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Real Effects of Internal Information Allocation: Evidence from a Field Experiment

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The study examines the effects of changes in the allocation and precision of internal cost information on operational performance. We conduct a randomized field experiment at a medium-sized service company. In the experiment, unit managers receive previously unavailable information about the time resources consumed by individual activities. The information treatment increases both the volume of decision-useful information and the complexity of this information, i.e., the costs of information processing. In the short run, we document an overall decrease in efficiency over the experimental period. Consistent with the notion that short-term adjustment costs outweigh the benefits from improved decision-making, cross-sectional analyses suggest that this negative performance effect is entirely attributable to activities with high information asymmetries and units with low-ability team leaders. Over a longer time horizon, non-experimental evidence shows a catch-up effect across all units and activities. In particular, we observe learning effects for low-ability team leaders, which suggests that cost accounting experience can act as a substitute for ability. Overall, our results suggest that an increase in the volume and precision of internal cost information can generate long-term operational improvement but comes at the cost of short-term frictions from the processing of more complex information.

Keywords: Intrafirm Information Allocation, Asymmetric Information, Real Effects, Costing Systems, Operating Performance, Activity-based Costing, Time-driven Activity-based Costing

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1. Introduction

A company's management accounting system is the primary source of decision-relevant information for managers. These systems vary in the volume and precision of cost accounting information, among other factors (e.g., Labro, 2019). Prior literature supports the view that managerial decisions generally benefit from improvements in a firm's cost accounting (Kaplan and Anderson, 2007; Cooper and Kaplan, 1988, 1991; Williams and Seaman, 2002; Banker, Bardhan and Chen, 2008; Campanale et al., 2014; Narayanan and Sarkar, 2002). However, the design of a costing system is endogenous with respect to its expected costs and benefits. Perhaps for this reason, evidence on the aggregate effects of costing systems on operational performance at the firm level is mixed (e.g., Ittner, Lanen and Larcker, 2002; Ittner, Larcker and Randall, 2003; Gosselin, 2007) and points to the role of firm and team characteristics (e.g., Anderson and Young, 1999, Anderson et al. 2002; Shields, 1995; Griffith and Neely, 2009; Hoozée and Bruggeman, 2010). Important downsides of greater intrafirm transparency arise from limitations in managers' capacity to acquire and process information, especially when the expansion of available information also increases the complexity of this information (Conlisk, 1980; Sims, 2003; Kacperczyk and Seru, 2007).

Publicly available data on the design and output of firms' costing systems are scarce (Labro, 2019; Lourenço, 2019). Consequently, most prior evidence is cross-sectional and comes from observational studies. Such data limitations make it difficult for researchers to establish the direction of causality. For example, changes in costing systems tend to be correlated with many corporate choices and manager characteristics that also determine firm performance, giving rise to potential selection bias in the estimation of the adoption effect. In contrast to previous studies, we investigate the effects of improved and more precise cost information on indicators of operational performance in a field experiment that is suited to minimize concerns about omitted variables and maximize internal validity.

We collaborate with a medium-sized service provider in the renewable energy sector and use a comprehensive costing system update as the setting for the field experiment. The company faces financial pressure, and its management aims to decrease overhead costs, which make up a substantial fraction of total costs (approx. 35% of total costs are unallocated labor costs). Hence, the costing system update is expected to uncover potential inefficiencies in the use of these personnel resources. The update can best be described as the introduction of a time-driven activity-based costing (ABC) system, which tracks the use of labor for individual activities and assigns these costs to distinct customer services. To reduce selection effects, we employ a random mechanism to implement a staggered adoption of the new costing system for different processes. We are able to compare changes in the costs, quality and efficiency of treated processes with those of identical untreated processes over more than one quarter (three and a half months) while controlling for unobservable time trends and time-invariant characteristics of the different teams and processes. We continue to document these operational outcomes for a post-experimental period of approximately one year.

The service provider is organized into two internal service units that are defined by the source of renewable energy and operate on a largely separate basis. While management of these two units is distinct, the type of activities that the units perform overlap. We exploit this organizational setup and randomly assign the availability of cost information for each activity to one of the two service units. During the experimental period, the new costing system provides managers in each service unit with precise information about the time consumption of the randomly selected activities as well as the corresponding cost of the capacity used. Managers do not receive this information about the other activities that continue to be treated under the former traditional two-stage costing system (with no cost information being available at the activity level at all). The activities relate to processes that differ in their internal priority status. Each service unit has different teams that are led by individual managers (team leaders). These team leaders receive additional cost information without any guidance on how to use the information to achieve the intended efficiency gains. These decisions are left to the team leaders' own judgment and interpretation of the data. We can rely on internal evaluation of the team leaders' ability to grasp differences in the reaction to the expanded set of cost information across different teams.

Our first set of analyses exploits the experimental variation in the availability of the cost information. Over the experimental period in the short run, we investigate changes in the process time, quality, and efficiency after the random assignment of process-level cost information to individual managers. On average, we find an increase in the required process time and a decrease in efficiency. However, this effect varies significantly and predictably across the ability of team leaders and two proxies for the magnitude of information asymmetries: the relative importance of the processes and the deviation of the new information from prior cost estimates. Specifically, we find that the negative operational effect is attributable to low-ability team leaders, low-priority processes and processes with higher-than-expected costs. In contrast, we observe slightly positive quality effects for high-ability team leaders.

These results show that new cost information does not uniformly translate into short-term operational improvements. Rather, they suggest that additional information can even lead to worse short-term outcomes in settings with high information asymmetries and low-ability managers. This finding is consistent with the incurrence of higher initial adjustment costs from the processing of the new information in those settings. Accordingly, the negative effect is mitigated if managers have a high ability to process new information more efficiently. Overall, our results are in line with the notion that more complex information processing leads to higher initial adjustment costs that potentially exceed the immediate benefits of more precise cost information.

Our second set of analyses investigates changes in process time, quality, and efficiency during the post-experimental period, after the company fully adopted the time-driven ABC system for all processes and teams. In contrast to our first set of analyses, we can only compare our treated processes with benchmarks that we do not select randomly, rendering the evidence from this analysis descriptive. In the long run, the provision of more precise cost information is associated with an average decrease in process time and an increase in overall efficiency, i.e., overall positive operational outcomes. These results, while descriptive, are consistent with a catch-up effect, with operational improvements realized over an extended period. Hence, our results support previous findings from analyses of balanced scorecard adoption (Ittner, Larcker and Randall, 2003) or ABC adoption (Kennedy and Affleck-Graves, 2001), where long-term benefits also exceed short-term effects.

Similar to the experimental period, we find meaningful cross-sectional differences in the outcomes. The immediate quality improvement achieved by high-ability team leaders in the short term is followed by long-term time and efficiency gains. While we observe an increase in process time and a deterioration of efficiency for low-ability team leaders in the short term, those effects tend to reverse over the long term. Interpreting the results of our long-term analyses at face value, the evidence suggests that the adoption of a more precise costing system leads to positive effects that persist in the long run once managers are able to process the more complex information adequately. Overall, these results are consistent with the notion that more precise internal information generates long-term operational improvements.

In the final set of analyses, we investigate whether previous experience with the type of newly adopted costing system (in our case, the time-driven ABC system) is beneficial for managers receiving new cost information. We do so by comparing short-term operational effects over time within the unit of the same manager. Specifically, we compare operational changes around the first-time adoption of the system with changes around later adoption of the same system for other processes within this unit. Our results underscore that experience is particularly beneficial for low-ability team leaders, while it does not further improve the outcomes in the units of high-ability team leaders. Thus, differences in the ability of team leaders matter most around the very initial adoption, when all managers lack experience with the new costing system. Put differently, managerial ability becomes less important once managers gain experience in the handling of cost information.

