

DISCUSSION PAPER SERIES

IZA DP No. 14892

**Short-Term Impacts of Targeted Cash
Grants and Business Development
Services: Experimental Evidence from
Entrepreneurs in Burkina Faso**

Michael Grimm
Sidiki Soubeiga
Michael Weber

NOVEMBER 2021

DISCUSSION PAPER SERIES

IZA DP No. 14892

Short-Term Impacts of Targeted Cash Grants and Business Development Services: Experimental Evidence from Entrepreneurs in Burkina Faso

Michael Grimm

University of Passau and IZA

Sidiki Soubeiga

University of Passau and World Bank

Michael Weber

World Bank and IZA

NOVEMBER 2021

Any opinions expressed in this paper are those of the author(s) and not those of IZA. Research published in this series may include views on policy, but IZA takes no institutional policy positions. The IZA research network is committed to the IZA Guiding Principles of Research Integrity.

The IZA Institute of Labor Economics is an independent economic research institute that conducts research in labor economics and offers evidence-based policy advice on labor market issues. Supported by the Deutsche Post Foundation, IZA runs the world's largest network of economists, whose research aims to provide answers to the global labor market challenges of our time. Our key objective is to build bridges between academic research, policymakers and society.

IZA Discussion Papers often represent preliminary work and are circulated to encourage discussion. Citation of such a paper should account for its provisional character. A revised version may be available directly from the author.

ISSN: 2365-9793

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Short-Term Impacts of Targeted Cash Grants and Business Development Services: Experimental Evidence from Entrepreneurs in Burkina Faso*

Most support programs targeted at small firms in low- and middle-income countries fail to generate transformative effects at a large scale due to bad targeting, too little flexibility, and the limited size of the support, among others. This paper assesses the short-term effects of a randomized targeted Government support program to small and medium-size firms that have been selected based on a business plan competition. One group received large cash grants of up to US\$8,000, with flexible conditions of use. A second group received grants of an equally important size but earmarked to business development services and thus less flexible and with a required own contribution of 20 percent. A third group serves as a control group. All firms operate in agribusiness or related activities in a semi-urban area. An assessment of the short-term impacts shows that beneficiaries of cash grants engage in better business practices, such as formalization and bookkeeping. They also invest more. Yet, this does not translate into higher profits and employment. There is no effect on investment and business practices among beneficiaries of grants for business development services. Yet, both treatment groups show a higher ability to innovate relative to the control group. The results also show that cash grants cushioned the adverse effects of the COVID-19 pandemic. A further round of data collection will soon allow to assess the longer-term effects of both interventions which may differ from the short-term effects analyzed here as both interventions may need time to unfold their full effects.

JEL Classification: D22, O12, Q13

Keywords: firm support programs, cash grants, finance, matching grants, business development services, agribusiness, randomized controlled trial

Corresponding author:

Michael Grimm
Chair of Development Economics
University of Passau
Innstrasse 29
94032 Passau
Germany

E-mail: michael.grimm@uni-passau.de

* We gratefully acknowledge funding from the World Bank's Multi-Donor Trust Fund (MDTF) through the Information for Development Program (InfoDev) and funding from the World Bank's Jobs Umbrella Trust Fund, which is supported by the Governments of Austria, Germany, Italy, Norway, Sweden, and the United Kingdom. Sidiki Soubeiga further thanks the German Academic Exchange Service (DAAD) for financial support. Moreover, the authors thank Martin Maxwell Norman, Mahaman Sani, and Felipe Alexander Dunsch for their support in designing and implementing the program and Renate Hartwig and Marcus Holmlund for very helpful comments and suggestions on this paper. We are grateful to the Maison de l'Entreprise du Burkina Faso (MEBF) for the excellent implementation of the interventions and to Sekou David Koné and Aziz Dao for their excellent assistance on the ground. Finally, the authors thank Innovations for Poverty Action (IPA) for the excellent collaboration in collecting the data and all study and survey respondents for their participation. The original design of the experiment is filed in AEARCTR-0004867. Authors retained full intellectual freedom throughout this process; all errors are our own.

1. Introduction

Many studies have shown that micro and small firms in low- and middle-income countries exhibit high marginal returns to investment, yet most of them also seem to rapidly reach their optimal size and can neither generate very high profits nor much employment (Banerjee and Duflo 2004; Blattman et al. 2016; de Mel, McKenzie, and Woodruff 2008; Fafchamps et al. 2014; Grimm, Krüger, and Lay 2011; Kremer et al. 2011; McKenzie and Woodruff 2006, 2008; Udry and Anagol 2006; Schündeln 2005). Banerjee and Duflo (2011) suggest that growing further requires in many cases adopting more sophisticated technologies and organizational structures which in turn demand entrepreneurial skills, risk-taking behavior, and determination, traits that many firm owners do not have (Banerjee et al. 2019; Karlan, Knight, and Udry 2015). This may explain why many support programs whether they used training, microcredit, cash grants, business development services (BDSs), or combinations of these have not shown transformative effects but instead, at best, had lasting effects on specific subgroups (Banerjee, Karlan, and Zinman 2015; Cho and Honorati 2014; Grimm and Paffhausen 2015). Building on these insights, such programs are increasingly implemented with targeting mechanisms that select those entrepreneurs that can potentially make the most out of such support. Those who lack the necessary skills and entrepreneurial spirit can then instead rather be targeted with social safety nets, hoping that they do find jobs in those firms that flourish.

With this background, this paper assesses the impact of cash grants and matching grants for a sample of entrepreneurs that have been carefully selected using a business plan competition and personal interviews in the Bagré growth pole area which is located in the Centre-Est region of Burkina Faso. In 2018, 2,279 men and women, many already owners of small or medium-size informal or formal firms, submitted a business plan to the Maison de l'Entreprise du Burkina Faso (MEBF), a semipublic local business organization, with a detailed plan on how they would use their grant in case they are chosen. In the course of the competition, which also included a standard business training and personal interviews, 1,200 entrepreneurs were selected based on their performance and their potential to develop their business and ultimately to create jobs and randomly allocated into two treatment groups (cash grants and matching grants) and a control group. Firms could ask for grants in either form for up to US\$8,000. Whereas cash grants can be used for any type of investment including machines, tools, livestock, construction, land, training, and inventories and came with only light procurement rules to prevent misuse outside the business, matching grants can be used only for technical training, require an own contribution of 20 percent, and follow much stricter rules and regulations. Matching grants have already been administered for some time by MEBF but so far are perceived as rather ineffective. A baseline survey was conducted in November/December 2018, disbursements started in August 2019, and a first follow-up survey was conducted in October/November 2020.¹ Both the implementation and accompanying evaluation were co-financed and technically supported by the World Bank.

This paper contributes to the literature on small firm growth in several respects. First, it adds to the literature on targeting business support (Crépon, El Komi, and Osman 2020; Fafchamps and Woodruff 2017; McKenzie 2017), and similar to McKenzie (2017), the grants in this project are much bigger than in most other support programs that targeted small and medium-size firms and have so far been rigorously assessed. Second, it is the first that compares cash grants that can be used for business purposes only with matching grants that are earmarked for technical support. Previous projects rather compared fully fungible cash grants with in-kind grants (de Mel, McKenzie, and Woodruff 2008) or cash grants with loans

¹ This study is registered in the American Economic Association (AEA) Randomized Control Trial (RCT) Registry (AEARCTR-0004867).

(Crépon, El Komi, and Osman 2020; Fiala 2018). Matching grants programs are quite common in developing countries as an instrument to support private sector development. Yet, there are hardly any rigorous impact evaluations (Campos et al. 2012; Hristova and Coste 2016). Third, this project is implemented by local institutions rather than an international NGO or donor organization, hence frictions in changing the implementation partner are avoided when upscaling this program. Fourth, unlike many other programs, the Bagré Growth Pole has focused on agribusiness activities such as livestock breeding and other activities related to agriculture including food processing and manufacturing of agricultural tools and inputs. This is an important sector given the concentration of poor people in rural areas and in agriculture in most parts of Sub-Saharan Africa. Fifth, the project is implemented in a difficult context. According to the Fragile States Index (FSI), Burkina Faso, once considered as one of the most stable countries in West Africa, is classified as the fourth ‘most worsened’ country in 2020.² Given that the literature on fragile countries is sparse and that fragile countries and conflicts seem to increase worldwide, this paper makes an important contribution to that literature.

This paper provides an assessment of the short-term effects and explores how the COVID-19 pandemic affected the intervention and whether the support could cushion some of the adverse effects of the pandemic. A full-fledged analysis with a focus on the medium- and longer-term effects will be conducted once further rounds of data have been collected.

The remainder of this paper is structured as follows. Section 2 gives an overview of the country context and the Bagré Growth Pole Project. Section 3 provides the details of the intervention and explains the intended pathways to impact. Section 4 outlines the evaluation design, including the randomization of applicants into treatment arms. Section 5 presents the specifications used for the estimation of impacts. Section 6 presents the results. Section 7 concludes.

2. Country context and background of the Bagré Growth Pole Project

Burkina Faso’s private sector is small and dominated by informal microenterprises, with only a small number of formal small and medium enterprises (SMEs) located in urban areas. Informal nonagricultural enterprises make up about 88 percent of the private sector and they account for roughly 60 percent of total employment. Informal firms are mainly present in commercial activities (58 percent), followed by other services (21 percent) and manufacturing (20 percent). Half of all informal enterprises have no employees or not more than one employee. One-third of them are not older than a year. Informal firms more than formal firms are constrained by low productivity, a low skill base, limited access to credit, and high input costs (especially electricity) (Weber 2018). Addressing these gaps and especially increasing the productivity of informal and formal businesses can play a critical role in expanding the private sector to better contribute to inclusive growth and job creation in Burkina Faso. The national development plan (*Plan National de Développement Economique et Social*, PNDES) defines private sector development as a key element that has to push the process of structural transformation to substantially contribute to job creation, diversification, and increased exports.

The ‘Bagré Growth Pole Project’ represents one of Burkina Faso’s major private sector operations.³ It supports entrepreneurship in agriculture and agri-processing, investments, and the establishment of an

² <https://fragilestatesindex.org/>.

³ You can access the implementation completion report of the project through the following link:

<https://documents1.worldbank.org/curated/en/909471626364231329/pdf/Burkina-Faso-Bagre-Growth-Pole-Project.pdf>

industrial park in the project area. The ultimate goal of the project is to deliver jobs. Especially youth and women are expected to benefit from the new employment opportunities. Since 2012, the project has offered matching grants to agricultural and nonagricultural firms of any size with an investment stake in the Bagré area including smallholders and professional associations as well as private providers of BDSs.⁴ The rationale of the matching grants scheme is to direct public subsidies to firms that need such services but are unable to cover the related costs while ensuring an adequate use of those subsidies.

Yet, a review of the program revealed that the results of the matching grants program were mixed, basically for three reasons: (a) long and complex selection procedures; (b) insufficient monitoring of grant recipients; and (c) procurement rules that prevent beneficiaries from using the grants to hire workers, purchase equipment, and acquire land. Therefore, the World Bank, MEBF, and Bagrépole sought to pilot cash grants as an alternative support instrument accompanied by a rigorous impact evaluation to shed light on the effectiveness of both instruments. The difference between both is that the matching grants are conditional on an own contribution, they require compliance with the World Bank's procurement rules, and they can only be used for capacity-building services. The cash grants neither require an own contribution nor a full compliance with the World Bank's procurement rules nor are they limited to the usage for services only. The cash grants can be used for technical assistance, equipment, or inventory. The impact assessment will not be able to disentangle the role of each single feature but rather assess the effects of each intervention package on firm performance and job creation along with their respective procedures of implementation.

The matching grants program is managed by MEBF in collaboration with Bagrépole, which is the managing and implementing agency of the entire Bagré Growth Pole Project. Bagrépole is a public-private company created by the Government. The Growth Pole Project covers nine municipalities including the municipality of Bagré. Bagré has been identified as a growth pole because of its high potential for agribusiness, horticulture, livestock, fish farming, and staple crops production. The Bagré Development Zone was initiated in 1989 with the construction of a hydropower dam. The development zone covers about 500,000 ha around the lake created by the dam, with 57,800 ha of irrigable land downstream of the dam called the zone de concentration. This zone constitutes the project area. Bagré is in the Boulgou Province which is in the Centre-Est region. The pole is close to large markets in Ghana, Togo, and Niger through adjacent trade corridors. The impact evaluation will cover all nine municipalities with a total population estimated at about 687,065 inhabitants, of which roughly 37,849 are in the Bagré municipality. Six of these municipalities are rural, and three are urban.

3. The intervention and the pathways to impact

3.1 Selection of beneficiaries and rollout

The intervention was planned over six phases: (a) advertisement of the program and communication of eligibility criteria, (b) collection of applications, (c) selection of applicants, (d) training of all selected

⁴ BDSs can provide a large array of services to businesses, smallholders, and associations, including specialized or general trainings such as training of livestock fattening, pisciculture, leadership and governance of associations, sewing, quality management, marketing, use of specific equipment, warrantee-system (inventory credit), safe use of pesticides, business management, and so on.

applicants, (e) randomization of selected applicants into treatment arms through a public lottery, and (f) grant disbursements. In the following paragraphs, we will describe each phase in detail.

Phase 1 - Advertisement of the program and communication of eligibility criteria. In January–March 2018, MEBF reached out to micro, small, and medium-size agricultural and nonagricultural enterprises (MSMEs) in the study area by means of public information sessions, the local radio channels, and meetings with local authorities and potential applicants. Prospective applicants were informed about the program details and the application requirements.

Phase 2 - Collection of applications. 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 fill out the application form which had 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 also had 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 annual 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 comprehensively define 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 submitted to MEBF.

Phase 3 - Selection of applicants. By May 2018, MEBF staff and staff of Bagrépole short-listed 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.

Phase 4 - Training of all selected applicants. MEBF invited all the preselected 1,612 applicants for a training based on a simplified module of the ‘Start and Improve Your Business Training’ (SIYB) developed by the International Labour Organization (ILO). As in McKenzie (2017), the intention of the training was to provide all applicants with the basics of business management, including accounting and financial and business planning, to help grant recipients finalize and implement their business plan. Of the preselected applicants, 1,575 attended the training. Using another round of face-to-face interviews, by August 2018, MEBF then selected, again based on performance and quality of the proposed project, a final list of 1,300 applicants, of these 1,300 the top 1,200 ranked firms were then selected for the lottery and the other 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. Power is discussed in more detail in Section 4.

Phase 5 - Randomization of selected applicants into treatment arms through a public lottery. In April and May 2019, with technical support from the World Bank, MEBF 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. The procedure is explained in detail in Section 4. Out of the 1,200 entrepreneurs, 26 did not participate in the lottery and hence were replaced by other candidates selected during a follow-up lottery to complete the sample.

Phase 6 - Grant disbursement. Each selected beneficiary of cash or matching grant could receive up to US\$8,000. The exact amount had to be specified and justified in the business plan. Following the lottery, grant agreements between MEBF and the beneficiaries were signed in August and September 2019. The disbursement of both types of grants was delayed and in general was relatively slow. Delays were further exacerbated by the COVID-19 pandemic as the country closed its airport and borders, imposed a curfew, and put the capital city in quarantine. Yet, by July 2020, all beneficiaries of cash grants (except two) had received their funds into their account and approval to start spending on their grants. By October 2020, just before the survey started, about 98 percent of all beneficiaries had withdrawn at least a part of their grant from their account. At that time, about 55 percent of the total amount granted to firms was withdrawn. The disbursement of matching grants was even more delayed, partly also because of COVID-19 as the pandemic made face-to-face meetings and hence any type of training even more difficult and because matching grants required more action by MEBF staff than cash grants. Yet, MEBF even organized a public fair to help entrepreneurs identify suppliers of BDSs with which they could use their grants to procure needed services.⁵ By July 2020, 35 percent of all beneficiaries of matching grants had made at least a first request and 18 percent had received approval. These shares increased to 89 percent and 82 percent by October 2020, respectively. Obviously, this difference in the speed of rollout will affect the comparability between the cash grant and the matching grant treatment. Nevertheless, matching grants are mostly used for services that are delivered in a shorter time than the time taken by most beneficiaries of cash grants to invest in equipment and infrastructure. Hence, despite the delayed start of matching grants disbursements, this treatment may yield rapid impacts on intermediate outcomes such as innovations, business practices, and formalization, which can be compared with the impact of cash grants.⁶ The entire timeline of the intervention is shown in Figure 1.

Apart from the fact that matching grants required a 20 percent contribution of the beneficiary and could only be used for BDSs whereas cash grants could be used for any type of service, investment, or inventory, both types of support also strongly differed in their disbursement procedures. Beneficiaries of matching grants had to go through five steps. First, MEBF staff supported the beneficiaries to prepare the disbursement requests; second, together they identified a supplier of the BDS that the beneficiary requested; third, the beneficiary had to prove to MEBF that he or she can contribute 20 percent of the total cost of the service;⁷ fourth, MEBF validated the disbursement, and fifth, the beneficiary received the requested service and was reimbursed for 80 percent of the cost in advance. Beneficiaries were allowed

⁵ The fair was public and targeted both matching and cash grant beneficiaries as well as BDS and equipment providers.

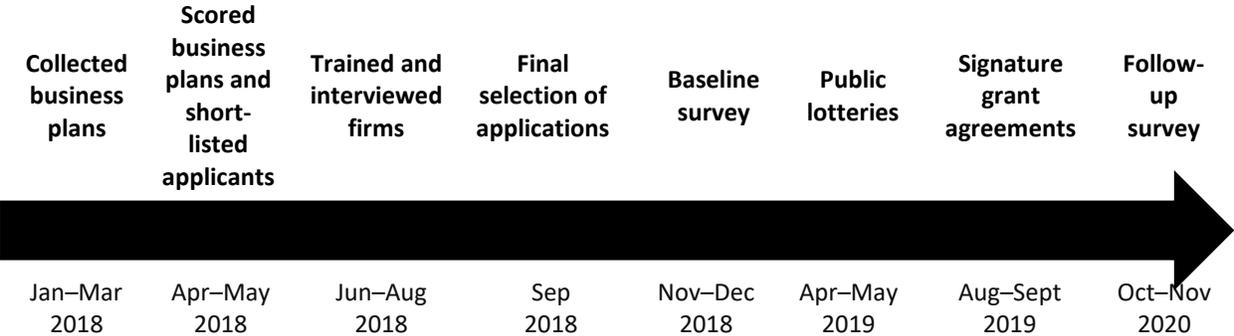
⁶ Once the disbursement is approved, most beneficiaries of matching grants enrolled in training lasting less than one month or participated in commercial fairs lasting less than one week. As such, matching grant beneficiaries improve their knowledge in a shorter time, and given that most of the firms are small and produce unsophisticated products, the treatment may yield rapid impacts on some outcomes shortly after disbursement started. In contrast, most beneficiaries of cash grants invested in equipment and infrastructure and by the program procurement rules had to go through a third-party service provider to make their investments, which may take time. Hence, apart from cash beneficiaries who used part of their grants for BDSs, the treatment may have a slower impact on some outcomes as compared with the impact of matching grants.

⁷ All firms were informed that a 20 percent own contribution will be required if they are selected as a matching grant recipient. The scoring and interviews checked the interests and soundness of business ideas of all short-listed candidates, though whether a specific firm would be able to pay the contribution was not checked ex ante for any firm but ex post for matching grant recipients only.

to submit several disbursement requests up to the total amount requested in the business plan. Cash beneficiaries were supported to open accounts with accredited banks and microfinance institutions and the grant was then paid into that account up front. The disbursement procedure then differed in whether it was the first request or a follow-up request. To start the disbursement with an amount less than 15 percent of the grant, the beneficiary withdrew the amount directly from the financial account without approval from MEBF. To withdraw an amount exceeding 15 percent of the grant, the beneficiary had to request approval from MEBF. Starting from the second disbursement, four tranches of disbursement rates were defined. Within each tranche, the beneficiary could disburse many times, but when disbursing in the next tranche, the beneficiary had to again request approval from MEBF to ensure sufficient control. Consultants hired by MEBF also supported these beneficiaries to prepare their disbursement requests, and MEBF and the beneficiaries' financial institutions coordinated to check whether the disbursements were done according to the rules.

