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Keeping Up With the Joneses: Economic Impacts of Overconfidence in Micro-Entrepreneurs

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Keeping Up With the Joneses: Economic Impacts of Overconfidence in Micro-Entrepreneurs*

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Abstract

This paper investigates the effects of incorrect beliefs over relative firm performance on micro-firm outputs through a randomized field experiment in Mozambique. At baseline, 76% of firm owners in the bottom of the distribution are overconfident about their firm's performance. The estimates reveal that correcting these beliefs through a simple, easily scalable information experiment closes the performance gap between treated firms in the bottom of the distribution at baseline and average and top firms by almost 43%. Moreover, the treatment increases the time a firm owner allocates to her business, improves strategic cooperation with the most important business partners, and affects the pricing strategy of treated firm owners. My results suggest that incorrect beliefs about relative performance are a binding constraint to firm growth that have large implications for managerial behavior and firm outcomes.
JEL Codes: D22, D91, O12.

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1 Introduction

In a simple framework, firms are maximizing profits and have perfect knowledge of the state of the world. To grow, they require access to financial and human capital, and better technologies or skills. Yet, empirical evidence from developing countries on the impact of changes in traditional input factors shows limited success.[1] The behavioral theory of the firm[2] challenges this simple framework and allows for managerial mistakes to affect firm outcomes and internal resource allocation decisions. One source of such manager mistakes has been shown to be overconfidence bias. Over-confidence bias affects economic decision-making in a variety of contexts including managers in high-income settings with high levels of education, information, and transparency.[3]

But overconfidence bias might be particularly prevalent and relevant among small firms and in developing countries. In small and micro-firms there is typically no accountability mechanism through managers or an executive board or even employees. This opens possibilities for firm owner decisions to be prone to error without any control mechanisms. Additionally in developing countries, information constraints about one's firm's relative performance may be stronger as access to information about how well other firms do can be obtained through observation only. These observations may be noisy and affected by selection neglect. Both factors are likely to influence a firm owner's perception about how well she is doing compared to others and lead to incorrect beliefs about relative firm performance with potentially large consequences for input decisions and firm success.

The present study was designed to understand whether micro-firms in a developing country setting show overconfident beliefs and, most importantly, whether *correcting* such beliefs affects firm outcomes and firm owners' input allocation decisions. The results show that a simple information treatment, where firm owners learn about their relative rank, effectively changes their behavior and closes the performance gap between those firms

[1]See for example de Mel et al. (2014), Blattman et al. (2014), and McKenzie and Woodruff (2013).

[2]Starting with Simon (1955) the traditional models were challenged explicitly by Cyert and March (1963) who shift the focus of a theory of the firm towards the decision-making process to predict resource allocation, pricing, and firm outputs.

[3]See for example Malmendier and Tate (2015) for a detailed overview on the existing evidence of overconfidence in managers of firms.

that performed worse before the intervention and those doing relatively well by 43% after one year. Relative performance information depends on the peers an individual is exposed to such that a completely anonymous ranking might affect individuals differently from a ranking that reveals peers, or reveals partial characteristics of the same. Especially, as overconfident beliefs may be formed and affected by multiple dimensions going beyond relative performance alone. The paper thus additionally asks the question whether revealing information about peers' characteristics such as age and gender affects firm outcomes and owner behavior.

I evaluate these two ideas with a field experiment among micro-firms in urban market clusters in Mozambique. I collected baseline beliefs and detailed data on firm performance as well as management practices for 323 firms across different sectors. I then used the baseline data to construct individual rankings for each firm specific to their product sector and randomly assigned those firms to two interventions and one control group. Four months and one year after the intervention, I collected detailed data on firm outcomes and firm owner behavior such as time allocation and management practices. After four months, I additionally collected data on business network cooperation through incentivized dictator games as stronger network ties might lead to increased knowledge of better management practices. Unlike past studies, I can furthermore observe changes in price management and revenues through monitoring firms over an entire business day at endline (one year after the intervention) improving over survey reports that might be subject to measurement error and experimenter demand effects.

I document descriptive evidence of overconfident beliefs in relative firm income at baseline for 76% of firm owners in the bottom of the distribution in my sample. The experimental evidence on correcting overconfidence bias shows that exposing firm owners to information about their true relative standing significantly and largely increases firm outcomes for firms in the bottom of the distribution but has limited effects on average and top firms. The treatment increases survey reports on revenue and profits by 136% and 54%, respectively, and monitored revenues after one year by 122% compared to similar firms in the control group. While observing peer characteristics in general decreases treatment effects, observing a female firm owner at the top of the sector distribution amplifies treatment effects further.

Additionally, I find positive and strong treatment effects on three potential mechanisms: time allocation towards the business, changes in the social proximity with a firm owner's business network, and pricing strategies. Correcting overconfident beliefs leads treated firm owners in the bottom of the distribution to allocate as much time to their businesses after the intervention as those owners of average and top firms in the control group. Secondly, treated firms in the bottom of the distribution also display higher prosociality towards their most important business partner providing evidence for increases in strategic business cooperation. Third, treated firm owners increase their average prices, charging almost double of what similar firms in the control group charge.

Last, I provide suggestive evidence that correcting beliefs leads to improved management practices - likely through nudging firm owners into tighter relationships and knowledge exchange with their business partners. The results show that while treated firm owners are not necessarily more likely to have adopted more business practices, they are more likely to behave more similarly to more successful peers. They seem more likely to have similar bookkeeping and business measures as well as investment practices such as a higher demand for bank loans and higher product diversity.

This paper contributes to a recent, growing literature on the importance of behavioral constraints for small and micro-firms. Campos et al. (2017) shows that a psychology-based intervention can be much more successful in increasing firm outcomes in a low income country context than traditional business skills training that is generally found to have limited success in increasing revenues and profits (see Quinn and Woodruff (2019) and Mckenzie (2020) for recent reviews on the evidence base regarding traditional business skills interventions). Similarly, Dalton et al. (2021) show that a handbook with best practices from local retail peers is only effective in changing business practices when combined with behavioral interventions, pointing towards behavioral constraints in information diffusion, and Batista and Seither (2019) show that an intervention targeting aspirations and expectations of micro-entrepreneurs can successfully increase firm outcomes when not constrained by a goal setting intervention.

The paper furthermore contributes to a small literature on correctly measuring revenue data for micro-entrepreneurs in low-income settings that typically operate in informal settings and without reliable accounting practices. I apply and test a measure developed by Batista and Seither (2021) that improves over survey measures for small sample sizes. This is similar in spirit to Anderson et al. (2021) that introduces a new survey measure of revenues and profits, and de Mel et al. (2009) that develops a new survey tool to measure micro-firm profits.

Last, this paper relates to the literature on relative performance feedback. There is a large evidence base on how relative feedback changes effort-based performance among students and workers. The size and direction of effects depend on the setting, incentive scheme, and prospects of feedback provision. Azmat and Iriberri (2010) provide students with information on how their GPA compares to the average. This information increases later grades by 5%. Eriksson et al. (2009) find that relative performance feedback decreases worker performance. Relative performance feedback increases performance under individual incentive schemes, but deteriorates performance under a tournament scheme (Hannan et al. (2008)). Feedback in the lab increases performance only when performance is related to pay (Azmat and Iriberri (2016)). Kuhnen and Tymula (2012) find that the prospect of receiving a ranking increases effort. These studies focus on workers or students, and few provide experimental field evidence. This paper contributes to this literature by presenting field experimental evidence for firms. It furthermore analyzes the impact of peer characteristic observability on ranking effects.

The results of this study are of wider interest despite the specific setting of the field experiment. I present novel evidence that incorrect beliefs about relative firm performance are relevant to owners of micro- and small businesses when information about peer performance is likely to be scarce. This is the case in low income countries where administrative data about micro-firm performance is not available but is potentially equally relevant in advanced economies that experience high levels of informality. Even in settings where informality is low and administrative data available it is reasonable to assume that search cost for small business owners are high such that relative firm performance information is costly to obtain - giving scope for overconfident beliefs that affect managerial behavior and firm outcomes.

The remainder of this paper is organized as follows. Section 2 presents further information on the experimental setting and design, and the different treatments. The data and sample are described in detail in Section 2.2. The estimation strategy is presented in Section 3.3. Section 3.4 discusses the main experimental results, and Section 4 presents robustness checks on self-reported sales data. Section 5 concludes.

