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Essays in Behavioral Development Economics

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Introduction

Our social networks affect our beliefs, decisions, and behaviors as the people we interact with on a regular basis transmit information about new technologies, job opportunities, or political opinions. They provide us with resources through informal insurance networks and remittances, and shape our education and occupation decisions. Comparing ourselves to our peers shapes our identities and the social norms we follow, and being exposed to role models affects our aspiration and reference points.

This thesis explores how social networks influence political participation, and the decisions of firm owners through information, social norms, and reference points. I analyze the effects of a different social network and environment through international migration, and the impact of being exposed to information about one's peers in two field experiments. I study these questions in the context of a low-income country that has experienced decades of social conflict and war - Mozambique. Social unrest as well as the consequences of natural disasters forced many of the Mozambicans living in rural areas to migrate to neighboring African countries and the urban areas of Mozambique.

In the first chapter, co-authored with Catia Batista and Pedro Vicente, we exploit the quasi-experimental nature of the drivers of migration from Mozambique to, mainly, South Africa to study how emigrating to a country with stronger political participation norms affects political attitudes and participation at home. Specifically, we identify the type of migrant social networks that changes political behavior, and the two primary

channels that drive these changes. We develop a theoretical framework that suggests that migration might change the social norms for political participation as well as improve knowledge about better quality political institutions. International migration might thus increase political awareness and participation both by migrants and by other individuals in their networks.

To test this hypothesis, we use detailed data on different types of migrant networks, namely geographic, kinship and chatting networks, as well as different measures of political participation and electoral knowledge. During the 2009 elections in Mozambique a team of enumerators collected data on self-reports, behavioral and actual voting measures. The nature of this data allows us to distinguish between the effect of international migration on political attitudes and actual behavior as well as between effects driven by changes in social norms and those driven by increased knowledge. The empirical results show that the number of migrants an individual is in close contact through regular chatting within a village significantly increases political participation of residents in that village – more so than family links to migrants. Whereas family links are equally effective in changing political attitudes, they have little effect on political participation. Additionally, chatting networks effectively change both the social norms of political participation and transmit knowledge about electoral processes. These results are robust to controlling for self-selection into migration as well as endogenous network formation. The chapter shows that even in the many contexts of South-South migration where both countries of origin and destination are imperfect political systems there may be domestic gains arising from international emigration.

Whereas the first chapter studies the effect of changing the social network itself, in the second chapter I analyze how an individual's relative position within her social network affects behavior in the context of micro-entrepreneurs. In Mozambique, as well as in many other developing countries, the supply of salaried jobs does not meet

the demand from internal migrants coming to the capital Maputo. As a consequence, many resort to self-employment in the urban market clusters or streets serving their neighborhoods. These firm owners often have no formal business training, financial literacy, or access to capital. Their businesses operate on very low productivity levels, often run out of stock and have to be closed, and rarely grow to provide employment opportunities or contribute to the formal economy.

Additionally to having limited access to classical inputs such as capital and technology, micro-firm owners face behavioral growth constraints. Firm owners have incomplete information about attainable incomes, and their relative performance standing. This in turn might affect their beliefs about the quality of their business network and the returns to effort. These incorrect beliefs lead to sub-optimal choices that stunt firm growth. In the second chapter, I study whether feedback on relative performance standing affects beliefs and, as a consequence, firm output. I exploit data from a unique field experiment that provides causal evidence of the effect of performance rankings on firm outputs. Firm owners in the treatment group observed their relative position within their sector as well as their peers' revenue data. One year after the intervention, at baseline high-performers are not affected by the treatment as the ranking contains only limited new information for them. However, low-performing firm owners at baseline significantly increase their revenues compared to low-performers in a control group. Treated low-performers close the performance gap to high-performing peers by 43%. This effect is, largely, explained by significant increases in work hours and pro-social behavior. Exploiting variation in the observability of peer characteristics, I show that the treatment is particularly effective when subjects observe that the most successful peer is a woman. Low-performers that, additionally to their own ranking, observe a female top seller, outperform at baseline high-performers by 14%.

The last chapter provides empirical evidence on the relationship between refer-

ence dependence and micro-firm growth. A lack of positive role models of successful entrepreneurs such as in the Mozambican context where most entrepreneurs operate subsistence businesses might imply low aspiration levels of firm owners. If we consider that small firms might not be profit-maximizing but instead be reference- and aspirations-constrained, an intervention focusing on increasing aspirations and changing an entrepreneurs reference point might be successful in fostering micro-firm growth. To test this hypothesis the third chapter, co-authored with Catia Batista, analyzes a randomized control trial designed to isolate the impact of reference points on entrepreneurship.

Approximately 600 micro-entrepreneurs were randomized into treatments providing information about a role model, additionally the importance of establishing realistic goals, and how to maintain funds within their enterprises. Six months after implementation we find significant positive effects of shifts in aspirations on effort levels and savings. These effects are maintained and can still be precisely estimated one year after implementation. On average, changes in investment behavior translate to revenue increases of 40%-45% compared to a control group. In contrast, human capital improvements have no effect on investment behavior. Furthermore, setting business goals mitigates the positive effects of role models on economic growth. Despite similar effects on effort levels, participants that condition their reference points on a one-year goal save less and the positive effects on revenues disappears over time.

Chapter 1

Migration, Political Institutions, and Social Networks

1.1 Introduction

The economic importance of international migration has been increasing steadily in the recent decades. Not only has the number of labor migrants increased massively, but also the financial flows generated by these migrants have been rising rapidly, often surpassing the national budgets of many developing countries.[1] As a result, the strand of economics literature that examines the potentially positive effects of emigration on the economic development of origin countries has been growing. It highlights that the positive effects of emigration on economic development may happen as a result of a number of mechanisms such as overcoming liquidity constraints through remittances, promoting human capital accumulation and entrepreneurship, and increasing foreign direct investment and international trade.[2] While the importance of good political

[1] World Bank (2018) "Moving for Prosperity – Global Migration and Labor Markets".

[2] Edwards and Ureta (2003) and Yang (2008) described how remittances may provide the financial resources to overcome credit constraints in migrant sending countries. Furthermore, return migration may bring not only financial resources, but also human capital, which can promote entrepreneurship

institutions for economic development is by now well established, as influentially described by Acemoglu et al. (2005), one area that has deserved relatively less attention in the economics literature is the relationship between international migration and the quality of political institutions in countries of migrant origin.[3]

The main objective of this paper is to make a specific contribution to this literature by examining in detail different mechanisms through which international migration may play a role in the diffusion of improved political attitudes and behavior of those left behind. For this purpose, we make use of a number of different measures of political participation (namely self-reports, behavioral and actual measures of political behavior), and of different types of migrant social networks (geographical, kinship, and chatting networks).

We start by proposing a theoretical framework where migration might change individual social identities and in this way intrinsic motivation for political participation, while it may also improve information and knowledge about better quality political institutions. Through these mechanisms, international migration might increase political awareness and participation. This effect may not only influence migrants themselves, but also trigger peer effects - thus impacting the social network of current and return migrants in their country of origin.

In order to evaluate whether international migration may foster political participation, and examine the importance of different types of migrant networks in this

and economic growth, as in Mesnard and Ravallion (2006) and Batista et al. (2017). Migrant networks may also foster increased Foreign Direct Investment (FDI) and international trade, as found by Gould (1994), Rauch and Trindade (2002), Kugler and Rapoport (2007) or Javorcik et al. (2011). An additional possibility empirically examined and supported by Beine et al. (2008) and Batista et al. (2012) is the "brain gain" hypothesis put forward by Mountford (1997) and Stark et al. (1997, 1998), according to which the simple prospect of emigration can promote human capital accumulation in migrant origin countries.

[3]Throughout this paper, we define the quality of political institutions as combining compliance to the electoral principle of democracy where rulers are made responsive to citizens through periodic elections, together with compliance to the participatory principle that can be summarized as active participation by citizens in all political processes - including not only elections, but also other forms of political engagement, as described by Coppedge et al. (2016).

transmission process, we exploit data from nationally representative household surveys conducted immediately before and after the 2009 national elections in Mozambique. These elections followed the lowest election turnout ever in Mozambique in 2004 (36% according to official numbers), which was also the lowest among all SADC countries. Because Mozambique is a country with substantial emigration to neighboring countries (especially to South Africa), this therefore seems like an ideal context in which to study the role of the increasingly important but relatively understudied South-South migration in transmitting norms in a context of imperfect democracies.

Our empirical analysis investigates whether an individual who is connected to one or more international migrants is affected differently in terms of his/her political attitudes and behavior depending on the characteristics of these connections. To evaluate in detail the different diffusion mechanisms of information and political attitudes through international migrant networks, we use different migrant network measures. Specifically, we distinguish between migrant geographical networks, i.e. how many households with at least one migrant in the family exist in the respondent's village; migrant kinship networks, i.e. the number of migrant households that are related by family links to the respondent; and migrant chatting networks, i.e. the number of migrant households the respondent regularly chats with. In order to test our theoretical hypotheses, this paper uses several survey and behavioral measures related to political participation and electoral knowledge – namely, self-reported voting behavior; a measure of actual voter turnout; a measure of electoral information; and a behavioral measure reflecting the respondents' intrinsic motivation for political participation.

For the purpose of investigating the relationship between migrant social networks and political attitudes and behavior, we estimate a Linear Probability Model (LPM), controlling for individual, household, and location characteristics. Because international migration may potentially be correlated with political attitudes via unobserved

factors that cannot be controlled for in our regressions, we also conduct Two-Stage Least Squares (2SLS) regressions that exploit 'quasi-natural experiments' given by the history of natural catastrophes that may plausibly have exogenously created migration flows. In addition, acknowledging the possibility of endogenous migrant network formation, particularly in the cases of chatting and kinship, we use secondary network links ("friends of friends" in the case of chatting networks) as an exclusion restriction to limit the potential correlation between the characteristics of individuals in Mozambique and the migrants in their networks.

The empirical results we obtain suggest that political participation can be learned and valued more highly when people migrate to countries with better quality political institutions, and that the newly obtained political participation norms may be passed on to peers. We confirm existing results on the positive effects of living close to migrant households on political engagement – for example, Batista and Vicente (2011) for Cape Verde. In addition, we find that increased political participation during elections seems to be mainly driven through contact with migrant households via regular chatting, rather than via family links to migrants. The evidence we examine is consistent with both information transmission and changed social norms for political participation via chatting with migrants. Family links seem to convey some information about the political process, but do not seem to significantly affect intrinsic motivation for political participation. Our findings are robust to endogeneity concerns about unobservable self-selection of migrants and endogenous network formation.

The remainder of the paper is organized as follows. Section 1.2 presents a broad literature review on the relationship between international emigration and political remittances, and the original contribution of this paper. Section 1.3 proposes a theoretical framework to describe different ways through which migratory experiences may influence political behavior. Section 1.4 describes the country context under which

the empirical part of this study was carried out. Next, section 1.5 follows with an introduction to the dataset and its descriptive statistics. Section 1.6 puts forward an econometric model and estimation strategy for the effects of interest. Finally, section 1.7 presents the empirical results of the LPM and 2SLS estimations and robustness tests, and section 1.8 concludes.

1.2 Literature Review

The economic, political and social importance of financial remittances sent by migrants to their home countries has by now been well-established and the focus of a large body of literature.[4] It has only been more recently that social scientists have focused their attention on the impacts of "social remittances". This designation was proposed by Levitt (1998) to emphasize that, in addition to financial remittances, migrants transfer new knowledge, practices, and norms to their countries of origin. Examples of social remittances that migrants may transfer back to their home countries are increased valuation of education and health, fertility norms, improved organizational skills and entrepreneurship, and higher demand for political accountability.

The question of whether international migration improves the quality of the domestic political system in the migrant countries of origin is related to the traditional "brain drain" debate put forward by Gruber and Scott (1966) and Bhagwati and Hamada (1974). Indeed, emigration has been traditionally regarded as hurting the supply of well-prepared individuals who can directly supply political services if those who leave are the best qualified to provide these services. In addition, the political system would also be negatively affected if emigration acts as a "safety valve" or "outside option" that makes individuals unhappy with the political status quo to leave their home coun-

[4]Brown and Jimenez-Soto (2015) provide a recent overview.

try thereby dampening the demand for better political institutions. This view follows Hirschman (1970)'s "exit" vs. "voice" dichotomy, according to which citizens unhappy with the domestic situation either choose to emigrate (exit) or to protest and contribute to political change (voice). In this setting emigration could be understood as a "safety valve", which released protest intensity in the home political system and therefore reduced demand for political improvements.

One can however argue that emigration may improve political regimes in several ways: diaspora effects brought about by current emigrants may promote political change by influencing local authorities to increase governance (supply side), or by intensified contact of the domestic population with better institutions abroad thereby promoting a desire for greater accountability (demand side); return emigrants experiencing an enriching environment abroad may also improve the quality of the domestic governments upon return by direct participation in the political system (supply side), or by bringing increased awareness and demand for political accountability (demand side).

The question of how emigration affects the quality of domestic politics is therefore an empirical question. This paper focuses specifically on examining the demand side of the political system by studying the impact of migrant networks on the political attitudes and behavior of those left behind.

Levitt (1998)'s notion of "social remittances" has been followed by a large number of contributions in demography, economics, political science, and sociology illustrating how migration can change political attitudes and behavior in countries of origin.[5] Initial contributions, such as Kapur and McHale (2005) or Kapur (2010), highlighted the promise of social remittances as tools for economic development of countries of migrant origin. Most early contributions studying how emigration has changed politics

[5]The concept of social remittances is necessarily grounded on the assumption that migrants assimilate social norms of the countries of destination. Evidence that migrants assimilate political norms in their host countries of migration is provided by Careja and Emmenegger (2012) and Chauvet et al. (2016) for very different contexts – respectively, Central and Eastern Europe, and Mali.

in countries of origin focused on the case of Mexico. Electoral outcomes were often described as more aligned with democratic values in high emigration areas, although political engagement and public good provision were observed to be affected positively or negatively depending on the specifics of the analysis.[6]

Spilimbergo (2009) conducted one of the first cross-country quantitative studies on the effects of migration on democratization by examining the impact of foreign education acquired in democratic countries on fostering democracy in student origin countries. He showed that migration may promote democracy, but left the question unanswered as to which specific mechanisms underlie this effect. Docquier et al. (2016) presented cross-country evidence of the positive impact of unskilled emigration from developing countries to OECD countries on the institutional quality of origin countries by using aggregate measures of democracy and economic freedom. The authors found significant institutional gains from the "brain drain" over the long run after considering incentive effects on human capital formation. They attribute these effects to an increase in the exposure of home country population to democratic values and norms. In a related study, Beine and Sekkat (2013) find suggestive cross-country evidence that the transmission of political norms seems to be stronger when emigrants are more educated. Lodigiani and Salomone (2015) describe how international migration to countries with higher female parliamentary participation has a positive and significant effect on the female parliamentary share at origin.

A related branch of literature has focused on the relation between financial remittances and political variables, and how these seem to be strongly correlated. O'Mahony (2013) shows that migrant remittances increase in election years particularly when elections are more contested and the home country poorer. Ahmad (2012, 2013) provide evidence that migrant remittances may deter political change, particularly in autocratic

[6]See, for example, Burgess (2005); Goodman and Hiskey (2008); Pérez-Armendáriz and Crow (2010); Aparicio and Meseguer (2012); Pfutze (2012).

regimes, although this effect may be counteracted by remittances being used to pay for private forms of local public goods - which may reduce the effectiveness of state patronage, and in this way promote political change (Adida and Girod (2010); Doyle (2015); Pfutze (2014); Tyburski (2012)).

Finally, related recent contributions (for example, Miller and Peters (2018); Peters and Miller (2018)) emphasize the role of emigration in reducing violent conflict – while showing that emigration to countries with better institutions may increase the more effective non-violent demand for political change, consistent with our results.

Most of the earlier empirical contributions use aggregate macroeconomic data and explore cross-country variation. For this reason, they cannot distinguish between supply and demand forces, nor capture in detail the mechanisms underlying the effects they identify. Batista and Vicente (2011) provided the first study to use both household-level survey and behavioral data from a voting experiment to examine the differential effects of return and current migrants, while also distinguishing between the impact of different countries of destination with varying degrees of governance. They found stronger results for the impact of return migrants - a result later corroborated by Chauvet and Mercier (2014), Mercier (2016), and Tuccio et al. (2016) which emphasized the role of return migration in promoting political participation and electoral competitiveness in various countries of migrant origin. Batista and Vicente (2011) also showed how improved levels of governance in different host countries (namely the United States relative to Portugal) positively influenced the magnitude of the migratory impact on demand for more political accountability. Barsbai et al. (2017) also support these findings by exploiting community and individual-level data from Moldova, as well as migration patterns to countries with different political regimes. In particular, they find that exposure to Western democratic values and norms promoted political change in municipalities with a higher number of emigrants. While the approach by Batista and Vicente (2011)

is innovative in the sense that it employs behavioral data, and points towards return migration from countries with better quality institutions as the driving force for the effect of emigration on political attitudes and behavior in countries of origin, it cannot explain how individual-level relationships with migrants affect the demand for better political institutions.

A different strand of literature focuses precisely on the diffusion of political values through social networks. Fafchamps and Vicente (2013) and Fafchamps et al. (2018) show that increasing the political literacy of experiment participants changed perceptions and electoral behavior, respectively, for those participants with more network connections, even if they were not directly targeted by the literacy campaign. Giné and Mansuri (2018) relate closely to this idea as they find positive spillover effects of an awareness campaign in Pakistan on female voter turnout. Similarly, Nickerson (2008) finds that about 60% of the propensity to vote is passed on to another household member in a randomized controlled trial in the United States. These findings suggest that norms about political participation are adopted and passed on to peers.

Our paper contributes to the existing literature in at least three different ways. First, our work innovates by examining the diffusion of political norms and information about electoral processes through different types of migrant networks – which we measure using detailed data on geographical networks, kinship networks, and chatting networks. More generally, our paper contributes by using a variety of political participation measures (self-reports, behavioral and actual voting measures) showing that stronger links with international emigrants increase the likelihood of domestic political participation by those left-behind. Finally, we contribute by studying the case of Mozambique, a country with substantial South-South emigration, almost exclusively to other sub-Saharan African countries. This is a setting where both migrant countries of origin and destination are imperfect democracies, and where the empirical question of

whether migrants can transfer improved political norms is not trivial or captured by the existing literature.

1.3 Theoretical Framework

Political participation is traditionally modeled as the outcome of an expected cost-benefit analysis.[7] An individual's benefit from political participation is defined as the expected utility derived from the outcome of a political process, and from an individual's intrinsic motivation. The cost of casting a vote can be broadly thought of as including the opportunity cost of going to the local polling station or the cost to obtain the necessary information about election candidates. An individual j can thus be thought of as maximizing the following expected utility function

$$\max_{x_j} E_{\Omega_j} U\left(G\left(x_j, x_{-j}\right), I_j\left(x_j; P_{c_j}\right)\right) - cost\left(x_j\right) \tag{1.3.1}$$

where the outcome of a political process is described by the function $G\left(x_j, x_{-j}\right)$, x_j is the action vector of individual j, and x_{-j} reflects the combined action of all individuals other than j; I_j is individual j's intrinsic motivation; P_{c_j} is individual j's prescribed behavior given his assignment to social category c_j; Ω_j is the information set available to individual j; and cost(x_j) is the cost for individual j of taking action x_j. Note that in this framework own actions and actions taken by others do not enter the utility function directly as, for example, casting a vote might not necessarily directly impact one's utility. The individual maximizes its net expected utility of taking a certain action given the actions of everybody else.

We define intrinsic motivation through an individual's identity, following Akerlof and Kranton (2000). For this purpose, let there be a set of social categories $\boldsymbol{C}$.[8] An in-

[7] See Dhillon and Peralta (2002) for a detailed description.

[8] A social category could be gender or ethnic group, though our model allows for more complex or

dividual j assigns himself to one of these categories, c_j, given his characteristics, ε_j. The determining characteristics that we take as drivers of political behavior through identity, and are thus relevant in the context of this paper, are an individual's gender, age, income, and most importantly for our case the society (which can be summarized by the geographical location) this individual lives in. Note that individual self-assignment may be unconscious, and differ from the social category others might assign an individual to. Each individual furthermore has a notion about the social categories of all other individuals, c_{-j}.[9]

Whether or not an individual derives utility gains or losses from intrinsic motivation is determined by the individual's actions, x_j, and whether or not these actions are according to the prescriptions P_{c_j} associated with the individual's social category c_j. We can think of these prescriptions as widely accepted norms that individuals follow to maintain their self-image. In the context of political participation, the impact of intrinsic motivation can be illustrated by the following example: in a society where casting a vote is the social norm, an individual might decide to vote despite no direct expected net benefits from it, as he derives intrinsic motivation utility gains by acting according with the social norms.

In this context, the set of prescriptions P_{c_j} can be described as:

$$P_{c_j} = P\left(x_{-j}; c_j\left(\varepsilon_j\right)\right) \tag{1.3.2}$$

where prescriptions P_{c_j}, that determine an individual's behavior, firstly depend on the social category c_j individual j attributes himself to. Belonging to this social category itself depends on characteristics ε_j of individual j, such as his geographical location.

narrower definitions of a social category.

[9]Being able to classify others in a social category (or box) helps an individual to interpret the behavior of others as appropriate or not, and copy behavioral patterns of peers belonging to the same social category.

Prescriptions also depend on the actions of others, x_j, to the extent that they reflect the behavior of other individuals perceived as belonging to the same social category and in this way, establish the prescribed standard of social norms.

The solution to the expected utility maximization problem (1.3.1) yields that the individual's expected marginal payoff from political participation has to be at least as high as the marginal cost of action.

$$E_{\Omega_j} U'_{x_j} \left(G\left(x_j, x_{-j}\right), I_j\left(x_j; P_{c_j}\right)\right) = cost'_{x_j}\left(x_j\right) \tag{1.3.3}$$

The theoretical framework just described allows us to examine two distinct channels through which migration may affect political behavior: a change in an individual's identity and thereby intrinsic motivation for political action, and a learning mechanism based on increased knowledge about political processes.

An individual that emigrates becomes exposed to a different environment. This change in surroundings affects the migrant's social category self-assignment, as it depends on the individual location. As the prescribed behavior P_{c_j} depends on individual j's social category, the individual faces different prescriptions after emigration. To avoid net utility losses, the individual migrant should update her political behavior x_j accordingly. This direct impact of migration on x_j may be thought of as what happens when an individual migrates and adopts different standards of political behavior – while he is still abroad or upon return to the home country.

A second more indirect effect of migration on political behavior may happen through the actions of others, independently of own migratory experiences. This effect may happen if peers in individual j's network have migratory experiences and their changed behavior is relevant to define P_{c_j}. As prescriptions are influenced by peers' actions, our framework predicts that migration can in this way change the behavior of non-migrants indirectly. This is the case if the opinion of peers, mirrored in their actions, has enough

weight within a social category to influence existing prescriptions.

The second channel through which migration may affect political behavior is through learning about political processes. If migration changes the information set Ω_j available to an individual j (for example by learning about democratic processes in the host country and their value), potentially increasing the value of political participation, the net marginal benefit of voting may increase and lead to more active political participation. The same effect may take place through the migratory experiences of peers that are a part of individual j's social network, and which can contribute to enlarging this individual's information set Ω_j, and in this way contribute to changing political participation of individual j residing in the country of origin.

1.4 Country Context: Mozambique

This study examines migration between Mozambique, and (to a large extent) its neighboring African countries such as South Africa, Malawi, and Tanzania. Mozambique is considered to be one of the poorest countries in the world with a GNI per capita of only 1.140$PPP in 2014. Despite its high growth rates of 7.14% on average between 2000 and 2014, Mozambique is still ranked 178 out of 187 countries in the Human Development Index. For many years, Mozambique has been an aid-dependent country. In 2013, for example, the country received official development assistance of almost 15% of its GNI (US$2.3b).[10]

The majority of the Mozambican population, around 78% in 2009,[11] is directly dependent on agriculture. Climate change is a major threat to these livelihoods as Mozambique is exposed to extreme weather events that have often affected several dozens of

[10]World Development Indicators (2015), World Bank.
[11]International Labour Organization, ILOSTAT database.

thousands of people in the last two decades.[12] The international donor community generally heavily supports emergency relief and rehabilitation programs in response to natural disasters, replacing the role of the Mozambican government to a large extent, as the Mozambican government does not have the necessary resources for disaster relief. This situation is particularly well documented since 2000.[13]

As a consequence, Mozambique has been an emigration country for a long time. Large migratory movements from Mozambique were traditionally labor-driven mainly from the southern Mozambican provinces to South African mines and commercial farms. In 2013, (formal) migrant remittances flows contributed towards GDP with 1.4% with inflows of approximately US$217 million.[14] According to estimates provided by the World Bank (2011),[15] the stock of Mozambican emigrants in 2010 was 1.2 million, or 5% of the resident population.[16] According to these nationally representative statistics, the main international destinations of Mozambican current emigrants in 2010 were South Africa, Malawi, Zimbabwe, Tanzania, Portugal, Swaziland, the United Kingdom, Germany, the United States, and Spain.[17]

Historically, after its independence from Portugal in 1975, as a result of ten years of war, Mozambique was led by the independence movement FRELIMO (Frente de Libertação de Moçambique) under a single-party, socialist regime. Only two years after independence had been negotiated, a civil war erupted between FRELIMO and

[12]Red Cross Mozambique (2013).

[13]In 2000, for example, a major flood hit the country and Mozambican President Chissano recognized in front of reporters that international aid was arriving very slowly to assist the victims of the flooding as reported in the Southern African Research and Documentation Centre's report in May 2000. Information available from http://reliefweb.int/report/mozambique/mozambique-natural-disasters-floods, last accessed on August 30, 2017.

[14]World Development Indicators (2015), World Bank.

[15]World Bank Migration and Remittances Factbook (2011), Second Edition. Washington, DC: World Bank. Available at https://openknowledge.worldbank.org/handle/10986/23743

[16]This is consistent with the large prevalence of migration evident in our survey, as illustrated by Table 1.1.

[17]This is reflected in our survey data where around 87% of emigrants went to South Africa as displayed in Table 1.2.

RENAMO (Resistência Nacional Moçambicana) that created large refugee movements to neighboring countries. With the end of the cold war, and the collapse of apartheid in South Africa, FRELIMO and RENAMO started negotiations that resulted in a new constitution allowing for a multi-party system, and a peace treaty signed in 1992. The newly established peace encouraged many of the refugees to return to their homes in Mozambique.