Beneficiaries were also encouraged to formalize, but they were not forced. Yet, if they did not formalize, they had to name an underwriter who had to confirm to MEBF that they would guarantee for the beneficiary.

Figure 1. Timeline of the program's main activities



Source: Own representation.

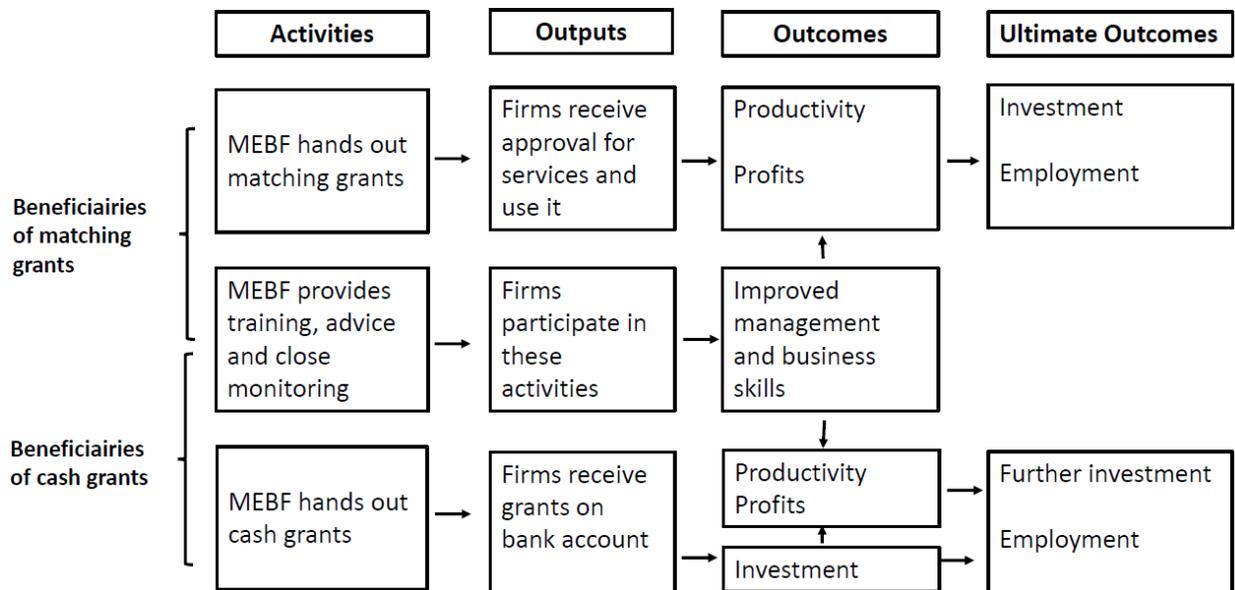
3.2 Pathways to impact

The overall goal of both interventions was to identify dynamic, growth-oriented, capital-constrained MSMEs to supply these firms with capacity-building services or alternatively capital to address shortages in skills and finance that paralyze their productivity and inhibit their growth. Hence, both interventions were intended to enhance productivity and profits. The ultimate goal of the interventions was an increase in employment in the intervention area and lasting effects on investment. The intended pathway leading to these goals is illustrated in Figure 2. Yet, BDSs and investments in capital could of course also lead to reduction of labor demand if production processes become more efficient and if labor is substituted by capital. The latter could especially happen if firms previously had to be labor intensive due to imperfect capital markets.

The rationale behind the matching grants intervention is to support recipient firms in selecting trainers or providers of BDSs that can address their needs. Firms may want to improve their business skills, optimize

their organization or production process by adopting new technologies, or even invent new products and services.⁸ This assumes a close collaboration between the implementing agency and the recipient firm to ensure that the firm does use the grant for a business-related service and not for any other purpose. On the demand side, earmarking a grant to specific services and asking for an own contribution as opposed to giving a cash grant directly to a business owner may increase commitment and hence address problems of self-control; it may also address problems of mental accounting and help protect the funds from requests from the family (Fafchamps et al. 2014). On the supply side, matching grant programs also come with huge administrative work due to the close monitoring and strict procurement rules as well as making sure that the recipient pays the 20 percent contribution; overall, this may lower the cost-effectiveness of such programs (Hristova and Coste 2016).

Figure 2. Pathways to impact



Source: Own representation.

Cash grants, in contrast, do not require co-funding and, in the case of the program under evaluation, give recipients more flexibility to allocate the grant according to their needs. As such, cash grants can address not only the firms' technical and management constraints but also their capital constraints more broadly. Moreover, cash grants come with a relatively light administrative burden and are thus less time-consuming for the beneficiary and maybe more cost-effective for the provider. They are also paid directly into an account of the recipient to create commitment and to ' earmark' the grant though there is obviously also a risk that recipients spend the cash on items unrelated to the business. Yet, in our case, as we described above, the monitoring is still relatively strict as beneficiaries need repeatedly approval for the expenses they make, so we anticipate misuse or fraud to be relatively low. This also means that the

⁸ Entrepreneurs get, for example, training in food processing, specific techniques of tailoring, breeding and livestock farming, or market gardening.

administrative costs are probably higher than in other cash grants programs that are discussed in the literature.

Both treatments should also have improved entrepreneurs' business practices, especially given the close monitoring and advisory role MEBF takes. Moreover, having won in the lottery may also have increased entrepreneurs' confidence and improved their attitudes toward their business (McKenzie 2017). It may also send positive signals to financial institutions and through this channel relax their credit constraint.

Both matching and cash grants to firms usually also raise concerns regarding their additionality, that is, whether firms receiving grants would not have undertaken the subsidized activities in any case, even without the subsidy. This would be expected to happen if firms were not credit constrained (McKenzie 2017). Yet, we believe this is not true for most entrepreneurs and inconsistent with what firms report in terms of capital needs and access to credit.

4. Randomization, balance tests, and attrition

4.1 Randomization

Earlier, we explained how MEBF attracted firms and prospective firms into the business plan competition and selected the most promising ones for participation in the public lottery. In this subsection, we provide more details about the stratified randomization.

In each municipality, we stratified applicants according to the size of the financial support they requested and justified in their business plan. 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, and 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 (US\$)	Cash group (#)	Matching group (#)	Control group (#)	Total (#)
1	≤ 1,999	68	68	70	206
2	2,000–2,999	153	154	153	460
3	3,000–3,999	106	106	105	317
4	4,000–4,999	42	41	41	124
5	5,000–5,999	12	12	12	36
6	6,000–6,999	5	5	7	17
7	7,000–8,000	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, which is substantially lower than the maximum grant that could be granted. Few firms requested US\$5,000 and more. This is interesting information regarding the sustainability of such a

program. The lowest grant is US\$628, and the highest is US\$11,659. By mistake, nine entrepreneurs in Block 7, of which four were cash grants and five were matching grants beneficiaries, were granted amounts larger than the ceiling of US\$8,000.⁹ As intended, the distributions of the grant amounts are nearly identical across both treatment groups.

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

Group	Business (#)	Mean (US\$)	Median (US\$)	Standard deviation (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, that is, in each stratum, the number of winners is proportional to the number of applicants.

The sample size of 1,200 entrepreneurs, that is, 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 percent power. Effects of 0.20 standard deviations and less are considered 'small' in the literature. If the calculations account in addition for an attrition rate of 20%, the minimum detectable effect size increases to about 0.22 standard deviations. If a power of 90 percent is imposed, the measurable effects increase to 0.23 standard deviations and 0.26 standard deviations, respectively. Given, that we have baseline data, at least for the existing firms, we can also estimate Analysis of Covariance specifications (ANCOVA) and use a large set of covariates which further adds to power.

On top of the quantitative surveys, we also used qualitative methods to probe into contextual factors, especially focus group discussions with firm owners and in-depth interviews with staff of MEBF and Bagrépole and also some grant beneficiaries. At baseline, these interviews were conducted 12 months after the quantitative survey and at midline in the same month as the quantitative survey. Moreover, we can draw on monitoring data on program demand, delivery of grants and services, actual program take-up, and process information. All sources taken together provide a rich data set complementing the survey data which can help understand the mechanisms linking program inputs to outputs and ultimate impacts and, more generally, can help validate our results.

4.2 Balance tests

Table A.1 and Table A.2 in the appendix show baseline characteristics and pairwise balance tests across all three groups. Table A.1 focuses on characteristics of the entrepreneur and Table A.2 on characteristics of the firm. Table A.1 shows that in our sample, entrepreneurs are on average about 42 years old, a bit less than half are women, most are married, about 55 percent are the head of their household and have about five to six children, a bit more than 20 percent have completed secondary school and have on

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

average 10 years of professional experience, and more than 90 percent have already completed a business training either in 2017 or in 2018. Almost all characteristics are balanced across the three groups. There are only some minor differences for religious affiliation, age, and gender. But even these differences are relatively small in absolute terms. Their firms exist on average for eight years; more than 50 percent are in agriculture including livestock, 20 percent are in services, and about 15 percent are in manufacturing. They have on average a capital stock of US\$3,500–6,500 and generate profits of about US\$500 over the six months preceding the baseline survey, yielding an annual return to capital ratio of about 15–30 percent and a profit margin of about 30–40 percent, yet profits may include a substantial part of the owner’s labor income. They employ on average 2.5 workers. About 40–45 percent keep books. Almost all characteristics are balanced across the three groups, yet there is substantial variation for the reported capital stock, driven by some extreme values, though even these differences are statistically not significant given the high variance of the distribution. Despite the overall very good balance, we will, for robustness reasons, include in all regressions a large set of controls to redress any potential bias. It will also increase the precision with which we estimate the impacts.

Since the sample comprises already existing firms and new firms and for many outcomes it makes sense to analyze impacts separately for these two types, we also did balance tests for these two subgroups. They are shown in the baseline report (Grimm, Soubeiga, and Weber 2020). The balance tests suggest that most observable characteristics are also balanced for these subgroups and hence already existing and new businesses are quite evenly distributed across the two treatment groups and the control group. Yet, in general, it must be noted that any subgroup analysis, whether by firm status, or by gender or grant size, is subject to lower power. Hence, at this stage, we mainly focus on the sample of all firms and the sample of already existing firms and leave a further analysis of heterogeneity for later stages when more data are available.

4.3 *Attrition*

Of the 1,200 entrepreneurs, 92.7 percent responded to the follow-up survey, implying an attrition rate of 7.3 percent. In our case, 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 traveling or the like 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 percent), followed by the rate in the control group (7.5 percent), and the group of cash grant beneficiaries (5.3 percent). Yet, if we run regressions of attrition on respondents’ 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. According to MEBF, no one dropped out because of the inability to pay the 20 percent contribution as they were allowed to find an arrangement with the BDS provider to somewhat delay the payment of their own contribution if that was needed, but could already receive the support service in the meantime.

5. Empirical specifications

Given the randomized evaluation design, impacts can in principle be assessed by simple mean comparisons of the outcomes of interest, Y_{ik} , 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

$$Y_{ik} = \beta_0 + \beta_1 T_{ik}^M + \beta_2 T_{ik}^C + X'_{ik0} \gamma + \delta_k + u_{ki}, \quad (1)$$

where T_{ik}^M and T_{ik}^C are dummy variables that take the value 1 if respondent i of block k was offered a matching or cash grant, respectively, and 0 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, that is, whether one treatment is significantly more effective than another. The vector X includes control variables observed at baseline 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 Y_{ik} . δ_k are block fixed-effects. 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 (ITT) effects as the treatment variables T_{ik}^M and T_{ik}^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. Yet, given the substantial delays in the disbursement of the matching grants and hence the limited comparability of matching grant beneficiaries with the cash grant beneficiaries at this stage of the intervention, we must interpret the effects associated with the matching grants rather as an anticipation effect than a real treatment effect.

Of particular interest is of course also the treatment heterogeneity, for example, with respect to the size of the grant, gender, sector of activity, or the initial capital stock. In this case, the treatment effects can be interacted with this characteristic, for example, the block category, B_{ij} , used for the randomization. As discussed above, the sample size at hand will obviously put limits on these analyses, so whether different effects can be associated with the different grant sizes, for example, will again depend on the general size of the effects and how large the differences across the blocks really are.

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. As mentioned above, such effects may occur if, for example, firms in the control group have backward or forward links with treated firms because they now receive better or cheaper intermediate inputs or can sell more final goods to them for their production. Negative effects could occur if control group firms are direct competitors of the treated firms and lose market shares because of the increased productivity differential. Assuming that such spillovers lose their importance with rising distance between treatment and control firms, the following regression can be estimated:

$$Y_{ik} = \beta_0 + \beta_1 T_{ik}^M + \beta_2 T_{ik}^C + \sum_j \beta_{3j} NT_{ikj}^{M,D} + \sum_j \beta_{4j} NT_{ikj}^{C,D} + X'_{ik} \gamma + \delta_k + u_{ik}, \quad (2)$$

where $NT_{ikj}^{M,D}$ and $NT_{ikj}^{C,D}$ stand for the number of treated firms j , matching and cash, respectively, in a distance D of firm i of block k . The distance D can be set, for example, to 500 m, 1 km, 2 km, 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 the firm, to allow for spillover effects that vary by treatment status.

The sampling design also allows to estimate ANCOVA specifications, that is, 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:

$$Y_{ik,t} = \beta_0 + \beta_1 T_{ik,t}^M + \beta_2 T_{ik,t}^C + \theta Y_{ik,PRE} + X'_{ik0} \gamma + \delta_k + u_{ik}, \quad (3)$$

where $Y_{ik,PRE}$ 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 (Frison and Pocock 1992; McKenzie 2012).

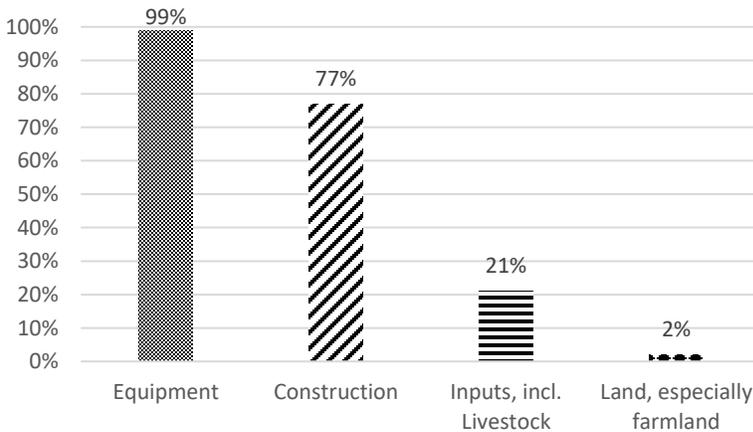
Depending on the nature of the outcome variable in each case and its distribution, we also use in some cases nonlinear models and where useful transform outcome variables with a simple log function or the inverse hyperbolic sine (ihs) function (Burbidge, Magee, and Robb 1988).

6. Results

6.1 Use of grants

Before we analyze the impacts of the intervention, we focus on the use of the two types of grants according to the entrepreneurs' business plans they had submitted to MEBF.

Figure 3. Planned investments and purchases (without BDSs)



Note: Equipment refers to the purchase of machines, production materials, and so on; Inputs refer to the purchase of livestock used in production (for example, donkey), livestock for breeding, and other production inputs; construction work refers to the construction of a warehouse, wall, hangar, cowshed, piggery, pond, and so on; and land refers to the acquisition of farmland, developed land, and so on.

Source: Business Plans.

These plans do not allow to distinguish what each entrepreneur would do in either treatment; they rather include both their potential needs in BDSs and their needs for equipment, inventory, livestock, and intermediate inputs. An entrepreneur drawn into the cash grant group could of course also spend the entire grant or a part of it on BDSs. Figure 3 shows the share of firms who have defined a need for the respective category. We do not include the share for BDSs as all firms had defined a need for that category in case they would be drawn for a matching grant.

6.2 *Effects on investment*

In this subsection, we analyze whether treated firms show higher investment than control firms. It is important to keep in mind that for beneficiaries of matching grants, any effect of investment would be a second-round effect as the support is earmarked for BDSs and cannot be used for equipment, construction, or land.

Further, it is important to note that the survey question on investment undertaken over the six months preceding the survey, that is, roughly April–September 2020, and, to avoid any Hawthorne effects, did not ask specifically about investment financed with the grant received. Figure 4 shows that according to the monitoring data, 43.5 percent of the beneficiaries of grants, or 174 firms, have withdrawn cash from their account. Nearly 55 percent had done so already between January and March 2020. These 174 firms that withdrew cash between April and September 2020 withdrew on average US\$1,955, or 25 percent of the total amount granted. The total sum already withdrawn by the time the survey started corresponds to 55 percent of the total amount granted. This is shown in Figure 5.

Overall, there are several reasons why even for beneficiaries of cash grants, the impact may deviate from the pure grants that were handed out. First, many grants had not yet been (fully) invested or were spent on inventory or BDSs rather than on equipment, construction, or land. Second, some had already used at least a part of their grant before April 2020. Third, the grants may have simply substituted for own resources that would have been used for investment anyway. Fourth, those who did invest may have matched their cash grants with additional resources from the firm and hence report investments much larger than the cash injection they got. Moreover, firms in the control group could have lowered their investment in case they expect to receive support in the future or to the contrary have increased their investment to withstand a possibly increased competition with treated firms.

Again, for beneficiaries of matching grants, we do not expect direct effects on investment, just indirect effects that are, for instance, triggered by an increased productivity owing to the BDSs. Figure 4 shows that by the time the survey started, 88 percent had made a request to MEBF and 82 percent had received approval and hence may have already benefited from support, yet this is probably too late, relative to the survey period, to already expect second-order effects on investment.

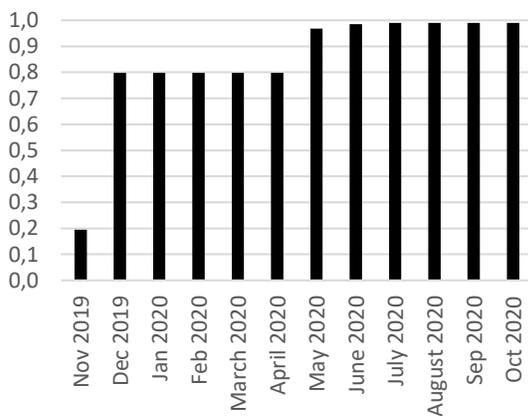
We use the log of the total amount of capital invested in equipment such as machines, furniture, tools, construction, inputs, and land in millions of XOF as an outcome and estimate simple ITT effects, that is, we are using the treatment status as the treatment and not the actual amount transferred to the firm. We use both simple ordinary least squares (OLS) and the ANCOVA specification. To avoid a bias through outliers, we trimmed the data by removing the top 1 percent of firms in the distribution.

The OLS specification shows a significant positive impact for the group of cash beneficiaries. The results are very similar if we use the ANCOVA specification, that is, control for baseline investment values. There

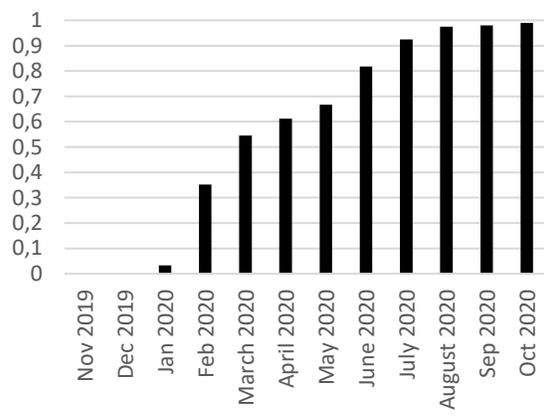
is hardly a difference in the effect size whether we use the sample of all firms controlling for their status at baseline—already existing or new—or whether we use the sample of already existing firms at baseline only. The estimated impact on investment corresponds to a modest increase of about 16 percent. In terms of shares, in the group of cash beneficiaries, 35 percent reported some investment (on average XOF 0.882 million or US\$1,600), whereas this share was only at 19 percent (on average XOF 0.154 million or US\$280) in the control group. So, clearly, more firms in the group of cash beneficiaries invested and they invested on average much higher amounts. It is interesting to see that the average of US\$1,600 is not far from the average cash withdrawn reported in the monitoring data for the same firms over the same period. As expected, we do not yet find any effects for beneficiaries of matching grants.

