# The Impact of Financial Education of Executives on Financial Practices of Medium and Large Enterprises*

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**Abstract:** This paper studies the impact of a course in finance for executives of medium and large enterprises through a randomized controlled trial (RCT) in Mozambique. Survey data and accounting data provide consistent evidence that managers change firm financial policies in response to finance education. The largest treatment effect is on short-term financial policies related to working capital. Reductions in accounts receivable and inventories generate an increase in cash flows used to finance long-term investments. Those changes also improve the performance of the treated firms. Overall, our results suggest that relatively small and low-cost interventions, such as a standard executive education program in finance, can help firms to mitigate financial constraints and potentially affect economic development.

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# 1 Introduction

Differences in productivity and profitability across firms are large and persistent (Syverson (2004), Syverson (2011) and Foster et al. (2008)). It has been shown that management practices contribute to explaining these differences as well as development levels across countries (e.g., Bloom and Van Reenen (2007), Bloom and Van Reenen (2011) and Bloom et al. (2013)). The analyses on the role of management practices have mostly focused on the lower or middle management practices of larger corporations or on the founders/CEOs of small or micro-enterprises (e.g., Bruhn and Zia (2013), Drexler et al. (2014), and Anderson et al. (2018)). There is no quasi-experimental evidence from executives of large companies, although their potential impact on economic development is larger since they effectively control a large part of the economy. More importantly, most studies have focused on general management practices and thus, the specific role of financial practices (such as capital budgeting, working capital management and capital structure) in large firm is largely understudied.[1]

This paper provides the first experimental evidence on the importance of financial education for financial practices and performance of medium and large firms. We conducted a randomized controlled trial (RCT) with top-level executives of medium and large companies in Mozambique, in which we randomized participation in an executive education course in finance. The course focused on investment and capital budgeting decisions, as well as financial decisions including working capital management, capital structure, and risk management. Existing literature on the impact of financial education and business training offers mixed evidence of its effectiveness depending on the educational settings and targeted population (see McKenzie and Woodruff (2012)). Thus, another contribution is to study whether formal education of top executives is an effective vehicle to improve financial practices of medium and large firms.

While financial decisions are irrelevant in a frictionless world, the ability to make optimal financial decisions can have a positive impact on firm value in contexts where financial frictions are potentially severe, as in developing economies.[2] Therefore, Mozam-

[1] There is survey evidence on large U.S. firms by Graham and Harvey (2001, 2002).

[2] The World Bank Enterprise Survey (2018) identified "Access to Finance" as one of the greatest obstacles for firms in Mozambique. "Corruption" followed by the "Practices of the Informal Sector", "Crime", and "Political Instability" were also mentioned as obstacles. Only 10% of firms in Mozambique have a bank loan or line of credit, compared to approximately 44% that referred to still needing a bank loan, and more than

bique is arguably a relevant environment for studying the impact of a financial education program. Using both self-reported survey data as well as accounting data from one of the world's largest accounting firms, we find that this program led to significant changes in financial policies and firm investments. The largest changes are in short-term financial policies related to working capital. We find that treated firms reduced working capital compared to the control group, by reducing accounts receivable and inventories, which has a positive impact on short-term cash flows helping firms to overcome their financial constraints, at least partially and in the short run. This is an important margin under the control of firms that they do not necessarily manage actively. Moreover, while we document a significant effect in inventories (consistent with Bloom et al. (2013)), changes in accounts receivable are larger. Those changes are often easier to implement when compared to implementing a more efficient inventory management. Overall, these changes improved firm performance measured by accounting returns, which is consistent with efficiency gains. Importantly, survey data and accounting data, which are obtained through different sources, show similar responses of the managers to the treatment. This is reassuring given the self-reported nature of the survey data.

During an exploratory stage, we collected data on firms and executives to design the program and the intervention. This included the willingness and interest of executives to participate, as well as their availability. This information helped to identify relevant topics for the course and optimal dates and schedule so that attendance was not compromised. The data collected at this stage also allowed us to document that CEOs' financial expertise is correlated with the sophistication of their financial practices. While those correlations are consistent with an actual effect of financial expertise on financial policies, omitted variables could bias the estimates.

To estimate the effect of financial expertise, we treated 93 top managers of medium and large firms in Mozambique with a free executive education course on corporate finance (similar to an MBA core course in content and length). To address concerns about endogenous selection into the treatment, we randomized amongst firms that expressed their interest in participating in the program. We followed a staggered design where firms were randomly allocated into two cohorts: a treatment group and a control group. The

21% had recent loan applications that were rejected. One reason could be intense collateral requirements since more than 90% of the loans required collateral, with an average of 271% of the loan value being requested as collateral.

first cohort – the treatment group – received the treatment in May 2017, while the second cohort – the control group – participated in the course in November 2018/April 2019. The development economics literature has extensively employed experiments to measure the impact of the financial literacy of small and micro-entrepreneurs (e.g., Bruhn and Zia (2013), Drexler et al. (2014), and Anderson et al. (2018)), but these have not been applied to larger companies. An exception is Bloom et al. (2013), who used an RCT to measure the effects of general management practices on the productivity of large plants in India.[3] However, their focus was on lower-tier plant managers rather than on executives, and they did not study financial education and financial policies. Obtaining large samples in the context of RCTs with large corporations is very difficult. For instance, Bloom et al. (2013) performed an experiment in 17 firms operating 28 plants. In this respect, a sample size of 93 firms appears notable.

The main results can be summarized as follows: we find a large and negative treatment effect on working capital that decreases by 0.41-0.51 standard deviations for treated firms compared to the control group. When decomposing this effect, we find that treated firms decrease their collection periods, reducing accounts receivable by 0.57 standard deviations, as well their inventories by 0.38 standard deviations. The reduction in accounts receivable might be related to the collection of existing accounts, potentially late ones, or the negotiation of new contracts with new terms. From our survey analysis we document that some firms hired additional personnel to deal with outstanding debts. These changes are expected to have a positive effect on liquidity in the short run. While we do not find any treatment effect on cash holdings or leverage, we find a significant effect on capital expenditures (between 12 and 14 percentage points, which corresponds to 0.47 standard deviations).

Complementary survey data evidence is consistent with our main findings. Treated firms report high intentions to change financial policies after participation in the course, especially related to working capital management. The survey also reveals that a sizeable fraction of firms is not able to adjust their capital structure (32.5%), risk management and valuation practices (17.5% each), mostly because they are subsidiaries of multinational companies, and these policies are set elsewhere in the business group. Moreover,

[3]Other experiments have found mixed evidence of the impact of basic business training on micro and small enterprises in developing countries (Karlan and Valdivia (2011); Bruhn et al. (2018); Karlan et al. (2015a).

when comparing treated firms to control firms 15 months after the course, we find that about 30.8% report that they implemented those intended changes in working capital management (compared to 3.7% of control firms). Corresponding figures for other financial policies are lower (11.5% for capital structure decisions and valuation and 7.7% for risk management). Importantly, firms also report in the survey that they implemented these changes *because* of the course they participated in 15 months earlier.

Whether these changes have led to more efficient decisions is not clear ex-ante. For instance, by collecting receivables too quickly or by reducing inventories too much, future sales might be compromised. To test if firms have moved toward more efficient policies, we analyze whether the treated firms show better performance relative to the control group. Given that most firms are private, we do not observe forward-looking measures such as market values.[4] Hence, we rely on accounting ratios to measure performance. By analyzing return on assets (ROA), we find that treated firms' ROA increases by 0.88 standard deviations compared to control firms. We also find that return on invested capital (ROIC) improves, whereas at the same time, we do not find any adverse effect on sales growth. The point estimates of the treatment effects are large but not implausible, particularly given that the confidence intervals include more modest estimates.[5]

Attending the finance course might affect financial policies through different, nonexclusive channels. Participants might learn new corporate finance concepts and methodologies from the instructor, they might refresh or consolidate previous knowledge, they might learn from their peers, or they might generate new business from networking with their classmates. While we cannot formally exclude that networking is driving the results, we do not find supportive evidence for this channel. First, around the dates when we delivered the course to the treatment group (May 2017), we organized a separate kick-off event for the control group, allowing it to network as well. Second, while the positive result on ROA could be consistent with a network channel, it is less obvious why working capital should be affected. Third, we would expect to see a positive effect on sales growth if networking generated new business opportunities among treated firms (which we do not find). Last, exploiting heterogeneity in the characteristics of the executives, we find

[4]There were eight listed firms in Mozambique in 2019. Of these firms, six are non-financial firms and three of them participated in our program. One, which went public after the intervention, was in the treatment group while the other two were in the control group.

[5]Bruhn et al. (2018) made a similar argument when measuring the impact of consulting for small- and medium-sized firms in Mexico.

that managers without prior finance education are benefiting the most from participating in the course. This result suggests learning to be the most plausible mechanism.

Overall, our results show that financial expertise of managers are important for firm policies and that relatively small-scale financial education programs can improve financial practices and decision making, and possibly affect economic development. One of the main contributions is providing the first causal evidence that enhancing the financial expertise of CEOs of medium and large firms can improve firm efficiency by alleviating potential financing and corporate liquidity constraints. While most firms in Mozambique point out difficulties in accessing external financing, we estimate an average positive impact on firm cash flows of at least 190,000 USD from changes in working capital (using the lower bound of the confidence intervals as a conservative estimate). We do not find any evidence of an increase in cash holdings or dividends, which suggests that firms spent this influx of cash. Consistent with this evidence, we estimate the increase in capital expenditures of at least 210,000 USD. We also estimate the impact of the intervention on firm value. The estimated DID effect on ROA of 0.205 is, for most of the firms, much larger than the estimated cost of a similar course (approximately 10,000 USD in tuition fees).

Why had firms and managers not already obtained financial education? There are several non-mutually exclusive potential reasons. First, there are no similar courses available locally, significantly raising the total cost of such a program (including traveling and opportunity costs). Second, firms might simply not be aware of the benefits of such executive training (e.g., Rivkin (2000)). Moreover, Kremer et al. (2019) argued that this behavior can be consistent with behavioral biases of managers of firms in developing countries, such as inattention, underestimation of returns, or overestimation of the risks involved.

The remainder of this paper is structured as follows. We discuss the contribution to related literature in the next section. Section 3 provides an overview of financial education and the financial practices of firms in Mozambique. We also present the experimental design and describe the executive education program and the data collection process. Section 4 shows the results of our intervention based on accounting and survey data. In that section, we also discuss the results and address some threats to internal validity of the experiment. In Section 5 we present subsample results (heterogeneous effects), we interpret the findings and offer some policy considerations. Section 6 concludes.

## 2 Literature Review

In a seminal paper Bertrand and Schoar (2003) showed that individual CEOs contribute to explaining observed heterogeneity in management practices and corporate policies, and concluded that CEOs possess different "styles". While there is a large literature that studies the relation of CEO characteristics and traits on firm decisions making (e.g., Bertrand and Schoar (2003), Malmendier and Tate (2005), Malmendier and Tate (2008), Malmendier et al. (2011), Kaplan et al. (2012), Hirshleifer et al. (2012), Custodio and Metzger (2013), Custodio and Metzger (2014), Custódio et al. (2019), or Schoar and Zuo (2017)), an interpretation of the documented associations remained challenging. These papers mostly relied on cross-sectional analysis and panel regressions exploiting within-firm variation due to CEOs switching firms (Dittmar and Duchin (2016)). As pointed out by Fee et al. (2013), Guenzel and Malmendier (2020) and Custodio and Metzger (2014), there is the concern that time-varying, unobservable characteristics of firms can drive both, the appointment of a specific type of CEO and their firm policies. For instance, Custodio and Metzger (2014) document that "financial expert" CEOs are more likely to be appointed by mature firms and focus on optimizing the liability side of a firm's balance sheet. "Non-finance CEOs", on the contrary, are more likely to manage growth firms with an emphasis on non-financial corporate policies. We, therefore, contribute to this large literature by providing the first causal evidence that CEOs affect corporate policies, by showing that enhancing the financial expertise of CEOs of large firms leads to changes in firm financial practices and improves firm efficiency.

We also contribute to a growing literature on building managerial capital of small, medium, and large corporations (e.g., McKenzie and Woodruff (2012)). Most of these studies focus on general management practices (e.g., Bloom et al. (2013), Bruhn et al. (2018) or Bandiera et al. (2020)), or link these practices to other corporate dimensions such as corporate culture (e.g., Banerjee et al. (2012)) and Blader et al. (2020)) or technology adoption (Giorcelli (2019)). Our work focus on financial practices of top executives of medium and large corporations, a dimension of management practices that is still understudied, but might be particularly important in environments with severe financial frictions. Along this dimension, our findings are consistent with the work showing that managers' financial expertise impacts the revenues and/or survival rates of corporations

in the context of small and micro-entrepreneurs in developing countries (e.g., Bruhn and Zia (2013), Drexler et al. (2014), and Anderson et al. (2018)), and it is correlated with firm financial policies, such as cash holdings or capital structure decisions in developed countries such as the U.S. (Custodio and Metzger (2014)).[6] Consistently, De Mel and Woodruff (2008) show that microenterprises in Sri Lanka are financially constrained either because of "a lack of savings institutions - or a lack of knowledge about how the savings institutions operate". We show that finance education matters for medium and large firms, and that relatively low-cost interventions, such as an 18-hour MBA-style finance executive education course, help to build relevant corporate finance skills. Finally, our results provide new insights on the mechanisms of impact of financial expertise in larger firms, as we show that improving short-term financial policies, such as working capital, can potentially relax financial constraints by improving firm liquidity in the short run. At a broader level, our results corroborate the idea that misallocation of capital and labor contributes to the observed Total Factor Productivity (TFP) gap of developing countries with respect to the U.S. (Hsieh and Klenow (2009)). The lack of managerial capital with respect to financial expertise might also be part of the explanation for the observed firm size distribution in developing economies. The extreme weight of micro and small enterprises and the lack of large companies in developing countries when compared to developed ones constitutes an empirical puzzle. It is therefore important to understand what prevents smaller and medium companies in these economies to grow. Alternative explanations, which are not mutually exclusive, include differences in the quality of institutions, the importance of the informal sector and lack of registration in developing economies, as well as the existence of financial constraints and managerial capital constraints. To the extent that enhancing financial expertise of managers can relax some of these financial constraints it is plausible to argue that this can also unleash the growth potential of firms.

Last, we contribute to the extensive literature on financial literacy (e.g., Lusardi (2005), Lusardi (2009), Lusardi and S.Mitchell (2007a), and Lusardi and S.Mitchell (2007b)) and financial literacy training (e.g., Cole and Shastry (2014), Cole and Zia (2009)) and its links to development. Most of these studies focus on financial literacy, financial education, and financial decision making of households. Less is known about financial literacy of managers of corporations and the potential impact on the efficiency of firms' financial

[6] Aktkinson (2017) provided a survey on financial education for MSMEs and potential entrepreneurs.

choices. Existing research in this area usually studies microentrepreneurs (e.g., Karlan and Valdivia (2011), Bruhn and Zia (2013), Drexler et al. (2014), Karlan et al. (2015b), Anderson et al. (2018), Brooks et al. (2018), Higuchi et al. (2019), Iacovone et al. (2019)) and focus mostly on very basic financial practices such as the importance of separating personal and business cash, or preparing account records. Existing research has also shown that standard accounting training and formal educational settings are not effective in improving financial literacy. One reason could be cognitive constraints as a key barrier to improving financial knowledge (Carpena et al. (2011)). Overall, there is mixed evidence with respect to the effectiveness of different financial literacy interventions (formal vs. informal training; training vs. advising or consultancy) . We show that a standard MBA course on corporate finance, delivered in a generic classroom setting, can improve financial literacy and corporate finance practices of CEOs of larger corporations, which are arguably more sophisticated subjects. This evidence is also consistent with the findings in Gosnell et al. (2020) that improved management practices can increase productivity among skilled labor.

# 3 Design and Implementation of the Experiment

This section explains our decision to conduct the experiment in Mozambique and the selection of firms to the experiment. It also describes an exploratory stage, during which we collected information about the background of CEOs (including financial education and experience), as well as firms' current financial practices. We also present the experimental design and sample description as well as details of the intervention, namely the structure of the program. Finally, we discuss the data collection procedure to evaluate the potential impact of the intervention.

## 3.1 Mozambique and the Focus on Medium and Large Firms

Mozambique is arguably a relevant context to study the impact of financial literacy of managers of large firms for several reasons. First, we expected to observe more heterogeneity in terms of existing financial education levels among top executives compared to those in developed countries due to the lack of executive education programs in finance

available in the country.[7] This heterogeneity might be helpful when measuring the effects of financial education on financial policies and firm performance. Second, survey statistics collected by the World Bank Enterprise Surveys (2018) suggest that firms in Mozambique face severe financial frictions (like many other Sub-Saharan African countries), and potentially relaxing these constraints might be important and valuable. Indeed, "Access to Finance" and "Corruption" are the greatest obstacles for firms in Mozambique, followed by "Practices of the Informal Sector", "Crime", and "Political Instability". Third, Mozambique had an important advantage for the implementation stage: most large companies' headquarters are located in the capital, Maputo. This helped with the logistics and organization of the intervention, and at the same time was expected to increase participation rates. Finally, we benefited from the existing links between NOVAFRICA, a knowledge center at Nova School of Business and Economics, and governmental organizations and NGOs in Mozambique, which helped to increase the visibility and credibility of the project.

We focused the intervention on medium and large firms because they control a large fraction of assets in the economy. Potential efficiency gains of these firms are therefore more likely to be economically relevant. Moreover, some capital allocation inefficiencies previously documented in the literature are mostly relevant for large and multidivisional firms (see, for instance, Krüger et al. (2015)). Finally, in the long run, there might also be some spillover of best financial practices from large to smaller firms, either because of large firms being role models for smaller firms or because of human capital that is moving with workers across companies. Both channels are likely to be more prominent in large firms.

### 3.2 Financial Expertise of Managers and Financial Practices

During an exploratory stage of the project we collected information about managers, including demographics, financial education and experience, as well as firms' characteristics and financial policies. This exploratory stage was helpful for several reasons. First, there are no available data on financial experience and firm financial polices for a large set of firms in Mozambique. Understanding the status quo in terms of CEO educational

[7] For instance, there is only one business school providing an MBA program on a regular basis (in cooperation with a Portuguese business school).

backgrounds and current finance practices, as well as learning more about the functioning of the financial markets, was important to design a meaningful course for the target audience. Second, it helped us to understand whether there was enough interest in participating in an executive education program in finance and to learn what content would be relevant for Mozambique. Finally, it allowed us to compare the financial expertise and practices of these firms with evidence from firms of similar size and sectors from the U.S.

