

# The Impact of Financial Education of Managers on Medium and Large Enterprises – A Randomized Controlled Trial in Mozambique\*

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**Abstract:** This paper studies the impact of a financial education program for top managers of medium and large enterprises in Mozambique through a randomized controlled trial (RCT). We use survey data and financial reporting data to show consistent evidence that managers adjust some financial policies in response to the education program. The largest treatment effects are on short-term financial policies related to working capital, generating a positive impact on cash flows due to reductions in accounts receivable and inventories. There is also a smaller but significant, positive impact on long-term investment. These firm policy changes improve the firm performance of the treated firms. Overall, our results suggest that relatively small and low-cost interventions, such as a short executive education program in finance, improve financial practices and can affect economic development.

*Keywords:* Financial literacy, financing constraints, CEOs, financial education, RCT  
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# 1 Introduction

Management practices help to explain differences in firm productivity and profitability, as well as development levels across countries (e.g., Bloom and Van Reenen (2011) or Bloom, Eifert, Mahajan, McKenzie, and Roberts (2013) ). This literature has mostly focused on the lower or middle management of larger corporations or on the founders/CEOs of small or micro-enterprises. There is no quasi-experimental evidence from executives of large companies, although their potential impact on economic development is also larger since they effectively control a large part of the economy. In this paper, we conduct a randomized controlled trial (RCT) with top-level executives of medium and large companies in Mozambique who participate in an executive education program in finance. The program focuses on investment and capital allocation decisions, as well as firm financial policies. 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 in which financial frictions are potentially severe, as in developing economies. The World Bank Enterprise Survey (2018) identified "Access to Finance" as one of the greatest obstacles for firms in Mozambique.<sup>1</sup> 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 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.

Given these potentially severe frictions, Mozambique seems to be a relevant and meaningful environment for analyzing the impact of executive education on firm financial policies and profitability. Using survey data and financial accounting data from one of the world's largest accounting firms, we find treatment effects of the provided course on firm investment and financial policies. The largest changes are in short-term financial policies related to working capital, generating a positive impact on cash flows due to reductions in accounts receivable and inventories. We also observe a smaller yet still significant positive change in investment in fixed capital in response to the treatment. At the same time, we do not find firms obtaining new or additional bank loans or lines of credit. Survey evidence confirms that credit markets are tight, and for this reason the

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<sup>1</sup>"Corruption" followed by the "Practices of the Informal Sector", "Crime", and "Political Instability" were also mentioned as obstacles.

changes in short-term financing policies can help firms to overcome financial constraints, at least in the short run. These policy changes improve firm performance measured by accounting returns, consistent with efficiency gains.

Our findings suggest that individual CEOs and, in particular, their financial education matter for corporate policies and ultimately for corporate performance. These findings are consistent with [Bertrand and Schoar \(2003\)](#), who argued that individual CEOs help to explain observed heterogeneity in management practices and corporate policies and concluded that CEOs possess different "styles". With respect to financial expertise, the existing research shows that managers' financial expertise impacts the revenues and/or survival rates of corporations in the context of small and micro-entrepreneurs<sup>2</sup> in developing countries (e.g., [Bruhn and Zia \(2013\)](#), [Drexler, Fischer, and Schoar \(2014\)](#), and [Anderson-Macdonald, Chandy, and Zia \(2018\)](#)), 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 \(2014\)](#)). Overall, evidence suggests that the impact of the financial expertise of CEOs on economic outcomes is potentially large.

Our study makes three contributions to the literature. First, we show in an RCT setting that individual CEOs and, in particular, their financial skills have causal effects on firm financial policies and performance. Second, we show that executive education matters 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 suggest that improving short-term financial policies, such as working capital, can potentially relax financial constraints by improving firm liquidity at least in the short run.

We start by documenting heterogeneity in financial expertise by CEOs in Mozambique and its correlation with financial practices. For example, while CEOs with a background in finance tend to use sophisticated project valuation and capital budgeting techniques, these practices are less common for CEOs without such a background. At the same time, they are more likely to use less sophisticated valuation techniques, such as hurdle rates. 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 more likely to follow financial theory and textbook rules and to avoid com-

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<sup>2</sup>[Aktkinson \(2017\)](#) provided a survey on financial education for MSMEs and potential entrepreneurs.

mon mistakes, such as using a unique firm cost of capital irrespective of the nature of the project (the Weighted Average Cost of Capital (WACC) fallacy).<sup>3</sup> While these results are suggestive of an effect of financial expertise on financial policies, omitted variables could bias the estimates.

To identify a treatment effect of financial expertise on firm policies, one would need to observe exogenous variation in financial expertise across firms. One way of achieving this goal in an experiment would be random allocation of CEOs to firms (which is infeasible in practice). We propose a different solution by randomizing the financial education of top managers and, at the same time, keeping the match between CEOs and firms constant. We "treat" managers with financial expertise by offering a free executive education course on corporate finance (MBA style) to top managers of 93 medium and large firms in Mozambique. Similar approaches have been employed in the development economics literature to measure the impact of the financial literacy of small and micro-entrepreneurs (e.g., Bruhn and Zia (2013), Drexler, Fischer, and Schoar (2014), and Anderson-Macdonald, Chandy, and Zia (2018)), but they have not been applied to larger companies. An exception is Bloom, Eifert, Mahajan, McKenzie, and Roberts (2013), who used an RCT to measure the effects of management practices on the productivity of large plants in India.<sup>4</sup> However, their focus was on lower-tier plant managers rather than on executives, and they did not study financial education or financial policies. Our paper is the first RCT project in economics with the intervention targets being executives from relatively large companies.<sup>5</sup>

To address concerns about endogenous selection in the treatment, we randomly staggered the timing of the treatment of firms that expressed their interest in participating in the executive education program. Firms were randomly allocated into two cohorts: a treatment group and a control group. 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. We offered the control group the course

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<sup>3</sup>The use of companywide discount rates to evaluate investment projects, rather than project-specific rates, has been called the weighted average cost of capital (WACC) fallacy (Graham and Harvey (2001) and Krüger, Landier, and Thesmar (2015)).

<sup>4</sup>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, Karlan, and Schoar (2018); Karlan, Knight, and Udry (2012)).

<sup>5</sup>Bloom, Eifert, Mahajan, McKenzie, and Roberts (2013) performed an experiment in 17 firms operating 28 plants; this relatively small number of observations reflects the difficulty in obtaining large samples in the context of RCTs with large corporations. In this respect, a sample size of 93 firms appears notable.

as well to provide an incentive to participate in the surveys and to make detailed financial data available. During the fifteen-month period, both groups of firms were contacted to collect financial data and to conduct follow-up surveys on financial practices. One survey was conducted immediately after the intervention to evaluate the intentions of managers to change their behavior, and a second survey was conducted 15 months after the intervention to evaluate implemented changes.

The main results can be summarized as follows. i) Treated firms report high intentions to change financial policies after participation in the course (73% of the firms intend to adjust their working capital management, 70% their risk management, 42% their valuation techniques and 48% their capital structure). 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. ii) Of treated firms, 30.8% report that they implemented changes in working capital management 15 months after the treatment. Corresponding figures for other financial policies are lower (11.5% for capital structure decisions and valuation and 7.7% for risk management). Moreover, firms report that they implemented these changes because of the treatment (i.e., the course in which they participated in 15 months before). While these results are suggestive of a treatment effect, we use the control group to address the concern that we might be capturing, for instance, a pure time-effect. When we compare mean differences between the treatment and control groups, we find a significant difference for changes in working capital management (significant at the 1% level) and changes in capital structure and risk management (significant at the 10% level). iii) We use accounting statements to analyze whether the survey evidence translates into hard facts in financial data and to analyze potential implications for firms' efficiency. Unfortunately, the financial data do not allow us to analyze directly potential changes to valuation techniques and risk management; hence, we focus our analysis on capital structure and (components of) working capital. Nevertheless, changes in firm accounting performance, if positive, will suggest changes in the direction of optimal capital allocation and risk management policies.

Using a difference-in-difference estimator, we find a large and negative effect on working capital: working capital decreases by 0.62 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 as well their inventories. These changes are expected to have a positive effect on liquidity in the short run. We do not find any effect on cash holdings or leverage, and treated firms use this cash inflow at least partially to increase capital expenditures. These findings are consistent with the survey results.

Whether these changes have led to policies that are more efficient or not is not clear *ex ante*. For instance, by collecting receivables too quickly or reducing inventory too much, sales might be adversely affected. To test whether firms have moved toward more efficient policies as a response to the treatment, we analyze whether the treated firms show better performance relative to the control group. Given that most firms are private, we do not observe their market values.<sup>6</sup> Hence, we must rely on accounting ratios to measure performance. 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.<sup>7</sup>

Attending the course on corporate finance 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 believe that there is strong support for this channel. While our results on ROA are consistent with a network channel, it is less obvious why working capital should be affected. Moreover, we would expect to see a positive effect on sales growth if networking generates new business among treated firms. Finally, during the delivery of the course for the treatment group in May 2017, we organized a networking event for the control group, allowing it to network as well.

While in theory, an RCT is a clean setting to identify the effects of finance educa-

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<sup>6</sup>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. Two of these companies were in the treatment group, and the other went public after the intervention.

<sup>7</sup>Bruhn, Karlan, and Schoar (2018) made a similar argument when measuring the impact of consulting for small- and medium-sized firms in Mexico.

tion on financial policies, there might be limitations in practice. For instance, the internal validity of our research design could be compromised by systematic differences in the treatment and control groups due to the small sample, high or uneven attrition rates, contagion effects, or changes in the expectations or behaviors of treated managers due to being part of an experiment. Normalized differences suggest that treatment and control group are relatively well balanced. None of these differences is above one (see [Imbens and Rubin \(2015\)](#)). Moreover, for the validity of our experiment, it would not be a concern if the groups differed in levels but exhibited parallel pretreatment evolution. We test this parallel trend assumption nonparametrically and do not find evidence on its violation for the main variables of interest. We also allow for the differential behavior and performance of firms along some observable dimensions (such as size) posttreatment and do not find that the estimated treatment effects are affected. With respect to compliance, participation among firms is very high: the compliance rate is 91%, with only four firms dropping out of the course.<sup>8</sup> We also repeat all of our tests on the population of firms assigned to treatment and estimate intention-to-treat effects (ITT). The results are very similar. Managers of treated companies might also change their behaviors and expectations differently from the control firms because of the intervention (see [Chemla and Hennessy \(2019\)](#)). In our setup, this problem is less prominent since both treatment and control managers expect to receive treatment. Finally, attrition might be a concern. While all of the firms signed an agreement to share data, we were not able to collect data from all of the companies, which is potentially problematic if the willingness to share the data is different for the treatment group compared to the control group. We use external data that are not self-reported and show similar treatment effects, addressing the concern that strategic data disclosure is biasing our estimates.

