(0.202)	(0.166)	(0.172)
Voucher + Encouragement × Internet	-0.419	-0.233	-0.275	-0.210
	(0.317)	(0.335)	(0.270)	(0.282)
Controls	NO	YES	NO	YES
Control Mean	2.037	2.037	1.510	1.510
Observations	740	711	740	711

Standard errors are in parentheses and are robust. Controls include employment, counties of operation, gender of the owner, sectors, languages used for business, geographical zone and the number of submitted bids. All controls are measured at baseline. Columns (1) & (2) refer to the refer to the expectations of the manager about the future bids of the firms. Columns (3) & (4) refer to the expectations of the manager about the firm's future contracts won through tenders. Columns (2) & (4)of both panels include controls.

TABLE A.9: HOW THE EFFECT ON TRAINING TAKE-UP VARIES WITH INTERNET AND ONLINE DEMAND SHOCKS

PANEL A : BASED ON ATTENDANCE				
	General Procurement Training (Part 1/2)		Bid Compilation Training (Part 2/2)	
	(1)	(2)	(3)	(4)
Voucher + Encouragement	0.128***	0.100***	0.115***	0.0836***
	(0.0209)	(0.0233)	(0.0203)	(0.0223)
Voucher + Encouragement × Online Demand x Internet	-0.00195	-0.00285	0.00753	0.00713
	(0.00714)	(0.00671)	(0.00987)	(0.00993)
Controls	NO	YES	NO	YES
Control Mean	0.010	0.010	0.010	0.010
Observations	1118	1077	1118	1077
PANEL B : SELF-REPORTED				
	Did Your Firm Go Through Training		How Many Training Sessions did Your Firm Attend	
	(1)	(2)	(3)	(4)
Voucher + Encouragement	0.0971	0.0257	0.155	-0.0235
	(0.0624)	(0.0714)	(0.175)	(0.210)
Voucher + Encouragement × Online Demand x Internet	-0.00905**	-0.0107***	-0.0204	-0.0355
	(0.00364)	(0.00405)	(0.0253)	(0.0273)
Controls	NO	YES	NO	YES
Control Mean	0.507	0.507	0.680	0.680
Observations	741	712	741	712

This table presents the heterogeneity of treatment effect with respect to Internet access at baseline and the internet-based demand shock on take up of both types of training. Here the Internet-based demand shock is computed from the date of encouragement to the date of training. Standard errors are in parentheses and are robust. Controls include employment, counties of operation, gender of the owner, sectors, languages used for business, geographical zone, the number of submitted bids, total number of tenders published in the sector, share of different types of tenders in the sector (tenders from local or international orgnizations, local or international governments, local or international private firms, local or international NGOs. All controls are measured at baseline. Panel A measures training attendance using data collected by the NGO Building Markets, while Panel B measures attendance based on self-reported attendance by the Firms.

TABLE A.10: HOW THE EFFECT ON CONTRACT QUALITY VARIES WITH INTERNET AND ONLINE DEMAND SHOCKS

	Ever had a contract of more than 6 months
	(1)
Took Training	0.0814
	(0.253)
Took Training × Online Demand	-0.0300
	(0.0284)
Took Training × Internet	0.0503
	(0.430)
Took Training × Online Demand x Internet	0.0567**
	(0.0234)
Internet Access	-0.0481
	(0.0818)
Controls	YES
Control Mean	0.232
Observations	712

This table presents the heterogeneity of treatment effect with respect to Internet access at baseline and the internet-based demand shock on the quality of contracts won. Standard errors are in parentheses and are robust. Controls include employment, counties of operation, gender of the owner, sectors, languages used for business, geographical zone, the number of submitted bids, total number of tenders published in the sector, share of different types of tenders in the sector (tenders from local or international organizations, local or international governments, local or international private firms, local or international NGOs. All controls are measured at baseline.

