AEARCTR-0004867

Matching or Cash Grants for Entrepreneurs: What is more Effective? Experimental Evidence from the Bagré Growth Pole Project in Burkina Faso.

Additional information

(to be read together with the information entered in the RCT registry template)

1. Description of the sample to be used in the study

In January to March 2018 MEBF reached out to micro, small and medium-sized agricultural and non-agricultural enterprises (MSMEs) in the study area by means of public information sessions, the local radio channels, as well as meetings with local authorities and potential applicants. Prospective applicants were informed about the program details and the application requirements.

Following a call for expressions of interest to receive a matching grant or a cash grant, MEBF deployed teams in the targeted municipalities to help applicants to fill out the application form which had the form of a simplified business plan template. In particular, the applicant had to provide details on the proposed project and, if applicable, details on the existing firm including its sector of activity, the main products and the production process. The business plan did also have to include the reasons for the investment decision, the objectives and expected results, an assessment of potential competitors and the plan for physical and soft investments (cost and quantities). The applicant also had to provide an estimate of the total cost of the project, the timeline for implementing the project and the jobs expected to be created. Finally, the applicant had to provide information about the performance of his or her business, if applicable, including anterior turnover. The support of MEBF staff was deemed necessary as the program intervened in a rural area where many applicants were illiterate and hence needed help to fill out the application and business plan template. MEBF staff also supported applicants to define comprehensively their needs in terms of technical assistance and trainings for their businesses as well as investments in equipment or inventory that they could buy in case they were offered a cash grant. In total, 2,279 business plans were collected.

By May 2018 MEBF staff and staff of Bagrépole shortlisted 1,612 of the 2,279 applicants. Each applicant was scored based on a predefined set of eligibility criteria. These included for example the cogency of the justifications on the needs expressed by the applicant, the relevance of the applicant's personal experience in relation to the proposed business and the potential of the proposed project to create and consolidate employment. Then MEBF conducted individual interviews with all preselected applicants to double check their level of commitment to their proposed business plan.

MEBF invited all pre-selected 1,612 applicants for a training based on a simplified module of the 'The Start and Improve Your Business Training' (SIYB) developed by the International Labor Organization (ILO). The intention of the training was to provide all applicants with the basics of business management, including accounting, financial and business planning to help grant recipients to finalize and implement their business plan. 1,575 of the pre-selected applicants attended the training. Using another round of face-to-face interviews MEBF then selected, again based on performance and quality of the proposed project, by August 2018 a final list of 1,300 applicants of which 1,200 were then foreseen for the lottery and 100 were put on a reserve list in case some applicants would drop out of the experiment before the implementation starts. A baseline survey with all 1,300 firms was conducted in November/December 2018. This survey was implemented with Innovations for Poverty Action (IPA). The sample size was chosen based on power considerations and budget constraints.

In April and May 2019 MEBF with technical support from the World Bank ran public lotteries in each participating municipality to select 400 beneficiaries for each of the two treatment arms and the control group. The lottery was supervised by a bailiff. We used a stratified randomization design to ensure a balanced grant size distribution across both treatment arms and the control group. Twenty-six out of 1,200 entrepreneurs did not participate in the lottery, and hence were replaced by other candidates selected during a follow-up lottery to complete the sample.

Given that the amount requested in cash could differ from the amount requested for a matching grant, we used the requested amount in cash for all firms for that purpose. This amount was in almost all cases higher than the one requested for a matching grant. Within each stratum each applicant had the same chance to be allocated to one of the three groups (matching, cash, control). Table 1 shows the number of entrepreneurs assigned into each stratum and randomly assigned to one of the three groups.

Table 1. Number of entrepreneurs per stratum and treatment arm

Blocks	Cash amount (in USD)	Cash group (#)	Matching group (#)	Control group (#)	Total (#)
1	. ≤ 1,999	68	68	70	206
2	2,000 to 2,999	153	154	153	460
3	3,000 to 3,999	106	106	105	317
4	4,000 to 4,999	42	41	41	124
5	5,000 to 5,999	12	12	12	36
6	6,000 to 6,999	5	5	7	17
7	7,000 to 8,000	14	14	12	40
	Total (#)	400	400	400	1,200

Source: Data from MEBF's report on the public lottery (2019).

Table 2 shows the mean and distribution within each treatment group. On average, every entrepreneur was granted US$ 3,420. The lowest grant is US$ 628 and the highest is US$ 11,659. By mistake nine entrepreneurs in Block 7 of which four cash grants and five matching grants beneficiaries were granted amounts larger than the ceiling of US$ 8,000.[1] As intended the distributions of the grant amounts are nearly identical across all three groups.

Table 2. Summary statistics of grants (US$) across groups

Group	Business (#)	Mean (US$)	Median (US$)	Standard Dev. (US$)	Minimum (US$)	Maximum (US$)
Cash	400	3,421	3,110	1,515	1,020	11,273
Matching	400	3,443	3,161	1,535	1,002	11,659

Source: Data from MEBF's report on the public lottery (2019).