Figure 4. Rollout of cash and matching grants

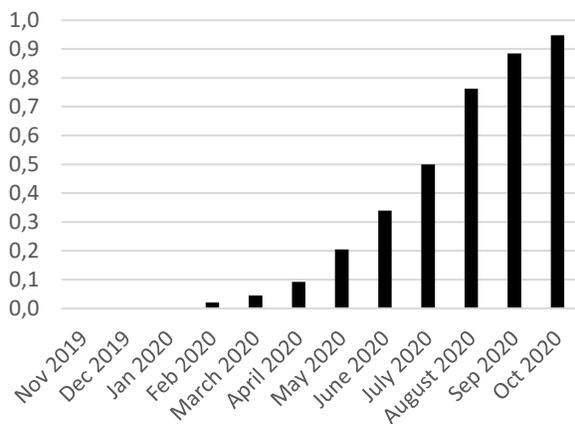
(a) Share of beneficiaries of cash grants who received transfer into their account by month



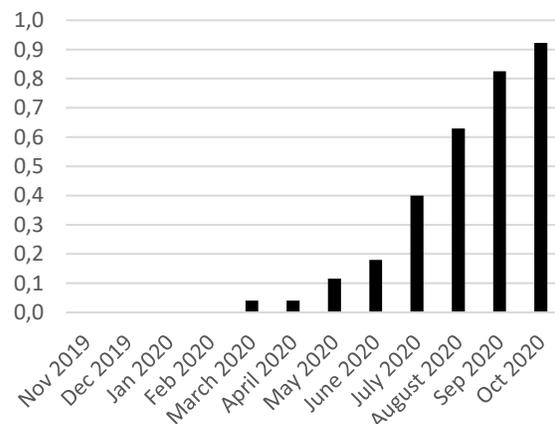
(b) Share of beneficiaries of cash grants who withdrew cash from their account by month



(c) Share of beneficiaries of matching grants who filed a request to MEBF by month



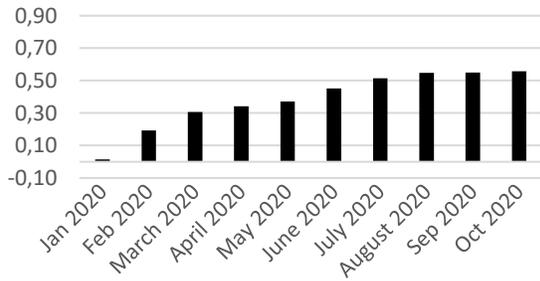
(d) Share of beneficiaries of matching grants who received approval for their request from MEBF by month



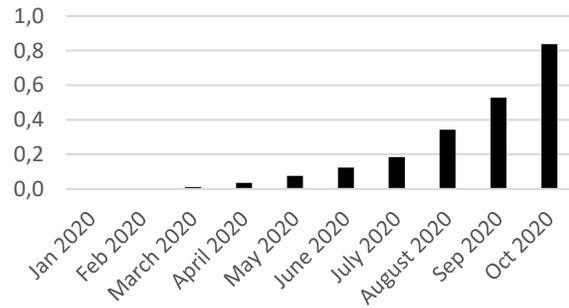
Source: Monitoring data.

Figure 5. Cumulative share of total grants used by month

(a) Cumulative share of total amount of cash grants withdrawn by month



(b) Cumulative share of total amount of matching grants approved by month



Source: Monitoring data.

We also plotted (unconditional) post-treatment cumulative distribution functions (CDFs) of investment by treatment group (Figure A.1, appendix). The graph shows that among those who invested, investment is substantially higher in the group of cash grant recipients compared to the two other groups. Their distribution is clearly to the right of the two other groups. The distributions of the matching grant recipients and the control group do not show a noticeable difference.

We do not find any systematic difference in the effect size within cash beneficiaries, whether we look at gender, the sector of activity, initial capital stock, or the size of the support requested (results not presented in Table 3). Yet, this also has to do with the fact that we are somewhat limited in power with the sample size at hand. Some of the differences across groups might simply be too small to be detectable.

Table 3. Intention-to-treat effects on total investment in physical capital and land (past 6 months)

	Total investment (log)	
	All	Existing
Post		
Cash	0.160*** (0.014)	0.155*** (0.013)
Matching	0.004 (0.010)	0.008 (0.009)
ANCOVA		
Cash	0.151*** (0.011)	0.151*** (0.011)
Matching	0.010 (0.010)	0.010 (0.010)
Control group mean (in XOF, millions), all	0.029	0.027
Standard deviation	0.133	0.122
Control group mean (in XOF, millions), investment>0	0.154	0.273
Standard deviation	0.145	0.252
Standard error (S.E.) clustered	Municipality	Municipality
Controls	Yes	Yes
Block fixed-effects	Yes	Yes
R2 (Post)	0.142	0.128
R2 (ANCOVA)	0.122	0.122
N (Post)	1,032	960
N (ANCOVA)	952	952

Note: ***p < .01, **p < .05, *p < .1. Investment has been trimmed at the 99th percentile. All control variables come from the baseline survey. The list of controls include a dummy that equals one if the firm is already existing (vs new); dummies for whether the firm's 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 one 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. The control group mean refers to the follow-up survey. The detailed regression results are provided in Table A.3 in the appendix.

6.3 *Effects on profits and turnover*

For both, beneficiaries of cash grants and beneficiaries of matching grants, we find negative treatment effects on turnover (total revenues on products and services over the last six months) and profits (total profits on products and services over the last six months) (Table 4 and Figure A.2 and Figure A.3). The unconditional posttreatment CDFs show that the distributions are clearly different between the 70th and 90th percentile and between the 80th and 95th percentile, respectively. Yet, for recipients of matching grants, the negative treatment effects turn insignificant if we use the ANCOVA specification. For treated firms that already existed at baseline, the reduction of profits is in the order of XOF 60,000–70,000 (or US\$108–126). For turnover, the estimates are less precise and for the subgroups of already existing firms in case of the ANCOVA specification, they are insignificant. In the simple OLS specification, the effect has roughly a size of –7 percent (or XOF 97,000, that is, US\$175). The reduction in revenues and profits may surprise, especially because of the positive effect on investment, but beneficiaries may need time to transform their business, for example, to reach out to new customers, develop new products, and learn to deal with new technologies and forms of organization. It might also be due to increased competition, which we investigate further when we explore spillover effects. In a comparable experiment with cash grants in Nigeria, McKenzie (2017) showed that the impacts on profits and sales were insignificant in the first year for all retained indicators except one but reached significant and economically sizeable effects in the second and third years after the rollout. Of course, it could also be that the COVID-19 pandemic and the associated decline in demand made it difficult for firms to market a higher output. The possible consequences of COVID-19 will be analyzed in more detail later.

If we analyze heterogenous effects by gender, initial capital stock, size of the grant, and sector, we do not detect any special pattern. Almost all interactions are insignificant. As noted earlier, this also has to do with the limited power we have to analyze such interactions. We find that only treated firms in the service sector seem to get around negative short-term effects on their profits. This may imply that transformations in the business model are faster, for example, in this sector compared to the manufacturing sector.

Table 4. Intention-to-treat effects on profits (total profits on products and services) and turnover (total revenues on products and services), past 6 months

	Profits		Turnover (log)	
	All	Existing	All	Existing
Post				
Cash	-0.068** (0.026)	-0.073** (0.029)	-0.077* (0.035)	-0.067 (0.037)
Matching	-0.059** (0.021)	-0.060** (0.023)	-0.048 (0.029)	-0.045 (0.033)
ANCOVA				
Cash	-0.057** (0.023)	-0.057** (0.023)	-0.058 (0.034)	-0.058 (0.034)
Matching	-0.045 (0.028)	-0.045 (0.028)	-0.013 (0.035)	-0.013 (0.035)
Control group mean (in XOF, millions)	0.254	0.260	1.374	1.381
Standard deviation	0.386	0.394	2.078	2.107
S.E. clustered	Municipality	Municipality	Municipality	Municipality
Controls	Yes	Yes	Yes	Yes
Block fixed-effects	Yes	Yes	Yes	Yes
R2 (Post)	0.132	0.138	0.166	0.167
R2 (ANCOVA)	0.170	0.170	0.297	0.297
N (Post)	1,024	952	1,019	946
N (ANCOVA)	940	940	931	931

Note: ***p < .01, **p < .05, *p < .1. Profits are in XOF, millions (XOF 1 million = US\$1,820), and turnover is in log. Profit and turnover have been trimmed at the 99th percentile. The lower number of observations compared to Table 3 is due to missing values for profits and turnover. All control variables come from the baseline survey. The list of controls include a dummy that equals one if the firm is already existing (vs 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 one 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. The control group mean refers to the follow-up survey. The detailed regression results are provided in Table A.4 and Table A.5 in the appendix.

6.4 *Effects on employment*

One of the ultimate goals that shall be achieved with the interventions under study is employment creation. Therefore, we included a whole battery of outcome indicators related to jobs and work in our surveys. Table 5 shows the results for the total number of employees, the number of paid wage workers, the number of unpaid workers and the number of days worked by the employees in a normal week. Overall, we do not find any significant effects on employment-related outcomes, neither for beneficiaries of cash grants nor for beneficiaries of matching grants, and whether we look at all firms or at already existing firms only, if at all, it seems that the short-term effects are slightly negative. The only exception is 'days worked'; it seems that beneficiaries of cash grants increased the number of days they are working per week by 0.16 from 5.66 to 5.82 or roughly 3 percent. This is a relatively small effect. Digging deeper reveals that treated firms seem to use less household helpers and casual work. This could hint to a higher capital intensity in these firms. Yet, more generally, we believe that the time that elapsed between the investments that have been undertaken and the survey is still too short to expect substantial employment effects; if employment creation materializes, it will certainly take more time. Here as well, it must be assumed that the COVID-19 pandemic did not offer favorable conditions for an expansion of jobs.

Table 5. Intention-to-treat effects on total, paid wage, and unpaid employments and days worked

	Total employment		Paid wage employment		Unpaid employment		Days worked	
	All	Existing	All	Existing	All	Existing	All	Existing
Post								
Cash	-0.242*	-0.200	-0.043	0.008	-0.063	-0.043	0.160**	0.081
	(0.112)	(0.134)	(0.094)	(0.108)	(0.089)	(0.097)	(0.058)	(0.051)
Matching	-0.053	-0.074	0.013	0.010	0.141	0.146	-0.145	-0.188*
	(0.232)	(0.262)	(0.114)	(0.121)	(0.084)	(0.094)	(0.097)	(0.088)
ANCOVA								
Cash	-0.091	-0.091	0.010	0.010	-0.039	-0.039		
	(0.149)	(0.149)	(0.088)	(0.088)	(0.098)	(0.098)		
Matching	0.022	0.022	0.052	0.052	0.135	0.135		
	(0.286)	(0.286)	(0.126)	(0.126)	(0.092)	(0.092)		
Control group mean	2.271	2.302	1.106	1.106	0.507	0.506	5.663	5.707
Standard deviation	2.290	2.327	1.881	1.908	1.236	1.246	1.585	1.559
S.E. clustered	Municipality	Municipality	Municipality	Municipality	Municipality	Municipality	Municipality	Municipality
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Block fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R2 (Post)	0.093	0.096	0.097	0.100	0.032	0.030	0.078	0.080
R2 (ANCOVA)	0.226	0.226	0.243	0.243	0.064	0.064		
N (Post)	1,067	989	1,042	969	1,042	969	851	796
N (ANCOVA)	989	989	969	969	969	969		

Note: ***p < .01, **p < .05, *p < .1. The dependent variables are the total number of employees, the number of paid wage employees, the number of unpaid employees, and the average number of days worked in a week (only available at midline). All control variables come from the baseline survey. The list of controls include a dummy that equals one if the firm is already existing (vs 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 one 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. The control group mean refers to the follow-up survey. The detailed regression results are provided in Table A.6 and Table A.7 in the appendix.

6.5 *Effects on business skills, business practices, banking, and formalization*

We also included in our survey a whole range of outcome indicators that relate to business skills and business practices including banking and formalization. Although all firms in our sample received SIYB Training developed by the ILO, treated firms more than untreated firms may have felt the necessity to adapt their practices with the upcoming investment opportunity, but it could of course also be that untreated firms tried to make up for the difference and hence were more inclined to adapt. For those firms that invested their grant on technical support, especially the beneficiaries of matching grants, new skills and practices may also have been transmitted together with the technical support they received.

Formalization (that is, registration with social security, the tax administration, and possibly with the trade registry) was strongly recommended by MEBF to all firms in the sample but not enforced or made obligatory. The results in Table 6 show that at least beneficiaries of cash grants have a probability to be formalized that is higher by about 7–9 percentage points relative to control firms. This corresponds to almost a doubling of the share of formalized firms; the share of formalized firms in the control group is about 7 percent. The estimated coefficients for beneficiaries of matching grants are also throughout positive but mostly very small and insignificant.

For beneficiaries of cash grants, we also see sizeable and highly significant positive effects of having a bank account (+22 percentage points), even if the control group mean of 62 percent is already impressive. This is probably due to the good targeting of the intervention. The large effect for cash beneficiaries is not surprising because beneficiaries of cash grants in contrast to beneficiaries of matching grants needed to open a bank account and obtained, if needed, assistance to do so to receive their grant. Although this makes ‘having a bank account’ more a project output than an outcome, this is an important achievement as experience with a formal bank and improved financial literacy might have lasting impacts for the beneficiaries.

For the beneficiaries of cash grants, we also see an increased probability by 13 percentage points (from a control group base of 29 percent) to keep books on sales and purchases. This may partly also follow from the increased formalization, which requires to do bookkeeping. This is also an outcome with potentially lasting effects.

The survey also asked the entrepreneurs several questions regarding their ability and plans to innovate by introducing, for example, new or modified products, services, management or operational processes, and their plans to use the internet. To analyze impacts on innovation, we aggregated the answers using Principal Component Analysis. At least based on OLS, we find sizeable positive effects in this case for both beneficiaries of cash and matching grants. We find impacts in the order of 0.18 and 0.15 units of standard deviation for beneficiaries of cash grants and matching grants, respectively. If we use the ANCOVA specification, the effects for beneficiaries of matching grants turn insignificant but are borderline, and the size of the effect does not change much. Hence, it seems that even beneficiaries of matching grants, maybe partly in anticipation, increased their innovation potential.

Overall, we cannot find significant heterogenous effects at this stage. With respect to gender, for women, we see slightly lower effects on formalization and slightly higher effects for bookkeeping and innovation, but these differences are statistically not significant. There is also no difference for banking. There is otherwise no systematic pattern that emerges with respect to the sector of activity, the size of the capital stock at baseline, or the size of the grant firms received.

Table 6. Intention-to-treat effects on formalization, banking, bookkeeping and innovation

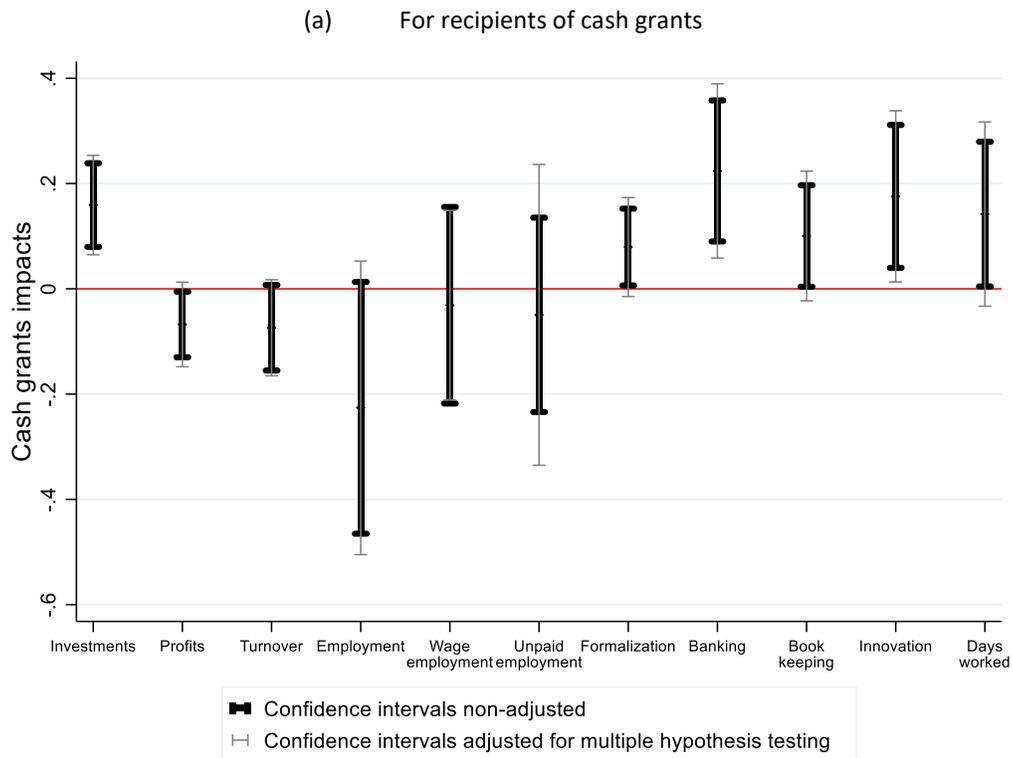
	Formalization		Banking		Bookkeeping		Innovation	
	All	Existing	All	Existing	All	Existing	All	Existing
Post								
Cash	0.078** (0.031)	0.090** (0.031)	0.217*** (0.046)	0.204*** (0.047)	0.102** (0.044)	0.123** (0.046)	0.182** (0.059)	0.182** (0.075)
Matching	0.035 (0.027)	0.036 (0.024)	0.044 (0.048)	0.032 (0.051)	0.006 (0.030)	0.022 (0.039)	0.151* (0.067)	0.160* (0.078)
ANCOVA								
Cash	0.103** (0.033)	0.103** (0.033)	0.209*** (0.043)	0.198*** (0.046)	0.131** (0.046)	0.131** (0.046)	0.196* (0.087)	0.196* (0.087)
Matching	0.052* (0.024)	0.052* (0.024)	0.034 (0.046)	0.020 (0.052)	0.026 (0.038)	0.026 (0.038)	0.124 (0.079)	0.124 (0.079)
Control group mean	0.066	0.064	0.619	0.627	0.292	0.282	-0.123	-0.121
Standard deviation	0.248	0.244	0.486	0.484	0.455	0.451	0.844	0.853
S.E. clustered	Municipality	Municipality	Municipality	Municipality	Municipality	Municipality	Municipality	Municipality
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Block fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R2 (Post)	0.142	0.145	0.168	0.172	0.157	0.151	0.085	0.089
R2 (ANCOVA)	0.218	0.218	0.312	0.308	0.173	0.173	0.127	0.127
N (Post)	1,042	969	1,107	970	1,042	969	1,009	937
N (ANCOVA)	969	969	1,107	970	969	969	913	913

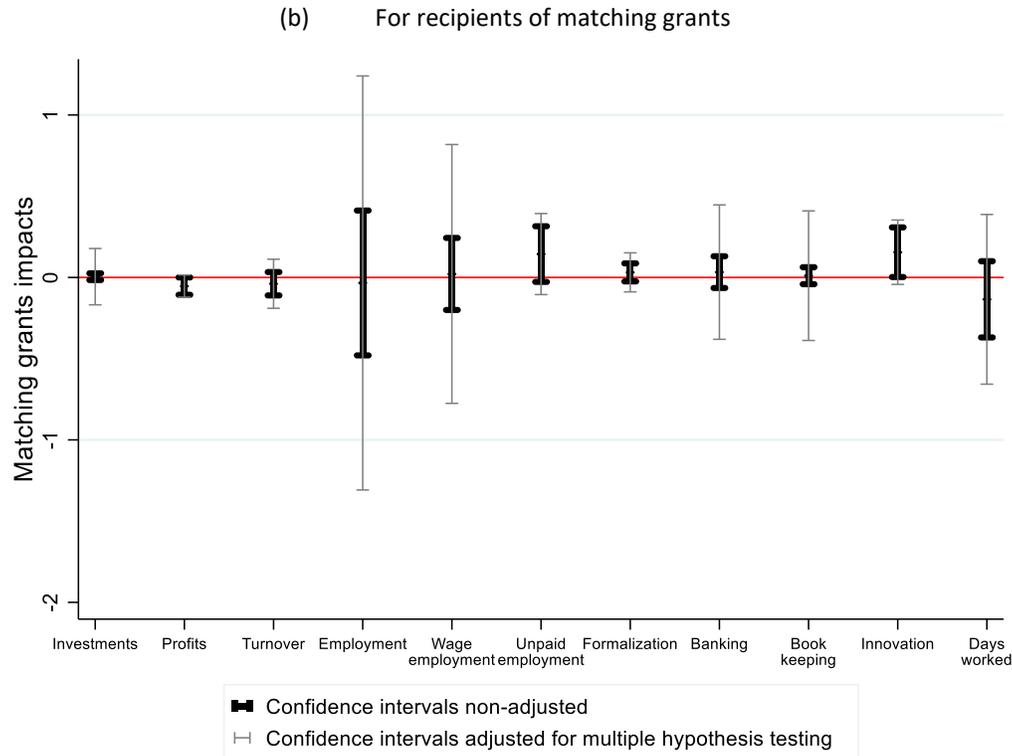
Note: *** $p < .01$, ** $p < .05$, * $p < .1$. Formalization, banking, and bookkeeping are dummies, and each equals one if the firm is formalized, owns a financial account, or keeps books on sales and purchases. Innovation is an index, that is, the first component from a principal component analysis applied on 15 dummies each taking the value 1 if the firm introduced either of the 15 listed innovations. All control variables come from the baseline survey. The list of controls include a dummy that equals one if the firm is already existing (vs 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 one 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. The control group mean refers to the follow-up survey. The detailed regression results are provided in Table A.8 and Table A.9 in the appendix.