Our paper makes two contributions. First, we add evidence to the literature on the relation between costing systems and firm performance (Labro, 2019) by providing evidence from an experimental setting that is suited to mitigate internal validity problems inherent to observational studies. This evidence extends the scarce research on causal effects in managerial accounting settings (Lourenço, 2019). In particular, we highlight the role of within-firm heterogeneity in manager quality and process priorities during the implementation of a new costing system. These findings help triangulate empirical evidence related to the implementation of improved and more precise cost information and, especially, a time-driven ABC system (Ittner, Lanen, Larcker, 2002; Shields, 1995; Balakrishnan et al., 2018; Hoozée and Bruggeman, 2010). Given the advances in data technology, these findings provide relevant insights into potential frictions that arise when firms start integrating large volumes of additional data into their costing systems.

Second, our paper extends prior literature on the role of managerial experience and corresponding learning (for an overview, see Argote 2012). In the context of accounting systems, evidence shows that experience is associated with the way managers respond to new incentives (Griffith and Neely 2009) and make accounting choices (Ahmed et al. 2019). We add to these findings by documenting the role of experience in the adoption and updating of costing systems and its interaction with the abilities of individual managers. In particular, our results suggest that experience can act as a substitute for managerial ability in the processing of new cost information and the improvement of subsequent decision-making.

2. Related Literature and Empirical Predictions

Cost measurement serves multiple purposes in companies. In particular, cost information helps evaluate management choices ex post through its role in performance measurement, and it facilitates decision-making ex ante (Demski and Feltham, 1976; Baiman and Demski, 1980). In our setting, an updated costing system provides managers with new and more precise information as an additional input into their decision-making process. Prior to the implementation of this system, managers did not have access to precise information about the time or cost resources consumed by individual activities. While the enriched information set can improve managers' decision-making, prior research points to heterogeneous adoption effects of new costing systems, often due to frictions in the adjustment of decision-making (see Labro, 2019). In particular, a net operational improvement will become observable only when the benefits of new cost information start to outweigh the initial adjustment costs. These costs depend on the individual attributes of managers, such as their ability or experience to process large amounts of new information (e.g., Griffith and Neely, 2009; Casas-Arçe et al., 2017). Initial adjustment costs also depend on firm-specific factors relating to the underlying cost objects and to the novelty and complexity of the cost data. The magnitude and the timing of operational benefits arising from enhanced cost measurement are therefore ambiguous and likely to vary across different units of a firm.

2.1. Costing System Adoption and Operational Performance

The empirical literature on costing system adoption tends to focus on the adoption of ABC systems. Several studies point to positive operational outcomes of ABC adoption, such as time and quality improvements that indirectly manifest in cost reductions (Banker, Bardhan, and Chen, 2008; Ittner, Lanen and Larcker, 2002; McGowan, 1998; Narayanan and Sarkar, 2002). However, the evidence is mixed regarding whether these perceived operational benefits translate into financial success. The literature provides evidence for such an association (e.g., Cagwin and Bouwman, 2002; Kennedy and Affleck-Graves, 2001; or Eldenburg et al., 2010, for a healthcare setting). Other studies fail to detect a direct association (Shields 1995; Ittner, Lanen and Larcker, 2002). Irrespective of the overall effect, there is substantial cross-sectional variation in the firm-level outcomes of ABC adoption due to, for example, different levels of employee resistance, implementation procedures and quality checks (Malmi, 1997; Major and Hopper, 2005).

The mixed evidence on the adoption effects of new costing systems is also consistent with findings on firm choices of management accounting systems in general. For example, the performance impact of a balanced scorecard implementation can be either positive or negative (De Geuser, Mooraj and Oyon 2009; Davis and Albright, 2004; Ittner, Larcker and Randall, 2003). Growing companies tend to benefit from the early adoption of professional accounting systems, but only if the system design fits the company's business model and strategy (Davila and Foster, 2005; Sandino, 2007). In a more recent longitudinal study, Labro and Stice-Lawrence (2019) find evidence of lower operating expenses and higher revenues after hospitals update their accounting systems in response to economic pressure.

In our setting of a costing system update, the treatment represents a change in the firm's internal information allocation. While rank-and-file employees generally possess private information about their operational areas (Parker and Kyi, 2006; Merchant, 1981), the features of the new costing system equip team leaders with new and much more precise information about the process times and performance of their team members. Pre-experimental survey evidence suggests that information asymmetry is substantial, with team leaders significantly misjudging the length of these process times.[1] The treatment thus reduces intrafirm information asymmetries between team leaders and their team members.

At the same time, the novelty and volume of cost data that become available to the team leaders makes the processing of the information complex. Prior evidence suggests that a new costing system's operational success largely depends on its constructive employment (Eldenburg et al., 2010; Narayanan and Sarkar, 2002). In our setting, given the financial situation of the company, team leaders felt some pressure from top management to use the data for more efficient cost management. However, the decisions about how to improve operations were largely left to their own discretion. Anecdotal observations suggest that these managers do not neglect the new information and rather seek cost management opportunities offered by the use of the new information. For example, managers who were surprised about actual process times were actively involved in the activity design and created templates to speed up simple tasks, such as the writing of an email, and thus reduce the use of human labor capacity. The change in process flows in response to the new information offers additional paths to reduce inefficiencies.

Generalizing our anecdotal evidence, we expect that a reduction in information asymmetries eventually leads to operational improvements, with the exact timing of the realization of these

[1] According to our data, team leaders underestimate the actual process time by 24% on average (Median 33%).

improvements remaining unclear. The timing depends on the required process adjustments and the volume and complexity of additional information. These adjustment costs negatively impact operational performance and, at least in the short run, can overcompensate for the benefits of operational improvements. The magnitude of these costs is most likely to vary in the cross-section and can be more pronounced in situations with larger information asymmetries and for specific managerial attributes.

2.2. Cross-sectional Variation in the Adoption Effects

Ability and Experience of Team Leaders

Individual managers matter for firm-level outcomes (Bertrand and Schoar, 2003; Bennedsen, Pérez-González, and Wolfenzon, 2020). Characteristics such as the tenure, experience and ability of managers play a vital role. Given the potential task complexity in processing and interpreting new information in our setting, it is highly plausible that the realization of operational improvements also hinges on the skills of the team leaders in charge of their implementation. This idea is line with previous research showing that higher-quality managers are better at reducing information asymmetry with external parties (Bamber, Jiang, and Wang, 2010) and acquiring precise and relevant private information (Kacperczyk and Seru, 2007).

The predicted direction of the effect is ambiguous. On the one hand, low-ability managers will have been exposed to greater information asymmetry in the first place as a result of their less efficient information acquisition. Our information allocation treatment, with standardized information about task performance provided directly to team leaders through the new costing system, makes the information more accessible and thus likely leads to a larger reduction in information asymmetry. These low-ability team leaders will thus benefit most when potential improvements in cost efficiency become more easily observable, which can lead to more substantial operational adjustments and, ultimately, greater operational improvements. Under this view, we would expect to observe stronger operational improvements for low-ability managers in the long term (i.e., after the adjustment of the underlying processes is completed). Put differently, when low-ability managers become more familiar with the new format of cost information over time, their experience from this learning process can substitute for ability and lead to better decision-making (Casas-Arce et al. 2017).

At the same time, low-ability managers are more likely to already be overstrained by their day-to-day routine, and thus, the volume and complexity of additional information will initially be a severe disruption of their workflow. These low-ability managers will face greater difficulties in interpreting the new information, translating the information into process design adjustments and, overall, incur greater adjustment costs. If these adjustment costs outweighed the greater assumed benefits, we would observe a lower adoption effect for these teams, with the magnitude of the difference being even more pronounced in the short term, when adjustment costs tend to be substantial. In addition, if high-ability managers have sufficient time to utilize additional information, they are better able to adequately respond and increase their team performance immediately (Griffith and Neely, 2009).