After the peace treaty, presidential and parliamentary elections were held in 1994, 1999, 2004, 2009, and 2014. FRELIMO won all these elections by a large margin and increased its vote share consistently. Across all national elections, electoral irregularities (mainly claimed by RENAMO, but also confirmed by international observers) had significant consequences for the overall results. The 2009 elections, the time around which our data has been collected, are considered to have followed international standards, despite small irregularities. Both Armando Guebuza, the Mozambican president from 2005 until 2015, and FRELIMO were elected unambiguously by 75% in 2009.

A variety of sources considers that the quality of democracy in Mozambique is imperfect. The V-DEM Electoral Democracy Index[18] was 1.89 for Mozambique in 2009, and 3.06 for South Africa, for example - a substantial statistically significant difference showing the potential for Mozambican migrants to South Africa to adopt political norms that are an improvement, in this sense, to those prevalent in their home country. Consistently with the V-DEM scores, Mozambique's political system is scored as 5 by the Polity IV index,[19] and classified as an "open anocracy" from 2009

[18]The V-DEM Electoral Democracy Index measures the extent to which the rulers are "responsive to citizens, achieved through electoral competition for the electorate's approval under circumstances when suffrage is extensive; political and civil society organizations can operate freely; elections are clean and not marred by fraud or systematic irregularities; and elections affect the composition of the chief executive of the country". See Coppedge et al. (2016) for additional detail.

[19]The Polity IV index classifies levels of democracy based on an evaluation of the competitiveness and openness of elections, the nature of political participation, and the extent of checks on executive authority. For each year and country, a "Polity Score" is determined which ranges from -10 to +10, with -10 to -6 corresponding to autocracies, -5 to 5 corresponding to anocracies, and 6 to 10 to democracies.

until 2017. South Africa, in contrast, was scored as 9 and classified as a "democracy" over the same time period. The Freedom House's Index of Freedom in the World currently classifies Mozambique as a "partly free country" where citizens generally show difficulties in grasping the importance of democracy, with a score of 52/100, whereas South Africa scores 78/100 and is considered a "free country". Finally, the Economist Intelligence Unit's (EIU) Democracy Index[20] ranks Mozambique 115 (out of 167), and classifies its political system as a "hybrid regime" (bordering the classification as an "authoritarian regime"). South Africa, in comparison, ranks 41 and is classified as a "flawed democracy" similar to the United States or Japan. Overall, these different measures point to the quality of democracy being generally low in Mozambique, and significantly lower than in South Africa.

Political participation is most closely related to the type of political attitudes and behavior we measure in our paper, and proxies for the type of political norms that Mozambican migrants may learn about while abroad and potentially transmit through their social networks. Two different indices confirm that Mozambican emigrants may experience improved political participation in South Africa relative to their home country. In 2009, the V-DEM Participatory Democracy Index[21] for Mozambique was 1.19 and for South Africa was 2.10, a substantial statistically significant difference. We should note, however, that this gap is lower than that observed when simply comparing the more general V-DEM Electoral Democracy Index. The partial EUI political participation index[22] awards Mozambique 5 out of 10 points, whereas South Africa scores

[20]The EIU Democracy Index is constructed based on 5 pillars: electoral process and pluralism, functioning of government, political participation, political culture and civil liberties.

[21]The V-DEM Participatory Democracy Index "embodies the values of direct rule and active participation by citizens in all political processes. While participation in elections counts toward this principle, it also emphasizes non-electoral forms of political participation, such as civil society organizations and other forms of both non-electoral and electoral mechanisms of direct democracy".

[22]Political participation is defined by voter turnout, autonomy and voice of minorities, participation of women in parliament, participation in political parties and NGOs, interest or engagement in politics, attendance of lawful demonstrations, adult literacy, interest in politics in news, and effort to promote political participation.

8.33 - the highest ranked country, Norway, scores 10.00. The evidence we find on the role of international migrant networks in transmitting attitudes and behavior related to political participation suggests that it is in this sense that emigration might be a promoter of broader democracy at home.

1.5 Data and Descriptive Statistics

The household survey data used in this paper was collected in Mozambique from mid-September until November 2009 by the CSAE at the University of Oxford. This timeframe corresponds to the period before and immediately after national elections took place. The data collected are nationally representative of the voting population of Mozambique that has mobile phone coverage. The fieldwork covered four out of the eleven provinces of the country (Cabo Delgado, Zambezia, Gaza, and Maputo-Province), and included 161 enumeration areas and 1766 households. Both Cabo Delgado and Zambezia are located in the North of Mozambique, whereas Gaza and Maputo-Province are reflective of the Southern provinces of the country. During the 2007 census around 37 percent of the Mozambican population lived in these four provinces combined.

The sampling base we used was the 2004 electoral map of the country, and the enumeration areas (EAs) were polling station catchment areas. Because the use of cell phones was necessary for the construction of our behavioral political participation measure (which made use of cellphone text messages),[23] we eliminated from the sampling base all polling locations without cell phone coverage. For this purpose, we obtained detailed data from the two cell phone operators on the geographic location of each of their antennae. These were then plotted on a map using their geographical coordinates, with a 5-km coverage radius drawn around each. All polling stations outside the covered

[23]For a detailed description of this measure, see Section 1.5.2 below.

area were dropped from the sampling base. In 2009, 60 percent of all polling stations in the country were covered by at least one operator.

From this sampling base, 161 polling locations were selected using two-stage clustered representative sampling on provinces, then on EAs. The number of registered voters per polling location is used as sampling weight. Since all registered voters in the sampling frame have the same probability of being sampled, the surveyed locations are nationally representative of the voting population of Mozambique that has mobile phone coverage. During the baseline survey, in the event that we found no cell phone coverage in a selected location, we replaced it by the closest polling location with cell phone coverage. This happened in seven locations.

Sampling within each EA followed standard procedures for household representativeness: nth house call by enumerators, starting from the polling station - typically a school located at the center of the EA. In each EA, approximately 11 households were interviewed. Our social network measures reflect the relationships between the household heads of each of these eleven households. Due to random sampling of households, our network measures are representative of the true, full social networks of each household within their EA.

Interviews at baseline were directed at the household head or his/her spouse. Interviews were conditional on having access to a cell phone for receiving and sending calls and messages. Respondents that did not own a cell phone but had access to one via a neighbor or family member nearby were included in the study. In each of the EAs, we conducted two face-to-face household surveys, one before the election, and one immediately after.

1.5.1 Descriptive Statistics

The importance and magnitude of international migration in Mozambique is reflected in Table 1.1, which illustrates the percentage of households with migrants in our sample. It shows that almost 33% of all households report having at least one migrant, and only 17.5% of households live in villages where no geographical neighbors ever migrated. Approximately 41% of households have a family member living in a different household than their own, that is currently or has been living abroad.This number increases slightly to around 48% of households that indicate to be regularly chatting with international migrant households.[24]

The migratory experiences in our dataset are mainly determined by emigration to South Africa, which accounts for about 87% of all destination countries. The other main migrant destinations are neighboring countries such as Tanzania, Zimbabwe and Malawi.[25] A detailed description of the frequency of different destination countries can be found in Table 1.2.

[24]Given that the average number of individuals per household in our sample is 5.87, the 5% national emigration rate provided by the World Bank Migration and Remittances Factbook (2011) seems rather consistent, although slightly higher, than the numbers obtained in our survey, where there were 0.21 current emigrants per surveyed household (the national emigration rate would imply 0.29 migrants per household). This slight undercount (0.08 missing migrants per household) is understandable in light of the method used to identify current migrants: only spouses and children of the household head were included in our dataset. This implies that we do not include any migrants that left with their whole families. But given that about 90% of emigration is to South Africa and that this is mostly circular migration, our method of identifying migrants does not seem to induce large undercounts. Moreover, because our objective in this paper is to measure the impact of emigration on domestic politics via contact with migrants, our survey's undercount does not seem problematic as the emigrants underrepresented are those less likely to keep active contact with their home country.

[25]This distribution is consistent with information from the World Bank Migration and Remittances Factbook (2011), and from census data on Mozambican emigrants for South Africa (8.6% sample of 2011 census), Malawi (10% sample of 2008 census), Tanzania (10% sample of 2012 census), and Portugal (5% sample of 2011 census) from IPUMS (2018). Minnesota Population Center. Integrated Public Use Micro-data Series, International: Version 7.0. Minneapolis, MN: IPUMS, 2018. https://doi.org/10.18128/D020.V7.0

Table 1.1: Migration - Household Characteristics (%)

	Number of Links	Migration Experience (%)
Households with at least one migrant		32.41
Migrant households in geographical network	0	17.5
	1	15.63
	2	10.48
	3	8.1
	4	11.1
	5	13.02
	6	6.85
	7	5.55
	8	4.25
	9	5.66
	10	1.87
Kinship relations with migrant households	0	58.28
	1	24.28
	2	7.89
	3	4.34
	4	2.34
	5	1.04
	6	1.47
	7	0.09
	8	0.09
	9	0.17
Chatting relations with migrant households	0	51.78
	1	23.59
	2	8.76
	3	5.55
	4	4.42
	5	2.43
	6	1.91
	7	0.69
	8	0.52
	9	0.35

Table 1.2: Destination Countries of All Migrants (%)

South Africa	86.62
Tanzania	5.16
Other African	1.64
Zimbabwe	1.41
Malawi	1.17
Swaziland	1.17
Other European	0.94
Portugal	0.70
Germany	0.47
Other	0.47
Cuba	0.23

Almost half of our sample is composed of women, and the average age is approximately 37 years as shown in Table 1.3. The education a respondent received is rather limited with approximately six years of schooling on average (primary education).

Table 1.3: Summary Statistics. All Households.

Variable	Obs.	Mean	Std. Dev.	Min.	Max.
Inked Finger Indicator	1112	0.29	0.45	0	1
Self-Reported Voting	1112	0.92	0.28	0	1
Learning-Corrected Self-Reported Voting	1112	0.85	0.36	0	1
Sending Text Message	1138	0.18	0.38	0	1
HH Head Female	1138	0.44	0.5	0	1
HH Head Age	1130	37.38	13.7	18	88
HH Years of Schooling	1136	5.79	4.09	0	18
Total Access to TV, Radio or Computer	1138	1.14	0.85	0	3

1.5.2 Detailed Description of Main Variables of Interest

Our main outcome variable of interest is the respondents' actual voting during the 2009 national elections. We furthermore complement our analysis by using self-reported voter turnout, an additional measure that corrects self-reported voting for learning about

electoral processes, and an alternative behavioral measure reflecting the experimental subjects' intrinsic desire to communicate their own policy priorities.

1.5.2.1 Actual Voting Measure

To obtain a measure more closely related to actual voting behavior, as opposed to simply limiting ourselves to analyzing self-reported voting behavior from the survey, we followed individuals through the 2009 elections and asked them to show us the finger that was inked after having voted. If the interviewer observed a correctly inked finger (i.e. respondents correctly identified the finger that was inked after having voted and the ink was still observable to the interviewer), we interpret this proxy as the respondent having actually voted. Table 1.3 shows that almost 30% of household heads voted in the 2009 elections as proxied by this outcome measure.[26]

Migrant networks might influence actual voting behavior as the contact with migrants may change respondents' political participation, namely through the combined mechanisms proposed by our conceptual framework. They might vote in compliance with changed political participation norms, as well as a result of having learned about the importance of elections in democratic regimes.

1.5.2.2 Self-Reported Voting Measure

We also use a standard survey question on whether the respondent reported having voted. Almost 91% of the respondents in our sample claimed to have voted during the 2009 elections. The contrast with our actual voting measure suggests a strong conformity bias where many respondents report to have voted without having done so.

[26]This participation rate is actually lower than 44%, which is the participation rate reported by the Mozambican electoral authorities using official electoral data. This has probably to do with the fact that our field team could not visit all households immediately after the election, and that the ink could have washed out over that time interval. The lag between our visit and the election was not systematically related to prevalence of migration, so that this underestimation of actual voting is not likely to affect our analysis.

Migrant networks might influence self-reported voting behavior as the contact with migrants may change respondents' attitudes towards political participation – although not necessarily their actions. In particular, self-reports of voting may be higher for migrant connected respondents since they may be better informed about the importance of political participation, and hence value it more and understand it as desirable behavior – even if this improved information did not create a strong enough net benefit to make our respondents actually vote.

1.5.2.3 Learning-Corrected Self-Reported Voting Measure

We furthermore make use of one more measure of self-reported voting, conditional on the respondents not only reporting to have voted, but also being able to show the correct finger that was inked after voting - even if the interviewers could not observe ink stains anymore. This measure includes 85% of the respondents in our sample as shown in Table 3. We take this measure as a proxy for information about voting procedures, which can be understood in the context of our study. Indeed, the data collection was conducted in rural areas where individuals live relatively close to each other in village settings. As the ink stain will be visible on those individuals that voted for several days (even after washing their hands), individuals that are in close contact with individuals who voted (which is more likely to happen in migrant households) will see more inked fingers, likely ask about the reason why this finger was inked, and hence learn about the finger inking procedure after voting. We propose that this form of contact will lead to increased knowledge about electoral processes, even if the individuals in our sample had no interest in learning about voting procedures or in actually voting. Of course, this is an imperfect measure of information about electoral processes, as it is only one detail about voting procedures. But the fact that 85% of respondents could indicate the right finger (significantly above the 50% one would get if answers were given at

random), when only 29% of respondents had their finger actually inked, indicates that this measure conveys valid information.

A positive impact of being in a migrant network on the correct finger indication but not on our actual voting measure can thus be interpreted as evidence supportive of migrant networks improving information about electoral processes, beyond changing the respondent's behavior by changing social norms. A closer connection with migrants may act as an information transmission channel - not only about the importance of political participation, but also about the electoral process itself. If respondents most tightly connected with migrants, differentially self-report not only to vote more often, but are also able to correctly show the inked finger, we can take this evidence as suggestive that migration is acting as an information channel emphasizing not only the importance of casting a vote (as otherwise individuals should not feel the need to misreport actual voting behavior), but also specific details about the electoral process.

1.5.2.4 Behavioral Political Participation Measure

Finally, we also conducted a simple behavioral experiment with our survey respondents. We proposed respondents the option to send cell phone text messages suggesting policy priorities for the president-elect's mandate. These suggestions would be forwarded to an independent Mozambican newspaper that would in turn publicize these suggestions, namely to the president-elect himself. This promise was made credible by the public official support of the newspaper to this initiative. Note that since sending a SMS message entails a small direct cost,[27] our measure is a costly action, which we interpret as an incentive-compatible measure of political participation.[28] As shown in Table

[27]The cost of sending a text message is small in the sense that it is not high enough to imply financial constraints to political participation for respondents. There is also the time cost of taking the action itself.

[28]We were able to identify the individual survey respondents that sent messages through cell-number matching. This matching was easy to achieve since participation in this study was conditional on having access to a cellphone as discussed above.

1.3, 18% of respondents sent SMS messages with their policy priority requests. Since experimental subjects were invited to send policy suggestions about any policy topic of their interest,[29] we interpret an increase in the likelihood of sending a text message as a higher desire to participate in the design of the government's political agenda, and thus increased intrinsic motivation for political participation.

1.5.2.5 International Migrant Networks

A household is considered an international migrant household if at least one of the household members is currently living or has ever lived outside of Mozambique for at least six months.[30] To obtain the number of migrants an individual is connected with through her social network, we interact this migration variable with the network links across all households within one enumeration area.

Our migrant network variables allow us to distinguish between network effects according to the social proximity of two survey respondents. This means that we not only evaluate the overall number of links with migrant households in a respondent's geographical network (i.e. within the same EA), but also, most innovatively, the number of migrant households in an individual's chatting and kinship network.

A chatting link is recorded if a respondent indicates to regularly talk with another respondent.[31] Note that the surveys were conducted in a rural setting and all respondents live in the same village. This implies that individuals normally chat personally with each other rather than through any intermediary platforms. We calculate kinship

[29]The policy priorities suggested were not linked to interventions related with government responses to natural disasters. This further supports our argument about the exogeneity of our natural disaster exclusion restriction.

[30]This definition of migrant household includes the household head: if he/she has ever lived outside of Mozambique for at least six months, his/her household will be considered a household migrant.

[31]The exact phrasing of the survey question used to define a chatting link was "How frequently do you calmly chat about the day events with the following individuals or members of their households? Not at all, sometimes, or frequently". We considered a link existed when the individual answered "sometimes" or "frequently".

links in the same way if some individual reports to be related to another respondent or members of her household by family links.[32] We allow for this link to be directed, i.e. a one-sided existence of a link is sufficient, as the concept of social categories is subjective, and does not need to be consistent across individuals.

The degree of connectedness with migrant households of a specific respondent is calculated according to each network's link classification as the total number of migrant households the respondent is connected to. Table 1.1 illustrates the distribution of network connectivity in our sample. Around 32% of all households are classified as being a migrant household. Only 17.5% of respondents live in a village where not a single household has a household member that is currently living or ever has lived abroad. This number changes dramatically considering kinship and chatting networks. Around 43% of respondents have kinship links to at least one migrant household and approximately 48% of respondents regularly chat to migrant household members.

1.6 Empirical Strategy

To test our hypotheses, we build an econometric model based on the theoretical framework described in Section 1.3. The relationship between emigration and political behavior is estimated for different outcome variables that reflect a respondent's political participation. The probability of political participation can be estimated with the following Linear Probability Model (LPM):

$$y_i = \alpha + \beta \sum_{j \neq i} (Network_{ij} \times migHH_j) + \delta X_i + \varepsilon_i \tag{1.6.1}$$

where y_i is an indicator variable denoting an individual's political participation,

[32] A kinship link between two households exists if the following question was responded positively: "Are the following individuals or members of their household relatives of yours, i.e. members of your family? Yes-No".

and X_i represents a vector of individual and geographic characteristics determining the likelihood of political participation. This vector includes demographic controls that determine the identity of an individual such as gender, and age. To capture effects arising from an enlarged information set, this vector furthermore includes the levels of schooling completed, as well as the access to information provision (such as radio, television, or internet access). We also control for the respondent's own migration history. Standard errors are clustered at the village level.

The binary variable $Network_{ij}$ indicates whether or not individual i is has a directed link to individual j. This variable takes value 1 if individual i reports to be connected with individual j, independently of the link reported by individual j. This specification is preferred to an undirected link between two individuals (where a link reported by only one of these individuals triggers a connection between them), because we are specifically interested in the effect of different types of migrant networks. Indeed, as described before, we construct three types of network variables (geographical, kinship and chatting networks), where the potential link corresponds to, respectively, the two respondents living in the same village, having a kinship relation, and regularly chatting with each other. Constructing social networks based on undirected links would bias our estimates of the impact of chatting and kinship relationships towards the estimation results on the impact of geographical networks because it would not account for the social proximity between two households, as measured by the chatting and kinship relationships. These relationships are particularly well captured by directed networks. The individual j's household is classified as a migrant household ($migHH_j = 1$) if any of its members ever emigrated. The sum over all j's (not including i) of the interaction term, $\sum_{j \neq i} (Network_{ij} \times migHH_j)$, determines the total number of migrant households individual i is connected with, excluding her own household.[33]

[33]Our results are robust to different definitions of the social migrant network such as including other household members' or the respondent's own migration experience in the network indicator.

1.6.1 Two Stage Least Squares Estimation

1.6.1.1 Potential endogeneity of migration decisions

This paper aims at determining the impact of different types of network links with migrants on political behavior. The main threat to identification is that individual migration decisions may be correlated with individual political participation through unobservable factors that cannot be controlled for using a Linear Probability Model. If so, our network variable would capture the effect of being connected with more individuals with particular political attitudes rather than the effect of being connected with more individuals that have been exposed to a different political environment through international migration. This would imply a correlation between our explanatory variable and the regression error term. We may face an omitted variable bias if individuals that are less (or more) politically active opt to emigrate to another country more often than people that participate in politics more (or less) often. In the case of Mozambique, the ongoing political instability, high corruption, and subpar working of democracy can affect individuals in their decision to leave the country.

To tackle this issue, we use a Two Stage Least Squares (2SLS) estimation approach.[34] We exploit the exogenous variation in the occurrence of natural catastrophes affecting harvests and cattle as sources of emigration. We make use of detailed data on natural disasters in Mozambique at the district level, allowing for large variation across EAs. In addition, we constructed an individual-level instrument by interacting the occurrence of droughts in the district of a respondent's village with her birth-year.

The instrumental variable for each household takes the value of the cumulative

As a robustness check to verify whether excluding the respondent's own migration experience as a covariate changes our estimation results, we run all regressions without controlling for the respondent's own migration experience as well. Our results are robust to including covariates controlling for the respondent's own migration experience or migration spells of other household members.

[34]Our results are robust to the estimation of an IV probit model instead of the 2SLS model.

number of droughts in the ten years prior to the respondent becoming 31 years old.[35] This instrument measures the intensity of droughts around the age at which household heads migrate, as measured in our survey. Especially in rural areas (the context of our study), harvests and cattle are often the livelihood of families, as there are almost no income sources from salaried work. We therefore expect the occurrence of a natural disaster to be highly correlated with an individual's decision to migrate in order to provide for her family. Our instrumental variable is indeed highly correlated with household migration as natural disasters substantially increase the pressure to emigrate in order to provide for the family back home. The reported F-statistics (shown in Tables 4 to 6) confirm our reasoning.

In the Mozambican context, weather shocks are unlikely to be correlated with political attitudes and behavior other than through migration. As described in the country context section, responses to natural disasters in Mozambique are provided by the international aid community as the Mozambican government has no resources to provide emergency relief programs.

As a robustness check, we also used alternative drought shocks to instrument for migrant selection. Over-identifying restriction tests displayed in column (4) of Tables 4 to 6 lend support to the exogeneity of our instruments for all outcomes of interest and all types of migrant networks. This alternative instrumental variable for the decision to migrate is constructed using the cumulative number of droughts in the ten years after the respondent becomes 29 years old and prior to being 40 years old.[36] We thus argue that our exclusion restriction fulfills the two necessary and sufficient criteria to be used as a valid instrumental variable.

[35] Our results are robust to the use of similar IVs constructed with different types of weather shocks as well as different age thresholds and time spans. The weather data used are from the UNDP (2013) DesInventar database.

[36] In other robustness checks, we used alternative drought shocks to instrument for migrant selection, and additional over-identifying restriction tests also lend support to exogeneity of the instruments we used. These results are available from the authors upon request.

The instrumental variables we use to account for self-selection of emigrants are constructed in two steps: We first interact the number of droughts a neighboring household was exposed to (in accordance with the above definitions) with our binary indicator of whether a network link exists between our respondent and the respective household. Second, we sum all interaction terms within the respondent's respective enumeration area.

We estimate the following 2SLS model:

$$y_i = \alpha + \beta \sum \widehat{Network_{ij}} \times migHH_j + \delta X_i + \varepsilon_i \tag{1.6.2}$$

$$\sum \widehat{Network_{ij}} \times migHH_j = \alpha + \theta_2 \sum Network_{ij} \times Droughts_j + \delta X_i + \varepsilon_i \tag{1.6.3}$$

This specification takes the endogenous decision to migrate into account by replacing the migrant network connectivity of individual i with the predicted migrant network connectivity based on our proposed exclusion restriction. The vector X_i contains individual and geographic controls as stated before.

1.6.1.2 Potential endogeneity of network formation

A second endogeneity concern arises from how network links are being formed. As recognized by Manski (1993), it is possible that there is endogeneity in the formation of migrant networks in that unobserved characteristics of migrant households are likely correlated to those of households in their networks – the well-known "reflection problem". In the context of our paper, if individuals are more likely to be friends (as is captured by our chatting network measure) with households with similar political attitudes, our explanatory variable would be correlated with the regression error

term. Similarly, kinship relationships might be endogenous through marriage preferences based on political attitudes and behavior.

For this reason, following the strategy proposed by Bramoullé et al. (2009), we propose to use undirected secondary links to migrant households as an exclusion restriction to identify the effects of the primary directed links to migrant households on the political outcomes we study. In the undirected network specification, we disregard the direction of influence such that a unilaterally reported link triggers a network indication for both households. More specifically, we instrument the respective network indicator with second-degree links between households. We compute the adjacency matrix between all households within an enumeration area and replace our original network variable with a binary indicator equal to one if and only if, two households are connected with each other through a third household. By construction, this variable is highly correlated with the initial direct network variable, but is unlikely to be correlated with individual political participation decisions as the two households do not chat with (or marry) each other directly.[37]

The reported F-statistics (shown in columns (5) and (6) of Tables 5 and 6) confirm the strength of the constructed instrument. When using alternative drought shocks to instrument for migrant selection, over-identifying restriction tests lend support to the exogeneity of the instrument as displayed in column (6) of Tables 5 and 6.[38] Because the estimates displayed in columns (5) and (6) of Tables 5 and 6 (obtained when accounting for self-selection into social networks) are generally larger than when this potential endogeneity is not accounted for, we conclude that even if the second-degree link instrument is not fully exogenous and is still at least partly driven by unobserv-

[37]Further, the undirected nature of the secondary network meets the identification condition of linear independence as formally shown by Bramoullé et al. (2009).

[38]The over-identifying restriction tests are constructed using the cumulative number of droughts in the ten years prior to the respondent being 29 years old, and prior to being 40 years old as an additional instrumental variable for the decision to migrate, and second-degree network links to control for endogenous network creation for the kinship and chatting network.

able characteristics related to political participation, our estimates seem to be biased downwards and thus understate the impact of migrant networks on changes in political attitudes and behavior.

We interact the network link variable with the same instrumental variable on natural shocks on a household (head) level as in our primary specification. We proceed by constructing the final instrument as the sum of interactions between a binary indicator of the existence of a second-degree link and the neighboring household's exposure to droughts as before. The final IV is then the sum of the total number of natural shocks that occurred to household heads to which the respondent is connected with through secondary links.

This is reflected in the following modification to the instrumental variable of the 2SLS model:

$$\widehat{Network_{ij} \times migHH_j} = \alpha + \theta_2 \sum Second - Degree\ Link_{ij} \times Droughts_j + \delta X_i + \varepsilon_i \tag{1.6.4}$$

This specification takes into account both the endogenous decision to migrate, and the endogenous creation of networks by simultaneously replacing the migrant network connectivity of individual i with the predicted migrant network connectivity based on our proposed exclusion restriction regarding individual migration decisions and network formation. The vector X_i contains individual and geographic controls, as stated before.