2 Setting, Design, and Treatments

Around 75% of the total employed in Sub-Saharan Africa are self-employed whereas the OECD average, for example, is 16.3%.[4] Most of the self-employed operate petty businesses with low profit margins and low survival rates. This paper studies how information constraints for these firms can lead to overconfidence bias that affects managerial behavior that can, in turn, explain the perpetuation of inequality in firm outcomes.

2.1 Setting

The study took place in the Greater Maputo region in Mozambique - specifically within the city boundaries of Matola City and Maputo City (see Figure 1). Mozambique is a country in East Africa with an abundance of natural resources but a private sector whose development is lacking behind. The country experienced conflict and social unrest since the independence war with Portugal started in 1964. The independence war ended in 1975 but was followed by 15 years of civil war between the two major political parties. Recent resurrections of civil conflict and natural disasters threatening the livelihoods of the rural population caused increased migratory flows to urban areas.

The share of self-employed workers in Mozambique in 2019 was one of the highest in

[4]World Development Indicators (2019). Source: International Labour Organization, ILOSTAT database.The indicator of status of employed in this dataset distinguishes between wage and salaried workers, and self-employed workers.

the region with 84%, as salaried work opportunities in the urban areas are scarce and many workers have to resort to self-employment. Most of the self-employed are organized in local market clusters. These market clusters are geographically confined and may be outdoors or indoors (indoor markets are more common closer to the city center). The distance between vendor stands is minimal and markets are often organized in sectors with many vendors selling or producing similar goods next to each other. Vendors in these markets pay market administration taxes but rarely pay income tax due to their low profit margins. The markets generally serve the residents living around them as the main source of food, household articles, clothes, and services as supermarkets are not affordable for the average population and stores outside of market clusters rare.

There is limited administrative information available in Mozambique about the size and structure of existing markets. To verify the representativeness of our sample for the full population of firms in the Maputo metropolitan region, we conducted a census survey. The census survey was guided by an administrative list with information which market clusters existed and rough estimates of the total number of firms in the respective market. We conducted census interviews in 76% of the markets that were located either in the city of Maputo or Matola, and had at least 100 firms listed. We excluded two informal markets whose structures implied a security risk to our enumerator teams. In total, we collected census data for 3.136 firms in 33 markets. The census data includes information about the age of the firm owner or manager, gender, nationality, and basic literacy. We collected firm-specific information on the firm's sector, prospects of remaining active in the market, and specific location instructions.

The baseline survey was conducted with 624 firm owners and managers that were randomly selected from a subset of firms that met our exclusion criteria. The exclusion criteria were critical for a field experiment described in detail in a companion paper (Batista and Seither (2019)). Excluded are firm owners older than 50 years, with a business operation horizon of less than a year, and foreigners. Additionally, we excluded all fruit/vegetable sellers, restaurants, illegal sales activities, traditional medicine, and wholesale merchants. For this study, we further excluded firms that did not provide revenue data during the baseline or that operated in sectors with less than ten competitors. We were able to locate 323 firm owners again during the intervention visit. These firms build the sample of this study.

2.2 Data

I tracked firms for approximately one and a half years after the census. During this time I collected extensive survey data, incentivized measures of pro-sociality towards an individual's main business contact, and a novel measure of revenue. The latter improves over survey data that might be subject to motivational lying. measurement errors, and experimenter demand effects.[5] In the baseline survey, I collected information on individual and household characteristics of the firm responsible, firm characteristics, business practices, and firm performance indicators.[6] The module on firm characteristics includes information about individual time allocation such as hours and days worked.

Monitored Revenues Survey measures of revenue data for micro-firms in developing countries are often subject to multiple measurement and recall errors. This is because firm owners might not have the skills, literacy, or technology to keep accounting books, and often rely on their memory to report revenue data in surveys. For this project in particular, we were furthermore concerned that survey reports would be influenced by experimenter demand effects and motivational lying if individuals care about their relative standing and reputation towards enumerators.

I thus conducted detailed data collection for the last follow-up survey[7], focusing on obtaining revenue data that improves over existing survey data measures. To do so, I monitored firm revenues over an entire business day with the team of enumerators. Survey 3 was administered as soon as possible and focused on the most important measures to not disrupt normal business activities. Each firm then had an observer sit close by the firm

[5] A detailed description and analysis of this measure can be found in Batista and Seither (2021). Details relevant to this paper can be found in this section below.

[6] The questionnaires also included a section on psychometric indicators such as aspirations and locus of control. This data was collected for a companion paper evaluating a field experiment on the role of aspirations and goal setting behavior on micro-firm performance (Batista and Seither (2019)).

[7] Due to the higher implementation cost of this measure we did not collect monitored revenues data for the baseline survey and survey 2.

owner for approximately eight hours - from 9am until 5pm which is when markets typically close in this context. At baseline, 94% of firms are open by 9am during regular weekdays and on Saturdays (the days we conducted interviews). During this time, enumerators tracked the realized sales in detail including the type of products sold, the number of units, unit price, and price charged.[8] The total price calculated as the product of number of sold units and unit price might differ from the price charged for two reasons: Firms charge less (round down) due to a lack of change,[9] or firms actually charge more than the unit price would suggest. I could not confirm whether the latter is due to miscalculations or strategic behavior. The monitored sales data allows us to obtain micro-firm revenue data with limited measurement error to estimate treatment effects, compare these estimates to survey outcomes, and additionally report treatment effects on pricing strategies.

Pro-Sociality Towards Business Partners I also measured pro-social preferences by eliciting modified dictator game decisions.[10] Individuals were asked to indicate the person they speak most with about their own or the other's business. The choice was restricted to a business person living in the Maputo metropolitan region. Choosing an actual business partner rather than playing with an anonymous counterpart was crucial to identify changes in cooperation with a business network on firm performance. I collected basic information about the relationship between our respondents and the recipients, and contact details of the recipients. The decision was implemented for the dictator on the same day and until the end of the next day for the recipient.[11] Individuals were offered 200MZN (US$3). The decision making process was illustrated with tokens and a decision board where tokens had to be distributed between oneself and the recipient (see Figure 2

[8]Enumerators furthermore collected data on client characteristics, firm owner behavior, and expenses. The approach, data, and cost-benefit analysis of this measure are described in detail in a companion paper by Batista and Seither (2021).

[9]See Beaman et al. (2014) for experimental field evidence on how a lack of change affects micro-firm performance.

[10]In total, individuals took 32 dictator decisions. The full set of dictator game decisions is to be exploited in a companion paper. Only one of the games was paid out. Which game was determined by a random draw of the individual at the time of data collection. The order of all dictator games was randomized.

[11]Asking respondents to indicate business partners often implied that recipients were part of the experimental sample. Whenever this was the case, recipients were asked to make their decisions first before receiving any payouts from other respondents. This implied that some recipients were paid later than one day after the decision was made.

for an illustration of how the dictator game was played). Each token was worth 10MZN (US$0.15). Individuals were then asked to decide whether they wanted to give all, parts, or none of the 200MZN to their colleague. The value allocated to the recipient was doubled (each token was worth US$0.30) whereas the dictator received the simple monetary value of tokens in her box (each token was worth US$0.15).[12] Due to the time cost of collecting this data I only elicited pro-social preferences at baseline and during Survey 2.

Table 1 shows baseline outcomes for firms in three categories: socioeconomic characteristics and household outcomes for the individual firm owner, individual business experience and firm characteristics, and firm outcomes as well as baseline statistics for two of the main mechanisms studied. The average age of the sampled individuals is 34 years, and 41% of them are women. On average, they have about 8 years of schooling and live in households with 5.9 household members. They spend around 7,351MZN (around US$100 in 2016) each month for household expenditures.

On average, they would leave their business and accept a salaried work position for 25,203MZN (US$350 in 2016)[13] but only 10% of the sample have ever had any formal training in their business sector. They operate their firms for a little less than eight years (including periods in which the firm was closed temporarily). In their firms, they own assets with a market value of approximately 11,797MZN (US$164 in 2016) and have less than 0.3 employees. Over two days these firms, on average, generate revenues of 3,546MZN (around US$50). The average firm owner works a little less than 10 hours a day and individuals in the sample shared 44MZN (22% of the endowment) with the recipient[14].