The exploratory stage ran between June and July 2015 (see Figure I). During this period, we contacted 218 companies obtained from KPMG "Top 100 Companies in Mozambique" reports from 2010-2014 and had 65 meetings with executives. At those meetings, we were able to collect 63 questionnaires.[8] The questionnaires were completed during a 30-minute face-to-face interview. The interviews were conducted at the companies' premises by a member of the research team. Although we specifically invited the CEO, sometimes our request was forwarded to the CFO, to a member of the accounting team, or in a few cases, to a non-finance related staff member.

The questionnaire surveyed the financial practices, manager characteristics, and overall business aspects of the company, following Graham and Harvey (2001) and Graham and Harvey (2002).[9] During the meeting, we also assessed the interest of managers in a free of charge executive education program on financial management. We specifically asked which topics they would find most relevant. These included capital budgeting, risk management, capital structure, working capital management, pay-out policy and mergers and acquisitions. Finally, we inquired about the executives' time availability and preferences for such a program to maximize attendance.

The answers to the survey also allowed us to have a first look at financial expertise, financial policies, and the interaction between these two in Mozambique. We document a substantial heterogeneity in financial expertise by CEOs in Mozambique. Approximately 82% of the CEOs have a background in finance, i.e. attended at least a course in finance. When analyzing financial practices in firms with and without "financial expert CEOs", we find large differences in their practices. For example, Figure II shows financial practices related to capital budgeting/valuation by firms run by financial expert CEOs, compared

[8]Two participants were busy at the scheduled time and committed to send us the questionnaire later by e-mail, which did not happen. These 63 pilot questionnaires correspond to 62 business groups (in this case, single companies) since we surveyed separately two managers from the same company.

[9]See also Correia (2012) for an assessment of financial policies in South Africa.

to non-financial expert CEOs. While a large majority of CEOs with a background in finance use sophisticated valuation techniques, such as net present value (NPV) (70%), or conduct sensitivity analysis (63%), these techniques are relatively uncommon for CEOs without such a background. Only 25% of CEOs with no financial background use NPV, and only 33% of them perform sensitivity analyses in their capital budgeting calculations. At the same time, they are more likely to use less sophisticated valuation techniques, such as hurdle rates (63%). These findings are consistent with U.S. evidence from Bertrand and Schoar (2003) and Custodio and Metzger (2014), who found that CEOs with MBAs or financial expertise are much more likely to follow financial theory and textbook rules and to avoid common mistakes, such as using a unique firm cost of capital irrespective of the nature of the project (the WACC fallacy).

These correlations between financial expertise of CEOs and their financial practices are consistent with the view that CEO education affects financial policies, however, a causal interpretation of these correlations remains difficult because of the endogenous decision by firms to appoint a financial expert CEO.

### 3.3 Experimental Design

Our experimental design is motivated by two common challenges faced by researchers when analyzing the effect of financial education on financial policies: i) the endogenous decision to appoint a financial expert CEO / to obtain financial education; and ii) limited availability of data.

The literature on the effects of managerial human capital on firm policies has mostly relied on cross-sectional analysis, which renders causal inference very challenging as endogenous matching between firms and managers biases the estimates (Guenzel and Malmendier (2020)). Since Bertrand and Schoar (2003), most studies have used panel regressions to estimate potential CEO effects using within-firm variation due to CEOs switching firms. However, Custodio and Metzger (2014) and Fee et al. (2013), for instance, cast doubt on this methodology for identifying managerial effects on policy choices. They argued that CEO turnover events are endogenous, and managerial "style changes" are anticipated by corporate boards at the time of the CEO selection decision. While firm-fixed effects absorb firm heterogeneity that is time invariant, it cannot be ruled out that firm time-varying characteristics, unobserved by the econometrician, such as some strategic

decisions, drive both financial policies and the characteristics of the appointed CEOs. In the context of financial expertise, Custodio and Metzger (2014) showed that firms run by managers with past work experience in finance have better access to external financing and allocate their firms' financial resources more efficiently. However, this study also shows that financial expert CEOs are more likely to be appointed by older firms, which suggests an endogenous matching.

To identify a treatment effect of financial expertise on firm policies, one would need to randomize financial expertise across firms. One way of doing so could be an actual random allocation of CEOs to firms, which would take care of endogenous matching. However, this experiment is not feasible in practice. Moreover, a random allocation of CEOs to firms does not deal with the concern that there are unobservable characteristics of CEOs that correlate with financial expertise. For instance, CEOs with financial expertise might be of higher (or lower) ability or talent.

To overcome endogeneity concerns we propose randomizing financial education of top managers while maintaining the match between CEOs and firms. To be specific, we treat managers with financial education by offering free MBA-style lectures on corporate finance and risk management to top managers. Such a randomized controlled trial (RCT) can be used to identify a treatment effect of finance education on financial policies.

The second challenge for our study is the availability of data. First, most companies in Mozambique are private, and access to financial statements is limited. Moreover, some outcomes, such as the use of specific valuation techniques or risk management instruments, are difficult to measure in those statements.

In order to address both concerns, endogeneity and data availability, we implemented the intervention in a staggered way, i.e., we ultimately taught both, the treatment and the control group. By treating both groups, we provide incentives to firms to share their financial statements with us, as well as to participate in face-to-face surveys, allowing us to collect data on nonstandard outcomes. The first cohort – the treatment group – received the treatment in May 2017, while the second cohort – the control group – received the same treatment in November 2018/April 2019 (see Figure I).

The staggered nature of the intervention also helps to address the concern that the formation of expectations could bias our estimates (Chemla and Hennessy (2019)) because despite the greater uncertainty for the control group, which is treated later, both

the treatment and control groups expect to be treated.[10] Last, it reduces ethical concerns of providing a permanent advantage to one of the groups.

To address the concern of endogenous selection into our treatment, we conducted the randomization among the firms that applied to the program.[11]. We also stratified the randomization by industry to ensure that the same industries were represented in both groups. As noted by Sutton (2014), a sample stratified by industry provides a "fair and complete picture of the country's industrial capabilities". Because there were subsidiaries of business groups in our sample (i.e., companies belonging to the same group that were managed by one or more participating managers) we made sure the these companies were part of the same group to minimize contamination concerns.

### 3.4 The Finance Course

The course was designed as a general course in corporate finance emphasizing topics identified as useful by the managers in the exploratory stage. The proposed outline contains standard topics of any corporate finance course (i.e., capital budgeting, valuation, and capital structure) plus modules on working capital management and risk management. The course was then organized in four modules:

1. Capital Budgeting and Valuation: this module covered standard techniques of firm and project valuation, such as discounted cash flows methods, net present value, internal rate of return, and payback period. It also covered asset pricing models, such as the CAPM, as tools to estimate project discount rates. By the end of this module the executives were expected to be able to read, understand and process financial information from financial reports (e.g., calculate basic financial ratios), as well as understand how to apply the different valuation techniques when making capital budgeting decisions. We also discussed some common valuation mistakes, such as the WACC fallacy, i.e., the use of a company-wide discount rate instead of a project-specific one, as well as ignoring the time value of money.

2. Capital Structure: this module presented a practical view of assessing the optimal capital structure of the firm, discussing the trade-off theory of debt financing, such

[10] We discuss other implications of the staggered implementation in more detail in Section 4.5.4.

[11] We analyse the characteristics of firms and executives interested in attending the course versus those who are not in table A15 in the appendix of the paper)

as the tax shield of debt and bankruptcy costs, respectively. The main goal of this module was to understand the trade-off between the costs and benefits of a given financial structure and source of financing and being able to apply these trade-offs in a real business case.

3. Working Capital Management: this module covered the concept of working capital and the impact of efficient working capital management on cash flows and cash holdings. This module also covered cash management and management of inventory, accounts receivable and accounts payable. For instance, participants were taught how to calculate the cost of trade credit and compare it to other sources of financing. It was also emphasised that by reducing working capital, firms can improve short term liquidity, and that significant decreases in working capital may free up cash and be used as an additional source of funding. It was also referred that reducing working capital is not necessarily optimal and trade-offs with the costs and benefits of using this firm policy were presented and discussed with a case study.

4. Risk Management: this module covered potential sources of risks and associated costs, a discussion of appropriate hedging instruments, implementation of risk management strategies, as well as their management and monitoring.

The four modules had a total of 18 hours (4.5 hours each), and was delivered both in Portuguese and English.[12] While the duration might appear relatively short, interventions in related studies have similar duration (e.g., two days or two half days (Bruhn and Zia (2013) and Field et al. (2010))). Moreover, our course is at the shorter end of these types of interventions but in line with sessions on similar topics in standard MBA core courses in corporate finance. Given that the participants were top executives, our exploratory survey also suggested that many CEOs/CFOs found it difficult to accommodate longer courses in their agendas. By keeping the intervention short, we might have increased participation, potentially at the expense of the intensity of the intervention. At the same time, shorter courses are less expensive and simpler to organize logistically – a potentially important criterion from a policy point of view.

[12]Table A2 in the appendix provides a more detailed overview of the schedule.

The format of the course was a mixture of lectures and case studies. The case studies illustrated the different topics in a relevant setting for larger firms operating in emerging markets. For instance, we used the following Harvard Business School case studies: New Earth Mining (evaluating a new investment opportunity in South Africa); Mozal (large investment project in Mozambique); and Supply Chain Finance at Procter and Gamble and Fibria (working capital management and its liquidity consequences for a supplier in Brazil).[13] Participants who attended a minimum of 75% of the classes received a participation certificate from Imperial College Business School.

### 3.5 Recruitment Process into the Experiment and Sample Description

In this section we describe how managers and firms were recruited to participate in the experiment as well as the sample size at each stage. Figure III reports the number of companies participating at different stages of the project. First, we invited (via email and telephone calls) 577 medium and large companies to sign up for an executive education program on finance. The list of invited companies is primarily composed of companies appearing in a KPMG report at least once in the period of 2009-2016 (391 companies). Additionally, we invited 186 companies associated with local business associations, namely CTA (Confederação das Associações Económicas de Moçambique) and ACIS (Associação de Comércio, Indústria e Serviços).[14] We restrict our sample to companies headquartered in Maputo.[15] This regional restriction enabled in-person interaction with participants, which was crucial throughout the project to engage the participants and to facilitate data collection. This requirement also reduced non-compliance of participants since it minimized the participants' cost of attending the training. We focused on top executives in these companies (CEOs and CFOs) since they usually take most strategic decisions, including as well financial decisions (see Graham et al. (2015)).

The advertised course was an Executive-level Program in Finance – "Finance and

[13] The course was delivered in both Portuguese and English (the group was split according to its language preferences) by the same instructor in the case of treatment group and by two different instructors in the case of the control group.

[14] We partnered with these two business associations since these are well known organizations in the country. This contributed to raising public awareness about our project.

[15] Sutton (2014) presented detailed profiles of 40 Mozambican companies, chosen to represent the leading firms in several industries. Of these 40 companies, 24 appear in our set of invited companies. The match is much larger when we exclude companies from mining industries (located in specific regions of the country and usually outside Maputo). Of 19 remaining firms, 16 were invited to participate in our project.

Strategy: Value Creation in Emerging Markets" – promoted under Imperial College Executive Education branding. The course was offered in Maputo free of charge and was exclusive to the companies participating in the research project. Additional information about the course was openly available at the Imperial College Executive Education webpage, including a market price of £6,500 per participant/free of charge for invited participants.[16]

We received 109 positive responses from companies, for which we scheduled face-to-face meetings to present further details about the program. Managers who were interested in the program formalized their interest on behalf of the company by submitting an application form. This form collected information about manager characteristics (demographics, educational background and professional experience) and company characteristics. The registration form also contained a data access agreement for the provision of financial information (income statement and balance sheet). Each company could participate with up to two attendees, provided that at least one of them was a top manager.[17] We received application forms from 111 participants, corresponding to 93 firms. These companies were then randomly allocated (stratified by industry) into the treatment (45 companies) and control groups (48 companies) two weeks before the first intervention. We ensured that companies that were part of the same business group were allocated to the same group. Out of the 45 firms allocated into treatment group, 41 effectively attended the course. Because more than one manager per firm was allowed to participate 46 managers were taking the course in the treatment group. The 41 companies that attended the course were part of 31 different business groups (Table I).

Panel A of Table II shows summary statistics for the participating firms (treatment and control groups) and differences between the two groups in the year before the intervention (2016). The average treated firm has total assets of 22.3 million USD, total revenue of 15.8 million USD, and 191 employees. The distributions are very skewed, and by chance, there are three very large firms in the control group, resulting in larger means of size-related variables in the control group (significant at the 10 percent level). When we compare financial ratios or the medians, both differences between the two samples

[16] See an excerpt of the brochure in the appendix of the paper (figure A1).
[17] We required one application form per attendee.

are much smaller.[18,19] Normalized differences are reported in the last column. The normalized differences are generally modest, with all normalized differences far below 1.00 in absolute value. More than half of them are below 0.30 and the remaining ones are in the range between 0.30 and 0.50.[20] Figure IV (left panel) reports the distribution of participating firms by sectors of activity. Services and retail sectors are the most represented in the sample, followed by construction, manufacturing, and tourism and accommodation sectors. A similar ranking is shown among non-participating firms (right panel).[21]

Panel B of Table II shows summary statistics for the top managers (the participants with the highest role in each participating business group) in the treatment and control groups, as well as the differences between the two groups. Approximately 61% of the managers in the treatment groups are the CEOs of companies and 29% the CFOs. These managers are generally highly educated, with 57% having a masters degree or higher. A large proportion also has a finance or accounting-related education, with only 19% of them reporting no education in finance or accounting at any level (unreported). Approximately 19% of the executives are female. Differences between the two groups are not statistically significant. The only exception is nationality. Approximately 55% of the managers in the treatment group are Mozambican, compared to 78% in the control group. The normalized differences are generally small, almost all of them below 0.30. One exception is the nationality as mentioned before with a normalized difference of 0.49.

We address potentially remaining concerns originating from a small sample in more detail in Section 4.5.3.

### 3.6 Implementation of the Experiment and Collection of Outcome Data

This section describes the implementation of experiment in more detail, including the timing of the interventions, a networking event for the control group, and the data collection process to measure its potential impact on firms' outcomes.

[18] Appendix Table A1 describes how each variable is constructed, as well as its sources.

[19] The average Capex / Assets is negative for both groups in 2016. We inspected this item for companies located in other Sub-Saharan Africa using the ORBIS database. In a sample of 575 companies, the mean and median Capex / Assets is -5% and -3%, respectively.

[20] See Imbens (2015) for a discussion of normalized differences.

[21] However, sectors traditionally more prevalent outside Maputo (such as tourism and accommodation or primary sector activities) exhibit higher share among non-participating firms as these have been excluded from our sample by our regional filtering.

### 3.6.1 Intervention 1: Course Delivery for Cohort 1 (Treatment Group) and Networking Event for Cohort 2 (Control Group)

The first edition of the course took place in May 2017. Out of the 45 firms allocated to treatment group, 41 attended the course (participation rate of 91%).[22] Before the start of the course, participants were required to complete a pre-learning survey. This survey replicated the exploratory project survey and collected baseline information on current financial practices of the company. At the end of the course, participants completed a post-learning exit survey. This survey was divided into a confidential part, in which participants were asked to evaluate the course, and a non-confidential part, in which they described their intentions to change financial practices in the future.

Network effects, instead or on top of the content of the course itself, could lead to changes in outcomes of interest. While potential network effects are less obvious for financial policies, there is the concern that it may impact revenue and profitability. Profitability is a critical outcome to understand whether potential changes in financial policies lead to more efficient outcomes. Networks can affect profitability in several ways: attendees could form new business relationships or share relevant information.

To address this concern, we organized an afternoon networking event for the control group, with the purpose of giving the control group the opportunity to mingle and network.[23] This event occurred around the dates of the first intervention, i.e., when the treatment group attended the course. We further discuss potential network effects as well as some other threats to the internal validity in detail in Section 4.5 of the paper.

### 3.6.2 Intervention 2: Course Delivery for Cohort 2 (Control Group)

Between September and November 2018, we contacted and visited companies in the control group. Out of 48 firms in the control group we were able to hold 40 meetings. In these meetings we conducted interviews using the pre-learning questionnaire (identical

[22] Four companies did not adhere to the randomized protocol. Two of them enrolled through e-mail/phone and promised to deliver the application form later. We were not able to reach them later. The other two enrolled and confirmed attendance in the first edition but did not appear on the day of the course. After a follow-up call, one manager stated that he was away due to an unexpected meeting abroad, whereas another firm was experiencing an internal re-structuring that required constant manager's presence.

[23] This event featured a short presentation of the executive education program, as well as speeches by invited high-profile individuals from the public and private sectors. Importantly, the network event did not featured any content of the course and was held at a different place to avoid interaction between treatment and control group.

to that applied to the treatment group).

In a few cases, the manager that had initially applied to the program had been replaced. For these cases, we briefed the new manager about the program and invited her or him to participate in the second edition of the course. The second cohort of the course was taught in November 2018 (in Portuguese) and April 2019 (in English). The course's content and teaching method were the same as in the first edition. At the end of the course, participants were required to complete the same post-learning exit survey as described in the previous subsection.

Out of 48 control companies, 27 effectively attended the course (participation rate of 56%).

### 3.6.3 Measuring Outcomes: Follow-up Survey and Financial Reports

The outcome measures are guided by the content of the course and the availability of data. We use survey tools to measure (intended and realized) changes in policies related to the four topics of the course: valuation and capital budgeting techniques, working capital management, capital structure, and risk management. It is challenging to directly measure valuation techniques and risk management in the available financial reports, so we restrict our analysis to working capital management and capital structure decisions when using accounting data. Nevertheless we can rely on accounting performance data as an outcome that aggregates the impact of changes in all of the policies.

Approximately 15 months after the first intervention, between September 2018 and November 2018, we surveyed managers in the treatment and the control groups and we collected accounting data from firms. We requested both groups' financial reporting data between 2013 and 2018. We provided companies with a template spreadsheet, including balance sheet, income statement and statement of cash flows items, that were then filled in by a firms accountant of CFO. During face-to-face surveys, we asked managers in the treatment group about implemented changes with respect to financial policies since the first intervention. Similarly, we asked the control group about which financial practices had been changed in the preceding 15 months and investigated expectations regarding future changes. By surveying the control group in a identical way, we intended to provide a counterfactual for implemented changes in financial practices by the treatment group.