Overall, our results show that the financial expertise of managers is important and that relatively small interventions, such as financial education, improve financial practices and decision making and can ultimately affect economic development. Using the median participating firm with a book value of assets of 6.23 million USD and the estimated DID effect on ROA of 0.205, the after tax impact of the intervention on firm value is estimated

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<sup>8</sup>For the 41 treated firms that took the course, class attendance at the manager level was high at 92%, with 72% of the managers attending all of the classes. Class attendance aggregated at the firm level was higher at 93%, with 85% of the firms (at least one participant per firm) attending all of the classes. High attendance can be explained by the requirement to attend at least 75% of the classes to receive a certificate of participation.

to be 869,000 USD; this estimate is considerably smaller, at approximately 20,000 USD after tax, when we are very conservative and use the lower bound of the confidence interval of the treatment effect. In any case, the treatment effects seem to exceed the direct expected costs of participating in such a program, which are estimated at approximately 10,000 USD, considering the tuition fees.

Given the large positive impacts, why had firms and managers not already taken a finance course? There are several nonmutually exclusive potential reasons for this fact. First, there are no similar courses available locally, significantly raising the total cost of participating in such a program (incl. traveling and opportunity costs). Second, firms might simply not be aware of the benefits of such executive training. [Kremer, Rao, and Schilbach \(2019\)](#) argued that this behavior can also be consistent with the behavioral biases of managers of firms in developing countries, such as an inattention to or an underestimation of returns or an overestimation of the risks involved, for instance.

The main contribution of this study lies in providing the first causal evidence that enhancing the financial expertise of CEOs of large firms can improve firm efficiency by mitigating financing frictions. Our results are in line with previous work arguing that misallocation of capital and labor contributes to the observed Total Factor Productivity (TFP) gap of developing countries with respect to the US ([Hsieh and Klenow \(2009\)](#)). 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".

This study also ties into other strands of the literature. First, we contribute to a growing literature on building managerial capital of small, medium, and larger corporations (see e.g., [McKenzie and Woodruff \(2012\)](#)). Most of these studies focus on management practices (see e.g., [Bloom, Eifert, Mahajan, McKenzie, and Roberts \(2013\)](#) or [Bruhn, Karlan, and Schoar \(2018\)](#)). We contribute to this literature by focusing on financial practices of executives of medium and large corporations, a dimension of management practices that is still understudied but might be particularly important in environments with significant financial frictions.

Second, we contribute to a large 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 \(2009\)](#)). 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 choices. Existing research in this area usually studies microentrepreneurs (e.g., [Karlan and Valdivia \(2011\)](#), [Bruhn and Zia \(2013\)](#), [Drexler, Fischer, and Schoar \(2014\)](#), [Anderson-Macdonald, Chandy, and Zia \(2018\)](#)) 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 are a key barrier to improving financial knowledge ([Carpena, Cole, Shapiro, and Zia \(2011\)](#)). We show that a standard MBA course on corporate finance on sophisticated topics, such as working capital management, delivered in a generic classroom setting, can improve the financial literacy and the corporate finance practices of CEOs of larger corporations, which are arguably more sophisticated subjects.

Finally, we provide causal evidence that individual CEOs matter for corporate decision making and ultimately firm performance. 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, Tate, and Yan \(2011\)](#), [Kaplan, Klebanov, and Sorensen \(2012\)](#), [Hirshleifer, Low, and Teoh \(2012\)](#), [Custodio and Metzger \(2013\)](#), [Custodio and Metzger \(2014\)](#), [Custodio, Ferreira, and Matos \(2017\)](#), or [Schoar and Zuo \(2017\)](#)), an interpretation of the documented associations remained challenging. Identification relied heavily on cross-sectional analysis and panel regressions exploiting within-firm variation due to CEOs switching firms. However, as pointed out by [Fee, Hadlock, and Pierce \(2013\)](#) and [Custodio and Metzger \(2014\)](#), there is the concern that time-varying unobservable characteristics of firm can drive both, the appointment of a specific type of CEO and their firm policies.

The remainder of this paper is structured as follows. The next section provides an overview of financial education and the financial practices of firms in Mozambique. In [Section 3](#), we present the experimental design and describe the executive education program (intervention) and the data collection process. [Section 4](#) shows the results of our intervention based on survey and accounting data. In [Section 5](#), we interpret the findings

and offer some policy recommendations. Section 6 concludes the study.

## **2 Financial Education and Financial Policies of Medium and Large Enterprises in Mozambique**

This section explains our decision to conduct the experiment in Mozambique and the selection of firms considered for the experiment. It also describes the design and outcome of an explorative stage, during which we collected information about the background (including financial education and experience) of CEOs, as well as firms' current financial practices. The results of this explorative stage were helpful for several reasons. First, there are no data on financial expertise and financial policy available for a large set of firms in Mozambique. Understanding the status quo in terms of CEO backgrounds and current finance practices, as well as learning more about the functioning of the financial markets, was important to designing a meaningful course for the target audience. Second, it helped us to understand whether there was an interest in participating in an "executive education" program in finance and the content that could 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.

### **2.1 Mozambique and the Selection of Firms for the Experiment**

We chose Mozambique to conduct the RCT for several reasons. First, we expected to observe more heterogeneity in terms of financial education among executive managers compared to managers of U.S. or European firms due to the lack of executive education programs in finance available in the country.<sup>9</sup> 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) suggested that firms in Mozambique face severe financial frictions (like many other Sub-Saharan African (SSA) 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",

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<sup>9</sup>For instance, there is only one business school providing an MBA program on a regular basis (in cooperation with a Portuguese business school).

"Crime", and "Political Instability". Third, Mozambique has an important advantage for the implementation stage: most large companies' headquarters are located in the capital, Maputo, which helps with the logistics and organization of the intervention and 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, as well as 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 proportion 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. For instance, [Krüger, Landier, and Thesmar \(2015\)](#) showed that firms do not properly adjust for risk in their capital budgeting decisions and that conglomerates underinvest (overinvest) in relatively safe (risky) divisions. In the long run, there might also be some spillover of best financial practices from large firms to smaller ones. First, large firms might be role models for smaller firms, and those firms might adopt some of the practices of large corporations. Second, there might be some direct knowledge/practice spillover originating from human capital that is moving across companies. Both channels are likely to be more prominent in large firms.

In addition, financial literacy has mostly been studied in the context of small enterprises (e.g., [Drexler, Fischer, and Schoar \(2014\)](#)) but little is known at the level of large corporations beyond there being a correlation between financial expertise and financial policies ([Custodio and Metzger \(2014\)](#); [Güner, Malmendier, and Tate \(2008\)](#)).

## **2.2 Financial Practices of Firms**

An explorative stage of the project occurred in 2015, during which we collected information about managers, including demographics and financial education and experience, as well as the firms' characteristics and financial policies. We used this information to design the executive education program and to compare the financial expertise and finance policies of medium and large enterprises in Mozambique with US evidence. The exploratory stage ran between June and July 2015 (see [Figure 1](#)). 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 the 65 meetings, we were able to collect 63 questionnaires.<sup>10</sup> 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 nonfinance related staff member.

These questionnaires surveyed the financial practices, manager characteristics and overall business aspects of the companies, following [Graham and Harvey \(2001\)](#) and [Graham and Harvey \(2002\)](#). We also used the survey to assess the interests of managers in a free of charge executive program on financial management. We specifically asked which topics that they would find more relevant, including capital budgeting, risk management, capital structure, working capital management, pay-out policy and mergers and acquisitions. Finally, we inquired about the executives' time availability for such a program to maximize attendance.

These questionnaires also allowed us to have a first look at financial expertise, financial policies, and the interaction between the two in Mozambique. We start by documenting substantial heterogeneity in financial expertise by CEOs in Mozambique. Approximately 43% of the CEOs have a background in finance, either by education or work experience. When analyzing financial practices in firms with and without financial expert CEOs, we find large differences in their practices. For example, [Figure 2](#) shows financial practices related to capital budgeting/valuation by firms run by financial expert CEOs, compared to nonfinancial 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

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<sup>10</sup>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.

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).

### 3 Design and Implementation of the Experiment

This section presents the experimental design and sample description. It then details the content of the program, as well as its implementation. Finally, we discuss the data collection procedure.

#### 3.1 Experimental Design and Sample Description

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 obtain financial education; and ii) limited availability of data.

While the documented correlations between the financial expertise of CEOs and their financial practices in the previous section are consistent with the view that CEO education affects financial policies, a clean interpretation of these correlations remains difficult. Researchers have examined whether corporate outcomes are affected by CEO characteristics, but no consensus has been reached (e.g., see [Chemmanur and Simonyan \(2017\)](#) for a survey of the literature). CEOs and firms are not randomly matched, and there is the concern that endogenous matching biases the estimates. Indeed, the literature on the effects of managerial human capital on firm policies has relied heavily on cross-sectional analysis, rendering makes causal inference very challenging. Some studies, such as [Bertrand and Schoar \(2003\)](#), have used panel regressions and have estimated potential CEO effects using within-firm variation due to CEOs switching firms. However, [Fee, Hadlock, and Pierce \(2013\)](#) cast doubt on this methodology for identifying managerial style 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. In other words, while firm-fixed effects allow for controlling unobserved firm heterogeneity that is time invariant, it cannot be excluding that firm time-varying characteristics, unobserved by the econometrician, such as some strategic decisions, drive both financial policies and the characteristics of the CEO who is appointed. In the context of financial expertise, [Custodio and Metzger \(2014\)](#) showed that firms run by managers that

have past work experience in finance have better access to external financing and allocate their firms' financial resources more efficiently. At the same time, however, they also provided evidence that financial expert CEOs are more likely to be appointed by more mature firms. These findings are also consistent with [Anderson-Macdonald, Chandy, and Zia \(2018\)](#), who studied the impact of marketing vs finance skills on the business performance of small-scale entrepreneurs. They found that more established businesses benefit significantly more from finance skills. 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 would be an actual random allocation of CEOs to firms. Unfortunately, this type of experiment is not feasible in practice. We propose a different solution, by randomizing financial education of top managers and, at the same time, maintaining the match between CEOs and firms constant. 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.