1.7 Empirical Results

In this section, we summarize the main empirical results. We first discuss the evidence on the relationship between geographical proximity to migrants and voting behavior.

The subsequent subsections go further in detailing how kinship and chatting relations with migrants may contribute to explaining the results obtained for geographical networks.

1.7.1 Geographical Proximity

The existing evidence on the role of international migration in shaping political attitudes and behavior, including our own conceptual framework, proposes that a higher number of migrants within a village should increase the political participation of others living in the same village. Under our hypothesis that migration increases the benefits of political participation and creates positive spillover effects, we would expect a positive effect of geographical migrant networks on voting behavior. This positive effect would be the result of Mozambican migrant destinations being mainly countries with a higher democracy index, and higher political participation.[39]

As shown in Table 1.4a, the empirical estimates obtained are in line with our theoretical predictions. Column (1) of Table 1.4a shows a positive and highly significant increase of 2.3 pp in the probability of actual voting per additional migrant household in the village according to a simple LPM estimate. Column (2) of Table 1.4a reports 2SLS estimates accounting for the endogeneity of the migration decision of peers in the same village, using as instrumental variable a measure of the cumulative exposure to droughts experienced by each household when the household head was between 20 and 30 years old. Column (3) reports similar 2SLS estimates also controlling for the respondent's own migration history. The 2SLS estimates confirm the LPM results, and somewhat increase the magnitude of the estimated coefficient: one more migrant household in a village increases the likelihood to vote in that village by between 3.3

[39]According to the various sources described in section 1.4, and despite the fact that the better political norms at destination being generally considered imperfect.

pp and 3.4 pp. As an additional robustness check, we furthermore report results for 2SLS estimates with two instrumental variables in Column (4), where the second instrumental variable is a measure of the cumulative exposure to droughts experienced by each household when the household head was between 30 and 40 years old. The estimated impact of one more migrant household in a village further increases to a 4.1 pp rise in the voting probability. Overall these empirical results support the prediction of our conceptual framework and past findings in the literature that migrant geographic networks promote political participation.

The magnitude of this positive result decreases when analyzing the impact on self-reported voting behavior, particularly under the LPM specification as shown in column (1) of Table 1.4b where this coefficient becomes close to zero. All the 2SLS estimates in columns (2) - (4) of Table 1.4b confirm the effects found for the actual voting measure, with a significant estimated impact of an increase between 1.6 and 1.9 pp in the probability of self-reporting to vote in presence of an additional migrant in the village.

Consistent with the existing literature, households in villages with more migrants, are found to be more politically active, although migrants seem to have a smaller effect on self-reported than actual voting. This difference can be explained by the conformity bias and resulting over-reporting of voting behavior discussed above, which may reflect an increased perception of the value of political participation because of information conveyed by migrants, as discussed in the previous sections.

We estimate a significantly stronger impact of geographical networks on the learning-corrected measure than on the simple self-report measure, as is clear in Table 1.4c. In all the estimated specifications, the impact of migrant networks is positive, significant and higher than when simply considering self-reported voting. We interpret this evidence as providing further support for an important informational role of migration through geographical networks. Indeed, migrants seem to transmit information about

the relevance of political participation and about the political process itself. This is consistent with migrant-connected respondents being significantly more likely to self-report voting, and also to show the correct inked finger – a display of better knowledge about the electoral process, in addition to the simple recognition of the importance of voting. This result is consistent with the evidence outlined in Section 1.4 that access to news about politics in Mozambique is limited. As a result, households contacting with migrants abroad might benefit from the additional information that migrants obtained abroad and transmit back to their networks in the home country.

Another potential theoretical mechanism that can explain the impact of migrant networks on political participation is a change in the social norms of migrant villages, which generates intrinsic motivation for political participation. If this is the case, we would expect that experimental subjects connected to migrants respond more strongly when given the possibility to express their policy priorities – even if this is not part of the standard political process of the country. As international migration from Mozambique is mainly to South Africa that has much higher political participation rates, the transmission of increased political participation social norms is likely to be a valid mechanism in our setting. Indeed, our behavioral measure of political engagement confirms this hypothesis, although only after accounting for the potential simultaneity bias of migration networks and political behavior. Although the effect of geographical migrant networks is not statistically significant and almost zero when using a LPM as shown in column (1) of Table 1.4d, the 2SLS estimates in columns (2) - (4) of Table 1.4d show that one more migrant household in a village increases political participation of its residents by between 3.9 pp and 5.3 pp.

Table 1.4: Effects of International Migrant Geographical Network

(a) Actual Voting dependent variable.

	LPM	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)
International Migrants within Locality	0.023***	0.034**	0.033**	0.041**
	(0.007)	(0.014)	(0.016)	(0.017)
Own Migration Experience Control	yes	no	yes	yes
Individual Controls Included	yes	yes	yes	yes
Instrumental Variables	-	A	A	A + B
Kleibergen-Paap Wald F-Statistic	-	34.66	33.29	19.10
Hansen J-Test p-value	-	-	-	0.30
Observations	1102	1102	1102	1071

(b) Self-Reported Voting dependent variable.

	LPM	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)
International Migrants within Locality	0.004	0.016**	0.019**	0.016**
	(0.004)	(0.007)	(0.008)	(0.008)
Own Migration Experience Control	yes	no	yes	yes
Individual Controls Included	yes	yes	yes	yes
Instrumental Variables	-	A	A	A + B
Kleibergen-Paap Wald F-Statistic	-	34.66	33.29	19.10
Hansen J-Test p-value	-	-	-	0.33
Observations	1102	1102	1102	1071

(c) Learning-Corrected Self-Reported Voting dependent variable.

	LPM	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)
International Migrants within Locality	0.011***	0.028**	0.029**	0.029**
	(0.004)	(0.011)	(0.013)	(0.012)
Own Migration Experience Control	yes	no	yes	yes
Individual Controls Included	yes	yes	yes	yes
Instrumental Variables	-	A	A	A + B
Kleibergen-Paap Wald F-Statistic	-	34.66	33.29	19.10
Hansen J-Test p-value	-	-	-	0.96
Observations	1102	1102	1102	1071

Table 1.4: Effects of International Migrant Geographical Network.

(d) Behavioral Measure dependent variable.

	LPM	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)
International Migrants within Locality	-0.001	0.039**	0.046**	0.053***
	(0.008)	(0.017)	(0.019)	(0.019)
Own Migration Experience Control	yes	no	yes	yes
Individual Controls Included	yes	yes	yes	yes
Instrumental Variables	-	A	A	A + B
Kleibergen-Paap Wald F-Statistic	-	33.59	31.44	18.54
Hansen J-Test p-value	-	-	-	0.55
Observations	1128	1128	1128	1097

Table Notes: Individual Controls include gender of household head (male), age of household head (years), highest education level completed by the household head, and access to radio, television and computers. We further control for province effects in all specifications. Instrumental Variable A in columns (2) – (3) is a measure of the cumulative exposure to droughts experienced by each household when the household head was between 20 and 30 years old. Column (4) reports results with two instrumental variables where the additional IV B is a measure of the cumulative exposure to droughts experienced by each household when the household head was between 30 and 40 years old. Please see text for details on the construction of the IV. Kleibergen-Paap Wald F-statistics and p-values of Hansen J-Test are reported where applicable. Standard errors in parentheses, clustered at the enumeration area level. * p<0.10, ** p<0.05, *** p<0.01.

The difference between the LPM and 2SLS estimates across all outcomes we use suggests negative self-selection of migrants in terms of their political attitudes. This is consistent with the results of Batista et al. (2017), which uses a number of sources of variation and estimation strategies to conclude that emigration from Mozambique seems to be driven by unobservable negative self-selection – in terms of entrepreneurship in that case. This is consistent with a context in which there is a long history of migration to South-African mines and farms, where large networks of migrants substantially decrease any pecuniary and non-pecuniary costs of migration.

Overall our estimates suggest that geographical migrant networks are likely to improve political participation in migrant countries of origin through both information and intrinsic motivation mechanisms.[40] One important question that remains is to

[40]A relevant caveat to our empirical results is that we cannot distinguish the changes in political

understand what type of personal relationship with the migrant drives the impact of migrant geographical networks on political participation. For this purpose, we look at two types of networks within the geographical network: chatting and kinship networks.

1.7.2 Chatting Networks

We are interested in understanding how friendship – and in particular friendship with international migrant households – may affect political behavior. Friendship is a complex concept and implies subjective definitions especially in a country context such as Mozambique, where there exist many local languages whose usage in rural areas dominates the official language Portuguese. We proxy friendship by asking respondents with whom in the sampled village households they regularly chat, as described in detail in the previous section.

Chatting with migrant households seems to significantly increase actual voting behavior. Columns (1) to (4) of Table 1.5a show a marginally significant positive impact of migrant chatting networks on political participation. These effects become larger and more precisely estimated when accounting for self-selection into migrant chatting networks. As shown in columns (5) to (6) of Table 1.5a, chatting with one more migrant household has a positive and significant effect on actual voting behavior of between 6.5 pp and 7.6 pp when controlling for migrant self-selection and endogenous friendship selection. Table 1.5b shows the effect of regularly speaking with migrant households on an individual's likelihood to self-report having voted. As before, we obtain highly significant positive effects of up to 3.9 pp in the probability to self-report voting per additional migrant household in the chatting network. This estimate is robust to controlling for self-selection of migrants and endogenous network formation.

participation arising because of international migration per se, from potential income effects generated by migrant international remittances because the value of these remittances received is not included in our dataset.

Table 1.5: Effects of International Migrant Chatting Networks.

(a) Actual Voting dependent variable.

	LPM	2SLS	2SLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
International Migrant Chatting Network	0.019*	0.026*	0.024	0.027*	0.065***	0.076***
	(0.010)	(0.014)	(0.014)	(0.015)	(0.023)	(0.024)
Own Migration Experience Control	yes	no	yes	yes	yes	yes
Individual Controls	yes	yes	yes	yes	yes	yes
Instrumental Variables	-	A	A	A + B	A + C	A + B + C
Kleibergen-Paap Wald F-Statistic	-	45.92	45.70	43.80	18.05	28.71
Hansen J-Test p-value	-	-	-	0.81	-	0.42
Observations	1102	1102	1102	1071	1102	1071

(b) Self-Reported Voting dependent variable.

	LPM	2SLS	2SLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
International Migrant Chatting Network	0.018***	0.017**	0.019**	0.024***	0.039***	0.038***
	(0.006)	(0.008)	(0.008)	(0.007)	(0.010)	(0.010)
Own Migration Experience Control	yes	no	yes	yes	yes	yes
Individual Controls	yes	yes	yes	yes	yes	yes
Instrumental Variables	-	A	A	A + B	A + C	A + B + C
Kleibergen-Paap Wald F-Statistic	-	45.92	45.70	43.80	18.05	28.71
Hansen J-Test p-value	-	-	-	0.45	-	0.81
Observations	1102	1102	1102	1071	1102	1071

Table 1.5: Effects of International Migrant Chatting Networks.

(c) Learning-Corrected Self-Reported Voting dependent variable.

	LPM	2SLS	2SLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
International Migrant Chatting Network	0.030***	0.030***	0.030**	0.036***	0.059***	0.065***
	(0.007)	(0.011)	(0.013)	(0.012)	(0.021)	(0.018)
Own Migration Experience Control	yes	no	yes	yes	yes	yes
Individual Controls	yes	yes	yes	yes	yes	yes
Instrumental Variables	-	A	A	A + B	A + C	A + B + C
Kleibergen-Paap Wald F-Statistic	-	45.92	45.70	43.80	18.05	28.71
Hansen J-Test p-value	-	-	-	0.42	-	0.45
Observations	1102	1102	1102	1071	1102	1071

(d) Behavioral Measure dependent variable.

	LPM	2SLS	2SLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
International Migrant Chatting Network	0.018*	0.026*	0.029**	0.034***	0.059**	0.054***
	(0.010)	(0.014)	(0.015)	(0.013)	(0.024)	(0.021)
Own Migration Experience Control	yes	no	yes	yes	yes	yes
Individual Controls	yes	yes	yes	yes	yes	yes
Instrumental Variables	-	A	A	A + B	A + C	A + B + C
Kleibergen-Paap Wald F-Statistic	-	45.13	44.32	46.11	14.15	35.319
Hansen J-Test p-value	-	-	-	0.92	-	0.7792
Observations	1128	1128	1128	1097	1128	1097

Table Notes: Individual Controls include gender of household head (male), age of household head (years), highest education level completed by the household head, and access to radio, television and computers. We further control for province effects in all specifications. Instrumental Variable A in columns (2) - (5) is a measure of the cumulative exposure to droughts experienced by each household when the household head was between 20 and 30 years old. The instrumental variable C in columns (5) and (6) additionally accounts for endogenous network creation by using second-degree network links. Columns (4) and (6) reports results with two instrumental variables where the additional IV B is a measure of the cumulative exposure to droughts experienced by each household when the household head was between 30 and 40 years old. Please see text for details on the construction of the IVs. Kleibergen-Paap Wald F-statistics and p-values of Hansen J-Test are reported where applicable. Standard errors in parentheses, clustered at the enumeration area level. * p<0.10, ** p<0.05, *** p<0.01.

This effect is much higher when examining the impact on the learning-corrected self-reported voting measure, as shown in Table 1.5c. We interpret this evidence as supportive of an important role of chatting with migrants for the transmission of information on the importance of political participation, and on the political process itself.

The estimation results displayed in Table 1.5d show that the effect of migrant chatting networks is also positive and significant on our behavioral measure of political participation after controlling for simultaneity biases. As reported in column (3) of Table 1.5d, the positive effect of talking to one more migrant household increases the likelihood of sending a text message by 2.6 pp when accounting for migrant self-selection, and by 2.9 pp when additionally controlling for the respondent's own migration history. Accounting for endogenous network formation, the likelihood of sending a text message increases to between 5.4 pp and 5.9 pp as shown in columns (5) and (6). This evidence supports that chatting with migrant households can act as an important driver of prescribed social norms on political participation.

1.7.3 Kinship Networks

We now turn to examining the role of kinship relations with migrant households in shaping political behavior of the left behind. A kinship relation between two households exists, if a respondent indicated to have family ties to the household head or any other member of another household in our sample within the respective EA. Since households were randomly sampled within each EA, we can expect the observed network links with migrants to be representative in magnitude to the overall kinship connectedness with migrant households of the respondent.

Table 1.6: Effects of International Migrant Kinship Network.

(a) Actual Voting dependent variable.

	LPM (1)	2SLS (2)	2SLS (3)	2SLS (4)	2SLS (5)	2SLS (6)
International Migrant Kinship Network	0.019	0.016	0.011	0.032	0.041	0.057
	(0.013)	(0.018)	(0.019)	(0.020)	(0.033)	(0.038)
Own Migration Experience Control	yes	no	yes	yes	yes	yes
Individual Controls	yes	yes	yes	yes	yes	yes
Instrumental Variables	-	A	A	A + B	A + C	A + B + C
Kleibergen-Paap Wald F-Statistic	-	46.89	47.43	30.26	24.81	22.30
Hansen J-Test p-value	-	-	-	0.19	-	0.53
Observations	1102	1102	1102	1071	1102	1071

(b) Self-Reported Voting dependent variable.

	LPM (1)	2SLS (2)	2SLS (3)	2SLS (4)	2SLS (5)	2SLS (6)
International Migrant Kinship Network	0.017***	0.030***	0.033***	0.028***	0.054***	0.038***
	(0.006)	(0.008)	(0.008)	(0.007)	(0.011)	(0.011)
Own Migration Experience Control	yes	no	yes	yes	yes	yes
Individual Controls	yes	yes	yes	yes	yes	yes
Instrumental Variables	-	A	A	A + B	A + C	A + B + C
Kleibergen-Paap Wald F-Statistic	-	46.89	47.43	30.26	24.806	22.30
Hansen J-Test p-value	-	-	-	0.26	-	0.02
Observations	1102	1102	1102	1071	1102	1071

Table 1.6: Effects of International Migrant Kinship Network.

(c) Learning-Corrected Self-Reported Voting dependent variable.

	LPM	2SLS	2SLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
International Migrant Kinship Network	0.028***	0.031	0.030	0.038**	0.076***	0.066***
	(0.009)	(0.020)	(0.021)	(0.016)	(0.015)	(0.014)
Own Migration Experience Control	yes	no	yes	yes	yes	yes
Individual Controls	yes	yes	yes	yes	yes	yes
Instrumental Variables	-	A	A	A + B	A + C	A + B + C
Kleibergen-Paap Wald F-Statistic	-	46.89	47.43	30.26	24.81	22.30
Hansen J-Test p-value	-	-	-	0.54	-	0.29
Observations	1102	1102	1102	1071	1102	1071

(d) Behavioral Measure dependent variable.

	LPM	2SLS	2SLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
International Migrant Kinship Network	0.018	0.028	0.031	0.027	0.033	0.033
	(0.014)	(0.022)	(0.023)	(0.019)	(0.031)	(0.025)
Own Migration Experience Control	yes	no	yes	yes	yes	yes
Individual Controls	yes	yes	yes	yes	yes	yes
Instrumental Variables	-	A	A	A + B	A + C	A + B + C
Kleibergen-Paap Wald F-Statistic	-	47.07	46.80	31.75	22.99	22.96
Hansen J-Test p-value	-	-	-	0.50	-	0.86
Observations	1128	1128	1128	1097	1128	1097

Table Notes: Individual Controls include gender of household head (male), age of household head (years), highest education level completed by the household head, and access to radio, television and computers. We further control for province effects in all specifications. Instrumental Variable in columns (2), (3), and (5) is a measure of the cumulative exposure to droughts experienced by each household when the household head was between 20 and 30 years old. The instrumental variable in column (5) additionally accounts for endogenous network creation by using second-degree network links. Column (4) and (6) reports results with two instrumental variables where the additional IV is a measure of the cumulative exposure to droughts experienced by each household when the household head was between 30 and 40 years old. Instrumental variables in column (6) also control for endogenous network creation. Please see text for details on the construction of the IVs. Kleibergen-Paap Wald F-statistics and p-values of Hansen J-Test are reported where applicable. Standard errors in parentheses, clustered at the enumeration area level. * p<0.10, ** p<0.05, *** p<0.01.

Our results in columns (1) to (4) of Table 1.6a suggest that kinship relations with migrant households are positively correlated with actual voter turnout. Our estimates point to a 1.1pp to 3.2pp effect, which cannot however be precisely estimated. Additionally controlling for the endogenous formation of network links in column (5) of Table 1.6a increases the effect of migrant networks to an imprecisely estimated effect between 4.1 pp and 5.7 pp. This seems to indicate that family ties to migrants are not the main driver of the strong impact of geographical networks on actual voting behavior we reported in Table 1.4a.

In terms of self-reported voting, kinship ties to migrants significantly increase self-reported voting behavior up to 5.4 pp even after controlling for unobservable self-selection in migration decisions and endogenous network formation, as is shown in columns (1) - (6) of Table 1.6b. This result does not seem very reliable however since the exogeneity of the secondary network instrument we use to account for endogenous network formation is rejected by the over-identifying-restriction test for the self-reported voting outcome. While exogeneity of this instrumental variable is rejected, the estimation bias seems to underestimate the true effects of migrant networks, so that we expect the true effect to be larger than our (likely biased) estimates.

This effect is stronger when correcting the self-reports for knowledge about the voting process, particularly when accounting for self-selection into kinship networks: as is displayed in columns (1) – (6) of Table 1.6c, the impact of migrant kinship networks varies between 2.8 pp using the LPM model, and 7.6 pp using the 2SLS estimates. This evidence supports that having a migrant in the family can importantly contribute to better information on both the importance of political participation, and the political process itself – even if it is not enough to bring these family members of migrants to actually vote.

In contrast to the results on self-reported voting, our behavioral measure of polit-

ical participation is not significantly affected by kinship ties with migrant households. Neither the LPM, nor the 2SLS specifications yield any statistically significant estimation results, although the point estimates are consistently positive. These results suggest that being family related to migrants may not be enough to cause significant changes in prescribed social norms, and hence on the intrinsic motivation for political participation.

1.7.4 Discussion of Results

The impacts we estimate are quantitatively substantial, particularly given the high prevalence of migration in Mozambique, as illustrated in Table 1.1. Indeed, taking 4.3 as the mean value of household migrants per village with migrants, living in a village with migrant households is responsible for an increase of 14.2 pp in the probability of actual voting in that village, and an increase of 19.8 pp in the probability of sending a text message with policy priorities to the president.

Again, in our sample the mean effect of regularly chatting with migrant households is an increase of 12.3 pp in the probability of actual voting, and an increase of 11.1 pp in the probability of sending the policy-demand text message. These effects are sizable, particularly in the context of an election that had a national turnout rate of 44% - implying that the effect of migrant networks would be between 28% and 32% of the overall turnout.

1.8 Concluding Remarks

There is a large body of literature in the social sciences examining the relationship between international emigration and politics in the home country of migrants. Our paper contributes to this by examining the diffusion of political norms and informa-

tion about electoral processes through different types of migrant networks – which we measure using detailed data on geographical networks, kinship networks and chatting networks.

Two mechanisms are likely to promote political participation through migrant networks: enlarging the information set of individuals in the home country, and changing their social norms governing political participation. Both of these mechanisms are likely to promote political participation provided the migrants transmit information and norms that are superior to those prevalent in their country of origin.

The choice of studying migration as a determinant of political participation in the context of the 2009 national elections of Mozambique is particularly relevant in this context. Mozambique is a low-income country with substantial South-South emigration, almost exclusively to other neighboring sub-Saharan African countries. This is a setting where both migrant countries of origin and destination are flawed democracies, and where the empirical question of whether migrants can transfer improved political norms back home is not trivial – while being of great relevance in a world where most migration flows happen in similar contexts.

Our empirical results suggest that political attitudes and behavior can be learned and valued more highly at home by individuals who are in contact with emigrants. We furthermore find that increased political participation seems to be mainly driven through contact with migrants through regular chatting, rather than through family links to migrants. The evidence we examine is consistent with both information transmission and changed social norms for political participation via chatting with migrants. Family links seem to convey some information about the political process, but do not seem to significantly affect broad political engagement.

Related to our findings, existing evidence establishes that there are several mechanisms via which migration may affect the strengthening of democratic institutions.

Adida and Girod (2010), Pérez-Armendáriz and Crow (2010) and Pfutze (2012), for example, emphasize the role of emigration in simultaneously improving governance and promoting political participation. Our results corroborate their findings.

While we confirm existing results on the positive effects of international emigration on political participation, the lack of heterogeneity in destination of Mozambican emigrants does not allow us to test for differential effects of migration to destinations with higher and lower democratic scores according to international rankings such as V-DEM or EIU, unlike Batista and Vicente (2011) for the case of Cape Verde. It will be important to produce additional research on this type of heterogeneous effects in countries with South-South migration flows to a variety of destinations.

In this paper, we use different measures of political participation - namely a proxy for actual electoral voting, and a behavioral measure based on a text message experiment that asked respondents to send a message with policy priorities to the president. The use of these very different measures provides credibility to our findings on the impact of emigration on political participation. Our findings are however more limited in terms of empirically distinguishing the mechanisms through which migrant networks affect political participation. Our proxies for improved electoral information and for changed political participation social norms/increased intrinsic motivation for political participation can only provide suggestive evidence of how different migrant networks transmit political participation. Further research using richer measures of electoral information and political norms would be of great academic interest and policy relevance.

Overall, our work suggests that migration policies whereby the best governed migration host countries open their doors to migrants from countries with poor accountability records might be an effective way to promote political participation in the migrant countries of origin. According to our findings, these host countries need not be the most developed and with highest democratic rankings. Enacting South-South 'brain

circulation' policies such as scholarship schemes not only in developed countries, but also in destination countries where governance is flawed and democracy is far from working perfectly, might be an effective tool to promote the strengthening of political institutions and ultimately economic development.

Chapter 2

Keeping Up with the Joneses: Ranking Effects on Effort, Cooperation, and Firm Performance

2.1 Introduction

Around 75% of the total employed in Sub-Saharan Africa are self-employed[1] - most of them operating petty businesses with low profit margins and low survival rates. Households depend on incomes generated by these firms as there are few salaried jobs available. However, few of these businesses evolve over time or contribute to growth in the formal economy. Understanding what holds these businesses back is a key challenge in development economics. The most common policies supporting micro-firm growth focus on two input types. They provide access to financial capital[2] or increase human

[1]World Development Indicators. Source: International Labour Organization, ILOSTAT database. Data retrieved in September 2018. The indicator of status of employed in this dataset distinguishes between wage and salaried workers, and self-employed workers. The share of self-employed workers in Mozambique is even higher with 84% despite high GDP growth rates in the past.

[2]Financial capital is provided in different forms such as access to micro-credit, (un-)conditional cash grants, and access to savings means. Blattman et al. (2014) and McKenzie (2017) find large

capital through business skills training. This paper investigates an alternative approach: Increasing firm performance by changing a firm owner's beliefs about the returns to effort and cooperation with their business network.

The effects of changing beliefs, and as a consequence effort and cooperative behavior, on firm performance are potentially large for two reasons. First, effort might be a crucial input factor, so far understudied in the micro-firm literature.[3] In firms with only one worker (the owner), effort in terms of work hours is the extensive margin of labor input. Micro-firm owners may be more elastic in the effort they supply rather than employment. Second, business cooperation between firm owners and their business network can be beneficial through two channels.[4] Increasing cooperation with their business network can improve business practices as individuals learn from their peers, and cooperation can increase firm outcomes by fostering stronger business partnerships. Stronger firm relations can, for example, reduce transportation cost of products by pooling orders from wholesale merchants.

To investigate whether perceptions of relative performance standing affect effort and cooperation choices, and how they relate to firm performance, I conducted a randomized control trial in the Maputo metropolitan region in Mozambique. I implemented two randomized interventions. The main intervention provides firm owners with information about their relative sales ranking. Each individual in the treatment group received a ranked list of ten anonymous vendors, including themselves, that was representative

increases in earnings from relaxing credit constraints by providing conditional cash grants. Batista et al. (2017) similarly find positive effects on firm revenue from providing entrepreneurs with access to savings means.

[3]There is a large literature in personnel economics identifying the relationship between effort and performance, how to incentivize effort of employees through contract structures, and the effect of such incentives on employee wellbeing.