For a large set of firms, we complement the data provided by our participating firms

with accounting information directly from external reports, namely the "Top 100 Companies in Mozambique", published annually by KPMG Mozambique.[24] Each report lists and ranks the 100 largest companies (according to total revenue) from the pool of companies that complete the KPMG annual survey. It also presents additional rankings of firms by industry. For each company, it provides main financial accounting figures, such as revenues, net income, assets, liabilities, equity, number of employees and new investments. The KPMG data also allowed us to validate the self-reported data and address the concern that some firms might be strategic in their choice of sharing data with the research team.[25]

Financial data were available in U.S. dollars and/or Mozambican metical depending on the source. We converted all values in metical to dollars using the exchange rate on the reporting date. Out of 93 participating companies, we were able to obtain at least one year of financial data for 86 companies. We also collected financial data from KPMG reports for non-participating firms for external validity purposes, to discuss selection into the program, and to have an additional (non-random) benchmark.

# 4 The Impact of Financial Education on Financial Policies and Firm Performance

This section analyzes the impact of the treatment, the financial education program, on firm financial policies and performance. We compare implemented changes in financial policies of firms whose managers participated in the executive education program in May 2017 (treated firms) with firms yet to be treated (control firms). We use accounting data as well as survey answers to measure the outcomes of interest.

## 4.1 Changes in Financial Policies (Accounting Data)

We first use accounting data to measure changes in financial policies and firm performance. The financial statements contain information that allow us to investigate potential changes in working capital management and capital structure. They also allow us to

[24] These reports contain the names and information of many of the largest corporations in Mozambique. They are publicly available and are used by local and foreign investors, public administrations and other institutions.

[25] We discuss the concern related to attrition in more detail in Section 4.5.2 of the paper.

measure potential efficiency gains of those implemented changes.

Table III reports the estimates of treatment effects on working capital and its components using ordinary least squares (OLS) to compare treatment and control firms in the cross-section (specification (1)) and using panel regressions exploiting within-firm variation (specifications (2) to (5)). We control for general changes in the business environment by including year fixed effects in specifications (4) and (5). In the last specification, we add firm size as an additional control given that we observe some differences with respect to size of treatment and control firms. In all regressions except in column (3), we cluster standard errors at the firm level; standard errors are bootstrapped in specification (3).

We start our analysis by investigating changes to the management of working capital (WC) in panel A of Table III. The coefficient of interest is the interaction term, corresponding to a difference-in-differences estimate. In columns (1) to (5), we scale WC by assets in the previous year, and in columns (6) to (10), WC is scaled by contemporaneous sales. When we scale WC by assets, we find a point estimate of $-0.194$ in specification (1) that is significant at the 5% level. The impact is economically significant: it corresponds to a negative impact on working capital of 0.51 standard deviations. Columns (2)-(5) show firm fixed effect estimates. We find slightly smaller coefficients between $-0.156$ and $-0.175$, corresponding to negative effects between 0.41 and 0.46 standard deviations. The estimates are statistically significant at the 5% level across firm fixed effects specifications and year dummies. Columns (6)-(10) show the impact of the treatment on working capital scaled by sales. Consistently, the effects are negative and significant at the 5% level. In panels B and C of Table III, we analyze the different components of working capital in greater detail. We find large and significant effects on accounts receivable (A/R). The difference-in-differences estimate is approximately $-17$ p.p., corresponding to a drop of approximately 0.57 standard deviations or a reduction of roughly 60-65 days in the collection period. We do not find any significant effect on accounts payable (A/P). We can only speculate why firms change A/R but not A/P after the intervention. One potential reason that we further investigate in our survey analysis is that firms can more easily change their own terms (with clients), while negotiating longer payment periods with suppliers might be more difficult. Another reason is that firms may increase their efforts to collect current outstanding accounts receivable for instance by hiring additional per-

sonnel, which we document in our survey results. Finally, we also find a negative effect on inventories. The point estimates range between $-0.093$ and $-0.101$, corresponding to a decrease of about 0.38 standard deviations, and are significantly different from zero at the 5% level. To take into account any variations in the data that arise from randomization itself, we report randomization-t p-values using the algorithm by Young (2019) in Table A3 in the Appendix.[26] As an alternative to the difference-in-differences estimator in our main specifications, we also report results of an ANCOVA estimator in Table A4 in the Appendix and find consistent evidence.

Overall, the results regarding working capital management suggest that firms respond to the treatment by decreasing the collection period, as well as their inventories. The result for inventories is consistent with Bloom et al. (2013). This reduction in working capital mechanically leads to a cash inflow, potentially affecting other corporate polices beyond a direct effect of the treatment.

Table IV reports the impact of the treatment on other firm policies: leverage, cash holdings and total investment in fixed assets (capex). Panel A shows that the effect of the intervention on the capital structure (leverage and cash holdings) is not statistically significant. This finding does not necessarily indicate that firms do not adjust their capital structures in response to the treatment. Indeed, different companies could react to the treatment by adjusting their leverage, for instance, in different directions given that some companies might be below their optimal leverage level, while other companies are above. However, we also make use of additional survey answers (available in Section 4.4) to further investigate whether firms implemented changes in capital structure. Those results are consistent with the accounting data evidence and only three companies stated that they implemented changes. Some firms are subsidiaries of larger (often international) corporations and do not have discretion over these policies. Moreover, many firms argue that credit markets in Mozambique are tight, and it is very difficult or too expensive to obtain debt.

Given that companies do not seem to change their capital structures or their cash holdings in response to the inflow of cash generated by the reduction of their working capital, it is interesting to investigate how this cash is used instead. Companies could in-

[26] For most outcomes, significance levels remain unchanged. For inventories p-values fall below 5% when using randomization-t p-values.

crease their dividends, use this cash to invest in fixed capital, or engage in other expenses. Although we do not have payout or granular expense data, we can analyze long-term investment (capital expenditures). In panel B of Table IV, we document a positive and significant treatment effect: firms that were part of the treatment group increased their capital expenditures by between 12 and 14 percentage points compared to the control group. This outcome corresponds to a positive impact on capital expenditures of 0.47 standard deviations.

We estimate an average positive impact on cash flows of 1.13 million USD from accounts receivable and 0.98 million USD from inventories. Though this might be perceived as a large number, note that the reduction in accounts receivable might be related to the collection of existing receivables, potentially late ones, or the negotiation of new contracts with shorter collection periods. Even when using the lower bound of the confidence intervals as a conservative estimate, the total impact on cash flow from changes in working capital is 0.19 million USD, which is a short term, one-off effect on cash flow. We also estimate the corresponding impact on cashflows from the increase in capital expenditures. We find an average cash flow impact of $-0.81$ million USD, with a conservative estimate (lower bound of the 95% confidence interval) of $-0.21$ million USD.

### 4.2 Performance of Implemented Changes in Financial Policies (Accounting Data)

Whether the implemented changes led to policies that are more efficient or not is not clear ex ante. For instance, reducing inventories and collecting receivables earlier will increase free cash flows in the short run. However, there might be adverse effects in the long run if inventories become too low or if collection periods are too short: Customers might be scared away because of products being out of stock or unattractive payment options.

To test whether firms have indeed moved toward more optimal policies as a response to the treatment, we analyze whether treated firms become more efficient relative to the control group. Given that most firms are private, we do not observe their market values. Hence, we rely on accounting ratios, such as return on assets (ROA) and return on invested capital (ROIC), to measure firm efficiency. We also analyze sales growth to test whether there are any adverse effects on sales.

Table V reports regression results on firm performance. Panel A shows the treatment effect of the intervention on ROA. We find a positive impact on firm performance between 0.21 and 0.23 using OLS and firm fixed effects, respectively. The effect on ROA is also statistically significant at the 5% level. The effect is equivalent to about 0.88 standard deviations of ROA. In Panel B, columns (1)-(5) show results using a measure of return to capital invested (ROIC). The estimated coefficient is between 1.27 using OLS and 1.36 using firm fixed effects, representing between 0.65 and 0.69 standard deviations of ROIC. This effect is statistically significant at the 10% level and at the 5% level when we estimate randomization p-values (see Table A3 in the Appendix). The point estimates of those treatment effects are large but not implausible, particularly given that the confidence intervals include more modest estimates as well. Last, we analyze sales growth to test whether there are any adverse effects of reducing inventories or collecting receivables more quickly. Table V Panel B reports the results. We do not find evidence of such an effect in the short run. The point estimates of the intervention on sales growth are positive, although they are not significantly different from zero. We also do not find a negative effect on sales growth in the two years after the treatment, as the point estimates are smaller but still positive (table A14). However, we cannot exclude that sales may decrease over a longer horizon and the fact that during the second year post treatment there might be some contamination due to part of the control group being treated.

Overall, the results suggest that the finance expertise of managers affects financial policies, in particular, short-term financing policies. These policy changes can improve firm performance by allowing firms to undertake value-enhancing investment projects through improved firm liquidity.

### 4.3 Intentions to Change Financial Policies (Exit Survey)

We complement our previous analyses with survey data to evaluate the intentions of treated firms to change financial policies. While financial statements have the advantage of being standardized data, they do not allow to directly observe changes in some financial policies such as capital budgeting and valuation or risk management. Therefore, we use survey data to analyze intended and effective changes in valuation techniques, working capital management, capital structure, and risk management. Those four topics correspond to the main topics of the course outline.

Table VI shows the results of the exit surveys by the participants at the end of the courses. Panel A of Table VI presents the results for the first cohort that was treated in May 2017 (treatment group). The survey reveals several interesting findings: i) There is great heterogeneity in terms of firms' ability to implement changes across different policies. "N/A" denotes cases in which firms argue that they are unable to adjust a particular policy. Capital structure appears to be the policy over which managers have the least discretion. Around 38% of the companies (13 of 34) state that they cannot change the capital structure themselves. Survey questions that aimed to understand the origins of these constraints suggest that some companies are subsidiaries of larger firms (often international firms) and do not have the flexibility to set their own capital structures. ii) Managers aim to implement changes in all financial policies. Among firms which have the discretion to set their own policies, disregarding missing cases, between 38% and 73% intend to implement changes in their policies that were discussed in the course. When we treat missing answers as "no", i.e., as non changes, the corresponding numbers are between 48% and 73%. iii) There is substantial heterogeneity across different policies in the intention intensity. Working capital management and risk management are the policies that managers intend to change the most (73% and 70%, respectively). There is lower intention to implement changes in capital structure and valuation techniques (48% and 42%, respectively).

Panel B shows the corresponding results when we add the answers of the second cohort (November 2018/April 2019). While there are some minor differences in the level, the qualitative picture remains robust.

Overall, the exit surveys provide strong evidence that firms intend to change their financial policies after the treatment. Those results are interesting in themselves and increase our confidence in the accounting results given that the intentions and in particular their heterogeneity are in line with the results obtained from accounting data in the previous section.

### 4.4 Changes of Financial Policies (15-month Survey)

On top of the evidence from accounting data, we also use additional survey evidence to measure whether treated firms implemented changes in financial policies. We surveyed participating companies, i.e., treatment and control firms, approximately 15 months after

the first intervention (and before the second intervention). There are potential reasons why firms might end up not implementing intended changes. For example, firms might not have the resources or the personnel to do so, there might be other items on the agenda with higher priority, or external conditions might impose constraints. Moreover, there could be reasons unrelated to the treatment that led firms to change their policies. To better understand the effect of the treatment itself, we explicitly asked treatment firms whether they changed firm polices *because* of the course. Similarly, we also surveyed the population of control firms and asked about changes in the preceding 15-months, allowing us to compare changes in financial polices between treatment and control firms.

Table VII shows the results. First, between 7.7% and 30.8% of the firms mention that they had implemented changes in financial policies in the preceding 15 months. Not unexpectedly, the implementation rates are smaller compared to the intentions reported in the exit survey.[27]

Consistent with the exit survey as well as the evidence from accounting data, working capital management is the most affected policy (approximately one third of treated companies that answered the survey state that they have implemented changes in their working capital management). There are fewer adjustments to capital structure decision and valuation techniques, consistent with the exit survey and accounting data. With respect to risk management, which ranked very high on the list of intentions to change at the exit survey, only very few companies (two companies) stated that they had implemented changes 15 months later. In the survey, we also asked for reasons that prevented firms from implementing planned changes. One main reason for not changing risk management practices appears to be a limited supply of hedging instruments/products on the Mozambique market. Second, analyzing the motivations for implementation changes in financial policies, firms seem to respond to the treatment. Almost all of the firms that reported that they had implemented changes in financial policies declared that they did so because of the course (second column of Table VII ). The changes in the different financial policies are not concentrated in just a few firms. In total, 54% of the treatment group report to have implemented changes in at least one policy. Overall is also treasuring that the survey evidence is consistent with the changes in financial polices measured through

[27]These results require careful interpretation due to attrition (we have not been able to reach some companies in the treatment group) and manager turnover. It might also be the case that managers forgot about implemented changes after the course or may felt those were minor.

accounting statements.

While these results are suggestive of a treatment effect, we can also use the control group to address the concern that we could capture a pure time-effect. It might be the case that changes in the economy led companies to change their financial policies, irrespective of the treatment. We conducted the survey for the control group at the same time as the survey for the treatment group, before the second intervention in November 2018 (when the control group participated in the course). The middle panel of Table VII shows the corresponding evidence for the control group. Only two firms reported that they have implemented changes related to financial policies (working capital management and valuation) over the preceding 15 months. The right panel of Table VII tests for significant differences between the means of treatment and control groups (using a one-sided t-test). We find a large and significant difference of 27.1 percentage points of firms having implemented changes in working capital management. This difference is significant at the 1% level. With respect to working capital management, additional open questions in the survey revealed that the main issue that most companies identified for themselves after the course was long collection periods. Companies aimed to overcome this problem in several ways, e.g., by: i) tracking (late) payments in a more systematic manner; ii) defining shorter payment terms; or iii) hiring additional personnel for accounts receivable (A/R) management. The differences in terms of changes in capital structure, risk management, and valuation techniques are smaller and less significant.

Overall, the comparison of the treatment and control groups is consistent with the view that attending the course led firms to change certain financial policies, especially those over which they have discretion. Moreover, the 15-month survey results are in line with the intentions by the treated firms to change financial policies during the exit survey, immediately after the treatment. Implementation rates are, however, lower compared to the intentions. These results are also consistent with the observed changes in financial reports which mostly show a decrease in working capital for treated firms and no significant changes in other financial policies.

### 4.5 Alternative Interpretations and Further Robustness Checks

While the experimental setup theoretically identifies the causal effect of the financial education program on financial policies, there might be certain limitations that could

affect the internal validity of the experiment in practice. In this section, we discuss these threats in more detail and provide additional tests on the internal validity. We also discuss the interpretation of main results and present further robustness tests.

### 4.5.1 Compliance

In our main analysis, we estimate the average treatment effect on the treated (ATT). While we have a very high compliance rate of approximately 91% there is the concern that the firms from the treatment group that did not attend the course bias our results. Ex ante, the direction of this potential bias is unclear. For instance, it might be the case that only very good firms, despite the initial enrollment, do not attend the course because they do not expect a large benefit from participating in the course; it might also be the case that firms that are in trouble do not attend the course since their CEOs are too busy otherwise. Badly performing firms dropping out of the sample could indeed be in line with our results on ROA, but it would be more difficult to tell a consistent story about why these firms also decrease their working capital. In practice, however, we do not find evidence of any systematic reasons for why firms that initially enrolled in the course did not attend. For instance, one CEO had an unexpected meeting abroad during the period of the first intervention.

We can include these four treatment firms that did not attend the course and estimate the intention-to-treat (ITT) effect of our intervention.[28] Table A5 in the appendix shows the results for our main variables of interest. We find similar and significant effects.

### 4.5.2 Attrition

While all participating firms signed a data agreement at enrollment in the program, not all firms shared their data in the end. There is the concern that compliance with sharing their financial data is systematically different for firms from the treatment and control group. In the case of *ROA*, for instance, it might be the case that well performing firms are more likely to share their financial data with us. If this was true for firms from both the treatment and the control groups, the difference-in-differences estimates might be still unbiased. However, it would be concerning if badly performing firms from the treatment

[28] Bruhn et al. (2018) estimated ITT as their main specification.

group were more likely not sharing their data. In this case, sample selection could bias our findings.[29]

We address this potential threat to the internal validity of our experiment in the following way. For a substantial subset of firms, we also have access to external accounting data from a large accounting firm, which are not self-reported by the firms to us. These data should not suffer from the concerns described above. We can use these external data to estimate a difference-in-differences effect for a subset of outcomes. Unfortunately, the granularity of the external accounting data does not allow us to estimate the effect of the intervention on all of the different components of working capital. Table A6 in the appendix shows the results for working capital and *ROA*. The point estimates have the same signs and are larger in absolute terms than our estimates when using all of the data, suggesting that – if anything – we might be underestimating the magnitude of the effect in our baseline specifications.

#### 4.5.3 Firm Heterogeneity in Small Samples and Pre-trends

As described in Section 3, we randomized the treatment status among firms that signed up for the program, and by construction, there should be no systematic differences between treatment and control firms. However, in small samples, this assumption is not necessarily true. There is the concern that, merely by chance, potential heterogeneity between the treatment and control groups might partly drive our findings. Table II shows that, overall, firms and the managers of those firms are not systematically different. Almost all of the differences in means and especially at the median are not significantly different between the two groups and normalized differences do not exceed one as well. An exception is size. The three largest companies in our sample (with total assets over 800 million USD) were assigned to the control group. Our results remain quantitatively unchanged when we exclude these three companies from the analysis (Table A7).[30]

We can also make use of the panel dimension of our data and test whether the treatment and control firms were on common trends before the intervention. For the validity of our experiment, it would be acceptable if the two groups were on different levels, as

[29] As stated in Duflo et al. (2007), "random attrition will only reduce a study's statistical power; however, attrition that is correlated with the treatment being evaluated may bias estimates."

[30] The normalized difference in total assets between the treatment and control groups drops from -0.43 to -0.27 when we exclude these three companies.

long as they were not on different pre-trends (common trend assumption in difference-in-difference tests). We test this assumption nonparametrically by plotting corresponding graphs for the main outcomes. Moreover, we make use of data of non-participating firms, i.e., firms which were not invite due to geographical constraints or which decided not to participate. Those firms provide an additional (though non-experimental) benchmark. Figure V shows averages of selected financial policies for firms in the treatment and control groups over the 2014-2018 period. The figures illustrate that, despite some differences in levels before the intervention, the treatment and control groups usually have parallel trends (especially during the year before the intervention between 2016 and 2017). One exception is capital expenditures, for which trends between the two groups appear to be different. However, in this specific case, the treatment group was actually on a negative trend before the intervention, while firms in the control group slightly increased their capital expenditures on the year before the treatment. Overall, the graphical analysis suggests that the parallel trends assumption is not violated since the treatment and control groups follow parallel trends before the intervention across a majority of outcomes of interest.