[12]Changing the price of giving was first proposed by Andreoni and Miller (2002). In the present study, only social preferences elicited through modified dictator games with a lower price of giving are a precise predictor of entrepreneurial success. We are interested in the impact of a treatment on changes of pro-social preferences and cooperation that might be potentially linked to firm outcomes through social learning channels. Why different modifications of the baseline game yield different relationships between pro-social preferences and firm outcomes is an open question for future research. One explanation seems to be that the standard dictator game is driven by social norms rather than actual pro-social preferences as described in List (2007).

[13]This suggests that the individuals in the sample are less likely to be subsistence entrepreneurs in line with the exclusion restriction of limiting the sample to entrepreneurs that have a business horizon of at least one year at the time of the census.

[14]The majority of existing studies finds that individuals share around 20% of their endowment with their peers.

Table 1 furthermore reports the main randomization check using baseline survey data. Treatment was randomized individually and stratified by gender. The treatment group includes all individuals that have received ranking information (independent of whether they have received additional information on peer characteristics). The first set of measures checks for balance along select covariates related to individual characteristics of firm owners. The second set of measures checks for balance of the key outcome measures at baseline. Treatment and control are imbalanced in only two of the 13 baseline characteristics (household size and number of employees).

Who Are the Low-Performers? To understand what distinguishes low-performers from their more successful peers at baseline I present differences in key characteristics in Table 2. Low-Performers are defined as individuals whose firm performance at baseline falls into the bottom of the distribution of their respective sector. Specifically, I define the cut-off at the 40th percentile. Firms below the 50th percentile receive a clear signal that they perform worse than the average firm. I compare these individuals to all other individuals - including those with median and above performance at baseline.

Respondents in our sample are of similar age, with low-performers being slightly younger (though not statistically significantly). There are more women in the bottom of the distribution than men but there is no difference in household size. Low-performers have the same amount of years of education and probability of having received formal business training compared to their peers. Firms in the bottom of the distribution are significantly younger. They exist for around 6.4 years whereas their peers opened firms 8.6 years before our baseline survey visit. This indicates that although individuals with low-performing firms have the same level of formal education they have 2 years less of business experience. The lack of business experience could proxy for low-performers having worse firm networks and business practices, misjudging the return to capital and effort, or having a smaller client base of regular customers.[15]

[15] In a separate regression I estimate the non-causal relationships between years of business experience and several key outcomes that might be relevant for firm performance (available upon request). There is no statistically significant or economically relevant relationship between business experience and bookkeeping or the calculation of business measures. There is some indication that more business experience leads to better inventory management and lower risk aversion. Kremer et al. (2013) provide detailed evidence on

There is no difference between low-performers and their peers in risk preferences or the likelihood of having invested in their firm during the last six months. Low-performers exert significantly less effort and share slightly less with their business network. They earn 80% less in revenues compared to their peers. The table shows that while more successful firm owners look the same in terms of own characteristics and investment behavior, there are substantial differences in their time allocation and business experience.

2.3 Experimental Design

The field experiment was designed around three main objectives: i) to obtain descriptive evidence on the existence of overconfidence bias in the context of micro-entrepreneurs, ii) to estimate the causal effect of correcting beliefs on firm outcomes and thus provide evidence on the cost of non-cognitive biases such as overconfidence on firm growth, and iii) to shed light on the underlying mechanisms that might drive changes in firm outcomes. Objectives i) and iii) are achieved through detailed surveys at different points in time. Causal identification of the impact of non-cognitive biases on firm outcomes is achieved through random assignment to either a control group or a treatment group. All firm owners in the treatment group received information about their relative performance compared to peers in the same sector. Those randomly assigned to the treatment were additionally split into two sub-groups: one with ranking information only and one with additional information about peer characteristics. The control group did not receive any information but was visited at the same time as the treatment groups with a short survey.

All surveys and the treatment visit were conducted with the primary responsible of the respective firm. This was either the owner herself or the manager in case the shop was rented. Managers have full decision power and receive either all profits or a large share. They have thus self-interests in maximizing profits similar to firm owners. For the remainder of the paper the term firm owner does not distinguish between direct owners and managers, and implies both.

We assign firms to either of the two treatment groups or the control group using a

the impact of improved inventory management and investments.

stratified randomization where the strata is a firm owner's gender. Gender might be a relevant factor for firm performance and the effectiveness of the ranking, such that stratification can improve the precision of our estimates (see Duflo et al. (2008) or Bruhn and McKenzie (2009)). Specifically, gender might affect firm performance as female vendors might have less flexibility in adjusting their time allocation or have less access to capital given the cultural context.

Timeline The detailed timeline of the project can be found in Figure 3. The baseline survey took place from August to September 2016 just after the census survey in July. Eligible firms (those that reported baseline data on revenues in the week prior to the baseline survey) were visited again in November to December of the same year during the treatment visit. All firms in all groups were visited during this time and we conducted a short survey on firm outcomes to ensure that treatment status was not observable by others and that treatment effects are not driven by a higher number of visits to the treatment groups only. Four months after the treatment visit we fielded another survey to measure short-term impacts (survey 2) of our information treatment on firm outcomes and potential mechanisms. During this survey round we collected detailed data on prosociality towards the main business contact to compare outcomes between those in the treatment group and those in the control group, and over time. One year after the intervention we fielded the endline survey (survey 3). We use data from both survey 2 and 3 to estimate the causal effects of the treatment on firm outcomes, and compare the control and the treatment group at baseline and after the treatment. As we describe later, some of the outcome measures are available at one (survey 3) or two periods (baseline and survey 2) only.

2.4 Treatments

During the treatment visit, firm owners in the treatment group received information about their relative firm outcomes - specifically their revenues reported during the baseline survey. The data was collected step-wise over the week prior to the baseline survey. First, enumerators asked respondents to indicate their primary products. For each product, enumerators then asked about the total value of sales for the respective product yesterday, the day before yesterday, etc. for a full week.[16] I collected data for five primary products and the total rest of sold goods. The total sales over one week is the sum over all products and all days. For those individuals that reported revenues over the week prior to the baseline interview I proceeded to construct individual rankings.

The rankings were based on each individual's sector of firm activity and included all market vendors in the total sample, independent of market location. Reporting a firm's rank with respect to sectors rather than market location, for example, is based on two concerns, and on semi-structured interviews during the design phase. First, for security reasons I needed to ensure anonymity of firm owners whose information was displayed in the ranking. This seemed more reasonable when constructing rankings that included all firms in the greater Maputo area instead of focusing on market clusters. Second, it was important to ensure that firms would identify with the others in the ranking to absorb the provided information as relevant to their own businesses and practices. Providing information about firms in the same market cluster might inform firm owners about local demand effects whereas information about firms in the same sector provides information about global demand effects that seem more relevant in this context as buyers can move from one market to another (implying substantial time and travel costs). Observing information about others in the same sector hence seemed more promising in shifting beliefs and behaviors of firm owners. Additionally, information about others in the same sector is less observable than the potential success of others in the same market cluster.

After collecting the baseline data, I determined the decile in which each individual falls at baseline. The rankings were calculated in the same way for the entire experimental sample but only distributed to the ones in the treatment group. A ranking consists of ten firms.[17] It includes the individual's firm as well as one firm per other decile. The

[16]This approach reduces measurement error as reported values for the more distant past are anchored at values closer to today that the respondent might remember more easily. In the context of micro-firms this is important as many firms have no accounting books or other system to track revenues and expenses.

[17]A few firms were dropped from the sample as there were less than ten firms vending similar products in our sample.

other firms shown are those with the median[18] sales in their respective decile. This means that all treated individuals in the same sector observe the same peers. As sector sizes are unequal, reporting ten representative firms ensures that all other features of the ranking are held constant. The ranking was distributed to 192 (out of a total experimental sample of 323) individuals between November and December 2016. There was minimal framing on how an individual could improve her ranking. Individuals did not keep their ranking sheet and there was no indication that they would be ranked again.

Ranking Figure 4 displays an example of an individual's ranking. Firm owners are anonymized and the individual's own position is clearly highlighted with her name and a colored bar. Additional to their relative position, individuals observe others' revenues as well as their own reported revenue over the same period. The sheet furthermore includes information about the roster information and the name of the sector of the firm. Firm owners receive two types of signals. Most importantly, they receive information about the accuracy of their beliefs regarding how well their firm is performing compared to other firms selling the same type of goods. Secondly, firm owners receive information about the range of the distribution, i.e. the earnings potential in their respective sector.