Accuracy of Prior Cost Estimates

Our treatment makes time and cost estimates for individual tasks of employees available to managers. While the standardized information is entirely new, each manager had a prior belief about the time required for each task. In other settings, such as traded securities, an announcement is said to be informative if it changes prior beliefs about the value of an asset (Beaver 1981). Market participants react to information dissemination with trading due to differences in agreement or changes in their knowledge (Holthausen and Verrechia, 1990). In our setting, we have no trading (overlap) among participants for individual processes and therefore no relevant differences in agreement between managers. However, our information treatment changes the knowledge more strongly if prior beliefs are further away from the actual time requirements, i.e., if information asymmetry was higher ex ante.

Time estimates by employees are prone to measurement error (Cardinaels and Labro, 2008) and often inaccurate (Ittner, 1999). In our setting, managers do not perform those tasks themselves, which potentially leads to more severe errors in their time estimates. Overall, we expect managers to focus more strongly on processes for which they had underestimated the time and costs, as the new information signals a greater potential for perceived improvements in the consumption of labor capacity. At the same time, if managers had overestimated costs, they will be more likely to focus on quality improvements rather than further cost reductions.

Priority of Activities

Our company internally differentiates between high-priority processes and low-priority processes. Internal information allocation plausibly affects both groups of processes differently. Specifically, we assume that managers' ex ante focus on high-priority processes was higher, as errors and inefficiencies in those processes are more severe from a company perspective. Hence, information asymmetries for low-priority processes should be larger before our treatment because of lower ex ante incentives to monitor, which renders our information treatment more informative for these processes. If this effect prevailed, we would observe sharper effects for low-priority processes.

However, information processing requires managerial capacity. Managers will keep their focus on high-priority processes after our treatment and first invest their time in the absorption of information about these processes. If managers' information absorption capacity is constrained, such a focus on the operational improvement of high-priority processes contemporaneously leads managers to pay less attention to low-priority processes, potentially hurting the time consumption and quality of these processes.

2.3. Short-Term vs. Long-Term Effects

Operational improvements will not initially be observable upon the release of more information if adjustment costs are severe. If these costs are material and exceed the initial benefits, short-term operational effects will even be negative. Even if managers are able to adjust processes quickly, some operational changes depend on employees' learning of the new process flow and thus will not immediately result in improvements.

Prior literature is generally in line with this argument. For example, Kennedy and Affleck-Graves (2001) find that the magnitude of the positive abnormal return is highest in the last 12 months of their sample period, indicating a delay in the realization of benefits after the implementation of an ABC system. Their results point to complexity and resource consumption counteracting potential operational improvements during ABC adoption. Similarly, Ittner, Larcker and Randall (2003) investigate the effects of balanced scorecard adoption on a sample of US financial services companies. They find evidence for increased satisfaction but also for a decrease in ROA associated with the adoption of a balanced scorecard. However, the negative financial effect reverses for companies that had employed the balanced scorecard for more than 2 years.

3. The Experimental Setting

3.1. The Company's Organization and Costing System

We partnered with a medium-sized service company from the renewable energy sector to develop a field experiment aimed at testing the effects of internal information allocation. The company has a track record of more than twenty years and is headquartered in Germany. Its annual revenues lie in the range of 350-450 million euros, and its staff amounts to approximately 1,000 employees (as of June 2019). The company has two major fields of activities organized in separate divisions: (1) project development for solar and onshore-wind energy plants and (2) operational and commercial management as well as the provision of maintenance services for renewable energy power plants. The field experiment is conducted in the latter division, which employs approximately 15% of the total staff. The division offers services for wind and solar power plants with more than 1 GW of installed capacity in Germany. The division is organized into two separate service units, defined by the type of renewable energy (wind and solar), as displayed in Figure 1. The structure of these two service units is largely parallel. A department head chairs each service unit. Multiple team leaders supervise teams of rank-and-file employees who are involved in activities related to client services. The activities of these two service units overlap to some extent; i.e., one team from each service unit performs identical activities (with customers being providers of solar energy in one unit and wind energy in the other unit). This feature of the organizational design offers natural pairs of treatment and control groups for our experimental design.

The division has identified five major business processes that represent the most common client services (e.g., an on-site maintenance service or an off-site service to resolve a technical incident). When a client orders a specific service, the accounting system institutes a ticket that serves as the division's primary cost object. Prior to the experimental costing system update, a traditional job costing system did not assign personnel costs to these tickets because team members were typically involved in different processes for different clients. Thus, a large portion of the divisional overhead remained unallocated, especially personnel costs, which amounted to approximately 35% of total costs. The unallocated overhead led the company's management to be concerned about the pricing of client services and inefficiencies in cost management. These prices were set by the divisional units without knowledge about how the services contributed to the division's overall resource consumption.

3.2. The Adoption of the New Costing System

When implementing the costing system update, the design of process maps, detailed work shadowing and interviews with team leaders and staff resulted in the identification of 38 different activities that contributed to the five types of services. The eleven teams in the division's two service units are actively involved in 22 of these activities (with the other activities not requiring any human labor or being administered by a central unit). Examples include the checking of a customer offer, the hiring of a repair mechanic, or the writing of an email. For all 22 of these activities, human labor is the major input factor and thus the binding resource constraint. Therefore, the company revised the costing system such that time estimates for the required labor became the allocation base for the assignment of indirect costs to the service tickets, where the required labor is defined by the activities related to a particular ticket. The allocation rate is based on the cost per time unit of labor.[2] The relatively low implementation and maintenance costs for the required set of internal information support this choice (Kaplan and Anderson, 2007).

In our view, management's demand for more detailed information arose from economic pressures on the product market. In particular, the company's inability to precisely measure the costs of client services became a competitive disadvantage. An internal team that reported directly to the division management developed the new costing system in coordination with us. Members of this team were not involved in the performance of any activities or the provision of any client services for which the new system changed the cost estimates, which mitigates the risk of correlated

[2] Since the company's newly developed costing system mainly derives its input for the cost allocation from time estimates for individual processes, the system is similar in spirit to a time-driven ABC system. Throughout the paper, we still label the system ABC.

unobserved factors determining the treatment design. The author team was also involved in the development of time and cost estimates for each activity that provided the main input into the new ABC system.[3] The company runs an IT system that is able to directly track the activity of employees with great accuracy (to the second). To reduce potential behavioral biases in employees' task performance, the time monitoring ran in the background of the system's interfaces with employees; i.e., the recording was not directly present.

The experimental manipulation during the adoption of the new costing system is the provision of activity-based cost information to the team leaders in the two service units. We take advantage of the fact that eight exactly identical activities are performed in each service unit; i.e., two teams perform exactly the same tasks while being managed and supervised in entirely separate units by different team leaders. We employ a random generator to assign these teams to the control and treatment groups. Figure 2 displays our treatment structure across both service units, and Figure 3 provides a graphical representation of a stylized experiment with only 2 teams and 2 processes.

The company initially adopted the new costing system on May 1, 2018. From that day on, for the first time, all team leaders received information about time and cost estimates for the activities which had been randomly assigned, while information about the activities in the control group was not disclosed. These time and cost estimates were delivered by email. Figure 4 shows a stylized version of such a cost report. Neither the team leaders nor the department heads received any further instructions or guidance on the adoption day. Later, the company's management sent an email to all department heads and team leaders on May 26, 2018, advising them to employ the

[3] Time estimates and resulting cost calculations were developed by a professional research assistant based on work shadowing and actual on-site observations of the workflow and the average employee's performance. The technical system was developed in cooperation with the company's accounting department. The research assistant's market-based salary was paid by the company. There was no further exchange of payments between the researchers involved in the study and the company.

newly available cost information for internal improvements. The experimental period ended on August 15, 2018, when top management released the missing time and cost estimates. Therefore, our experimental period over which we estimate the adoption effects of the new costing system spans 3.5 months (see Figure 3 for an illustration). After the end of the experimental period, we were able to continue tracking the data for an additional period until June 27, 2019. We use this extended time period to collect non-experimental evidence on long-run effects.