A different concern is that there are changes in the economy that coincide with the timing of our treatment and that affect firms in the treatment and control groups differently. Two dimensions in which treatment and control firms appear to differ are firm size and the nationality of the CEO. For instance, there is the concern that changes in the business environment allow small firms, for instance, to outperform larger firms after 2017. This difference in average size between the treatment and control groups is mostly driven by two large firms, which by chance were assigned to the control group, and excluding these firms does not change our results. However, we also aim to test more directly whether firms with certain characteristics change their behavior post-2017. Given that we observe the largest differences with respect to firm size and the nationality of the CEO, we include different, flexible functional forms of firm size as well as the nationality of the CEOs in additional tests.

We report the results of those tests in Table A8. In specification (1), we include different functional forms of contemporaneous measure of firm size (*Assets*) and allow for a differential impact of those measures in the post-treatment period by also including an interaction term ln *Size x Post*. In specification (2), we allow for a different functional

form and include *Size*, $Size^2$, and $Size^3$ as well as their interactions with Post in the regressions. Given that the treatment may affect the size of the companies, we use a measure of size before the treatment, i.e., measured in 2016 instead of contemporaneous measures in specifications (3) and (4). Finally, in the last specification, we include a dummy variable for CEO being *Mozambican* as well as its interaction with the *Post* dummy in the regression.

Panel A shows the results for working capital, panel B for the average collection period, and panel C for ROA. The estimated treatment effects of our intervention do not change much and remain significant.[31] Overall, our tests alleviate the concern that the documented effects are unrelated to the treatment itself but are driven by some heterogeneity in the treatment and control groups due to a limited sample size.

#### 4.5.4 Staggered Implementation of the Treatment

As explained in Section 3, we implemented the treatment in a staggered way. The main motivation was the provision of incentives for the control group to share their accounting data and participate in the surveys (Duflo et al. (2007)). However, there are other relevant consequences for identifying a treatment effect.

On the one hand, there is the general concern that the managers of treated companies change their behaviors and update their expectations differently from the control firms because of the intervention. Indeed, managers might update their beliefs with respect to future firm performance and respond accordingly (see Chemla and Hennessy (2019)). For instance, if CEOs believe that they will be able to have better access to credit markets in the future, they may already start investing today. Moreover, the planned participation in the course might remind CEOs or make them aware of the importance of financial policies. As a consequence, they might change financial policies unrelated to the actual participation. In our setup, this problem is less prominent since both the treatment and control managers expect to receive the same treatment, though there was some uncertainty for the control group with respect to the exact date of the course.

On the other hand, there is the concern that control firms, which will be treated at a later stage, put some already planned changes of financial policies on hold if they expect to improve decision-making after the participation in the course. Overall, we do not think

[31] The only exception is specification 5 (CEO being Mozambican) of panel A.

that this a big concern in our context as well.

First, we can make use of another set of firms to test whether the control firms in our sample change their policies in anticipation of being treated at some point in future. For financial outcomes for which we have data for non-participating firms, i.e., for firms that were not eligible for or decided not to participate in the program, we compare their pre- and post-trends with our control firms. Figure V shows similar trends before and after the first intervention for the control group and non-participating firms, which does not support the idea that control firms put actions on hold. Second, if firms were already anticipating a large positive effect of financial education, it is less clear why they had not already participated in such a course before. Third, we were not specific about the date of the course for the control group (we just informed they were allocated to a second cohort).[32] It is therefore less credible that firms delayed potentially important decisions because of the course. Finally, when we interviewed the control group in the 15-months survey and asked about implemented changes, none of the control firms mentioned that they put changes on hold because of the anticipated participation in the course.

#### 4.5.5 Contamination

Another concern is that our experiment suffers from "contamination". For instance, we cannot fully exclude the possibility that treated managers shared their knowledge or course materials with managers in the control group because Maputo is a relatively small city. However, this would work against us finding any strong result (and we found no evidence that control firms changed their behavior compared to a set of non-participating firms). We prevented the most likely contagion to occur by performing randomization at the business group level, instead of at the firm level. This procedure implies that all managers from the same business group are part of the same cohort. Moreover, the treatment occurred in a classroom setting, with an instructor; therefore, it is unlikely that the control group would have access to the same treatment as the treatment group. The most plausible type of interaction between the treatment and control groups could be the sharing of materials, for which close substitutes were already available, either online or in textbooks.

[32] In our email correspondence we stated that "the second edition of the course will be held at a date to be confirmed, based on the availability of both parties."

#### 4.5.6 Further Robustness Tests

We run a battery of additional robustness tests.

To take into account any variations in the data that arise from randomization itself, we report randomization-t p-values using the algorithm by Young (2019) in Table A3 in the Appendix. Results remain largely unchanged; p-values for inventories and *ROIC* drop below the 5% level when using this alternative estimator.

When auto-correlations are low, there can be large improvements in power by using ANCOVA instead of difference-in-differences estimators (see McKenzie (2012)). We report those alternative estimates in Table A4 in the Appendix. Results remain robust.

Some firms belong to the same business group. As a robustness test, we exclude all non-core subsidiaries from our data. Table A9 shows the results for the main outcomes of interest. The results are unchanged (the point estimates are even slightly higher).

We also consider different time windows in our estimation of the treatment effect. Table A10 shows results for our main outcomes when we consider data after 2013, 2015, or 2016. While the point estimates slightly change depending on the estimation window, qualitatively, the results remain unchanged.

A few firms experienced CEO turnover during the period of the experiment. As a robustness test, we exclude them from the analysis. Table A11 shows the results, which remain qualitatively unchanged. We have fewer observations, however, and some coefficients are only significant at the 10-percent level.

Though we do not include banks in our main sample, we can further exclude firms that operate in the financial industry (e.g., insurance companies). Table A12 shows the results. The main results remain unchanged.

To further address the robustness of our empirical measures, we use alternative definitions of financial ratios to measure working capital and accounting performance. In our main specifications, we use the lagged value of the book value of assets in the denominator. Table A13 displays the results when we scale the outcomes by contemporaneous book value of assets. The results are consistent with our baseline definition.

# 5 Policy and Welfare Considerations

## 5.1 Heterogeneous Effects

While our sample is likely too small to detect significant heterogeneous treatment effects, analyzing subsamples of interest might still be informative for understanding which firms and managers are more likely to benefit from the treatment. We split the sample along four dimensions: first, we analyze whether firms that are more likely to be financing constrained are benefiting more from the treatment. We proxy financing constraints by firm size (using both assets and employment) and previous cash holdings and leverage. We conjecture that firms that are financing constrained may benefit relatively more from improved financial policies. Second, we analyze whether executives with expertise in finance profit more from the course. We proxy expertise in finance either by previous experience as CFO or educational background in finance. With respect to financial expertise, we do not have a strong prior. On the one hand, the course could be reinforcing previous experience in finance. On the other hand, it might be the case that the learning effect is particularly large for executives who were not exposed to finance before. Third, we test whether previous general education is important for grasping and implementing corporate finance theory. For instance, previous research has shown that formal education and more sophisticated topics were less successful in the context of finance education for micro-entrepreneurs who, on average, have relatively low levels of education. Last, we test whether managers who have more discretion in changing firms' financial policies profit relatively more from finance education.

Figure VI shows the point estimates as well as confidence bands (at 90% and 95% confidence levels) for different subsamples. Each point estimate and confidence band originates from a separate estimation. The dependent variable is return on assets (ROA) and we use ANCOVA which is expected to increase power in settings of low autocorrelations.[33]

The results can be summarized as follows. Point estimates are positive across subsamples suggesting that finance education is valuable to all groups of managers or firms. Also, firms that are expected to be financing constrained are benefiting relatively more from the finance course. We find that smaller firms and firms with low cash holdings

[33] Please refer to Section 4.5.6 for more general robustness tests using ANCOVA.

or low leverage have a more pronounced effect in ROA.[34] Third, we find that executives without prior expertise in finance profit more from finance education. This suggests that parts of the topics are likely to be already known by individuals with prior finance experience - maybe not surprisingly given that parts of the course were intentionally relatively standard. At the same time, the results suggest that the positive effects are driven by participants who learned something new rather than by participants who "reactivated" knowledge that they had learned before. Fourth, we find that participants with a high degree of formal education (Master's degree or higher) profit more from our intervention suggesting that a certain level of education (or cognitive ability) might be needed to grasp the theoretical concepts and their implications, and to implement them in practice. This is consistent with prior literature on financial education of micro-entrepreneurs that documents that formal education on sophisticated theories has a lower impact compared to less formal education and rules of thumbs, potentially because of cognitive barriers (see Carpena et al. (2011)).

We also find that firms whose executives have more discretion about setting financial policies ("Discretion over policies") profit more from attending the course. Motivated by this result, we match financial reporting data with the 15-month survey answers ("Changes in at least one dimension (15-Month Survey)"). We observe a large and significant coefficient on ROA (at the 95% confidence levels) among the group of companies that reported to have implemented changes in at least one of the dimensions discussed in the program. We find a non-significant negative effect among companies reporting no changes. These results are also consistent with our survey evidence showing that some participants would like to change certain policies in response to the intervention but were unable to do so because those policies were decided at the business group level. This evidence further increases our confidence that the observed changes in ROA are driven by changes in financial policies triggered by the intervention and not due to unobserved events affecting systematically one of the groups (due to small sample issues, for instance).

[34]The coefficient is statistically significant from zero at 90% confidence level in several subsamples, namely "Small Assets" and "Low Cash".

### 5.1.1 Are the observed effects persistent?

We use financial data from KPMG reports to test whether the observed effects persist two years after the treatment. Table A14 shows the results. Columns (1)-(3) show the effect on working capital. We find persistent effects in working capital but estimates are less precise for the second year. The 2nd year post-treatment effect coefficient is negative, and in the first specification of similar magnitude, but not statistically significant across specifications. This result suggests that there is a short-term impact of education on working capital with positive impact on liquidity that is not reversed in the subsequent year. Columns (4)-(6) show the results for firm performance measured by ROA. The 2nd year post treatment effect is still positive and of slightly smaller magnitude, but only significant in the specification without firm fixed effects.

One needs to be cautious when interpreting long-term effects due to the staggered nature of the intervention (Duflo et al. (2007)). Because both treatment and control group receive the intervention at a certain point in time, at that stage the control group is no longer a valid counterfactual. In this experiment, part of the control group was treated at the end of the second year, and before financial data for 2018 is reported. Another reason why we might have less precise estimates is the fact that we only rely on data from KPMG to estimate the long term effects. Unfortunately we do not have enough post-treatment data for the control group so that we could use it to estimate treatment effects.

## 5.2 Policy Considerations

While the experimental design helps to identify the treatment effect of the intervention, it remains unclear through which channel the executive education course on corporate finance exactly affects financial policies. While answering this question is interesting in itself, it also has important implications for policy.

The treatment, i.e., participation in an executive education program, is a bundle of different simultaneous experiences: i) potential learning from the instructor; ii) potential learning from classmates; and iii) other aspects of the classroom experience unrelated to the content of the course, that could affect outcomes (e.g., Cai and Szeidl (2018) provides experimental evidence that networking leads to new business development between par-

ticipants). Therefore it is difficult to identify the exact learning channel. However, we have several pieces of evidence that suggest that networking (which is the mechanism least associated to learning) is *not* the main driver of our findings. While the results regarding *ROA* could be consistent with the hypothesis that treated firms interact with each other to generate new business, we do not find a significant impact on sales (see Panel B of Table V).[35] Moreover, the documented changes in working capital are also not easy to reconcile with a networking story. In addition, we organized an event for the firms from the control group that occurred around the dates of the first intervention. This event gave control firms the opportunity to get to know each other and network as well.[36] Considering the previous arguments, the support for a networking explanation of the findings appears rather limited.

The importance of the classroom setting versus learning the content elsewhere, e.g., by self-studying or by enrolling into online education, is related to the question about the frictions that prevented executives from obtaining education in finance earlier. One potential reason is simply unawareness of the importance of finance education for corporate efficiency. In this case, self-studying or enrollment in online courses appears to be a good and inexpensive way of implementing financial education. Another reason could be the limited supply of such programs in Mozambique. Indeed, in Mozambique, there are no comparable executive education programs on finance. Online courses or textbooks might only be very imperfect substitutes for a classroom education led by a professor and using case discussions and active participation. To the best of our knowledge, the closest available programs are based in South Africa, and the expected costs (both in terms of money and time) are higher.

A second interesting question is whether participants learned something completely new or whether they were only reminded of the importance of some financial concepts. A hybrid version of these two extreme ends would be cases in which executives learned the foundations during (pre-experience) university degrees, but only the professional experience combined with a more applied teaching method (e.g., case-based) allowed them to apply the theoretical concepts in practice. We believe that a pure reminder (and versions thereof, such as the uptake of self-studies after enrollment into our program)

[35] We also look into sales growth over a two year period and we do not find significant changes

[36] A remaining caveat is that the placebo event was shorter than the 18-hour course for the treatment group, and there might have been fewer opportunities to establish relationships.

cannot explain the findings. Indeed, one advantage of our setup is that the control group knows that it will be treated as well, and enrollment in the program would remind both the treatment and control group. Moreover, we find that firms run by executives without any prior experience in finance (as measured by CFO positions or work experience in the finance sector) profit relatively more than those with already experienced executives in finance.

From a policy point of view, it is not only important to know how to increase finance education among executives but also whether such an improvement in finance education is welfare improving. While we cannot really answer this question with our setup, we believe that it is still valuable to speculate about potential welfare implications. First, we show that treated firms manage to run their firms at a lower level of inventories freeing up resources. The impact on inventories are in line with the observed effects in Bloom et al. (2013). At the same time, capital expenditures increase, potentially enhancing the productivity of firms as suggested by increases in ROA and ROIC. Moreover, there might also be other policies that are more difficult to measure and that benefit from improved financial decision making. Last, we document large effects of the intervention on working capital, and specifically accounts receivable. Welfare implications of those changes are less clear. If the accounts receivable of one firm go down, the accounts payable of its customers must decrease as well, and the overall impact on societal welfare is somehow unclear. However, some customers are likely from abroad, including customers or firms from developed countries that plausibly have better access to external financing. In such cases, the economy of Mozambique is likely to benefit. Moreover, our analysis on heterogeneous effects show that results are more pronounced for small firms in our sample, which are likely to face greater constraints in access to external funds.

Whether our findings can be generalized to other firms inside or outside Mozambique is a very challenging question. Nevertheless, we provide several tests that support the external validity of our findings, at least with respect to other firms in Mozambique. When we compare firms that applied to our program (participants) with other firms present in the KPMG reports that were not eligible because headquartered outside Maputo or that did not apply (non-participants), we do not find them to be significantly different in terms of observable firm characteristics. We also compare the characteristics of participating executives with those of executives from other firms in Mozambique

using additional data from LinkedIn. Table A15 shows the results. We do not find that firms that chose to enroll into the program are significantly different from other firms. Using the full LinkedIn sample, we also find no significant differences in tenure or MBA training. When we restrict the sample to include only firms with at least 25 employees or 100 followers registered on LinkedIn, to better match our own sample in terms of firm size, we find no significant differences between the two samples except for gender. More interestingly, we also compare participants in our program with the U.S. sample from Graham and Harvey (2001). The results are presented in panel C of Table A15. When we restrict the U.S. sample to firms of similar revenue to our sample, we do not find any significant differences in tenure or level of education.

Overall our sample of firms and managers seems to be comparable to other firms and managers in Mozambique in regard to observable characteristics. Compared to the U.S., we also find managers' characteristics to be similar to those firms of similar sizes in Mozambique. Given that financial market development in the U.S. is very different from the one in Mozambique, we do not claim that the conclusion of our study can easily be applied to the U.S. and further research is required.

## 6 Conclusion

This paper evaluates the impact of managers' financial education on firm financial policies and performance. A randomized controlled trial (RCT) with top managers of 93 medium and large companies in Mozambique shows a positive effect on firm return on assets (ROA) of an 18-hour executive education program in finance. Our results suggest that deficiencies in financial expertise of managers at large firms can be an important constraint on firm performance, particularly in contexts with severe financial frictions.

Using accounting data as well as survey data, we find that managers changed firm financial policies after the intervention. We find negative and significant average treatment effects on working capital. The estimated effects on working capital management are large and significant: working capital decreases by 0.41-0.51 standard deviations for the treated firms, compared to the control group. The changes in working capital are due to decreases in accounts receivable and inventory, which have a positive impact on firm cash flows. This effect is likely alleviating, at least in the short run, potential financial

constraints, as most firms in Mozambique report to be financially constrained. Consistent with the idea that financial constraints are alleviated, we also find that treated firms increase their investment in fixed capital. The effects on firm performance are economically relevant as well: ROA increases by approximately 0.88 standard deviations for the treated group, compared to the control firms. Firm performance results suggest that firm financial policies changed efficiently. In addition, we find that CEOs without prior finance experience and firms that face higher financing constrains are benefiting relatively more from the course.

Our results suggest that relatively low-cost interventions, such as an 18-hour executive education course on corporate finance and risk management which has an approximate market value of $10,000, can improve financial practices and decision making and could ultimately affect economic development. In comparison, the experiment by Bloom et al. (2013) conducted in 28 plants operated by 17 firms ran approximately three years with a total consulting cost of $1.3 million, approximately $75,000 per treatment plant and $20,000 per control plant.

While earlier research on financial education in the context of household finance or finance for small and micro firms in developing countries has suggested that generic classroom-based financial education is not working (Zia (2009)), our evidence suggests that this type of education is effective for top managers. There are many reasons that could explain these differences. For instance, the content (corporate finance) is very different, as well as the pool of recipients. While most research in developing countries has focused on poor, relatively less educated households and entrepreneurs, the average manager participating in our program is well educated. This fact might be important since previous research has suggested that cognitive constraints are a key barrier to improving financial knowledge (Carpena et al. (2011)). Understanding what type of education is most efficient remains an important avenue for future research, especially whether online courses that can reach a large audience at a very low cost achieve similar results.