I hypothesize that the ranking affects firm owners differentially depending on their ranking position. I group firms with positions 1-6 as average and top firms and those with positions 7-10 as firms in the bottom of the distribution.[19] Those at the bottom of the distribution are expected to be affected by the treatment more strongly as they are more likely to display over-confident beliefs and are more likely to receive new information about their earnings potential. Average and top firms on the contrary are more likely to have under-confident beliefs and do not receive new information about higher earning potentials (for those in the top of the distribution). We expect to observe positive treatment effects for firms in the bottom of the distribution but negative treatment effects for those in the top.

[18]Choosing the median firm controls for outliers.

[19]As most firms owners indicate their firms to do as well as the average firm it seems important to differentiate between average firms and those in the bottom rather than comparing treatment effects to firms above and below the median only.

Peer Characteristics Half of the treatment group additionally observed a small set of peer characteristics (see Figure 5 for an example). Treated individuals in this group observed whether their respective peers are male or female and their age. Information on age is included to make the observability of gender as a research interest less salient. The remaining information is the same as above.

Information about peer characteristics might enhance treatment effects if it causes firm owners to absorb the ranking information more strongly. For example, if women assume that successful firms are mostly operated by men then information about peers' gender can correct (if truthful) these beliefs and affect treatment effects of the ranking itself. On the other hand, Batista et al. (2020) shows that less information about peer characteristics improves social learning in rural village networks in Mozambique. Additionally, the gender of the top vendor (position 1 in the ranking) might play a prominent role for treatment effects. I hypothesize that observing a male versus a female firm owner at the top of the distribution will affect firm owners differently for two reasons: i) male firm owners might deduct that earnings potentials are even easier to achieve for themselves when already attained by a woman, and ii) female firm owners might perceive a female top vendor in a role model capacity and thus receive additional information about their own earnings potential as well.

3 Results

3.1 Descriptive Evidence on Over-Confidence

With perfect information about peer performance, showing individuals their ranking should not have any effects. In market clusters in developing countries, however, peer performance is likely to be unobservable. This might already be the case for peers within the same market but can be expected to be even stronger for peers working in the same sector but in markets that are located in other neighborhoods. Not having accurate information about peer performance can lead to inaccurate beliefs about own relative performance that can affect management decisions and outcomes of firms. To understand whether correcting inaccurate beliefs have the potential to change firm outcomes we thus want to understand the distribution of beliefs over relative performance outcomes first.

At baseline I asked respondents about their perception of how well their business is doing compared to peers working in the same sector. I ask about relative performance beliefs in the same market rather than the same sector (the information I ultimately provide during the treatment visit) for two reasons: i) for the treatment information to be novel information that firm owners have not been primed to think about before and ii) to elicit beliefs corresponding to the literacy level of our experimental sample. Asking about their beliefs over peers in the same sector would have implied higher cognitive effort and potentially higher measurement errors. Additionally, the comparison group would have been harder to compute as it is less clear which peers a firm owner takes into account. Similarly, I asked firm owners to categorize themselves into doing as well as the average firm in their market cluster or better/worse rather than asking firm owners to rank themselves out of 10 representative firms.

The majority of individuals in the sample had inaccurate beliefs about their relative position as illustrated in Figure 6. The figure compares the firm owners' self-assessments to their true percentile based on revenue data reported at baseline. The first row shows beliefs of firm owners in the bottom of the distribution whereas the last row can be interpreted as top firms and those in the middle as average performing firms. Only 24% of firms in the bottom of the distribution accurately believe that they are doing worse than the average firm in the market they operate in. Around 8% of those in the bottom actually believe they are doing better than the average. Firms in the top of the distribution have similarly incorrect beliefs about their relative position. Only 18% of high-performers accurately believe that they are performing better than the average. On the contrary, 17% of high-performers believe that they perform worse than the average.

The strong majority of individuals believe that their firms are doing equally well as the average firm in their market. This bunging effect can be explained through humbleness but might also capture concerns such as higher kinship taxes or fear of repercussions from financial authorities should firm owners not trust the confidentiality of data collection by the team. These concerns should not hold for firm owners in the bottom of the distribution such that their outcomes provide descriptive evidence on the existence of over-confident beliefs in micro-entrepreneurs.

3.2 Attrition

Firm owners were interviewed twice after the treatment visit - four months (Survey 2) and one year (Survey 3) after the intervention. Firm owners that could not be found during Survey 2 were nevertheless attempted to be re-interviewed during Survey 3 that provides evidence on the long-term and thus most relevant effects of the treatment visit. Survey 3 was administered to 85% of the sample. Overall, 23% of firm owners in the sample could not be interviewed in all three survey rounds but were interviewed during at least two of the surveys and are thus included in the empirical analysis.

Table 3 presents differential attrition by treatment status. The dependent variable is equal to one if the firm owner was interviewed in all three survey rounds. Differential attrition would be a cause for concern if firms in the treatment group would drop out as a result of having received feedback. This could for example be the case if firms in the bottom of the distribution become discouraged by their relative position and close their businesses. This would potentially leave the more successful of the bottom firms in the sample such that the empirical results would overestimate the effect of the treatment.

However, differential attrition seems to move in the opposite direction as shown in Table 3. Firm owners in the control group are less likely to have been interviewed in all survey rounds than firm owners in the treatment group and thus have lower survival rates. This could be the case if the treatment indeed improves firm outcomes and thus survival rates of treated firms such that the likelihood of being able to re-interview treated firms increases.

3.3 Estimation

I present evidence on firm outcomes and potential mechanisms and estimate the ATE. Note that the ATE is equal to the ITT as only individuals with active firms at the time of the intervention visit are included in the analysis.[20] The ATE estimates are based on the following difference-in-differences with individual fixed effects model for firm or individual i in survey round $t = 1, 2, 3$:

$$y_{it} = \alpha_i + \boldsymbol{\beta_1}(\mathbf{Treat_{it}}) + \boldsymbol{\beta_2}\,(\mathbf{Positive_i}) + \boldsymbol{\beta_3}\,(\mathbf{Treat_{it}} \times \mathbf{Positive_i}) + \gamma_t + \theta_i + \varepsilon_{it} \quad (1)$$

where y_{it} is the outcome of interest. Let $Treat_{it}$ be an indicator for assignment to treatment equal to 1 if individual i has seen the ranking and 0 otherwise. $Positive_{it}$ denotes a binary variable that is equal to 1 if an entrepreneur was in or above the 50th percentile at baseline, and 0 otherwise. The interaction term $Treat_{it} \times Positive_{it}$ indicates the additional effect from the treatment for a firm that was defined as an average or top firm at baseline. γ_t and θ_i are survey round and individual fixed effects, respectively. As randomization is at the firm level, I use robust standard errors clustered at the individual level. I furthermore bootstrap standard errors using 100 replications, and report Romano and Wolf (2005) q-values accounting for multiple hypothesis testing within families of outcomes. For outcomes where baseline data is not available I estimate an OLS model including strata and survey round fixed effects with robust standard errors. Post-intervention outcomes are pooled across survey rounds whenever multiple data points are available.

The main coefficient of interest is $\boldsymbol{\beta_1}$: the treatment effect of receiving relative performance feedback averaged over all post-intervention survey rounds under the identifying assumption of random assignment (conditional on half of the treatment group having observed peer characteristics). β_3 is the additional effect of receiving positive feedback for those individuals that received the treatment. The overall impact of the ranking on average and top performers is thus determined by the sum of β_1 and β_3. Linear hy-

[20]Individuals that were not in the markets but were interviewed at baseline had either closed their firms, were traveling, or did not consent to participate in the study. Even if some of those individuals were identified again at a later point they are excluded from this analysis as they are significantly different from the study sample that was active in the markets at the time of the intervention. Nevertheless, results are robust to including the full sample and can be obtained upon request.

potheses tests are reported in all tables below the respective coefficients of interest. The reported estimates compare the difference in outcomes of treated entrepreneurs pre- and post-intervention to the changes in outcomes of the counterfactual with the same position in the distribution at baseline. In other words, $\beta_1 + \beta_3$ is the impact of the ranking on high-performers compared to high-performers that did not observe their ranking. To determine the relative change of the treatment group compared to the control, I report the control mean for each group post-intervention separately.