Figure 3 also graphically displays the timeline of our experiment. Our sample period spans from January 19, 2018, to June 27, 2019. The pre-experimental period spans from the beginning of our sample until the first adoption (Treatment 1) of the updated costing system on May 1, 2018, and consists of time blocks 1 to 3. The date of first adoption is also the date at which each process receives the random treatment. Thus, our identifiable treatment period (experimental period) extends over time blocks 4 to 6. At the second adoption date (August 15, 2018), the new cost information (and the corresponding time estimates) also become available for all randomly selected control processes. In our short-term analyses, we use time blocks 7 to 9 as our post-experimental period. Exact dates as well as our time blocks are listed in Table 1.

Overall, then, the setting provides us with a valuable opportunity to test our empirical predictions. First, the randomization feature in the adoption of the new costing system enables us to identify causal short-term relationships and thus establish relatively high internal validity. Second, the size of the company and the nature of the processes are comparable to those of SMEs in the European service sector. With 150 employees, the division of the company is comparable to the average medium-sized enterprise in Europe, which has between 50 and 250 employees. The SME sector is also economically important because SMEs account for more than half of the value added and approximately two-thirds of the total employment in the EU's nonfinancial business sector (European Commission, 2019). Therefore, our results have external validity and may generalize to other relevant settings. Third, the risk of employees being distracted by the treatment is minimized through the presence of an advanced IT system that automatically collects process time and process outcomes in the background. This system was already in place long before the company started to use the data for collecting cost information.

4. Short-term Analysis

4.1. Outcome Measures

We rely on the standard output that is provided by the company's IT-based reporting system to measure operational performance. These measures thus represent key performance indicators that are also available and in use for the internal evaluation; i.e., they correspond well with managerial incentives, especially at the team leader level. We use three different measures to capture different dimensions of performance and combine these measures into one overall score.

Our first measure is the process time, which indicates the time it takes an employee to execute a specific task. This measure directly relates to the efficient use of human labor as the primary input factor into the division's provision of services. Since the allocation of indirect costs uses a time-based rate for labor costs, the measure also directly translates into the costs of each activity. We transform the raw data into the natural logarithm of the time measure (+1) to reduce the skewness of the underlying distribution. Keeping everything else constant, a reduction in the actual time represents a more efficient use of resources and thus an operational improvement.

Our second measure is the frequency of bad outcomes. This measure relates to the quality of task performance. The system records a major setback if an activity requires input from at least one additional unscheduled process. We rely on these records and employ the frequency of observed activities that end in a major setback as a measure for bad outcomes. Specifically, we use an indicator variable equal to one if a major setback occurred.

Our third outcome variable is the frequency of repeated processes. This measure captures quality assurance in task performance. The system logs a repeated activity if the task performed by an employee resulted in a minor mistake that can be adjusted by repeating the same task. The company views this variable as a signal of higher quality assurance of employees when they are directed to correct their mistakes. We employ an indicator variable equal to one if an activity requires such a repetition.

In addition, we combine the three outcome measures into one aggregate factor score. We derive the score from a principal component analysis of the three variables process time, frequency of bad outcomes and frequency of repetition (quality assurance). We follow Karolyi (2015) in constructing the score. Specifically, we use the first two eigenvalues, which capture more than 50% of the total variation. We interpret this factor score as a measure of operational efficiency, as we combine our proxies for costs and quality into one score. Higher values of our factor score indicate an increase in efficiency.

4.2. Cross-Sectional Splits

To evaluate heterogeneous treatment effects, we perform several cross-sectional splits based on three dimensions. We are able to distinguish between the relative importance of processes, the deviation of the new information from prior cost estimates and the ability of team leaders.

Our first sample split is based on the relative importance of individual processes. We differentiate between highly important processes (High-Priority) and standard processes (Low-Priority). Highly important processes are essential to the company and should be performed with accuracy, taking precedence over regular processes. Our classification relies on internal company records. Employees are aware of the classification scheme as well as the priority assigned to each process. Approximately 31% of all processes within our experimental period are classified as high-priority processes.

The second sample split relies on deviations between managerial expectations and actual costs. Specifically, we compare team leaders' ex ante time estimates and the actual time requirements, which represent actual process costs. For all processes, team leaders and department heads submitted individual time estimates before our initial information treatment. If managers on average underestimate (overestimate) the actual time required for a process, we classify this process as a low (high) time estimate. In our sample, managers underestimate the costs for 69% of total processes and overestimate the costs for 31%.

Our last sample split is based on the ability of team leaders. We differentiate between three groups, classified as high, medium and low ability. The team leader scores are provided by a central unit and rely on past internal evaluations. High-ability team leaders are associated with attributes such goal orientation or assertiveness. Low-ability team leaders are associated with attributes such as personal overload or general complacency. Each group includes an identical number of team leaders. In addition, there is a group of medium-ability team leaders. It turns out that this group of team leaders only provides central services to both service units, and each type of process is unique such that it is performed only at their team level. Hence, no counterfactual would be available for our analysis, and randomization would not be possible. Therefore, we exclude these medium-ability team leaders from our random treatment, and we provided them with all ABC information about their processes at the very first treatment date. Our pre-experimental survey data support this internal classification, as low-ability managers underestimate the actual process times by 26% on average, while high-ability managers underestimate the time by only 11% (both untabulated).

4.3. Regression Framework and Data

In our first analysis, we investigate short-term effects by comparing treated to identical untreated processes. We test our empirical predictions by estimating the following generalized difference-in-differences regression model:

$$\text{Outcome}_{i,j,t} = \beta_1 * \text{Treated}_{i,j} * \text{Post}_{t=1} + \beta_2 * \text{Treated}_{i,j} * \text{Post}_{t=2} + \text{Time FE} + \text{Team} * \text{Process FE} + \varepsilon \quad (1)$$

where i indicates the process, j indicates the team performing the process, and t indicates the time period of observation. $\text{Outcome}_{i,j,t}$ is measured in period t using (1) the natural logarithm of team j's required time (+1) for performing process i, (2) the frequency of observations in which process i ended in a major setback when performed by team j, (3) the frequency of observations in which process i performed by team j required a repetition afterwards and (4) a factor score based on all three previous outcome variables (see above, section 4.1). The variable $\textit{Treated}_{i,j}$ is equal to 1 if ABC information about process i was randomly assigned to be released to team j and 0 otherwise. The variable $\text{Post}_{t=1}$ is equal to 1 in the experimental period and 0 otherwise. The variable $\text{Post}_{t=2}$ is equal to 1 in the post-experimental period and 0 otherwise.[4] Our coefficient of interest is interaction term β_1 , which captures the treatment effect. We include time fixed effects (at the level of each time block) to control for time-specific shocks affecting all processes, such as potential seasonality of the business or different frequencies of public holidays. In addition, we include

[4] As both the treatment and control processes receive the treatment in this period, we only include and interact this additional time dummy for illustrative purposes.

interacted team-process fixed effects to control for time-invariant differences between our treatment and control processes on a granular level.[5]

In the next step, we further split our treatment dummy β_1 from regression 1 to evaluate heterogeneous treatment effects.

$$\text{Outcome}_{i,j,t} = \beta_1 * \text{Treated}_{i,j}\ \&\ \text{Low Split} * \text{Post}_t + \beta_2 * \text{Treated}_{i,j}\ \&\ \text{High Split} * \text{Post}_t + \beta_3 * \text{Priority}_{i,j} + \beta_4 \text{Priority}_{i,j} * \text{Treated}_{i,j} + \text{Time FE} + \text{Team} * \text{Process FE} + \varepsilon \qquad (2)$$

The *High Split* and *Low Split* indicators refer to (1) the priority[6] of process i, (2) the department heads' prior time estimates for process i and (3) the ability of team leaders[7] responsible for process i. We replace our *Treated* indicator from equation 1 with two separate (nonoverlapping) indicators: *Treated & Low Split* and *Treated & High Split.* This coding allows us to compare and interpret the total effects of our treatment across the groups more directly than a three-way interaction model with incremental effects (see Christensen, Hail and Leuz, 2013). We include the same fixed effects as in equation (1). The interacted Team * Process fixed effects absorb the base terms for the splits based on prior time estimates and team leader ability. The *Priority* indicator controls for the time-invariant difference between high-priority processes, and the interacted term with *Treated* controls for time-invariant differences between high-priority processes in the treatment and control groups. In all regression specifications of equations (1) and (2), we cluster our robust standard errors in two ways. We cluster them on a process level to correct for time-series

[5] Because of the inclusion of time and team-process fixed effects, separate dummies for pretreatment differences would drop out.