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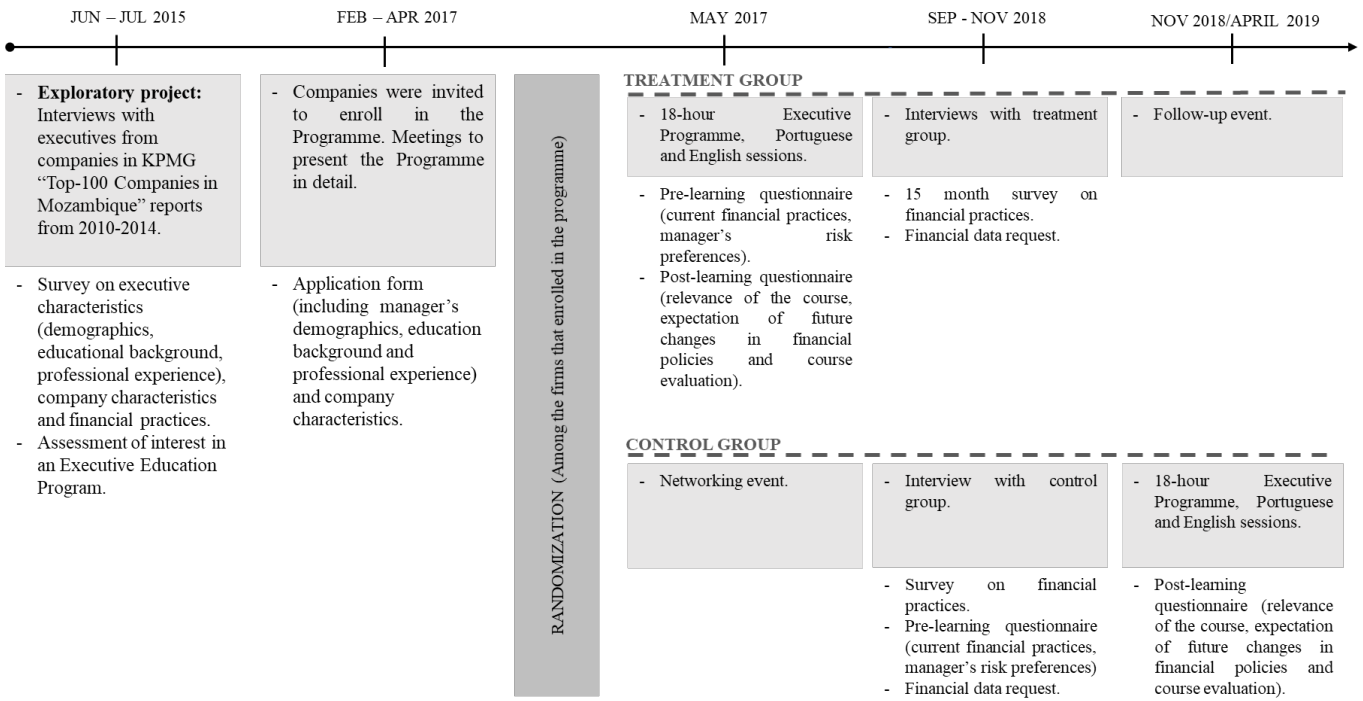
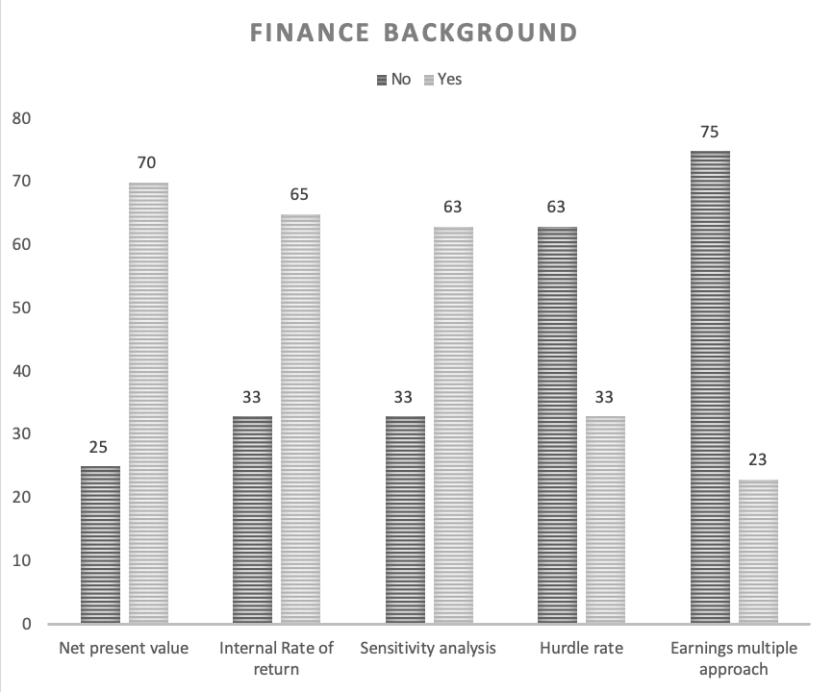
# 7 Figures

**Figure I:** Project Timeline



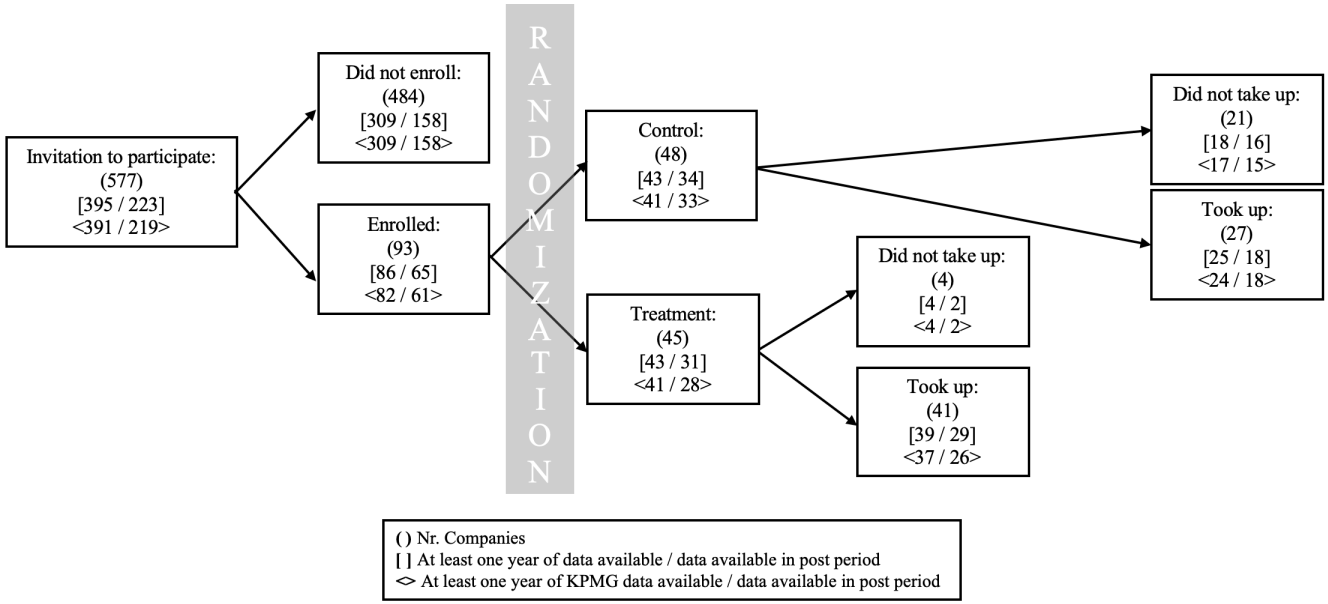

This timeline describes the field work between June 2015 and April 2019. For each stage, it describes the work performed, as well as the information collected regarding companies and managers.

**Figure II:** Financial Experience and Financial Policies



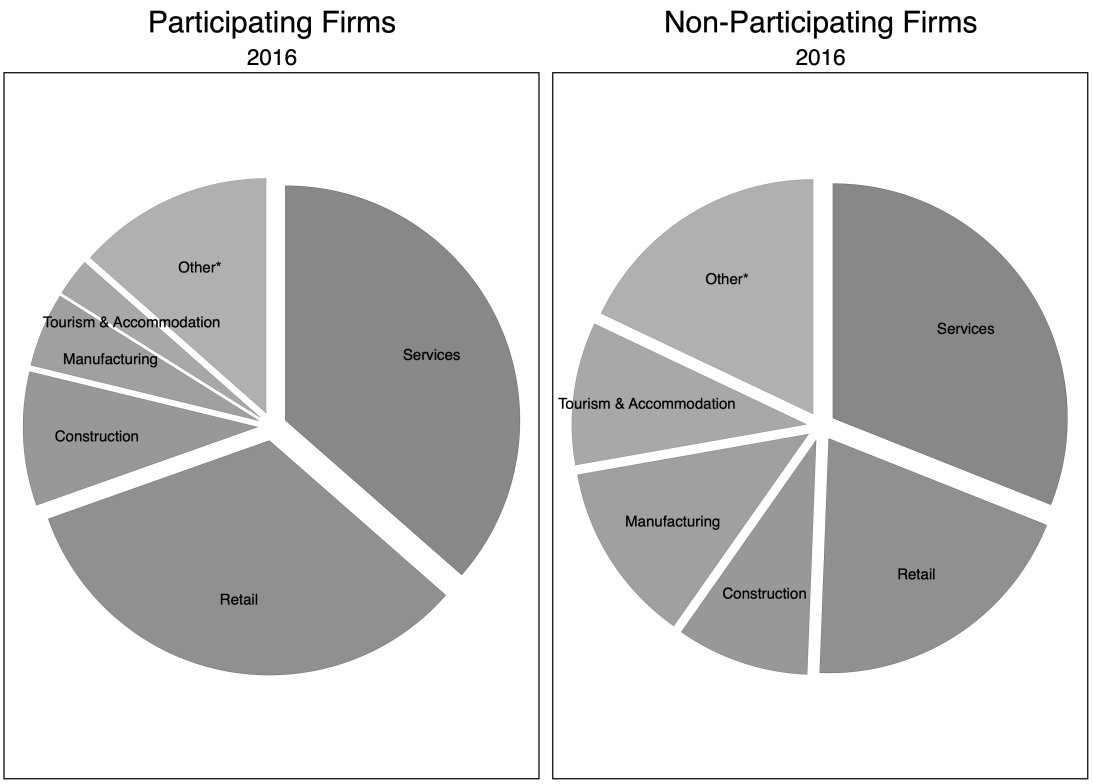

This graph displays the percentage of managers using different valuation techniques according to financial experience. Financial experience is based on previous background in finance, i.e., managers who have attended at least one finance course at any higher education degree. Source: Survey Jun-Jul 2015.

**Figure III:** Numbers of Companies in Different Stages of the Experiment




This diagram shows the number of companies participating in each stage of the experiment (round brackets). It also reports the numbers of companies for which we have at least one year of financial data, either from KPMG or self-reported data (square brackets) or from KPMG (angle brackets). 'Took up' and 'Did not take up' refers to companies assigned to the treatment or control group that did not participate in the respective intervention, i.e., did not attend the course.

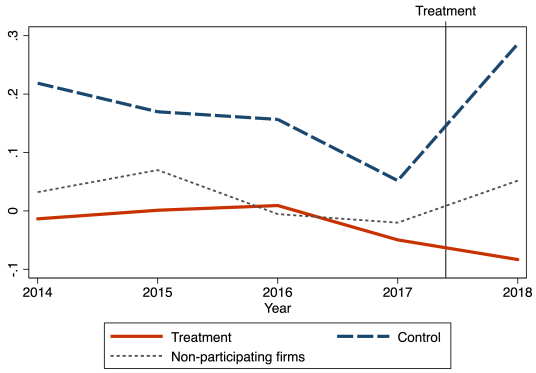
**Figure IV:** Sectors of Activity



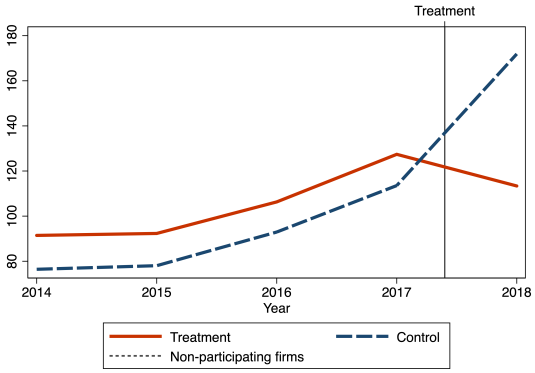

This figure displays the distribution of participating (left) and non-participating (right) firms by sectors of activity.

**Figure V:** Evolution of Selected Financial Outcomes

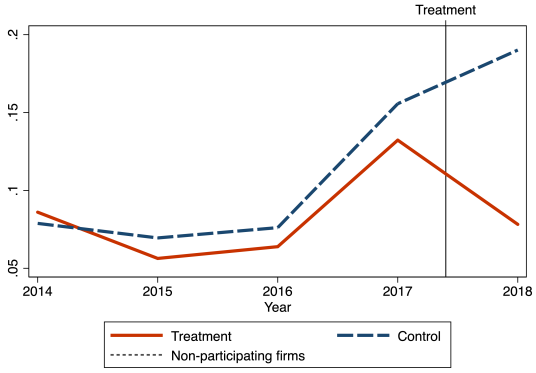
**(a)** Working Capital/(Lag) Assets



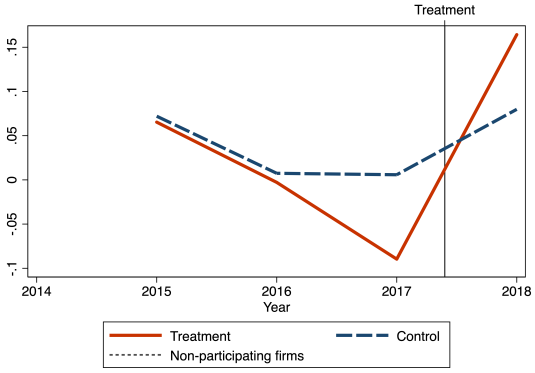

**(b)** Average Collection Period



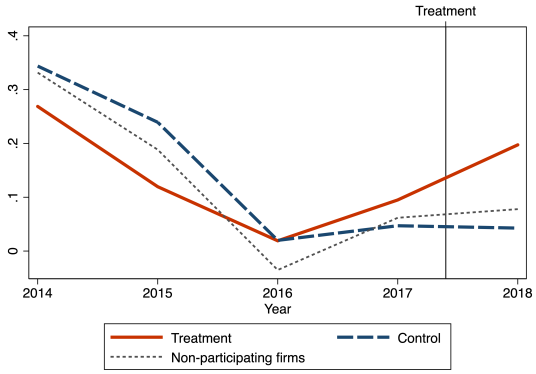

**(c)** Inventories/Sales



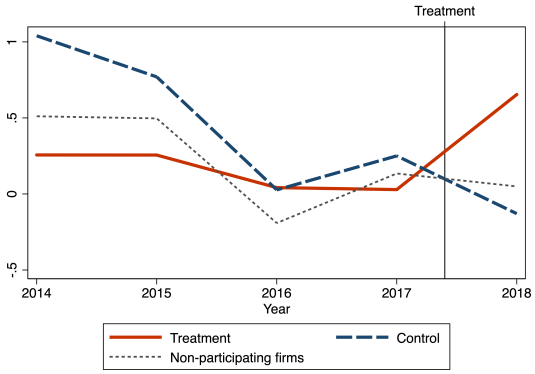

**(d)** Capex/Assets




**(e)** ROA

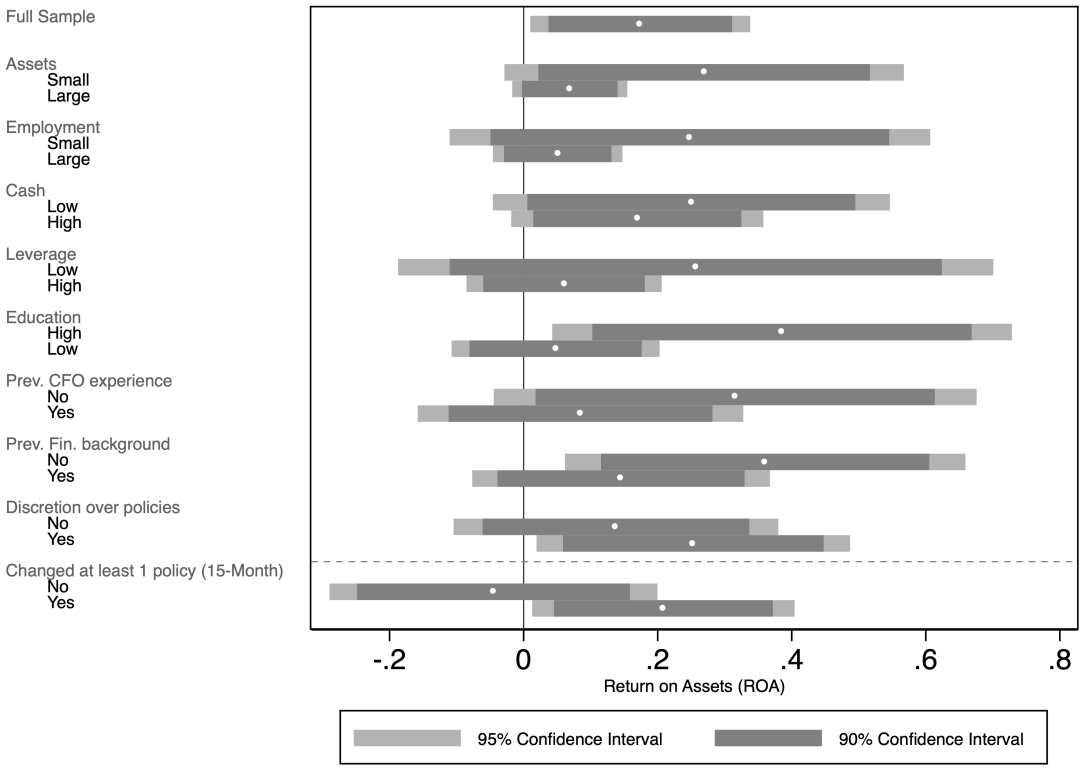



**(f)** ROIC




The graphs present mean financial outcomes over time for firms included in the treatment and control samples. We also present the average for the remaining non-participating KPMG companies (omitted for financial outcomes for which only hand-collected data is available). Financial outcomes are Working Capital, Average Collection Period, Inventories, Capital Expenditure, Return on Assets (ROA) and Return on Invested Capital (ROIC). The vertical line denotes the date of the first intervention (treatment group). On the horizontal axis, each date represents the beginning of each year.

**Figure VI:** Heterogeneous Treatment Effects on ROA




This graph displays the treatment effects of the finance education program for different subsamples. Each bar shows results of a different ANCOVA (the point estimate as well as the 90% and 95% confidence bands). Small/large assets, employment, cash and leverage denote whether a firm is of below/above the median of the respective distribution. High education refers to having obtained a Master's degree (or higher). Previous CFO experience denotes whether the course participant had prior CFO experience. Previous financial background denotes whether the participant has educational background in finance. Discretion over policies is a dummy variable that is equal to one if the participant has discretion over financial policies. Changed at least 1 policy (15 Month) denotes whether participants reported to have changed at least one policy in the 15-month follow-up survey. All manager-level characteristics refer to the top manager in each company.