More generally, the throughout positive effects on these softer intermediate outcomes may transform in the medium and longer terms into more sizeable and lasting effects for the ultimate outcomes.

Figure 6 summarizes the impacts along with their 95 percent confidence intervals across all outcomes we analyzed. 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. The reason is that even if none of the treatments has an effect, with an assumed significance level of 5 percent statistically, 1 out of 20 impacts will show up as significant. Hence, the adjusted confidence intervals are more conservative and redress this possible bias by reducing the probability of false rejections (errors of type 1).

Figure 6. Impacts and confidence intervals (adjusted and non-adjusted for multiple hypotheses testing)





Note: The adjusted confidence intervals are obtained from regressions that use (a) recipients of cash grants and the control group only and (b) the matching grant recipients and the control group only.
Source: Estimates based on Bagré Growth Pole Data Set.

6.6 Spillovers

Given that the treatment status varies within each of the nine municipalities and given that many of the treated firms operate within the same sector, it is worth exploring possible spillover effects. These are interesting for two reasons. First, it is important to check whether firms in the control group are affected by beneficiary firms which would reduce their suitability as a control group. Second, it is worth exploring whether the gains of beneficiaries depend on the competition they face (McKenzie 2017). In our case, especially, the slightly negative treatment effects on profits and turnover that we find for recipients of cash grants despite increased investment could, for instance, be due to the competition they face with other treated firms. Competition might make it difficult to increase sales especially in a context where the market size is relatively small and offers little possibilities to expand, for example, through exportation. Table 6 shows the effects of the number of treated firms (with cash) alternatively within 500 m, 2 km, and 5 km for recipients of cash grants, recipients of matching grants, and control firms.¹⁰ These results suggest that overall, there are only very small, if any, spillover effects. The only remarkable pattern that comes out of these estimations is that the negative treatment effect on profits decreases slightly with the number of cash beneficiaries within a radius of 5 km. Yet, again, overall, we see little spillovers so far, neither negative nor positive. This issue will be further explored as more rounds of data become available. The survey includes various variables that measure in one way or another the competition firms face.

¹⁰ On average there are 7.1, 20.7 and 32.5 cash beneficiaries within a radius of 500 m, 2 km and 5 km respectively.

These variables can be interacted with the treatment status. Yet, so far, the sample size is too small to investigate that type of heterogeneity.

Table 7. Spillover effects on profits stemming from cash beneficiaries

	500 m		2 km		5 km	
	All	Existing	All	Existing	All	Existing
Post						
Cash	-0.0526 (0.031)	-0.055 (0.032)	-0.065 (0.045)	-0.062 (0.050)	-0.137*** (0.032)	-0.133*** (0.032)
Cash*#Cash within a radius of	-0.003 (0.001)	-0.003 (0.002)	-0.0003 (0.001)	-0.0006 (0.001)	0.002*** (0.001)	0.002*** (0.000)
...						
Matching	-0.053* (0.028)	-0.055* (0.028)	-0.073** (0.028)	-0.076* (0.033)	-0.096** (0.029)	-0.096** (0.032)
Matching*#Cash within a radius of	-0.002 (0.002)	-0.001 (0.002)	0.0004 (0.001)	0.0006 (0.001)	0.0001** (0.000)	0.0001 (0.001)
...						
#Cash	-0.001 (0.001)	-0.001 (0.001)	-0.0007 (0.001)	-0.0009 (0.001)	-0.0009 (0.001)	-0.001 (0.001)
ANCOVA						
Cash	-0.027 (0.028)	-0.027 (0.028)	-0.0306 (0.040)	-0.031 (0.040)	-0.106*** (0.026)	-0.106*** (0.026)
Cash*#Cash within a radius of	-0.004** (0.002)	-0.004** (0.002)	-0.001 (0.001)	-0.001 (0.001)	0.0015*** (0.000)	0.0015*** (0.000)
...						
Matching	-0.032 (0.039)	-0.032 (0.039)	-0.056 (0.046)	-0.056 (0.046)	-0.079 (0.047)	-0.079 (0.047)
Matching*#Cash within a radius of	-0.002 (0.002)	-0.002 (0.002)	0.0004 (0.001)	0.0004 (0.001)	0.001 (0.001)	0.001 (0.001)
...						
#Cash	-0.0003 (0.001)	-0.0003 (0.001)	-0.0002 (0.001)	-0.0002 (0.001)	-0.0005 (0.001)	-0.0005 (0.001)
S.E. clustered	Munic.	Munic.	Munic.	Munic.	Munic.	Munic.
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Block fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
R2 (Post)	0.113	0.119	0.109	0.115	0.111	0.116
R2 (ANCOVA)	0.161	0.161	0.157	0.157	0.157	0.157
N (Post)	1,024	952	1,024	952	1,024	952
N (ANCOVA)	940	940	940	940	940	940

Notes, see next page.

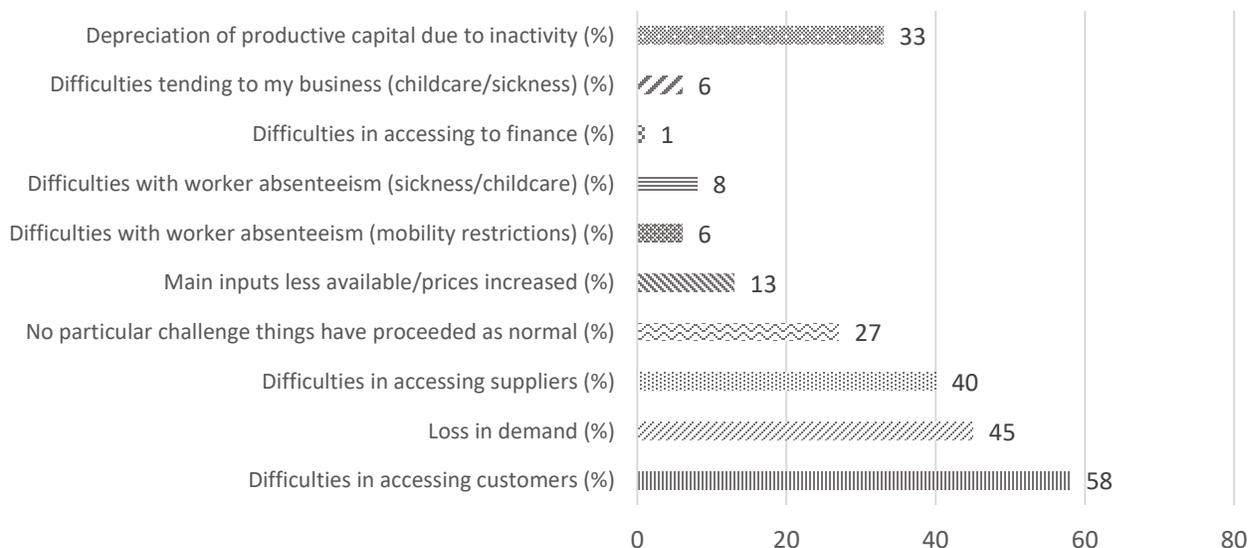
Note: ***p < .01, **p < .05, *p < .1. Profit has been trimmed at the 99th percentile. All control variables come from the baseline survey. The list of controls include a dummy that equals one if the firm is already existing (vs new); dummies for whether the firm sector is industry or services, with agriculture being the reference; a share of correct recalls by the owner from a digit span memory test; a dummy that equals one 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. The control group mean refers to the follow-up survey. The detailed regression results are provided in Table A.10 in the appendix.

6.7 Did the intervention mitigate the adverse effects of the COVID-19 pandemic?

The period between the baseline and the follow-up survey overlaps with the worldwide COVID-19 pandemic that also affected Burkina Faso. The first case of COVID-19 in Burkina Faso was detected on March 9, 2020. The country went through a prolonged lockdown, a nightly *couvre feu*, and a closure of the international airport for several months, which affected imports and exports of goods as well as the number of international travelers. Although the health impact was less dramatic as initially feared, the impact on the economy was clearly visible.

Indeed, almost 60 percent of our sampled firms reported in the follow-up survey that they faced difficulties. About 58 percent reported problems in reaching out to their customers, 45 percent reported a decline in demand, 40 percent reported a problem in getting their supplies, and 13 percent reported problems with worker absenteeism (Figure 7).

Figure 7. Percentage of firms which encountered difficulties due to COVID-19 and government response measures



Source: Bagré Growth Pole Data Set.

Firms were also explicitly asked whether they faced changes in their production, sales, investment, employment, and overall performance over the past six months, that is, over the period April/May 2020 to October/November 2020. Whereas in the cash group 44 percent reported a decline in their production, this share was 52 percent and 51 percent in the matching grants and control groups, respectively. Hence, the support in the form of cash grants may have protected cash beneficiaries to some extent or at least mitigated the negative impact of the crisis. Indeed, sales also decreased less for them; 52 percent of them reported a decline vs 59 percent and 58 percent in the two other groups, respectively. The same pattern is also visible for investment and reported performance. The share that reports a decline in performance is with 19 percent significantly lower for beneficiaries of cash grants compared to beneficiaries of matching grants and the control group where 24 percent and 25 percent reported a decline, respectively. Recall that most of the matching grant recipients received their support very late or not yet at all, hence not in time to mitigate the effects of the pandemic. Yet, these differences between beneficiaries of cash grants and all others are on average relatively small, so the protective effect was not more than moderate. We also do not see any difference in terms of workers temporarily or permanently laid off or having had to accept reduced salaries.

Table 8 analyzes these differences across the three groups more systematically. Based on the reported information, we coded six binary variables that take the value one if the entrepreneur reported that due to COVID-19 production, sales, investment, temporary employment, permanent employment, or performance respectively declined. If the entrepreneur reported that they remained unchanged or even increased, the respective variables take the value zero. We then run regressions similar to those in our impact evaluation, to test whether beneficiaries of cash grants reported less often a decline in the respective outcomes.

Note that this analysis differs in several respects from our analysis above. Whereas here we compare *qualitative trends* over the past six months across the three groups, above we compared *quantitative levels* over the past six months across the three groups.

The regression results suggest that beneficiaries of cash grants were less likely to report a decline in production, investment, and performance due to the COVID-19 pandemic.

Again, the fact that beneficiaries of cash grants were better protected than the beneficiaries of matching grants or firms in the control group is not necessarily at odds with the finding above that the cash grants have not yet had any positive impact on profits and turnover. Over the past six months, cash beneficiaries, on average, may well have generated lower profits than firms in the control group, but still, a smaller share could have experienced a negative trend. The negative effect on profits can even be driven by a relatively small share of firms that incurred very low or negative profits. It may just imply that fewer firms experienced a decline in performance, but those that had experienced a more substantial loss. Since we cannot quantitatively compare the trends over these six months, but just the levels, we cannot test this hypothesis.

Table 8. Effects of COVID-19 on decline of production, sales, investment, employment, and general performance

	Production		Sales		Investment		Employment		Performance	
	All	Existing								
Cash	-0.123*	-0.081	-0.109	-0.064	-0.280***	-0.268***	0.035	0.060	-0.196*	-0.181*
	(0.067)	(0.060)	(0.094)	(0.087)	(0.091)	(0.075)	(0.246)	(0.267)	(0.107)	(0.092)
Matching	0.075	0.103	0.081	0.087	-0.024	0.019	0.093	0.063	-0.035	-0.011
	(0.074)	(0.077)	(0.094)	(0.096)	(0.115)	(0.118)	(0.196)	(0.166)	(0.105)	(0.106)
Control group mean	0.508	0.524	0.578	0.606	0.219	0.230	0.024	0.024	0.243	0.255
Standard deviation	0.501	0.500	0.494	0.489	0.414	0.422	0.154	0.154	0.430	0.436
S.E. clustered	Municipality									
Controls	Yes									
Block fixed-effects	Yes									
Pseudo R2	0.068	0.058	0.080	0.054	0.056	0.044	0.121	0.132	0.053	0.046
N (Post)	1,107	970	1,107	970	1,107	970	1,107	970	1,107	970

Note: ***p < .01, **p < .05, *p < .1. Each of the dependent variables, production, sales, investment, employment, and general performance is a dummy that equals one if it decreased for the firm due to COVID-19. The list of controls includes a dummy that equals one if the firm is already existing (vs 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 one 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. The control group mean refers to the follow-up survey. The detailed regression results are provided in Table A.11 in the appendix.

6.8 *Qualitative evidence*

To get further insights into the impact of the support given to firms in the treatment groups and to understand what factors may explain the delay in achieving some of the goals, following the baseline survey we conducted 13 focus group discussions with about 120 participants in total and 54 semi-structured interviews with treated and control firms as well as community leaders.

In line with the results above that we derived from the structured survey, matching grant beneficiaries also reported in these rather open conversations increased business management knowledge that is helping them to innovate and to diversify their product offerings, to improve product quality, and to save on costs for more reinvestments in the firm. Cash grant beneficiaries reported in particular increased investments in physical capital (equipment and infrastructure), inventory, and production inputs. Even firms in the control group reported some improvements in business management practices following the basic training that all sampled firms had to follow. These entrepreneurs especially hailed their improved knowledge in financial management, marketing, and customer relationships. A control firm said: “Yes, the competitiveness of my firm improved in particular in the management of the firm and in the domain of financial management.”

Yet, among the treated entrepreneurs, many found the selection and disbursement procedures lengthy and disturbing for their production plans. But many respondents have rather positive expectations for the months to come. Beneficiaries of cash and matching grants expect a major change in their production owing to the training and financial support received. One beneficiary said: “In the next twelve months, our production situation will change thanks to the knowledge acquired from the training; we can build business relationships with other firms thanks to the commercial fairs, and this will increase our business knowledge further.” A beneficiary of a cash grant said: “The production will increase with the construction of a bigger building and the acquisition of a full set of equipment for a modern hairdressing salon. Also, I plan to hire employees and train myself.” Interviewees also reported more generally that the activities of the Bagré Growth Pole project created opportunities and incentives, which motivated them to grow their existing businesses or to engage in entrepreneurship. For example, the provision of irrigated lands, inputs for livestock production, and BDSs also induced the development of markets for products that are locally consumed and exported to neighboring countries.

Regarding the distinct impact of the program on men and women, most entrepreneurs reported that female entrepreneurs produce and sell more than male entrepreneurs. Many perceive women as demonstrating better entrepreneurial traits such as perseverance, strong engagement, trustworthiness, and saving and marketing abilities which would enable them to grow their businesses. Yet, it is also emphasized that some activities are only suited to men while others are only suited to women given local norms and customs. In addition, it was said several times that female entrepreneurs can grow their firm above a certain size only if their husband and their family support them.

Finally, the focus group discussions and in-depth interviews suggest that many entrepreneurs in the treatment groups face additional constraints that need to be removed to unlock the full potential of their activities. Many entrepreneurs said that they are not yet fully exploiting their equipment and would need to hire more staff but are unable to pay the wages. Many reported that they are not able to hire qualified workers who often require a higher wage and a formal contract. A beneficiary of a cash grant said: “Yes, I think I need more workers for managing my firm,” and a beneficiary of a matching grant said: “I am not hiring because I do not have the financial means to pay a worker for now.” Other beneficiaries prefer to improve the conditions of their current workers or to employ family labor to save costs for reinvestment

in their firms. A beneficiary of a matching grant said: “There is no need of hiring more workers now, I prefer to treat better my current workers.”

7. Conclusion

We analyzed the short-term impacts of two innovative interventions to enhance private sector development and to create jobs in a resource-poor and rather rural setting. The interventions relied on a rigorous targeting based on a business plan competition and offered sizeable grants and complementary training in business practices as well as a careful monitoring. These are all features that many previous interventions that have been subject to rigorous evaluations were missing and, at least partly, may explain that they generated only moderate or no effects.

Yet, the interventions of this study had to deal with many difficulties, including institutional frictions, an increasing countrywide political and social instability, and the COVID-19 pandemic. Together, these seriously affected the project implementation, especially by delaying the transfers to the beneficiaries and by dampening demand making it difficult to draw benefits from new investments. Nevertheless, the results based on the first follow-up survey can provide many interesting insights and are rather promising from the perspective of the implementers, at least with respect to the beneficiaries of cash grants, who were served on average a bit earlier than recipients of matching grants.

Investment significantly increased for the beneficiaries of cash grants. More firms in the group of cash beneficiaries invested, and they invested on average much higher amounts. Yet, the period was too short to have triggered any additional investment beyond the grants handed out. There is also no effect on investment for recipients of matching grants; yet, matching grants were earmarked for BDSs only, so any investment effect would be a second-order effect. The impact on profits for beneficiaries is slightly negative. Although, we cannot pinpoint the exact mechanism behind, a plausible explanation could be that the investments caused additional costs and that more time is needed to increase production and sales especially in the context of several overlapping crises. There is also not yet an effect on employment, maybe for the same reasons; yet, at least for recipients of cash grants, one can detect a small effect on the number of working days suggesting a higher effort level. The reason for optimism gives results on a whole range of intermediate outcomes. The impacts on formalization, banking, bookkeeping, and a summary measure of innovative behavior are throughout positive for beneficiaries of cash grants and the latter even for beneficiaries of matching grants. The next follow-up survey planned for end-2021 will show whether these changes also result in sizeable impacts on the targeted ultimate outcomes, especially employment.

