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The additionality impact of a matching grant programme for small firms: experimental evidence from Yemen

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ABSTRACT

Matching grants are one of the most common types of private sector development programmes used in developing countries. But government subsidies to private firms can be controversial. A key question is that of additionality: do these programmes get firms to undertake innovative activities that they would not otherwise do, or merely subsidise activities that would take place anyway? Randomised controlled trials (RCTs) can provide the counterfactual needed to answer this question, but efforts to experiment with matching grant programmes have often failed. This article uses an RCT of a matching grant programme for firms in Yemen to demonstrate the feasibility of conducting experiments with well-designed programmes, and to measure the additionality impact. In the first year, the matching grant is found to have led to more product innovation, firms upgrading their accounting systems, marketing more, making more capital investments and being more likely to report their sales grew.

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innovation

1. Introduction

Matching grants consist of partial subsidies (typically 50%) provided by government programmes to try to spur firms to undertake innovative activities such as creating new products, technological upgrading, exporting, using business development services (BDS) and, more broadly, firm expansion. They are one of the most common policy tools used in private sector development projects in developing countries, and have been included in more than 60 World Bank projects totalling over US\$1.2 billion, funding over 100,000 micro, small and medium enterprises.¹ Yet credible evidence on whether these grants induce firms to undertake innovative activities that they would not otherwise do (*additionality*) is limited: there are several case studies and non-experimental evaluations² that are subject to the concern that firms that self-select into such programmes may differ in unobservable dimensions from those not selected for funding, while a number of recent attempts to conduct randomised experiments to evaluate these programmes in sub-Saharan Africa have failed for reasons such as lack of applications or governments changing their minds on random selection (Campos et al. 2014). The one exception is work by Bruhn, Karlan, and Schoar (2012) on a matching grant programme in Puebla, Mexico, that had a higher than usual match rate (70-90% subsidy) for consulting services, and found firms receiving the subsidy did not significantly innovate more, but did do more marketing, improved their accounting and increased sales and profits.

A potential rationale for these subsidies is that owners of small firms may under-invest in learning and innovation, either because they underestimate the gains from using BDS, because

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The underlying research materials for this article can be accessed through the World Bank's Open Data Library at <http://microdata.worldbank.org/index.php/catalog/2526>.

these gains spillover to other firms, or because of credit market imperfections. In particular, two characteristics of innovation make financing more difficult: (i) innovation produces an intangible asset and (ii) the returns to innovation investment are uncertain (e.g. Hall and Lerner (2009) and Kerr and Nanda (2014)). Reluctance to make such investments may be particularly high in fragile and conflict-afflicted states, in which market failures may be more pervasive and the underlying uncertainty increases the option value of waiting to make investments (Bernanke 1983; Dixit and Pindyck 1994). Matching grants may help firms overcome this reluctance and spur new activity, or conversely, these other factors may limit the demand for such programmes only to the few firms planning on undertaking these activities anyway, in which case the subsidy would not have any additionality. To date there is no evidence from fragile states as to which channel dominates.

This article reports on a randomised experiment testing the impact of matching grants for BDS in the Republic of Yemen. The programme provided firms with a matching grant of up to \$10,000 as a 50% subsidy towards the cost of business services like finance and accounting systems, website creation, training, marketing, participation in exhibitions, and some associated goods such as office and IT equipment. The programme was intended to run in two annual calls, but the second year was canceled due to the outbreak of civil conflict in 2015. We therefore report on the randomised experiment run on the first round.

The results show that such programmes can be designed in a way that results in many firms applying, randomisation is feasible, and that the programme does appear to result in additional innovation and firm growth. Building on lessons from past matching grant programmes, the programme was designed to make it easy for firms to apply, and approximately four times as many eligible firms ended up applying as there was funding available. A public randomisation ceremony proved an effective and transparent way of choosing among eligible applicants. A follow-up survey was conducted in March 2015 just as civil conflict was beginning to break out. Although this survey had relatively high attrition (45%), the sample interviewed remain balanced between treatment and control on observed characteristics, and the results show that the matching grant led to firms being 37 percentage points more likely to engage in innovation, introducing more new products, carrying out more marketing, being more likely to introduce a new accounting system, training more workers, making more capital investments and being 48 percentage points more likely to say their sales grew over the past year. These results suggest that additionality was a result of this matching grant programme.