In a second step, I estimate the impact of peer characteristic observability with the following model:

$$\begin{aligned} y_{it} = \alpha_i + \boldsymbol{\beta_1}(\mathbf{Treat_{it}}) + \boldsymbol{\beta_2}\,(\mathbf{Positive_i}) + \boldsymbol{\beta_3}\,(\mathbf{Treat_{ti}} \times \mathbf{Positive_i}) \\ + \boldsymbol{\eta_1}\,(\mathbf{PeerInfo_{it}}) + \boldsymbol{\eta_2}\,(\mathbf{TopGender_i}) + \boldsymbol{\eta_3}\,(\mathbf{PeerInfo_{it}} \times \mathbf{TopGender_i}) \\ + \gamma_t + \theta_i + \varepsilon_{it} \end{aligned} \quad (2)$$

$PeerInfo_{it}$ is an indicator for assignment to treatment equal to 1 if individual i observed peer characteristics. $TopGender_i$ controls for the effect of operating in a sector where individual i would have a observed a woman if peer characteristics were observable. The interaction term $PeerInfo_i \times TopGender_i$ is the additional effect of observing a woman at the top for those that observed peer characteristics. As before, the econometric specification includes survey round and individual fixed effects. I restrict the sample by excluding the last decile to avoid potentially confounding effects due to multicollinearity between the individual's own gender and the indicator for $TopGender_i$ for those individuals that are top sellers.

The β coefficients are now the unconditional differential treatment effects of receiving individual ranking information. I am interested in the coefficients η_1 and η_3. η_1 is the treatment effect of receiving information about peer characteristics whereas η_3 is the additional treatment effect of observing that the top seller in one's ranking is a woman.

3.4 Firm Outcomes

Tables 4 - 5 present results on three aspects of firm outcomes: survey reports of revenues, monitored revenues, and survey reports of profits. All outcomes are winsorized at the 1st and 99th percentile to control for outliers. The main concern about self-reported firm outcome data is measurement bias due to recalling error. The further away a sales day the more complicated for an individual to remember the exact sales value. To reduce this risk, we focus on revenue data for the last two days prior to the interview only. To estimate treatment effects on profits, we focus on the measure developed by de Mel et al. (2009) that asks firm owners directly about their profit over the last month after accounting for all expenditures related to the business over the same time horizon, i.e. it asks firm owners about the money they had left over in their pockets at the end of last month. While the first revenue outcome data was collected during all survey rounds, profits data was only collected for the two follow-up surveys, and monitored revenue data for the last follow-up survey only.

3.4.1 Treatment Effects of Information Experiment

The ATE estimates for reported sales, monitored sales, and profits are displayed in Columns (1), (4), and (7) of Table 4, respectively. Clustered standard errors are reported in parentheses and adjusted q-values in brackets next to each ATE. Effect sizes are measured in changes in the Metical value (Mozambique's currency) for the pooled sample such that the reported effect is the treatment effect of learning about one's ranking averaged over all post-intervention periods.

The treatment increased firm outcomes significantly across all three outcomes.[21] On average, firms increase their revenues by between MZN950 to MZN1,1150 *per day* up to one year after the intervention. Given the differential treatment effects estimation that controls for the effect of the treatment for average and top firms in the third line of Table 4, we can interpret this effect as the effect of the treatment on those firms that are in

[21] Comparing the different outcomes to each other we observe that reported and monitored revenue values are in line given that reported sales cover revenues over two days rather than one day as for monitored sales suggesting that the expected misreports are small. The profit margins of the firms in our sample yet are small such that revenue increases and profit increases are of a similar magnitude.

the bottom of the distribution. Comparing the treatment effect to the mean revenues post-intervention of those in the bottom of the distribution at baseline (thus the same type of firms) in the control group, the effect size implies that firms more than double revenues after the intervention. Further calculations additionally show that this implies that the performance gap between firms in the bottom of the distribution and average and top performers in the control group closes by almost 43%. Firms in the bottom of the distribution also catch up in terms of profits. These firms increase their monthly profits by 54% compared to the control group (statistically significant at the 5% level). By increasing their profit, firms close the gap to high-performers in the control group by 48%.

Average and top firms do not seem to benefit from the treatment and even experience slightly lower revenues than average and top firms in the control group. The joint test statistics though do not allow me to reject the null hypothesis such that the analysis is inconclusive. Additionally it does not seem that profits of average and top firms are affected. These results are in line with my hypotheses that average and top firm will obtain only limited new information about their relative performance or earnings potentials. The reported results are robust to multiple hypotheses testing.

3.4.2 Treatment Effects of Peer Characteristics

Estimates of the full specification, including binary indicators for whether a firm owner observed peer characteristics are presented in Table 5. The displayed results furthermore show the effect of observing a female entrepreneur at the top of the distribution.[22] For this analysis we restrict the sample to firms below the 10th decile to prevent faulty analysis due to multicollinearity bias. Overall the conclusions about the positive impact of the treatment on firms in the bottom of the distribution is robust to including these additional interaction terms although estimates on monitored revenues and profits are less precise due to the smaller sample size and lower statistical power. Although I can only estimate precise effects of providing peer characteristics information for reported revenues, my

[22]Estimation results estimating the effect of providing peer characteristics information only do not yield significantly different results and can be obtained upon request.

results overall suggest that observing peer characteristics has almost no or negative effects on firm outcomes (similar to the results found by Batista et al. (2020)).

Surprisingly, observing a female firm owner at the top of the sales distribution strongly increases firm outcomes across all measures (precisely estimated for the survey measures only though). Firms in the bottom of the distribution that received the treatment and additionally observed a female top firm owner increased their reported revenues over two days by approximately MZN4.284 - this implies that they outperformed the average firm in the upper half of the distribution by 15%. The impact on monitored sales are even larger although not precisely estimated. Treatment effects on profits lead to similar conclusions and suggest that firms in the treatment group that observe a female top firm owner outperform the average firm in the upper half of the distribution by 21%.

3.5 Time Allocation, Business Networks, and Pricing Strategies

The results above show that the information experiment significantly increases firm outcomes, especially for those at the bottom of the distribution - by correcting overconfident beliefs and changing potential earnings expectations. But what are the behavioral changes caused by the experiment that are likely correlated with firm outcomes?

There are three potential main mechanisms that exploit a firm's existing inputs and infrastructure that have been found to contribute to firm growth in the literature: labor input, strengthened ties with one's business networks, and a firm's pricing strategy. Given that firms in the bottom of the distribution are very small and unlikely to have any employees I focus on own labor input, i.e. the time a firm owner allocates towards her business. I asked firm owners how many hours per day they personally take care of their business.[23] To measure any changes in the strength of a firm owner's business network (especially the most important business partner) I conducted modified dictator games with firm owners and their business partners as described in detail in Section 2.2. I interpret any changes in contributions towards the receiver as a signal of tighter relationships between

[23]The results are robust to other time allocation measures such as days per week taking care of business personally or calculating business hours by asking about opening and closing times (as well as breaks) for each day of the week individually.

both players that could lead to increased social learning about better business practices or business collaborations.[24] Lastly, firm owners might decide to charge higher average prices for their products after learning about their ranking. To test this hypothesis we exploit data from the monitored sales experiment we describe in detail in Section 2.2. We use detailed data on each individual sale to compute the average price a firm owner charges for their goods.

Treatment effects for the three outcomes are reported in Table 6.[25]

3.5.1 Treatment Effects on Time Allocation

The treatment effect on time allocation choices for those at the bottom of the distribution is large and significant. Treated firm owners in the bottom of the distribution work 0.95 hours more per day than firm owners in the control group (mean = 9.27 hours). This corresponds to a 10% increase in own labor supply. Importantly, treated individuals initially performing worse than their peers allocate as much time to their businesses after the intervention as average and top firms in the control group. The treatment has a negative and statistically significant impact on average and top firms. The p-value of the joint significance test (0.02) suggests that providing individuals with information that their firm is doing relatively well decreases their time allocation to their firm by 0.5 hours. Nevertheless, treated average and top firms still allocate more time to their firms than the average firm owner in the bottom of the distribution in the control group.

3.5.2 Treatment Effects on Pro-Social Behavior within the Business Network

Contributions to the modified dictator game are measured in units of tokens shared with the recipient. The endowment was 20 tokens that had a monetary value of approximately US$3. To ensure that any treatment effects are driven by the provided information only, we furthermore include a control variable for income for this specification. Specifically,

[24] For example Cai and Szeidl (2018) find evidence that strengthening business network ties by organizing meeting groups between entrepreneurs significantly improves social learning between business and increases firm outcomes.