A second challenge for our study is the availability of data. First, most companies 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 these statements. For this reason, we provide incentives to firms to share financial statements with us and complement these statements with survey data from interviews, allowing us to collect nonstandard data. For a large set of firms, we complement these data with accounting information directly from external reports, including "Top 100 Companies in Mozambique", published annually by KPMG Mozambique.<sup>11</sup> The last piece of data allows us to validate the self-reported data and helps to address the concern that some firms might be strategic in their choice of sharing data with us.<sup>12</sup>

We construct the treatment and control groups of our experiment in two steps. First, we invite 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 ap-

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<sup>11</sup>These reports contain the names and information of many of the largest corporations in Mozambique. These reports are publicly available and are used by local and foreign investors, public administrations and other institutions. 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 ranks 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.

<sup>12</sup>We discuss the concern related to attrition in more detail in Section 4.5.2 of the paper.

pearing in a KPMG report at least once in the period of 2009-2016 (391 companies). Additionally, we invited companies associated with local business associations, namely, CTA (Confederação das Associações Económicas de Moçambique) and ACIS (186 companies).<sup>13</sup> We restrict our sample to companies headquartered in Maputo,<sup>14</sup> enabling in-person interaction with participants, which was crucial throughout the project to engage the participants with the program and to facilitate data collection. This requirement also reduced noncompliance of participants since it minimized the participants' cost of attending the training. We focused on executives in these companies since they usually take most strategic decisions, including financial decisions (see [Graham, Harvey, and Puri \(2015\)](#)).

To address the concern of endogenous selection into the treatment, we randomly allocated firms that applied to the program into two groups: the treatment group and the control group. The randomization stratified by industry to ensure that the same industries were represented in both groups.<sup>15</sup> We then implemented the treatment, the course on finance, for the two groups in a staggered manner. 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.<sup>16</sup> The reasoning for teaching the control group too had the following rationale. First, it incentivizes the control group to share their accounting data and participate in the surveys. Second, it helps to address the concern that the formation of expectations could bias the experiment ([Chemla and Hennessy \(2019\)](#)) because the treatment and control groups both expect to be treated.

The advertised course was an Executive-level Program in Finance – "Finance and Strategy: Value Creation in Emerging Markets" – promoted under Imperial College Ex-

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<sup>13</sup>We partnered with these two business associations since their work receives national recognition. This fact contributed to raising public awareness about our project.

<sup>14</sup>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 extractive industries (those located in specific regions of the country and usually outside Maputo). Of 19 remaining firms, 16 were invited to participate in our project.

<sup>15</sup>The randomization procedure was conducted at the company level and stratified by industry. As noted by Sutton (2014), a sample stratified by industry provides a "fair and complete picture of the country's industrial capabilities". However, there were several business groups in our sample (i.e., one manager might oversee several companies belonging to the same group). Given that the intervention is at the manager level, we could not allow for treatment and control companies with the same manager. Therefore, after an initial random assignment for the pool of companies, we observed the assignment of the most relevant company (according to size) in each business group and extended this assignment to all companies with managers.

<sup>16</sup>While not affecting the internal validity of the experiment, it is still interesting to analyze the characteristics of firms and executives interested in attending the course versus who are not. Please see also Table A11 in the appendix of the paper.



Executive Education branding. The course was offered in Maputo free of charge and was limited 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.<sup>17</sup>

Upon receiving 109 positive responses, 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). We allowed each company to send up to two attendees, provided that at least one of them was a senior manager.<sup>18</sup> We received application forms from 111 participants, corresponding to 93 firms. These companies were then randomly allocated 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 cohort. Forty-six managers effectively participated in the program, representing 41 companies and 31 business groups (Table 1).

Panel A of Table 2 shows summary statistics for the participating firms (treatment and control groups) and differences between the two groups in the year before the intervention. 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 two 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 are much smaller.<sup>19</sup>

Panel B of Table 2 shows summary statistics for the top managers 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 master's degree or

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<sup>17</sup>See an excerpt of the brochure in the appendix of the paper (A1).

<sup>18</sup>We required one application form per attendee.

<sup>19</sup>Appendix Table A13 describes how each variable is constructed, as well as its sources.



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. 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.

Although the experimental setup helps to identify a causal effect of financial executive education on firm outcomes, there might be still limitations in practice. For instance, attendance of the course is voluntary, and (failure to) share the data might be nonrandom. Moreover, the mechanisms through which a finance executive education course impacts firm outcomes might be wider than the learning channel itself. Indeed, the classroom experience could affect dimensions not directly related to the content of the course. Managers might benefit from networking with managers of other firms while attending the course. If they start doing business together, it can eventually translate into higher revenues, although it is unrelated to learning. To alleviate some of these potentially confounding effects of networking, we organized a networking event for companies in the control group. This event occurred around the dates of the first intervention, i.e., when the treatment group attended the course. We discuss the networking event, as well as some other threats to the internal validity and the interpretation of our findings, in detail in Sections 4.5 and 5 of the paper.

### **3.2 Design of the Course**

The course was designed as a general course in corporate finance but emphasized topics identified as weaknesses by the managers in the explorative stage (see Section 2). The proposed course 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 consisted of 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 CAPM, as tools to estimate project discount rates. Some common valuation mistakes, such as misuse of the WACC irrespective of the specific risk of the project,

were also covered in the course.

2. **Capital Structure** This module presented a practical view of assessing the optimal capital structure of the firm, listing the advantages and disadvantages of debt financing, such as the tax shield of debt and bankruptcy costs, respectively.
3. **Working Capital Management** This topic 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 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.
4. **Risk Management** This module covered the identification of risks and associated potential costs, analysis of the causes of risk of financial loss, determination of various hedging strategies, implementation of risk management strategies, and management and monitoring of results. The approach to this topic was that an effective risk management program can reduce losses and improve financial performance.

The intended learning outcomes of these four modules can be summarized as follows:

1. Read, understand and process (e.g., calculate basic financial ratios) financial information from financial reports;
2. Understand the impact of working capital management on firm liquidity and funding needs;
3. Understand the appropriate valuation techniques to use when making capital budgeting decisions and avoid common mistakes in valuation, for instance, by not considering the time value of money;
4. Understand the trade-off between the costs and benefits of a given financial structure and source of financing; and
5. Identify sources of risk and risk management practices, for instance, hedging using insurance or financial instruments.

The course was organized in four modules spanning 18 hours (4.5 hours each).<sup>20</sup> While this duration might appear relatively short, courses in related studies have similar

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<sup>20</sup>Table A12 in the appendix provides a more detailed overview of the schedule.

durations (e.g., two days or two half days (Bruhn and Zia (2013) and Field, Jayachandran, and Pande (2010))). Moreover, our course is at the shorter end but in line with sessions on related topics in typical MBA core courses in corporate finance. Given that the participants were top executives, our survey results 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.<sup>21</sup> At the same time, shorter courses are less expensive and simpler to organize logistically – a potentially important criterion from a policy point of view.

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 in the US and Brazil). 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 treated group and by two different instructors in the case of the control group. Participants who attended a minimum of 75% of the classes received a participation certificate from Imperial College Business School.

### **3.3 Delivery of the Course and Data Collection**

This section describes the timing of the different interventions, the courses for the treatment and control groups, a networking event for the control group, and the data collection process in more detail.

#### **3.3.1 Intervention 1: Delivery of the Course for Cohort 1 (Treatment Group) and Networking Event for Cohort 2 (Control Group)**

The first edition of the course took place in May 2017. Of the 45 treatment firms, 41 attended and stayed for the full duration of the course (participation rate of 91%).<sup>22</sup> Figure

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<sup>21</sup>It might be interesting to experiment with the length of such a course in future extensions of this project.

<sup>22</sup>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

3 reports the number of companies participating in different stages of the project. Before the start of the course, participants were required to complete a prelearning 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 postlearning exit survey. This survey was divided into a confidential part, in which participants were asked to evaluate the course, and a nonconfidential part, in which they described their intentions to change financial practices in the future.

A concern that remains is that there are confounding effects related to the treatment. In particular, network effects, instead of the content of the course itself, could lead to changes in some outcomes of interest. While these potential network effects are less obvious for financial policies, we are more concerned with them affecting profitability. Profitability is a critical outcome in understanding 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 or knowledge. To address this concern, we organized an afternoon networking event for the control group, the purpose of which was to give the control group the opportunity to mingle and network. This event featured a short presentation of the executive education program, as well as speeches by high-profile individuals from the public and private sectors.<sup>23</sup>

### **3.3.2 Intervention 2: Delivery of the Course for Cohort 2 (Control Group)**

Between September and November 2018, we contacted and visited companies in the control group (40 meetings of 48 control companies). In these meetings, we administered the prelearning questionnaire (similar to that applied in the treatment group). Moreover, we also asked which financial practices had changed since May 2017 and investigated expectations regarding future changes. This survey was intended to provide a counterfactual for implemented changes in financial practices by the treatment group. In these interviews, we also requested financial accounting data.

In a few cases, the manager that had applied to the program was replaced. For these

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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 in Angola, whereas another firm was experiencing an internal re-structuring that required the manager's presence

<sup>23</sup>Importantly, the network event did not overlap with the content of the course.

cases, we briefed the new manager about the program and invited her or him to participate in the second intervention. 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 postlearning exit survey as described in the previous subsection.

Of 48 control companies, 27 attended on the days of the course (participation rate of 56%).

### **3.3.3 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) changes in policies related to the four topics of the course: valuation techniques, working capital management, capital structure, and risk management. Valuation techniques and risk management are difficult (or impossible) to directly measure in the available financial reports, so we must restrict our analysis to working capital management and capital structure decisions when using accounting data.

Approximately 15 months after the first intervention, between September 2018 and November 2018, we surveyed managers in the treatment group. We asked them about implemented changes with respect to financial policies since the first intervention. We also requested their financial reporting data between 2013 and 2018. We provided companies with a template spreadsheet, including a balance sheet, an income statement and a statement of cash flows items, to complete.