# 8 Tables

**Table I:** Number of Managers and Companies Participating in the Programme

| Time | What | Firms | Managers |
|---|---|---|---|
| **Pre-Treatment** | **Invitations and applications to the programme; randomization** | | |
| | Companies that applied to the programme | 93 | - |
| | - Treated companies | 45 | - |
| | - Control companies | 48 | - |
| | Financial data | | |
| | - Treated companies | 36 | - |
| | - Control companies | 42 | - |
| **Treatment (2017)** | **Intervention I** | | |
| | - Programme attendees | 41 | 46 |
| | - Control event attendees | 18 | 17 |
| **Post-Treatment** | **15month survey** | | |
| | - Treated companies | 30 | 22 |
| | - Control companies | 39 | 31 |
| | **Financial data** | | |
| | - Treated companies | 32 | - |
| | - Control companies | 35 | - |

The table displays the number of participating companies and managers at different stages of the project.

**Table II:** Baseline Summary Statistics

**Panel A: Treatment/Control**

| | Treatment | | | | Control | | | | Mean | | Median | | Norm. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | **Obs.** | **Mean** | **Median** | **St.Dev.** | **Obs.** | **Mean** | **Median** | **St.Dev.** | **Diff.** | **p-value** | **Diff.** | **p-value** | **Diff.** |
| Total Assets (m USD) | 34 | 22.29 | 4.60 | 47.60 | 42 | 126.33 | 9.86 | 339.06 | -104.05* | 0.08 | -5.26 | 0.49 | -0.43 |
| Sales (m USD) | 34 | 15.84 | 3.12 | 38.94 | 42 | 58.94 | 8.32 | 132.90 | -43.10* | 0.07 | -5.20 | 0.11 | -0.44 |
| Sales Growth | 34 | -0.36 | -0.36 | 0.23 | 38 | -0.15 | -0.27 | 0.69 | -0.20 | 0.11 | -0.10* | 0.10 | -0.39 |
| # Employees | 32 | 191.06 | 81.50 | 248.63 | 38 | 308.26 | 102.5 | 541.02 | -117.20 | 0.26 | -21.00 | 0.81 | -0.28 |
| Cash / Assets | 19 | 0.10 | 0.06 | 0.10 | 26 | 0.17 | 0.11 | 0.21 | -0.07 | 0.21 | -0.05* | 0.09 | -0.40 |
| Leverage | 25 | 0.15 | 0.10 | 0.20 | 32 | 0.26 | 0.11 | 0.37 | -0.10 | 0.20 | -0.01 | 0.91 | -0.35 |
| Capex / Assets | 19 | -0.09 | -0.06 | 0.11 | 24 | 0.01 | -0.02 | 0.23 | -.10* | 0.09 | -.05 | 0.63 | -0.55 |
| Return on Assets (ROA) | 33 | 0.12 | 0.05 | 0.27 | 41 | 0.05 | 0.07 | 0.22 | 0.07 | 0.25 | -0.02 | 0.64 | 0.27 |
| Return on Invested Capital (ROIC) | 32 | -0.04 | 0.04 | 1.40 | 41 | 0.76 | 0.18 | 2.33 | -0.80* | 0.09 | -0.14 | 0.28 | -0.42 |
| Working Capital / (Lag) Assets | 33 | -0.06 | -0.09 | 0.38 | 42 | 0.11 | 0.14 | 0.38 | -0.17* | 0.06 | -0.23 | 0.20 | -0.44 |
| Working Capital / Sales | 33 | 0.00 | -0.05 | 0.86 | 42 | 0.07 | 0.11 | 0.63 | -0.06 | 0.71 | -0.15 | 0.20 | -0.08 |
| Inventories / Sales | 18 | 0.14 | 0.05 | 0.27 | 25 | 0.16 | 0.03 | 0.24 | -0.01 | 0.88 | 0.02 | 0.66 | -0.05 |
| A/R / Sales | 19 | 0.37 | 0.27 | 0.34 | 26 | 0.31 | 0.23 | 0.28 | 0.06 | 0.55 | 0.04 | 0.90 | 0.18 |
| A/P / Sales | 18 | 0.32 | 0.12 | 0.41 | 26 | 0.23 | 0.12 | 0.26 | 0.09 | 0.39 | -0.01 | 0.76 | 0.26 |
| Avg. Collection Period | 19 | 134.3 | 100.04 | 125.15 | 26 | 113.55 | 84.79 | 103.94 | 20.75 | 0.55 | 15.25 | 0.90 | 0.18 |

**Panel B: Treatment/Control Managers (Top Manager)**

| | Treatment | | | | Control | | | | Mean | | Median | | Norm. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Obs. | Mean | Median | St.Dev. | Obs. | Mean | Median | St.Dev. | Diff. | p-value | Diff. | p-value | Diff. |
| Male | 31 | 0.81 | 1 | 0.40 | 36 | 0.75 | 1 | 0.44 | 0.06 | 0.59 | | | 0.13 |
| Age (years) | 30 | 43.73 | 41 | 7.96 | 34 | 45.26 | 43.5 | 10.71 | -1.53 | 0.52 | 2.5 | 0.43 | -0.16 |
| Tenure (years) | 31 | 7.28 | 5 | 5.77 | 35 | 7.60 | 5 | 7.23 | -0.32 | 0.84 | 0 | 0.84 | -0.05 |
| Mozambican | 31 | 0.55 | 1 | 0.51 | 36 | 0.78 | 1 | 0.42 | -0.23** | 0.05 | | | -0.49 |
| CEO | 31 | 0.61 | 1 | 0.50 | 36 | 0.53 | 1 | 0.51 | 0.09 | 0.49 | | | 0.17 |
| CFO | 31 | 0.29 | 0 | 0.46 | 36 | 0.31 | 0 | 0.47 | -0.02 | 0.89 | | | -0.03 |
| CXO | 31 | 0.00 | 0 | 0.00 | 36 | 0.03 | 0 | 0.17 | -0.03 | 0.36 | | | -0.24 |
| Masters or higher | 30 | 0.57 | 1 | 0.50 | 33 | 0.42 | 0 | 0.50 | 0.14 | 0.27 | | | 0.28 |
| MBA | 30 | 0.20 | 0 | 0.41 | 33 | 0.21 | 0 | 0.42 | -0.01 | 0.91 | | | -0.03 |
| Finance Background | 27 | 0.59 | 1 | 0.50 | 33 | 0.61 | 1 | 0.50 | -0.01 | 0.92 | | | -0.03 |
| Accounting Background | 27 | 0.74 | 1 | 0.45 | 33 | 0.58 | 1 | 0.50 | 0.16 | 0.19 | | | 0.35 |

Panel A displays summary statistics for the main financial variables of 93 firms participating in the programme (Treated/Control Sample). Financial data is obtained from KPMG "Top-100 Companies in Mozambique" report, and hand collected. Panel B displays demographic, educational and professional characteristics of top managers reported in the application forms for treatment and control firms (top manager is defined as the most senior participant filling in the application form for a given business group). The (descending) order of seniority considered is CEO, CFO, accountant or related, other directors or staff and sales manager or related. When more than one manager had a top position due to turnover during the project, we considered the manager with the longest reported tenure. The category 'Masters or higher' contains the 'MBA' category. In the last column, we present normalized differences. Normalized differences are defined as the difference in means between the treatment and control groups, divided by the square root of half the sum of the treatment and control group variances (Imbens and Rubin, 2015). All values are reported as of 2016. *, **, *** Significance at 10, 5 and 1%, respectively.

**Table III:** Changes in Financial Policies - Working Capital (Financial Reports Regressions)

**Panel A: Working Capital (WC)**

| | Working Capital/Assets | | | | | Working Capital/Sales | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| Treatment x Post | -0.194** | -0.174** | -0.174** | -0.175** | -0.156* | -0.620** | -0.679** | -0.679** | -0.678** | -0.630** |
| | [0.088] | [0.082] | [0.088] | [0.081] | [0.082] | [0.279] | [0.290] | [0.293] | [0.291] | [0.286] |
| Treatment | -0.180** | | | | | -0.137 | | | | |
| | [0.073] | | | | | [0.133] | | | | |
| Post | 0.127** | 0.088* | 0.088** | | | 0.139** | 0.100* | 0.100 | | |
| | [0.053] | [0.048] | [0.044] | | | [0.056] | [0.060] | [0.065] | | |
| Constant | 0.159*** | | | | | 0.176** | | | | |
| | [0.050] | | | | | [0.074] | | | | |
| Observations | 523 | 523 | 523 | 523 | 523 | 605 | 605 | 605 | 605 | 605 |
| R-squared | 0.053 | 0.007 | 0.007 | 0.044 | 0.046 | 0.035 | 0.055 | 0.055 | 0.069 | 0.073 |
| Firm FE | No | Yes | Yes | Yes | Yes | No | Yes | Yes | Yes | Yes |
| Year FE | No | No | No | Yes | Yes | No | No | No | Yes | Yes |
| Control for size | No | No | No | No | Yes | No | No | No | No | Yes |
| Bootstrap s.e. | No | No | Yes | No | No | No | No | Yes | No | No |
| Clustered s.e. | Yes | Yes | No | Yes | Yes | Yes | Yes | No | Yes | Yes |
| Number of firms | | 78 | 78 | 78 | 78 | | 81 | 81 | 81 | 81 |

**Panel B: Accounts Receivable and Average Collection Period**

| | Accounts Receivable | | | | | Avg. Collection Period | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| Treatment x Post | -0.205*** | -0.178** | -0.178** | -0.176** | -0.170** | -74.912*** | -64.865** | -64.865*** | -64.075** | -61.889** |
| | [0.071] | [0.069] | [0.072] | [0.069] | [0.076] | [25.748] | [25.151] | [24.872] | [25.016] | [27.815] |
| Treatment | 0.061 | | | | | 22.218 | | | | |
| | [0.075] | | | | | [27.422] | | | | |
| Post | 0.223*** | 0.216*** | 0.216*** | | | 81.279*** | 78.780*** | 78.780*** | | |
| | [0.063] | [0.064] | [0.065] | | | [23.158] | [23.425] | [25.191] | | |
| Constant | 0.248*** | | | | | 90.616*** | | | | |
| | [0.043] | | | | | [15.797] | | | | |
| Observations | 214 | 214 | 214 | 214 | 214 | 214 | 214 | 214 | 214 | 214 |
| R-squared | 0.054 | 0.145 | 0.145 | 0.189 | 0.191 | 0.054 | 0.145 | 0.145 | 0.189 | 0.191 |
| Firm FE | No | Yes | Yes | Yes | Yes | No | Yes | Yes | Yes | Yes |
| Year FE | No | No | No | Yes | Yes | No | No | No | Yes | Yes |
| Control for size | No | No | No | No | Yes | No | No | No | No | Yes |
| Bootstrap s.e. | No | No | Yes | No | No | No | No | Yes | No | No |
| Clustered s.e. | Yes | Yes | No | Yes | Yes | Yes | Yes | No | Yes | Yes |
| Number of firms | | 45 | 45 | 45 | 45 | | 45 | 45 | 45 | 45 |

**Panel C: Accounts Payable and Inventories**

| | Accounts Payable | | | | | Inventories | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| Treatment x Post | -0.309 | -0.267 | -0.267 | -0.268 | -0.267 | -0.101** | -0.097** | -0.097** | -0.098** | -0.093** |
| | [0.256] | [0.242] | [0.194] | [0.245] | [0.247] | [0.043] | [0.044] | [0.040] | [0.044] | [0.044] |
| Treatment | 0.353 | | | | | -0.007 | | | | |
| | [0.278] | | | | | [0.034] | | | | |
| Post | 0.123** | 0.099** | 0.099** | | | 0.094** | 0.090** | 0.090** | | |
| | [0.049] | [0.044] | [0.043] | | | [0.038] | [0.037] | [0.036] | | |
| Constant | 0.168*** | | | | | 0.096*** | | | | |
| | [0.028] | | | | | [0.024] | | | | |
| Observations | 210 | 210 | 210 | 210 | 210 | 207 | 207 | 207 | 207 | 207 |
| R-squared | 0.043 | 0.017 | 0.017 | 0.020 | 0.020 | 0.033 | 0.056 | 0.056 | 0.129 | 0.131 |
| Firm FE | No | Yes | Yes | Yes | Yes | No | Yes | Yes | Yes | Yes |
| Year FE | No | No | No | Yes | Yes | No | No | No | Yes | Yes |
| Control for size | No | No | No | No | Yes | No | No | No | No | Yes |
| Bootstrap s.e. | No | No | Yes | No | No | No | No | Yes | No | No |
| Clustered s.e. | Yes | Yes | No | Yes | Yes | Yes | Yes | No | Yes | Yes |
| Number of firms | | 44 | 44 | 44 | 44 | | 43 | 43 | 43 | 43 |

The table displays the difference-in-differences estimator for firm financial outcomes. The sample includes treated and control firms that participated in the programme for which financial data is available. The sample period is 2008-2017. *, **, *** Significance at 10, 5 and 1%, respectively.

**Table IV:** Changes in Financial Policies - Cash, Leverage and Capital Expenditures (Financial Reports Regressions)

**Panel A: Cash and Leverage**

| | Cash | | | | | Leverage | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| Treatment x Post | 0.037 | 0.036 | 0.036 | 0.036 | 0.059 | -0.132 | -0.052 | -0.052 | -0.051 | 0.027 |
| | [0.046] | [0.047] | [0.046] | [0.047] | [0.047] | [0.115] | [0.074] | [0.079] | [0.072] | [0.089] |
| Treatment | -0.113** | | | | | -0.115* | | | | |
| | [0.045] | | | | | [0.068] | | | | |
| Post | 0.001 | -0.005 | -0.005 | | | 0.095 | 0.048 | 0.048 | | |
| | [0.036] | [0.036] | [0.033] | | | [0.109] | [0.070] | [0.077] | | |
| Constant | 0.222*** | | | | | 0.311*** | | | | |
| | [0.039] | | | | | [0.059] | | | | |
| Observations | 203 | 203 | 203 | 203 | 203 | 439 | 439 | 439 | 439 | 439 |
| R-squared | 0.065 | 0.005 | 0.005 | 0.134 | 0.170 | 0.028 | 0.002 | 0.002 | 0.050 | 0.123 |
| Firm FE | No | Yes | Yes | Yes | Yes | No | Yes | Yes | Yes | Yes |
| Year FE | No | No | No | Yes | Yes | No | No | No | Yes | Yes |
| Control for size | No | No | No | No | Yes | No | No | No | No | Yes |
| Bootstrap s.e. | No | No | Yes | No | No | No | No | Yes | No | No |
| Clustered s.e. | Yes | Yes | No | Yes | Yes | Yes | Yes | No | Yes | Yes |
| Number of firms | | 45 | 45 | 45 | 45 | | 74 | 74 | 74 | 74 |

**Panel B: Capital Expenditures**

| | Capital Expenditures | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Treatment x Post | 0.134*** | 0.131*** | 0.131*** | 0.126** | 0.144** |
| | [0.049] | [0.049] | [0.051] | [0.049] | [0.056] |
| Treatment | -0.059** | | | | |
| | [0.026] | | | | |
| Post | 0.052* | 0.061** | 0.061** | | |
| | [0.029] | [0.028] | [0.027] | | |
| Constant | 0.028 | | | | |
| | [0.022] | | | | |
| Observations | 164 | 164 | 164 | 164 | 164 |
| R-squared | 0.115 | 0.142 | 0.142 | 0.197 | 0.212 |
| Firm FE | No | Yes | Yes | Yes | Yes |
| Year FE | No | No | No | Yes | Yes |
| Control for size | No | No | No | No | Yes |
| Bootstrap s.e. | No | No | Yes | No | No |
| Clustered s.e. | Yes | Yes | No | Yes | Yes |
| Number of firms | | 44 | 44 | 44 | 44 |

The table displays the difference-in-differences estimator for firm financial outcomes. The sample includes treated and control firms that participated in the programme for which financial data is available. The sample period is 2008-2017. *, **, *** Significance at 10, 5 and 1%, respectively.

**Table V:** Changes in Firm Performance (Financial Reports Regressions)

**Panel A: Return on Assets (ROA)**

| | ROA | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Treatment x Post | 0.228** | 0.204** | 0.204* | 0.211** | 0.219** |
| | [0.108] | [0.100] | [0.107] | [0.100] | [0.103] |
| Treatment | -0.069 | | | | |
| | [0.089] | | | | |
| Post | -0.197** | -0.211*** | -0.211*** | | |
| | [0.075] | [0.071] | [0.071] | | |
| Constant | 0.240*** | | | | |
| | [0.080] | | | | |
| Observations | 521 | 521 | 521 | 521 | 521 |
| R-squared | 0.011 | 0.018 | 0.018 | 0.112 | 0.112 |
| Firm FE | No | Yes | Yes | Yes | Yes |
| Year FE | No | No | No | Yes | Yes |
| Control for size | No | No | No | No | Yes |
| Bootstrap s.e. | No | No | Yes | No | No |
| Clustered s.e. | Yes | Yes | No | Yes | Yes |
| Number of firms | | 76 | 76 | 76 | 76 |

**Panel B: Return on Invested Capital (ROIC) and Sales Growth**

| | ROIC | | | | | Sales Growth | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| Treatment x Post | 1.270* | 1.352** | 1.352* | 1.360* | 1.334** | 0.072 | 0.137 | 0.137 | 0.153 | 0.187 |
| | [0.698] | [0.678] | [0.768] | [0.689] | [0.669] | [0.120] | [0.122] | [0.126] | [0.122] | [0.124] |
| Treatment | -0.456 | | | | | -0.062 | | | | |
| | [0.365] | | | | | [0.045] | | | | |
| Post | -0.896** | -0.952*** | -0.952** | | | 0.062 | -0.022 | -0.022 | | |
| | [0.366] | [0.356] | [0.401] | | | [0.081] | [0.081] | [0.075] | | |
| Constant | 0.767** | | | | | 0.125*** | | | | |
| | [0.345] | | | | | [0.032] | | | | |
| Observations | 506 | 506 | 506 | 506 | 506 | 532 | 532 | 532 | 532 | 532 |
| R-squared | 0.015 | 0.019 | 0.019 | 0.044 | 0.044 | 0.006 | 0.003 | 0.003 | 0.203 | 0.205 |
| Firm FE | No | Yes | Yes | Yes | Yes | No | Yes | Yes | Yes | Yes |
| Year FE | No | No | No | Yes | Yes | No | No | No | Yes | Yes |
| Control for size | No | No | No | No | Yes | No | No | No | No | Yes |
| Bootstrap s.e. | No | No | Yes | No | No | No | No | Yes | No | No |
| Clustered s.e. | Yes | Yes | No | Yes | Yes | Yes | Yes | No | Yes | Yes |
| Number of firms | | 76 | 76 | 76 | 76 | | 78 | 78 | 78 | 78 |

The table displays the difference-in-differences estimator for firm financial performance. The sample includes treated and control firms that participated in the programme for which financial data is available. The sample period is 2008-2017. *, **, *** Significance at 10, 5 and 1%, respectively.