The follow-up survey will then also allow to conduct a rigorous comparison between cash grants and matching grants. Besides the differences in use, they differ significantly in the procurement rules that are attached to them. Already during this first phase of the project, it became apparent that strict procurement rules create substantial costs in terms of time and staff. In-depth discussions with beneficiaries document their difficulties to understand and comply with the complexities of the entire disbursement process. Hence, we will compare the costs of procurement to the potential loss due to misuse and fraud to see which is the most cost-effective avenue to provide support. Misuse and fraud may for example have occurred in cases where firms have ‘disbursed’ large amounts of money, but according to our data did not make any significant investments or did not consult any business support services. Yet, it will of course not be trivial to detect such cases.

Although this is not a focus of this assessment, it is nevertheless interesting to see that the cash grants also mitigated the adverse effects of the COVID-19 pandemic. Compared to beneficiaries of matching grants and firms in the control group, recipients of cash grants reported less often a decline of production, sales, investment, employment, and the general performance due to the pandemic. This is an important result for the debate on how the private sector can be protected from this crisis.

Appendix

Table A.1. Baseline characteristics of the entrepreneur and pairwise balance tests across all three groups

Variable	Matching (M)		Cash (C)		Control (CT)		M vs CT	p-value	
	Mean	N	Mean	N	Mean	N		C vs CT	C vs M
<i>Age, gender, and marital status</i>									
Age (#)	42.39	400	42.02	400	43.57	400	0.14	0.06	0.64
Entrepreneurs age below 35 (#)	0.28	400	0.29	400	0.24	400	0.19	0.11	0.75
Female (=1)	0.43	400	0.37	400	0.44	400	0.78	0.07	0.13
Married or in union (=1)	0.92	400	0.92	400	0.91	400	0.90	0.70	0.80
<i>Education and trainings (=1)</i>									
No schooling	0.33	397	0.32	399	0.36	398	0.28	0.17	0.78
Primary school	0.23	397	0.26	399	0.22	398	0.66	0.16	0.34
Secondary school	0.23	397	0.21	399	0.20	398	0.25	0.55	0.58
Trained in 2017 or 2018	0.94	400	0.93	400	0.95	400	0.28	0.22	0.89
<i>Score of Digit Span Test (over 7)</i>									
Literate	1.87	294	1.81	298	1.85	271	0.88	0.77	0.64
Illiterate	0.82	106	1.04	102	0.91	129	0.42	0.33	0.06
<i>Religion (=1)</i>									
Christian	0.36	400	0.38	400	0.31	400	0.13	0.02	0.42
Muslim	0.64	400	0.61	400	0.69	400	0.14	0.02	0.42
<i>Ethnic group (=1)</i>									
Bissa	0.67	400	0.68	400	0.68	400	0.71	0.94	0.65
Mossi	0.24	400	0.23	400	0.22	400	0.56	0.74	0.80
Household head (=1)	0.57	387	0.60	392	0.56	388	0.80	0.23	0.34
Years of work experience (#)	16.62	400	17.18	400	17.27	400	0.41	0.91	0.47
Own children (#)	5.46	381	5.65	379	5.96	384	0.11	0.34	0.54

Note: N indicates the number of observations. p-values are from t-tests with equal variances. The marital status includes free union. Education corresponds to the highest level attained. The training concerns business management. The work experience includes work in any activity sector.

Table A.2. Baseline characteristics of the firm and pairwise balance tests across all three groups

Variable	Matching (M)		Cash (C)		Control (CT)		p-value		
	Mean	N	Mean	N	Mean	N	M vs CT	C vs CT	C vs M
<i>Age and sector of activity (=1)</i>									
Years since business created (#)	8.60	344	8.63	345	9.09	356	0.40	0.41	0.96
Agriculture, excluding livestock	0.11	341	0.08	343	0.13	355	0.39	0.02	0.14
Agriculture, including livestock	0.51	341	0.56	343	0.51	355	0.99	0.14	0.15
Manufacturing	0.16	341	0.15	343	0.16	355	0.94	0.67	0.73
Services	0.20	341	0.19	343	0.18	355	0.65	0.75	0.89
<i>Physical and financial capital</i>									
Physical capital stock (US\$)	3,778.69	333	6,484.62	334	3,530.77	348	0.89	0.35	0.44
Ever took a formal loan (=1)	0.27	268	0.27	258	0.29	238	0.67	0.72	0.95
<i>Business performance and staff</i>									
Profit (US\$), past six months	489.02	341	466.32	344	531.47	354	0.60	0.43	0.80
Turnover (US\$), past six months	1,209.82	343	1,228.35	345	1,244.67	355	0.83	0.91	0.91
Employees (#)	2.58	344	2.48	345	2.76	356	0.34	0.10	0.57
<i>Business practices and attitudes (=1)</i>									
Bookkeeping	0.44	337	0.39	344	0.43	350	0.72	0.33	0.19
Monitoring products stock	0.27	229	0.23	235	0.23	229	0.39	0.97	0.36
Willing to take risks	0.71	400	0.71	400	0.73	400	0.43	0.39	0.94
Member of a business association (=1)	0.25	400	0.29	400	0.32	400	0.03	0.32	0.23
<i>Business plan and grant</i>									
Grant amount (US\$)	3,442.88	400	3,420.52	400	3,395.99	400	0.66	0.82	0.84
Business plan score (over 100)	61.36	400	60.96	400	61.38	400	0.96	0.24	0.25

Note: N indicates the number of observations. p-values are from t-tests with equal variances. Formal loan is from a bank or microfinance institution. The calculation of profits and turnovers considered all products and services sold over the six months preceding the survey.

Table A.3. Intention-to-treat effects on total investment in physical capital and land (past 6 months), detailed regression results

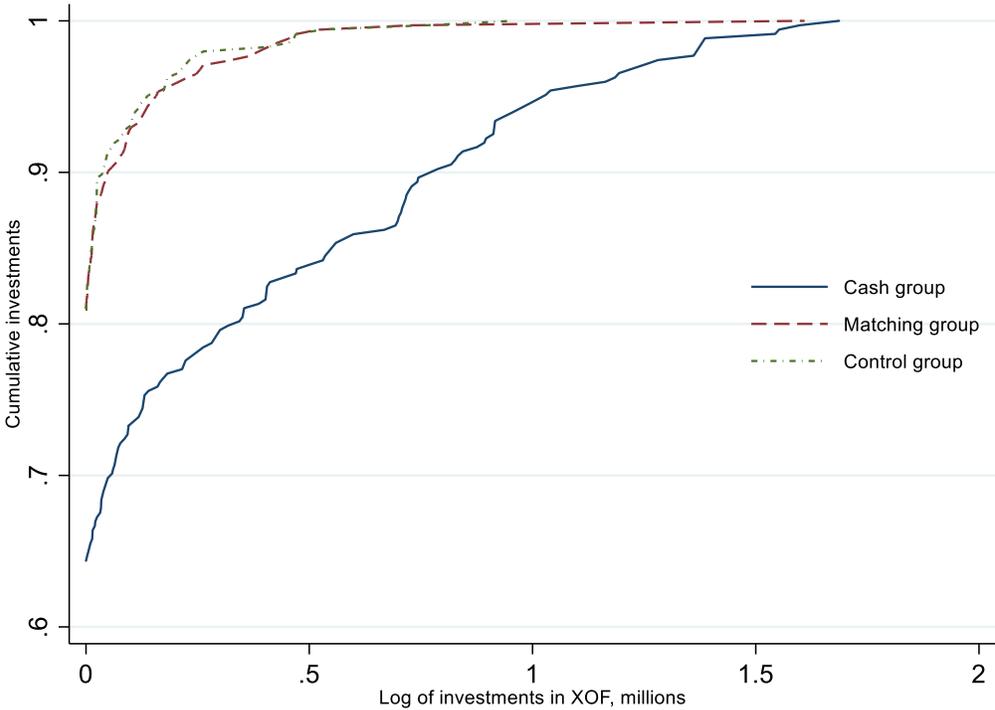
Variables	Total investment (log)			
	Post		ANCOVA	
	All	Existing	All	Existing
Cash	0.160*** (0.014)	0.155*** (0.013)	0.151*** (0.011)	0.151*** (0.011)
Matching	0.004 (0.010)	0.008 (0.009)	0.010 (0.010)	0.010 (0.010)
Existing (=1)	-0.050 (0.034)			
Dependent variable (baseline value)			0.149* (0.071)	0.149* (0.071)
Industry (=1)	0.028 (0.023)	0.021 (0.018)	0.013 (0.016)	0.013 (0.016)
Services (=1)	0.035 (0.032)	0.027 (0.030)	0.025 (0.026)	0.025 (0.026)
Digitspan correct recalls (%)	0.001 (0.001)	0.001 (0.001)	0.000 (0.001)	0.000 (0.001)
Female (=1)	-0.024 (0.018)	-0.025 (0.019)	-0.024 (0.018)	-0.024 (0.018)
Age in 2018	0.000 (0.001)	-0.000 (0.001)	0.000 (0.001)	0.000 (0.001)
# work experience	0.000 (0.000)	0.001 (0.001)	0.000 (0.000)	0.000 (0.000)
Muslim (=1)	0.101** (0.035)	0.078*** (0.021)	0.079*** (0.022)	0.079*** (0.022)
Christian (=1)	0.098* (0.049)	0.076 (0.045)	0.080* (0.043)	0.080* (0.043)
Bissa ethnic group (=1)	0.050*** (0.010)	0.043*** (0.011)	0.045*** (0.011)	0.045*** (0.011)
Mossi ethnic group (=1)	0.012 (0.010)	0.011 (0.011)	0.017 (0.011)	0.017 (0.011)
Read/write French and a local language (=1)	-0.015 (0.027)	-0.033 (0.025)	-0.023 (0.025)	-0.023 (0.025)
Read/write a local language only (=1)	-0.008 (0.037)	-0.004 (0.043)	-0.008 (0.041)	-0.008 (0.041)
Primary education (=1)	0.035 (0.024)	0.049 (0.030)	0.043 (0.027)	0.043 (0.027)
Secondary education (=1)	0.011 (0.032)	0.034 (0.039)	0.021 (0.040)	0.021 (0.040)
Other education (=1)	0.038** (0.016)	0.036 (0.022)	0.036 (0.021)	0.036 (0.021)

Table A.3 continued

Variables	Total investment (log)			
	Post		ANCOVA	
	All	Existing	All	Existing
# household size	-0.002** (0.001)	-0.002** (0.001)	-0.002** (0.001)	-0.002** (0.001)
Household head (=1)	-0.015 (0.020)	-0.015 (0.018)	-0.017 (0.019)	-0.017 (0.019)
# household assets	0.001 (0.002)	0.001 (0.002)	-0.000 (0.002)	-0.000 (0.002)
Community leader in household (=1)	-0.024 (0.019)	-0.022 (0.019)	-0.021 (0.017)	-0.021 (0.017)
Block 2	0.006 (0.018)	0.003 (0.017)	0.005 (0.018)	0.005 (0.018)
Block 3	-0.011 (0.024)	-0.010 (0.025)	-0.008 (0.026)	-0.008 (0.026)
Block 4	0.065* (0.031)	0.058 (0.034)	0.051 (0.039)	0.051 (0.039)
Block 5	0.037 (0.042)	0.039 (0.041)	0.043 (0.040)	0.043 (0.040)
Block 6	0.116 (0.092)	0.139 (0.096)	0.027 (0.028)	0.027 (0.028)
Block 7	0.131 (0.112)	0.122 (0.123)	0.130 (0.127)	0.130 (0.127)
Constant	-0.099 (0.053)	-0.092** (0.034)	-0.091** (0.036)	-0.091** (0.036)
S.E. clustered	Municipality	Municipality	Municipality	Municipality
R2	0.142	0.128	0.122	0.122
N	1,032	960	952	952

Note: ***p < .01, **p < .05, *p < .1. Investment has been trimmed at the 99th percentile. All control variables come from the baseline survey. The list of controls include a dummy that equals one if the firm is already existing (vs new); dummies for whether the firm's 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 one 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.

Figure A.1. Posttreatment CDFs of investment by treatment group



Source: Bagré Growth Pole Data Set.

Table A.4. Intention-to-treat effects on profits (total profits on products and services), past 6 months, profits are in XOF, millions (XOF 1 million = US\$1,820), detailed regression results

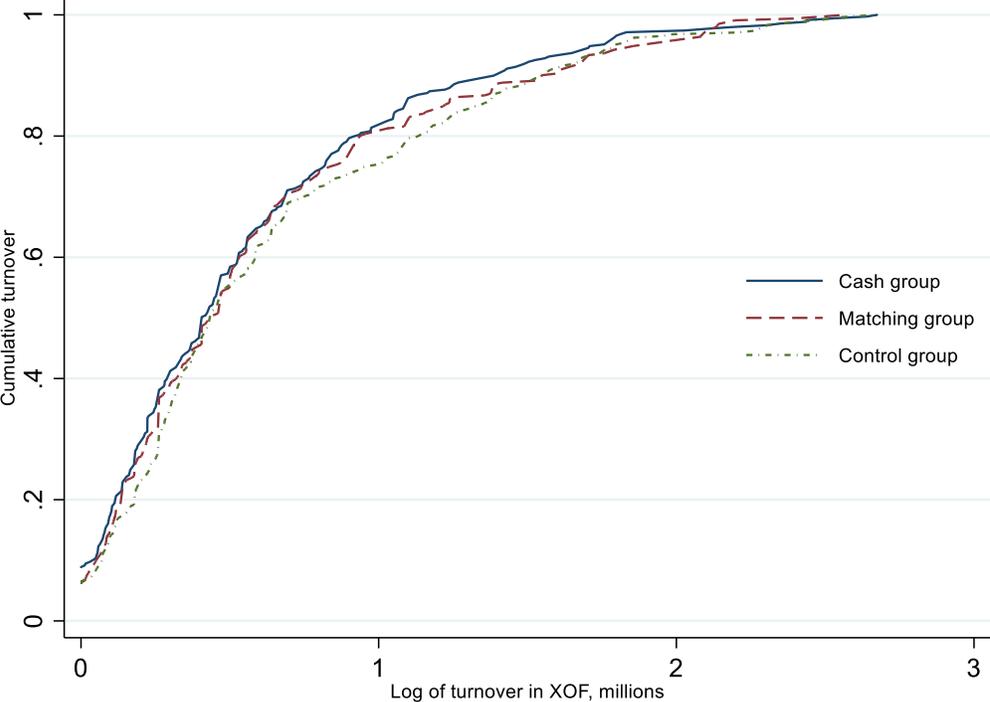
Variables	Profits			
	Post		ANCOVA	
	All	Existing	All	Existing
Cash	-0.068** (0.026)	-0.073** (0.029)	-0.057** (0.023)	-0.057** (0.023)
Matching	-0.059** (0.021)	-0.060** (0.023)	-0.045 (0.028)	-0.045 (0.028)
Existing (=1)	0.078** (0.029)			
Dependent variable (baseline value)			0.206*** (0.032)	0.206*** (0.032)
Industry (=1)	-0.122*** (0.020)	-0.138*** (0.026)	-0.112*** (0.024)	-0.112*** (0.024)
Services (=1)	-0.064 (0.050)	-0.070 (0.055)	-0.040 (0.052)	-0.040 (0.052)
Digitspan correct recalls (%)	0.002* (0.001)	0.002* (0.001)	0.002 (0.001)	0.002 (0.001)
Female (=1)	-0.065** (0.027)	-0.068** (0.026)	-0.058** (0.024)	-0.058** (0.024)
Age in 2018	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)
# work experience	-0.003** (0.001)	-0.003** (0.001)	-0.002** (0.001)	-0.002** (0.001)
Muslim (=1)	0.067* (0.035)	0.053 (0.039)	0.039 (0.040)	0.039 (0.040)
Christian (=1)	0.053 (0.048)	0.044 (0.055)	0.027 (0.052)	0.027 (0.052)
Bissa ethnic group (=1)	-0.082** (0.035)	-0.080* (0.038)	-0.053 (0.029)	-0.053 (0.029)
Mossi ethnic group (=1)	-0.041 (0.051)	-0.039 (0.054)	-0.018 (0.036)	-0.018 (0.036)
Read/write French and a local language (=1)	-0.063 (0.043)	-0.052 (0.049)	-0.047 (0.048)	-0.047 (0.048)
Read/write a local language only (=1)	-0.057 (0.041)	-0.071* (0.038)	-0.059* (0.028)	-0.059* (0.028)
Primary education (=1)	0.068** (0.028)	0.058 (0.033)	0.055 (0.037)	0.055 (0.037)
Secondary education (=1)	0.019 (0.056)	0.007 (0.059)	0.020 (0.065)	0.020 (0.065)
Other education (=1)	0.062** (0.023)	0.061** (0.025)	0.056*** (0.015)	0.056*** (0.015)

Table A.4 continued

Variables	Profits			
	Post		ANCOVA	
	All	Existing	All	Existing
# household size	0.001 (0.001)	0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)
Household head (=1)	0.025 (0.021)	0.016 (0.018)	0.004 (0.016)	0.004 (0.016)
# household assets	0.008** (0.003)	0.009** (0.003)	0.005 (0.003)	0.005 (0.003)
Community leader in household (=1)	0.003 (0.009)	-0.003 (0.012)	-0.008 (0.009)	-0.008 (0.009)
Block 2	-0.012 (0.028)	-0.012 (0.032)	-0.013 (0.032)	-0.013 (0.032)
Block 3	-0.016 (0.034)	-0.019 (0.037)	-0.034 (0.033)	-0.034 (0.033)
Block 4	0.107 (0.069)	0.120 (0.072)	0.077 (0.053)	0.077 (0.053)
Block 5	0.161* (0.084)	0.178* (0.092)	0.078 (0.046)	0.078 (0.046)
Block 6	0.215 (0.152)	0.227 (0.158)	0.230 (0.153)	0.230 (0.153)
Block 7	-0.048 (0.039)	-0.076 (0.055)	-0.061 (0.052)	-0.061 (0.052)
Constant	0.094 (0.105)	0.179 (0.108)	0.175 (0.097)	0.175 (0.097)
S.E. clustered	Municipality	Municipality	Municipality	Municipality
R2	1,024	952	940	940
N	0.132	0.138	0.170	0.170

Note: ***p < .01, **p < .05, *p < .1. Profit has been trimmed at the 99th percentile. All control variables come from the baseline survey. The list of controls include a dummy that equals one if the firm is already existing (vs 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 one 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. The control group mean refers to the follow-up survey.

Figure A.2. Post-treatment CDFs of turnover (log) by treatment group



Source: Bagré Growth Pole Data Set.