We also collected financial information for the period between 2008 and 2018 from the report "Top 100 Companies in Mozambique" published annually by KPMG Mozambique. We use these data to add financial information from companies that did not or could not share their financial data and to assess the quality and consistency of the data provided by the companies through the Microsoft Excel spreadsheets (for some companies we have both self-reported hand collected data and KPMG reports data). This information was collected for both treatment and control firms. Financial data were available in dollars and/or metical depending on the source. We converted all values in metical to dollars using the exchange rate on the reporting date. Out 93 participating companies, we were

able to obtain at least one year of financial data for 86 companies.

## **4 The Effect of Financial Education on Financial Policies and Efficiency**

This section analyzes the effect of financial education on financial policies. We measure the intentions of treated firms to implement changes in financial policies after the courses in May 2017 and November 2018/April 2019. We also compare implemented changes in the financial policies of firms taught in May 2017 (treated firms) and firms yet to be treated (control firms) in September-October 2018, i.e., before the delivery of the course to the control group. We use both survey evidence and accounting data to measure the outcomes of interest.

### **4.1 Intention to Change Financial Policies (Exit Survey)**

We start our analysis by evaluating the intentions of treated firms to change financial policies. We analyze valuation techniques, working capital management, capital structure, and risk management, which are the main themes of the delivered courses. Table 3 shows the results of the exit surveys by the participants at the end of the courses.

Panel A of Table 3 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 cannot adjust a particular policy. Capital structure appears to be the policy over which managers have the least discretion. Almost 40% of the companies (13 of 34) say 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 that have the discretion to set their own policies, disregarding cases in which managers did not answer a question ("missing"), between 38% and 73% intend to implement changes in their policies that were discussed in the course. When we treat missing answers as "no", the corresponding numbers are between 48% and 73%. iii) Depending on the policy, there is substantial het-

erogeneity in the intention intensity. Working capital management and risk management are the policies that managers are most likely to change (73% and 70%, respectively). There are fewer intended changes in capital structure and valuation techniques (48% and 42%, respectively).

Panel B shows the corresponding results when we include the answers of the second cohort that was treated in 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.

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

Although firms express their intentions to change several corporate policies, it remains unclear to what extent they (are able to) implement these changes. To shed light on actual implementation, we surveyed participating companies, i.e., treatment and control firms, approximately 15 months after the first intervention and before the second intervention. There are indeed 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 priorities, external conditions might impose constraints, etc. Moreover, there could be reasons unrelated to the treatment that lead firms to change their policies. To better understand the effect of the treatment itself, we explicitly asked firms whether they changed firm policies *because* of the course. More importantly, we also surveyed the population of control firms, allowing us to compare changes in financial policies between treatment and control firms as well.

Table 4 shows the results. First, between 7.7% and 30.8% of the firms mention that they implemented changes in financial policies in the preceding 15 months. Not unexpectedly, the implementation rates are much smaller compared to the intentions reported in the exit survey. Consistent with the exit survey, working capital management is the most affected policy (approximately one third of treated companies state that they have implemented changes in their working capital management). Capital structures and valuation techniques are relatively less revised consistently across the two surveys. One exception is risk management, which ranked very high on the list at the exit survey, but only very few companies (two companies) state that they have implemented changes 15 months later. In the survey, we also asked for reasons that prevented firms from imple-

menting 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 4 ).

While these results are suggestive, we can also use the control group to address the concern that we could capture a pure time-effect, for instance. Indeed, 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 4 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 4 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 reveal 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) using shortened 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 also 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.



### 4.3 Changes in Financial Policies (Financial Accounting Data)

While the previous two sections use survey data by treatment and treatment and control firms, we can also measure changes in financial policies in their financial reports. The financial statements contain information that allow us to investigate potential changes in working capital management and capital structure. Changes in risk management and valuation techniques are more difficult to measure without survey data. The financial data also allow us to measure the potential efficiency effects of the executive education program.

Table 5 reports the estimates of treatment effects on main financial policies 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. In most regressions, we cluster standard errors at the firm level; standard errors are bootstrapped in specification (3). To take into account any variations in the data that arise from randomization itself, we also report randomization-t p-values using the algorithm by Young (2019) in Table A1 in the Appendix.<sup>24</sup>

As suggested by the survey evidence in Sections 4.1 and 4.2, we start our analysis by investigating changes to the working capital (WC) management in panel A of Table 5. The coefficient of interest is the interaction term, corresponding to a difference-in-difference estimate. In columns (1) to (5), we scale WC by lagged assets, and in columns (6) to (10), WC is scaled by contemporaneous sales. When we scale WC by lagged assets, we find a point estimate of  $-0.170$  that is significant at the 10% level, corresponding to a negative impact on working capital of 0.49 standard deviations (based on the pooled sample of treatment and control firms). Columns (2)-(5) show firm fixed effect estimates. We find similar, slightly larger coefficients between  $-0.198$  and  $-0.216$ . 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 – treated firms decrease their working capital by approximately 0.44 standard deviations – and significant at the 1% level. These estimates

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<sup>24</sup>For most outcomes, significance levels remain unchanged. In two cases, for inventories and ROIC, p-values fall below 5% when using randomization-t p-values.

are consistent with the minimum detectable effects for working capital calculated using pre-treatment KPMG data of 0.199, as reported in Table A2 in the Appendix.

In panels B and C of Table 5, we analyze the different components of working capital in greater detail. Consistent with evidence from the 15-month survey (see Section 4.2), we find large and significant effects on accounts receivable (A/R). The difference-in-difference estimate is approximately  $-18\%$ , corresponding to a drop of approximately 0.61 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), which is consistent with the survey evidence as well. We can only speculate why firms change A/R but not A/P after the intervention. One potential reason is that firms can more easily change their own terms (with clients), while negotiating longer payment periods with suppliers might be more difficult. Finally, we also find a negative effect on inventories. The point estimates range between  $-0.149$  and  $-0.160$  and are significantly different from zero at the 10% level (and at the 5% level when we estimate randomization p-values, reported in Table A1 in the Appendix).

Overall, the results regarding working capital management suggest that firms respond to the treatment by decreasing the collection period, as well as their inventories. This reduction in working capital leads to a cash inflow, potentially affecting other corporate policies beyond a direct effect of the treatment. Table 6 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, these findings are also well in line with the survey evidence on capital structure decisions, in which only 3 companies stated that they implemented changes. As discussed before, some firms are subsidiaries of larger (often international) corporations and do not have discretion over these policies. They also claim that credit markets in Mozambique are tight, and it is very difficult or too expensive to obtain debt. We calculated minimum detectable effects in cash holdings and leverage to be 0.097 and 0.183, which are reasonably larger than the estimated coefficients

(see Table A2 in the Appendix).

If companies do not change their capital structures or their cash holdings in response to the inflow of cash after the reduction of their working capital, it is interesting to investigate how this cash is used. For instance, companies can increase 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 6, we document a positive and significant treatment effect of the course: 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. Using the lower bound of the confidence intervals as a much more conservative estimate, the total impact on cash flow is 0.19 million USD, which is a short term, one-off effect on cash flow as a result of the change in working capital. 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. Because we find a positive treatment effect on capex, we estimate the corresponding cash outflow. We find an average cash flow impact of  $-0.81$  million USD, with a conservative estimate of 0.21 million USD.

#### **4.4 Efficiency of Implemented Changes in Financial Policies (Financial 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 small or if collection periods are too short. Customers might be scared away because of products being out of stock or unattractive payment options, for instance.

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. One limitation of these accounting measures is that they are not forward looking and only capture potential adverse effects that materialize in the short run, and we cannot exclude, for instance, that sales will decrease over a longer horizon.

Table 7 shows the regression on firm performance. Panel A shows the treatment effect on ROA. We find a positive impact on firm performance between 0.21 and 0.22 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 1.47 using OLS and 1.56 using firm fixed effects, representing between 0.75 and 0.80 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 A1 in the Appendix). The point estimates of the most treatment effects are large but not implausible, particularly given that the confidence intervals include more modest estimates as well. These estimates are consistent, and slightly above the minimum detectable effects for ROA calculated using pre-treatment KPMG data of 0.154, as reported in Table A2 in the Appendix.

Last, we analyze sales growth to test whether there are any adverse effects of reducing inventories or collecting receivables more quickly. We do not find evidence of such an effect. The point estimates of the intervention on sales growth are actually positive, although they are not significantly different from zero. 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.

#### **4.5 Threats to Internal Validity and Robustness Tests**

While the experimental setup identifies the 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 some of these threats in more detail and provide additional tests on the internal validity.

### 4.5.1 Compliance

In our main analysis, we estimate the average effect of treatment on the treated (ATT). While we have a very high compliance rate of approximately 91% (compared to approximately 53% in Bruhn, Karlan, and Schoar (2018), for instance), there is the concern that the firms that did not attend the course could bias our results. Ex ante, the direction of this bias is unclear, however. For instance, it might be the case that only “good” firms do not attend the course because they do not expect to profit from participating in the course; it might also be the case that firms that are in trouble do not attend the course since they are too busy otherwise. Badly performing firms dropping out of the sample would indeed be in line with our results on ROA but it would be more difficult to tell a consistent story about why these remaining firms also decrease their working capital. In practice, however, we do not believe that there are systematic reasons for why firms that initially enrolled in the course did not attend. For instance, two CEOs who initially expressed their interest in participating in the course (and were allocated to the treatment group) never replied to our invitation to enroll in the course; one other CEO had an unexpected meeting abroad during the period of the first intervention.

Nevertheless, we can include these four treatment firms that did not attend the course and estimate the intention-to-treat (ITT) effect of our intervention.<sup>25</sup> Table A3 in the appendix shows the results for our main variables of interest. We do not find any strong evidence of certain types of firms systematically dropping out. The point estimates and standard errors remain basically unchanged.

### 4.5.2 Attrition

While all of the firms signed a data agreement at enrollment in the program, not all firms could share their data in the end. There is the concern that compliance with sharing financial data is systematically different for firms from the treatment and control groups. 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 case was true for firms from both the treatment and the control groups, the difference-in-difference estimates might be still unbiased, however. Nevertheless, it would be concerning were badly performing firms

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<sup>25</sup>Bruhn, Karlan, and Schoar (2018) estimated ITT as their main specification.

from the treatment group not to share their data but the same were not true for badly performing firms from the control group. In this case, sample selection could bias our findings.