[4]Cooperative behavior is defined in its broadest sense including cooperative activities such as colluding on prices, sharing business advice, and informal finance such as loans. Higher cooperation increases a firm owner's social capital that has positive returns as shown in Blattman et al. (2016) or Fafchamps et al. (2018). The effects of cooperation might be negative as a high degree of income sharing norms might be disadvantageous (Jakiela and Ozier (2016)).

of the full distribution of vendors in their sector. It displayed each vendor's weekly sales at baseline and their relative position. The subject's own name and position were highlighted. Half of the treatment group, additionally to observing their rank, received information about peer characteristics. This information included peer gender and age. This second treatment allows me to isolate the impact of peer characteristics on behavioral changes.

A firm owner's effort choice is sub-optimal with respect to profit maximization if the perceived returns to effort are lower than the real returns to effort. The ranking contains information relevant to effort optimization if it provides firm owners with new information about the highest levels of revenues attainable in their sectors. If individuals calculate the return to effort based on their expectation of future revenues, information about peer revenues allows them to update their beliefs and optimal effort decisions. As peer revenues are not observable and there is little information about possible revenue levels, the ranking can be expected to contain a large amount of information for low-performing individuals. Individuals in the bottom of the distribution receive a negative signal provoking them to change their effort level as they change their beliefs about the returns to effort but also if they care about their relative standing itself.[5] Similarly, an individual might process the information the ranking revealed to reevaluate her business network cooperation. After receiving a negative signal and observing that a higher level of revenues is very likely to be possible, an individual might strategically increase cooperation to benefit from a better firm network.

[5]The change in effort might be positive if firm owners become encouraged to work more to increase their sales. Firm owners might also become discouraged and work less or even close their stores. The theoretical predictions in the tournament literature suggest that both, subjects at the very bottom and at the top, decrease their effort after observing their relative standing. The effect of performance feedback is expected to be positive in non-competitive environments for the whole distribution of subjects. In this setting it is unclear whether the business environments resembles more closely a tournament setting or a non-competitive setting as there are many clients and firms. The empirical results of this paper suggest that market clusters mainly composed of micro-firms are less competitive than they appear.

There are three main reasons why effects from the additional treatment might differ and why observing the top seller's gender might matter. First, experimental evidence finds women to be less likely to self-select into competition which could imply larger pro-social preferences.[6] Individuals observing a female top seller might change their pro-social preferences reflecting her behavior. Second, a female top seller might provide an additional signal about the attainability of higher sales. Low-performing women see a role model that encourages them to aspire more. Third, the social status of women in many developing countries is low. Women are considered to be less capable and have less agency. Individuals observing a female at the top might understand her success as more feasible to attain for themselves.

I use three main data sources to measure impact: surveys, lab-in-the-field data, and actual sales data. The study follows 315 micro-firm owners[7] over one and a half years. The survey team visited each firm four times, the first time to collect baseline data to be used in elaborating the performance ranking and to check for sample balance. Then, two months after the baseline for the intervention and to present the rankings. Finally, firms were visited four months and one year after the intervention to measure the impact of the ranking. The survey data contains information about firm owners' socio-economic background, effort allocation, business practices, and firm outcomes. At baseline and four months after the intervention, firm owners also played a set of incentivized games eliciting risk and pro-social preferences. Cooperation is proxied by pro-social preferences, including how much an individual cares about, shares income, and helps members of her social network. I measure pro-social preferences by contributions in an incentivized dictator game. I asked individuals to indicate the person

[6]See Niederle and Vesterlund (2007).

[7]Firms were already established and are representative of the population of micro-firm owners in Maputo. Per week, firms earned less than US$70 on average in profits which represents less than US$2 per day in per capita income. Hence, improving the productivity of these firms is not only a priority of the Mozambican government to stimulate the private sector but also to increase incomes of the urban population.

they talk with the most about their own or the other person's business. Individuals then decided how much of their endowment to share with this person. At the end-line survey, I additionally collected monitored sales data of each firm over an entire day to check for the reliability of survey-reported sales data.

I estimate differential average treatment effects for high- and low-performing firms. Low-performing firms are defined as those whose weekly sales at baseline are below the 50th percentile. High-performers include all other firms - those with an average sales performance and above. This definition allows me to distinguish treatment effects between a clearly negative and a positive ranking signal.

There are three main findings from my experiment. First, being exposed to the ranking increases effort of low-performing individuals. I measure effort as the amount of hours an individual works at her firm. Treated low-performing firm owners work more hours than low-performing firm owners in the control group. They work as many hours after the intervention as high-performers in the control group. The effect remains statistically significant one year after the intervention. For high-performing individuals the impact of learning about their ranking is negative. They report a small but significant decrease in effort compared to high-performers in the control group.

Second, the ranking changes cooperation of low-performing individuals. Treated low-performers share more than low-performers in the control group. I interpret this effect as a positive impact of exposure to the ranking on firm cooperation. The treatment effect on high-performers is small and not statistically different from zero.

Third, and most importantly, changes in effort and cooperation are associated with changes in firm performance among low-performing firm owners. I report treatment effects on sales and profits over the last two days, and on self-reported profits in the previous month. At baseline low-performing individuals increase both their sales and profits over both time periods. Sales of low-performers more than double compared to

low-performers in the control group. Self-reported profits of low-performers are 54% higher than in the control group. The increase in firm performance closes the income gap between treated individuals in the bottom of the distribution at baseline, and high-performers in the control group by 48%. High-performing individuals, however, are not affected by the ranking. Their firm outcomes remain unchanged.

A mediation analysis suggests that both effort and cooperation are relevant inputs that are strongly associated with the positive impact of the treatment on low-performers. However, it is unclear whether the gender composition of social networks matters. I estimate the additional effect of gender information on the ranking effect. Specifically, I analyze the impact of providing information that the top seller in a group is a woman. My results show that the impact of the ranking is strongest for those that also receive information about peer characteristics. The additional effect of observing a female top seller is not significantly different from zero in terms of effort and cooperation choices but it is large and statistically significant for firm outcomes. Low-performers at baseline that observed a female top seller increase their reported sales and last month's profits relative to low-performers in the control group. This effect is 90% higher than the effect from receiving the ranking alone. This suggests that observing peer characteristics contains relevant information changing firm outcomes through additional channels other than effort and cooperation.

The biggest identification threat to this study is the reliability of self-reported sales and profit data. Motivational lying has minimal costs as we cannot confirm self-reports about past periods. To diminish the risk of misreports a team different from the team of enumerators conducting the surveys distributed the ranking. We provided information about the individual's relative position only once such that misreporting will not increase positions in future rankings. Nevertheless, misreporting at one of the follow-up visits could potentially be worthwhile if the individual cares about the researcher's opin-

ion. To check the validity of effects on self-reported sales, we tracked sales of individuals over one entire business day. Low-performing individuals increase their observed sales one year after the intervention. The impact of the ranking on these monitored sales is robust regarding both effect size and precision of the estimates. Low-performers in the treatment group more than doubled sales compared to low-performers in the control group.

This study provides experimental evidence on the effect of changes in micro-firm owners' beliefs about returns to work effort and cooperation on actual behavior, and of ranking information on firm performance. It addresses several gaps in the literature on the constraints to micro-firm development. One gap in this literature is the absence of evidence on individual effort as a production input in micro-firms. Most evaluations focus on business skills training or access to financial capital. The returns to traditional skills training are small (McKenzie and Woodruff (2013)). Although firms adopt improved business practices in the short run, they are often dropped over time. Impacts on profits or sales, as a consequence, are small and can often not be precisely estimated. Trainings focusing on simple financial heuristics however do increase firm outcomes (Drexler et al. (2014), Batista et al. (2017)). Further, access to formal savings accounts increases productive investments (Dupas and Robinson (2013)), and business grants transform male-run enterprises when combined with skills training (Berge et al. (2017); de Mel et al. (2014)).[8] But it is unclear whether micro-firm owners optimize over their effort allocation given their skills and capital.

This paper also relates to a growing literature that focuses on the impact of mentoring and consulting on firm performance. Management consulting increases total factor productivity of small and medium enterprises (SMEs) in the long run in Mexico (Bruhn et al. (2018)). Lafortune et al. (2018) find that a consulting intervention in Chile in-

[8]Further examples on the impact of financial capital on firm outcomes include Blattman et al. (2014); Dupas and Robinson (2013); Fafchamps et al. (2014).

creases household income. The latter is less cost-effective though than a role model intervention. Mentorship by experienced entrepreneurs increases profits of micro-firms in Kenya by 20 percent (Brooks et al. (2018)). Recent experimental studies that estimate the impact of business networks find comparable results. Monthly meetings of business associations increase revenue - especially for those firms with better peers (Cai and Szeidl (2018)). The effects are driven by increased knowledge sharing and newly-created business partnerships. This paper contributes to this literature by estimating the returns to pro-social behavior. I provide evidence that pro-social behavior positively correlates with firm performance at baseline. My experimental results show that pro-social behavior is changeable through a small nudge and that this increase in cooperation also correlates with changes in firm performance.

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 remainder of this paper is organized as follows. Section 2.2 presents further

information on the experimental design, and a conceptual framework displaying through which channels the treatment might affect firm outcomes. The data and sampling framework are described in detail in Section 2.3. The estimation strategy is presented in Section 2.4. Section 2.5 discusses the experimental results, and Section 2.6 presents robustness checks on self-reported sales data. Section 2.7 concludes.

2.2 Background

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. The majority of individuals in the sample had inaccurate beliefs about their relative position. Only 24% of low-performers accurately state that they are doing worse than the average firm in the market they operate in. Around 8% of low-performers actually believe they are doing better than the average. High-performers 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.

Further, the gender of peers should not matter if it does not contain additional information about the attainability of higher revenues and better business practices. As background, I describe the experimental design and then characterize the relationship between ranking information and firm performance in a simple conceptual framework. Using insights from baseline data I identify the likely effect on firm outcomes of changes in effort and cooperation.

2.2.1 Experimental Design

This study was conducted with established micro-firms in urban market clusters of the greater Maputo region. Between August and September 2016, we collected baseline data from 624 firm owners. For those individuals that reported revenues over the week prior to the baseline interview I constructed individual rankings. The rankings were based on each individual's sector of firm activity. I then determined the decile in which each individual falls at baseline. A few firms were dropped from the sample as there were less than ten firms vending similar products in our sample. Figure 2.2.1 displays an example of an individual's ranking.

Figure 2.2.1: Example of Ranking Sheet.

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 first 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.

A ranking consists of ten firms. It includes the individual's firm as well as one firm per other decile. The other firms shown are those with the median sales in their respective decile. This means that all individuals in the same sector observe the same peers. Choosing the median firm controls for outliers. Firm owners are anonymous and the individual's own position is clearly highlighted with her name and a colored bar. Additional to their relative position, individuals observe other's revenues as well as their own over the same period. 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. Half of the treatment group could additionally observe peer characteristics (see Figure 2.2.2 for an example).

For each firm, treated individuals 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. Treatment assignment was stratified by gender. Randomization was done on the individual level.

Figure 2.2.2: Example of Ranking Sheet 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.

2.2.2 Should a Ranking Change Entrepreneurial Behavior and Firm Performance?

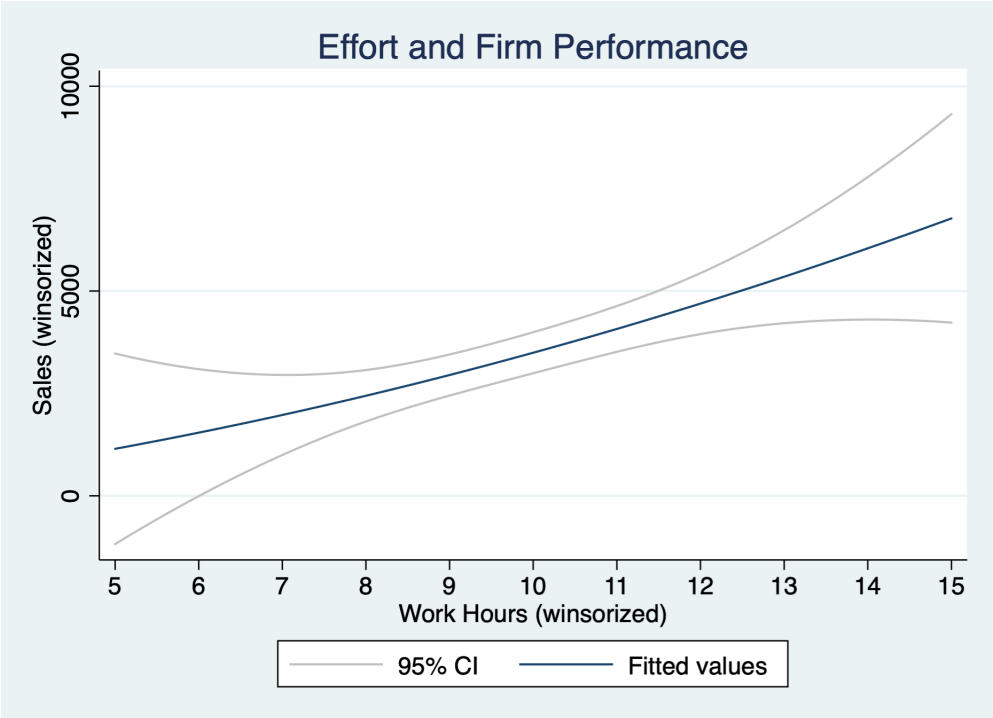
The intervention was designed to affect firm performance through two main intermediary outcomes: effort and cooperation. I propose an individual decision model, where subscript i represents a micro-firm. A micro-firm has only one worker - the firm owner. The production function thus becomes an individual problem. An entrepreneur's income depends on three input factors. Her individual skills, S_i, which may include school

education as well as practical knowledge on how to run a business. It depends on the financial capital the individual invests, k_i, that includes investments in the firm structure as well as inventory. Third, income depends on the effort level, e_i. This framework is analog to a production function where output is a function of capital and labor inputs, and a firm's technology. Assume that the income of the individual, y_i, is a function of these inputs of the following form:

$$y_i = S_i f(k_i)\, g(e_i) \tag{2.2.1}$$

Both capital and effort enter the production function indirectly. Figure 2.2.3 displays the quadratic relationship between hours worked and firm performance at baseline.

Figure 2.2.3: Relationship Between Effort and Firm Performance.



Notes: This figure plots the quadratic relationship between effort and firm revenue at baseline for around 600 micro-firms. Both hours worked and revenue data are winsorized at the 1st and 99th percentile. Sales are reported in the Mozambican local currency, Meticais.

Regression estimates for the baseline sample of approximately 600 micro-firms suggest that one additional hour worked increases sales by almost 30%.

The key part of this model is an individual's effort choice. At baseline, an individual's effort choice depends on the individual's expected return to effort. Assume that there are no outside options (off-firm work) and that an individual chooses between leisure time and hours worked in the firm. The household income and the firm production function become thus interdependent.[9] The optimization problem of the individual is determined as a classical division problem between labor and leisure subject to a budget constraint. The wage rate for in-firm work is a function of the individual's skills, and the output and input prices of the firm. Given that market fluctuations and general economic conditions affect both input and output prices, the wage rate for in-firm work is uncertain. We can summarize the relationship between effort and firm income at baseline as follows:

$$e_i = g_1 \left(E_{\Omega_i} \left[y_i \right] \right) \tag{2.2.2}$$

Equation 2.2.2 defines the effort choice of individual, i, as a function of her expected income. The parameter Ω_i determines the information set about future incomes available to individual i. The information set is heterogenous across individuals and might depend on factors such as education, self-confidence, or risk aversion.

There are two main reasons why an individual's perception about others' incomes should affect her effort. First, information about the success of peers might change expectations about future income. Many entrepreneurs in developing countries become self-employed as there are little salaried employment opportunities. As a consequence, being an entrepreneur is often associated with low incomes and has a negative reputation. Receiving information about the income levels of colleagues allows individual, i,

[9]See Lopez (1984) for a full model exemplary of the agricultural sector in Canada.

to update her beliefs about attainable income levels. This in turn affects effort. This effect should be strongest for low-performing individuals as there is little additional information for individuals at the top. This is similar to Cullen and Perez-Truglia (2018) that estimate an employee's effort level as a function of own salary and perception about peer salaries. When provided with information about the average salary of their boss, workers increase effort to increase the likelihood of future higher salaries. The ranking affects the information set, Ω_i, as it provides additional information on possible revenue levels.

Furthermore, there is a long-standing literature in economics and social psychology on the effects of peer incomes on happiness and utility. The Easterlin paradox (Easterlin (1974), Easterlin (1995)) illustrates that average happiness has remained constant over time while per capita GDP increased. The general consensus attributes the effect to the importance of relative income considerations in the utility function (Clark et al. (2008)). Social comparison models[10] predict that perceptions of peer income shape intrinsic motivation. This means that individuals derive utility from improving their income level relative to their peers. As income depends on effort, information about peer income affects effort levels. We can model the individual's effort choice with information about peer income as

$$e_i = g_1\left(E_{\Omega_i}\left[y_i\right]\right) + g_2\left(y_{-ig}\right) \tag{2.2.3}$$

where y_{-ig} denotes incomes of peer $-i$ in sector g. For individuals that care about their relative position by itself, providing information about their ranking changes effort. The effect is different for low-performing and high-performing individuals. Furthermore,

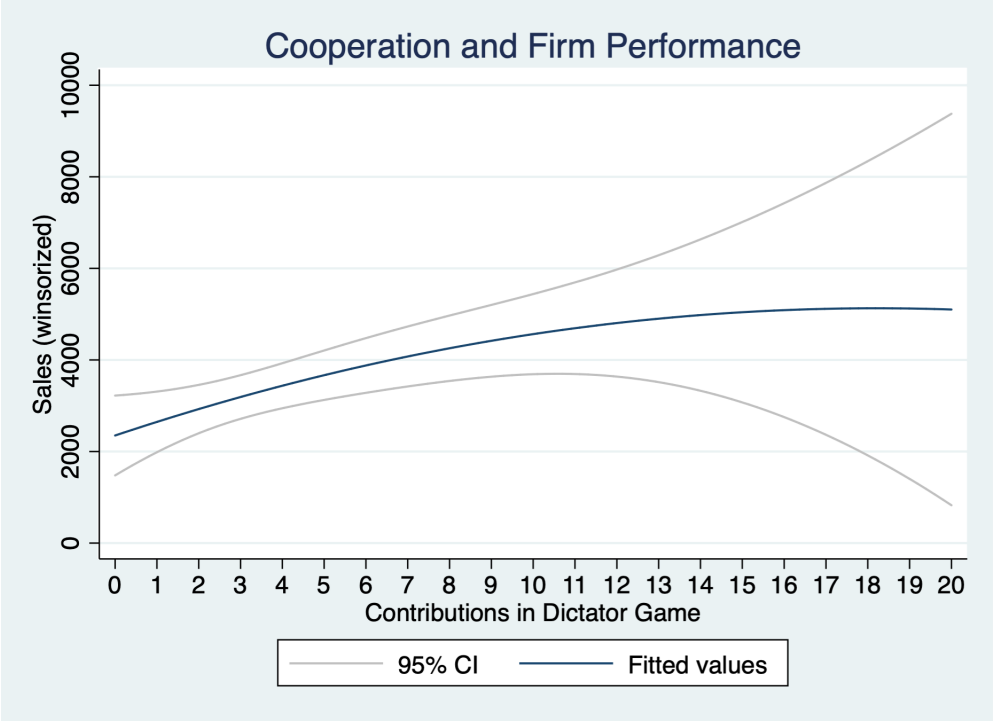
[10]Social comparison theory was first developed by social psychologist Leon Festinger (1954). It states that individuals compare themselves to others to determine their own worth. Comparing ourselves with others serves as a way of self-motivation. In its initial definition comparison works as a benchmark to make evaluations of oneself. By comparing ourselves to a more successful peer, we might be driven to achieve more and improve our abilities.

it is unclear a priori whether the effect is positive or negative. Low-performers increase their effort to increase their future income and achieve a higher status. On the other hand, they might become discouraged and decrease effort. High-Performer might increase their effort to maintain their top status or slack off as they have already achieved an outstanding position.

The second relevant factor for the production function that might change firm outcomes is cooperation within business networks. Firm-to-firm interactions have implications for firm performance through supply chain networks (Acemoglu et al. (2012)), access to credit (McMillan and Woodruff (1999)), and the diffusion of information and business practices (Cai and Szeidl (2018), Fafchamps and Quinn (2018)). The general consensus of this literature is that stronger business networks benefit firm outcomes. Increasing cooperation with one's business network increases firm outputs through reduced input prices, facilitates access to capital, k_i, and increases the skills set, S_i. On the other hand, cooperation that leads to social pressure to share income decreases firm outcomes by affecting investment decisions (Jakiela and Ozier (2016), Squires (2018)). The non-causal relationship between cooperation[11] and firm performance for the baseline sample is depicted in Figure 2.2.4. Firm revenues increase in cooperation with diminishing returns. Regression estimates for the baseline sample suggest that one additional token shared increases sales by around 10%.

[11]Cooperation is measured as contributions in a dictator game where the counterpart is the individual's business partner. Decisions were incentivized and payouts made on the same day for the "dictator" and during the following day for the counterpart. A detailed description of the game and measure can be found in Section 2.3.2.2.

Figure 2.2.4: Relationship Between Cooperation and Firm Performance.



Notes: This figure plots the quadratic relationship between cooperation and firm revenue at baseline for around 600 micro-firms. Revenue data is winsorized at the 1st and 99th percentile. Sales are reported in the Mozambican local currency, Meticais. Respondents could share any amount between 0 and 20 with the recipient in an incentivized dictator game. Units reported are the number of tokens shared where each token is worth 20MZN for recipients and 10MZN for dictators.

The ranking might affect cooperation in two ways: Individuals intensify their cooperation with their business partner to further exploit the benefits of business networks as described in the literature. This includes access to credit through private loans, support with supply chain management, and sharing information about better business practices. Further, individuals might decrease their contributions to their business networks if they change their perceptions about their social obligations to share. Individuals that are ranked lower than what they initially expected to be ranked, might become more self-centered and feel less obliged to share income. The gender of the top seller is of relevance if female top sellers have different cooperation norms than male

top sellers. Individuals that can observe the top seller's gender might change their own cooperation behavior to copy these different cooperation norms and imitate firm success. A detailed description of the difference between male and female top sellers can be found in Section 2.3.5.

2.3 Data and Sampling Strategy

This study took place in the Maputo metropolitan region in Mozambique. 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. Salaried work opportunities are scarce and many workers resort to self-employment. Most of the self-employed are organized in local market clusters. The sample drawn for this study focuses on micro-firms in these markets. In this section, I describe the sampling strategy and data sources, as well as relevant characteristics of the sample and randomization balance checks.

2.3.1 Sample Selection

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 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. We excluded two informal markets whose structures implied a security risk to our enumerator teams. We collected census data for 3.136 firms in 33 markets. The census data includes information about the firm responsible's age, 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 (2018)). 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, gastronomy firms, illegal sales activities, traditional medicine, and wholesale merchants. For this study, I 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 316 firm owners again during the intervention visit. These firms build the sample of this study.

2.3.2 Data

We tracked micro-firms for approximately one and a half years from the census. During this time, we conducted three in-person surveys: shortly after the census survey (baseline survey), four months after the intervention to measure short-term impacts (Survey 2), and one year after the intervention (Survey 3). The ranking was shown in a separate intervention visit two months after the baseline survey visit.

2.3.2.1 Survey Data

All surveys 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. We did not interview employees. In the baseline survey, we collected information on individual and household characteristics of the firm responsible, firm characteristics, business practices, and firm performance indicators. The module on firm characteristics includes information about individual time allocation such as hours and days worked. During Survey 2 we collected information on all time-variant measures included in the baseline survey. The survey was completed by 94% of the sample. Survey 3 is a shorter version of Survey 2 focusing on measures on firm characteristics and firm performance, and was administered to 85% of the sample.

I use the baseline survey to check for balance between the control and treatment group, and to rank firms. The ranking is based on a firm's sales during the week prior to the interview. The data was collected step-wise. 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. We collected data for five primary products and the total rest of sold goods.

To estimate the effect of the ranking on firm performance indicators other than revenues, I also calculate firm profits. Similar to collecting sales data for the week prior to the interview, we collected information on five main expenditures categories. We obtained information about each day of the preceding week regarding personal and product-related transport cost, the cost of purchasing new products and materials, employee salaries, and operational cost (including market taxes). We also collected

data about any additional expenditures for a respective day. These concern almost exclusively cost for meals during the day and the usage of bathrooms. To calculate profits, I sum all main expenditures (excluding other cost) and subtract them from the indicated sales. In Survey 2 and Survey 3 we collected additional profit data by asking individuals how much money they had left over after paying for all expenses in the last month.[12]

The main concern about self-reported sales and profit 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, I focus on sales and profit data for the last two days prior to the interview only. I further verify my results with real sales data that we collected as described in Section 2.6.1.

2.3.2.2 Lab-in-the-Field Data

During the baseline survey and Survey 2 we collected information on risk and pro-social preferences. To elicit risk preferences, we asked individuals to take an incentivized decision between investing in a lottery and a safe amount. Individuals received 1.000MZN (around US$15) as endowment without any framing what the money should be used for. They were then asked to decide how much of this value they wanted to invest in a risky lottery that would double the investment with a 50% probability. With 50% probability they would loose half of the investment. Investments could only be made in 100MZN steps. The win was to be paid out within one day.

We also measured pro-social preferences by eliciting modified dictator game decisions.[13] During the baseline survey and Survey 2, individuals were asked to indicate

[12]Individuals that were unsure about the exact amount were asked to indicate an interval. I use the midpoint value of the interval for observations where the exact value is missing.

[13]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.

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. We 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 (our respondent) on the same day and until the end of the next day for the recipient.[14]

We offered individuals 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. 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).[15] Due to the data collection cost of this measure, we only elicited pro-social preferences at baseline and during Survey 2.

[14]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.

[15]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 significant predictor of entrepreneurial success. I am interested in the impact of a treatment on changes of pro-social preferences and cooperation, and as a consequence firm revenues. 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).

2.3.3 Descriptive Statistics and Randomization Balance

The average age of micro-firm owners in the sample is 34 years, and 41% of them are women. On average, they have about 8 years of schooling and only 10% ever received any formal training in their sector. Our participants live in households with 5.9 household members and spend around 7,351MZN each month for household expenditures. They own around nine household assets. They operate their firms for a little less than eight years (including periods in which the firm was temporarily closed). In their firms, they own assets with a market value of approximately 11,797MZN. On average, over two days they generate revenues of 3,546MZN or 1,483MZN in profits. The average firm owner works a little less than 10 hours a day for 6 days per week. Respondents in our sample shared 44MZN (22% of the endowment) with the recipient[16] and invested a little more than half of their endowment in the risky lottery.