**Table VI:** Intention to change Financial Policies (Exit Survey)

**Panel A: Cohort 1 (May 2017)**

| *Intention to implement changes in corporate policies* | | | | | | | |
|---|---|---|---|---|---|---|---|
| | **Yes** | **No** | **N/A** | **Miss.** | **#** | **% Yes** | **% Yes (incl. missing, excl. N/A)** |
| Working capital | 27 | 7 | 3 | 3 | 40 | 73% | 73% |
| Risk management | 23 | 6 | 7 | 4 | 40 | 64% | 70% |
| Valuation | 14 | 12 | 7 | 7 | 40 | 42% | 42% |
| Capital structure | 13 | 8 | 13 | 6 | 40 | 38% | 48% |

**Panel B: Pooled cohorts 1 & 2 (May 2017, November 2018, April 2019)**

| *Intention to implement changes in corporate policies* | | | | | | | |
|---|---|---|---|---|---|---|---|
| | **Yes** | **No** | **N/A** | **Miss.** | **#** | **% Yes** | **% Yes (incl. missing, excl. N/A)** |
| Working capital | 44 | 14 | 4 | 6 | 68 | 71% | 69% |
| Risk management | 40 | 15 | 8 | 5 | 68 | 63% | 67% |
| Valuation | 30 | 19 | 8 | 11 | 68 | 53% | 50% |
| Capital structure | 27 | 18 | 16 | 7 | 68 | 44% | 52% |

The table displays the intentions of managers to change corporate policies. The data was collected in the exit survey at the end of the course. "N/A" means that a corporate policy cannot be changed because firm does not have discretion over that policy (e.g., subsidiary of a foreign firm). "Miss." refers to a missing answer. Depending on the specification, we disregard this answer in the aggregation or, being conservative, interpret it as a "No". The left tables show the raw answers of the individual managers. Source: Exit survey of cohort 1 (May 2017), Exit survey of cohort 2 (November 2018, April 2019).

**Table VII:** Changes in Financial Policies after 15 Months (15M Survey)

| *After 15 months, have you implemented changes in corporate policies?* | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | **Treatment** | | | | | **Control** | | | | **Difference** | |
| | **Yes** | **Yes (b/c of course)** | **No** | **#** | **%Yes** | **Yes** | **No** | **#** | **%Yes** | **Diff.** | **p-value** |
| Working capital | 8 | 8 | 18 | 26 | 30.80% | 1 | 26 | 27 | 3.70% | 27.10% | 0.00*** |
| Risk management | 2 | 2 | 24 | 26 | 7.70% | 0 | 27 | 27 | 0.00% | 7.70% | 0.07* |
| Valuation | 3 | 2 | 23 | 26 | 11.50% | 1 | 26 | 27 | 3.70% | 7.80% | 0.14 |
| Capital structure | 3 | 2 | 23 | 26 | 11.50% | 0 | 27 | 27 | 0.00% | 11.50% | 0.04** |
| At least one policy | 14 | 12 | 12 | 26 | 53.85% | 2 | 25 | 27 | 7.41% | 46.42% | 0.00*** |

The table displays the implemented changes of corporate policies by managers 15 months after the first treatment (May 2017) and before the second treatment (November 2018). The data was collected through a survey in Sep-Oct 2018. 'N/A' means that a corporate policy cannot be changed because firm does not have discretion over that policy (e.g. subsidiary of a foreign firm). Depending on the specification, we disregard this answer in the aggregation or, being conservative, interpret it as a 'No'. In the last row, we present the number of companies reporting to have changed at least one of the above financial policies. The middle of part of the table shows the corresponding answers by control firms (i.e., firms that participated in the experiment but were not taught in the course in May 2017). The right part of the table shows the difference between treatment and control firms and p-values of the corresponding one-sided t-tests. Source: 15M survey (Sep-Oct 2018). *, **, *** Significance at 10, 5 and 1%, respectively.

# The Impact of Financial Education of Executives on Financial Practices of Medium and Large Enterprises

## Online Appendix

Cláudia Custódio, Diogo Mendes, and Daniel Metzger

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**Figure A1:** The course brochure

**(a)**

**(b)**

## Overview

The business environment in emerging markets demands a dynamic and well-structured financial and strategic design. Understanding the characteristics of those markets is crucial to implement best practices.

The practical content of the programme will facilitate the application of innovative and disruptive approaches to the management challenges in emerging markets.

## Descrição

O ambiente empresarial em mercados emergentes requer um desenho financeiro e estratégico dinâmico e estruturado.

Compreender as características desses mercados é, portanto, essencial na implementação de boas práticas.

O conteúdo prático do programa irá facilitar a aplicação de abordagens inovadoras e disruptivas face aos desafios que a gestão em mercados emergentes coloca.

## Who should attend?

Senior managers, executives, and directors wishing to construct innovative and successful approaches towards the challenges in African emerging economies.

## A quem se destina?

Gestores seniores, executivos e directores que desejem construir abordagens inovadoras e com sucesso face aos desafios que se colocam em mercados emergentes africanos.

All participants will be awarded a Certificate of Attendance from Imperial College Business School Executive Education by the end of the course.

A todos os participantes será concedido um Certificado de Presença emitido pelo Imperial College Business School Executive Education.



## Among other topics in Corporate Finance we will cover:

### Working Capital Management

Managing working capital is crucial to the long-term financial sustainability. This section discusses how prioritizing working capital allows companies to make strategic investments, which in turn drive operational efficiencies.

### Capital Structure

Financial leverage constitutes an important part of a corporation's day-to-day operations. Given the ever-changing market conditions, it is important to understand how a firm can achieve its optimal leverage ratio, and the benefits, costs and risks associated with it. Participants will gain hands-on practice with rigorous methods to account for leverage in a firm's capital structure.

### Risk Management

Insight on the use of value enhancing and risk reducing strategies constitute a capital set of skills in the current business environment. This section aims to guide you towards the development of financial foresight, allowing for the prediction of new financial and corporate risks.

### Valuation

Rigorous understanding of valuation allows you to maximise the potential of a company, drive investment decisions and lead the restructuring, M&A and financing challenges that your organization faces.

## Entre outros tópicos em Finanças da Empresa, nós iremos abordar:

### Gestão de Fundo de Maneio

Gerir o fundo de maneio de uma empresa é crucial para a sustentabilidade de longo prazo. Esta secção visa entender a importância do fundo de maneio na tomada de decisões estratégicas, com vista a gerar mais-valias operacionais.

### Estrutura de Capital

A alavancagem financeira integra o dia-a-dia operacional de uma empresa. Considerando o dinamismo dos mercados, é importante entender como uma empresa pode atingir o seu nível óptimo de dívida, assim como os benefícios, custos e riscos associados. Os participantes irão ganhar uma visão prática sobre métodos rigorosos na análise de estrutura de capital.

### Gestão de Risco

O conhecimento de estratégias de gestão de risco constitui uma valência de extrema importância nas atuais condições de mercado. Esta secção tem como objectivo o desenvolvimento de técnicas de previsão financeira, nomeadamente na antecipação de novos riscos financeiros e empresariais.

### Avaliação de Projectos

O conhecimento rigoroso de métodos de avaliação permite a maximização do potencial de uma empresa, e auxilia na tomada de decisões de investimento, restruturação, M&A e financiamento que as empresas enfrentam.

**Table A1:** Variables description

| Variables | Description | Source | | |
|---|---|---|---|---|
| | | Hand Collected | KPMG | Survey |
| Total Assets (m USD) | Total Assets (book value) (million USD). | ✓ | ✓ | |
| Sales (m USD) | Revenue (million USD). | ✓ | ✓ | |
| Sales Growth | Percentage change in revenue relative to previous year. | ✓ | ✓ | |
| # Employees | Number of employees. | ✓ | ✓ | |
| Cash / (Lag) Assets | Cash over one-year lagged total assets. | ✓ | ✓ | |
| Leverage | Long-term total liabilities over one-year lagged total assets. | ✓ | ✓ | |
| Capex / (Lag) Assets | Capital expenditure over one-year lagged total assets. Capital expenditure is computed as property, plant and equipment minus one-year lagged property, plant and equipment plus depreciation and amortization. | ✓ | ✓ | |
| Return on (Lag) Assets (ROA) | Operating income over total assets. Operating income is defined as revenues minus operating costs. | ✓ | ✓ | |
| Return on Inv. Cap. (ROIC) | Operating income over total assets minus current liabilities. Operating income is defined as revenues minus operating costs. | ✓ | ✓ | |
| Working Capital / (Lag) Assets | Working Capital over one-year lagged total assets. Working capital is defined as current assets minus current liabilities. | ✓ | ✓ | |
| Working Capital / Sales | Working Capital over sales. | ✓ | ✓ | |
| Inventories / Sales | Inventories over sales. | ✓ | | |
| A/R / Sales | Accounts receivable over sales. | ✓ | | |
| A/P / Sales | Accounts payable over sales. | ✓ | | |
| Avg. Collection Period | Accounts receivable over sales times 365 (days). | ✓ | | |
| Male | Male. | | | ✓ |
| Age (years) | Age in years. | | | ✓ |
| Tenure (years) | Current tenure. | | | ✓ |
| Mozambican | Mozambican nationality. | | | ✓ |
| CEO | CEO/General Manager/Managing Partner | | | ✓ |
| CFO | CFO/Financial Director/Head of Financial Department. | | | ✓ |
| Masters or higher | Highest educational attainment higher or equal than masters. Includes the following categories: masters, post-graduation, MBA and PhD. | | | ✓ |
| MBA | Highest educational attainment in MBA. | | | ✓ |
| Acc. or Finance Background | Manager has attained accounting and finance courses at any education level. | | | ✓ |
| Treatment | Treatment equals one if a company or business group was assigned to the treatment group (first cohort). | | | |
| Post | Post equals one in 2017 and thereafter (year-end). | | | |

The table presents a description of each variable as well as its sources.

**Table A2:** The Schedule of the Course

| **Day 1 - Morning** | **Day 1 - Afternoon** | **Day 2 - Morning** | **Day 2 - Afternoon** |
|---|---|---|---|
| The basics: time value of money; investment decision rules<br>Capital budgeting and Valuation | Working capital management | Capital Structure (Debt vs. Equity decisions) | Risk Management (Insurance and Hedging decisions) |
| The case of **New Earth Mining**<br>(Capital budgeting and valuation in emerging markets) | The case of **Fibria Celulose SA and Procter and Gamble**<br>(Working capital in emerging markets) | The case of **UST**<br>(Leverage recapitalization) | The case of **Mozal**<br>(Risk Management) |

The table describes the course schedule, contents and case studies discussed in class.

**Table A3:** P-values using Randomization Inference

| | **Sampling p-value** | **Randomization-t p-value** |
|---|---|---|
| Working Capital / Assets | 3.5% | 5.45% |
| Accounts Receivable | 1.4% | 1.50% |
| Avg. Collection Period | 1.4% | 1.50% |
| Accounts Payable | 28.0% | 26.05% |
| Inventories | 3.2% | 1.99% |
| Cash | 45.1% | 49.71% |
| Leverage | 48.1% | 49.10% |
| Capital Expenditures | 1.4% | 1.18% |
| ROA | 3.9% | 2.42% |
| ROIC | 5.2% | 1.90% |
| Sales Growth | 21.1% | 24.82% |

The table compares p-values of our main specification that includes firm and year fixed effects and clusters at the firm-level with corresponding randomization-t p-values, computed using *randcmd* in STATA with 10,000 iterations (Young, 2019).

**Table A4:** ANCOVA estimates

| | NWC / Assets | A/R | A/P | Inventories | Cash |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Treatment | -0.216** | -0.178** | -0.025 | -0.093** | 0.028 |
| | [0.088] | [0.071] | [0.110] | [0.044] | [0.045] |
| Mean outcome in pre-period | 0.751*** | 1.046*** | 0.223* | 1.577*** | 0.948*** |
| | [0.138] | [0.131] | [0.120] | [0.391] | [0.185] |
| Observations | 63 | 43 | 43 | 42 | 43 |
| R-squared | 0.455 | 0.502 | 0.178 | 0.658 | 0.485 |

| | Leverage | Capex | ROA | ROIC | Sales Growth |
|---|---|---|---|---|---|
| | (6) | (7) | (8) | (9) | (10) |
| Treatment | -0.015 | 0.064* | 0.174** | 1.010 | 0.060 |
| | [0.061] | [0.037] | [0.082] | [0.687] | [0.115] |
| Mean outcome in pre-period | 1.292*** | -0.199 | 0.252** | 0.441** | 0.335*** |
| | [0.239] | [0.208] | [0.098] | [0.195] | [0.105] |
| Observations | 45 | 42 | 63 | 62 | 65 |
| R-squared | 0.730 | 0.129 | 0.142 | 0.110 | 0.121 |

The table displays the ANCOVA estimator for firm financial policies and performance. Specifically, we regress the post-treatment outcome variable on the treatment indicator and on the pre-treatment mean of the outcome variable. The pre-treatment mean is calculated as the average of the outcome variable in the years before the treatment (includes data from 2008-2016). The sample includes treated and control firms that participated in the programme for which financial data is available. The analysis excludes all companies in the financial sector. We report robust standard errors. *, **, *** denotes significance at the 10-, 5- and 1-% level, respectively.

**Table A5:** Intention-to-Treat (ITT) Estimates on Financial Policies

| | Working Capital | | | Avg. Collection Period | | | ROA | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Treatment x Post | -0.194** | -0.176** | -0.161** | -73.094*** | -63.880** | -61.651** | 0.234** | 0.212** | 0.228** |
| | [0.085] | [0.078] | [0.077] | [25.392] | [24.876] | [26.989] | [0.105] | [0.098] | [0.099] |
| Treatment | -0.176** | | | 14.565 | | | -0.080 | | |
| | [0.071] | | | [26.150] | | | [0.088] | | |
| Post | 0.127** | 0.088* | | 81.279*** | 78.780*** | | -0.197** | -0.211*** | |
| | [0.053] | [0.048] | | [23.139] | [23.409] | | [0.075] | [0.071] | |
| Constant | 0.159*** | | | 90.616*** | | | 0.240*** | | |
| | [0.050] | | | [15.785] | | | [0.080] | | |
| Observations | 542 | 542 | 542 | 224 | 224 | 224 | 540 | 540 | 540 |
| R-squared | 0.052 | 0.007 | 0.044 | 0.052 | 0.145 | 0.193 | 0.012 | 0.018 | 0.115 |
| Firm FE | No | Yes | Yes | No | Yes | Yes | No | Yes | Yes |
| Year FE | No | No | Yes | No | No | Yes | No | No | Yes |
| Control for size | No | No | Yes | No | No | Yes | No | No | Yes |
| Bootstrap s.e. | No | No | No | No | No | No | No | No | No |
| Clustered s.e. | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of firms | | 81 | 81 | | 47 | 47 | | 79 | 79 |

The table displays the difference-in-differences estimator for firm financial policies and performance. The sample includes treated and control firms that participated in the programme for which financial data is available, as well as companies that were assigned to treatment but did not participated in the programme (Intention-to-Treat). The sample period is 2008-2017. *, **, *** Significance at 10, 5 and 1%, respectively.

**Table A6:** External Data (KPMG) only

| | Working Capital | | | ROA | | |
|---|---|---|---|---|---|---|
| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** |
| Treatment x Post | -0.240** | -0.210*** | -0.201** | 0.382** | 0.396* | 0.229 |
| | [0.095] | [0.079] | [0.079] | [0.185] | [0.210] | [0.251] |
| Treatment | -0.201** | | | -0.256 | | |
| | [0.076] | | | [0.182] | | |
| Post | 0.129** | 0.078 | | -0.308* | -0.342* | |
| | [0.059] | [0.049] | | [0.156] | [0.180] | |
| Constant | 0.170*** | | | 0.356** | | |
| | [0.048] | | | [0.168] | | |
| Observations | 500 | 500 | 500 | 502 | 502 | 502 |
| R-squared | 0.054 | 0.007 | 0.040 | 0.011 | 0.005 | 0.069 |
| Firm FE | No | Yes | Yes | No | Yes | Yes |
| Year FE | No | No | Yes | No | No | Yes |
| Control for size | No | No | Yes | No | No | Yes |
| Bootstrap s.e. | No | No | No | No | No | No |
| Clustered s.e. | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of firms | | 77 | 77 | | 76 | 76 |

The table displays the difference-in-differences estimator for firm financial policies and performance. The sample includes treated and control firms that participated in the program for which financial data from KPMG are available. The sample period is 2008-2017. *, **, ***: Significance at 10, 5 and 1%, respectively.

**Table A7:** Excluding very large companies

| | Working Capital | | | Avg. Collection Period | | | ROA | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Treatment x Post | -0.199** | -0.176** | -0.156* | -81.703*** | -71.634*** | -69.748** | 0.242** | 0.227** | 0.248** |
| | [0.091] | [0.084] | [0.085] | [25.664] | [25.110] | [28.104] | [0.111] | [0.104] | [0.107] |
| Treatment | -0.181** | | | 26.716 | | | -0.080 | | |
| | [0.076] | | | [27.411] | | | [0.094] | | |
| Post | 0.131** | 0.089* | | 88.070*** | 85.549*** | | -0.211*** | -0.233*** | |
| | [0.057] | [0.053] | | [23.062] | [23.380] | | [0.079] | [0.076] | |
| Constant | 0.161*** | | | 86.118*** | | | 0.251*** | | |
| | [0.054] | | | [15.764] | | | [0.086] | | |
| Observations | 502 | 502 | 502 | 209 | 209 | 209 | 502 | 502 | 502 |
| R-squared | 0.052 | 0.007 | 0.046 | 0.063 | 0.165 | 0.214 | 0.012 | 0.020 | 0.118 |
| Firm FE | No | Yes | Yes | No | Yes | Yes | No | Yes | Yes |
| Year FE | No | No | Yes | No | No | Yes | No | No | Yes |
| Control for size | No | No | Yes | No | No | Yes | No | No | Yes |
| Bootstrap s.e. | No | No | No | No | No | No | No | No | No |
| Clustered s.e. | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of firms | | 75 | 75 | | 44 | 44 | | 73 | 73 |

The table displays the difference-in-differences estimator for firm financial policies and performance. The sample includes treated and control firms that participated in the program for which financial data are available. We exclude companies with total assets over 800 million USD. The sample period is 2008-2017. *, **, *** Significance at 10, 5 and 1%, respectively.