We address this potential threat to the internal validity of our experiment in the following way. For a substantial subset of our data (i.e., firm-years), we also have access to external accounting data from a large accounting firm (KPMG), 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-difference effect for a subset of outcomes. Unfortunately, the granularity of the accounting data by the third party does not allow us to estimate the effect of the intervention on all of the different components of working capital. Table A4 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 Pretrends**

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, there is heterogeneity between the treatment and control groups that is driving our findings. Table 2 shows that firms and the managers of those firms are not too systematically different. Almost all of the differences in means and especially at the median are not significantly different between the two groups. Normalized differences do not exceed one as well. However, we can also make use 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 were the two groups on different levels, as long as they were not on different pretrends (common trend assumption in difference-in-difference tests). We test this assumption nonparametrically by plotting corresponding graphs for the main outcomes.

Figure 4 shows averages of these financial policies for firms in the treatment and control groups over the 2015-2018 period. The figures illustrate that, despite some differences

in levels before the intervention, the treatment and control groups usually have parallel trends (especially in 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 treated and control groups follow parallel trends before the intervention across a majority of outcomes of interest.

One remaining 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 consider five different specifications of firm size as well as the nationality of the CEOs in additional tests. 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 posttreatment period by also including an interaction term  $\ln \text{Size} \times \text{Post}$ . In specification (2), we allow for a different functional form and include  $\text{Size}$ ,  $\text{Size}^2$ , and  $\text{Size}^3$  as well as their interactions with  $\text{Post}$  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.

We report the results in Table A5. 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. 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 Other Threats to the Internal Validity of the Experiment**

In this section, we discuss other threats to the internal validity that are not explicitly testable (or we have not found a good way of doing so).

There is the concern 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, it would actually work against us finding any results. We also attempted to prevent 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.

Another concern is that the managers of treated companies change their behaviors and update their expectations differently from the control firms because of the intervention. For instance, managers might update their beliefs with respect to future firm performance and respond accordingly (see [Chemla and Hennessy \(2019\)](#)). In our setup, this problem is less prominent since both the treatment and control managers expect to receive the same treatment.

#### **4.5.5 Robustness Checks**

We also run a battery of additional robustness tests. To take into account any variations in the data that arise from randomization itself, we also report randomization-t p-values using the algorithm by [Young \(2019\)](#) in Table A1 in the Appendix. Results remain largely unchanged; p-values for inventories and *ROIC* drop below the 5% level when using this alternative algorithm.



Some firms belong to the same business group. As a first robustness test, we exclude all noncore subsidiaries from our data. Table A6 shows the results for the main outcomes of interest. The results are unchanged (the point estimates are even slightly higher).

Then, we consider different time windows in our estimation of the treatment effect. Table A7 shows results for our main outcomes when we consider data after 2013, 2015, or 2016. While the point estimates slightly change depending on the time period, the qualitative results remain unchanged.

Because some firms experienced CEO turnover during the period of the experiment, we have excluded them from the analysis. Table A8 shows the results, which remain qualitatively unchanged. We have fewer observations, however, and some coefficients are only significant at the 10-percent level.

We exclude firms that also operate in the financial industry (e.g., insurance companies). Table A9 shows the results. The main results remain unchanged.

To 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 A10 displays the results when we scale the outcomes by contemporaneous book value of assets. The results are consistent with our baseline definition.

## 5 Interpretation and Policy Recommendations

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

The treatment, i.e., participation in the executive education program, is basically a bundle of different simultaneous experiences: i) there is potential learning from the instructor; ii) there is potential learning from classmates; and iii) there might be other aspects of the classroom experience, unrelated to the content of the course, that could affect outcomes (e.g., networking and generation of new deals between participants). It is difficult to identify the exact learning channel. However, we have several pieces of evidence that suggest that networking is *not* the main driver of our findings. While the

results regarding *ROA* could be potentially in line 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 7). This outcome is not too surprising since the firms came from different segments, and opportunities to establish business ties appear rather limited. Moreover, the documented changes in working capital are also not easy to reconcile with a network-only story. Finally, 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. 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. However, considered together, 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-studies or by enrolling into an online course, is also related to the question of the frictions that prevented executives from obtaining an education in finance earlier. One potential reason is simply unawareness of the importance of finance education for corporate efficiency. In this case, self-studies 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 (yet). Online courses or textbooks might be only 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 (money and timewise) 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) 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 groups.

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 can certainly not answer this question with our setup, we believe that it is still valuable to speculate about potential welfare implications. First, one could argue that large parts of the effect of the intervention affect working capital, and specifically accounts receivable. 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. One might also argue, however, that some customers are from abroad, including customers or firms from developed countries. In such as cases, the economy of Mozambique is likely to benefit. Moreover, we show that other policies, such as inventories or capital expenditures, are affected as well, potentially increasing the productivity of firms. Finally, there might also be other policies that are more difficult to measure that benefit from the improved financial decisions.

Whether our findings can be generalized to other firms inside or outside Mozambique is another difficult question. We provide several tests, however, that support the external validity of our findings. When we compare firms that applied to our program with other firms present in the KPMG reports but that did not apply, 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 firms in Mozambique present on the LinkedIn network. Table A11 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 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, 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 sample in [Graham and Harvey \(2001\)](#). The results are presented in panel C of Table A11. When we restrict the 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 want to 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 expertise on firm financial policies and performance. A randomized, controlled trial with top managers of 93 medium and large companies in Mozambique shows a positive effect on firm return on assets of an 18-hour executive education program in finance. Our results suggest that deficiencies in managerial financial expertise at large firms can be an important constraint on firm performance.

Using survey data and firm financial information, we find that managers changed firm financial policies after the intervention. We find a significant and large treatment effect regarding working capital and average collection periods. The effects on working capital management are large and significant: working capital decreases by 0.49-0.62 standard deviations for the treated firms, compared to the control group. This effect is likely to alleviate, at least in the short run, potential financial constraints. The effects on firm performance are economically relevant: ROA increases by approximately 0.85 standard deviations for the treated group, compared to the control firms.

These results confirm that financial expertise of managers has a large impact on firm performance through the adoption of financial practices that promote value creation and alleviate financial constraints at the firm level. Moreover, our results suggest that relatively low-cost interventions, such as an 18-hour executive education course on corporate finance and risk management, 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 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 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, 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, Cole, Shapiro, and Zia (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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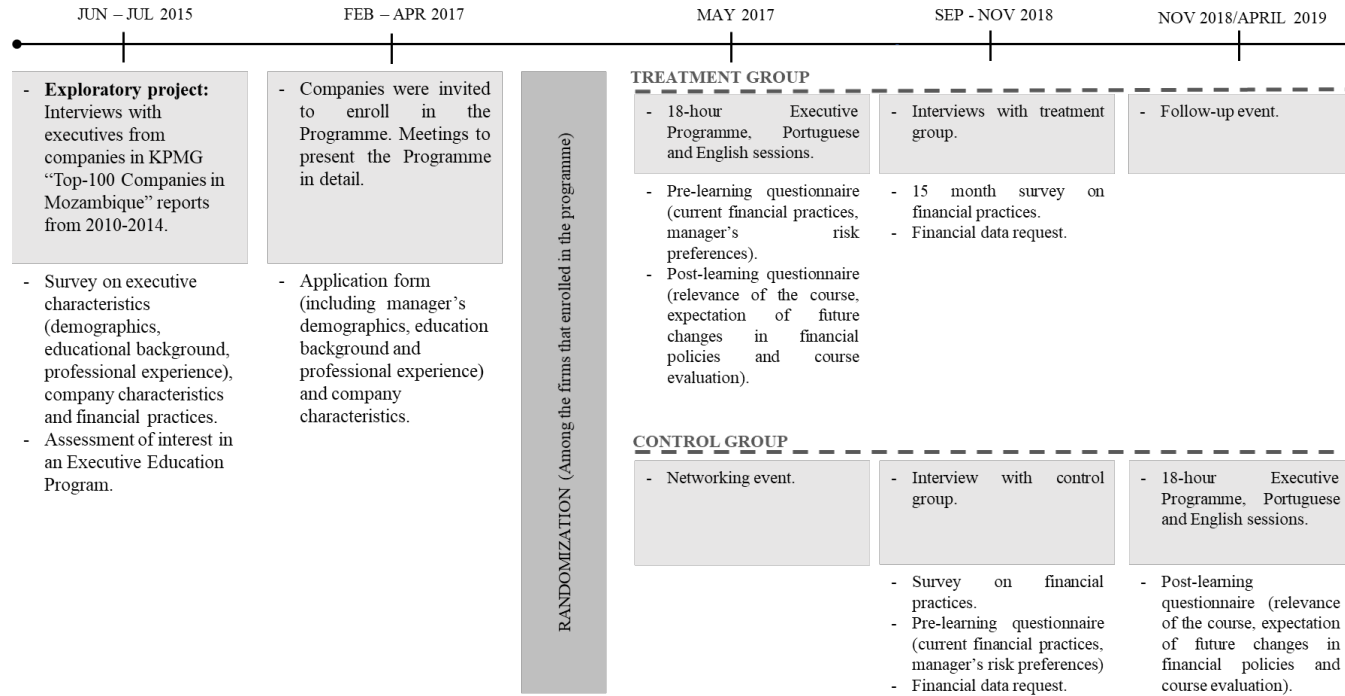
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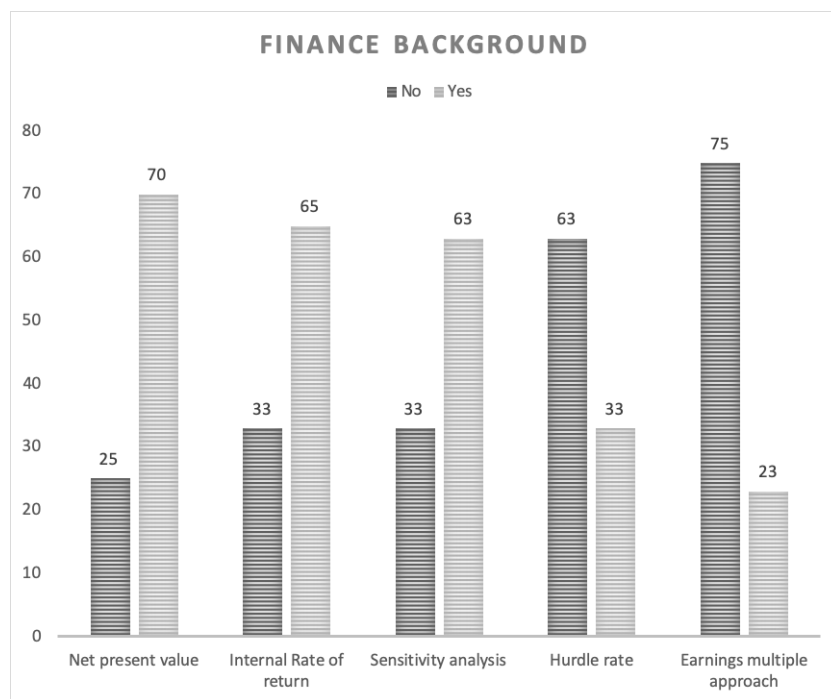
## 7 Figures