The remainder of the article is structured as follows: [Section 2](#) discusses the context and details of the matching grant programme; [Section 3](#) describes the randomisation process and take-up of the programme; [Section 4](#) provides the results of the impact evaluation, and [Section 5](#) concludes.

2. Context and details of the programme

The Republic of Yemen is the poorest in the Arab world, with a per-capita GDP of US\$1,473 (2013). As in most developing countries, micro and small enterprises constitute the majority of private firms, with 88% of firms estimated to have less than 5 workers, 4.7% between 5 and 9 and only 3.5% having more than 10 workers. Smaller firms tend to be less productive, and may face more constraints to taking up technology and innovation.

In the two years preceding the launch of the programme studied here, Yemen had undergone a revolution marked by protests, armed clashes, a contraction of the economy and the eventual removal of President Saleh who was replaced by his vice-president, Abd Rabbo Mansour al-Hadi. Real GDP shrank 12.7% in 2011 and only recovered 2.4% in 2012. In a survey of 200 firms in June, 2012, one year before the matching grant programme was launched, 72% of firms surveyed reported declining sales, and there was a 12% average decline in employment across firms surveyed. Most firms (about four out of five) reported availability of electricity, macroeconomic instability and political instability as major or severe constraints to business (Stone, Badawy, and Assaf 2012). Coupled with an already existing difficult business environment, and productivity

levels which are among the lowest in the region,³ these economic conditions made it particularly difficult for businesses to develop and grow. Overall, these conditions persisted and ultimately worsened with the severe fuel shortages and violent conflict escalating towards the end of the period of programme implementation.

2.1. The matching grant programme

The Enterprise Revitalization and Employment Pilot (EREP) was designed as a two-year pilot project aimed at improving firm capabilities and the employability of recent graduates. The matching grant component of this programme aimed to provide grants for BDS to 400 enterprises, 200 in each of two years.⁴ Firms could apply for a grant of up to \$10,000, as a 50% subsidy towards the cost of procurement of BDS, training and goods (as a minority component) to improve business practices, technology or products, or to reach new markets, as defined in a Business Development Plan submitted by the firm.

The programme implementation was designed with the lessons of other matching grant programmes in mind, many of which had struggled to get sufficient applicants due to issues such as too strict eligibility criteria, complicated application procedures, political capture and insufficient advertising (see Campos et al. 2014).

In particular, the focus on small and medium enterprises was achieved through the setting of a modest grant ceiling; the project did not limit eligibility by firm size. The eligibility criteria were simple, requiring that the firms must be operating in Yemen, have offices in either Sana'a or Aden, be in operation for at least 6 months, not be in a prohibited activity (such as weapons manufacture) and have submitted a complete application form. In order to make it easy for firms with limited access to the internet to apply, applications were accepted either online or on paper. The application form was reasonably short, and collected basic information on the firm such as employment level, sales, whether they export, types of innovative activities conducted in the past three years, and then basic information on what BDS they would like and an agreement to pay half the costs of these.

The programme was launched in August 2013 (see [Appendix 1](#)) and widely advertised through a 45-day campaign that included a project website, advertisements on the Yemeni official television channel and on radio, in many local newspapers, on Facebook and Google Ads, and through workshops and events held at local universities, hospitals, and business organisations. BDS advisors hired as consultants to the programme also visited firms in person to promote the programme and attract applications. Firms could apply between 1 October 2013 and 26 December 2013. They were then subjected to a simple screening process to ensure they met the eligibility criteria.

2.2. Context: conflict resumes

The first year of the programme took part in 2014, with the matching grants implemented as described later. However, in August 2014, part-way through this process, the rebel Houthis began demonstrations in Sana'a against increased fuel prices, and in September they took control of the city of Sana'a. A U.N.-brokered peace agreement was made in which they agreed to withdraw once a national unity government was formed. The situation worsened at the start of 2015, with the Houthis seizing control of the state television, President Hadi resigning and fleeing to Aden, and civil conflict breaking out in late March and early April 2015. As a result of these events, the second year of the programme was cancelled, and as a result, this analysis focuses on the first year of the programme.