Table A.5. Intention-to-treat effects on turnover (total revenues on products and services), past 6 months, in XOF, millions (XOF 1 million = US\$1,820) and in log, detailed regression results

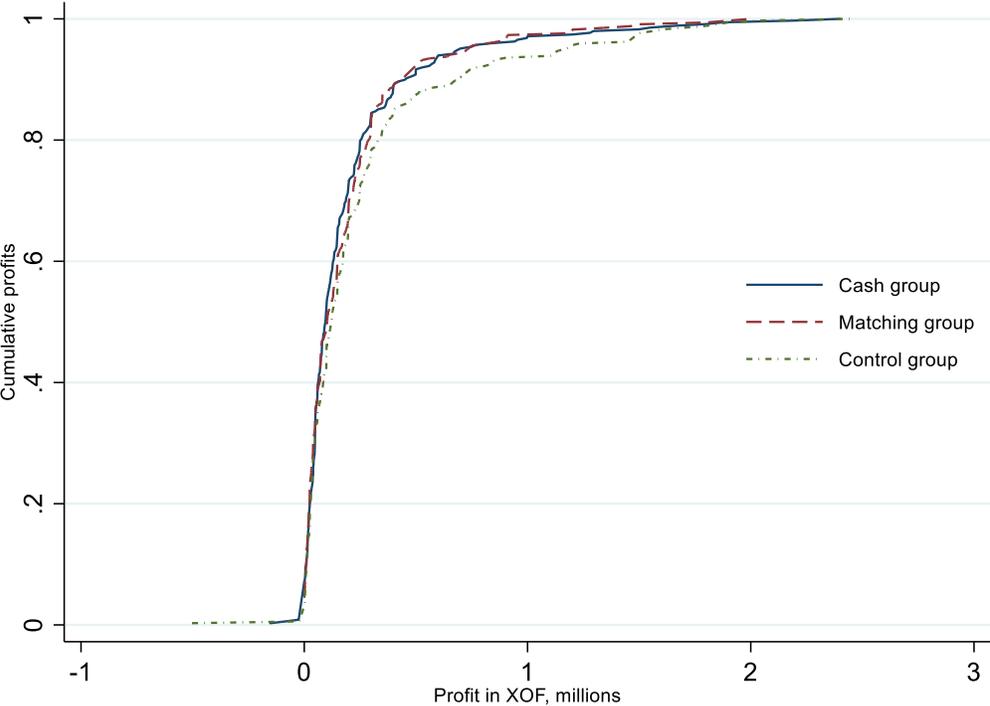
Variables	Turnover (log)			
	Post		ANCOVA	
	All	Existing	All	Existing
Cash	-0.077*	-0.067	-0.058	-0.058
	(0.035)	(0.037)	(0.034)	(0.034)
Matching	-0.048	-0.045	-0.013	-0.013
	(0.029)	(0.033)	(0.035)	(0.035)
Existing (=1)	0.168**			
	(0.052)			
Dependent variable (baseline value)			0.374***	0.374***
			(0.045)	(0.045)
Industry (=1)	0.226***	0.206***	0.111*	0.111*
	(0.057)	(0.051)	(0.049)	(0.049)
Services (=1)	0.329**	0.324*	0.253*	0.253*
	(0.139)	(0.151)	(0.121)	(0.121)
Digitspan correct recalls (%)	0.002***	0.002***	0.001*	0.001*
	(0.001)	(0.001)	(0.001)	(0.001)
Female (=1)	-0.145	-0.165*	-0.123**	-0.123**
	(0.078)	(0.079)	(0.044)	(0.044)
Age in 2018	-0.000	-0.000	-0.000	-0.000
	(0.001)	(0.002)	(0.002)	(0.002)
# work experience	-0.002	-0.002	-0.002	-0.002
	(0.001)	(0.002)	(0.002)	(0.002)
Muslim (=1)	0.196*	0.165	0.100	0.100
	(0.099)	(0.104)	(0.115)	(0.115)
Christian (=1)	0.100	0.077	0.067	0.067
	(0.094)	(0.103)	(0.109)	(0.109)
Bissa ethnic group (=1)	-0.249***	-0.250***	-0.162***	-0.162***
	(0.070)	(0.055)	(0.047)	(0.047)
Mossi ethnic group (=1)	-0.192**	-0.185**	-0.130*	-0.130*
	(0.066)	(0.067)	(0.063)	(0.063)
Read/write French and a local language (=1)	-0.125**	-0.132**	-0.102	-0.102
	(0.047)	(0.052)	(0.070)	(0.070)
Read/write a local language only (=1)	-0.011	-0.039	-0.038	-0.038
	(0.020)	(0.031)	(0.027)	(0.027)
Primary education (=1)	0.039	0.041	0.040	0.040
	(0.054)	(0.056)	(0.052)	(0.052)
Secondary education (=1)	0.070	0.059	0.066	0.066
	(0.077)	(0.074)	(0.085)	(0.085)
Other education (=1)	0.039	0.026	0.024	0.024
	(0.042)	(0.037)	(0.036)	(0.036)

Table A.5 continued

Variables	Turnover (log)			
	Post		ANCOVA	
	All	Existing	All	Existing
# household size	-0.001 (0.002)	-0.002 (0.002)	-0.002 (0.002)	-0.002 (0.002)
Household head (=1)	0.045 (0.072)	0.023 (0.066)	0.004 (0.043)	0.004 (0.043)
# household assets	0.018*** (0.005)	0.018** (0.006)	0.008 (0.004)	0.008 (0.004)
Community leader in household (=1)	-0.006 (0.028)	0.004 (0.027)	0.021 (0.021)	0.021 (0.021)
Block 2	0.093* (0.042)	0.115** (0.045)	0.090 (0.051)	0.090 (0.051)
Block 3	0.144*** (0.036)	0.146*** (0.036)	0.076 (0.044)	0.076 (0.044)
Block 4	0.280** (0.109)	0.340** (0.108)	0.185** (0.080)	0.185** (0.080)
Block 5	0.405*** (0.099)	0.391*** (0.065)	0.222** (0.078)	0.222** (0.078)
Block 6	0.281 (0.155)	0.306 (0.169)	0.142 (0.141)	0.142 (0.141)
Block 7	0.137 (0.119)	0.169 (0.135)	0.138 (0.147)	0.138 (0.147)
Constant	0.182 (0.195)	0.389* (0.185)	0.289 (0.166)	0.289 (0.166)
S.E. clustered	Municipality	Municipality	Municipality	Municipality
R2	0.166	0.167	0.297	0.297
N	1,019	946	931	931

Note: ***p < .01, **p < .05, *p < .1. Turnover has been trimmed at the 99th percentile. All control variables come from the baseline survey. The list of controls include a dummy that equals one if the firm is already existing (vs 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 one 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. The control group mean refers to the follow-up survey.

Figure A.3. Posttreatment CDFs of profits by treatment group



Source: Bagré Growth Pole Data Set.

Table A.6. Intention-to-treat effects on total and paid wage employment, detailed regression results

Variables	Total employment				Paid wage employment			
	Post		ANCOVA		Post		ANCOVA	
	All	Existing	All	Existing	All	Existing	All	Existing
Cash	-0.242*	-0.200	-0.091	-0.091	-0.043	0.008	0.010	0.010
	(0.112)	(0.134)	(0.149)	(0.149)	(0.094)	(0.108)	(0.088)	(0.088)
Matching	-0.053	-0.074	0.022	0.022	0.013	0.010	0.052	0.052
	(0.232)	(0.262)	(0.286)	(0.286)	(0.114)	(0.121)	(0.126)	(0.126)
Existing (=1)	0.519**				0.276			
	(0.185)				(0.209)			
Dependent variable (baseline value)			0.357***	0.357***			0.417***	0.417***
			(0.048)	(0.048)			(0.063)	(0.063)
Industry (=1)	0.993***	1.002***	0.552**	0.552**	0.832***	0.831***	0.567***	0.567***
	(0.198)	(0.206)	(0.182)	(0.182)	(0.110)	(0.117)	(0.103)	(0.103)
Services (=1)	0.711***	0.681***	0.320*	0.320*	0.749***	0.734***	0.350	0.350
	(0.141)	(0.159)	(0.170)	(0.170)	(0.201)	(0.198)	(0.232)	(0.232)
Digitspan correct recalls (%)	0.006	0.005	0.004	0.004	0.000	-0.000	-0.004	-0.004
	(0.004)	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Female (=1)	0.163	0.137	0.215	0.215	-0.194	-0.213	-0.203	-0.203
	(0.175)	(0.225)	(0.222)	(0.222)	(0.133)	(0.164)	(0.147)	(0.147)
Age in 2018	0.008	0.007	-0.002	-0.002	0.006	0.008	0.003	0.003
	(0.009)	(0.011)	(0.010)	(0.010)	(0.006)	(0.006)	(0.003)	(0.003)
# work experience	0.003	0.006	0.007	0.007	-0.007*	-0.005	-0.002	-0.002
	(0.008)	(0.009)	(0.008)	(0.008)	(0.004)	(0.004)	(0.003)	(0.003)
Muslim (=1)	-0.024	-0.204	0.263	0.263	-0.472	-0.577	-0.263	-0.263
	(0.432)	(0.388)	(0.459)	(0.459)	(0.455)	(0.482)	(0.483)	(0.483)
Christian (=1)	-0.246	-0.412	0.079	0.079	-0.503	-0.638	-0.268	-0.268
	(0.439)	(0.401)	(0.512)	(0.512)	(0.402)	(0.434)	(0.436)	(0.436)
Bissa ethnic group (=1)	-0.115	-0.097	0.259	0.259	-0.311	-0.270	-0.001	-0.001
	(0.218)	(0.213)	(0.268)	(0.268)	(0.321)	(0.323)	(0.350)	(0.350)
Mossi ethnic group (=1)	-0.043	-0.048	0.217	0.217	-0.356	-0.360	-0.064	-0.064
	(0.210)	(0.231)	(0.313)	(0.313)	(0.303)	(0.313)	(0.302)	(0.302)

Table A.6 continued

Variables	Total employment				Paid wage employment			
	Post		ANCOVA		Post		ANCOVA	
	All	Existing	All	Existing	All	Existing	All	Existing
Read/write French and a local language (=1)	-0.673** (0.215)	-0.751** (0.255)	-0.710*** (0.196)	-0.710*** (0.196)	-0.379** (0.159)	-0.435** (0.188)	-0.289 (0.187)	-0.289 (0.187)
Read/write a local language only (=1)	-0.024 (0.235)	-0.064 (0.252)	-0.230 (0.264)	-0.230 (0.264)	0.030 (0.292)	0.056 (0.326)	-0.041 (0.352)	-0.041 (0.352)
Primary education (=1)	0.441** (0.141)	0.518*** (0.145)	0.439** (0.153)	0.439** (0.153)	0.114 (0.076)	0.204* (0.096)	0.055 (0.166)	0.055 (0.166)
Secondary education (=1)	0.474 (0.277)	0.622* (0.331)	0.614* (0.314)	0.614* (0.314)	0.119 (0.165)	0.268 (0.189)	0.134 (0.178)	0.134 (0.178)
Other education (=1)	0.270 (0.239)	0.296 (0.246)	0.284 (0.211)	0.284 (0.211)	0.062 (0.136)	0.058 (0.158)	0.059 (0.163)	0.059 (0.163)
# household size	0.032*** (0.009)	0.032** (0.011)	0.017* (0.008)	0.017* (0.008)	0.012 (0.007)	0.011 (0.008)	0.001 (0.007)	0.001 (0.007)
Household head (=1)	0.413* (0.185)	0.363 (0.223)	0.285 (0.204)	0.285 (0.204)	0.054 (0.173)	0.003 (0.184)	-0.080 (0.157)	-0.080 (0.157)
# household assets	0.051*** (0.014)	0.053*** (0.013)	0.030** (0.012)	0.030** (0.012)	0.044*** (0.010)	0.047*** (0.011)	0.027 (0.015)	0.027 (0.015)
Community leader in household (=1)	0.129 (0.102)	0.126 (0.108)	0.147 (0.130)	0.147 (0.130)	0.040 (0.107)	0.049 (0.110)	0.065 (0.097)	0.065 (0.097)

Table A.6 continued

Variables	Total employment				Paid wage employment			
	Post		ANCOVA		Post		ANCOVA	
	All	Existing	All	Existing	All	Existing	All	Existing
Block 2	0.057 (0.142)	0.085 (0.136)	0.076 (0.103)	0.076 (0.103)	0.015 (0.113)	0.080 (0.104)	0.075 (0.107)	0.075 (0.107)
Block 3	0.220 (0.185)	0.216 (0.183)	0.162* (0.085)	0.162* (0.085)	0.098 (0.156)	0.105 (0.151)	0.091 (0.123)	0.091 (0.123)
Block 4	0.303 (0.270)	0.313 (0.296)	0.154 (0.169)	0.154 (0.169)	0.415* (0.188)	0.454* (0.204)	0.258 (0.230)	0.258 (0.230)
Block 5	0.905 (0.903)	0.957 (0.961)	0.724 (0.717)	0.724 (0.717)	0.956* (0.465)	0.976 (0.541)	0.746* (0.352)	0.746* (0.352)
Block 6	0.600 (0.703)	0.881 (0.773)	0.512 (0.626)	0.512 (0.626)	0.605** (0.255)	0.726** (0.269)	0.391* (0.198)	0.391* (0.198)
Block 7	0.918* (0.493)	1.290* (0.582)	0.864 (0.486)	0.864 (0.486)	0.892 (0.490)	1.117* (0.584)	0.943 (0.526)	0.943 (0.526)
Constant	-0.334 (0.795)	0.312 (0.824)	-0.364 (0.931)	-0.364 (0.931)	0.522 (0.622)	0.711 (0.676)	0.383 (0.729)	0.383 (0.729)
S.E. clustered	Municipality	Municipality	Municipality	Municipality	Municipality	Municipality	Municipality	Municipality
R2	0.093	0.096	0.226	0.226	0.097	0.100	0.243	0.243
N	1,067	989	989	989	1,042	969	969	969

Note: ***p < .01, **p < .05, *p < .1. The dependent variables are the total number of employees and the number of paid wage employees, respectively. All control variables come from the baseline survey. The list of controls includes a dummy that equals one if the firm is already existing (vs 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 one 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.

Table A.7. Intention-to-treat effects on unpaid employment and the number of days worked, detailed regression results

Variables	Unpaid employment				Days worked	
	Post		ANCOVA		Post	
	All	Existing	All	Existing	All	Existing
Cash	-0.063 (0.089)	-0.043 (0.097)	-0.039 (0.098)	-0.039 (0.098)	0.160** (0.058)	0.081 (0.051)
Matching	0.141 (0.084)	0.146 (0.094)	0.135 (0.092)	0.135 (0.092)	-0.145 (0.097)	-0.188* (0.088)
Existing (=1)	0.111 (0.115)				0.300* (0.153)	
Dependent variable (baseline value)			0.170*** (0.035)	0.170*** (0.035)		
Industry (=1)	-0.140 (0.174)	-0.126 (0.173)	-0.144 (0.154)	-0.144 (0.154)	-0.595*** (0.157)	-0.623** (0.190)
Services (=1)	-0.103 (0.215)	-0.091 (0.220)	-0.020 (0.218)	-0.020 (0.218)	-0.242 (0.147)	-0.209 (0.136)
Digitspan correct recalls (%)	0.005** (0.002)	0.005* (0.002)	0.004* (0.002)	0.004* (0.002)	-0.001 (0.002)	-0.002 (0.001)
Female (=1)	0.245 (0.162)	0.267 (0.188)	0.300 (0.183)	0.300 (0.183)	-0.410** (0.155)	-0.431*** (0.128)
Age in 2018	-0.000 (0.005)	-0.002 (0.006)	-0.004 (0.006)	-0.004 (0.006)	-0.006 (0.005)	-0.002 (0.007)
# work experience	0.008* (0.004)	0.010* (0.005)	0.010* (0.004)	0.010* (0.004)	-0.005 (0.006)	-0.006 (0.007)
Muslim (=1)	0.288 (0.308)	0.222 (0.338)	0.416 (0.250)	0.416 (0.250)	-0.418 (0.508)	-0.390 (0.523)
Christian (=1)	0.106 (0.316)	0.074 (0.347)	0.219 (0.253)	0.219 (0.253)	-0.483 (0.502)	-0.465 (0.497)
Bissa ethnic group (=1)	0.044 (0.111)	-0.002 (0.130)	0.063 (0.115)	0.063 (0.115)	-0.360** (0.110)	-0.313* (0.156)
Mossi ethnic group (=1)	0.168 (0.118)	0.157 (0.138)	0.170 (0.124)	0.170 (0.124)	-0.376*** (0.112)	-0.346** (0.127)
Read/write French and a local language (=1)	-0.144 (0.120)	-0.134 (0.112)	-0.152 (0.119)	-0.152 (0.119)	-0.139 (0.140)	-0.103 (0.123)
Read/write a local language only (=1)	0.187 (0.111)	0.149 (0.114)	0.140 (0.142)	0.140 (0.142)	0.290 (0.166)	0.409** (0.171)
Primary education (=1)	0.034 (0.071)	0.015 (0.079)	0.018 (0.088)	0.018 (0.088)	0.306 (0.182)	0.310 (0.196)
Secondary education (=1)	0.201 (0.159)	0.184 (0.153)	0.209 (0.161)	0.209 (0.161)	0.074 (0.209)	0.091 (0.208)
Other education (=1)	0.007 (0.090)	0.011 (0.083)	0.002 (0.077)	0.002 (0.077)	0.077 (0.190)	-0.023 (0.174)

Table A.7 continued

Variables	Unpaid employment				Days worked	
	Post		ANCOVA		Post	
	All	Existing	All	Existing	All	Existing
# household size	0.011 (0.007)	0.012 (0.007)	0.008 (0.005)	0.008 (0.005)	-0.000 (0.007)	0.000 (0.009)
Household head (=1)	0.105 (0.147)	0.115 (0.162)	0.140 (0.159)	0.140 (0.159)	-0.163 (0.125)	-0.187 (0.122)
# household assets	0.010 (0.010)	0.011 (0.011)	0.013 (0.011)	0.013 (0.011)	-0.000 (0.014)	0.007 (0.014)
Community leader in household (=1)	0.022 (0.089)	0.010 (0.100)	0.025 (0.110)	0.025 (0.110)	-0.079 (0.130)	-0.086 (0.127)
Block 2	0.094 (0.125)	0.107 (0.128)	0.123 (0.121)	0.123 (0.121)	0.108 (0.098)	0.092 (0.120)
Block 3	0.091 (0.169)	0.108 (0.178)	0.121 (0.156)	0.121 (0.156)	0.267 (0.222)	0.238 (0.224)
Block 4	0.011 (0.180)	0.036 (0.192)	0.086 (0.191)	0.086 (0.191)	0.168 (0.107)	0.149 (0.129)
Block 5	-0.240 (0.185)	-0.219 (0.175)	-0.121 (0.174)	-0.121 (0.174)	-0.181 (0.544)	-0.330 (0.463)
Block 6	0.039 (0.441)	0.085 (0.473)	0.106 (0.481)	0.106 (0.481)	-0.341 (0.469)	-0.385 (0.498)
Block 7	0.283 (0.160)	0.407** (0.152)	0.453** (0.151)	0.453** (0.151)	-0.073 (0.227)	-0.024 (0.198)
Constant	-0.555 (0.490)	-0.374 (0.450)	-0.650 (0.443)	-0.650 (0.443)	6.826*** (0.494)	6.931*** (0.550)
S.E. clustered	Municipality	Municipality	Municipality	Municipality	Municipality	Municipality
R2	0.032	0.030	0.064	0.064	0.078	0.080
N	1,042	969	969	969	851	796

Note: ***p < .01, **p < .05, *p < .1. The dependent variables are the total number of employees and the number of paid wage employees, respectively. All control variables come from the baseline survey. The list of controls include a dummy that equals one if the firm is already existing (vs 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 one 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.