Figure 1: Project Timeline



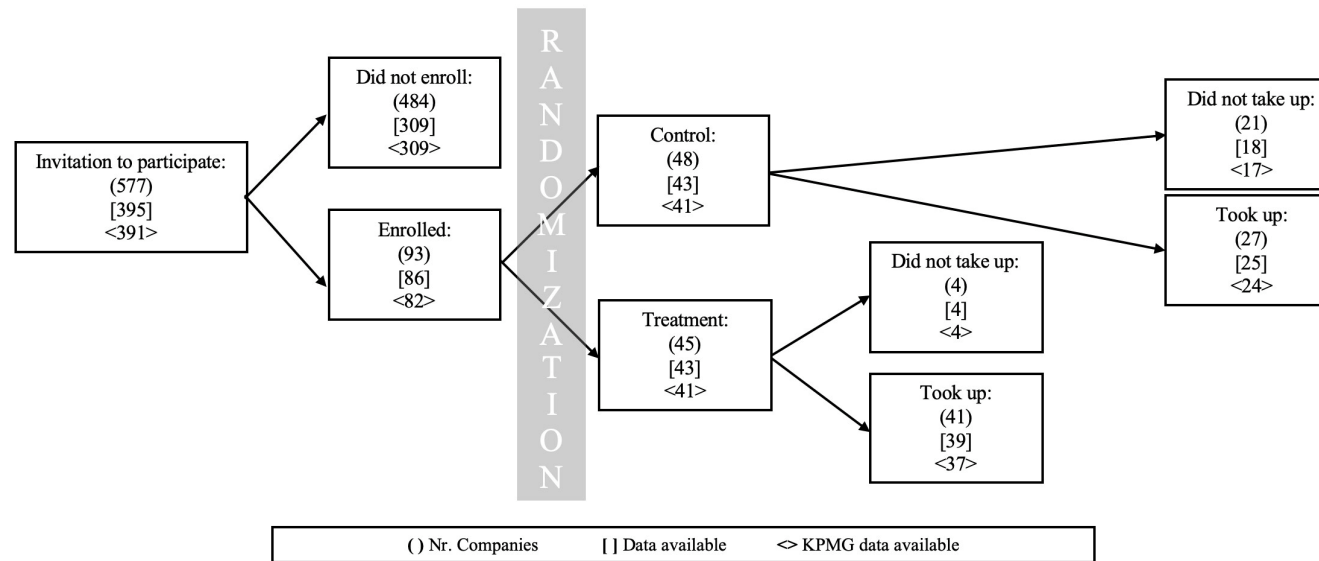
This timeline describes the field work between June 2015 and April 2019. For each stage, it describes the work performed, as well as the information collected regarding companies and managers.

**Figure 2: Financial Experience and Financial Policies**



This graph displays the percentage of managers using different valuation techniques according to financial experience. Financial experience is defined as a 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.

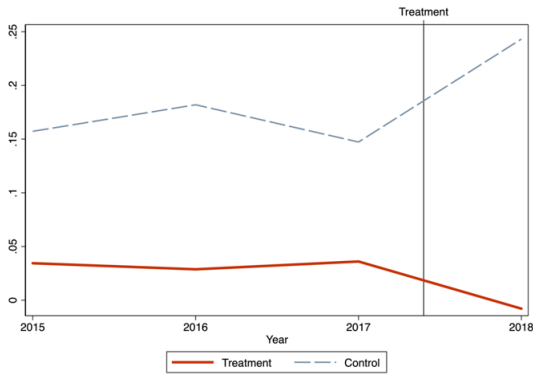
**Figure 3:** Numbers of Companies in Different Stages of the Experiment



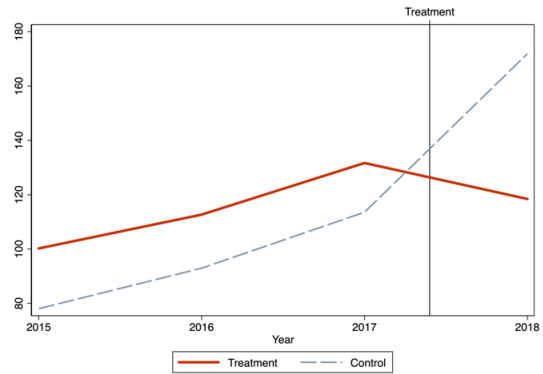
This diagram shows the numbers of companies participating in each stage of the experiment (round brackets). It also reports the numbers of companies for which we have at least one year of financial data, either from KPMG or self-reported data (square brackets) or from KPMG (angle brackets).

**Figure 4: Evolution of Selected Financial Outcomes**

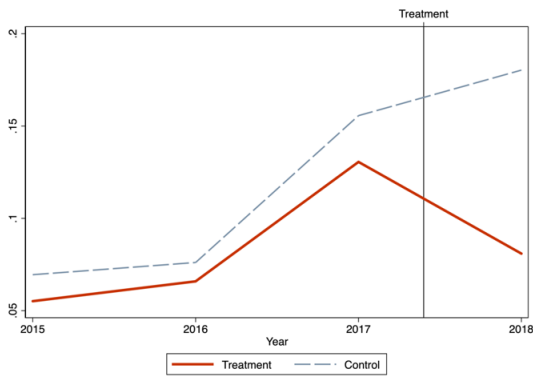
**(a) Working Capital/(Lag) Assets**



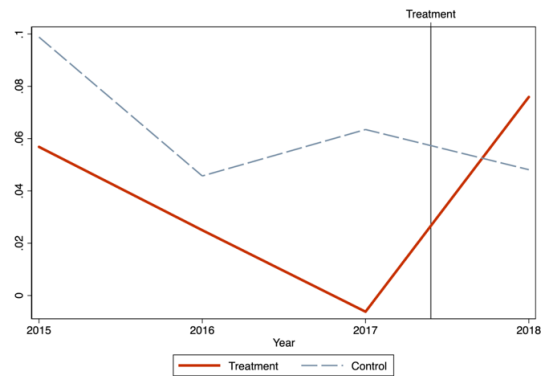
**(b) Average Collection Period**



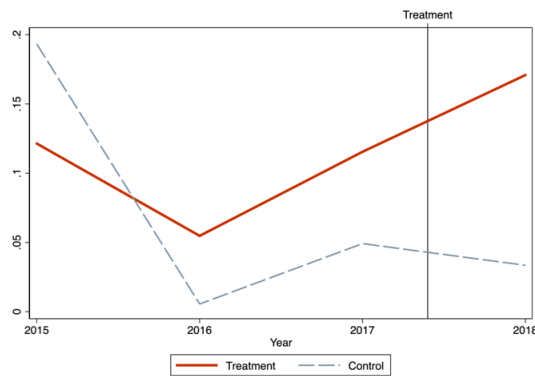
**(c) Inventories/Sales**



**(d) Capex/Assets**



**(e) ROA**



The graphs present mean financial outcomes over time for 85 firms included in the treatment and control samples. Financial outcomes are Working Capital, Average Collection Period, Inventories, Capital Expenditure and Return on Assets (ROA). The vertical line denotes the date of the first intervention (treatment group). On the horizontal axis, each date represents the beginning of each year.

## 8 Tables

**Table 1: Number of Managers and Companies Participating in the Programme**

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

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

**Table 2: Baseline Summary Statistics**

<b>Panel A: Treatment/Control</b>													
	<b>Treatment</b>				<b>Control</b>				<b>Mean</b>		<b>Median</b>		<b>Norm.</b>
	<b>Obs.</b>	<b>Mean</b>	<b>Median</b>	<b>St.Dev.</b>	<b>Obs.</b>	<b>Mean</b>	<b>Median</b>	<b>St.Dev.</b>	<b>Diff.</b>	<b>p-value</b>	<b>Diff.</b>	<b>p-value</b>	<b>Diff.</b>
Total Assets (m USD)	34	22.29	4.6	47.6	42	126.33	9.86	339.06	-104.04	0.08	-5.26	0.49	0.43
Sales (m USD)	34	15.84	3.12	38.94	42	58.94	8.32	132.9	-43.1	0.07	-5.2	0.11	0.44
Sales Growth	34	-0.35	-0.36	0.23	38	-0.14	-0.27	0.76	-0.21	0.12	-0.09	0.1	0.37
# Employees	32	191.06	81.5	248.63	38	308.26	102.5	541.02	-117	0.26	-21	0.81	0.28
Cash / Assets	19	0.1	0.06	0.1	26	0.17	0.11	0.21	-0.07	0.21	-0.05	0.09	0.43
Leverage	25	0.15	0.1	0.2	32	0.26	0.11	0.37	-0.11	0.2	-0.01	0.91	0.37
Capex / Assets	23	-0.01	0	0.11	36	0.06	0.01	0.33	-0.07	0.33	-0.01	0.92	0.28
ROA	33	0.12	0.05	0.27	41	0.05	0.07	0.22	0.07	0.25	-0.02	0.64	0.28
ROIC	32	-0.04	0.04	1.37	41	0.74	0.18	2.23	-0.78	0.09	-0.14	0.28	0.42
NWC / (Lag) Assets	24	0.04	-0.05	0.41	38	0.15	0.17	0.31	-0.11	0.23	-0.22	0.19	0.3
NWC / Sales	24	0.13	-0.03	0.91	38	0.12	0.14	0.59	0.01	0.96	-0.17	0.19	0.01
Inventories / Sales	18	0.13	0.05	0.23	25	0.16	0.03	0.24	-0.03	0.73	0.02	0.66	0.13
A/R / Sales	19	0.36	0.27	0.32	26	0.31	0.23	0.28	0.05	0.59	0.04	0.9	0.17
A/P / Sales	18	0.32	0.12	0.41	26	0.23	0.12	0.26	0.09	0.39	0	0.76	0.26
Avg. Collection Period	19	131.68	100.04	116.91	26	113.55	84.79	103.94	18	0.59	15	0.9	0.16