3. Randomisation, take-up and data collection

A key issue in a number of other matching grant programmes has been a lack of take-up for the programme, with the small number of applications receiving dooming plans for experimental

analysis based on oversubscription designs (Campos et al. 2014). The combination of an easy application process and the need among firms for the programme meant that this was not an issue in Yemen. In total 820 applications were received (53% from Sana'a, 47% from Aden). This was slightly four times the number of grants available for the first round (200). About 19 applications were rejected due to firms not being eligible for the programme or not agreeing to cover their share of the costs.

3.1. Random selection

Firms were selected for the programme from among the eligible applicants in public randomisation events held in Sana'a on 9 January 2014 and Aden on 12 January 2014. The only variable used in stratification was city, with separate drawings occurring in the two cities. The events were open to the public, and attendees included some of the applicants, members of the project's Advisory Committee and media including television. In each city, 100 firms were randomly drawn, along with a reserve list in case any of the originally selected firms withdrew from the programme. In the end, this resulted in 216 firms being randomly assigned to the treatment group. We choose a random sample of 100 firms from the eligible applicants in each city to be the control group for follow-up.

Table 1 compares the baseline (application form) characteristics of the firms in the treatment and control groups. The firms are quite heterogeneous, ranging in size from the smallest firm having no paid employees to one firm with 950 employees. The median firm has 5 employees, with a mean of 14.8 and standard deviation of 53. One-third of firms report no sales at baseline, which may be missing data in many cases. Conditional on positive sales, the median firm has annual sales of 20,000 rials (\$93) and the mean \$18.9 million rials (\$88,000), with a standard deviation of 143 million rials. This heterogeneity (along with item non-response) severely limits the statistical power of this experiment to measure impacts on financial outcomes and employment, and as a result, we will focus largely on binary outcomes.

Table 2 provides a tabulation of the types of firms in terms of industry sector. We again see substantial heterogeneity, with the most common firm types being retail sales, selling and producing food, clothing, education and training, and computer services. The firms are almost entirely domestically focused (only 2% export), are mostly owned by men and the majority have not been undertaking many innovative activities in the three years prior to application: only 16% had

Table 1. Baseline characteristics for experimental sample and for follow-up sample.

	Full experimental sample			Sample interviewed at follow-up		
	Treatment	Control	<i>p</i> -value	Treatment	Control	<i>p</i> -Value
Owner is female	0.09	0.16	0.027	0.11	0.16	0.202
Firm is in Aden	0.50	0.50	<i>s</i>	0.50	0.55	<i>s</i>
Number of male employees	13.56	9.41	0.320	8.46	6.51	0.306
Number of female employees	3.59	2.87	0.499	2.11	2.15	0.832
Expanded business in last 3 years	0.25	0.24	0.713	0.26	0.20	0.372
Increased employment in last 3 years	0.12	0.13	0.773	0.11	0.11	0.929
Marketing campaign in last 3 years	0.30	0.31	0.821	0.27	0.31	0.595
Expanded to new market in last 3 years	0.13	0.15	0.556	0.11	0.14	0.452
Improved product line in last 3 years	0.19	0.14	0.210	0.16	0.15	0.821
Has an ISO qualification	0.02	0.01	0.286	0.02	0.00	0.159
Trained workers in last 3 years	0.27	0.22	0.201	0.27	0.23	0.636
Went to international expo in last 3 years	0.06	0.06	0.845	0.05	0.05	0.966
Exported in 2012	0.02	0.03	0.656	0.02	0.01	0.749
Sales in 2012	6757172	18833812	0.309	2502169	5695261	0.274
Joint orthogonality test <i>p</i> -value			0.318			0.863
Sample size	216	200		128	98	

s denotes variable was used for stratification. *p*-Values are from regression controlling for location strata.

Table 2. Types of firms.

Firm type	Number
Retail sales	27
Foodstuffs	25
Clothing	23
Education and training	17
Miscellaneous services	17
Computer training, internet café, computer services	15
Medical diagnosis, lab services, equipment provision, etc.	13
Telecom and selling scratch cards	13
Advertising services	12
Perfumes and cosmetics, beauty	8
Restaurant	8
Construction materials, consulting services and distributing	7
Manufacturing	7
Miscellaneous consulting services	7
Travel and car rental	7
Accounting services	4
Dental treatment	3
Administrative sciences and projects	1
Other	12

Classification for firms responding to follow-up survey.

improved a product line, only 2% had any type of quality certification like ISO, 24% had trained workers, 6% had attended an international exposition or fair and 13% had expanded their market.