[25] We focus on treatment effects for the short model in this section. Treatment effects for the full model are coherent with the analysis above and can be obtained on request.

we control for business revenues over the last two days before the game was played.[26]

The ATE estimate of the treatment for firm owners whose firm is in the bottom of the distribution is again positive and statistically significant. Treated firm owners almost double the number of tokens shared compared to those in the bottom of the distribution in the control group. Treated individuals increase the amount of tokens they share by 2.22 units over a base level of 2.77 units in the control group. Treated firm owners thus share even more than firm owners of average or top firms in the control group (mean = 4.58). These results suggest that ranking information encourages firm owners in the bottom of the distribution to increase their contributions with their business network rather than becoming more selfish. I interpret this effect as reflecting changes in social proximity of business partners rather than changes in individuals' preferences, and thus as a potential driver of higher firm outcomes.

3.5.3 Treatment Effects on Pricing Strategies

Similar to the specification on pro-sociality we include a covariate on income to estimate treatment effects on pricing strategies to reduce confounding effects from increases in income alone. Our results show that, additional to increasing labor input and strengthening network ties, treated firm owners charge higher average prices for their products than their peers in the control group. In fact, they charge almost double the price than firm owners in the bottom of the distribution in the control group. Changes in the pricing structure stemming from selling higher quality products (that imply higher cost) could explain the large increase in revenues for treated firms while effects on profits are more moderate.

Overall, our results provide evidence for three channels through which the treatment increases firm outcomes for treated firm owners. Treated firm owners behave more similarly to peers in the control group with average or top firm performance outcomes. These changes are substantial given the potentially high cost of allocating more time to one's firm, an individual's business partners, and learning business strategies from others.

[26] In line with recent evidence by Blanco and Dalton (2019) income does not seem to affect dictator game contributions as the impact of past revenues on dictator decisions can be precisely (at the 10% level) estimated at zero.

3.6 Management Skills and Practices

A common policy approach to support business growth of micro-enterprises is to teach management skills and practices similar to those used in bigger firms. Impact evaluations of skills programs often show limited effects on firm outcomes as sample sizes are not large enough, technology adoption rates are low, and business practices dropped again in the medium-run (see McKenzie and Woodruff (2013), Quinn and Woodruff (2019), and Mckenzie (2020)). Table 7 shows results on some outcomes typically measured for such studies. A different approach is described in the studies by Cai and Szeidl (2018) that exogenously create business networks such that firms learn from each other given their local context. Firm owners in our experimental sample could learn about better management practices for their firms by either reaching out to their existing business network more strongly (as tested in Section 3.5.2) and learn from their peers or by seeking out support from local NGOs and government organizations supporting local micro-firm growth.

Given the sample size for this study it is not surprising that treatment effects for outcomes related to management skills and practices cannot be estimated precisely. Nevertheless, the results in Table 7 provide suggestive evidence that treated firms in the bottom of the distribution behave more similarly to average and top firms after the intervention. I report treatment effects for four[27] different secondary outcomes: bookkeeping, establishing business measures, the demand for bank loans, and investments in product diversity. The first two outcomes are directly related to management practices whereas the latter two aim at capturing changes in investment behavior.

Practices around bookkeeping are measured in an index that captures whether a firm owner keeps books on sales, clients that bought on credit, and an inventory list. Firms across the distribution are surprisingly similar in their bookkeeping practices and average and top firms in the control group are only slightly more likely to keep books. Accordingly, though positive, the treatment effect on bookkeeping is small and cannot be precisely

[27] Treatment effect estimations on additional outcome measures can be obtained on request. Further outcome measures lead to similar interpretations but can neither be estimated precisely due to the sample size that was powered to detect the main outcome measures reported above.

estimated.

Surprisingly, average and top firms seem to be less likely to keep track of business measures. The business measures index is based on a survey question whether firm owners calculate on a regular basis their sales, expenditures, profits, and which of their products sold most. While those firm owners in the bottom of the distribution (in the control group) calculated 1.8 of these business measures, average and top firm owners calculated only 1.5 measures on average. Although not statistically significantly, treated firm owners in the bottom of the distribution approximate their behavior by being less like to calculate business measures compared to their peers in the control group.

Successfully obtaining bank loans for micro-firms in Mozambique is typically challenging so that I resort to a survey question whether a firm owner attempted to obtain a bank loan but was rejected to measure treatment effects on financial capital. In general, the average number of firm owners that try to obtain a loan in the control groups is very low. Treated firm owners are more likely to have asked for a loan but the effect size is very small and cannot be precisely estimated. Nevertheless, as before, treated firm owners in the bottom of the distribution are more similar to average and top firms after the intervention.

I proxy for general investment behavior by estimating treatment effects on investments in product diversity specifically.[28] The outcome measure asks firm owners about approximately how many distinct products they sell in their business. In the control group, firms in the bottom of the distribution sell around 2.5 products less than average and top firms. After the intervention, treated firms increase their product range by almost three products on average to catch up with better performing firms.

Jointly, this evidence suggests that while it is difficult to precisely estimate treatment effects on specific skills or practices, the treatment potentially nudges firm owners to behave more similarly to those firm owners that have better firm performance indicators. As before, effects on those firms that are doing relatively well are null or very small.

[28]When asked about investment goals, firm owners often mention product diversity as their main goal which requires financial capital and time investments to be able to expand a firm's product range.

4 Robustness Checks

Individuals that received a negative signal of their ranking (information that their performance is below the average) might be more likely to tell that they increased their sales even if they did not. In this case, the survey data would be subject to measurement error that is correlated with treatment status. I address this concern by providing evidence on monitored sales data additional to survey outcomes and by validating the survey measures below.

4.1 Differences Between Survey and Monitored Revenues

Table 8 reports the means of the survey data (sales over the last two days before the interview) and monitored sales for the full sample and the control and treatment group. The respective means are reported for firms in the bottom of the distribution and average and top firms separately as the main concern is the validity of self-reports of firm owners that received negative feedback. I also report correlation coefficients between the two measures for each respective group.

For firms in the bottom of the distribution the two measures are strongly correlated with each other with an average correlation coefficient of 0.6. The correlation coefficients of the control and the treatment group are very similar with a coefficient of 0.61 and 0.6, respectively. There does not seem to be any misreporting correlated with treatment status for those whose firms were in the bottom of the distribution at baseline. For average and top firms self-reports in the control group are less reliable as an indicator of true sales.

One way to test for bias in the treatment effects on survey measures is to take the difference between survey and monitored sales and regress it on treatment.[29] Equation 3 defines the empirical model to be estimated:

[29] This strategy follows Blattman et al. (2017). The identifying assumption that the tracked sales data is closer to true sales is met by design as sales were observed by trained enumerators.

$$Outcome_i^S - Outcome_i^V = \beta_0 + \beta_1 Treat_i + \beta_2 Positive_i + \beta_3 \left(Treat_i \times Positive_i\right) + \gamma_1 Gender_i + \gamma_2 X_i + \varepsilon_i \tag{3}$$

γ_i controls for gender fixed effects and X_i includes weekday and market cluster fixed effects. If a β-coefficient is negative, then treated individuals are more likely to under-report their sales during the survey. The survey measure would then underestimate the increase in sales due to the intervention. A positive β-coefficient suggests that survey-based treatment effects are over-estimated. The estimation results show that none of the coefficients on treatment indicators are statistically significant. There is no evidence of desirability bias for firms at the bottom of the distribution which would imply an under-estimation of the true treatment effect.

5 Conclusion

This paper provides evidence on the impacts of inaccurate beliefs on economic decision-making and firm outcomes in the context of urban markets in a low-income country. Inaccurate beliefs over own relative performance and attainable income levels are particularly relevant in low income economies for two reasons: i) information about peers' performance is scarce and firm outcomes of others difficult to observe, and ii) inaccurate beliefs can explain the low take-up of government programs targeting financial and skills constraints of micro-firms that aim at promoting private sector development and job creation.

I document that a simple information treatment providing firm owners with information about their relative revenues compared to nine peers reflective of the sector distribution of revenues can have highly positive effects on firm outcomes such as revenues and profits - particularly for firms in the bottom of the distribution that are more likely to be overconfident over their relative performance and, implicitly, their attainable income levels. I furthermore show that three important mechanisms correlated with firm outcomes are affected by the information treatment: time allocation towards the business, social proximity with nearest business partners, and pricing strategies. This is in contrast with standard assumptions that firm owners optimize over existing input factors and are constrained in their business growth by external factors only.