**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	81%	1	0.4	36	75%	1	0.44	6%	0.59			0.14
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	7.6	5	7.23	-0.32	0.84	0	0.84	0.05
Mozambican	31	55%	1	0.51	36	78%	1	0.42	-23%	0.05			0.49
CEO	31	23%	0	0.43	36	14%	0	0.35	9%	0.36			0.23
CFO	31	29%	0	0.46	36	31%	0	0.47	-2%	0.89			0.04
CXO	31	0%	0	0	36	3%	0	0.17	-3%	0.36			0.25
Higher than Masters	30	57%	1	0.5	33	42%	0	0.5	15%	0.27			0.3
MBA	30	20%	0	0.41	33	21%	0	0.42	-1%	0.91			0.02
Acc. or Fin. Background	27	81%	1	0.4	33	73%	1	0.45	8%	0.43			0.19

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 managers reported in the application forms for treatment and control firms. The category 'Masters or higher' contains the 'MBA' category. 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. 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 3: Intention to change Financial Policies (Exit Survey)****Panel A: Cohort 1 (May 2017)**

<i>Intention to implement changes in corporate policies</i>							
	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)**

<i>Intention to implement changes in corporate policies</i>							
	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 4:** Changes in Financial Policies after 15 Months (15M Survey)

<i>After 15 months, have you implemented changes in corporate policies?</i>											
	<b>Treatment</b>					<b>Control</b>				<b>Difference</b>	
	<b>Yes</b>	<b>Yes (b/c of course)</b>	<b>No</b>	<b>#</b>	<b>%Yes</b>	<b>Yes</b>	<b>No</b>	<b>#</b>	<b>%Yes</b>	<b>Diff.</b>	<b>P-value</b>
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**

The table displays the implemented changes of corporate policies by managers 15 months after the first treatment (May 2017) and before the second treatment in 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 (subsidiary of a foreign firm). Depending on the specification, we disregard this answer in the aggregation or, being conservative, interpret it as a 'No'. 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.

**Table 5: Changes in Financial Policies (Financial Data 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	-0.170*	-0.216**	-0.216***	-0.216**	-0.198**	-0.320***	-0.406***	-0.406***	-0.409***	-0.386***
	[0.087]	[0.083]	[0.081]	[0.082]	[0.083]	[0.121]	[0.134]	[0.128]	[0.135]	[0.128]
Treatment	-0.172**					-0.165				
	[0.077]					[0.140]				
Post	0.137**	0.100*	0.100*			0.123**	0.086	0.086		
	[0.059]	[0.056]	[0.056]			[0.057]	[0.065]	[0.062]		
Constant	0.185***					0.218**				
	[0.052]					[0.087]				
Observations	409	409	409	409	409	466	466	466	466	466
R-squared	0.069	0.020	0.020	0.058	0.066	0.033	0.045	0.045	0.067	0.076
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 firm_id		68	68	68	68		71	71	71	71

**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	-0.216*** [0.069]	-0.185*** [0.067]	-0.185*** [0.066]	-0.183*** [0.067]	-0.179** [0.075]	-76.332*** [25.270]	-64.642** [24.683]	-64.642** [25.398]	-64.213** [24.675]	-62.596** [27.558]
Treatment	0.070 [0.079]					22.864 [26.893]				
Post	0.225*** [0.062]	0.214*** [0.063]	0.214*** [0.065]			82.626*** [22.547]	78.472*** [22.847]	78.472*** [24.256]		
Constant	0.246*** [0.043]					89.269*** [15.469]				
Observations	212	212	212	212	212	212	212	212	212	212
R-squared	0.052	0.132	0.132	0.181	0.181	0.058	0.153	0.153	0.197	0.198
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 firm_id		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	-0.352 [0.291]	-0.309 [0.275]	-0.309 [0.269]	-0.308 [0.277]	-0.306 [0.277]	-0.149* [0.077]	-0.159* [0.088]	-0.159 [0.103]	-0.160* [0.089]	-0.153* [0.087]
Treatment	0.391 [0.314]					0.040 [0.070]				
Post	0.124** [0.048]	0.101** [0.044]	0.101** [0.045]			0.093** [0.038]	0.091** [0.037]	0.091*** [0.033]		
Constant	0.167*** [0.028]					0.097*** [0.025]				
Observations	208	208	208	208	208	205	205	205	205	205
R-squared	0.041	0.017	0.017	0.022	0.022	0.009	0.013	0.013	0.058	0.059
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 firm_id		44	44	44	44		43	43	43	43

The table displays the difference-in-difference 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 2008-2018. \*, \*\*, \*\*\* Significance at 10, 5 and 1%, respectively.

**Table 6: Changes in Cash, Leverage and Capital Expenditures (Financial Data Regressions)**

**Panel A: Cash and Leverage**

	Cash					Leverage				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment x Post	0.032 [0.044]	0.031 [0.046]	0.031 [0.047]	0.033 [0.046]	0.057 [0.046]	-0.138 [0.125]	-0.055 [0.081]	-0.055 [0.090]	-0.055 [0.079]	0.029 [0.097]
Treatment	-0.107** [0.043]					-0.120* [0.071]				
Post	0.007 [0.034]	0.000 [0.034]	0.000 [0.038]			0.100 [0.119]	0.051 [0.078]	0.051 [0.082]		
Constant	0.215*** [0.036]					0.318*** [0.062]				
Observations	201	201	201	201	201	436	436	436	436	436
R-squared	0.064	0.005	0.005	0.123	0.167	0.027	0.002	0.002	0.047	0.117
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 firm_id		45	45	45	45		74	74	74	74

**Panel B: Capital Expenditures**

	Capital Expenditures				
	(1)	(2)	(3)	(4)	(5)
Treatment x Post	0.134*** [0.049]	0.131*** [0.049]	0.131*** [0.045]	0.126** [0.049]	0.144** [0.055]
Treatment	-0.059** [0.026]				
Post	0.052* [0.029]	0.061** [0.028]	0.061** [0.027]		
Constant	0.028 [0.022]				
Observations	164	164	164	164	164
R-squared	0.115	0.142	0.142	0.197	0.211
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 firm_id		44	44	44	44

The table displays the difference in difference 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 2008-2018.

\*, \*\*, \*\*\* Significance at 10, 5 and 1%, respectively.

**Table 7: Changes in Performance (Financial Data Regressions)**

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

	ROA				
	(1)	(2)	(3)	(4)	(5)
Treatment x Post	0.230** [0.108]	0.205** [0.100]	0.205** [0.099]	0.212** [0.100]	0.222** [0.102]
Treatment	-0.070 [0.089]				
Post	-0.198** [0.075]	-0.211*** [0.071]	-0.211*** [0.072]		
Constant	0.241*** [0.081]				
Observations	519	519	519	519	519
R-squared	0.011	0.018	0.018	0.112	0.113
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 firm_id		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	1.471*	1.551*	1.551*	1.563*	1.545*	0.076	0.148	0.148	0.165	0.200
	[0.835]	[0.832]	[0.817]	[0.845]	[0.820]	[0.120]	[0.125]	[0.128]	[0.125]	[0.126]
Treatment	-0.497					-0.061				
	[0.411]					[0.048]				
Post	-0.960**	-1.014**	-1.014***			0.058	-0.031	-0.031		
	[0.398]	[0.388]	[0.368]			[0.080]	[0.085]	[0.089]		
Constant	0.829**					0.129***				
	[0.389]					[0.034]				
Observations	504	504	504	504	504	530	530	530	530	530
R-squared	0.014	0.018	0.018	0.042	0.043	0.005	0.003	0.003	0.192	0.194
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 firm_id		76	76	76	76		78	78	78	78

The table displays the difference in difference 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 2008-2018. \*, \*\*, \*\*\* Significance at 10, 5 and 1%, respectively.



# The Impact of Financial Education of Managers on Medium and Large Enterprises – A Randomized Controlled Trial in Mozambique

## Appendix

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## A Appendix

### A.1 Figures appendix

**Figure A1:** The course brochure

(a)





#### 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.

(b)

#### 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, reestruturação, M&A e financiamento que as empresas enfrentam.

## A.2 Tables appendix

**Table A1:** P-values using Randomization Inference

	<b>Sampling p-value</b>	<b>Randomization-t p-value</b>
Working Capital / Assets	1.10%	2.82%
Accounts Receivable	0.90%	0.43%
Avg. Collection Period	1.30%	1.53%
Accounts Payable	27.20%	25.00%
Inventories	7.90%	4.02%
Cash	48.20%	53.57%
Leverage	49.40%	47.48%
Capital Expenditures	1.40%	1.72%
ROA	3.80%	2.82%
ROIC	6.80%	2.92%
Sales Growth	19.10%	23.10%

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 1,000 iterations (Young, 2019).

**Table A2: Minimum Detectable Effect for Selected Outcomes**

	<b>Mean</b>	<b>SD</b>	<b>Size</b>	<b>% Treatment</b>	<b>% Compl.</b>	<b>Sign. Level</b>	<b>Power</b>	<b>MDE</b>
Return on Assets (ROA)	0.04	0.27	93	50%	91%	10%	80%	0.154
Cash / Assets	0.13	0.17	93	50%	91%	10%	80%	0.097
Leverage	0.22	0.32	93	50%	91%	10%	80%	0.183
Working Capital / Assets	0.05	0.35	93	50%	91%	10%	80%	0.199

The table shows the Minimum Detectable Effect (MDE) for selected outcomes computed as follows:

$$\text{MDE} = (t_{1-k} + t_{\alpha/2}) \times \sqrt{\frac{1}{P(1-P)}} \sqrt{\frac{\sigma^2}{N} \frac{1}{c-s}}$$

where  $k$  is the power of the test,  $\alpha$  is the significance level,  $N$  is the sample size,  $P$  is the proportion of companies allocated to treatment group,  $c$  is the compliance rate (share of companies assigned to treatment that actually received the treatment) and  $s$  is the share of companies assigned to control that actually received the treatment. The mean and standard deviation (first two columns) are computed on the KPMG data as of 2016. In the computation, we use our sample size as well as the realized compliance rate. We use a power of 80% as suggested in Bloom (1995) and Duflo, Glennerster, and Kremer (2007). We assume  $s$  to be equal to zero.