TABLE A.11: HOW THE EFFECT ON TENDERS AND CONTRACTS VARIES WITH INTERNET AND *Total* DEMAND SHOCKS

	# of bids submitted		Total # of contracts		# of contracts won through a tender		# of contracts won w/o tender		Total value of contracts in USD	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Took Training	-1.142	-0.956	-0.966	-1.592**	-0.0465	-0.0847	-0.921*	-1.496**	-5299.9	-5395.0
	(1.055)	(0.593)	(0.588)	(0.720)	(0.179)	(0.270)	(0.512)	(0.616)	(7490.9)	(8015.1)
Took Training	0.0279	0.0100	0.0373**	0.0447**	-0.00477	-0.00511	0.0421***	0.0495***	43.26	6.908
× Total Demand	(0.0381)	(0.0186)	(0.0163)	(0.0190)	(0.00427)	(0.00799)	(0.0159)	(0.0174)	(244.9)	(251.5)
Took Training	-0.0943	-0.462	1.923*	2.316*	-0.599	-0.509	2.537***	2.805***	-3481.7	-15124.1
× Internet	(1.692)	(1.143)	(1.147)	(1.225)	(0.639)	(0.623)	(0.948)	(1.030)	(25904.9)	(26069.1)
Took Training × Total Demand x Internet	0.0264	0.0612*	-0.0203	-0.0196	0.0333**	0.0373**	-0.0539**	-0.0564***	822.7	1087.8
	(0.0559)	(0.0322)	(0.0273)	(0.0270)	(0.0159)	(0.0152)	(0.0223)	(0.0216)	(646.7)	(664.3)
Controls	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
Control Mean	0.352	0.352	0.482	0.482	0.155	0.155	0.327	0.327	5322.458	5322.458
Observations	739	710	740	711	741	712	740	711	741	712

This table presents the heterogeneity of treatment effect with respect to Internet access at baseline and the *total demand shock* within a sector. Standard errors are in parentheses and are robust. Controls include employment, counties of operation, gender of the owner, sectors, languages used for business, geographical zone, the number of submitted bids, total number of tenders published in the sector, share of different types of tenders in the sector (tenders from local or international organizations, local or international governments, local or international private firms, local or international NGOs. All controls are measured at baseline. Columns (1) & (2) refer to the number of bids submitted for tenders. Columns (3) to (8) refer to the number of contracts won overall, through a tender process and without a tender process. Columns (9) & (10) refer to the total value of contracts won by firms.

TABLE A.12: HOW THE EFFECT ON CONTRACT QUALITY VARIES WITH INTERNET AND *TOTAL* DEMAND SHOCKS

	Ever had a contract of more than 6 months	
	(1)	(2)
Took Training	-0.198	-0.0892
	(0.241)	(0.225)
Took Training × Total Demand	0.00492	-0.00168
	(0.00692)	(0.00634)
Took Training × Internet	0.258	0.220
	(0.457)	(0.434)
Took Training × Total Demand x Internet	0.00896	0.0159*
	(0.0117)	(0.00951)
Internet Access	0.216***	-0.0544
	(0.0665)	(0.0804)
Controls	NO	YES
Control Mean	0.232	0.232
Observations	741	712

This table presents the heterogeneity of treatment effect with respect to Internet access at baseline and the *total demand shock* within a sector. zone, the number of submitted bids, total number of tenders published in the sector, share of different types of tenders in the sector (tenders from local or international organizations, local or international governments, local or international private firms, local or international NGOs. All controls are measured at baseline.

TABLE A.13: HOW THE EFFECT ON EXPECTATIONS VARIES WITH INTERNET AND ONLINE DEMAND SHOCKS

	How many tenders do you expect your firm to bid on in the next in the next 6 months?	
	(1)	(2)
Took Training	1.755	1.307
	(1.217)	(1.066)
Took Training × Online Demand	-0.0532	-0.0666
	(0.0947)	(0.0897)
Took Training × Internet	-1.346	0.00470
	(1.802)	(1.634)
Took Training × Online Demand x Internet	0.0295	-0.0581
	(0.0776)	(0.0760)
Internet Access	0.0517	0.146
	(0.335)	(0.283)
Controls	YES	YES
Control Mean	2.037	1.510
Observations	711	711