Table 2.1 reports the main randomization check using baseline survey data. Treatment was randomized individually 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 one of the 15 baseline characteristics (household size). Information on the number of individuals living in a household was collected in Survey 2 but not Survey 3. Additional robustness checks including household size as a covariate in the short-term analysis do not alter my results. All empirical results regarding pooled regressions are thus robust to excluding the one randomization strata with imbalance.

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

Table 2.1: Baseline Summary Statistics and Test of Balance for Select Covariates.

	Control			Treatment			All			t-test
	N	mean	se	N	mean	se	N	mean	se	difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(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
Formal business training	124	0.11	(0.03)	191	0.09	(0.02)	315	0.10	(0.02)	0.02
Household size	124	5.53	(0.23)	191	6.11	(0.19)	315	5.88	(0.15)	-0.58*
Expenditures per month	123	6991.28	(338.98)	190	7584.54	(332.33)	313	7351.40	(241.95)	-593.26
Household asset score	125	9.31	(0.19)	191	9.48	(0.16)	316	9.41	(0.12)	-0.16
Years in business	125	7.39	(0.63)	191	8.18	(0.50)	316	7.87	(0.39)	-0.79
Shop asset score (market value in 2016)	125	10529.91	(2409.83)	190	12630.14	(3191.08)	315	11796.72	(2147.19)	-2100.23
Investment in risky lottery	118	5.46	(0.33)	188	5.22	(0.27)	306	5.31	(0.21)	0.23
Sales last two days (winsorized)	98	3282.33	(553.32)	187	3683.90	(408.07)	285	3545.82	(328.09)	-401.58
Profit last two days (winsorized)	95	1500.14	(548.19)	185	1474.97	(398.61)	280	1483.51	(321.85)	25.17
Hours worked per day	123	9.73	(0.15)	189	9.74	(0.11)	312	9.74	(0.09)	-0.01
Days worked per week	124	6.34	(0.07)	188	6.32	(0.08)	312	6.33	(0.06)	0.01
Dictator game contribution	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, profits, and hours and days 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.

2.3.4 Who Are the Low-Performers?

To understand what distinguishes low-performers from their more successful peers at baseline I present differences in their key characteristics in Table 2.2. Low-Performers are defined as individuals whose firm performance at baseline falls into the bottom of the distribution. Specifically, I define the cut-off at the 50th 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 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.[17]

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 net-

[17]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. I find weak evidence that business experience leads to better inventory management and lower risk aversion. Kremer et al. (2013) provide detailed evidence on the impact of improved inventory management and investments.

Table 2.2: Difference Between Bottom and Top Vendors for Select Covariates.

	Bottom			Median & 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
Formal business training	107	0.09	(0.03)	208	0.11	(0.02)	-0.01
Household size	107	5.89	(0.26)	208	5.88	(0.18)	0.01
Years in business	107	6.44	(0.68)	209	8.60	(0.47)	-2.16***
Risk preferences	106	5.22	(0.37)	207	5.37	(0.25)	-0.15
Investment (past 6 months)	78	0.50	(0.06)	138	0.52	(0.04)	-0.02
Hours worked /day	106	9.45	(0.15)	206	9.88	(0.11)	-0.43**
Dictator game contribution	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***
Profit last two days	99	22.85	(245.09)	181	2282.43	(469.54)	-2259.58***

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, profits, and hours and days 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.

work. This causes them to earn 80% less in revenues compared to their peers. The difference in profits is even more striking. The reported results suggest that changes in effort and business experience are likely to explain a large proportion of any observed changes in firm performance.

2.3.5 Are Female Entrepreneurs Different?

If the gender of peers is observable it is not clear a priori what traits individuals can potentially copy from women that excel in their firm sector. To gain some insights in what way women differ, I illustrate key characteristics of female and male top sellers in Table 2.3. Due to the small sample of top sellers most differences although large in size are not significantly different from each other. Male top sellers are 7.5 years younger than female but they operate their firms for the same amount of time. This suggests that men start their successful businesses earlier rather than women taking more time in achieving similar success. Whereas both men and women have the same amount of years of formal schooling, 14% of men have formal business training compared to 8% of women. Households of top men are slightly larger in size.

Around 67% of women and 70% of men have invested in their firm during the last six months. Men invest around 100MZN (US$1.50) more in the risky lottery than women. There is almost no difference in effort between men and women at the top with the average top seller working close to ten hours a day. Men share a little less than 20% of their endowment, and women share 28% of their endowment (the sample average is 22% as described above). This difference is not statistically different from zero. Although men report higher revenues than woman there is almost no difference in profits of male and female top sellers.

Table 2.3: Difference Between Male and Female Top Vendors for Select Covariates.

	Male			Female			t-test
	N	mean	se	N	mean	se	difference
	(1)	(2)	(3)	(4)	(5)	(6)	(10)
Baseline covariate							
Age	14	29.71	(2.39)	13	37.23	(2.25)	-7.52**
Years of schooling	14	7.71	(0.69)	13	8.00	(0.58)	-0.29
Formal business training	14	0.14	(0.10)	13	0.08	(0.08)	0.07
Household size	14	6.43	(0.76)	13	5.54	(0.74)	0.89
Years in business	14	9.43	(1.94)	13	9.46	(2.10)	-0.03
Risk preferences	14	6.36	(1.04)	12	5.33	(1.33)	1.02
Investment (past 6 months)	10	0.70	(0.15)	6	0.67	(0.21)	0.03
Hours worked /day	14	9.93	(0.40)	13	10.00	(0.59)	-0.07
Dictator game contribution	14	3.86	(0.65)	12	5.67	(1.02)	-1.81
Sales last two days	13	15802.31	(3476.58)	12	11580.00	(2940.15)	4222.31
Profit last two days	13	7803.69	(3495.21)	12	7714.83	(2657.61)	88.86

Notes: This table shows summary statistics for female and male top sellers. Top sellers are those whose firm performance lies above the 90th percentile of their sector-specific distribution. Reported is a selection of covariates. Sales, profits, and hours and days 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.

2.4 Estimation Strategy

The theoretical framework in Section 2.2.2 predicts that the effects from relative performance feedback can be both positive and negative. The sign of the overall effect is thus an empirical question. I exploit the availability of pre- and post-intervention data to estimate a difference-in-differences model. Randomization was done at the individual level stratified by gender. As gender might be a relevant factor for firm performance and the effectiveness of the ranking, stratification improves the precision of my estimates (see Duflo et al. (2008) or Bruhn and McKenzie (2009)). To estimate the causal impact of the ranking on outcomes, I estimate Average Treatment Effects (ATE). Note that the ATE is equal to the Treatment-on-the-Treated Effect (TTE) as only individuals that could be found at the time of the intervention visit are included in the sample.[18] There is thus full compliance.

Let $Treat_i$ be an indicator for assignment to treatment equal to 1 if individual i has seen her ranking and 0 otherwise. $Positive_i$ denotes a binary variable that is equal to 1 if an entrepreneur is in or above the 50th percentile, and 0 otherwise. $Post$ is a binary indicator that equals 0 for pre-intervention data and 1 otherwise. I control for survey round effects and the randomization strata, own gender, to improve efficiency. $Outcome_{it}$ is a measure for individual i of her effort, cooperation, and firm performance indicators. I pool all data where two post-intervention periods are available. For those outcomes where only one post-period of data is available this is clarified in the respective table.[19] The standard errors, ε_i, are clustered at the level of the randomization unit,

[18] 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 those individuals were identified again at a later point they are excluded from this study sample. Nevertheless, results are robust to including the full sample and can be obtained upon request.

[19] Additional estimates for both four-months and one-year analyses are similar in size to the pooled estimates and over time. Estimates for the one-year only impact are generally less precise, as expected, given the loss in power for a smaller sample. The lack of precision in estimating one-year effects of self-reported firm revenues is compensated by precise estimates of treatment impacts on observed revenue data.

the individual. I estimate the following equation:

$$Outcome_i = \alpha_i + \delta_1 Treat_i + +\delta_3 Positive_i + \delta_4 \left(Treat_i \times Positive_i\right) + \delta_2 Post$$
$$+ \boldsymbol{\beta_1}(\mathbf{Treat_i} \times \mathbf{Post}) + \boldsymbol{\beta_2}\left(\mathbf{Positive_i} \times \mathbf{Post}\right) + \boldsymbol{\beta_3}\left(\mathbf{Treat_i} \times \mathbf{Positive_i} \times \mathbf{Post}\right)$$
$$+ \gamma_1 Gender_i + \gamma_2 SurveyRound + \varepsilon_i \quad (2.4.1)$$

The interaction term $Treat_i \times Positive_i$ identifies the differential treatment effect for individuals that received a positive signal as their performance was at the median level or above. β_3 is the additional effect of receiving positive feedback for those individuals that received the treatment. The impact of the ranking on high-performers is determined by the sum of β_1 and β_3. Linear hypotheses tests are reported in all tables below the respective coefficients of interest. β_1 identifies the ATE of the ranking on low-performing individuals under the identifying assumption of random assignment. 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, β_1 is the impact of the ranking on low-performers compared to low-performers that did not observe their relative position. Whereas, $\beta_1 + \beta_3$ 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.

I estimate the impact of peer characteristic observability as an additional effect over the ranking impact alone. $PeerInfo_i$ 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 randomization strata fixed effects. Standard errors are clustered at the individual level. I restrict the sample to individuals that are not top sellers (excluding the last percentile) 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 full model is specified as follows:

$$\begin{aligned} Outcome_i = \alpha_i + \delta_1 Treat_i + +\delta_3 Positive_i + \delta_4 \left(Treat_i \times Positive_i\right) \\ + \mu_1 PeerInfo_i + +\mu_2 TopGender_i + \mu_3 \left(PeerInfo_i \times TopGender_i\right) + \delta_2 Post \\ + \boldsymbol{\beta_1}(\mathbf{Treat_i \times Post}) + \boldsymbol{\beta_2}\left(\mathbf{Positive_i \times Post}\right) + \boldsymbol{\beta_3}\left(\mathbf{Treat_i \times Positive_i \times Post}\right) \\ +\boldsymbol{\eta_1}\left(\mathbf{PeerInfo_i \times Post}\right)+\boldsymbol{\eta_2}\left(\mathbf{TopGender_i \times Post}\right)+\boldsymbol{\eta_3}\left(\mathbf{PeerInfo_i \times TopGender_i \times Post}\right) \\ + \gamma_1 Gender_i + \gamma_2 SurveyRound + \varepsilon_i \quad (2.4.2) \end{aligned}$$

The interpretation of β coefficients is as above. The main interest of this specification is the estimation of the effect when individual i observes that the top seller is a woman. The joint effect of η_1 and η_3 determines the additional effect of observing a top woman on the treatment effect of rankings. As before, I estimate joint effects for low- and high-performers separately. The p-values of the linear hypotheses tests of joint significance are reported in the respective column of each outcome in the relevant tables.

2.5 Results

In this section, I present the empirical results in two steps. First, I analyze the impact of observing one's ranking on effort and cooperation. I then present evidence how the behavioral changes translate into changes in firm performance by discussing the impact of rankings on three firm output measures. In Section 2.5.2 I discuss results on the impact of gender observability.

2.5.1 Impact of Firm Rankings

Tables 2.4 - 2.6 present estimation results of the differential treatment effect of learning about one's rank. All outcomes on effort and firm outputs are winsorized at the 1st and 99th percentile to control for potential outliers. I report multiple measures of effort and firm performance in Tables 2.4 and 2.6. For these outcome families, I report Romano-Wolf adjusted q-values to correct for multiple hypotheses testing (Romano and Wolf (2005)). For each outcome I report the means for low- and high-performers in the control group separately. I also provide p-values of linear hypothesis tests of a joint effect of observing the ranking and receiving a positive signal.

2.5.1.1 Impact on Individual Effort

I begin my analysis by estimating the impact of observing one's ranking on personal effort choice. We can think of effort in two ways: productivity of hours worked (intensive margin) and the quantity of labor and work hours supplied (extensive margin). The available data allows me to estimate the impact of the treatment on the extensive margin of effort by measuring how many hours per day and how many days in a regular week an individual personally takes care of her shop. Panel A of Table 2.4 contains ATE estimates on outcomes for the pooled sample. Table A.2 in Appendix A contains

dynamic treatment effects on outcomes. Standard errors clustered at the level of the individual are reported in parenthesis next to each estimate. Romano-Wolf adjusted q-values for each estimate are reported in brackets. All regressions are difference-in-differences regressions where only the ATE estimates are reported. Survey and strata fixed effects are included in all regressions.

The impact of the ranking treatment on effort choices of low-performers is large and significant. Low-performers work 0.862 (s.e. 0.346) hours more per day than low-performers in the control group (mean = 9.27 hours). This corresponds to a 9% increase in labor supply. Importantly, treated individuals initially performing worse than their peers exert almost as much effort after the intervention as do high-performers in the control group. Low-performers also increase the number of days worked during a normal week by 0.4 days (s.e. 0.16). This increase leads to low-performers working as many days as high-performers in the control group after observing their ranking. These estimates are robust to correcting for multiple hypotheses testing.

Appendix Table A.2 provides corresponding ATE estimates after four months in Panel B and one year in Panel C. Effects are very similar in size and significance to the pooled sample estimates. After four months, low-performers increase their effort by 0.87 (0.34) hours per day. After one year, low-performers in the treatment group still exert higher effort by working 0.842 (s.e. 0.47) hours more than the control group. At both time periods this means that nudging higher effort through providing ranking information closes the effort gap between treated low-performers and high-performers in the control group. The treatment impact on effort is confirmed for Panel C estimating the impact of the treatment on days worked in the shop. The ATE estimate is not statistically significant in the short-run. This is likely due to the fact that there is little variation in days worked between low- and high-performers in the control group after four months to begin with.

Table 2.4: Ranking Impact on Effort - winsorized outcomes.

	Difference-in-difference regression					
	Hours worked			Days worked		
	ATE	se	q-value	ATE	se	q-value
Outcome	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Pooled Sample						
Ranking	0.862**	(0.346)	[0.0198]	0.401**	(0.163)	[0.0198]
Positive Signal	0.866***	(0.317)	[0.0198]	0.482***	(0.176)	[0.0198]
Ranking × Positive Signal	-1.371***	(0.407)	[0.0000]	-0.390*	(0.228)	[0.0891]
Control mean - bottom	9.271			5.983		
Control mean - top	10.299			6.325		
Joint test - p-value	0.0182			0.9444		
adjusted r-squared	0.062			0.019		
Observations (cluster)	863 (315)			862 (315)		

Notes: Outcome measures are winsorized at the 1st and 99th percentile. All coefficients are difference-in-differences estimates. All regressions include randomization strata and survey fixed effects. Baseline differences and time trend effects are controlled for in all regressions though not reported. 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%.

The ranking has a negative impact on high-performers. The ATE coefficient on hours worked is negative and statistically significant. 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 effort by 0.51 hours. Treated high-performers still exert higher effort than the average low-performer in the control group. The dynamic treatment effects confirm this result. We do not observe statistically significant effects of the treatment on high-performers for the amount of days they work per week.

The ranking is thus successful in increasing the effort level of low-performers without impairing high-performers as the effect on them is small or zero. The positive effect on low-performers is consistent over time. This suggests that individuals that were once nudged into higher effort levels correctly update their beliefs on the return to effort and maintain a higher effort level.

2.5.1.2 Impact on Pro-Social Behavior

Table 2.5 presents estimates of the effects of rankings on cooperation. The degree to which individuals cooperate with their business networks is proxied by eliciting pro-social preferences through dictator games. Individuals took decisions with respect to one specific person (one link) of their business network. I interpret effects on pro-social preferences with this one person as representative for changes in cooperative behavior with the individual's entire business network. We collected data on dictator game contributions at baseline and during Survey 2 (after four months). The reported results are thus ATE estimates for the short run only. As before, the preferred specification is a difference-in-differences regression model. Standard errors are clustered at the individual level and reported in parentheses, Romano-Wolf q-values are reported in brackets next to them.

Table 2.5: Ranking Impact on Cooperation.

	Difference-in-difference	
	Standard dictator game	
	ATE	se
Outcome	(1)	(2)
Short-term impact		
Ranking	2.460***	(0.817)
Positive Signal	2.113***	(0.779)
Ranking × Positive Signal	-1.991*	(1.066)
Control mean - bottom	2.839	
Control mean - top	4.549	
Joint test - p-value	0.1723	
adjusted r-squared	0.012	
Observations (cluster)	598 (313)	

Notes: All coefficients are difference-in-differences estimates. All regressions include randomization strata fixed effects. Baseline differences and time trend effects are controlled for in all regressions though not reported. Standard errors clustered at the individual level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Cooperation is measured in units of tokens shared with the recipient. The endowment was 20 tokens that had a monetary value of approximately US$3. The ranking has no impact on the cooperative behavior of high-performing individuals. Although the additional effect of receiving a positive signal on the impact of the ranking is negative and statistically significant at the 10% level, the joint effect is not distinguishable from zero. Further, the negative effect is smaller in size than the large positive of the ranking itself.

The ATE estimate of the treatment for low-performers is positive and highly significant. Low-performers almost double the number of tokens shared compared to low-performers in the control group. This effect is statistically significant at the 1%

level. Treated individuals increase the amount of tokens they share by 2.46 units over a base level of 2.84 units in the control group of low-performers. Four months after the intervention, treated low-performers share even more than high-performers in the control group (mean = 4.55). These results suggest that ranking information encourages low-performers to increase their cooperation with their business network rather than becoming more selfish and decreasing their cooperation.

2.5.1.3 Impact on Firm Performance

The conceptual framework in Section 2.2.2 suggests that increases in effort and cooperation lead to increases in firm outcomes. I estimate treatment effects on firm outcomes for a family of three different firm performance indicators: sales over the last two days, researcher-calculated profits over the last two days, and self-reported profits over the last month. All three outcomes are winsorized at the 1st and 99th percentile. The ATE estimates for sales, profits, and self-reported profits are displayed in Columns (1), (4), and (7) respectively of Table 2.6. Clustered standard errors are reported in parentheses and adjusted q-values in brackets next to each estimate. Effects are measured in changes in the Metical value (Mozambique's currency) for the pooled sample in Panel A.

The ranking has no effect on firm outcomes of high-performing individuals. The results in Tables 2.4 and 2.5 show that the ranking does not change high-performers' cooperative behavior and has only a small impacts on their effort choice. Firm outcomes of high-performers that received information about their relative position are thus similar to those of high-performers in the control group.

Changes in the behavior of low-performers, however, translate into changes in firm outcomes. Low-performers in the control group increase their sales by 1,623MZN (significant at the 5% level). They more than double their sales compared to their peers

Table 2.6: Ranking Impact on Firm Performance - winsorized outcomes.

	Difference-in-difference regression						OLS regression		
	Sales			Profit			Self-reported profit		
Outcome	ATE (1)	se (2)	q-value (3)	ATE (4)	se (5)	q-value (6)	ATE (7)	se (8)	q-value (9)
Panel A: Pooled Sample									
Ranking	1,623.606**	(738.883)	[0.0594]	1,397.214*	(756.298)	[0.0693]	1,255.836**	(574.817)	[0.0594]
Positive Signal	-1,206.381	(976.271)	[0.2970]	-434.876	(998.386)	[0.6832]	2,599.745***	(549.178)	[0.0000]
Ranking × Positive	-2,175.318	(1,333.257)	[0.2178]	-1,462.301	(1,348.797)	[0.2970]	-1,241.847	(843.098)	[0.2970]
Control mean - bottom	1,395.586			345.707			2,330.000		
Control mean - top	3,764.678			1,733.034			4,902.726		
Joint test - p-value	0.6214			0.9537			0.9822		
adjusted r-squared	0.049			0.013			0.026		
Observations (cluster)	815 (314)			810 (314)			535 (302)		

Notes: Outcome measures are winsorized at the 1st and 99th percentile. Coefficients in Columns (1) and (4) are difference-in-differences estimates. Baseline differences and time trend effects are controlled for in all regressions though not reported. Coefficients in Column (7) are OLS estimates as baseline data on 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%.

(the mean of low-performers in the control group is 1,395MZN). The treatment thus causes low-performers to close the performance gap to high-performers by almost 43%. Low-performers also catch up in terms of calculated profits. The increase in profits equalizes the difference in profits between treated low-performers and high-performers in the control group.

Column (7) in Table 2.6 displays the treatment impact on self-reported profits. We asked respondents in our sample how much money they had left over after paying for all expenses in the last month. This data is available for both post-treatment surveys but not for the baseline survey. The reported results are OLS estimates of the pooled sample. The results are strikingly similar to results on sales. Low-performers increase their monthly profits by 54% compared to the control group. This effect is statistically significant at the 5% level. By increasing their profit, firms close the gap to high-performers in the control group by 48%.

These results are robust to multiple hypotheses testing. In summary, the ranking-induced changes in behavior translate into improvements in firm outcomes. Given the very large impact on firm outcomes, it is unlikely that changes in effort and cooperation alone fully explain the changes in firm performance. Other factors affected by the ranking not measured in this study might further contribute to the increase in firm outcomes. Nevertheless, these results show that low-performing micro-firms are not optimizing their effort and cooperation choices. Further, providing information about their relative position nudges firm owners into optimal behavior without further instructions on how to close the performance gap. But what motivates behavioral changes?

2.5.2 Does it Matter If the Top Seller is a Woman?

To gain further insights on why information about one's relative position matters, I exploit exogenous variation across the observability of peer characteristics. I varied whether an individual receives her ranking only (Figure 2.2.1) or if she additionally observes the gender and age of peers (Figure 2.2.2). In this section, I am particularly interested in the impact of observing a woman at the top. Female top sellers exert a similar amount of effort as their male peers as shown in Table 2.3. Any impact on changes in effort from observing a female top seller should thus be driven by changes in expectations about attainable incomes rather than the imitation of business practices.

Tables 2.7 - 2.9 display estimation results for treatment effects of the ranking and gender observability combined. The samples are restricted to firms below the 90th percentile. I estimate the joint effects of observing a female top seller and receiving information about one's relative position as before for low- and high-performers separately.

2.5.2.1 Effort and Cooperation Optimization

Table 2.7 reports results for effort choices measured in hours and days worked. The first row displays the effect of the ranking after controlling for the impact of peer characteristics in the reduced sample pooling over all periods. The treatment effect of the ranking itself is robust and statistically significant at the 5% level for both effort measures. It is similar in size and robust to controlling for multiple hypotheses testing. To evaluate the impact of a female top seller, we are interested in the joint test of the linear hypothesis that the ranking coefficient, the peer info coefficient, and the interaction term between peer info and gender of the top seller is statistically significant.

Table 2.7: Ranking and Peer Info Impact on Effort - winsorized outcomes.

	Difference-in-difference regression					
	Hours worked			Days worked		
Outcome	ATE (1)	se (2)	q-value (3)	ATE (4)	se (5)	q-value (6)
Panel A: Pooled Sample						
Ranking	0.850**	(0.405)	[0.0495]	0.416**	(0.192)	[0.0495]
Positive Signal	0.547	(0.357)	[0.0594]	0.502*	(0.261)	[0.0594]
Ranking × Positive	-1.099**	(0.442)	[0.0099]	-0.394	(0.315)	[0.1386]
Peer Info	-0.043	(0.252)	[0.8713]	-0.032	(0.204)	[0.9703]
Top Gender	-0.316	(0.334)	[0.4752]	-0.019	(0.255)	[0.4752]
Peer Info × Top Gender	0.454	(0.469)	[0.7228]	0.178	(0.430)	[0.7228]
Control mean - bottom	9.309			5.982		
Control mean - top	9.870			6.171		
Joint test bottom - p-value	0.0097			0.1469		
Joint test top - p-value	0.7239			0.7433		
adjusted r-squared	0.065			0.010		
Observations (cluster)	687 (251)			685 (251)		

Notes: Outcome measures are winsorized at the 1st and 99th percentile. All coefficients are difference-in-differences estimates. All regressions include randomization strata and survey fixed effects. Baseline differences and time trend effects are controlled for in all regressions though not reported. 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%.

The augmented treatment including peer characteristics is not statistically differentiable from the simple ranking. But low-performers that observed peer characteristics react to the treatment more strongly. The joint effect is positive and statistically significant at the 1% level for hours worked. It is less precise but positive for days worked. Low-performers that observe that the top seller in their sector is a woman, increase their effort by 1.26 hours compared to low-performers in the control group that did not

see their ranking. The effect of the ranking itself is 0.85 hours.[20]

Table 2.8: Ranking and Peer Info Impact on Cooperation.

	Difference-in-difference	
	Standard dictator game	
	ATE	se
Outcome	(1)	(2)
Short-term impact		
Ranking	2.576***	(0.956)
Positive Signal	2.762***	(0.973)
Ranking × Positive Signal	-2.761**	(1.227)
Peer Info	-0.663	(0.805)
Top Gender	0.298	(0.877)
Peer Info × Top Gender	1.891	(1.398)
Control mean - bottom	2.862	
Control mean - top	4.800	
Joint test bottom - p-value	0.0034	
Joint test top - p-value	0.4739	
adjusted r-squared	0.010	
Observations (cluster)	477 (249)	

Notes: All coefficients are difference-in-differences estimates. All regressions include randomization strata fixed effects. Baseline differences and time trend effects are controlled for in all regressions though not reported. The sample is restricted to individuals whose baseline performance did not lie above the 90th percentile. Standard errors clustered at the individual level. * significant at 10%; ** significant at 5%; *** significant at 1%.

[20]As a robustness check to self-reported work hours, I estimate treatment effects for the time that firm owners open their businesses. During the baseline survey and Survey 2 (four months after the treatment intervention) we asked firm owners about the time they open and close their business during a normal week. This measure is likely to be less prone to measurement bias due to recalling error as opening hours do not vary. As the question is also less salient than the direct effort question, I argue that it is a valid measure to check the robustness of treatment effects on reported work hours. The coefficients should be interested as percentage changes of one hour. The results in Table A.5 confirm the positive impact of the ranking on effort. For all days besides Wednesdays and Saturdays, low-performers that observed their ranking open their shops between 25 and 48 minutes earlier than their peers in the control group (statistically significant at minimum at 5% level). Contrary to the results on reported hours worked, observability of a woman at the top has no effect on effort choices.