Random selection ensures that the treatment and control samples look similar on average: a joint test of orthogonality cannot reject the null hypothesis of balance ($p = 0.318$).

3.2. Take-up

Firms which were selected in the random draw were invited to workshops in February in which they were told about the procedures for procurement, and the process for filling out a business development plan (BDP). The plan set out what the firm planned to do with the matching grant, and was reviewed by a project management committee for approval. These BDPs were reviewed as received between March and June 2014, and once approved, agreements were signed. Firms were then reimbursed for the agreed services upon completion of the consulting services and providing evidence of payment.⁵ Figure 1 shows the main services requested by firms. The most popular requests were for help in marketing, accounting and worker training.

According to administrative records, 170 firms had signed agreements by 30 October 2014. However, a number of these firms dropped out as a result of concerns about the unstable security situation, and a fuel crisis meaning that they did not want to take the risk of paying 50% of the cost of the planned activity. According to the follow-up survey, 57.8% of the treatment group used the matching grant in the end.

Table 3 compares the baseline attributes of those firms which did take up the matching grant to those in the treatment group which did not, for the sample answering the follow-up survey. We see that the two groups are not statistically different in most characteristics. This in part reflects the relatively small sample size, but it is not the case that those who take the grant are large firms while those who drop out are small firms for example. Those who take up the grant are more likely to have expanded their business in the three years prior to their application, but no more likely to have increased employment, done new marketing or improved their product during this time. A probit of take-up on these observable characteristics drops having attended an international expo since none of the dropouts had done this, and having exported, but then cannot reject that the other variables jointly cannot predict take-up. This is consistent with take-up decisions being driven by idiosyncratic factors arising from the general economic environment, rather than sorting the firms according to their inherent productivity or innovativeness.

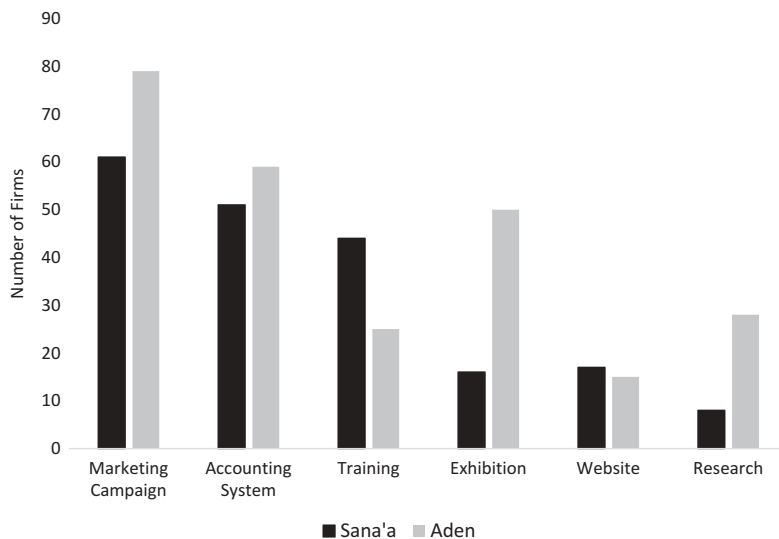


Figure 1. Main business development services requested by firms. Source: Administrative data from matching grant applicants.

Table 3. Differences in baseline attributes by take-up status.

	Take-up	Don't take-up	<i>p</i> -Value
Owner is female	0.122	0.093	0.522
Firm is in Aden	0.527	0.463	s
Number of male employees	8.041	9.037	0.664
Number of female employees	2.338	1.796	0.400
Expanded business in last 3 years	0.338	0.148	0.010
Increased employment in last 3 years	0.108	0.111	0.930
Marketing campaign in last 3 years	0.284	0.241	0.657
Expanded to new market in last 3 years	0.108	0.111	0.959
Improved product line in last 3 years	0.189	0.130	0.387
Has an ISO qualification	0.014	0.019	0.817
Trained workers in last 3 years	0.297	0.222	0.302
Went to international expo in last 3 years	0.081	0.000	0.013
Exported in 2012	0.014	0.019	0.817
Sales in 2012	39,54,254	512,275	0.127
Joint orthogonality test <i>p</i> -value			0.424 ^a
Sample size	74	54	

^aNote this drops exported in 2012 and went to international expo from probit.

s denotes variable was used for stratification. *p*-Values are from regression controlling for location strata.