The results show that internal constraints of firm owners, such as inaccurate beliefs, are binding constraints for firm success and ultimately growth. This highlights the importance for policy to account for internal constraints when designing programs to promote private sector development. These implications open up further research possibilities for analyzing the underlying effects of information about peer performance, how governments might design effective policies to overcome internal constraints, and how interventions targeting internal constraints can be combined with interventions targeting other constraints to increase take-up.

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Tables

Table 1: Baseline Summary Statistics and Test of Balance for Select Covariates.

	Control			Treatment			All			t-test
	N (1)	mean (2)	se (3)	N (4)	mean (5)	se (6)	N (7)	mean (8)	se (9)	difference (10)
Baseline covariate										
Age	122	33.11	(0.80)	191	34.03	(0.66)	313	33.67	(0.51)	-0.93
Female	125	0.43	(0.05)	191	0.40	(0.04)	316	0.41	(0.03)	0.03
Years of schooling	124	7.97	(0.27)	191	8.09	(0.21)	315	8.04	(0.17)	-0.13
Number of hh members	124	5.53	(0.23)	191	6.11	(0.19)	315	5.88	(0.15)	-0.58*
Hh expenditures/month	123	6,991.28	(338.98)	190	7,584.54	(332.33)	313	7,351.40	(241.95)	-593.26
Reservation wage	120	20,674.85	(1503.33)	189	28,078.53	(4982.90)	309	25,203.31	(3106.63)	-7,403.68
Had formal business training	124	0.11	(0.03)	191	0.09	(0.02)	315	0.10	(0.02)	0.02
Years in business	125	7.39	(0.63)	191	8.18	(0.50)	316	7.87	(0.39)	-0.79
Shop asset score (2016 value)	125	10,529.91	(2409.83)	190	12,630.14	(3191.08)	315	11,796.72	(2147.19)	-2,100.23
Employees	124	0.395	(0.085)	191	0.236	(0.05)	315	0.298	(0.045)	0.16*
Sales last 2 days (winsorized)	98	3,282.33	(553.32)	187	3,683.90	(408.07)	285	3,545.82	(328.09)	-401.58
Hours worked per day	123	9.73	(0.15)	189	9.74	(0.11)	312	9.74	(0.09)	-0.01
Pro-sociality Measure	129	4.56	(0.27)	189	4.36	(0.25)	312	4.40	(0.18)	0.25

Notes: This table shows summary statistics for the full sample of entrepreneurs included in the study as well as summary statistics for the control and the treatment group. Column (8) reports the sample mean. The treatment group includes all subjects that have seen the ranking. Reported is a selection of covariates. Sales, and hours worked are winsorized at the 1st and 99th percentile. The value displayed for t-tests are the differences in the means across the groups. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table 2: Difference Between Vendors for Select Covariates.

	Bottom			Average & Top			t-test
	N	mean	se	N	mean	se	difference
	(1)	(2)	(3)	(4)	(5)	(6)	(10)
Baseline covariate							
Age	107	33.07	(0.94)	206	33.99	(0.60)	-0.92
Female	107	0.46	(0.05)	208	0.39	(0.03)	0.07
Years of schooling	107	8.08	(0.29)	208	8.03	(0.20)	0.05
Household size	107	5.89	(0.26)	208	5.88	(0.18)	0.01
Risk preferences	106	5.22	(0.37)	207	5.37	(0.25)	-0.15
Reservation wage	106	25546.32	(7140.08)	203	25024.21	(2926.05)	522.11
Formal business training	107	0.09	(0.03)	208	0.11	(0.02)	-0.01
Investment (past 6 months)	78	0.50	(0.06)	138	0.52	(0.04)	-0.02
Years in business	107	6.44	(0.68)	209	8.60	(0.47)	-2.16***
Hours worked per day	106	9.45	(0.15)	206	9.88	(0.11)	-0.43**
Pro-sociality measure	107	4.15	(0.31)	211	4.589	(0.23)	-0.44
Sales last two days	101	948.00	(91.11)	184	4971.79	(474.24)	-4023.79***

Notes: This table shows summary statistics for firms in the bottom of the distribution and the median and top. The bottom group includes all subjects that fall below the 50th percentile. Reported is a selection of covariates. Sales, and hours worked are winsorized at the 1st and 99th percentile. The value displayed for t-tests are the differences in the means across the groups. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table 3: Differential Attrition over all Survey Rounds.

	Control	Treatment	Difference	(s.e.)
	Mean	Mean		
Interviewed in all rounds	0.65	0.77	-0.12	0.09*

Table 4: Treatment Effects on Main Outcomes (in MZN).

	Fixed Effects Model			OLS Models					
	Sales (survey measure)			Sales (monitored)			Profit (survey measure)		
Outcome	ATE (1)	se (2)	q-value (3)	ATE (4)	se (5)	q-value (6)	ATE (7)	se (8)	q-value (9)
Treat	1,899.78***	(715.91)	[0.02]	1,147.31**	(504.86)	[0.01]	1,258.34**	(574.83)	[0.02]
Positive	-970.66	(932.90)	[0.25]	945.84***	(345.56)	[0.16]	2,641.66***	(545.82)	[0.16]
Treat × Positive	-2,340.51*	(1, 309.27)	[0.16]	-1,458.06**	(644.69)	[0.01]	-1,285.73	(841.17)	[0.01]
Control mean - bottom	1,395.59			939.25			2,330.00		
Control mean - top	3,664.48			1,844.73			4,944.58		
Joint test - p-value	0.69			0.45			0.97		
adjusted r-squared	0.04			0.01			0.03		
Observations (cluster)	828 (320)			275			537 (303)		

Notes: Outcome measures are winsorized at the 1st and 99th percentile. Coefficients in Column (1) are difference-in-differences with fixed effects estimates. Coefficients in Columns (4) and (7) are OLS estimates as baseline data on monitored sales and self-reported profits is not available. All regressions include randomization strata and survey fixed effects. Standard errors clustered at the individual level are reported in parentheses. Romano-Wolf adjusted q-values correcting for multiple hypotheses testing for the family of outcomes shown in this table are reported in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5: Treatment and Peer Characteristics Effects on Main Outcomes (in MZN).

	Fixed Effects Model			OLS Models					
	Sales (survey measure)			Sales (monitored)			Profit (survey measure)		
	ATE	se	q-value	ATE	se	q-value	ATE	se	q-value
Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treat	2.647,43***	(817, 00)	[0.02]	992,90*	(574, 47)	[0.07]	1,163.78*	(595.27)	[0.07]
Positive	-270,81	(1.049, 60)	[0.62]	599,56	(385, 13)	[0.19]	2,546.04***	(633.73)	[0.00]
Treat × Positive	-1.945,41	(1.340, 66)	[0.26]	-1.306,28*	(669, 85)	[0.09]	-1,119.98	(881.74)	[0.26]
Peer Info	-2.483,36***	(859, 38)	[0.00]	61,39	(552, 17)	[0.86]	-833.83	(666.39)	[0.37]
Top Gender	-1.213,04	(795, 59)	[0.14]	-625,22**	(314, 78)	[0.14]	-1,140.66*	(625.24)	[0.14]
Peer Info × Top Gender	4.119,66**	(1.879, 75)	[0.10]	1.128,80	(1.130, 14)	[0.33]	3,206.33**	(1, 384.03)	[0.10]
Control mean - bottom	1,414.87			951,88			2,409.46		
Control mean - top	3,736.43			1.555,63			4,926.08		
Joint test - p-value	0.58			0.54			0.96		
adjusted r-squared	0.05			0.01			0.04		
Observations (cluster)	668 (251)			220			427 (239)		

Notes: Outcome measures are winsorized at the 1st and 99th percentile. Coefficients in Column (1) are difference-in-differences with fixed effects estimates. Coefficients in Columns (4) and (7) are OLS estimates as baseline data on monitored sales and self-reported profits is not available. All regressions include randomization strata and survey fixed effects. The sample is restricted to individuals whose baseline performance did not lie above the 90th percentile. Standard errors clustered at the individual level are reported in parentheses. Romano-Wolf adjusted q-values correcting for multiple hypotheses testing for the family of outcomes shown in this table are reported in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6: Treatment Effects on Main Mechanisms.