Similarly, individuals that observe a female top seller strongly react to the treatment by changing their cooperation levels. The joint effect is significant at the 1% level. Low-performers that observe their ranking together with peer characteristics share 3.8 tokens more than low-performers in the control group. This corresponds to about one third of their endowment. These results are driven by changes in the behavior of female micro-firm owners as shown in Table A.8 of Appendix A. The additional effect of peer characteristic observability is precise and large for women. I interpret this result as evidence that especially women receive further information that higher incomes are attainable and learn from their female peers.

2.5.2.2 Impact on Firm Performance

Observing a woman at the top increases sales of low-performing individuals by 2,025.92MZN (US$30) compared to low-performers that have observed the ranking only. Not only is the effect of the treatment on firm outcomes strongest for those that observe peer characteristics, but the two treatment types are significantly different from each other. Those that observed a female top seller outperform high-performers in the control group (excluding top sellers) by 13%. The treatment effects on profits are similar in size though not statistically significant. The lack in precision might be attributable to the loss in statistical power as I reduce the sample size by almost 20% when excluding top seller observations. Importantly, the treatment effect of observing one's ranking is robust to controlling for peer characteristic observability. Further, providing information about peer's gender contains relevant information for micro-firm owners and reinforces the positive impact of a ranking itself. Though not necessarily through changes in effort and cooperation.

Table 2.9: Ranking and Peer Info Impact on Firm Performance - winsorized outcomes.

	Difference-in-difference regression								
	Sales			Profit			Self-reported profit		
Outcome	ATE (1)	se (2)	q-value (3)	ATE (4)	se (5)	q-value (6)	ATE (7)	se (8)	q-value (9)
Panel A: Pooled Sample									
Ranking	2,262.170***	(829.773)	[0.0396]	1,895.462**	(848.063)	[0.0495]	1,163.777*	(595.265)	[0.0891]
Positive	-396.324	(1,004.385)	[0.1584]	614.820	(1,121.127)	[0.7921]	2,546.043***	(633.732)	[0.0000]
Ranking × Positive	-1,860.055	(1,302.907)	[0.6733]	-1,910.593	(1,426.382)	[0.6733]	-1,119.981	(881.738)	[0.6733]
Peer Info	-2,373.271***	(831.703)	[0.0297]	-1,359.768	(955.873)	[0.1386]	-833.833	(666.394)	[0.5941]
Top Gender	-1,306.866*	(752.691)	[0.0792]	-221.491	(800.292)	[0.2673]	-1,140.656*	(625.240)	[0.0198]
Peer Info × Top Gender	4,399.194**	(1,935.754)	[0.0396]	2,336.326	(1,934.157)	[0.1089]	3,206.334**	(1,384.025)	[0.0396]
Control mean - bottom	1,414.870			288.759			2,409.455		
Control mean - top	3,736.427			2,214.600			4,926.083		
Joint test	0.0346			0.1425			0.5742		
Joint test top	0.2149			0.6338			0.2801		
adj. r-sq.	0.053			0.021			0.040		
Observations	668 (251)			664 (251)			427 (239)		

Notes: Outcome measures are winsorized at the 1st and 99th percentile. Coefficients in Columns (1) and (4) are difference-in-differences estimates. Baseline differences and time trend effects are controlled for in all regressions though not reported. Coefficients in Column (7) are OLS estimates as baseline data on 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%.

2.6 Robustness Checks

Survey measures on sales and profits raise two main concerns. First, sales reports might be subject to misreporting due to recalling error. I minimize this threat by focusing on sales of the last two days before the interview rather than a whole week or month. Nevertheless, especially individuals with very low levels of education might struggle reporting sales data as they are less likely to keep track of their business figures. Second, and most important, individuals that received a negative signal of their ranking (information that their performance is below the average) might be more likely to tell us that they increased their sales. In this case, the firm outcome data would be subject to measurement error that is correlated with treatment status. I address these concerns by validating survey measures of sales with enumerator observed sales data.

2.6.1 Validation Measure

At Survey 3, one year after the intervention, we tracked sales of firms during an entire business day. Enumerators arrived at a firm location around 9am. At baseline, 94% of firms are open by this time during regular weekdays and on Saturdays (the days the enumerator team conducted interviews). Survey 3 was administered as soon as possible and focused on the most important measures to not disrupt normal business activities. Enumerators then stayed with the firm for the entire day until around 5pm. For each client that approached the shop, the enumerator tracked the client's gender and whether or not a sale was realized. For each sale, the enumerator tracked the type of product that was sold, the unit price of the product and how many units were sold, and the total price charged. The total price might differ from the price charged for two reasons: Firms charge less (round down) due to a lack of change,[21] or firms actually

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

Table 2.10: Comparison of Survey and Tracked Sales Means at End-line.

	All	Control Group	Ranking Group
		A. Low-Performers	
Survey Mean	2149.198	1634.148	2406.722
	(3396.458)	(1846.434)	(3941.765)
Tracked Mean	1639.297	939.25	1983.582
	(2973.943)	(1102.377)	(3509.074)
Correlation Coefficient	0.6045	0.6126	0.6026
		B. High-Performers	
Survey Mean	2690.206	2503.127	2824.374
	(3886.29)	(3240.316)	(4301.407)
Tracked Mean	1568.144	1691.648	1479.571
	(2540.349)	(2106.88)	(2817.646)
Correlation Coefficient	0.5219	0.2960	0.6164

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%.

charge more than the unit price would suggest. We could not confirm whether the latter is due to miscalculations or strategic behavior.

2.6.2 Differences Between Survey and Tracked Sales Data

Table 2.10 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 groups separately. The respective means are reported for firms in the bottom of the distribution and the average and top separately as our main concern is the validity of self-reports of individuals that received negative feedback. I also report correlation coefficients between the two measures for each respective group.

For low-performers 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 is no misreporting correlated with treatment status for low-performers. For high-performers 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 tracked sales and regress it on treatment.[22] Equation 2.6.1 defines the empirical model to be estimated:

$$Outcome_i^S - Outcome_i^V = \beta_0 + \beta_1 Treat_i + \beta_2 Positive_i + \beta_3 \left(Treat_i \times Positive_i\right) + \gamma_1 Gender_i + \gamma_2 X_i + \varepsilon_i \tag{2.6.1}$$

γ_i controls for gender fixed effects and X_i includes weekday fixed effects. If a β-coefficient is negative, then treated individuals are more likely to underreport 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 low-performers, and if anything the bias would suggest an underestimation of the true treatment effect.[23]

2.6.3 Results for Tracked Sales Data

As there is no baseline data available for tracked sales I report OLS estimates for the main treatment effects in Table 2.11. Table 2.12 displays effects for the full specification controlling for peer characteristic observability for the reduced sample excluding top

[22]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 monitored by trained enumerators.

[23]These results are not displayed but available upon request.

sellers.

Table 2.11: Ranking Impact on Firm Performance - winsorized outcome.

	OLS regression	
	Observed sales	
	ATE	se
Outcome	(1)	(2)
Long-term impact		
Ranking	1,149.282**	(504.903)
Positive Signal	971.479***	(348.033)
Ranking × Positive Signal	-1,485.816**	(646.313)
Control mean - bottom	939.25	
Control mean - top	1844.73	
Joint test - p-value	0.4105	
adjusted r-squared	0.009	
Observations (cluster)	274	

Notes: Outcome measure is winsorized at the 1st and 99th percentile. Coefficients are OLS estimates as baseline data is not available. All regressions include randomization strata and survey fixed effects. Standard errors clustered at the individual level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2.11 confirms the findings on self-reported firm outcomes. Observing one's ranking increases sales by 1,149MZN (significant at 5% level). This implies that treated low-performers double their sales compared to low-performers in the control group. Compared to high-performers in the control group, low-performers that observed their ranking fully closed the performance gap after one year. The effect on high-performers in the treatment group is null suggesting that the ranking improves firm performance of the worst firms without prejudicing high-performers.

The tracked sales data further provides evidence that those that benefit most from the ranking are individuals that observed peer characteristics. Table 2.12 shows treat-

ment impacts including indicators for gender observability. The effect of the ranking itself is robust to controlling for the impact of gender observability.

Table 2.12: Ranking and Peer Info Impact on Firm Performance - winsorized outcome.

	OLS regression	
	Observed sales	
	ATE	se
Outcome	(1)	(2)
Long-term impact		
Ranking	992.897*	(574.473)
Positive Signal	599.561	(385.133)
Ranking × Positive Signal	-1,306.275*	(669.851)
Peer Info	61.386	(552.171)
Top Gender	-625.220**	(314.777)
Peer Info × Top Gender	1,128.804	(1,130.143)
Control mean - bottom	951.88	
Control mean - top	1555.63	
Joint test bottom - p-value	0.1011	
Joint test top - p-value	0.6694	
adjusted r-squared	0.012	
Observations (cluster)	220.000	

Notes: Outcome measure is winsorized at the 1st and 99th percentile. Coefficients are OLS estimates as baseline data 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. * significant at 10%; ** significant at 5%; *** significant at 1%.

2.7 Conclusion

Without full information about the attainability of future incomes, individuals are likely to misjudge the returns to effort. Increasing cooperation with one's business network

might be potentially very costly and unalterable in the future. The relatively high cost to experimentation might explain why micro-firm owners forego large increases in income for small changes in behavior. I attempt to elicit the relevance of effort and cooperation for firm performance by conducting a simple field experiment. Individuals in the treatment group observe their relative sales in their sector whereas individuals in the control group receive no additional information. Rankings contain two types of information for micro-firms: how much others earn with their firms and what the attainable revenue limit is for those peers, and for firms in the bottom, that their current practices are likely to be not optimal.

My experimental results generated three facts. First, micro-firm owners react to negative ranking feedback by changing their effort choices. Micro-firm owners in the bottom of the distribution significantly increase the amount of hours worked. Second, the ranking induces low-performing firm owners to cooperate more. Third, changes in effort and cooperation help low-performers to close the performance gap to high-performers in the control group by 48%.

These results show that effort and cooperation are relevant determinants of micro-firm performance. Contrary to general assumptions, firm owners do not optimize over effort and cooperation, and forego large revenues. Together with recent studies on business networks by Cai and Szeidl (2018) or Fafchamps and Quinn (2018), this study suggests that micro-firm owners can potentially learn from their peers, and benefit from stronger business networks. Interventions fostering network creation and inducing higher effort levels are potentially cost-effective as shown in this study. Many organizations supporting firm development, collect baseline data on revenues as part of their program and could share relative performance information at a low cost.

This study furthermore touches upon the impact of peer characteristic observability. The effect of the ranking seems to be strongest for those individuals that received some

information about their peers. Although this information was not sufficient to identify specific individuals, peer's gender seems to contain relevant information. Although gender observability positively impacts firm outcomes the effects on effort and cooperation cannot be precisely estimated. This suggests that additional mechanisms, other than changes in effort and cooperation, drive the observed changes in firm performance.

Further research will have to identify additional channels through which information on relative rankings affect firm performance. A key question is how such rankings should be presented most efficiently. Future insights on the impact of business network interventions randomized on different levels might further generate evidence on general equilibrium effects of the impact of changes in cooperation and effort. My findings have implications for policies that support enterprise growth - in particular of those firms that perform worst.

Chapter 3

Reference Points and Entrepreneurship

3.1 Introduction

Micro-enterprises are an important income source of many poor households in developing countries, yet evidence on the constraints that hold these businesses back from developing into SMEs and how to relax them is scarce. Productivity gains from human capital interventions are surprisingly small[1] and returns to access to financial capital in most settings puzzlingly low.[2]

This raises a number of questions. Are internal constraints binding for the development of micro-enterprises? Is an intervention relaxing internal constraints more efficient than interventions teaching basic business skills? Are there complementarities that can be achieved by combining both approaches? Perhaps the most important, but difficult, question to answer is what specific types of internal constraints are most important and

[1] See McKenzie and Woodruff (2013) for a detailed summary on the returns to traditional business skills training.

[2] See de Mel et al. (2014) and Blattman et al. (2014) for two examples of conflicting evidence on the impact of cash drops on entrepreneurship.

how to design successful policies to overcome them.

These questions become particularly prevalent in conflict-ridden countries such as Mozambique. Decades of independent and civil wars, and ongoing conflict between the incumbent party and the opposition have not only constrained economic resources and access to education but also impaired the population's trust in the future. Without entrepreneurs believing in economic growth and stability it is unlikely that they will set themselves business objectives that foster investments. Furthermore, a lack of positive examples and role models to follow will likely restrain higher aspiration levels, leading to suboptimal business efforts and practices.

We tackle these questions in urban vendor clusters of the greater Maputo area, i.e. formal street markets in the capital of Mozambique and its neighboring sister city Matola. Entrepreneurs in these markets typically operate on extremely low profitability margins, have no formal training, low general education levels, and limited perspectives for business growth. We identified an experimental sample of approximately 790 market vendors in these clusters of which 600 were randomly assigned to watch an inspirational video, additionally learn about and set a "SMART" goal, receive basic business skills training, neither or a combination of these interventions.

The video documents the path to success of a young male entrepreneur that started his business career as a street merchant selling school supplies in the city of Maputo. Without any major external support, he managed to continuously grow his business and is nowadays the owner of several stores in different market clusters and a two-story supermarket in a newly constructed neighborhood of Matola. We showed the video to entrepreneurs individually at their stalls in a separate visit after having collected baseline information about household and business outcomes. By providing participants with a new perspective on business growth (through role model exposure) we expect to shift their reference points and increase vendors' aspirations in the long run. Aspirations

are, similarly to self-confidence, expected to translate into greater efficacy, effort, and ultimately business success through increased sales and profits.

An immediate concern about changing aspirations arises in settings where knowledge on how to save and invest or set feasible business goals is limited such that higher aspirations might not necessarily translate into productive behavior. We approach these limitations by providing vendors with either simple rules-of-thumb to separate their personal and business expenses, supporting them in developing a business goal to be achieved over the course of maximum one year, or both. The goal setting intervention is modeled on standard management practices generally known as "SMART" (Specific, Measurable, Achievable, Results-focused, Time-bound) goals adapted to the literacy level of our participants.

To test the impact of our interventions, we returned six months and 14 months after implementation to collect survey data on psychometric indicators, business practices, and household and business outcomes. The effects of our simple nudge of watching a short video are impressively large. Shifting entrepreneurs' reference points increases sales by approximately 68% compared to the control group. Additionally providing them with a goal setting training though diminishes this effect substantially. We cannot confirm any effects from business skills training by itself. These effects seem to be driven by three major mechanisms. Our role model intervention significantly increases savings over both time periods by 34%. These effects are driven by formal savings in bank accounts. Entrepreneurs that watched our video additionally increase their effort by working more days per week and having longer shop hours than entrepreneurs in the control group. Entrepreneurs in both the video group and the extended video group increase risky behavior as measured by an incentivized lottery question.

Our results expand on recent evidence by Campos et al. (2017) that evaluate the effectiveness of a personal initiative training versus the effects of basic business and

marketing skills. Transforming the mindsets of entrepreneurs towards greater innovativeness without providing additional business skills training or cash grants leads to significant increases of profits by 30%. These results support the idea that internal constraints might be much more important to entrepreneurial growth than previously thought. However, the training under examination in this study is a multi-component treatment that does not allow to evaluate through which channels these constraints operate, or whether a combination of psychological interventions, and business skills training might translate higher innovativeness into business success more effectively. Among internal constraints, aspirations have gained widespread attention as a potentially limiting factor in economic growth. From a theoretical perspective, the idea that aspirations and economic change might be related was first formalized by Ray (2006). Building upon this contribution, Genicot and Ray (2017) argue that individual aspirations are shaped by society-wide economic conditions and that these in turn affect individual investment incentives. Dalton et al. (2015) again show that aspirations failure is a consequence of poverty that might lead to a behavioral poverty trap. Bernard et al. (2014) present evidence that suggests a significant impact of aspirations on forward-looking behavior – an important predictor of business growth. By exposing study participants to potential role models, the authors find a positive, persistent effect on aspirations as well as a positive effect of aspirations on children's education.

This study adds to our understanding of constraints to micro-entrepreneurial business growth, particularly the importance of reference points and how to shift them. Without changing entrepreneurs' perspectives and business goals first, financial and human capital interventions are unlikely to yield positive returns.

3.2 Country Background: Mozambique

The randomized control trial used to answer the research questions examined in this paper was conducted in Mozambique. These research questions, namely those related to the impact of internal constraints on micro-enterprise development and to their interaction with the micro-entrepreneurs' general lack of business skills, are particularly relevant in conflict-ridden countries such as Mozambique. Recent decades of independence and civil wars, and ongoing conflict between the incumbent party and the opposition have not only constrained economic resources and access to education, but also impaired the population's trust in the future. Without entrepreneurs believing in economic growth and stability, it is unlikely that they will set themselves business objectives that foster investment and long run business growth – which necessarily require trading off current for future gains. Furthermore, a lack of positive examples and role models to follow is likely to restrain micro-entrepreneurs' higher aspiration levels, potentially leading to suboptimal business efforts and practices.

Mozambique became independent from the Portuguese colonization in 1975. In the preceding 10 years, there was an independence war and social unrest that disrupted the Mozambican economy. Since the end of this war with Portugal in 1975, the country has been led under a single-party, socialist regime. Two years after independence, the country's two major political parties began a very destructive civil war. Estimates suggest that up to one million people died as a consequence of war and five million civilians have been displaced. In 2013, the peace agreement signed in 1992 was broken when upheavals reemerged. Hundreds were killed and displaced, with tensions spiking in the north and center of the country in 2015 and 2016. Most of the Mozambican population, around 70% in 2016, live in rural areas from agriculture. These livelihoods are at constant risk due to recurring natural disasters such as floods. The increasing climate change is expected to lead to an increase in the occurrence of such disasters.

As a consequence of both conflict and natural disasters, the country faces high internal and international migration.[3] Most Mozambicans move to the largest Mozambican cities and to South Africa. Mozambique's capital, Maputo, is the country's most populous city. Around 9%, 2.7 million, of the Mozambican population live in the Maputo metropolitan area. There are few formal, salaried jobs. Most workers earn money through a mix of casual employment and petty businesses. The turnover in these businesses is high and workers regularly interrupt their business activities. Concentrated in local market clusters, these entrepreneurs face a high degree of competition. They have no formal business training, little education, and limited access to capital.

Supporting the development of such businesses is a major concern of the government. The Mozambican private sector is underdeveloped with only a few large firms[4] and an almost entirely absent SME sector. Most businesses are too small to pay taxes. Understanding the constraints holding micro-businesses in Mozambique back allows for government policies that foster SME development and economic growth. As self-employment is the only income source of many urban households, supporting business growth is also a poverty alleviation scheme.

3.3 Experimental Design and Implementation

To estimate both the effect of changes in reference points and potential complementarities of these with goal-setting and business skill training, we used a 2x3 factorial design, as illustrated in Figure 3.3.1.

[3]See Batista and Vicente (2018) for further evidence on the relationship between natural disasters and migration in Mozambique.

[4]See the Enterprise Mapping made by Sutton for the IGC.

Figure 3.3.1: 2X3 Experimental Design.

	PLACEBO VIDEO	VIDEO ONLY	VIDEO + GOALS
CONTROL	98 vendors	96 vendors	98 vendors
BUSINESS SKILLS	98 vendors	89 vendors	89 vendors

3.3.1 Interventions

3.3.1.1 Aspirations Video

The video showcases the successful career and business development of Sr. Tivane. He is a young entrepreneur that developed from a street vendor to a businessman with several shops and a grocery store spanning two floors. His shops are located in markets that are not included in our sample. None of the entrepreneurs that watched the video indicated that they knew his story before the video. The video shows several sequences of the neighborhood and the store of Sr. Tivane while a narrator describes his way of success. They are complemented with interview sequences with the owner himself, his wife, a development worker, and the country director of the *Associação NOVAFRICA para o Desenvolvimento Empresarial e Económico de Moçambique* (NOVAFRICA Association for Mozambican Business and Economic Development). The statements emphasize the success of Sr. Tivane as a businessman and potential role model. There was minimal framing on how to imitate his success. The video is explicit in the importance of ambitions to grow a business and get out of poverty.

Role Model Background and Pilot We piloted videos about four different entrepreneurs. The video about Sr. Tivane proved to be most successful in inspiring entrepreneurs. The pilot group identified with the role model. Much like the majority of our sample, Sr. Tivane has little education and no formal business training. He started his career as a flying vendor selling school products in the streets of Maputo.

He then managed to rent a stall in one of the markets and grew his business successively. At the same time, his business is sufficiently developed to mark a significant difference between our experimental sample and our role model. He has several employees that run his shops in urban markets in Maputo and the grocery store in his neighborhood.

Implementation and Aims The runtime of the video is 8 minutes – this seemed to be the approximate time span for which the treatment group remained engaged and attentive. The video was shown individually on tablets with headphones. We visited vendors during their business hours at their shops. The video was followed by a brief discussion of the video content with the trainer. The video was designed to encourage two types of change: First, it increases aspirations – forward-looking reference points – by providing information on the attainability of entrepreneurial success. Bernard et al. (2014) have shown that aspirations influence behavior and that they respond to interventions. These empirical results validate theoretical models about the role of aspirations for savings and investments (Genicot and Ray (2017)). Second, the video describes in detail the intrinsic motivation and ambitions of our role model. It increases self-efficiency by emphasizing the importance of own effort. The narrative tried to convince entrepreneurs that our role model's business success was not a result of external help or luck.

3.3.1.2 Goal Setting

We adapted the SMART goal setting approach to the context of micro-entrepreneurs to strengthen their goal setting behavior. The acronym defines a desirable goal as one that is specific, measurable, attributable, realistic, and time-sensitive. The trainers first explained the concept of a SMART goal with examples of fictional characters. Each entrepreneur was then asked to select a goal for their own business. The training then

followed the SMART scheme in specifying the objective and respective cost. Based on the overall cost and the responsible's monthly savings capacity the trainer helped define a financing model. If the initial goal was deemed unrealistic for the next year, the entrepreneur was asked to revise and choose a different goal. We left a hand-written copy of the goal achievement plan and a leaflet explaining the definition of SMART goals with the entrepreneur.

3.3.1.3 Placebo Video

We produced a short video in Portuguese about the Limpopo National Park. The purpose of this video is to ensure that any effects found on aspirations and business outcomes of individuals are not driven by the experience of watching a video itself.[5] The placebo video is similar in length to the aspirations video. It was displayed on tablets and with headphones as well.

3.3.1.4 Basic Skills Training

The business skills training is an adaptation of the rules-of-thumb approach by Drexler et al. (2014). Instead of providing entrepreneurs with complex accounting and financial literacy skills, the rules-of-thumb approach provides entrepreneurs with simple rules on how to run their business. We focus on four main lessons: First, the trainer explains the importance of separating business and household accounts. Keeping business money apart helps entrepreneurs calculate their profits and evaluate their business performance. A central mechanism, however, is nudging entrepreneurs into mental accounting. Mental accounting increases business savings[6] and investments, and thus subsequently business growth. The trainer then explains that money for different pur-

[5]Bernard et al. (2015) provide a detailed exposition of the importance of placebo treatments with video interventions.

[6]See Thaler (1999) for a detailed exposition on mental accounting.

poses should not only be stored separately but also safely. We provide each entrepreneur with two money pouches to increase adoption of these rules. The third rule asks entrepreneurs to establish a salary for themselves based on their needs and business income. The last rule allows entrepreneurs to borrow money from their business account for their household as long as it is paid back within seven days. The training was provided individually at the business. We left a leaflet with a summary of the training in Portuguese[7] and a flyer with the four rules of thumb with each entrepreneur after the training.

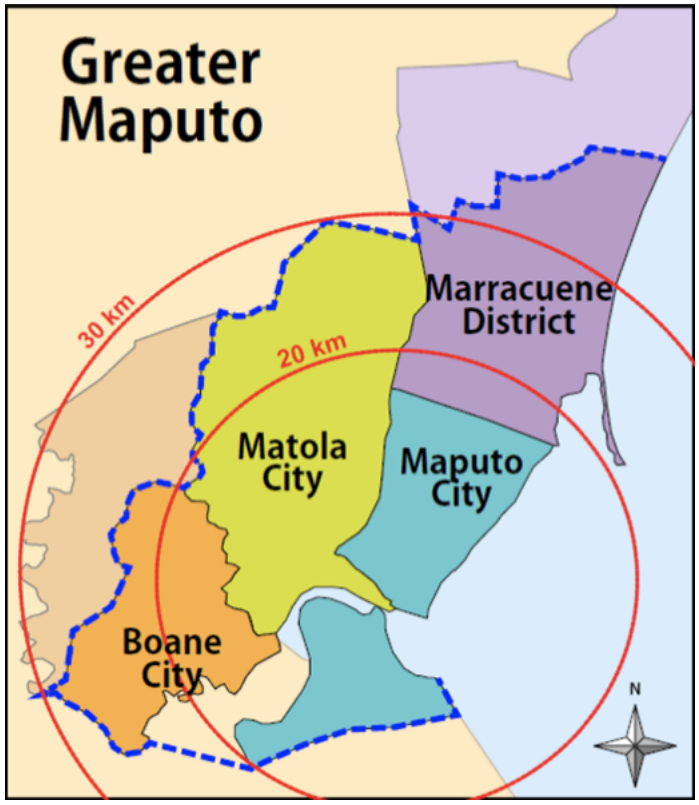
3.3.2 Sampling, data collection and experimental implementation

3.3.2.1 Sampling

We randomly selected about 600 micro-entrepreneurs in different markets to participate in our study. With no reliable administrative data on the universe of micro-entrepreneurs in the metropolitan area of Maputo, we set out to obtain census data for a representative sample of market vendors. In 2016, 82 markets were officially registered. We focused our sample on markets located in the cities of Maputo and Matola. We excluded markets located outside these city borders as depicted in Figure 3.3.2 and the island of Inhaca. We further excluded markets that were indicated on our administrative list and confirmed by our local RA team to have less than 100 vendors. Due to security concerns we also excluded two informal markets. The sampling strategy at this stage then followed neighborhood market clusters. In areas where we could not find the initial market or it had moved we proceeded with a census of market vendors in the new

[7]Over 94% of our sample indicate that they speak Portuguese. As Changana, the second most spoken language in the Maputo region, is mostly a spoken language but rarely written, we decided to provide our supporting material in one language only.

Figure 3.3.2: Maputo Metropolitan Area.



Source: JICA Report (2014)

local market. In total, we obtained a representative sample of 3.136 market vendors in 33 different markets. This represents approximately 76% of all markets within our sampling framework.