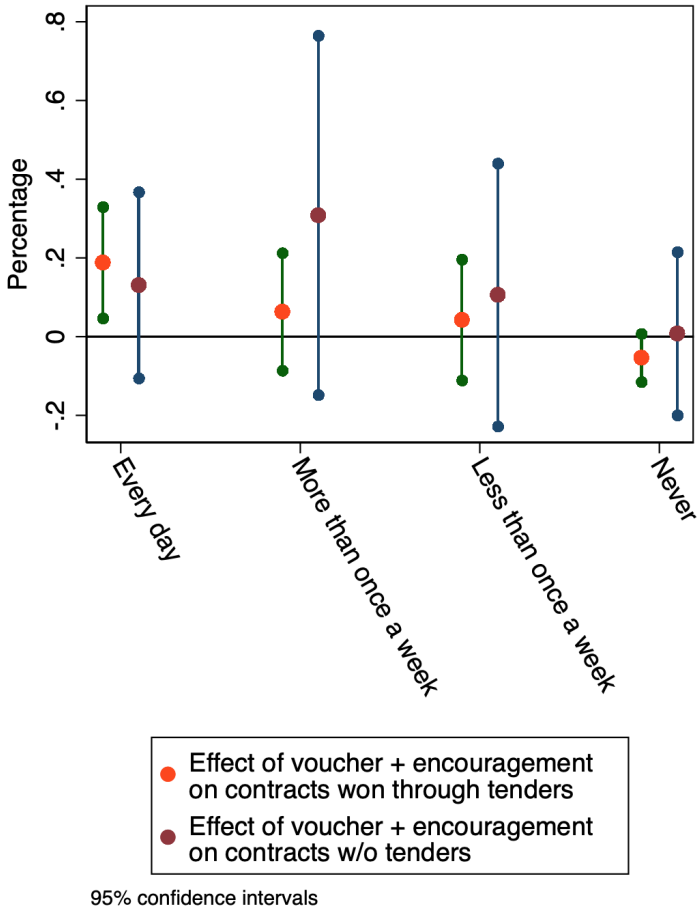
This table presents the heterogeneity of treatment effect with respect to Internet access at baseline and the internet-based demand shock. Standard errors are in parentheses and are robust. Controls include employment, counties of operation, gender of the owner, sectors, languages used for business, geographical zone, the number of submitted bids, total number of tenders published in the sector, share of different types of tenders in the sector (tenders from local or international orgnizations, local or international governments, local or international private firms, local or international NGOs. All controls are measured at baseline.

TABLE A.14: HOW THE EFFECT ON EXPECTATIONS VARIES WITH INTERNET AND *TOTAL* DEMAND SHOCKS

	How many tenders do you expect your firm to bid on in the next in the next 6 months?		Of the tenders your firm will bid on in the next 6 months, how many do you expect you will win?	
	(1)	(2)	(3)	(4)
GP Training	0.694	1.601	0.662	1.057
	(0.908)	(1.101)	(0.802)	(0.939)
Took Training × Total Demand	0.0276	-0.00908	0.0193	0.000862
	(0.0205)	(0.0245)	(0.0181)	(0.0218)
Took Training × Internet	-1.799	-1.162	-0.722	0.0586
	(1.540)	(1.780)	(1.358)	(1.590)
Took Training × Total Demand x Internet	-0.0108	0.00922	-0.0175	-0.0184
	(0.0325)	(0.0327)	(0.0291)	(0.0300)
Internet Access	0.828***	0.0173	0.551**	0.0684
	(0.265)	(0.320)	(0.222)	(0.266)
Controls	NO	YES	NO	YES
Control Mean	2.037	2.037	1.510	1.510
Observations	740	711	740	711

This table presents the heterogeneity of treatment effect with respect to Internet access at baseline and the internet-based demand shock. Standard errors are in parentheses and are robust. Controls include employment, counties of operation, gender of the owner, sectors, languages used for business, geographical zone, the number of submitted bids, total number of tenders published in the sector, share of different types of tenders in the sector (tenders from local or international orgnizations, local or international governments, local or international private firms, local or international NGOs. All controls are measured at baseline.

FIGURE A.1: EFFECT OF VOUCHER + ENCOURAGEMENT ON CONTRACTS BY CONTRACT TYPE AND INTERNET USAGE



This graph plots the coefficients of heterogeneous effect of the training with respect to Internet usage on contracts won. The orange dots represent the effect of the treatment on contracts won through tenders while the maroon dots represent the heterogeneous impact of the training on contracts won without tenders.