	Fixed Effects Models				OLS Model	
	Hours worked		Pro-Sociality		Pricing	
	ATE	se	ATE	se	ATE	se
Outcome	(1)	(2)	(3)	(4)	(5)	(6)
Treat	0,95***	$(0,35)$	2,22**	$(0,87)$	69,32**	$(34,74)$
Positive	0,94***	$(0,32)$	2,66***	$(0,83)$	51,49	$(38,35)$
Treat × Positive	-1,45***	$(0,41)$	-2,26**	$(1,16)$	-9,03	$(95,76)$
Control mean - bottom	9.27		2.77		72,17	
Control mean - top	10.24		4.58		158,94	
Joint test - p-value	0.02		0.96		0.47	
adjusted r-squared	0.04		0.04		0.05	
Observations (cluster)	875 (321)		570 (316)		235	

Notes: All regressions include randomization strata and survey fixed effects. The models estimating coefficients in column (3) and (5) control for income effects by adding covariates about revenues over the last two days for each time period. Standard errors clustered at the individual level are reported in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 7: Treatment Effects on Secondary Mechanisms.

	Fixed Effects Models						OLS Model	
	Bookkeeping Index		Business Measures Index		Loan Attempts		Investment in Products	
	ATE	se	ATE	se	ATE	se	ATE	se
Outcome	(1)	(2)	(3)	(4)	(7)	(8)	(9)	(10)
Treat	0,14	(0, 33)	-0,37	(0, 40)	0,03	(0, 05)	2,91	(2, 04)
Positive	0,00	(0, 32)	-0,63	(0, 38)	0,07	(0, 05)	2,46	(1, 69)
Treat × Positive	-0,09	(0, 39)	0,38	(0, 48)	-0,06	(0, 06)	-2,26	(2, 99)
Control mean - bottom	1,00		1,77		0,00		7,15	
Control mean - top	1,19		1,50		0,04		9,46	
Joint test - p-value	0.79		0.96		0.53		0.77	
adjusted r-squared	0,06		0,03		-0,00		0,01	
Observations (cluster)	606		605		863		537	

Notes: All regressions include randomization strata and survey fixed effects. Standard errors clustered at the individual level are reported in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

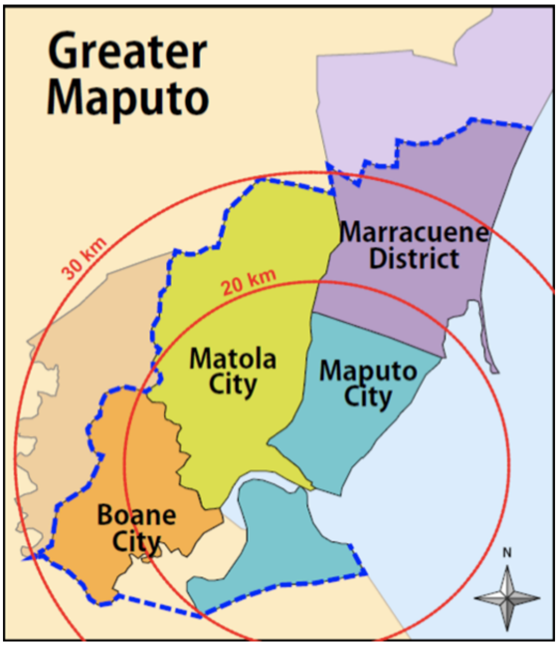
Table 8: Comparison of Survey and Monitored Revenue Means at End-line.

	All	Control Group	Treatment Group
	A. Firms in Bottom of Distribution		
Survey Mean	2149.198	1634.148	2406.722
	(3396.458)	(1846.434)	(3941.765)
Monitored Mean	1639.297	939.25	1983.582
	(2973.943)	(1102.377)	(3509.074)
Correlation Coefficient	0.6045	0.6126	0.6026
	B. Average and Top Firms		
Survey Mean	2690.206	2503.127	2824.374
	(3886.29)	(3240.316)	(4301.407)
Monitored Mean	1568.144	1691.648	1479.571
	(2540.349)	(2106.88)	(2817.646)
Correlation Coefficient	0.5219	0.2960	0.6164
	Regression Results		
			s.e.
Treat	-377.43		(659.56)
Positive	-76.52		(539.18)
Treat × Positive	605.10		(879.22)
Observations		246	

Notes: Outcome measures are winsorized at the 1st and 99th percentile. Standard deviations are reported in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1%.

Figures

Figure 1: Maputo Metropolitan Area.



Source: JICA Report (2014)

Figure 2: Illustration of Dictator Game Setup.

Figure 3: Project Timeline.

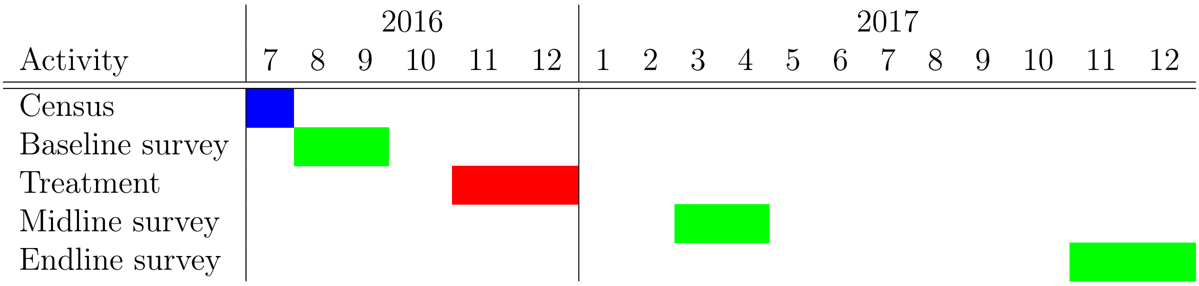


Figure 4: Example of Treatment.

Folha de Tabela de Classificação - Projecto Formação Empresarial em Moçambique - NOVAFRICA

NOME DO MERCADO: **MAZAMBANE** **Numero da Entrevista:** **7085**
Tratamento: **2**

SAPATOS, CHINELOS E MALAS

NOME: **MARIA SILVA**
CONTACTOS: **123456789**

Tabela de Classificação

	NOME	IDADE	GÉNERO	VENDAS POR SEMANA
1	XXX			**25,400.00**
2	XXX			**17,520.00**
3	XXX			**12,600.00**
4	XXX			**9,780.00**
5	XXX			**6,900.00**
6	XXX			**5,810.00**
7	MARIA SILVA			**4,700.00**
8	XXX			**3,900.00**
9	XXX			**1,450.00**
10	XXX			**1,103.00**

Notes: This figure depicts an example of a ranking sheet individuals in the treatment group observed. Their own name was highlighted whereas peers are displayed anonymously. Shown is the relative position as well as the revenues of one week. Revenue data was collected during a baseline survey two months prior to the intervention visit.

Figure 5: Example of Secondary Treatment with Observable Peer Characteristics.

Folha de Tabela de Classificação - Projecto Formação Empresarial em Moçambique - NOVAFRICA

NOME DO MERCADO: **MAZAMBANE**

Numero da Entrevista: **7085**
Tratamento: **3**

SAPATOS, CHINELOS E MALAS

NOME: **MARIA SILVA**
CONTACTOS: **123456789**

Tabela de Classificação

	NOME	IDADE	GÉNERO	VENDAS POR SEMANA
1	XXX	34	FEMININO	**25,400.00**
2	XXX	35	MASCULINO	**17,520.00**
3	XXX	24	MASCULINO	**12,600.00**
4	XXX	27	MASCULINO	**9,780.00**
5	XXX	42	FEMININO	**6,900.00**
6	XXX	24	MASCULINO	**5,810.00**
7	MARIA SILVA	39	FEMININO	**4,700.00**
8	XXX	29	MASCULINO	**3,900.00**
9	XXX	21	FEMININO	**1,450.00**
10	XXX	20	MASCULINO	**1,103.00**

Notes: This figure depicts an example of a ranking sheet individuals in the second treatment group observed. Their own name was highlighted whereas peers are displayed anonymously. Shown is the relative position as well as the revenues of one week. Revenue data was collected during a baseline survey two months prior to the intervention visit. Additionally to the revenue data, individuals can observe a peer's gender and age.

Figure 6: Descriptive Evidence of Over-Confidence.