Table A.8. Intention-to-treat effects on formalization and banking, detailed regression results

Variables	Formalization				Banking			
	Post		ANCOVA		Post		ANCOVA	
	All	Existing	All	Existing	All	Existing	All	Existing
Cash	0.078** (0.031)	0.090** (0.031)	0.103** (0.033)	0.103** (0.033)	0.217*** (0.046)	0.204*** (0.047)	0.209*** (0.043)	0.198*** (0.046)
Matching	0.035 (0.027)	0.036 (0.024)	0.052* (0.024)	0.052* (0.024)	0.044 (0.048)	0.032 (0.051)	0.034 (0.046)	0.020 (0.052)
Existing (=1)	0.052 (0.029)						0.391*** (0.030)	0.384*** (0.033)
Dependent variable (baseline value)			0.347*** (0.048)	0.347*** (0.048)	0.041 (0.076)	0.023 (0.101)	0.011 (0.075)	0.037 (0.092)
Industry (=1)	0.077* (0.039)	0.090* (0.040)	0.045* (0.024)	0.045* (0.024)	0.050 (0.041)		0.012 (0.038)	
Services (=1)	0.137** (0.049)	0.139** (0.054)	0.074 (0.055)	0.074 (0.055)	-0.006 (0.028)	0.007 (0.029)	0.002 (0.034)	0.022 (0.033)
Digitspan correct recalls (%)	0.000 (0.001)	0.000 (0.001)	0.000 (0.001)	0.000 (0.001)	0.011 (0.022)	0.006 (0.024)	-0.009 (0.029)	-0.009 (0.023)
Female (=1)	-0.041 (0.030)	-0.048 (0.033)	-0.022 (0.022)	-0.022 (0.022)	0.001 (0.001)	0.001 (0.001)	0.001 (0.000)	0.001 (0.001)
Age in 2018	0.001 (0.001)	0.001 (0.001)	0.000 (0.001)	0.000 (0.001)	-0.098* (0.043)	-0.099* (0.045)	-0.094** (0.036)	-0.086* (0.042)
# work experience	0.000 (0.000)	0.000 (0.001)	0.000 (0.001)	0.000 (0.001)	0.001 (0.001)	-0.000 (0.002)	-0.000 (0.001)	-0.001 (0.001)
Muslim (=1)	0.099* (0.050)	0.084* (0.040)	0.109* (0.047)	0.109* (0.047)	-0.001 (0.001)	-0.001 (0.001)	-0.002 (0.001)	-0.002 (0.001)
Christian (=1)	0.075 (0.058)	0.055 (0.046)	0.084 (0.056)	0.084 (0.056)	0.052 (0.119)	0.025 (0.122)	0.061 (0.128)	0.043 (0.133)

Table A.8 continued

Variables	Formalization				Banking			
	Post		ANCOVA		Post		ANCOVA	
	All	Existing	All	Existing	All	Existing	All	Existing
Bissa ethnic group (=1)	-0.082 (0.061)	-0.086 (0.065)	-0.069 (0.058)	-0.069 (0.058)	0.076 (0.109)	0.064 (0.108)	0.095 (0.115)	0.089 (0.118)
Mossi ethnic group (=1)	-0.033 (0.078)	-0.044 (0.075)	-0.050 (0.071)	-0.050 (0.071)	0.005 (0.051)	0.017 (0.056)	0.034 (0.040)	0.040 (0.042)
Read/write French and a local language (=1)	0.063 (0.043)	0.067 (0.044)	0.040 (0.038)	0.040 (0.038)	-0.007 (0.061)	0.009 (0.070)	-0.010 (0.049)	-0.001 (0.060)
Read/write a local language only (=1)	-0.023 (0.033)	-0.022 (0.035)	-0.027 (0.036)	-0.027 (0.036)	0.092** (0.039)	0.094** (0.038)	0.028 (0.037)	0.039 (0.032)
Primary education (=1)	-0.023 (0.020)	-0.022 (0.023)	-0.013 (0.023)	-0.013 (0.023)	0.067 (0.077)	0.072 (0.078)	0.039 (0.066)	0.052 (0.068)
Secondary education (=1)	0.034 (0.032)	0.038 (0.039)	0.053 (0.044)	0.053 (0.044)	-0.005 (0.053)	0.001 (0.053)	-0.022 (0.042)	-0.023 (0.039)
Other education (=1)	-0.002 (0.025)	0.001 (0.026)	0.009 (0.028)	0.009 (0.028)	0.019 (0.057)	0.032 (0.060)	-0.008 (0.055)	-0.021 (0.058)
# household size	-0.002 (0.001)	-0.002 (0.001)	-0.001 (0.001)	-0.001 (0.001)	0.028 (0.035)	0.035 (0.040)	0.003 (0.032)	0.009 (0.031)
Household head (=1)	0.050** (0.015)	0.044** (0.015)	0.042** (0.018)	0.042** (0.018)	-0.003 (0.002)	-0.003 (0.002)	-0.001 (0.001)	-0.001 (0.002)
# household assets	0.009*** (0.002)	0.009*** (0.002)	0.006*** (0.002)	0.006*** (0.002)	0.025 (0.038)	0.046 (0.032)	-0.002 (0.025)	0.030 (0.020)
Community leader in household (=1)	0.008 (0.019)	0.017 (0.018)	0.024 (0.017)	0.024 (0.017)	0.022*** (0.002)	0.021*** (0.002)	0.012*** (0.002)	0.012*** (0.002)

Table A.8 continued

Variables	Formalization				Banking			
	Post		ANCOVA		Post		ANCOVA	
	All	Existing	All	Existing	All	Existing	All	Existing
Block 2	0.018 (0.025)	0.017 (0.024)	0.018 (0.019)	0.018 (0.019)	-0.011 (0.025)	-0.006 (0.028)	-0.021 (0.022)	-0.014 (0.027)
Block 3	-0.028 (0.029)	-0.036 (0.033)	-0.030 (0.031)	-0.030 (0.031)	-0.015 (0.040)	-0.023 (0.045)	-0.014 (0.042)	-0.015 (0.044)
Block 4	0.017 (0.014)	0.022 (0.015)	0.028* (0.014)	0.028* (0.014)	-0.037 (0.043)	-0.047 (0.047)	-0.037 (0.039)	-0.041 (0.040)
Block 5	0.216*** (0.043)	0.195*** (0.029)	0.151*** (0.041)	0.151*** (0.041)	0.025 (0.056)	0.009 (0.057)	0.009 (0.056)	0.002 (0.053)
Block 6	0.115 (0.101)	0.052 (0.101)	0.030 (0.093)	0.030 (0.093)	0.109 (0.060)	0.115* (0.050)	0.035 (0.041)	0.042 (0.030)
Block 7	-0.014 (0.101)	0.015 (0.109)	0.002 (0.088)	0.002 (0.088)	-0.019 (0.105)	-0.071 (0.153)	-0.031 (0.121)	-0.056 (0.166)
Constant	-0.233** (0.091)	-0.161 (0.092)	-0.166 (0.098)	-0.166 (0.098)	0.245* (0.128)	0.356** (0.136)	0.227 (0.131)	0.280* (0.141)
S.E. clustered	Municipality	Municipality	Municipality	Municipality	Municipality	Municipality	Municipality	Municipality
R2	0.142	0.145	0.218	0.218	0.168	0.172	0.313	0.310
N	1,042	969	969	969	1,107	970	1,107	970

Note: ***p < .01, **p < .05, *p < .1. Formalization and banking are dummies; each equals one if the firm is formalized or owns a financial account. All control variables come from the baseline survey. The list of controls includes a dummy that equals one if the firm is already existing (vs 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 one 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.

Table A.9. Intention-to-treat effects on bookkeeping and innovation, detailed regression results

Variables	Bookkeeping				Innovation			
	Post		ANCOVA		Post		ANCOVA	
	All	Existing	All	Existing	All	Existing	All	Existing
Cash	0.102** (0.044)	0.123** (0.046)	0.131** (0.046)	0.131** (0.046)	0.182** (0.059)	0.182** (0.075)	0.196* (0.087)	0.196* (0.087)
Matching	0.006 (0.030)	0.022 (0.039)	0.026 (0.038)	0.026 (0.038)	0.151* (0.067)	0.160* (0.078)	0.124 (0.079)	0.124 (0.079)
Existing (=1)	0.046 (0.058)				0.138 (0.121)			
Dependent variable (baseline value)			0.175*** (0.042)	0.175*** (0.042)			0.211* (0.095)	0.211* (0.095)
Industry (=1)	0.067* (0.031)	0.074** (0.031)	0.071** (0.029)	0.071** (0.029)	0.228 (0.134)	0.209 (0.145)	0.096 (0.088)	0.096 (0.088)
Services (=1)	0.041 (0.039)	0.034 (0.043)	0.028 (0.037)	0.028 (0.037)	0.271** (0.113)	0.248** (0.101)	0.167 (0.095)	0.167 (0.095)
Digitspan correct recalls (%)	0.002** (0.001)	0.002** (0.001)	0.002* (0.001)	0.002* (0.001)	0.005* (0.002)	0.005 (0.003)	0.003 (0.003)	0.003 (0.003)
Female (=1)	-0.054 (0.036)	-0.068 (0.041)	-0.062* (0.033)	-0.062* (0.033)	-0.073 (0.064)	-0.024 (0.079)	-0.009 (0.091)	-0.009 (0.091)
Age in 2018	-0.002* (0.001)	-0.002* (0.001)	-0.002 (0.001)	-0.002 (0.001)	-0.005 (0.003)	-0.005 (0.003)	-0.006 (0.004)	-0.006 (0.004)
# work experience	-0.003* (0.001)	-0.003* (0.001)	-0.003* (0.001)	-0.003* (0.001)	-0.001 (0.003)	-0.001 (0.003)	0.002 (0.003)	0.002 (0.003)
Muslim (=1)	-0.190 (0.127)	-0.148 (0.114)	-0.185 (0.120)	-0.185 (0.120)	0.072 (0.211)	0.041 (0.245)	0.051 (0.250)	0.051 (0.250)
Christian (=1)	-0.162 (0.124)	-0.120 (0.109)	-0.150 (0.118)	-0.150 (0.118)	-0.089 (0.209)	-0.141 (0.245)	-0.150 (0.250)	-0.150 (0.250)

Table A.9 continued

Variables	Bookkeeping				Innovation			
	Post		ANCOVA		Post		ANCOVA	
	All	Existing	All	Existing	All	Existing	All	Existing
Bissa ethnic group (=1)	-0.023 (0.043)	0.005 (0.047)	0.048 (0.047)	0.048 (0.047)	-0.038 (0.109)	-0.074 (0.123)	-0.027 (0.116)	-0.027 (0.116)
Mossi ethnic group (=1)	0.050 (0.039)	0.076 (0.053)	0.105* (0.053)	0.105* (0.053)	-0.044 (0.107)	-0.117 (0.095)	-0.086 (0.082)	-0.086 (0.082)
Read/write French and a local language (=1)	0.168** *	0.133**	0.102*	0.102*	-0.000 (0.088)	-0.030 (0.105)	-0.024 (0.106)	-0.024 (0.106)
Read/write a local language only (=1)	0.098 (0.065)	0.101 (0.072)	0.074 (0.063)	0.074 (0.063)	0.034 (0.188)	0.053 (0.189)	0.088 (0.225)	0.088 (0.225)
Primary education (=1)	-0.030 (0.041)	0.000 (0.040)	-0.001 (0.039)	-0.001 (0.039)	0.151 (0.135)	0.200 (0.152)	0.165 (0.158)	0.165 (0.158)
Secondary education (=1)	0.037 (0.054)	0.060 (0.068)	0.052 (0.064)	0.052 (0.064)	0.226 (0.129)	0.292** (0.113)	0.210* (0.100)	0.210* (0.100)
Other education (=1)	0.022 (0.039)	0.015 (0.040)	0.005 (0.036)	0.005 (0.036)	0.064 (0.088)	0.069 (0.088)	0.011 (0.086)	0.011 (0.086)
# household size	-0.003 (0.002)	-0.003 (0.002)	-0.003 (0.002)	-0.003 (0.002)	-0.004 (0.003)	-0.003 (0.004)	-0.003 (0.004)	-0.003 (0.004)
Household head (=1)	0.050 (0.050)	0.054 (0.056)	0.050 (0.051)	0.050 (0.051)	0.041 (0.112)	0.045 (0.112)	-0.013 (0.097)	-0.013 (0.097)
# household assets	0.008** (0.003)	0.009** (0.003)	0.006 (0.003)	0.006 (0.003)	0.019** (0.006)	0.023*** (0.007)	0.020** (0.008)	0.020** (0.008)
Community leader in household (=1)	0.075* (0.033)	0.082* (0.037)	0.076* (0.036)	0.076* (0.036)	-0.048 (0.040)	-0.041 (0.039)	-0.033 (0.052)	-0.033 (0.052)

Table A.9 continued

Variables	Bookkeeping				Innovation			
	Post		ANCOVA		Post		ANCOVA	
	All	Existing	All	Existing	All	Existing	All	Existing
Block 2	0.002 (0.029)	0.002 (0.034)	0.002 (0.040)	0.002 (0.040)	-0.018 (0.058)	-0.028 (0.066)	0.001 (0.083)	0.001 (0.083)
Block 3	0.054 (0.047)	0.035 (0.051)	0.035 (0.059)	0.035 (0.059)	-0.080 (0.085)	-0.081 (0.089)	-0.050 (0.078)	-0.050 (0.078)
Block 4	0.074 (0.053)	0.068 (0.064)	0.057 (0.065)	0.057 (0.065)	0.052 (0.134)	0.018 (0.149)	0.047 (0.144)	0.047 (0.144)
Block 5	0.071 (0.075)	0.027 (0.099)	0.023 (0.095)	0.023 (0.095)	0.223 (0.226)	0.013 (0.144)	-0.007 (0.121)	-0.007 (0.121)
Block 6	-0.042 (0.122)	0.008 (0.150)	0.015 (0.140)	0.015 (0.140)	0.188 (0.215)	0.206 (0.259)	0.193 (0.358)	0.193 (0.358)
Block 7	0.073 (0.120)	0.099 (0.163)	0.071 (0.169)	0.071 (0.169)	0.186 (0.167)	0.294* (0.132)	0.290** (0.111)	0.290** (0.111)
Constant	0.320* (0.172)	0.315* (0.151)	0.295* (0.151)	0.295* (0.151)	-0.415 (0.356)	-0.307 (0.310)	-0.207 (0.315)	-0.207 (0.315)
S.E. clustered	Municipality	Municipality						
R2	0.157	0.151	0.173	0.173	0.085	0.089	0.127	0.127
N	1,042	969	969	969	1,009	937	913	913

Note: *** $p < .01$, ** $p < .05$, * $p < .1$. Bookkeeping is a dummy that equals one if the firm keeps books on sales and purchases. Innovation is an index, that is, the first component from a principal component analysis applied on 15 dummies each taking the value 1 if the firm introduced either of the 15 listed innovations. All control variables come from the baseline survey. The list of controls include a dummy that equals one if the firm is already existing (vs 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 one 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.

Table A.10. Spillover effects on profits stemming from cash beneficiaries, detailed regression results

Variables	500 m				2 km			
	Post		ANCOVA		Post		ANCOVA	
	All	Existing	All	Existing	All	Existing	All	Existing
Cash	-0.053 (0.032)	-0.056 (0.034)	-0.028 (0.028)	-0.028 (0.028)	-0.064 (0.048)	-0.063 (0.053)	-0.032 (0.041)	-0.032 (0.041)
Cash*#Cash within a radius of ...	-0.002 (0.001)	-0.002 (0.002)	-0.004** (0.002)	-0.004** (0.002)	-0.000 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)
Matching	-0.055* (0.029)	-0.056* (0.029)	-0.032 (0.038)	-0.032 (0.038)	-0.077* (0.034)	-0.077* (0.037)	-0.056 (0.046)	-0.056 (0.046)
Matching*#Cash within a radius of ...	-0.001 (0.002)	-0.001 (0.002)	-0.002 (0.002)	-0.002 (0.002)	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)
#Cash within a radius of ...	-0.001 (0.001)	-0.001 (0.001)	-0.000 (0.001)	-0.000 (0.001)	-0.001 (0.001)	-0.001 (0.002)	-0.000 (0.001)	-0.000 (0.001)
Dependent variable (baseline value)			0.206*** (0.032)	0.206*** (0.032)			0.206*** (0.033)	0.206*** (0.033)
Industrie (=1)	-0.120*** (0.020)	-0.138*** (0.026)	-0.111*** (0.024)	-0.111*** (0.024)	-0.119*** (0.018)	-0.136*** (0.025)	-0.110*** (0.023)	-0.110*** (0.023)
Services (=1)	-0.061 (0.052)	-0.069 (0.056)	-0.040 (0.054)	-0.040 (0.054)	-0.061 (0.051)	-0.068 (0.055)	-0.040 (0.053)	-0.040 (0.053)
Digitspan correct recalls (%)	0.002* (0.001)	0.002* (0.001)	0.002 (0.001)	0.002 (0.001)	0.002* (0.001)	0.002* (0.001)	0.002 (0.001)	0.002 (0.001)
Female (=1)	-0.067** (0.028)	-0.072** (0.026)	-0.062** (0.023)	-0.062** (0.023)	-0.062** (0.026)	-0.069** (0.024)	-0.059** (0.022)	-0.059** (0.022)
Age in 2018	0.001 (0.001)							
# work experience	-0.003** (0.001)	-0.003** (0.001)	-0.002** (0.001)	-0.002** (0.001)	-0.003** (0.001)	-0.003** (0.001)	-0.002** (0.001)	-0.002** (0.001)
Muslim (=1)	0.066 (0.037)	0.048 (0.037)	0.034 (0.036)	0.034 (0.036)	0.063 (0.038)	0.042 (0.035)	0.030 (0.036)	0.030 (0.036)
Christian (=1)	0.052 (0.049)	0.040 (0.053)	0.022 (0.049)	0.022 (0.049)	0.050 (0.048)	0.034 (0.051)	0.018 (0.050)	0.018 (0.050)

Table A.10 continued

Variables	500 m				2 km			
	Post		ANCOVA		Post		ANCOVA	
	All	Existing	All	Existing	All	Existing	All	Existing
Bissa ethnic group (=1)	-0.076*	-0.080*	-0.053*	-0.053*	-0.079*	-0.085*	-0.055*	-0.055*
	(0.036)	(0.040)	(0.029)	(0.029)	(0.034)	(0.040)	(0.028)	(0.028)
Mossi ethnic group (=1)	-0.036	-0.039	-0.019	-0.019	-0.032	-0.035	-0.015	-0.015
	(0.047)	(0.054)	(0.037)	(0.037)	(0.046)	(0.052)	(0.036)	(0.036)
Read/write French and a local language (=1)	-0.063	-0.051	-0.047	-0.047	-0.066	-0.053	-0.049	-0.049
	(0.044)	(0.050)	(0.048)	(0.048)	(0.043)	(0.049)	(0.047)	(0.047)
Read/write a local language only (=1)	-0.061	-0.071*	-0.058*	-0.058*	-0.064	-0.075*	-0.062*	-0.062*
	(0.042)	(0.038)	(0.028)	(0.028)	(0.043)	(0.039)	(0.030)	(0.030)
Primary education (=1)	0.068**	0.055	0.053	0.053	0.074**	0.061*	0.057	0.057
	(0.028)	(0.033)	(0.036)	(0.036)	(0.027)	(0.032)	(0.036)	(0.036)
Secondary education (=1)	0.017	0.006	0.020	0.020	0.023	0.012	0.023	0.023
	(0.057)	(0.059)	(0.064)	(0.064)	(0.058)	(0.059)	(0.066)	(0.066)
Other education (=1)	0.064**	0.062**	0.057***	0.057***	0.065**	0.064*	0.058**	0.058**
	(0.024)	(0.026)	(0.015)	(0.015)	(0.026)	(0.028)	(0.017)	(0.017)
# household size	0.001	0.001	-0.001	-0.001	0.001	0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Household head (=1)	0.028	0.014	0.003	0.003	0.030	0.016	0.003	0.003
	(0.023)	(0.017)	(0.015)	(0.015)	(0.024)	(0.018)	(0.016)	(0.016)
# household assets	0.008**	0.009**	0.005	0.005	0.008**	0.009**	0.005	0.005
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Community leader in household (=1)	0.002	-0.004	-0.010	-0.010	0.003	-0.003	-0.008	-0.008
	(0.009)	(0.011)	(0.009)	(0.009)	(0.009)	(0.011)	(0.009)	(0.009)