Since, the distribution of requests significantly deviated from a uniform distribution across the seven strata, the number of winners that had to be drawn was determined in each stratum according to the share of all requests (cash and matching grants) in that stratum, i.e. in each stratum the number of winners is proportional to the number of applicants.

The sample size of 1,200 entrepreneurs, i.e. 400 per group, had been chosen based on power calculations and cost considerations. A sample size of 400 firms per group allows detecting effects in the order of about 0.20 standard deviations, with at least 80% power. If the

[1] The four cash grant beneficiaries received $11,273, $11,000, $9,291, and $8,741 respectively. The five matching grant beneficiaries received $11,659, $11,091, $10,009, $9,436, and $9,364 respectively.

calculations account in addition for a reasonable amount of sample attrition, the minimum detectable effect size increases to about 0.22 standard deviations. If a power of 90% is imposed the measurable effects increase to 0.23 and 0.26 standard deviations respectively. Given, that we have baseline data, at least for the existing firms, we can also estimate ANCOVA specifications and use a large set of co-variates which further adds to power.

Balance tests for a large range of observables and across the three treatment arms are provided in a detailed Baseline report that can be obtained upon request from the authors.

A follow-up survey undertaken about two years after the baseline survey indicates an attrition rate of 7.3%. Attrition has two major sources: Respondents who could not be found for instance because they moved away or because they were temporally not available because of a traveling or alike and respondents who refused to participate in the second wave of the survey. Yet, in each case several attempts had been made to reach out to all participants interviewed at baseline and to limit attrition to its absolute minimum. With respect to the treatment arms, the attrition rate is higher in the group of matching grant beneficiaries (9.3%), followed by the rate in the control group (7.5%), and the group of cash grant beneficiaries (5.3%). Yet, if we run regressions of attrition on respondent's characteristics and the treatment arms, the coefficients associated with the latter are insignificant. The observables that are correlated with attrition include the sector of activity, the number of employees and a range of household characteristics such as marital status, family size, religious affiliation, ethnicity and household's assets. When estimating impacts we will use regressions and control for these factors.

2. Key data sources

The study includes a baseline survey and two follow-up surveys, the first one two years after the baseline survey and the second one three years after the baseline survey. These surveys are based on structured questionnaires and are administered in face to face interviews by IPA using CAPI software and tablets.

On top of the quantitative surveys, we also use qualitative methods to probe into contextual factors, especially focus group discussions with firm owners and in-dept interviews with staff of MEBF, Bagrépole and also some grant beneficiaries. Moreover, we can draw on monitoring data on program demand, delivery of grants and services, actual program take-up and process information.

3. Hypotheses to be tested throughout the causal chain:

Impacts will be tested for the following ultimate and intermediate outcomes. Both treatments (cash grants and matching grants) will be tested against the control group and cash grants will be compared with matching grants.

1. Ultimate outcomes
    a. Survival (firm still operating)
    b. Productivity (returns to capital)
    c. Employment (total employment (staff), paid employment (staff), unpaid employment (staff), days worked) and wages paid

2. Intermediate outcomes

a. Profit, value added, turn over
b. Investment in equipment, construction, land, livestock, inventory
c. Management and other business skills (book keeping, financial literacy)
d. Capacity to innovate (measured via the first component from a principal component analysis applied on 15 dummies each taking the value 1 if the firm introduced either of 15 listed innovations)
e. Formalization
f. Access to credit

It is also tested whether the treatment had an impact on how firms report to having been affected by the COVID crisis, i.e. whether they report to having seen a decline of production, of sales, of investment, of employment, and of their general performance.

Heterogeneity in impacts for both treatment arms will be analysed along the following characteristics:

- Between already existing and new firms
- Between male owned and female owned firms
- According to size of the grant received and
- the sector of activity.

The impact assessment will also include an analysis of potential spill-overs from (the number of) treated firms within 500m, 2km and 5km (cash grants, matching grants) on other treated and untreated firms, including heterogeneity in spill-overs by treatment status.

4. Specify how variables will be constructed

With continuous outcomes we will run regressions with absolute values and log-transformed vales (or alternatively using the inverse hyperbolic sine transformation for those variables that can also take negative vales such as profits).

To analyse treatment effects on business practices and knowledge as well as the capacity to innovate we will estimate impacts outcome by outcome indicator and by calculating indices using Principal Component Analysis.

To avoid a bias through outliers, we will trim all monetary variables (investment, profits and turnover) at the 99^{th} percentile.

Missing values will not be imputed.

We will check in each case whether non-response is correlated with treatment status and if it is, construct bounds for our treatment estimates that are robust to this.