3.3. Data collection

A call for the second year of the programme took place in November 2014, with random selection in December 2014. However, with the worsening security situation in Yemen, this second year of the programme was abandoned. We therefore quickly launched a follow-up survey in March 2015. This timing is approximately 4–10 months after firms had used their matching grants, and right before widespread civil conflict broke out.

The survey was conducted as a phone survey given that safety issues, gas shortages and time constraints prohibited an in-person survey. The survey collected basic company information such as operating status and employment, the firm's innovation activities and use of consultants, financing from banks and the grant programme, and on the firms' exports and sales. Like most surveys in developing countries of firms this size, we must rely on self-reporting of these activities,

lacking any alternative sources of data with which to cross-check these responses.⁶ The survey was conducted by Apex consulting and succeeded in interviewing 226 of the target 416 firms (a response rate of 54.3%). Attrition was higher in the control group (51%) than the treatment group (40.7%), with this difference statistically significant ($p = 0.036$). Attrition rates did not vary significantly by city. Of those firms interviewed, 97.8% had survived (four treated and one control had closed). It seems likely that those who had closed because of fuel shortages or safety concerns would be more likely to have attrited.

This response rate and its difference by treatment status is a concern, although given the circumstances with suicide bombings and airstrikes beginning at the end of March ([Appendix 1](#)), no further actions could be taken to chase down non-responders. The last three columns of [Table 1](#) compare the baseline application characteristics for the treatment and control samples that did respond to the survey. We see that these characteristics look similar for most variables to the full sample in the first three columns and, more importantly, are similar for treatment and control. We cannot reject the null hypothesis of balance on these observables with a joint orthogonality test ($p = 0.863$). Typically we might expect grants and training to enhance the survival prospects of firms that would otherwise close down (e.g. de Mel, McKenzie, and Woodruff 2014; McKenzie 2015). If this is the case, this would suggest that any bias from differential response would be in the direction of the additional respondents in the treatment group being less innovative firms that would have closed (and therefore attrited) if they had been in the control group – in which case our estimates could be lower bounds on the programme effects. As a final check on robustness, in [Appendix 2](#), we construct Lee (2009) lower bounds and find that even in this case the programme had a significant impact.

4. Results

We have seen from [Figure 1](#) that the main uses firms requested the matching grants for were for implementation of accounting systems, marketing plans and training. When asked directly in the follow-up survey whether they would have undertaken these activities without the grant (i.e. about additionality), 43% of those using the grant say they would not have done so, 41% say they would have done so at a smaller scale and 16% say they would have done the same activity anyway. We use our experiment to provide more rigorous measurement of the additionality of the programme.

We estimate the impact of being selected for a matching grant (the intention to treat, ITT) effect by estimating the following equation for outcome Y and firm i :

$$Y_i = bT_i + \delta Aden_i + \varepsilon_i \quad (1)$$

where T_i is an indicator for being randomly selected for a grant, and $Aden$ is a dummy variable for the city of Aden to account for the stratified randomisation. To show robustness to concerns that attrition may have resulted in imbalance between the treatment and control groups, we also re-estimate Equation (1) controlling for the set of baseline variables in [Table 1](#). Since we have seen that the two groups in fact seem balanced on these characteristics, we should expect that adding these characteristics will not change b substantively. In addition, we estimate the impact of actually receiving and using the matching grant (the local average treatment effect, LATE) by instrumenting the receipt of treatment with assignment to treatment. Since none of the control group received a grant, the LATE is also the treatment-effect-on-the-treated.

Given the logistical and time constraints on fielding the follow-up survey, we were limited in the number and complexity of questions we could ask. In addition, as noted earlier, the heterogeneity of firms limits our power for examining impacts on financial outcomes. We did try asking about sales at the end of the survey, but had 51% item non-response amongst the firms that did answer the survey. As a result, we largely examine binary outcomes that help address the issue of

additionality: did the grants get firms to undertake more innovative activities than they otherwise would have done, or did it just subsidise them to do what they were going to do anyway?