Most of these markets are organized in designated open-air areas. They serve the population in the respective neighborhood and supply groceries, clothes, household and construction goods. Vendors in these markets typically sell in structures similar to small stalls or on benches. The most advanced shops are covered and lockable, and have access to electricity. Other shops barely have a roof or walls, and vendors collect their products at the end of the day to take them home. We only sampled businesses with a designated vending area. Flying vendors with movable carts that switch between different markets are excluded.

For these 3.136 market vendors we obtained census data on individual characteristics relevant to our interventions. During the census we recorded the name of the shop owner or managing staff, their age, gender, nationality, and whether they had basic literacy. We furthermore obtained data on their perspective of remaining active in the respective market during the next year, their consent to participate in our study, as well as the location of the shop and the shop responsible's contact details.

We applied the following exclusion restrictions: Due to possible identification problems with our Mozambican role model, we excluded all foreign market vendors; All market vendors with a business horizon of less than a year and more than 50 years old[8] were excluded from the sample; Due to business type homogeneity, we excluded all fruit/vegetable sellers, restaurants, bars, and kiosks, as well as any shops selling illegal products, traditional medicine, or wholesale merchants.

Applying these eligibility criteria, we obtained a representative subsample of 788 market vendors in 32 markets. We proceeded by randomly selecting 624 market vendors in 29 markets for the baseline survey. Three markets were excluded as less than six vendors meeting our requirements operated on those markets. During our treatment visit we were able to located 568 vendors, 95% of our sample, again. Treatment allocation was randomized on the individual unit and stratified by market clusters.

3.3.2.2 Experimental implementation

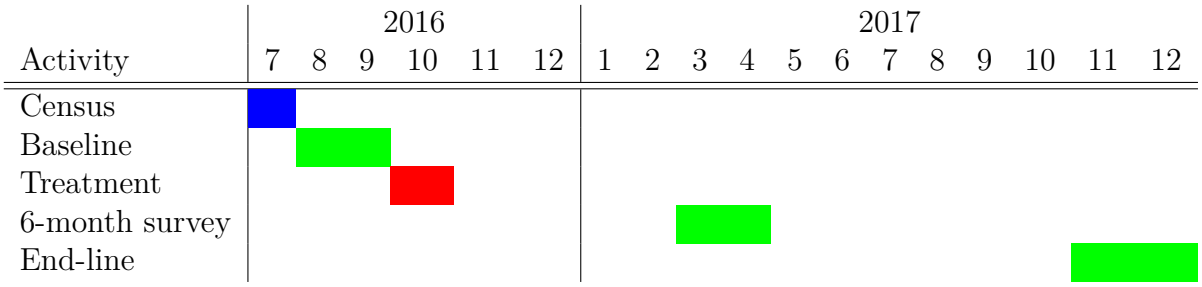
As illustrated in Figure 3.3.3 the baseline survey took place one month after the census.

We randomized entrepreneurs by computerized assignment conditional on a completed baseline survey.[9] We completed baseline surveys for 600 individuals. Treatment

[8]Between 2010 and 2015, life expectancy at birth in Mozambique was 56 years according to the United Nations Population Division report "World Population Prospects" (2017).

[9]Around 3.8% of the interviews conducted at baseline were not completed as entrepreneurs interrupted the interview and were not willing to continue. One entrepreneur had to be excluded from the sample due to religious reasons that did not allow him to participate in the dictator games or watch a video. Given that all entrepreneurs either saw the role model video or the placebo he could not

Figure 3.3.3: Project Timeline.



assignment was stratified by market clusters. The training visits took place in the month after the completion of all baseline surveys. We ran end-line surveys 6 months and 13-14 months after the training visits.

Balance We could find 94.6% of our baseline sample during the treatment visit. This resulted in 16.9% assignment to the role model video only, 17.25% to the role model video with goal setting training, 17.25% to the rules-of-thumb training only, 15.67% to the role model video and rules-of-thumb training, 15.67% to the group that received all components, and 17.25% to neither. Figure 3.3.1 illustrates this distribution. Table 3.1 reports balance tests for each treatment for select covariates.

3.3.2.3 Data Collection and Measurement Strategies

We tried to survey each firm owner four times: 1) at baseline before the training visit, 2) during the training visit for additional baseline data on business outcomes and immediate effects on psychometric indicators, 3) "short-run" surveys 6 months after the training, and 4) at end-line 13-14 months after the training. Subjects also conducted a set of incentivized dictator and risk aversion games after the baseline and "short-run" survey. For the following analysis we restrict our sample to subjects that were present during the training visit. Of those, we collected data on 86% at the 6-month visit and

participate in the study.

Table 3.1: Descriptive Statistics and Randomization Balance for Select Covariates.

	Means, full sample						
Variables	Control	Video	Video & Goals	Business Skills	Video * Business Skills	Video & Goals Business Skills	Joint F-stat p-value
	Demographics						
Age	33.03	1.02	0.63	-0.76	-0.12	-0.14	0.89
		(1.34)	(1.3)	(1.34)	(1.32)	(1.33)	
Female	0.45	0.02	-0.05	-0.06	0.00	-0.04	0.80
		(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	
Education	8.06	-0.07	0.22	0.11	-0.23	0.03	0.93
		(0.43)	(0.42)	(0.45)	(0.43)	(0.41)	
Formal Training	0.11	0.03	-0.01	-0.02	-0.06	-0.04	0.29
		(0.05)	(0.04)	(0.04)	(0.04)	(0.04)	
Household Size	6.02	-0.40	-0.44	-0.28	0.29	-0.28	0.36
		(0.33)	(0.32)	(0.36)	(0.38)	(0.33)	
	Business Characteristics						
Employees	0.32	0.00	0.03	-0.06	-0.05	0.01	0.95
		(0.12)	(0.11)	(0.10)	(0.12)	(0.11)	
Workdays/Week	6.37	-0.11	-0.20	-0.07	-0.22	-0.02	0.53
		(0.14)	(0.13)	(0.11)	(0.16)	(0.09)	
Workhours/Day	9.8	-0.01	-0.30	-0.07	-0.25	0.02	0.76
		(0.26)	(0.25)	(0.23)	(0.27)	(0.24)	
	Lagged Outcomes						
Sales (median)	1,873.30	-263.07	-443.13	57.16	185.70	553.19	0.31
		(418.80)	(339.54)	(480.37)	(498.92)	(578.73)	
Profit (median)	1,210.02	176.24	58.50	13.69	584.05	982.08*	0.47
		(433.14)	(371.34)	(416.93)	(524.18)	(596.65)	

Note: Standard errors of the differences reported in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1%.

80% at the end-line visit.

On average participants are 33 years old. Almost half of our sample is composed of women. Vendors spent approximately 8 years in school, providing them with less than high-school education. Only eleven percent of participants have ever received formal training in their business area and households are composed of six members on average. Two and a half of these household members are dependents - this includes biological children and other minors living in the same household. Vendors that watched the video and received business skills training are slightly more likely to live in households with many children.

Businesses have on average 0.3 employees. Business owners are personally in charge of their stalls 6.4 days during a normal week. Each day they work almost 10 hours. This effort translates into 1.873Mts in sales (this is equivalent to US$12 applying the 2016 exchange rate average at the time of the baseline survey). The reported sales are the median sales during the week prior to our baseline visit. This control for outlier sales days. Median profits are slightly higher for the treatment group that received all components of our intervention.

3.3.3 Key Outcomes

We focus our analysis on four main groups of outcomes: one final outcome group – business outcomes – and three intermediate outcome groups that drive final outcomes. The core hypotheses and potential mechanisms were outlined in a 2015 International Growth Center proposal. We report treatment effects for all time periods where the respective measure is available. We also estimate the pooled treatment effect over the "short-run" and end-line survey. This strategy allows us to understand the dynamic effects of our trainings and estimate more precise effects by increasing statistical power when more than one post-training period is available.

Business outcomes focus on an entrepreneur's sales and profits. We asked subjects to indicate their sales for the week prior to the interview by day and product category. This strategy is less prone to recalling error than asking about an entrepreneur's total sales over the last week. Expenditures are reported following the same strategy. We calculate profits as the difference between sales and expenditures. Our results are robust to different sales measures. In Table 3.2 we report treatment effects for sales and profits for the last three days prior to the interview date and self-reported profits[10] over the last month.

The main intermediate outcome group is changes in forward-looking behavior. We asked subjects about their total savings as well as savings amounts that are stored with others, at bank accounts, and at home. We also asked subjects whether they had invested any money in their business and if so how much. We complement this data with self-reports on having business objectives and goals, and an incentivized risk aversion measure.

We measure effort as the number of days a subject is personally taking care of the shop. As this indicator is self-reported, we complement it with the total opening hours of the shop over the week. We interpret indicators on client management as further measures of individual effort. Our trainings could furthermore affect business outcomes by incentivizing subjects to obtain skills training. We estimate effects on changes in business practices in three dimensions: the likelihood on keeping books about sales, clients buying on credit, and product inventory; computing business measures; and whether subjects took out any business money for personal expenses.

[10]The exact phrasing of the question (translated from Portuguese) was "How much of your sales did you have left over after paying all expenses of your business last month?". Subjects that did not know the exact amount were asked to indicate one out of seven profit bins. We use the median of each bin as profit value whenever the subject did not indicate the exact value. Our estimates are robust to estimating treatment effects on profit bins rather than absolute values.

3.4 Identification Strategy

Given our experimental design, we estimate the coefficients of interest as average treatment effects (ATE) using the following Difference-in-Differences specification:

$$Y_{it} = \theta + \alpha T_{it} + \gamma Post + \beta[T_{it} \times Post] + \delta X_{it} + \epsilon_{it} \tag{3.4.1}$$

Where Y_{it} is the outcome of interest for individual i at survey round t, and X_{it} is a vector of controls that includes market fixed effects and survey round fixed effects - where the latter apply only for the pooled sample specification. Y_{it} is the pooled effect of the 6-month and the end-line survey values in those specifications where both time periods are available.

$$\beta = [\beta_V \beta_{VG} \beta_S \beta_{VS} \beta_{VGS}] \tag{3.4.2}$$

is the vector of treatment effects, where V denotes the Video intervention, G stands for the goal-setting intervention, and S represents the skill-training intervention.

$$T_{it} = [(Video_{it})(VideoGoal_{it})(Skills_{it})(Video_{it} \times Skills_{it})(VideoGoal_{it} \times Skills_{it})] \tag{3.4.3}$$

is a vector of dummy variables representing (random) assignment to the three main treatment arms. In the full specification, interaction terms are also included.

For all outcomes variables, we report the main effects and the full specification separately. The main effects are the ATE from random assignment to watching the aspirations video; watching the video and receiving the goal setting treatment; and rules-of-thumb training. The interaction term estimates the additional effect of the rules-of-thumb training on the effect of watching the video (or watching the video and

receive goal setting treatment). Robust standard errors are clustered at the individual level - our unit of randomization.[11] To reduce sensitivity to outliers, we winsorized outcome data on sales, profits, and savings at the 1st and 99th percentile. We estimate 2.6.1 using OLS for continuous dependent variables, and Probit for all binary outcomes.

3.5 Empirical Results

3.5.1 Firm Performance

We measure firm performance using three distinct outcome measures: self-reported sales, calculated profits, and self-reported profits. Table 3.2 reports the impact on sales of the three treatment arms in columns (1), (3), and (5). Effect size is shown in Meticais – the local currency.

Table 3.2: Treatment Effects on Firm Performance.

(a) Pooled Effects.

Dependent Variable:		Sales		Profit	
		ATE	se	ATE	se
		(1)	(2)	(3)	(4)
video – β_V		1.207***	(0.405)	0.888**	(0.373)
video & goal – β_{VG}		0.755*	(0.388)	0.367	(0.371)
skills training – β_S		0.428	(0.405)	0.102	(0.383)
video * skills training – β_{VS}		-1.198**	(0.603)	-1.002*	(0.518)
video & goal * skills training – β_{VGS}		-0.641	(0.615)	0.149	(0.506)
mean dep. variable post-treatment		7.674		7.249	
$\beta_{VG} - \beta_V = 0$	F-stat	0.307		0.099	
$\beta_V + \beta_S + \beta_{VS} = 0$	F-stat	0.265		0.977	
$\beta_{VG} + \beta_S + \beta_{VGS} = 0$	F-stat	0.208		0.116	
r-squared adjusted		0.032		0.030	
number of observations (cluster)		1,403		892	

[11] We follow Abadie et al. (2017) in our reasoning for clustering standard errors at the individual level.

In the short run, sales increased among those that received either of the two trainings focused on changes in reference points. Surprisingly, the more comprehensive intervention that includes goal setting training yields smaller effects than the simple intervention. Additionally training subjects on goal setting and establishing a business goal decreases the positive effect from improved aspirations on sales. The same conclusions hold when pooling over both survey rounds with no improvements over precision for winsorized outcomes. Changing subjects' reference points increases sales by approximately 40% compared to the control group that watched a placebo video. The impact of our role model video on sales is significant at the 5% level. Our results on the impact of our video alone are robust to the inclusion of interaction terms. We discuss the impact of the interaction terms in detail in Section 3.7.1.

Table 3.2: Treatment Effects on Firm Performance.

(b) 6-months Effects.

Dependent Variable:		Sales		Profit	
		ATE	se	ATE	se
		(1)	(2)	(3)	(4)
video – β_V		1.362***	(0.511)	0.460	(0.513)
video & goal – β_{VG}		0.921*	(0.518)	-0.137	(0.521)
skills training – β_S		0.567	(0.499)	-0.413	(0.522)
video * skills training – β_{VS}		-1.352*	(0.728)	-0.670	(0.734)
video & goal * skills training – β_{VGS}		-0.461	(0.715)	0.647	(0.715)
mean dep. variable post-treatment		7.706		7.525	
$\beta_{VG} - \beta_V = 0$	F-stat	0.413		0.204	
$\beta_V + \beta_S + \beta_{VS} = 0$	F-stat	0.252		0.266	
$\beta_{VG} + \beta_S + \beta_{VGS} = 0$	F-stat	0.033		0.859	
r-squared adjusted		0.030		0.035	
number of observations (cluster)		985		466	

Table 3.2: Treatment Effects on Firm Performance.

(c) One-year Effects.

Dependent Variable:		Sales		Profit	
		ATE	se	ATE	se
		(1)	(2)	(3)	(4)
video – β_V		1.057**	(0.441)	1.217**	(0.488)
video & goal – β_{VG}		0.569	(0.416)	0.904*	(0.499)
skills training – β_S		0.289	(0.489)	0.664	(0.543)
video * skills training – β_{VS}		-1.021	(0.696)	-1.337*	(0.711)
video & goal * skills training – β_{VGS}		-0.852	(0.734)	-0.442	(0.705)
mean dep. variable post-treatment		7.870		6.974	
$\beta_{VG} - \beta_V = 0$	F-stat	0.294		0.450	
$\beta_V + \beta_S + \beta_{VS} = 0$	F-stat	0.473		0.325	
$\beta_{VG} + \beta_S + \beta_{VGS} = 0$	F-stat	0.991		0.034	
r-squared adjusted		0.030		0.037	
number of observations (cluster)		922		426	

Notes: Outcomes are inverse hyperbolic sine transformations of sales over the last two days prior to interview and self-reported profits as described in the text. All regressions on sales are difference-in-difference estimations. Regressions on profit are OLS as baseline measures are not available. Survey round fixed effects are included for all pooled regressions. Columns (1) and (3) display ATEs of each intervention. βV is the treatment effect for all individuals that have seen the video but not received goal setting training. βVG is the treatment effect for all individuals that have seen the video and received goal setting training. βS equals one for all individuals that received rules-of-thumb training. βVS determines the additional effect of receiving skills training on watching the video for individuals that only watched the video. βVGS is the additional effect of receiving skills training on watching the video and receiving goal setting training. P-values of the respective linear hypothesis tests for joint effects are reported below coefficients. Columns (2) and (4) show standard errors in parentheses. Standard errors are clustered at the individual level. * significant at 10%; ** significant at 5%; *** significant at 1%.

As we did not obtain data on self-reported profits at baseline, the results in Table 3.2 provide OLS estimates for post-treatment periods only. Although not statistically significant, the video raises self-reported profits in the short-run. The impact for those that also established a goal is close to zero. We find positive impacts of the video

significant at the 5% level on profits in the pooled sample. Those that watched the video increased profits by almost 26% compared to the control group. We do not find significant impacts of the enhanced video intervention or the rules-of-thumb training on profits.

These results are puzzling as the general opinion tends to favor holistic entrepreneurship programs over small nudges. It is also surprising that our effects increase over time instead of fading out. Our initial hypothesis stated that goal setting training supports micro-entrepreneurs in achieving the goals they set themselves. By ensuring that goals are realistic and developing a plan on how to achieve them, subjects should find it easier to translate higher aspirations into business success. A potential explanation for our findings could be constrained reference points. By enhancing subjects' focus on one specific goal we might limit changes in forward-looking behavior towards this goal. To see this, we assess forward-looking behavior in three ways. We estimate dynamic treatment effects on savings, and specifically types of savings. Given the heterogeneity of productive investments by business type we estimate the impact on the likelihood of investments in the last six months and the invested amount.

3.5.2 Forward-Looking Behavior

Table 3.3 reports treatment effects on intermediary outcomes related to forward-looking behavior. For continuous variables we provide difference-in-differences estimates with standard errors clustered at the unit of randomization, the individual. For design purposes we collected end-line data only for our main outcome variables of interest. We estimate probit difference-in-differences effects for binary outcome measures.

Table 3.3: Treatment Effects on Forward-Looking Behavior.

Dependent Variable:		Bank Savings		Having a Goal		Sales Objective	
		ATE	se	ATE	se	ATE	se
		(1)	(2)	(3)	(4)	(5)	(6)
video – β_V		8,156.301**	(3,899.530)	0.139*	(0.084)	0.153*	(0.079)
video & goal – β_{VG}		8,689.668***	(3,345.864)	0.050	(0.083)	-0.049	(0.083)
skills training – β_S		3,083.474	(4,068.193)	-0.012	(0.080)	-0.026	(0.084)
video * skills training – β_{VS}		-1,334.998	(5,350.114)	-0.093	(0.112)	-0.187	(0.115)
video & goal * skills training – β_{VGS}		-1,830.591	(4,568.492)	-0.033	(0.111)	0.015	(0.117)
mean dep. variable post-treatment		7,308.40		0.805		0.439	
$\beta_{VG} - \beta_V = 0$	F-stat p-value	0.829		0.258		0.009	
$\beta_V + \beta_S + \beta_{VS} = 0$	F-stat p-value	0.014		0.683		0.472	
$\beta_{VG} + \beta_S + \beta_{VGS} = 0$	F-stat p-value	0.004		0.956		0.478	
r-squared adjusted		0.048		0.023		0.096	
number of observations (cluster)		1,003		963		414	

Notes: Regressions on bank savings and having a goal are difference-in-difference estimations. Estimates are only available for baseline and 6-month survey. Coefficient of "Having a goal" are based on Linear Probability Model. "Having a sales objective" is only available for the 6-month survey. Estimates are OLS coefficients of a Linear Probability Model. Column (1), (3), and (4) display ATEs of each intervention. βV is the treatment effect for all individuals that have seen the video but not received goal setting training. βVG is the treatment effect for all individuals that have seen the video and received goal setting training. βS equals one for all individuals that received rules-of-thumb training. βVS determines the additional effect of receiving skills training on watching the video for individuals that only watched the video. βVGS is the additional effect of receiving skills training on watching the video and receiving goal setting training. P-values of the respective linear hypothesis tests for joint effects are reported below coefficients. Columns (2) and (4) show standard errors in parentheses. Standard errors are clustered at the individual level. * significant at 10%; ** significant at 5%; *** significant at 1%.

3.5.2.1 Savings

We report the total amount of savings in Meticais in bank accounts in Column (1) of Table 3.3. In the short-run, increases in aspirations lead to increases in savings. These effects are significant at the 5% and the 1% level for the video only and the enhanced video group, respectively. The amount saved is higher for those that watched the video and also set a specific goal. Watching the video only increases savings by 112% compared to the control group after 6 months. Those that also set a specific goal have 119% more savings than the control group. The positive impact of rules-of-thumb training on savings is not distinguishable from zero.

3.5.2.2 Forward-Looking Business Practices

Business Goals and Sales Objective We also ask subjects if they have established a business goal for the next six months. Respondents were free to indicate any type and number of goals they wanted. We assess the impact of our interventions on the likelihood of having established any business goal. Results are shown in Column (3) of Table 3.3. Those that watched the video are more likely to have a goal for their business and a sales objective.

3.5.3 Effort

Higher business ambitions, nudged by the video, might also trigger increases in effort. We assess the impact of aspiration changes on effort on two dimensions: working hours and shop opening hours.

3.5.3.1 Working Hours

We collected data on two measures of effort in relation to working hours. First, we asked subjects to indicate how many days per week they personally take care of their

Table 3.4: Treatment Effects on Effort.

(a) Pooled Effects.

Dependent Variable:		Workdays per Week	
		ATE	se
		(1)	(2)
video – β_V		0.313**	(0.147)
video & goal – β_{VG}		0.105	(0.130)
skills training – β_S		0.150	(0.127)
video * skills training – β_{VS}		-0.101	(0.257)
video & goal * skills training – β_{VGS}		-0.185	(0.202)
mean dep. variable post-treatment		6.277	
$\beta_{VG} - \beta_V = 0$	F-stat p-value	0.246	
$\beta_V + \beta_S + \beta_{VS} = 0$	F-stat p-value	0.052	
$\beta_{VG} + \beta_S + \beta_{VGS} = 0$	F-stat p-value	0.560	
r-squared adjusted		0.006	
number of observations (cluster)		1,470	

business. As we cannot assess the quality of effort during business hours, we focus on the extensive margin of work effort. Our results are reported in Table 3.4. At the short-run, subjects in either aspirations intervention group significantly increase their effort after controlling for interaction effects of treatments. Subjects in both groups work around 0.3 days more (significant at the 5% level) than the control group. After one year, this effect fades off for subjects that received goal setting treatment. The impact on their effort level though equally strong in the beginning, is short lived. Those that only watched the video maintain their higher effort level at approximately the same magnitude even one year after the intervention. This effect can be precisely estimated at the 5% level (10% level after including interaction terms).

Table 3.4: Treatment Effects on Effort.

(b) 6-months Effects.

Dependent Variable:		Workdays per Week		Shop Hours	
		ATE	se	ATE	se
		(1)	(2)	(3)	(4)
video – β_V		0.317**	(0.157)	392.845**	(154.935)
video & goal – β_{VG}		0.277**	(0.129)	360.097**	(153.362)
skills training – β_S		0.201	(0.143)	182.225	(162.044)
video * skills training – β_{VS}		-0.218	(0.283)	-279.454	(259.478)
video & goal * skills training – β_{VGS}		-0.307	(0.213)	-228.291	(231.064)
mean dep. variable post-treatment		6.299		6,245.374	
$\beta_{VG} - \beta_V = 0$	F-stat p-value	0.817		0.834	
$\beta_V + \beta_S + \beta_{VS} = 0$	F-stat p-value	0.166		0.143	
$\beta_{VG} + \beta_S + \beta_{VGS} = 0$	F-stat p-value	0.222		0.054	
r-squared adjusted		-0.008		0.124	
number of observations (cluster)		1,037		1,045	

Table 3.4: Treatment Effects on Effort.

(c) One-year Effects.

Dependent Variable:		Workdays per Week	
		ATE	se
		(1)	(2)
video – β_V		0.310*	(0.168)
video & goal – β_{VG}		-0.089	(0.185)
skills training – β_S		0.103	(0.158)
video * skills training – β_{VS}		0.031	(0.279)
video & goal * skills training – β_{VGS}		-0.054	(0.269)
mean dep. variable post-treatment		6.310	
$\beta_{VG} - \beta_V = 0$	F-stat p-value	0.073	
$\beta_V + \beta_S + \beta_{VS} = 0$	F-stat p-value	0.022	
$\beta_{VG} + \beta_S + \beta_{VGS} = 0$	F-stat p-value	0.812	
r-squared adjusted		0.005	
number of observations (cluster)		983	

Notes: All regressions are difference-in-difference estimations. Survey round fixed effects are included for all pooled regressions. Column (1) displays ATEs of each intervention. βV is the treatment effect for all individuals that have seen the video but not received goal setting training. βVG is the treatment effect for all individuals that have seen the video and received goal setting training. βS equals one for all individuals that received rules-of-thumb training. βVS determines the additional effect of receiving skills training on watching the video for individuals that only watched the video. βVGS is the additional effect of receiving skills training on watching the video and receiving goal setting training. P-values of the respective linear hypothesis tests for joint effects are reported below coefficients. Columns (2) and (4) show standard errors in parentheses. Standard errors are clustered at the individual level. * significant at 10%; ** significant at 5%; *** significant at 1%.

This measure is self-reported and might thus give reason to concerns if measurement error for this outcome is correlated with treatment status. We confirm our results with a less salient measure on business hours. For each day of the week we collect data on shop opening and closing hours. We then compute the total amount of hours (in minutes) the business is operating. The results on this exercise are reported in column (3) of Table 3.4b. Our estimates confirm our previous results. Those that received

any of our aspirations treatments work approximately 4.2 hours more than the control group. These effects are significant at the 5% level. Controlling for interaction terms those that watched the video only work about 6.5 hours more, and the second group 6 hours more than the control group (both significant at the 5% level).

3.5.4 Business Practices

A further explanation for the positive impact of role models on firm performance could be improved business skills. Higher aspirations could encourage entrepreneurs to seek out better skills. If so, we should observe positive effects of our interventions on business practices. We estimate effects for a family of business practices measures that are covered in traditional entrepreneurship programs.

3.5.4.1 Bookkeeping

Columns (1) – (6) in Table 3.5 show short-term effects on keeping track of sales, clients that buy on credit, and product inventory. Those that watched the video are more likely (significant at 5% level) to have books on clients that buy on credit and their product inventory. Those that watched the video and received goal setting treatment do not change their bookkeeping practices.