[6] The priority status of the same individual process i can vary within team j; i.e., it can be classified as having either high-priority or low-priority.

[7] For ability, we have a three-way sample split, with team leaders' ability being classified as high, medium, or low (according to the company's internal scheme; see above, section 4.2).

dependence as well as on a time-block level to correct for cross-sectional dependence (Gow et al., 2010).[8]

We summarize the descriptive statistics of our outcome variables and their pretreatment differences in Tables 2 and 3. Table 2, Panel A, includes only the 3,915 observations from randomly selected treatment and control processes (in the short-term analyses) and shows significant pretreatment differences in two of the four dependent variables (*Process Time* and *Overall Efficiency*). In Table 2, Panel B adds all observations from non-randomly chosen control processes, which we use only in the long-term analyses, to the sample. Here, we find significant pretreatment differences for all outcome variables, which suggests that the randomization is successful in reducing these systematic differences.

To provide further support for the effectiveness of our randomization procedure, Table 3 provides summary statistics on pretreatment differences between treatment and control groups at the level of identical individual processes. Two processes (1 and 4) with 941 observations do not show any pretreatment differences at all. Process 6 (with 35 observations) shows one statistically significant difference for *Bad Outcomes*. Three processes (2, 3, and 5) with 2,273 observations show two such differences. There is only one process (Process 7), with 554 observations (14.6% of the total sample), for which randomization did not result in any reduction of these differences. These statistics indicate reasonably successful randomization given our sample size.[9] The pretreatment differences in Tables 2 and 3 still underscore the importance of the *Process * Team*

8 Gow et al. (2010) show that two-way clustering produces unequivocally better inferences than other approaches, even in the case of few clusters. We have between 8 and 18 clusters on a process level and between 9 and 15 clusters on a time level, depending on the exact specification.

9 We control for pretreatment differences with granular fixed effects (Team * Process fixed effects). One of the 8 identical processes has too few control observations in the pretreatment period to be included in this table.

fixed effects, which control for different levels in the outcome variables both between the control and treatment groups and across the different teams.

4.4. Results: Short-term Analyses

Table 4 summarizes the main evidence from our experimental design. The results show the average short-term effect of the costing system update, i.e., the release of new and more granular cost information at the activity level. We find a statistically significant increase in the process time and a significant decrease in the overall efficiency after implementation in the experimental period. These changes are relative to the outcomes of identical processes being performed by other teams that did not receive the ABC information (or, more precisely, did receive the ABC information for other processes). The economic magnitude is substantial, as the treatment leads in the short term to an average increase of 68.88% in the process time. During the post-experimental period, in which the control processes are also treated, both differences become statistically insignificant. Overall, Table 4 provides evidence of negative operational effects in the short term upon the reallocation of internal cost information. This finding is consistent with the notion that short-term adjustment costs exceed efficiency gains.

Tables 5 and 6 present the evidence on the cross-sectional heterogeneity of the treatment effects. In Table 5, Columns 1-4, the *Treated* dummy interacts with the relative importance of treated processes. We find that the increase in process time and bad outcomes as well as the decrease in quality assurance in the experimental period are statistically stronger for low-priority processes (at the 10% level for *Process Time* and at the 5% level for *Bad Outcomes* and *Quality Assurance*). While the overall efficiency decreases significantly (at the 5% level) for low-priority processes in the experimental period (*Treated & Low Split*), the difference in the decrease in the treatment and control groups lacks statistical significance (p-value=0.1519). The result is consistent with the notion that initial adjustment costs are more pronounced for processes with low-priority, for which we expect larger initial information asymmetries.

Table 5, Columns 5-8, report the results for the sample partitioned based on deviations of the new information from prior cost estimates by managers. During the experimental period, the increase in process time and the higher frequency of bad outcomes are significantly stronger for underestimated processes than for overestimated processes. For the distinct group of underestimated processes (*Treated & Low Split*), we observe an increase in the required time and a decrease in efficiency during our experimental period. These effects are statistically insignificant in the post-experimental period. For the distinct group of overestimated processes, we find an increase in process quality (i.e., a negative coefficient estimate for *Bad Outcomes*), which endures in the post-experimental period. These findings indicate that the negative treatment effects are largely attributable to processes for which management underestimated the required time, which again points to a plausible role of information asymmetries in explaining the treatment effects.

Table 5, Columns 9-12, shows the results for the sample partitioned based on the ability of team leaders. Note that medium-ability team leaders, who did not participate in the experimental treatment, are included in this analysis. Therefore, the sample size increases to 19,970 observed processes.[10] We also provide a graphical representation of our sample split in Figure 5 to compare treatment differences between high- and low-ability team leaders over the experimental period and the post-experimental period. Overall, the effects of the release of ABC information vary significantly between high-ability and low-ability team leaders, with the negative adoption effects being almost entirely attributable to the latter group. We observe larger increases in process time,

[10] The baseline control group is not affected, as all medium-ability team leaders are assigned to the treatment group; i.e., they receive the treatment for all of their processes. Therefore, the inclusion of medium-ability team leaders does not affect the magnitude or significance of our coefficients for high-ability and low-ability teams.

a higher frequency of bad outcomes and a lower overall efficiency for these low-ability team leaders during the experimental period (*Treated & Low Split*). The decrease in *Quality Assurance* is also stronger for low-ability team leaders, albeit statistically insignificant at conventional levels (p-value=0.1015). For high-ability team leaders, we find slight quality improvements, which manifest in a significantly lower frequency of *Bad Outcomes* and endures during the post-experimental period.

Overall, these results from Table 5 put the main results into perspective and show substantial heterogeneity in the treatment effects across the information asymmetry surrounding the underlying processes and the ability of team leaders who receive the new information based on the updated ABC system. The negative adjustment costs that prevail in the main results are largely attributable to the processes with the greatest information asymmetries (prior to the treatment) and to teams with low-ability team leaders. We find weak evidence for a slight increase in the operational performance of teams with high-ability team leaders. We will further evaluate the persistence of these effects in the long run in section 5.

Table 6 expands the analysis of the heterogeneity in the treatment effects and partitions the coefficients in two ways based on the relative importance of the processes and the ability of team leaders. These results support the findings from the one-way partitioning, adding to the notion that it is the combination of process characteristics and team leader ability that explains the effects. The adjustment costs are highest for processes with both the lowest priority (i.e., plausibly the highest information asymmetries) and a low-ability team leader being in charge (*Treated & Low Ability & Low Priority*). During the experimental period, we observe a statistically significant increase in process time, a higher frequency of bad outcomes, and lower overall efficiency.

5 Long-term Analyses

5.1. Empirical Approach

In this section, we investigate the long-term outcomes of our information treatment. In contrast to our previous analysis, we investigate a substantially longer time period and compare treated processes to unrelated control processes within the company over a period of approximately 1.5 years. We do so by including all time blocks until June 27, 2019. Similar to our short-term analysis, we estimate our treatment effects based on the model explained in section 4.3 (see Equation 2), with the exception that we adjust the time coding of the post-experimental periods to the longer horizon (see Table 1 for details on the definition of the time periods). Identical to our short-term analysis, the first post dummy is equal to 1 for treated processes during the experimental period. We employ two additional time dummies for the medium term and long term, each capturing a longer time period than the post-experimental dummy in our short-term analysis. The medium-term period spans from August 15, 2018, to March 11, 2019, and the long-term period spans from March 12, 2019, to the end of our sample period on June 27, 2019 (see Table 1).