Table A.10 continued

Variables	500 m				2 km			
	Post		ANCOVA		Post		ANCOVA	
	All	Existing	All	Existing	All	Existing	All	Existing
Block 2	-0.011 (0.028)	-0.009 (0.031)	-0.010 (0.031)	-0.010 (0.031)	-0.014 (0.028)	-0.012 (0.030)	-0.014 (0.031)	-0.014 (0.031)
Block 3	-0.010 (0.032)	-0.013 (0.034)	-0.028 (0.031)	-0.028 (0.031)	-0.015 (0.032)	-0.018 (0.035)	-0.033 (0.032)	-0.033 (0.032)
Block 4	0.104 (0.069)	0.121 (0.071)	0.078 (0.051)	0.078 (0.051)	0.103 (0.070)	0.119 (0.073)	0.076 (0.053)	0.076 (0.053)
Block 5	0.160* (0.083)	0.179* (0.092)	0.077 (0.049)	0.077 (0.049)	0.162* (0.086)	0.182* (0.094)	0.081 (0.050)	0.081 (0.050)
Block 6	0.209 (0.154)	0.223 (0.157)	0.223 (0.155)	0.223 (0.155)	0.214 (0.153)	0.226 (0.154)	0.227 (0.152)	0.227 (0.152)
Block 7	-0.059 (0.039)	-0.076 (0.052)	-0.063 (0.048)	-0.063 (0.048)	-0.062 (0.039)	-0.080 (0.052)	-0.067 (0.050)	-0.067 (0.050)
Constant	0.169 (0.106)	0.194 (0.113)	0.183 (0.100)	0.183 (0.100)	0.173 (0.114)	0.203 (0.124)	0.184 (0.111)	0.184 (0.111)
S.E. clustered	Municipality							
R2	0.133	0.142	0.175	0.175	0.130	0.140	0.172	0.172
N	1,024	952	940	940	1,024	952	940	940

Table A.10. *continued*

Variables	5 km			
	Post		ANCOVA	
	All	Existing	All	Existing
Cash	-0.135*** (0.035)	-0.132*** (0.035)	-0.105*** (0.028)	-0.105*** (0.028)
Cash*#Cash within a radius of ...	0.002** (0.001)	0.002** (0.001)	0.001** (0.000)	0.001** (0.000)
Matching	-0.097** (0.032)	-0.095** (0.034)	-0.079 (0.046)	-0.079 (0.046)
Matching*#Cash within a radius of ...	0.001* (0.000)	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)
#Cash within a radius of ...	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)
Dependent variable (baseline value)			0.205*** (0.030)	0.205*** (0.030)
Industrie (=1)	-0.123*** (0.019)	-0.139*** (0.025)	-0.113*** (0.024)	-0.113*** (0.024)
Services (=1)	-0.063 (0.050)	-0.068 (0.054)	-0.040 (0.051)	-0.040 (0.051)
Digitspan correct recalls (%)	0.002* (0.001)	0.002* (0.001)	0.002 (0.001)	0.002 (0.001)
Female (=1)	-0.064* (0.030)	-0.070** (0.027)	-0.059** (0.025)	-0.059** (0.025)
Age in 2018	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)
# work experience	-0.003** (0.001)	-0.003** (0.001)	-0.002** (0.001)	-0.002** (0.001)
Muslim (=1)	0.070* (0.036)	0.052 (0.037)	0.041 (0.037)	0.041 (0.037)
Christian (=1)	0.055 (0.048)	0.044 (0.052)	0.028 (0.051)	0.028 (0.051)
Bissa ethnic group (=1)	-0.073* (0.039)	-0.081 (0.046)	-0.045 (0.032)	-0.045 (0.032)
Mossi ethnic group (=1)	-0.035 (0.044)	-0.036 (0.050)	-0.017 (0.034)	-0.017 (0.034)
Read/write French and a local language (=1)	-0.064 (0.044)	-0.052 (0.049)	-0.048 (0.048)	-0.048 (0.048)
Read/write a local language only (=1)	-0.064 (0.043)	-0.074* (0.039)	-0.060* (0.031)	-0.060* (0.031)
Primary education (=1)	0.075** (0.026)	0.063* (0.030)	0.057 (0.035)	0.057 (0.035)
Secondary education (=1)	0.023 (0.056)	0.013 (0.058)	0.022 (0.065)	0.022 (0.065)

Table A.10 continued

Variables	5 km			
	Post		ANCOVA	
	All	Existing	All	Existing
Other education (=1)	0.066** (0.025)	0.064** (0.026)	0.058*** (0.016)	0.058*** (0.016)
# household size	0.001 (0.001)	0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)
Household head (=1)	0.029 (0.025)	0.016 (0.019)	0.004 (0.017)	0.004 (0.017)
# household assets	0.008** (0.003)	0.009** (0.003)	0.005 (0.003)	0.005 (0.003)
Community leader in household (=1)	0.004 (0.010)	-0.003 (0.012)	-0.007 (0.010)	-0.007 (0.010)
Block 2	-0.014 (0.029)	-0.011 (0.031)	-0.013 (0.031)	-0.013 (0.031)
Block 3	-0.016 (0.035)	-0.019 (0.038)	-0.033 (0.035)	-0.033 (0.035)
Block 4	0.103 (0.069)	0.119 (0.072)	0.076 (0.053)	0.076 (0.053)
Block 5	0.162* (0.087)	0.182* (0.095)	0.076 (0.047)	0.076 (0.047)
Block 6	0.216 (0.154)	0.229 (0.155)	0.231 (0.151)	0.231 (0.151)
Block 7	-0.058 (0.042)	-0.077 (0.055)	-0.060 (0.055)	-0.060 (0.055)
Constant	0.185 (0.106)	0.216 (0.117)	0.189 (0.106)	0.189 (0.106)
S.E. clustered	Municipality	Municipality	Municipality	Municipality
R2	0.132	0.141	0.173	0.173
N	1,024	952	940	940

Note: *** $p < .01$, ** $p < .05$, * $p < .1$. Profit has been trimmed at the 99th percentile. All control variables come from the baseline survey. The list of controls include a dummy that equals one if the firm is already existing (vs 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 one 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.

Table A.11. Effects of COVID-19 on decline of production, sales, investment, employment, and general performance, detailed regression results

Variables	Production		Sales		Investments		Employment		Performance	
	All	Existing	All	Existing	All	Existing	All	Existing	All	Existing
Cash	-0.123*	-0.081	-0.109	-0.064	-0.280***	-0.268***	0.035	0.060	-0.196*	-0.181*
	(0.067)	(0.060)	(0.094)	(0.087)	(0.091)	(0.075)	(0.246)	(0.267)	(0.107)	(0.092)
Matching	0.075	0.103	0.081	0.087	-0.024	0.019	0.093	0.063	-0.035	-0.011
	(0.074)	(0.077)	(0.094)	(0.096)	(0.115)	(0.118)	(0.196)	(0.166)	(0.105)	(0.106)
Existing (=1)	0.639***		0.868***		0.621***		0.243		0.520***	
	(0.113)		(0.148)		(0.153)		(0.331)		(0.135)	
Industrie (=1)	0.557***	0.547***	0.383***	0.372***	0.117	0.097	0.698***	0.752***	0.180	0.163
	(0.073)	(0.061)	(0.053)	(0.050)	(0.134)	(0.149)	(0.227)	(0.229)	(0.156)	(0.167)
Services (=1)	0.421***	0.402***	0.364***	0.343***	0.243*	0.226*	0.916***	0.878***	0.142	0.099
	(0.124)	(0.123)	(0.079)	(0.090)	(0.130)	(0.136)	(0.230)	(0.209)	(0.134)	(0.132)
Digitspan correct recalls (%)	0.009***	0.007***	0.006***	0.005***	0.003	0.001	-0.002	-0.001	0.004***	0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.006)	(0.006)	(0.001)	(0.002)
Female (=1)	-0.099	-0.043	-0.051	-0.061	0.052	0.037	0.223	0.386	0.093	0.066
	(0.096)	(0.078)	(0.109)	(0.061)	(0.165)	(0.154)	(0.354)	(0.436)	(0.134)	(0.123)
Age in 2018	-0.001	-0.005	0.001	-0.002	0.003	0.002	-0.006	-0.011	0.001	-0.001
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)	(0.011)	(0.011)	(0.004)	(0.004)
# work experience	-0.001	0.003	-0.007*	-0.004	0.003	0.002	0.005	0.013	-0.003	-0.004
	(0.005)	(0.005)	(0.004)	(0.003)	(0.006)	(0.006)	(0.011)	(0.010)	(0.004)	(0.005)
Muslim (=1)	1.324**	1.313**	1.378**	1.357**	4.162***	4.184***	3.707***	3.789***	4.115***	4.215***
	(0.568)	(0.597)	(0.618)	(0.642)	(0.225)	(0.228)	(0.298)	(0.346)	(0.286)	(0.365)
Christian (=1)	1.266**	1.218**	1.278**	1.258**	4.239***	4.267***	3.725***	3.771***	4.164***	4.289***
	(0.539)	(0.559)	(0.579)	(0.603)	(0.243)	(0.235)	(0.286)	(0.263)	(0.285)	(0.307)
Bissa ethnic group (=1)	-0.348***	-0.436***	-0.423***	-0.468**	-0.300**	-0.262	-0.056	0.041	-0.290***	-0.285***
	(0.083)	(0.123)	(0.140)	(0.201)	(0.135)	(0.175)	(0.123)	(0.136)	(0.106)	(0.105)
Mossi ethnic group (=1)	-0.208*	-0.263*	-0.101	-0.120	-0.415**	-0.395**	-0.122	-0.057	-0.301*	-0.306**
	(0.116)	(0.154)	(0.115)	(0.156)	(0.161)	(0.171)	(0.227)	(0.275)	(0.169)	(0.154)

Table A.11 continued

Variables	Production		Sales		Investments		Employment		Performance	
	All	Existing	All	Existing	All	Existing	All	Existing	All	Existing
Read/write French and a local language (=1)	-0.251*	-0.319***	-0.226	-0.280***	-0.160	-0.166	0.511**	0.390*	-0.112	-0.130
	(0.136)	(0.100)	(0.149)	(0.095)	(0.145)	(0.128)	(0.207)	(0.215)	(0.111)	(0.117)
Read/write a local language only (=1)	-0.015	-0.112	0.332***	0.229	0.177	0.227	-0.170	-0.099	0.292	0.231
	(0.166)	(0.140)	(0.105)	(0.142)	(0.221)	(0.226)	(0.446)	(0.458)	(0.283)	(0.273)
Primary education (=1)	0.057	0.133	0.205*	0.262**	0.337***	0.355***	-0.497**	-0.371**	0.347**	0.394***
	(0.155)	(0.166)	(0.121)	(0.134)	(0.096)	(0.084)	(0.196)	(0.179)	(0.142)	(0.151)
Secondary education (=1)	0.021	0.192	0.162	0.295*	0.424***	0.494***	-0.488	-0.242	0.295**	0.383***
	(0.179)	(0.164)	(0.180)	(0.163)	(0.116)	(0.125)	(0.336)	(0.285)	(0.146)	(0.123)
Other education (=1)	0.116	0.169	0.071	0.097	-0.004	0.036	0.043	0.010	-0.163	-0.094
	(0.103)	(0.128)	(0.124)	(0.110)	(0.140)	(0.144)	(0.188)	(0.167)	(0.117)	(0.135)
# household size	-0.009	-0.009	-0.001	-0.002	0.015**	0.013*	0.010	0.011	0.019**	0.018*
	(0.007)	(0.007)	(0.009)	(0.010)	(0.007)	(0.007)	(0.009)	(0.010)	(0.009)	(0.010)
Household head (=1)	-0.027	-0.006	0.023	-0.001	0.117	0.092	0.027	0.057	0.207	0.180
	(0.069)	(0.083)	(0.109)	(0.124)	(0.150)	(0.149)	(0.195)	(0.249)	(0.175)	(0.164)
# household assets	0.006	0.003	0.013	0.008	0.000	-0.006	-0.001	0.011	-0.006	-0.016***
	(0.007)	(0.009)	(0.012)	(0.013)	(0.007)	(0.006)	(0.017)	(0.020)	(0.005)	(0.006)
Community leader in household (=1)	0.079	0.108	0.013	0.042	-0.004	-0.015	-0.059	-0.018	0.096	0.126
	(0.059)	(0.072)	(0.098)	(0.104)	(0.103)	(0.106)	(0.208)	(0.225)	(0.098)	(0.083)

Table A.11 continued

Variables	Production		Sales		Investments		Employment		Performance	
	All	Existing	All	Existing	All	Existing	All	Existing	All	Existing
Block 2	-0.112 (0.160)	-0.083 (0.165)	-0.091 (0.101)	-0.080 (0.114)	0.093 (0.107)	0.064 (0.109)	0.292 (0.256)	0.322 (0.262)	0.092 (0.089)	0.042 (0.093)
Block 3	-0.055 (0.214)	-0.061 (0.227)	-0.166 (0.180)	-0.194 (0.194)	-0.139 (0.223)	-0.192 (0.220)	0.199 (0.204)	0.127 (0.236)	-0.173 (0.244)	-0.239 (0.239)
Block 4	-0.106 (0.229)	-0.143 (0.262)	-0.185 (0.193)	-0.207 (0.221)	0.030 (0.230)	0.036 (0.255)			-0.009 (0.174)	0.003 (0.204)
Block 5	-0.109 (0.169)	-0.176 (0.254)	0.028 (0.232)	-0.020 (0.331)	0.425 (0.268)	0.533** (0.243)	0.278 (0.459)	0.344 (0.449)	0.353 (0.228)	0.459** (0.216)
Block 6	0.221* (0.119)	0.703* (0.382)	0.261 (0.258)	0.488 (0.400)	0.105 (0.379)	0.203 (0.392)	0.324 (0.431)	0.378 (0.525)	-0.239 (0.399)	-0.173 (0.412)
Block 7	0.057 (0.340)	0.074 (0.392)	-0.073 (0.289)	-0.130 (0.305)	0.337 (0.308)	0.290 (0.482)	0.556** (0.251)	0.837*** (0.301)	0.206 (0.302)	0.169 (0.456)
Constant	-1.723*** (0.651)	-0.895 (0.648)	-1.885*** (0.682)	-0.777 (0.643)	-5.941*** (0.482)	-5.176*** (0.345)	-6.376*** (0.777)	-6.570*** (0.576)	-5.573*** (0.476)	-4.892*** (0.475)
S.E. clustered	Municipality	Municipality	Municipality	Municipality	Municipality	Municipality	Municipality	Municipality	Municipality	Municipality
Pseudo R2	0.068	0.058	0.08	0.054	0.056	0.044	0.121	0.132	0.053	0.046
Observations	1,107	970	1,107	970	1,107	970	998	877	1,107	970

Note: ***p < .01, **p < .05, *p < .1. Each of the dependent variables, production, sales, investment, employment, and general performance is a dummy that equals one if it decreased for the firm due to COVID-19. The list of controls includes a dummy that equals one if the firm is already existing (vs 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 one 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.

References

- Banerjee, A., and E. Duflo. 2004. "Do Firms Want to Borrow More? Testing Credit Constraints Using a Directed Lending Program." CEPR Discussion Paper 4681.
- Banerjee, A., and E. Duflo. 2011. *Poor Economics*. London: Penguin Books.
- Banerjee, A., D. Karlan, and J. Zinman. 2015. "Six Randomized Evaluations of Microcredit: Introduction and Further Steps." *American Economic Journal: Applied Economics* 7 (1): 1–21.
- Banerjee, A., E. Breza, E. Duflo, and C. Kinnan. 2019. "Can Microfinance Unlock a Poverty Trap for Some Entrepreneurs?" NBER Working Paper No. w26346.
- Blattman, C., E. P. Green, J. Jamison, M. C. Lehmann, and J. Annan. 2016. "The Returns to Microenterprise Support among the Ultrapoor: A Field Experiment in Postwar Uganda." *American Economic Journal: Applied Economics* 8 (2): 35–64.
- Burbidge, J. B., L. Magee, and A. L. Robb. 1988. "Alternative Transformations to Handle Extreme Values of the Dependent Variable." *Journal of the American Statistical Association* 83 (401): 123–127.
- Campos, F., A. Coville, A. M. Fernandes, M. Goldstein, and D. McKenzie. 2012. "Learning from the Experiments That Never Happened: Lessons from Trying to Conduct Randomized Evaluations of Matching Grant Programs in Africa." Policy Research Working Paper 6296, World Bank, Washington, DC.
- Cho, Y., and M. Honorati. 2014. "Entrepreneurship Programs in Developing Countries: A Meta Regression Analysis." *Labour Economics* 28 (C): 110–130.
- Crépon, B., M. El Komi, and A. Osman. 2020. "Is It Who You Are or What You Get? Comparing the Impacts of Loans and Grants for Microenterprise Development." Poverty Action Lab, MIT.
- de Mel, S., D. McKenzie, and C. Woodruff. 2008. "Returns to Capital in Microenterprises: Evidence from a Field Experiment." *Quarterly Journal of Economics* 123 (4): 1329–1372.
- Fafchamps, M., and C. Woodruff. 2017. "Identifying Gazelles: Expert Panels vs. Surveys as a Means to Identify Firms with Rapid Growth Potential." *World Bank Economic Review* 31 (3): 670–686.
- Fafchamps, M., D. McKenzie, S. Quinn, and C. Woodruff. 2014. "Microenterprise Growth and the Flypaper Effect: Evidence from a Randomized Experiment in Ghana." *Journal of Development Economics* 106 (C): 211–226.
- Fiala, N. 2018. "Returns to Microcredit, Cash Grants and Training for Male and Female Microentrepreneurs in Uganda." *World Development* 105 (C): 189–200.
- Frison, L., and S. J. Pocock. 1992. "Repeated Measures in Clinical Trials: Analysis Using Mean Summary Statistics and Its Implications for Design." *Statistics in Medicine* 11 (13): 1685–1704.
- Grimm, M., and A. L. Paffhausen. 2015. "Do Interventions Targeted at Micro-Entrepreneurs and Small and Medium-Sized Firms Create Jobs? A Systematic Review of the Evidence for Low and Middle Income Countries." *Labour Economics* 32: 67–85.
- Grimm, M., J. Krüger, and J. Lay. 2011. "Barriers to Entry and Returns to Capital in Informal Activities: Evidence from Sub-Saharan Africa." *Review of Income and Wealth* 57: S27–S53.

- Grimm, M., S. Soubeiga, and M. Weber. 2020. *Matching or Cash Grants for Entrepreneurs: What Is More Effective? Experimental Evidence from the Bagré Growth Pole Project in Burkina Faso*. World Bank, Washington, DC.
- Hristova, D., and A. Coste. 2016. "How to Make Grants a Better Match for Private Sector Development." World Bank, Washington, DC. <https://openknowledge.worldbank.org/handle/10986/26434>.
- Karlan, D., R. Knight, and C. Udry. 2015. "Consulting and Capital Experiments with Microenterprise Tailors in Ghana." *Journal of Economic Behavior & Organization* 118 (C): 281–302.
- Kremer, M., J. N. Lee, J. M. Robinson, and O. Rostapshova. 2011. "The Return to Capital for Small Retailers in Kenya: Evidence from Inventories." Harvard University.
- McKenzie, D. 2012. "Beyond Baseline and Follow-up: The Case for More T in Experiments." *Journal of Development Economics* 99 (2): 210–221.
- McKenzie, D. 2017. "Identifying and Spurring High-Growth Entrepreneurship: Experimental Evidence from a Business Plan Competition." *American Economic Review* 107 (8): 2278–2307.
- McKenzie, D., and C. Woodruff. 2008. "Experimental Evidence on Returns to Capital and Access to Finance in Mexico." *World Bank Economic Review* 22 (3): 457–482.
- McKenzie, D. J., and C. Woodruff. 2006. "Do Entry Costs Provide an Empirical Basis for Poverty Traps? Evidence from Mexican Microenterprises." *Economic Development and Cultural Change* 55 (1): 3–42.
- Schündeln, M. 2005. "Modeling Firm Dynamics to Identify the Cost of Financing Constraints in Ghanaian Manufacturing." Proceedings of the German Development Economics Conference, Kiel 2005 29, Verein für Socialpolitik, Research Committee Development Economics.
- Udry, C., and S. Anagol. 2006. "The Return to Capital in Ghana." *American Economic Review* 96 (2): 388–393.
- Weber, M. 2018. "Burkina Faso Jobs Diagnostic: Overview and Suggestions for a Strategic Framework for Jobs." Jobs Series, No. 15. World Bank, Washington, DC.
- World Bank, 2021. Burkina Faso - Bagre Growth Pole Project. Washington, D.C. <http://documents.worldbank.org/curated/en/909471626364231329/Burkina-Faso-Bagre-Growth-Pole-Project>