4.1. Impacts on innovative activities

Table 4 examines the impact of the matching grant on the likelihood the firm undertook different innovative activities. This compares the treatment to the control firms, regardless of what firms requested the use of the grant for, to identify which activities display additionality after allowing for any fungibility in fund use. Panel A presents the ITT impacts with only a control for city (the randomisation strata) and Panel B the same impacts after controlling further for the range of observable baseline characteristics in Table 1. We see the two rows yield very similar results in terms of magnitudes and statistical significance, which is in line with the evidence presented earlier that the follow-up sample appears balanced on baseline characteristics. Panel C then presents the LATE estimates.

Column 1 shows that the matching grants resulted in a sizeable and statistically significant increase in product innovation: firms receiving the grants were 30.3 percentage points (p.p.) more likely to introduce a new product, which is more than a doubling of the 26.5% rate in the control group. Column 2 shows the impact on process innovation is positive, but not statistically significant. Columns 3, 4 and 5 show that in accordance with what firms reported wanting the grants for, they do more marketing (19.8 p.p. increase), introduce new accounting systems (48.5 p.p. increase) and do more worker training (33.2 p.p. increase) with the grants – in each case approximately doubling the control group rates. We saw at baseline that it was incredibly rare for firms to get quality certification, and Column 6 shows that this continues to be the case, with only 4% of the control group getting a new quality certification and the matching grants having no impact on this rate. Column 7 shows those with a matching grant are 27.4 p.p. more likely to introduce a new human resources (HR) system. Column 8 shows a positive, but not statistically significant, increase in the likelihood of attending a trade show abroad.

We consider two aggregate measures of innovative activities as a way of summarising this information and dealing with multiple hypothesis testing. Column 9 aggregates to form the number of activities, while Column 10 examines whether any innovative activity was undertaken. We see strong and statistically significant impacts on both measures. Firms receiving grants are 37.1 percentage points more likely to have undertaken an innovative activity, and take 1.8 innovative activities more than the control group on average. Taken together, these results indicate that the matching grant programme did have additionality in terms of getting firms to innovate more.

4.2. Impacts on firm inputs and growth

We then turn in Table 5 to examine the extent to which these activities increased firm inputs and firm outcomes. A key additional caveat here is that some of the innovative activities pursued by the firm with the grant may take some time to ultimately affect firm growth. For example, in their study of matching grants for consulting in Mexico, Bruhn, Karlan, and Schoar (2012) find no impact on employment in the first year, but that administrative data appear to show employment increases three years later. A second caveat here is that the instability in Yemen may have caused firms to hesitate in hiring additional workers, as well as limited the scope for firms to grow.

Columns 1 and 2 of Table 5 show that there is a negative, but not statistically significant, impact on permanent and temporary workers. Recall that the sample is rather heterogeneous in terms of initial employment size, and as a result, our power is low to detect changes in employment: a 95% confidence interval for the LATE impact on permanent employees is (-22, +14) relative to a control group mean of 11.6. So the data are largely uninformative about the employment impact.

Table 4. Impacts of the matching grant programme on firm innovation.

	Introduced new product	Introduced new process	Marketing campaign	New accounting system	New training for workers	New quality certification	New HR system	Attended trade show abroad	Number of innovative activities	Any innovative activity
Panel A: ITT impact with only stratification controls										
Selected for grant	0.154** (0.063)	0.070 (0.055)	0.101* (0.056)	0.248** (0.065)	0.169** (0.066)	-0.001 (0.027)	0.140** (0.043)	0.051 (0.059)	0.932*** (0.249)	0.189*** (0.060)
Panel B: ITT impact with baseline controls										
Selected for grant	0.170** (0.067)	0.046 (0.055)	0.097* (0.058)	0.246** (0.066)	0.178*** (0.067)	-0.005 (0.026)	0.109*** (0.040)	0.060 (0.058)	0.901*** (0.248)	0.181*** (0.061)
Panel C: LATE impact										
Received grant	0.303** (0.124)	0.137 (0.107)	0.198* (0.108)	0.485*** (0.126)	0.332*** (0.126)	-0.003 (0.053)	0.274*** (0.085)	0.100 (0.114)	1.825*** (0.471)	0.371*** (0.113)
Sample size	226	226	226	226	226	226	226	226	226	226
Control mean	0.265	0.173	0.173	0.306	0.418	0.041	0.061	0.224	1.663	0.622

Robust standard errors in parentheses; *, **, and *** indicate significance at the 10%, 5% and 1% levels, respectively. Panels A and C control for city (used for stratifying the randomisation); Panel B for the full set of baseline controls in Table 1.