In this analysis, we also employ unidentical and non-randomly selected processes, which never received the treatment, as a control group. Time and cost information of these control processes never became available throughout our sample period, as the control processes consist of external and internal processes that were not part of the costing system update or ABC adoption. External control processes, e.g., the acceptance of a bill from the company, are performed by clients. Internal control processes are independently performed by another firm unit that was not the subject of our experiment. In comparison to those from our short-term analyses, the results from our long-term analyses should thus be interpreted with caution and viewed as descriptive evidence, as selection effects and other potentially unobservable factors more plausibly affect our results.

5.2. Results: Long-term Analyses

We report the results of the long-term analyses in Tables 7 and 8. Table 7 displays the average effect size. We find no significant short-term effect in our experimental period and an increase in quality assurance over a medium time frame. Over a longer time frame, we find an overall efficiency increase and less time required per process. Therefore, our results indicate that on average, a long-term operational improvement is present after the costing system update. Table 8 splits the coefficients of Table 7 based on the ex ante ability of team leaders. For teams with high-ability team leaders, we again find evidence for positive effects through efficiency improvements in the short term, when fewer bad process outcomes occur. Over the long term, we find evidence for a reduction in process time and an overall efficiency increase. In the case of medium-ability team leaders, we find positive long-term effects. Specifically, the process time decreases significantly, while other coefficient estimates are not statistically significant, with their magnitude still pointing towards overall improvement. For low-ability team leaders, we find an overall decrease in efficiency and increase in processing times immediately after the adoption of ABC. However, both effects vanish over medium- and long-term horizons. The coefficient estimates indicate process improvements for time, quality and efficiency but are not statistically significant. Hence, our set of results is consistent with the notion that managers react to information allocation by adjusting processes. Furthermore, short-term adjustment costs are larger in situations in which ex ante information asymmetries are more pronounced, while over a longer time frame, operational improvements are persistent. However, due to the non-experimental nature of the data, we cannot rule out that the long-term positive effects for low-ability team leaders could also arise from an increase in the ability or experience of managers or a simple mean reversion.

6. Learning Effects

In an additional set of analyses, we examine whether managerial experience with the new format of cost information affects the association between the availability of more granular cost information and operational improvements. Note that we observe two treatment groups. First, treated teams that received time-driven cost information for the very first time at the particular time of observation. Second, treated teams that had become familiar with the cost information format when it was previously disclosed for other processes. Previous exposure to the cost information is a dummy variable coded as 1 (*ABC Experience*) if the respective manager received ABC information beforehand for another process and 0 (*No ABC Experience*) if the respective manager has not received this information beforehand.[11] We compare the initial effect around these two different adoption dates between high-ability (*High Ability*) and low-ability (*Low Ability*) team leaders. Similar to our long-term analysis, we employ non-randomly selected processes as a baseline control group. We include time as well as interacted team-process fixed effects. We present the results in Table 9.

Our results suggest that experience with more granular cost information is more relevant for low-ability team leaders. For this group, we observe significant operational improvements in time and efficiency only if they had prior experience with ABC. For high-ability team leaders, the difference is not significant, suggesting that their improvement rate benefits less from experience. In line with our previous set of analyses, the results again display significant differences in process time and efficiency between high- and low-ability team leaders around the very initial adoption when neither group had any experience with the information treatment. Once both managers have gained experience, the subsequent operational improvements no longer differ between the groups. Overall, then, experience with a costing system can enhance the decision-making of low-ability

[11] For *No ABC Experience*, the adoption effect is estimated during the experimental period. For *ABC Experience*, the adoption effect is estimated during the post-experimental period.

team leaders when they receive more granular cost information, while high-ability team leaders do not exhibit significant improvement rate differences based on prior experience.

7. Conclusions

This paper investigates short-term operational effects and long-term associations of changes in the allocation and precision of cost information within a company. To establish short-term causal evidence, we conduct a randomized field experiment at a medium-sized service company where management starts to collect and use information about the costs of individual activities related to the provision of customer services. Management adopts an activity-based costing system in different business units that are randomly selected. Upon adoption of the new system, unit managers receive previously unavailable process-level information about the time and costs of individual activities. The randomization of our information treatment allows us to mitigate one of the main problems in evaluating effects through costing system updates—the fact that changes in costing systems occur endogenously—by comparing the time, quality and efficiency of treated processes with those of untreated processes within the same company as well as within the same team over several months.

On average, we find a short-term increase in the required process time and a decrease in efficiency. However, this effect varies significantly with the ability of team leaders, the relative importance of the processes and the deviation of the new information from prior cost estimates. Specifically, we find that the negative operational effect is attributable to low-ability team leaders, low-priority processes and processes with higher-than-expected costs. In comparison, we observe positive quality effects for high-ability team leaders. Overall, these results are in line with the notion that agency costs are reduced by the provision of additional information but that information processing depends on managerial skills. The more complex the information processing, the higher are the initial adjustment costs, which potentially exceed the immediate benefits of more precise cost information.

In our second set of analyses, we investigate long-term associations.iu our first set of analyses, we can only compare our treated processes with benchmarks that we do not select randomly. In the long run, the provision of more precise cost information is associated with an average decrease in process time and an increase in overall efficiency, i.e., overall positive operational outcomes. These results, while descriptive, are consistent with a catch-up effect, with operational improvements realized over an extended period. Furthermore, we find evidence on learning effects, as low-ability managers benefit from experience with prior cost information. Overall, these results are consistent with the notion that more precise internal information allocation generates long-term operational improvement, while short-term adjustment costs arise. The positive performance effect occurs faster in situations with less information asymmetry, higher managerial ability and greater managerial experience with a more granular level of cost information.

One potential limitation of our study affecting the causal interpretation is omitted and correlated factors, such as spillover effects. In our short-term analyses, we randomize the treatment on an individual team and process level. For each identical process, one of two teams is treated, and the other team is part of the control group. With this approach, we aim to minimize the risk of spillover effects between our treatment and control groups. We are not aware of any communication among different teams in relation to our information treatment, but we could not fully prohibit communication between them. We further control for omitted factors by including time fixed effects as well as interacted process and team fixed effects. Therefore, we control for time-invariant omitted variables on a granular level as well as for time-variant omitted factors as long as they affect all processes equally. In contrast to our short-term analyses, the control groups are non-randomly chosen in our long-term analyses. Therefore, readers should be cautious in interpreting these results as causal evidence. While our approach establishes high internal validity over a limited timeframe, we cannot determine whether our results are externally valid in different settings. Further research is needed to address this question.

Appendix

Variable description

This table provides descriptions of all variables.

Dependent:	
Process Time	Time required for an employee to complete a certain task (computed as the natural logarithm of the time +1).
Bad Outcomes	An indicator variable equal to one if the performed process resulted in a major process setback.
Quality Assurance	An indicator variable equal to one if the performed process required a repetition afterwards.
Overall Efficiency	A factor score including time and quality outcomes from the individual variables Process Time, Quality Assurance and Bad Outcomes. Higher values display increases in efficiency.
Independent:	
Treated (Experimental Period)	An indicator variable equal to one for all treated processes in the experimental period (in total 3.5 months).
Treated (Post-Experimental Period)	An indicator variable equal to one in the post-experimental period for all processes which were treated during the experimental period (in total 3.5 months).
Low and High Priority	An indicator variable equal to one if the respective process is defined as a low or high priority process by the company.
Low and High Management Time Estimate	An indicator variable equal to one if the department head underestimated or overestimated employees time requirements to perform this process.
Low, Medium and High Ability	An indicator variable equal to one if the respective team leader is ex ante viewed by a central unit as low, medium or high ability.
Medium-Term Period	The time period after the experimental period and before the long-term period. The period starts August 15, 2018 and ends March 11, 2019.
Long-Term Period	The last time period available to calculate long-term effects of the treatment. The period starts March 12, 2019 and ends June 27, 2019.
No ABC (ABC) Experience	An indicator variable equal to one if the respective manager did not receive (did receive) ABC information beforehand for another process.