**Table A3: 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	-0.177** [0.083]	-0.215*** [0.079]	-0.201** [0.078]	-74.507*** [24.895]	-63.648** [24.390]	-62.233** [26.682]	0.236** [0.105]	0.212** [0.098]	0.231** [0.099]
Treatment	-0.165** [0.074]			15.282 [25.655]			-0.081 [0.088]		
Post	0.137** [0.059]	0.100* [0.056]		82.626*** [22.528]	78.472*** [22.831]		-0.198** [0.075]	-0.211*** [0.071]	
Constant	0.185*** [0.052]			89.269*** [15.456]			0.241*** [0.081]		
Observations	427	427	427	222	222	222	538	538	538
R-squared	0.068	0.020	0.068	0.056	0.152	0.200	0.012	0.018	0.116
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 firm_id		70	70		47	47		79	79

The table displays the difference in difference 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 2008-2018. \*, \*\*, \*\*\* Significance at 10, 5 and 1%, respectively.

**Table A4: External Data (KPMG) only**

	Working Capital			ROA		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment x Post	-0.275*** [0.094]	-0.224*** [0.081]	-0.214*** [0.076]	0.416 [0.253]	0.502* [0.289]	0.552* [0.320]
Treatment	-0.193** [0.081]			-0.288 [0.254]		
Post	0.137* [0.077]	0.083 [0.069]		-0.377 [0.227]	-0.417 [0.267]	
Constant	0.193*** [0.059]			0.452* [0.246]		
Observations	321	321	321	315	315	315
R-squared	0.125	0.031	0.111	0.013	0.008	0.063
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 firm_id		49	49		48	48

The table displays the difference in the difference 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 2008-2018. \*, \*\*, \*\*\*: Significance at 10, 5 and 1%, respectively.

**Table A5: Differential Effects to Firm Characteristics in the Post-treatment Period**

**Panel A: Working Capital**

	<b>Working Capital</b>				
	<b>(1)</b>	<b>(2)</b>	<b>(3)</b>	<b>(4)</b>	<b>(5)</b>
Treatment x Post	-0.209** [0.081]	-0.218** [0.088]	-0.228*** [0.081]	-0.226** [0.087]	-0.237** [0.101]
Observations	409	409	400	400	320
R-squared	0.068	0.067	0.062	0.025	0.07
Number of firm_id	68	68	64	64	55
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Control x Post	In size	cubic size	In size 2016	cubic size 2016	Mozambican

**Panel B: Average Collection Period**

	<b>Average Collection Period</b>				
	<b>(1)</b>	<b>(2)</b>	<b>(3)</b>	<b>(4)</b>	<b>(5)</b>
Treatment x Post	-64.321** [30.121]	-65.368** [24.831]	-65.896** [26.156]	-67.132** [25.002]	-61.934** [25.276]
Observations	212	212	212	212	198
R-squared	0.199	0.238	0.2	0.187	0.187
Number of firm_id	45	45	45	45	42
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Control x Post	In size	cubic size	In size 2016	cubic size 2016	Mozambican
Clustered s.e.	Yes	Yes	Yes	Yes	Yes

**Panel C: ROA**

	<b>ROA</b>				
	<b>(1)</b>	<b>(2)</b>	<b>(3)</b>	<b>(4)</b>	<b>(5)</b>
Treatment x Post	0.225** [0.101]	0.252** [0.106]	0.217** [0.100]	0.235** [0.106]	0.285*** [0.105]
Observations	519	519	506	506	402
R-squared	0.113	0.118	0.114	0.021	0.155
Number of firm_id	76	76	71	71	59
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Control x Post	In size	cubic size	In size 2016	cubic size 2016	Mozambican
Clustered s.e.	Yes	Yes	Yes	Yes	Yes

The table displays the difference in the difference 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 A6: Focus on the Largest Segment**

	Working Capital			Avg. Collection Period			ROA		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment x Post	-0.203*	-0.245**	-0.219**	-82.543***	-68.714**	-68.022**	0.235**	0.214**	0.229**
	[0.106]	[0.102]	[0.104]	[27.141]	[25.994]	[30.253]	[0.116]	[0.104]	[0.111]
Treatment	-0.186**			18.014			-0.014		
	[0.092]			[26.983]			[0.081]		
Post	0.159**	0.114*		76.335***	71.710***		-0.165***	-0.175***	
	[0.070]	[0.066]		[24.029]	[24.336]		[0.058]	[0.054]	
Constant	0.205***			89.805***			0.199***		
	[0.062]			[16.639]			[0.063]		
Observations	312	312	312	183	183	183	387	387	387
R-squared	0.074	0.022	0.074	0.057	0.144	0.166	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 firm_id		51	51		39	39		55	55

The table displays the difference in the difference 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 2008-2018. \*, \*\*, \*\*\* Significance at 10, 5 and 1%, respectively.

**Table A7: Different Time Periods**

	Working Capital			Avg. Collection Period			ROA		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment x Post	-0.173* [0.090]	-0.185** [0.082]	-0.213** [0.085]	-62.596** [27.558]	-70.517*** [25.427]	-62.199*** [22.017]	0.228*** [0.084]	0.133* [0.077]	0.131** [0.061]
Observations	277	169	112	212	132	88	321	198	133
R-squared	0.051	0.093	0.161	0.198	0.220	0.298	0.151	0.035	0.076
Number of firm_id	66	63	59	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	≥2013	≥2015	≥2016	≥2013	≥2015	≥2016	≥2013	≥2015	≥2016

The table displays the difference in the difference 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 2018. \*, \*\*, \*\*\*: Significance at 10, 5 and 1%, respectively.

**Table A8: Excluding CEO turnovers**

	Working Capital			Avg. Collection Period			ROA		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment x Post	-0.121 [0.097]	-0.176* [0.092]	-0.163* [0.092]	-82.805*** [25.736]	-69.595*** [24.922]	-67.495** [27.640]	0.252** [0.112]	0.222** [0.100]	0.220** [0.098]
Treatment	-0.135 [0.094]			30.626 [30.004]			-0.047 [0.078]		
Post	0.135** [0.062]	0.095 [0.058]		82.626*** [22.577]	78.472*** [22.874]		-0.161** [0.071]	-0.175*** [0.063]	
Constant	0.185*** [0.056]			89.269*** [15.490]			0.192*** [0.072]		
Observations	346	346	346	197	197	197	458	458	458
R-squared	0.041	0.013	0.057	0.061	0.152	0.204	0.011	0.019	0.104
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 firm_id		58	58		42	42		67	67

The table displays the difference in the difference 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 2008-2018. \*, \*\*, \*\*\*: Significance at 10, 5 and 1%, respectively.

**Table A9:** Excluding Financial Companies

	Working Capital			Avg. Collection Period			ROA		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment x Post	-0.161*	-0.219**	-0.204**	-76.430***	-71.285**	-65.590**	0.239**	0.217*	0.231**
	[0.094]	[0.091]	[0.090]	[26.990]	[26.987]	[29.475]	[0.116]	[0.109]	[0.110]
Treatment	-0.159**			16.831			-0.065		
	[0.076]			[27.847]			[0.096]		
Post	0.133**	0.099		88.863***	85.116***		-0.205**	-0.222***	
	[0.063]	[0.061]		[25.245]	[25.314]		[0.081]	[0.077]	
Constant	0.153***			89.163***			0.249***		
	[0.046]			[16.464]			[0.087]		
Observations	372	372	372	195	195	195	481	481	481
R-squared	0.062	0.019	0.073	0.063	0.165	0.221	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 firm_id		63	63		41	41		71	71

The table displays the difference in difference 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 2008-2018. \*, \*\*, \*\*\* Significance at 10, 5 and 1%, respectively.

**Table A10: Outcomes scaled by Contemporaneous Total Assets**

	Working Capital			ROA		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment x Post	-0.070 [0.065]	-0.106* [0.063]	-0.110* [0.057]	0.198** [0.093]	0.194** [0.090]	0.198** [0.090]
Treatment	-0.181*** [0.068]			-0.061 [0.073]		
Post	0.052 [0.044]	0.024 [0.043]		-0.167*** [0.061]	-0.191*** [0.060]	
Constant	0.191*** [0.045]			0.200*** [0.065]		
Observations	466	466	466	607	607	607
R-squared	0.082	0.008	0.031	0.010	0.016	0.090
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 firm_id		71	71		81	81

The table displays the difference in difference 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 2008-2018. \*, \*\*, \*\*\* Significance at 10, 5 and 1%, respectively.

**Table A11: 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	173	80.32	5.39	258.29	13.22	0.72	1.22	0.65
Sales (m USD)	78	44.95	6.24	113.13	172	28.92	3.74	84.87	16.03	0.22	2.50	0.22
Sales Growth	74	-0.23	-0.30	0.58	135	-0.14	-0.25	0.61	-0.09	0.28	-0.05	0.34
# Employees	72	307.81	99.5	605.69	172	440.37	89.5	1,327.05	-133.00	0.42	10.00	0.67
Leverage	59	0.22	0.11	0.30	153	0.21	0.04	0.32	0.01	0.95	0.07	0.36
Capex / Assets	61	0.04	0.01	0.26	66	0.73	0.02	5.38	-0.69	0.32	-0.01	0.18
Return on Assets (ROA)	76	0.08	0.06	0.24	173	0.03	0.04	0.28	0.05	0.21	0.02	0.65
Return on Inv. Cap. (ROIC)	75	0.39	0.15	1.91	170	0.45	0.19	1.92	-0.06	0.83	-0.04	0.61
Working Capital/ (Lag) Assets	64	0.10	0.12	0.35	73	0.02	0.01	0.36	0.08	0.18	0.11	0.11
Working Capital / Sales	64	0.12	0.09	0.71	73	-0.07	0.01	0.88	0.19	0.19	0.08	0.20

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

	Our Sample		LinkedIn						Differences					
	Obs.	Mean	(Full Sample)		(Employees≥25)		(Followers≥100)		(Full Sample)		(Employees≥25)		(Followers≥100)	
			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. \*, \*\*, \*\*\* Significance at 10, 5 and 1%, respectively.

**Table A12:** The Schedule of the Course

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

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



**Table A13: 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 of 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.