Table 5. Impacts of the matching grant programme on firm inputs and outcomes.

	Full-time workers	Temporary workers	New capital investment	Used a consultant	Has a website	Received a new loan	Exported	Sales grew
Panel A: ITT impact with only stratification controls								
Selected for grant	-2.013 (4.717)	-2.712 (3.299)	0.321*** (0.064)	0.214*** (0.057)	0.011 (0.055)	-0.013 (0.058)	-0.009 (0.033)	0.243*** (0.065)
Panel B: ITT impact with baseline controls								
Selected for grant	-3.417 (4.369)	-3.082 (3.576)	0.310*** (0.064)	0.221*** (0.059)	0.017 (0.057)	-0.024 (0.059)	0.008 (0.035)	0.236*** (0.067)
Panel C: LATE impact								
Received grant	-3.913 (9.176)	-5.271 (6.433)	0.630*** (0.132)	0.419*** (0.110)	0.022 (0.108)	-0.026 (0.114)	-0.017 (0.066)	0.477*** (0.132)
Sample size	225	225	226	226	226	226	226	226
Control mean	11.571	6.724	0.347	0.163	0.214	0.245	0.071	0.316

Robust standard errors in parentheses, *, ** and *** indicate significance at the 10%, 5% and 1% levels, respectively. Panels A and C control for city (used for stratifying the randomisation); Panel B for the full set of baseline controls in Table 1.

Column 3 shows that firms receiving the matching grant were substantially more likely to make a new capital investment that cost more than US\$1000.⁷ Just over one-third of the control group (34.7%) had made such an investment, and the point estimate suggests 97% of those receiving grants made such an investment. This is consistent with some use of the matching grants being for capital investments to accompany the services provided. Column 4 shows that firms receiving the grants are also considerably more likely to have used a consultant. Columns 5, 6 and 7 show no significant impacts on the likelihood the firm has a website, receives a new loan or exports.

The heterogeneity in sales and high item non-response for a question on the volume of sales limits our ability to measure the return to the matching grant in terms of amount of sales. However, our questionnaire did ask firms how their sales in 2014 compared to their sales in 2013. About 31.6% of the control group said their sales had grown over the year. The last column of [Table 5](#) shows that recipients of the matching grant are 47.7 p.p. more likely to say their sales grew during 2014. This provides some evidence to suggest that the new products introduced and other innovative activities did translate into an improvement in firm outcomes in the first year.

5. Conclusions

Matching grants have been widely used by governments to attempt to spur private sector innovation and growth. However, it has proved difficult to assess the impact of these programmes, and a key concern has been whether they create additionality, resulting in benefits beyond those which the private sector would have otherwise produced even in the absence of the programme (Phillips 2001). This article demonstrates the feasibility of a randomised evaluation of a matching grant programme, and finds strong evidence that it has generated additional innovative activities. It provides the first such evidence from a fragile state environment, showing that even under these difficult circumstances, a well-designed programme can succeed in getting firms to innovate.

There are several caveats that must be taken into account when interpreting the results of this article. The first is that the eruption of civil conflict in Yemen led to higher than usual attrition in our follow-up survey. The data available suggest that this is not greatly biasing our results. Second, the conflict also means that we cannot say anything about the long-term impacts of these grants. Third, the fact that the second year of the grants programme was cancelled halved our sample size from what was originally planned, which together with the heterogeneity in the sample of applicants means we have no statistical power to examine impacts on employment or sales levels, nor to examine heterogeneity of impacts with firm characteristics. Finally, while our analysis addresses the issue of additionality, we are not able to measure whether these grants result in externalities for other firms in the economy. Nevertheless, given the popularity of these programmes and the dearth of evidence on their effects, we believe the results are a useful first step in assessing the effectiveness of matching grants.