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Figure 1. A stylized illustration of the company's organization

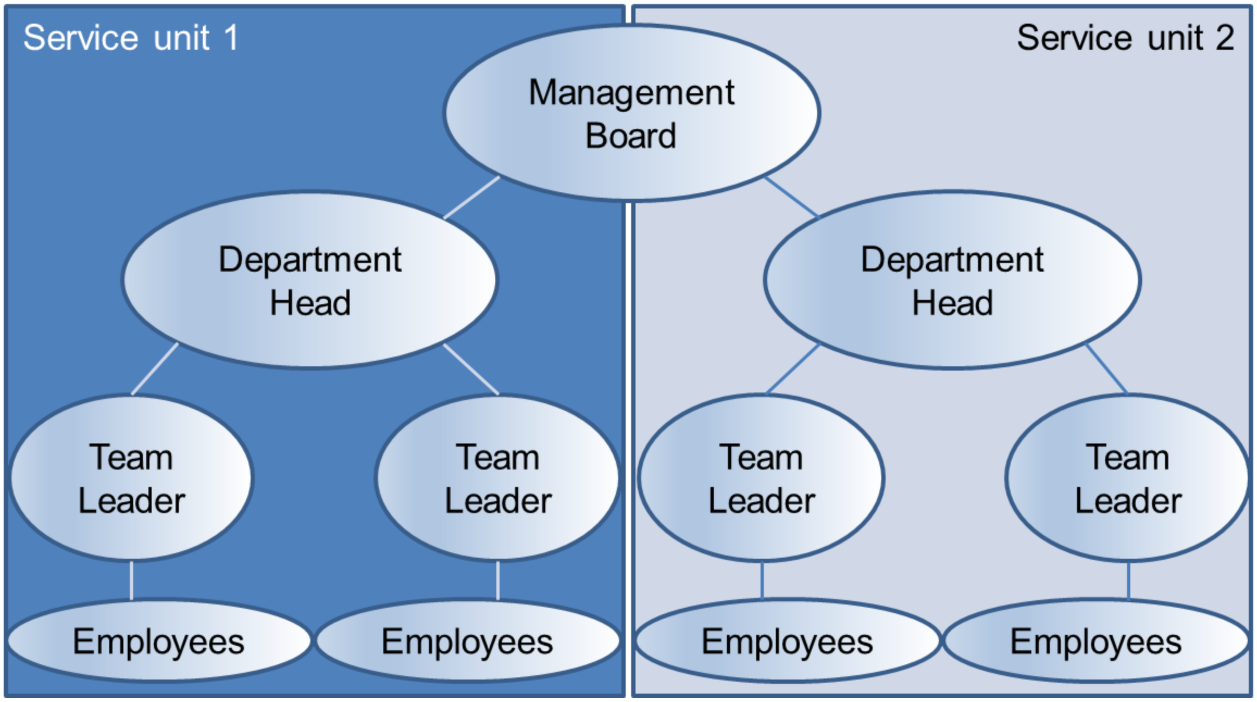


Figure 2. Experimental Treatment by Processes

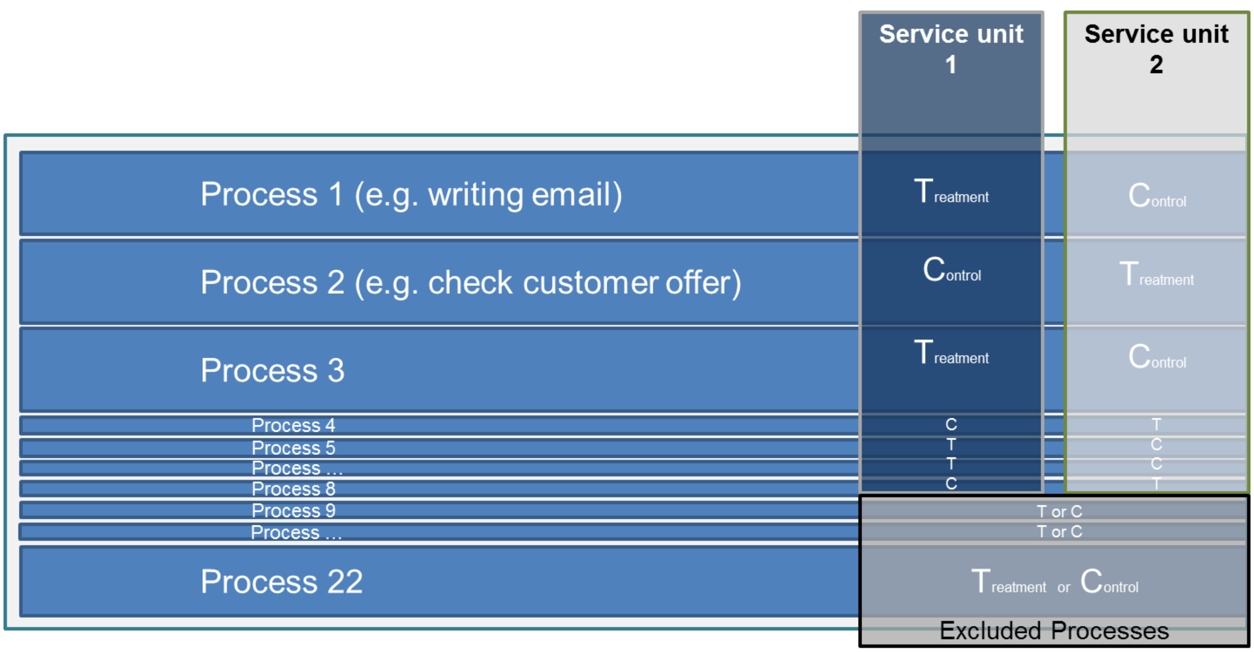


Figure 3. Timeline of the experiment

Start: 01/19/2018

Treatment 1: 05/01/2018

Treatment 2: 08/15/2018

End: 06/27/2019

Time Period	Pre-Experimental	Experimental	Post-Experimental
Process 1 Service Unit 1	**Control**	**Treatment**	**Treatment**
Process 1 Service Unit 2	Control	Control	Treatment
Process 2 Service Unit 1	**Control**	**Control**	**Treatment**
Process 2 Service Unit 2	Control	Treatment	Treatment

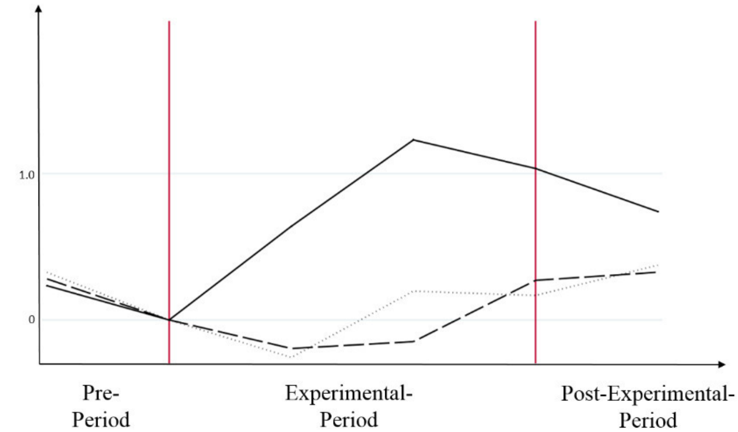
Figure 4. Example cost report (fictitious figures)