5. Specify the treatment effect equation to be estimated

Given the randomized evaluation design, impacts can in principle be assessed by simple mean comparisons of the outcomes of interest, Y_i, between the two treatment groups and the control group. Yet, to redress minor imbalances that occurred despite the randomization and to

redress a potential bias due to attrition, we use regressions techniques to include appropriate control variables. Hence, we estimate:

$$Y_i = \beta_0 + \beta_1 T_i^M + \beta_2 T_i^C + X_{i0}'\gamma + u_i, \quad (1)$$

where T_i^M and T_i^C are dummy variables that take the value 0 if respondent i was offered a matching or cash grant respectively, and zero otherwise. β_1 and β_2 are the effects of either treatment relative to the control group. Simple Wald tests can be conducted to test whether β_1 and β_2 do significantly differ, i.e. whether one treatment is significantly more effective than another. The vector *X* includes control variables and γ stands for regression coefficients that are associated with these. β_0 is the intercept and shows the control group mean of the outcome Yi. u_i is the error term. Since each entrepreneur has been sampled in one of the nine municipalities, we cluster standard errors at the municipality level. The parameters β_1 and β_2 give intention to treat effects (ITT) as the treatment variables T_i^M and T_i^C do not measure whether a firm actually used (or could already use) the grant but only if it was offered (or promised) a grant.

Treatment heterogeneity will be analysed by interacting the treatment variables with the corresponding characteristics and including them linearly.

It will also be analyzed whether firms in the control group are positively or negatively affected by the presence of treated firms in their area of activity. Assuming that such spill overs lose their importance with rising distance between treatment and control firms, the following regression will be estimated:

$$Y_i = \beta_0 + \beta_1 T_i^M + \beta_2 T_i^C + \sum_j \beta_{3j} \quad NT_{ij}^{M,D} + \sum_j \beta_{4j} \quad NT_{ij}^{C,D} + X_{i0}'\gamma + u_i, \quad (3)$$

where $NT_{ij}^{M,D}$ and $NT_{ij}^{C,D}$ stand for the number of treated firms, matching and cash respectively, in a distance *D* of firm *i*. The distance *D* can be set for example to 500m, 1km, 2km and so on. Alternatively, to better capture forward and backward linkages, it is also possible to consider the number of treated firms in the same sector of activity or further up or down the value chain. The number of treated firms in a certain distance can also be interacted with the treatment status of firm, to allow for spill-over effects that vary by treatment status.

The sampling design also allows to estimate ANCOVA specifications, i.e. to condition all impact estimates on pre-intervention outcomes. This further increases the power. ANCOVA is of greatest value when the autocorrelation of outcome measures across time is low (McKenzie, 2012). Equation (1) transformed into an ANCOVA specification can be written as follows:

$$Y_{i,t} = \beta_0 + \beta_1 T_{i,t}^M + \beta_2 T_{i,t}^C + \theta Y_{i,PRE} + X_{i0}'\gamma + u_i, \quad (4)$$

where $Y_{i,PRE}$ is the pre-intervention (or baseline) level of the outcome. ANCOVA is more efficient than either the simple post estimator shown in Equation (1) or the difference-in-difference estimator, which would be another possible specification with pre and post-intervention data (McKenzie, 2012, Frison and Pocock, 1992).

The list of controls include a dummy that equals one if the firm is already existing (vs. is new), dummies for whether the firm sector is industry or services, with agriculture being the reference, a share of correct recalls by the owner from a digit span memory test, a dummy that equals if the owner is female, the age and number of years of professional experience of the owner, dummies for whether the owner is Christian or Muslim, dummies for whether the owner is of the two largest ethnic groups (Bissa or Mossi), dummies for whether the owner reads and writes French and a local language or reads and writes a local language only, dummies for

whether the owner attained primary, secondary, or other education levels, with the excluded being no education level, the size of the owner household, the number of assets owned, and dummies for whether the owner is a household head, and whether a member in the owner household is a community leader.

6. Robustness to multiple hypothesis testing

To account for the fact that we test impacts across multiple outcomes, we also present confidence intervals that are adjusted using Romano-Wolf stepdown adjusted p-values.

For this we consider the following domains of outcomes:

a. Employment (total employment, paid employment, unpaid employment, days worked) and wages paid
b. Profit, value added, turn over
c. Investment (equipment, construction, land, livestock, inventory)
d. Formalization, management and other business skills (book keeping) incl. capacity to innovate (measured via the first component from a principal component analysis applied on 15 dummies each taking the value 1 if the firm introduced either of 15 listed innovations)

7. Procedures to be used for addressing survey attrition

We run probit regressions to check whether attrition is correlated with any of the treatments or the other observable characteristics we have. To redress a possible bias, we include the control variables above in all regressions. Should attrition turn out to be very high, what we do not expect, we could also consider using matching techniques to constitute a balanced panel at baseline that is continuously observed over time.