Notes

1. Compared with Lebanon, Egypt, Saudi Arabia and Syria – see Yemen Investment Climate Assessment Update, World Bank, 2011.
2. Firms could also apply for subsidies to hire interns, with some firms applying only for these internship subsidies, some applying for the BDS grants and some applying for both. About 20% of the firms in our treatment group also applied for interns. We discuss the impact of the internship programme on youth employment in McKenzie, Assaf, and Cusolito (2016).
3. Data from a World Bank Latin American and the Caribbean overview available at <http://go.worldbank.org/OVDGTHSWY0>.
4. See Biggs (1999), Phillips (2001, 2002), Castillo et al. (2011), Crespi, Maffioli, and Melendez (2011), Gourdon et al. (2011) and Lopez-Acevedo and Tan (2011).
5. In some cases, particularly for very small firms or start-ups, direct payments were made to vendors for the balance of the costs once the firm provided evidence of payment of 50%.

6. See, for example, de Mel, McKenzie, and Woodruff (2008) in Sri Lanka and Blattman, Fiala, and Martinez (2014) in Uganda for the use of self-reported firm outcomes in grant interventions, and the set of papers reviewed by McKenzie and Woodruff (2014) for the use of self-reported firm outcomes in business training interventions. Recall error is likely to be less of a problem for the types of binary measures used for most of our measures (did you do action X or not?) than for monetary totals. Since the survey firm was different from the grant implementor, and the survey was presented in just understanding how firms had done over the past year of a difficult economy, we believe this mitigates against 'pleasing the grantor' bias.
7. This was asked as a binary question, about whether they had made such an investment or not, so we do not know the amount of investment made.

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Appendix 1. Timeline

August 2013: Program launched

1 October 2013 – 26 December 2013: Applications accepted

29 December 2013 – 5 January 2014: Screening for eligibility

January 2014: Random selection of firms for matching grants

February 2014: workshop to inform firms of procurement process, and firms fill out templates for approval

March–October 2014: firms implement business development plans, receive grants

September 2014: Houthi rebels take control of capital of Sana'a, UN peace deal announced in which they agree to withdraw once national unity government is formed.

January–February 2015: Houthis seize state TV; President Hadi resigns and flees to Aden; Houthis take charge of government

March 2015: Follow-up survey conducted

March 2015: Islamic state carries out suicide bombings (March 20), Saudi-led coalition launches air strikes and imposes naval blockade (March 26), clashes in Aden and other cities, by April widespread conflict.

Appendix 2. Lee Lower Bounds

We have shown in the text that the treatment and control groups are balanced in terms of observable baseline characteristics, despite having differential attrition. As a result, our results were robust to the inclusion of these characteristics in the regression. Moreover, we argued that any bias from differential attrition may well be a downward one, if the matching grants keep firms open that would have otherwise closed. Nevertheless, as a worst case scenario, we also consider here Lee (2009) bounds. We do this for the outcomes found to have the largest and most significant treatment impacts in Tables 4 and 5. If the treatment group had had the same attrition rate as the control group, it would have had 22 fewer observations in it at follow-up. To construct the Lee lower bounds, we assume that all of these observations were successes. For example, we randomly set as missing 22 of the 54 treatment group observations that reported introducing a new product. The table below shows the resulting estimates of the ITT. As we would expect, these lower bounds reduce the estimated impacts. Nevertheless, all of the lower bounds are still positive, and three out of six still statistically significant. Therefore, even in the unlikely case that all the additional control group firms who were not interviewed were innovating and growing their sales, the matching grant program is still found to have additionality.

Table A2. Lee lower bounds on key impacts.

	Introduced new product	New Accounting System	New training for workers	Any Innovative Activity	New capital investment	Used a consultant	Sales Grew
ITT Impact	0.034 (0.064)	0.154** (0.068)	0.084 (0.070)	0.149** (0.064)	0.253*** (0.068)	0.088 (0.056)	0.151** (0.068)
Sample Size	204	204	204	204	204	204	204

Robust standard errors in parentheses, *, **, and *** indicate significance at the 10, 5, and 1 percent levels respectively. Regressions also control for city.