**Table A8:** Differential Effects to Firm Characteristics in the Post-treatment Period

**Panel A: Working Capital**

| | Working Capital | | | | |
|---|---|---|---|---|---|
| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** |
| Treatment x Post | -0.181** | -0.181** | -0.192** | -0.184** | -0.117 |
| | [0.079] | [0.084] | [0.078] | [0.084] | [0.089] |
| Observations | 523 | 523 | 509 | 509 | 403 |
| R-squared | 0.051 | 0.047 | 0.049 | 0.010 | 0.053 |
| Number of firms | 78 | 78 | 73 | 73 | 61 |
| Firm FE | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes |
| Control x Post | ln size | cubic size | ln size 2016 | cubic size 2016 | Mozambican |
| Clustered s.e. | Yes | Yes | Yes | Yes | Yes |

**Panel B: Average Collection Period**

| | Average Collection Period | | | | |
|---|---|---|---|---|---|
| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** |
| Treatment x Post | -64.005** | -65.990** | -66.050** | -68.168** | -62.445** |
| | [30.289] | [25.010] | [26.422] | [25.365] | [25.528] |
| Observations | 214 | 214 | 214 | 214 | 199 |
| R-squared | 0.193 | 0.237 | 0.193 | 0.181 | 0.185 |
| Number of firms | 45 | 45 | 45 | 45 | 42 |
| Firm FE | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes |
| Control x Post | ln size | cubic size | ln size 2016 | cubic size 2016 | Mozambican |
| Clustered s.e. | Yes | Yes | Yes | Yes | Yes |

**Panel C: ROA**

| | ROA | | | | |
|---|---|---|---|---|---|
| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** |
| Treatment x Post | 0.223** | 0.251** | 0.216** | 0.235** | 0.285*** |
| | [0.101] | [0.106] | [0.100] | [0.106] | [0.105] |
| Observations | 521 | 521 | 508 | 508 | 403 |
| R-squared | 0.112 | 0.117 | 0.113 | 0.021 | 0.155 |
| Number of firms | 76 | 76 | 71 | 71 | 59 |
| Firm FE | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes |
| Control x Post | ln size | cubic size | ln size 2016 | cubic size 2016 | Mozambican |
| Clustered s.e. | Yes | Yes | Yes | Yes | Yes |

The table displays the difference-in-differences estimator for firm financial policies and performance. The sample includes treated and control firms that participated in the program for which financial data are available. We control for different specifications of size (Assets) and the nationality of the executives, as well as for their interactions with a dummy that is equal to one for the period post-treatment. *, **, ***: Significance at 10, 5 and 1%, respectively.

**Table A9:** Focus on the Largest Segment

| | Working Capital | | | Avg. Collection Period | | | ROA | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Treatment x Post | -0.192* | -0.128 | -0.121 | -80.096*** | -68.070** | -66.407** | 0.234** | 0.214** | 0.225** |
| | [0.099] | [0.091] | [0.093] | [27.765] | [26.631] | [30.607] | [0.115] | [0.104] | [0.111] |
| Treatment | -0.162* | | | 16.562 | | | -0.013 | | |
| | [0.089] | | | [27.192] | | | [0.080] | | |
| Post | 0.140** | 0.092 | | 74.883*** | 72.082*** | | -0.163*** | -0.175*** | |
| | [0.062] | [0.055] | | [24.693] | [24.981] | | [0.057] | [0.054] | |
| Constant | 0.178*** | | | 91.257*** | | | 0.198*** | | |
| | [0.060] | | | [16.979] | | | [0.062] | | |
| Observations | 392 | 392 | 392 | 185 | 185 | 185 | 389 | 389 | 389 |
| R-squared | 0.043 | 0.005 | 0.048 | 0.053 | 0.138 | 0.160 | 0.011 | 0.016 | 0.135 |
| Firm FE | No | Yes | Yes | No | Yes | Yes | No | Yes | Yes |
| Year FE | No | No | Yes | No | No | Yes | No | No | Yes |
| Control for size | No | No | Yes | No | No | Yes | No | No | Yes |
| Bootstrap s.e. | No | No | No | No | No | No | No | No | No |
| Clustered s.e. | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of firms | | 57 | 57 | | 39 | 39 | | 55 | 55 |

The table displays the difference-in-differences estimator for firm financial policies and performance. The sample includes treated and control firms that participated in the program for which financial data are available. For each business group, only the largest segment is considered. The sample period is 2008-2017. *, **, *** Significance at 10, 5 and 1%, respectively.

**Table A10:** Different Time Periods

| | Working Capital | | | Avg. Collection Period | | | ROA | | |
|---|---|---|---|---|---|---|---|---|---|
| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** | **(7)** | **(8)** | **(9)** |
| Treatment x Post | -0.127* | -0.167** | -0.158** | -61.889** | -71.151*** | -64.087*** | 0.223** | 0.133* | 0.132** |
| | [0.076] | [0.071] | [0.068] | [27.815] | [25.847] | [22.823] | [0.084] | [0.077] | [0.061] |
| Observations | 327 | 200 | 134 | 214 | 132 | 88 | 323 | 198 | 133 |
| R-squared | 0.044 | 0.080 | 0.131 | 0.191 | 0.219 | 0.289 | 0.147 | 0.035 | 0.076 |
| Number of firms | 76 | 72 | 72 | 45 | 45 | 45 | 74 | 71 | 71 |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Control for size | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Bootstrap s.e. | No | No | No | No | No | No | No | No | No |
| Clustered s.e. | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Years | $\geq$2013 | $\geq$2015 | $\geq$2016 | $\geq$2013 | $\geq$2015 | $\geq$2016 | $\geq$2013 | $\geq$2015 | $\geq$2016 |

The table displays the difference-in-differences estimator for firm financial policies and performance. The sample includes treated and control firms that participated in the program for which financial data are available. The sample periods considered are from 2013, 2015 and 2016 until 2017. *, **, ***: Significance at 10, 5 and 1%, respectively.

**Table A11:** Excluding CEO turnovers

| | Working Capital | | | Avg. Collection Period | | | ROA | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Treatment x Post | -0.117 | -0.095 | -0.081 | -81.373*** | -69.801*** | -66.748** | 0.251** | 0.222** | 0.217** |
| | [0.094] | [0.084] | [0.084] | [26.177] | [25.357] | [27.837] | [0.111] | [0.100] | [0.098] |
| Treatment | -0.161* | | | 30.123 | | | -0.046 | | |
| | [0.085] | | | [30.600] | | | [0.077] | | |
| Post | 0.125** | 0.083* | | 81.279*** | 78.780*** | | -0.160** | -0.175*** | |
| | [0.055] | [0.050] | | [23.189] | [23.452] | | [0.071] | [0.063] | |
| Constant | 0.158*** | | | 90.616*** | | | 0.191*** | | |
| | [0.054] | | | [15.819] | | | [0.071] | | |
| Observations | 456 | 456 | 456 | 199 | 199 | 199 | 460 | 460 | 460 |
| R-squared | 0.036 | 0.004 | 0.051 | 0.057 | 0.145 | 0.197 | 0.011 | 0.019 | 0.103 |
| Firm FE | No | Yes | Yes | No | Yes | Yes | No | Yes | Yes |
| Year FE | No | No | Yes | No | No | Yes | No | No | Yes |
| Control for size | No | No | Yes | No | No | Yes | No | No | Yes |
| Bootstrap s.e. | No | No | No | No | No | No | No | No | No |
| Clustered s.e. | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of firms | | 68 | 68 | | 42 | 42 | | 67 | 67 |

The table displays the difference-in-differences estimator for firm financial policies and performance. The sample includes treated and control firms that participated in the program for which financial data are available. The analysis excludes all business groups for which the top manager has been replaced at any point during the research project. The sample period is 2008-2017. *, **, ***: Significance at 10, 5 and 1%, respectively.

**Table A12:** Excluding Financial Companies

| | Working Capital | | | Avg. Collection Period | | | ROA | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Treatment x Post | -0.188* | -0.176** | -0.160* | -74.900*** | -71.510** | -64.808** | 0.238** | 0.216* | 0.228** |
| | [0.094] | [0.088] | [0.088] | [27.599] | [27.538] | [29.739] | [0.116] | [0.109] | [0.110] |
| Treatment | -0.166** | | | 16.075 | | | -0.063 | | |
| | [0.073] | | | [28.449] | | | [0.095] | | |
| Post | 0.120** | 0.087 | | 87.364*** | 85.425*** | | -0.203** | -0.222*** | |
| | [0.056] | [0.052] | | [25.939] | [25.967] | | [0.080] | [0.077] | |
| Constant | 0.130*** | | | 90.662*** | | | 0.247*** | | |
| | [0.045] | | | [16.839] | | | [0.086] | | |
| Observations | 485 | 485 | 485 | 197 | 197 | 197 | 483 | 483 | 483 |
| R-squared | 0.046 | 0.007 | 0.047 | 0.059 | 0.157 | 0.214 | 0.010 | 0.018 | 0.119 |
| Firm FE | No | Yes | Yes | No | Yes | Yes | No | Yes | Yes |
| Year FE | No | No | Yes | No | No | Yes | No | No | Yes |
| Control for size | No | No | Yes | No | No | Yes | No | No | Yes |
| Bootstrap s.e. | No | No | No | No | No | No | No | No | No |
| Clustered s.e. | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of firms | | 73 | 73 | | 41 | 41 | | 71 | 71 |

The table displays the difference-in-differences estimator for firm financial policies and performance. The sample includes treated and control firms that participated in the programme for which financial data is available. The analysis excludes all companies in the financial sector. The sample period is 2008-2017. *, **, *** Significance at 10, 5 and 1%, respectively.

**Table A13:** Outcomes scaled by Contemporaneous Total Assets

| | Working Capital | | | ROA | | |
|---|---|---|---|---|---|---|
| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** |
| Treatment x Post | -0.131* | -0.115* | -0.105 | 0.199** | 0.194** | 0.205** |
| | [0.070] | [0.068] | [0.066] | [0.094] | [0.092] | [0.092] |
| Treatment | -0.152** | | | -0.061 | | |
| | [0.064] | | | [0.074] | | |
| Post | 0.074* | 0.051 | | -0.167*** | -0.192*** | |
| | [0.042] | [0.039] | | [0.062] | [0.062] | |
| Constant | 0.153*** | | | 0.200*** | | |
| | [0.043] | | | [0.066] | | |
| Observations | 607 | 607 | 607 | 612 | 612 | 612 |
| R-squared | 0.053 | 0.005 | 0.029 | 0.009 | 0.016 | 0.085 |
| Firm FE | No | Yes | Yes | No | Yes | Yes |
| Year FE | No | No | Yes | No | No | Yes |
| Control for size | No | No | Yes | No | No | Yes |
| Bootstrap s.e. | No | No | No | No | No | No |
| Clustered s.e. | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of firms | | 81 | 81 | | 81 | 81 |

The table displays the difference-in-differences estimator for firm financial policies and performance. The sample includes treated and control firms that participated in the programme for which financial data is available. Working Capital and ROA are scaled by contemporaneous book value of total assets. The sample period is 2008-2017. *, **, *** Significance at 10, 5 and 1%, respectively.

**Table A14:** Changes in Financial Policies (Dynamic effects)

| | Working Capital | | | ROA | | | Sales Growth | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Treatment x Post(1-year) | -0.194** | -0.163* | -0.207** | 0.228** | 0.217** | 0.213** | 0.072 | 0.117 | 0.174 |
| | [0.088] | [0.082] | [0.086] | [0.108] | [0.103] | [0.104] | [0.120] | [0.117] | [0.117] |
| Treatment x Post(2-year) | -0.225 | -0.102 | -0.151 | 0.202* | 0.184 | 0.174 | -0.071 | 0.004 | 0.048 |
| | [0.176] | [0.156] | [0.160] | [0.117] | [0.124] | [0.121] | [0.103] | [0.136] | [0.142] |
| Post(1-year) | 0.127** | 0.089* | | -0.197** | -0.218*** | | 0.062 | 0.004 | |
| | [0.053] | [0.049] | | [0.075] | [0.074] | | [0.081] | [0.075] | |
| Post(2-year) | 0.098 | 0.042 | | -0.112 | -0.147 | | 0.061 | -0.026 | |
| | [0.062] | [0.053] | | [0.082] | [0.088] | | [0.071] | [0.104] | |
| Treatment | -0.180** | | | -0.069 | | | -0.062 | | |
| | [0.073] | | | [0.089] | | | [0.045] | | |
| Constant | 0.159*** | | | 0.240*** | | | 0.125*** | | |
| | [0.050] | | | [0.080] | | | [0.032] | | |
| Observations | 574 | 574 | 574 | 571 | 571 | 571 | 583 | 583 | 583 |
| R-squared | 0.061 | 0.006 | 0.047 | 0.012 | 0.022 | 0.107 | 0.007 | 0.003 | 0.182 |
| Firm FE | No | Yes | Yes | No | Yes | Yes | No | Yes | Yes |
| Year FE | No | No | Yes | No | No | Yes | No | No | Yes |
| Control for size | No | No | Yes | No | No | Yes | No | No | Yes |
| Bootstrap s.e. | No | No | No | No | No | No | No | No | No |
| Clustered s.e. | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of firms | | 78 | 78 | | 76 | 76 | | 78 | 78 |

The table displays the difference-in-differences estimator for firm financial outcomes one and two years after the experiment. The sample includes treated and control firms that participated in the programme for which financial data is available. The sample period is 2008-2018. *, **, *** Significance at 10, 5 and 1%, respectively.

**Table A15:** External Validity

**Panel A: Enrolment in the Programme: Firms**

| | Enrolled Companies | | | | Non-enrolled Companies | | | | Mean | | Median | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Obs. | Mean | Median | St.Dev. | Obs. | Mean | Median | St.Dev. | Diff. | p-value | Diff. | p-value |
| Total Assets (m USD) | 78 | 93.54 | 6.61 | 285.76 | 175 | 79.45 | 5.39 | 256.93 | 14.10 | 0.70 | -1.21 | 0.65 |
| Sales (m USD) | 78 | 44.95 | 6.24 | 113.13 | 174 | 28.59 | 3.65 | 84.43 | 16.36 | 0.20 | -2.59 | 0.22 |
| Sales Growth | 74 | -0.24 | -0.30 | 0.53 | 136 | -0.17 | -0.25 | 0.47 | -0.07 | 0.34 | 0.04 | 0.31 |
| # Employees | 72 | 307.81 | 99.5 | 605.69 | 174 | 435.33 | 89.00 | 1320.19 | -127.53 | 0.43 | -10.50 | 0.62 |
| Leverage | 59 | 0.22 | 0.11 | 0.30 | 154 | 0.22 | 0.04 | 0.32 | 0.00 | 0.97 | -0.07 | 0.34 |
| Return on Assets (ROA) | 76 | 0.08 | 0.06 | 0.24 | 175 | 0.03 | 0.04 | 0.28 | 0.05 | 0.21 | -0.01 | 0.65 |
| Return on Invested Capital (ROIC) | 75 | 0.40 | 0.15 | 1.98 | 172 | 0.45 | 0.19 | 1.97 | -0.05 | 0.84 | 0.04 | 0.61 |
| Working Capital / (Lag) Assets | 77 | 0.03 | 0.08 | 0.38 | 172 | 0.00 | 0.01 | 0.42 | 0.04 | 0.53 | -0.07 | 0.25 |
| Working Capital / Sales | 77 | 0.03 | 0.08 | 0.73 | 171 | -0.14 | 0.00 | 1.51 | 0.17 | 0.36 | -0.07 | 0.27 |

**Panel B: Participating Managers' Characteristics vs LinkedIn**

| | Our Sample | | LinkedIn | | | | | | Differences | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | (Full Sample) | | (Employees≥25) | | (Followers≥100) | | (Full Sample) | | (Employees≥25) | | (Followers≥100) | |
| | Obs. | Mean | Obs. | Mean | Obs. | Mean | Obs. | Mean | Diff. | p-value | Diff. | p-value | Diff. | p-value |
| Male | 67 | 78% | 790 | 92% | 50 | 96% | 67 | 94% | -0.14 | 0.00*** | -0.18 | 0.01*** | -0.16 | 0.01*** |
| Tenure (years) | 66 | 7.45 | 761 | 6.98 | 48 | 7.67 | 66 | 7.39 | 0.47 | 0.59 | -0.22 | 0.86 | 0.06 | 0.96 |
| Masters or higher | 63 | 49% | 407 | 37% | 29 | 52% | 41 | 49% | 0.12 | 0.06* | -0.03 | 0.82 | 0.00 | 0.97 |
| MBA | 63 | 21% | 407 | 15% | 29 | 28% | 41 | 20% | 0.06 | 0.25 | -0.07 | 0.47 | 0.01 | 0.89 |

**Panel C: Participating Managers' Characteristics vs Graham and Harvey (2001)**

| | Our Sample | | Graham and Harvey (2001) | | | | Differences | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | | | (Full Sample) | | (Sales≤100M) | | (Full Sample) | | (Sales≤100M) | |
| | Obs. | Mean | Obs. | Mean | Obs. | Mean | Diff. | p-value | Diff. | p-value |
| Male | 67 | 78% | - | - | - | - | - | - | - | - |
| Tenure (years) | 66 | 7.45 | 366 | 6.68 | 92 | 7.59 | 0.76 | 0.20 | -0.14 | 0.87 |
| Masters or higher | 63 | 49% | 354 | 60% | 91 | 52% | -0.11 | 0.11 | -0.02 | 0.77 |
| MBA | 63 | 21% | 354 | 38% | 91 | 27% | -0.18 | 0.01*** | -0.07 | 0.34 |

Panel A displays summary statistics for the main financial variables of 93 firms participating in the programme (Treated/Control Sample), and for firms that did not enrol in the programme. Financial data is obtained from KPMG "Top-100 Companies in Mozambique" report, and hand collected. Panel B presents a comparison between top managers in our sample and a representative sample obtained through LinkedIn. The LinkedIn sample was obtained through manual extraction on LinkedIn using the following filters: location (Mozambique) and title (CEO/General Manager/CEO/Financial Director). The search occurred on the 17th of July 2019 and we obtained 790 entries (current CEO/CFO). The first two columns exhibit descriptive statistics on the top managers of treatment and control companies (pooled) in our sample. The next two columns correspond to the aggregate LinkedIn sample. In the following four columns, we condition the analysis to executives of companies with more than 25 (registered) employees or at least 100 followers. The significant reduction in the number of observations is due to many missing data on employees and followers. In Panel C, we present the statistics computed on Graham and Harvey (2001) survey data. We also condition the analysis on companies with lower than US $100M in sales. The category 'Masters or higher' contains the 'MBA' category. In panels B and C, we present t-test statistics for the mean difference between our sample and LinkedIn or Graham and Harvey (2001) samples, respectively. We thank John Graham and Campbell Harvey for making the data available. *, **, *** Significance at 10, 5 and 1%, respectively.