Table 3.5: Treatment Effects on Tracking Business Measures

Dependent Variable:		Sales book		Clients that bought on credit		Inventory	
		ATE	se	ATE	se	ATE	se
		(1)	(2)	(3)	(4)	(5)	(6)
video – β_V		0.076	(0.085)	0.200**	(0.087)	0.176**	(0.084)
video & goal – β_{VG}		0.010	(0.091)	0.109	(0.093)	-0.100	(0.086)
skills training – β_S		0.133	(0.090)	0.098	(0.091)	0.105	(0.090)
video * skills training – β_{VS}		-0.125	(0.122)	-0.207	(0.129)	-0.254**	(0.124)
video & goal * skills training – β_{VGS}		0.004	(0.131)	-0.069	(0.137)	0.157	(0.127)
mean dep. variable post-treatment		0.225		0.266		0.220	
$\beta_{VG} - \beta_V = 0$	F-stat p-value	0.439		0.332		0.001	
$\beta_V + \beta_S + \beta_{VS} = 0$	F-stat p-value	0.343		0.312		0.751	
$\beta_{VG} + \beta_S + \beta_{VGS} = 0$	F-stat p-value	0.127		0.155		0.070	
r-squared adjusted		0.052		0.024		0.070	
number of observations (cluster)		1,020		1,028		1,033	

Notes: All regressions are difference-in-difference estimations for Linear Probability Models. Estimates are only available for baseline and 6-month survey. Columns (1), (3), and (5) display ATEs of each intervention. βV is the treatment effect for all individuals that have seen the video but not received goal setting training. βVG is the treatment effect for all individuals that have seen the video and received goal setting training. βS equals one for all individuals that received rules-of-thumb training. βVS determines the additional effect of receiving skills training on watching the video for individuals that only watched the video. βVGS is the additional effect of receiving skills training on watching the video and receiving goal setting training. P-values of the respective linear hypothesis tests for joint effects are reported below coefficients. Columns (2) and (4) show standard errors in parentheses. Standard errors are clustered at the individual level. * significant at 10%; ** significant at 5%; *** significant at 1%.

3.5.4.2 Taking Business Money

The rules-of-thumb training focuses on the importance of separating business and household accounts. To assess whether subjects adopted the taught practices we collect self-reported data on whether entrepreneurs took any money from the business to pay household expenses during the last six months. This framing is less salient than the direct question of whether subjects separate accounts. Six months after the training, subjects in the rules-of-thumb group are less likely to have taken money from the business to pay household expenses. This effect is statistically significant at the 1% level after controlling for interaction effects. None of our aspirations treatments have an impact on taking business money.

Table 3.6: Treatment Effects on Adoption of Skills Training.

Dependent Variable:		Take money from business	
		ATE	se
		(1)	(2)
video – β_V		-0.070	(0.060)
video & goal – β_{VG}		-0.071	(0.061)
skills training – β_S		-0.186***	(0.069)
video * skills training – β_{VS}		0.227**	(0.094)
video & goal * skills training – β_{VGS}		0.110	(0.099)
mean dep. variable post-treatment		0.863	
$\beta_{VG} - \beta_V = 0$	F-stat	0.987	
$\beta_V + \beta_S + \beta_{VS} = 0$	F-stat	0.653	
$\beta_{VG} + \beta_S + \beta_{VGS} = 0$	F-stat	0.035	
r-squared adjusted		0.010	
number of observations (cluster)		454	

Notes: Regression is OLS estimates of a Linear Probability Model. Estimates are only available for 6-month survey. Column (1) displays ATEs of each intervention. βV is the treatment effect for all individuals that have seen the video but not received goal setting training. βVG is the treatment effect for all individuals that have seen the video and received goal setting training. βS equals one for all individuals that received rules-of-thumb training. βVS determines the additional effect of receiving skills training on watching the video for individuals that only watched the video. βVGS is the additional effect of receiving skills training on watching the video and receiving goal setting training. P-values of the respective linear hypothesis tests for joint effects are reported below coefficients. Columns (2) and (4) show standard errors in parentheses. Standard errors are clustered at the individual level. * significant at 10%; ** significant at 5%; *** significant at 1%.

3.6 Potential Threats to identification

Self-reported Business Data Self-reported measures of sales and profits are a threat to identification if measurement error is correlated with treatment status. In our design, sales measures might be prone to measurement error in two ways: first, treated subjects might become better in tracking their sales due to an increased skill set; and second, treated subjects might misreport sales out of compliance desires. In

the absence of administrative data, we developed a sales measure to validate survey measures of business outcomes through actual sales observation and recording. Section 2.6 reports the approach and measure in detail. Self-reported business data is strongly correlated with observed sales. There is no systematic difference in this correlation across treatments. It is thus unlikely that our results are driven by a compliance bias and motivational lying.

Spillovers Our estimates might be biased in the presence of spillovers. We do not have the research design to estimate equilibrium effects from an increase in competition. Nevertheless, the majority of our sample operates in large market clusters where equilibrium effects are unlikely to drive our results. The mechanisms explaining treatment effects on sales are furthermore largely independent from equilibrium effects. Spillovers between treatment arms where subjects learn from or inspire each other would lead us to underestimate our effects. As we have no reliable data on social networks within these markets we cannot estimate this effect. The two effects, if present, should cancel each other out.

3.7 Is skills training a substitute or a complement to aspirations?

Our experimental design allows us to estimate complementarity effects of skills training on changes in reference points. Given the sample size of our study we are only powered to detect large effects. As additionally providing skills training is substantially costlier than our aspirations interventions alone, we believe this exercise is still worthwhile. If complementarity effects are small it would not be cost-efficient to provide such additional trainings. The respective results are reported in the same tables as

the main effects. The interaction terms can be interpreted as the additional impact of rules-of-thumb training on the respective aspirations treatment.

3.7.1 Firm Performance

Effects on firm performance are shown in Table 3.2. Additionally training subjects on rules-of-thumb has a negative impact on sales. Effects are significant at the 5% level on the short-run only for those that also received goal setting treatment. The negative impact is substantial. It offsets the gains from the video and goal setting treatment entirely (though not statistically significant).

Impacts on self-reported profits are slightly more positive. The additional impact from skills training in the short-run is not distinguishable from zero. One year after the training, additional rules-of-thumb training significantly (5% level) reduces profits for those that watched the video only. The additional effect of the rules-of-thumb training is not larger than the effect of the video and the skills training combined.

3.7.2 Effort

The general treatment effect on effort is reduced by additional skills training. Those subjects that received training on rules-of-thumb exert consistently less effort than those that have watched the video only. Similarly, those that received skills and goal setting training work less hours than those that did not learn about the rules-of-thumb (not statistically significant).

3.7.3 Business Practices

Our results in Table 3.5 provide evidence that subjects with skills training are less likely to keep track about clients buying on credit and their product inventory than

their peers that watched the video only.

These results suggest that additional skills training is a substitute to aspirations interventions rather than a complement. A potential explanation could be that separating accounts is less costly than increasing aspirations. Defining and executing a plan to fulfill personal ambitions requires a higher level of cognitive effort. In our setting the impact of rules-of-thumb is limited. Focusing cognitive effort in applying the learned rules rather than translating ambitions into actions can potentially explain the negative impact of additional skills training.

3.8 Discussion and Implications

Increasing the productivity of micro-entrepreneurs is a major challenge for policy makers, international institutions, and NGOs. Mozambique is currently highly dependent on its coal and aluminum exports, and private sector development is still lagging behind. Existing evidence has provided little guidance for stakeholders on how a transformation of micro-enterprises into productive SMEs can be achieved. This paper provides novel insights on the importance of reference points for business success. Increasing aspirations and shifting reference points can substantially increase profitability of enterprises that have formerly operated on low profit margins.

We estimate the impact of video exposure to a role model, additional goal setting treatment, and rules-of-thumb on sales, calculated profits, and self-reported profits. Nudging a change in reference points through a video intervention increases sales by 40% and profits by 29% compared to a control group. Effects are observable both in the short-run and over the course of one year. This effect is driven by statistically significant changes in forward-looking behavior and effort. Subjects that watched the video save more, are more likely to have sales and business goals, work more hours and

show improved client management. There is some evidence that those subjects improve bookkeeping on a subset of dimensions but not in computing key business measures.

In the short-run, we find similar results on effort for those that additionally received goal setting training. These effects, on the contrary to the group that watched the video only, disappear over time. Although the positive effects on savings are similar for this group, they translate into higher business outcomes in the short-run only.

We find no or negative effects of rules-of-thumb training on any of these outcomes. Finally, we examine the joint effect of the different interventions and their complementarities. Providing skills training crowds out the positive effects of changing reference points for most of our outcomes. A possible explanation could be the cognitive cost of realizing higher ambitions compared to implementing simple rules-of-thumb.

Our evidence supports the view that internal constraints limit micro-entrepreneurs in their business ambitions and from exploiting positive returns to human capital interventions. While much of the literature has focused on the theoretical concepts behind a behavioral firm and the role of reference points for savings decisions in insurance contexts, we are the first to document experimentally the interaction between reference points and business success.

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Part I

Appendix A

Table A.1: Difference Between Male and Female Vendors for Select Covariates - Full Sample.

	Male			Female			t-test
	N	mean	se	N	mean	se	difference
	(1)	(2)	(3)	(4)	(5)	(6)	(10)
Baseline covariate							
Age	185	31.924	[0.603]	128	36.195	[0.849]	-4.271***
Years of schooling	186	8.285	[0.195]	129	7.698	[0.285]	0.587*
Formal business training	186	0.113	[0.023]	129	0.085	[0.025]	0.028
Household size	186	5.941	[0.197]	129	5.798	[0.218]	0.142
Years in business	186	7.349	[0.468]	129	8.636	[0.675]	-1.286
Risk preferences	180	5.189	[0.267]	125	5.464	[0.337]	-0.275
Investment (past 6 months)	128	0.516	[0.044]	88	0.511	[0.054]	0.004
Hours worked per day	184	9.984	[0.110]	128	9.383	[0.142]	0.601***
Dictator game contribution	184	4.375	[0.212]	127	4.425	[0.334]	-0.050
Sales last two days	170	3596.382	[436.537]	114	3465.377	[501.213]	131.005
Profit last two days	167	1343.054	[431.136]	112	1672.071	[485.200]	-329.018

Notes: This table shows summary statistics for female- and male-owned firms. Reported is a selection of covariates. Sales, profits, and hours and days 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 A.2: Ranking Impact on Effort - winsorized outcomes.

Outcome	Difference-in-difference regression					
	Hours worked			Days worked		
	ATE	se	q-value	ATE	se	q-value
	(1)	(2)	(3)	(4)	(5)	(6)
Panel B: Short-term impact						
Ranking	0.874**	(0.344)	[0.0198]	0.203	(0.136)	[0.1089]
Positive Signal	0.874**	(0.343)	[0.0198]	0.310*	(0.159)	[0.0297]
Ranking × Positive Signal	-1.377***	(0.433)	[0.0000]	-0.182	(0.214)	[0.3663]
Control mean - bottom	9.406			6.188		
Control mean - top	10.439			6.358		
Joint test - p-value	0.0562			0.8997		
adjusted r-squared	0.065			0.013		
Observations (cluster)	602 (315)			601 (315)		
Panel C: One-year impact						
Ranking	0.842*	(0.471)	[0.0990]	0.631**	(0.315)	[0.0990]
Positive Signal	0.856**	(0.429)	[0.0990]	0.684**	(0.318)	[0.0990]
Ranking × Positive Signal	-1.360**	(0.547)	[0.0495]	-0.630*	(0.365)	[0.0792]
Control mean - bottom	9.11			5.74		
Control mean - top	10.14			6.29		
Joint test - p-value	0.064			0.935		
adjusted r-squared	0.043			0.013		
Observations (cluster)	573 (315)			573 (315)		

Notes: Outcome measures are winsorized at the 1st and 99th percentile. All coefficients are difference-in-differences estimates. Baseline differences and time trend effects are controlled for in all regressions though not reported. All regressions include randomization strata and survey fixed effects. Panel B reports treatment effects after four months. Panel C reports treatment effects after one year. 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 A.3: Ranking Impact on Firm Performance - winsorized outcomes.

	Difference-in-difference regression								
	Sales			Profit			Self-reported profit		
Outcome	ATE (1)	se (2)	q-value (3)	ATE (4)	se (5)	q-value (6)	ATE (7)	se (8)	q-value (9)
Panel B: Short-term impact									
Ranking	2,324.342**	(950.880)	[0.0297]	1,930.184**	(967.242)	[0.0396]	1,529.357**	(642.011)	[0.0297]
Positive	8.966	(1,184.607)	[0.9505]	-295.685	(1,175.590)	[0.9505]	3,394.061***	(733.657)	[0.0000]
Ranking × Positive	-2,994.617*	(1,671.825)	[0.1782]	-2,415.788	(1,632.512)	[0.1782]	-1,717.932	(1,056.038)	[0.1782]
Control bottom	1,187.806			800.097			2,055.313		
Control top	4,747.975			2,306.716			5,444.842		
Joint test	0.6274			0.7123			0.8251		
adj.	0.045			0.007			0.035		
r-squared									
Observations (cluster)	569 (312)			564 (312)			280 (280)		
Panel C: One-year impact									
Ranking	826.094	(651.265)	[0.4653]	756.417	(924.826)	[0.4653]	937.785	(816.932)	[0.4653]
Positive	-2,636.206***	(950.822)	[0.0198]	-607.909	(1,103.930)	[0.5941]	1,691.572**	(719.977)	[0.0495]
Ranking × Positive	-1,206.146	(1,289.934)	[0.7030]	-350.458	(1,536.357)	[0.7921]	-708.961	(1,112.193)	[0.7624]
Control bottom	1,634.15			-176.00			2,655.56		
Control top	2,539.34			1,018.14			4,314.14		
Joint test	0.7329			0.7412			0.7622		
adj.	0.088			0.023			0.003		
r-squared									
Observations (cluster)	530 (309)			525 (308)			255 (255)		

Table A.4: Ranking and Peer Info Impact on Effort - winsorized outcomes.

	Difference-in-difference regression					
	Hours worked			Days worked		
Outcome	ATE (1)	se (2)	q-value (3)	ATE (4)	se (5)	q-value (6)
Panel B: Short-term impact						
Ranking	0.920**	(0.397)	[0.0099]	0.228	(0.168)	[0.2673]
Positive Signal	0.434	(0.420)	[0.2376]	0.298	(0.251)	[0.2376]
Ranking × Positive Signal	-1.091**	(0.498)	[0.0396]	-0.151	(0.305)	[0.5248]
Peer Info	0.008	(0.285)	[0.8911]	-0.067	(0.200)	[0.9802]
Top Gender	-0.056	(0.345)	[0.8614]	0.101	(0.234)	[0.7030]
Peer Info × Top Gender	0.121	(0.506)	[0.8812]	0.114	(0.433)	[0.8812]
Control mean - bottom	9.400			6.200		
Control mean - top	9.825			6.179		
Joint test bottom - p-value	0.0412			0.4578		
Joint test top - p-value	0.9371			0.8126		
adjusted r-squared	0.071			0.008		
Observations (cluster)	479 (251)			478 (251)		
Panel C: One-year impact						
Ranking	0.747	(0.534)	[0.1287]	0.633*	(0.351)	[0.1188]
Positive Signal	0.686	(0.452)	[0.1188]	0.748*	(0.390)	[0.1188]
Ranking × Positive Signal	-1.105*	(0.568)	[0.0792]	-0.679	(0.441)	[0.0792]
Peer Info	-0.093	(0.338)	[0.8812]	0.009	(0.256)	[0.9406]
Top Gender	-0.639	(0.472)	[0.1287]	-0.164	(0.344)	[0.5446]
Peer Info × Top Gender	0.850	(0.644)	[0.6238]	0.261	(0.496)	[0.6238]
Control mean - bottom	9.20			5.72		
Control mean - top	9.92			6.16		
Joint test bottom - p-value	0.0241			0.0952		
Joint test top - p-value	0.4994			0.6879		
adjusted r-squared	0.046			-0.002		
Observations (cluster)	456 (251)			455 (251)		

Notes: Outcome measures are winsorized at the 1st and 99th percentile. All coefficients are difference-in-differences estimates. Baseline differences and time trend effects are controlled for in all regressions though not reported. All regressions include randomization strata and survey fixed effects. Panel B reports treatment effects after four months. Panel C reports treatment effects after one year. 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 A.5: Ranking and Peer Info Impact on Effort - winsorized outcome.

	Difference-in-difference regression											
	Shop opening time											
	Monday		Tuesday		Wednesday		Thursday		Friday		Saturday	
	ATE	q-value	ATE	q-value	ATE	q-value	ATE	q-value	ATE	q-value	ATE	q-value
Outcome	(1)	(2)	(4)	(5)	(7)	(8)	(10)	(11)	(13)	(14)	(16)	(17)
Short-term impact												
Ranking	-56.485***	[0.030]	-47.925**	[0.059]	2.262	[0.960]	-80.525**	[0.099]	-42.055**	[0.099]	-30.436	[0.802]
	(18.761)		(18.879)		(35.978)		(35.119)		(19.539)		(37.335)	
Positive	-13.257	[0.881]	-5.455	[0.911]	13.872	[0.941]	-49.393	[0.564]	8.002	[0.970]	26.697	[0.941]
	(28.975)		(21.592)		(36.551)		(40.603)		(17.948)		(42.375)	
Ranking × Positive	29.180	[0.614]	19.163	[0.693]	-26.205	[0.951]	73.380	[0.248]	-2.852	[0.980]	-10.616	[0.980]
	(33.988)		(28.207)		(43.793)		(45.550)		(26.635)		(57.015)	
Peer Info	31.894	[0.317]	10.414	[0.822]	-11.883	[0.832]	-14.143	[0.832]	27.449	[0.386]	-30.486	[0.832]
	(22.239)		(16.771)		(27.029)		(24.239)		(21.654)		(42.476)	
Top Gender	11.635	[0.980]	-21.813	[0.376]	-2.154	[0.871]	-23.037	[0.426]	-12.168	[0.485]	-29.640	[0.634]
	(19.744)		(20.161)		(21.485)		(22.323)		(16.416)		(32.726)	
Peer Info × Top Gender	-15.859	[0.951]	-15.758	[0.980]	-15.737	[0.980]	41.423	[0.644]	-9.239	[0.980]	67.178	[0.812]
	(30.971)		(52.513)		(55.708)		(36.474)		(32.094)		(86.369)	
Control bottom	791		793.33		790		816.67		790		770	
Control top	772.5		795		775		775.00		795		782.5	
Joint test bottom	0.1486		0.2428		0.6113		0.1640		0.3917		0.9423	
Joint test top	0.7546		0.5616		0.3744		0.6098		0.3696		0.9586	
adj. r-squared	0.024		0.046		0.018		0.052		0.038		-0.003	
Observations (cluster)	482 (251)		482 (251)		482 (251)		482 (251)		482 (251)		482 (251)	

Notes: All coefficients are difference-in-differences estimates. Baseline differences and time trend effects are controlled for in all regressions though not reported. All regressions include randomization strata and survey fixed effects. Panel B reports treatment effects after four months. Panel C reports treatment effects after one year. 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 A.7: Ranking Impact on Effort - winsorized outcomes - Male vs. Female Effects.

	Difference-in-difference regression							
	Male				Female			
Outcome: Work Hours	ATE (1)	se (2)	ATE (3)	se (4)	ATE (5)	se (6)	ATE (7)	se (8)
Pooled Sample								
Ranking	1.040***	(0.376)	1.122**	(0.448)	0.717	(0.677)	0.553	(0.733)
Positive Signal	0.703**	(0.307)	0.461	(0.387)	1.108*	(0.661)	0.795	(0.701)
Ranking × Positive Signal	-1.305***	(0.463)	-1.167**	(0.510)	-1.615**	(0.756)	-1.133	(0.762)
Peer Info			-0.304	(0.356)			0.317	(0.326)
Top Gender			-0.254	(0.332)			-0.606	(0.719)
Peer Info × Top Gender			0.013	(0.481)			1.411	(1.008)
Control mean - bottom	9.568		9.657		8.773		8.700	
Control mean - top	10.400		10.082		10.167		9.500	
Joint test bottom - p-value			0.0939				0.0400	
Joint test top - p-value	0.3279		0.4700		0.0083		0.2492	
adjusted r-squared	0.021		0.009		0.026		0.004	
Observations (cluster)	507 (187)		416 (153)		358 (129)		271 (98)	

Notes: Outcome measures are winsorized at the 1st and 99th percentile. All coefficients are difference-in-differences estimates. Baseline differences and time trend effects are controlled for in all regressions though not reported. All regressions include randomization strata and survey fixed effects. Panel B reports treatment effects after four months. Panel C reports treatment effects after one year. Standard errors clustered at the individual level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.6: Ranking and Peer Info Impact on Firm Performance - winsorized outcomes.

	Difference-in-difference regression								
	Sales			Profit			Self-reported profit		
	ATE	se	q-value	ATE	se	q-value	ATE	se	q-value
Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel B: Short-term impact									
Ranking	3,196.740***	(1,130.288)	[0.0198]	2,490.834**	(1,156.502)	[0.0594]	1,260.405*	(683.991)	[0.0990]
Positive	1,280.288	(1,430.793)	[0.8515]	1,678.420	(1,401.700)	[0.9604]	-1,869.820*	(1,134.476)	[0.0000]
Ranking × Positive	-3,598.397**	(1,817.906)	[0.3168]	-4,198.447**	(1,782.184)	[0.2871]	3,613.358***	(854.475)	[0.3168]
Peer Info	-3,014.887**	(1,170.752)	[0.0198]	-1,538.497	(1,155.189)	[0.1386]	-247.547	(780.821)	[0.8713]
Top Fem.	-1,868.826*	(1,011.426)	[0.0297]	75.150	(966.820)	[0.2475]	-812.621	(964.190)	[0.2376]
Peer Info × Top Fem.	5,791.446**	(2,383.470)	[0.0396]	2,633.401	(2,248.979)	[0.1683]	3,036.198	(1,890.523)	[0.1683]
Control bottom	1,137.862			724.966			2,027.333		
Control top	5,186.925			3,732.150			5,642.167		
Joint test bottom	0.0179			0.1260			0.1437		
Joint test top	0.3372			0.7953			0.4149		
adj. r-squared	0.063			0.026			0.055		
Observations (cluster)	469 (251)			465 (251)			224 (224)		
Panel C: One-year impact									
Ranking	1,207.142	(752.254)	[0.5248]	1,200.428	(1,152.785)	[0.5347]	1,028.800	(978.483)	[0.5347]
Positive	-2,373.824***	(766.770)	[0.0000]	-600.048	(1,158.223)	[0.4059]	1,360.588*	(811.574)	[0.1386]
Ranking × Positive	122.418	(1,049.370)	[0.9703]	699.612	(1,566.599)	[0.0000]	-308.334	(1,197.844)	[0.0000]
Peer Info	-1,626.975**	(761.530)	[0.3267]	-1,213.675	(1,155.099)	[0.4158]	-1,463.500	(995.023)	[0.4158]
Top Fem.	-596.048	(653.826)	[0.3960]	-559.260	(1,238.864)	[0.3960]	-1,430.567*	(773.489)	[0.0990]
Peer Info × Top Fem.	2,750.555*	(1,648.550)	[0.1485]	2,054.320	(2,053.131)	[0.1683]	3,314.718**	(1,578.518)	[0.0891]
Control bottom	1,736.200			-217.240			2,868.00		
Control top	2,078.714			480.257			4,210.00		
Joint test bottom	0.1579			0.2666			0.6312		
Joint test top	0.1297			0.1995			0.4009		
adj. r-squared	0.082			0.018			0.007		
Observations (cluster)	441 (250)			437 (249)			203 (203)		

Table A.8: Ranking Impact on Cooperation - Male vs. Female Effects.

	Difference-in-difference regression							
	Male				Female			
Outcome: Cooperation	ATE (1)	se (2)	ATE (3)	se (4)	ATE (5)	se (6)	ATE (7)	se (8)
Pooled Sample								
Ranking	2.534**	(1.094)	2.666**	(1.277)	2.485**	(1.249)	2.022	(1.411)
Positive Signal	1.872*	(1.035)	2.097	(1.319)	2.396**	(1.179)	3.425**	(1.360)
Ranking × Positive Signal	-1.442	(1.436)	-1.761	(1.664)	-3.056*	(1.608)	-3.658**	(1.743)
Peer Info			-0.828	(1.088)			-0.516	(1.162)
Top Gender			-0.442	(0.931)			1.203	(1.967)
Peer Info × Top Gender			0.634	(1.618)			4.312*	(2.599)
Control mean - bottom	2.789		2.944		2.917		2.727	
Control mean - top	4.787		5.083		4.250		4.375	
Joint test bottom - p-value			0.1274				0.0203	
Joint test top - p-value	0.2422		0.6702		0.5739		0.4075	
adjusted r-squared	0.019		-0.001		-0.007		0.019	
	352 (185)		287 (151)		248 (128)		190 (98)	

Notes: All coefficients are difference-in-differences estimates. All regressions include randomization strata fixed effects. Baseline differences and time trend effects are controlled for in all regressions though not reported. The sample is restricted to individuals whose baseline performance did not lie above the 90th percentile. Standard errors clustered at the individual level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.9: Ranking Impact on Firm Performance - Male vs. Female Effects.

	Difference-in-difference regression							
	Male				Female			
	ATE	se	ATE	se	ATE	se	ATE	se
Outcome: Sales	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pooled Sample								
Ranking	1,724.047*	(918.604)	2,341.927*	(1,199.594)	1,669.849	(1,152.723)	2,093.056***	(805.843)
Positive Signal	-1,764.154	(1,163.911)	-1,521.655	(1,121.548)	-179.244	(1,642.494)	1,260.804	(2,044.527)
Ranking × Positive Signal	-1,225.443	(1,654.624)	-370.259	(1,579.329)	-4,140.871*	(2,132.881)	-4,175.587*	(2,291.724)
Peer Info			-2,826.520**	(1,264.698)			-1,752.323*	(914.097)
Top Gender			-1,840.365*	(947.591)			-894.136	(1,449.633)
Peer Info × Top Gender			4,072.842**	(1,977.091)			4,956.015	(4,126.585)
Control mean - bottom	1,615.649		1,693.657		1,007.857		901.316	
Control mean - top	3,542.714		3,263.787		4,259.922		4,529.786	
Joint test bottom - p-value			0.0417				0.2246	
Joint test top - p-value	0.7180		0.1307		0.1712		0.7823	
adjusted r-squared	0.033		0.036		0.062		0.059	
	481 (186)		406 (153)		337 (129)		262 (98)	

Notes: Outcome measures are winsorized at the 1st and 99th percentile. Coefficients in Columns (1) and (4) are difference-in-differences estimates. Baseline differences and time trend effects are controlled for in all regressions though not reported. Coefficients in Column (7) are OLS estimates as baseline data on self-reported profits is not available. All regressions include randomization strata and survey fixed effects. Panel B reports treatment effects after four months. Panel C reports treatment effects after one year. Standard errors clustered at the individual level. * significant at 10%; ** significant at 5%; *** significant at 1%.