Process	Task in Process	Time in Minutes	Unit	**Example Scenario**		
				Amount	Time Total	Costs
Client Offer	Look-up Client	0.5	-	1	0.50	1.00 €
	Set-up client Offer Sheet	1.05	-	1	1.05	2.10 €
	Set-up Article	2.14	Per Article	1	2.14	4.28 €
	Include Travel Time	0.47	Per Visit	2	1.34	2.68 €
	Change Internal Status	0.31		1	0.31	0.62€
			Activity Total:		**6:14**	**10.68 €**

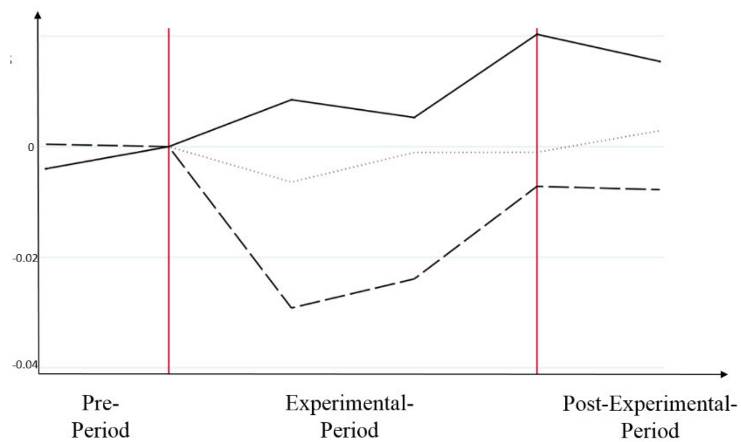
Figure 5. Average treatment effects for identical processes:
Sample splits by team leader ability

These figures plot the change in three outcome variables: 1) Process Time, 2) Bad Outcomes and 3) Overall Efficiency based on the most restrictive sample, with treated processes being benchmarked against identical control processes which are not treated during the experimental period. These control processes receive the treatment later in the post-experimental period. Outcome measures are demeaned on a process level. The control group in each figure consists out of the combined control processes of all teams.

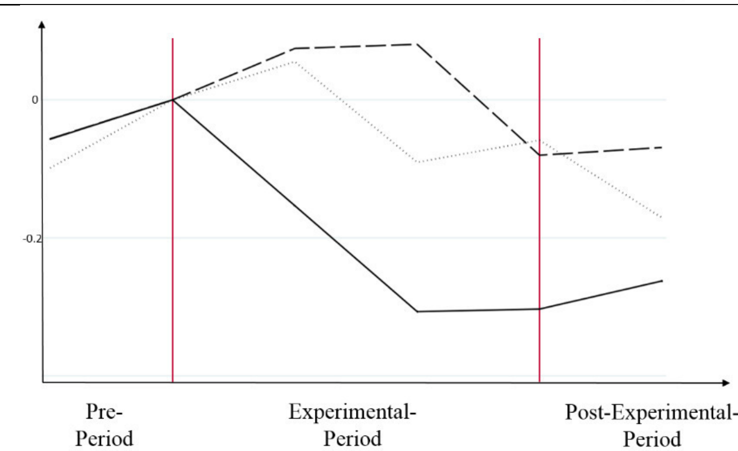
1)
Process Time



2)
Bad Outcomes



3)
Overall Efficiency



Legend:

- Low-ability Team Leader (solid line)
- High-ability Team Leader (dashed line)
- Control (dotted line)

Table 1: Sample Period

This table provides an overview of our sample periods used in the short-term and long-term analyses. Highlighted grey rows indicate the time periods with randomized treatments.

Time period	Block	Short-term Analyses	Long-term Analyses
01/19/2018 - 02/19/2018	1	Pre-Experimental Period	Pre-Experimental Period
02/20/2018 - 03/25/2018	2	Pre-Experimental Period	Pre-Experimental Period
03/26/2018 - 04/30/2018	3	Pre-Experimental Period	Pre-Experimental Period
05/01/2018 - 06/07/2018	4	Experimental Period	Experimental Period
06/08/2018 - 07/11/2018	5	Experimental Period	Experimental Period
07/12/2018 - 08/14/2018	6	Experimental Period	Experimental Period
08/15/2018 - 09/17/2018	7	Post-Experimental Period	Medium-Term
09/18/2018 - 10/22/2018	8	Post-Experimental Period	Medium-Term
10/23/2018 - 11/26/2018	9	Post-Experimental Period	Medium-Term
11/27/2018 - 01/02/2019	10	Not included	Medium-Term
01/03/2019 - 02/05/2019	11	Not included	Medium-Term
02/06/2019 - 03/11/2019	12	Not included	Medium-Term
03/12/2019 - 04/14/2019	13	Not included	Long-Term
04/15/2019 - 05/21/2019	14	Not included	Long-Term
05/22/2019 - 06/27/2019	15	Not included	Long-Term

Table 2: Pre-intervention Differences between Treatment and Control Groups

This table presents pretreatment averages in our outcome variables between our treatment and control groups. Panel A lists the outcome variables for the short-term analyses. Panel B lists the variables used in our long-term analyses. Significance is denoted by ***, **, and * for 1%, 5%, and 10%, respectively.

Panel A: Short-term Analyses

	Treatment Group		Control Group					
	N	Mean	N	Mean	Diff	SE	t-Stat	p-Value
Process Time	2213	2.321	1702	3.599	-1.280	0.063	-20.25	0.000***
Bad Outcomes	2213	0.024	1702	0.020	0.004	0.005	0.85	0.404
Quality Assurance	2213	0.062	1702	0.065	-0.004	0.008	-0.40	0.673
Overall Efficiency	2213	0.245	1702	-0.161	0.405	0.022	17.90	0.000***

Panel B: Long-term Analyses

	Treatment Group		Control Group					
	N	Mean	N	Mean	Diff	SE	t-Stat	p-Value
Process Time	6182	2.252	1781	2.611	-0.358	0.057	-6.25	0.000***
Bad Outcomes	6182	0.015	1781	0.059	-0.045	0.004	-10.80	0.000***
Quality Assurance	6182	0.061	1781	0.042	0.020	0.006	3.15	0.002***
Overall Efficiency	6182	0.048	1781	-0.165	0.212	0.020	11.00	0.000***

Table 3: Pre-intervention Differences between Treatment and Control Groups by Processes

This table presents pretreatment averages in our outcome variables between our treatment and control groups used in the short-term analysis (Table 2, Panel A) and split by individual processes. Process 8 has 111 observations in the treatment group and only one observation in the control group and is therefore excluded from this table. Significance is denoted by ***, **, and * for 1%, 5%, and 10%, respectively.

Panel A: Process 1

	Treatment Group		Control Group					
	N	Mean	N	Mean	Diff	SE	t-Stat	p-Value
Process Time	105	4.718	282	4.833	-0.115	0.126	-0.90	0.365
Bad Outcomes	105	0.029	282	0.018	0.011	0.017	0.65	0.507
Quality Assurance	105	0.095	282	0.060	0.035	0.029	1.20	0.231
Overall Efficiency	105	-0.547	282	-0.541	-0.005	0.053	-0.10	0.917

Panel B: Process 2

	Treatment Group		Control Group					
	N	Mean	N	Mean	Diff	SE	t-Stat	p-Value
Process Time	636	2.773	729	3.063	-0.290	0.092	-3.15	0.002***
Bad Outcomes	636	0.018	729	0.025	-0.007	0.008	-0.95	0.345
Quality Assurance	636	0.101	729	0.017	0.084	0.012	6.90	0.000***
Overall Efficiency	636	0.057	729	0.066	-0.009	0.033	-0.25	0.785

Panel C: Process 3

	Treatment Group		Control Group					
	N	Mean	N	Mean	Diff	SE	t-Stat	p-Value
Process Time	575	1.621	177	1.625	-0.004	0.141	-0.05	0.977
Bad Outcomes	575	0.002	177	0.012	-0.009	0.005	-1.75	0.078*
Quality Assurance	575	0.043	177	0.012	0.032	0.016	2.00	0.044**
Overall Efficiency	575	0.485	177	0.522	-0.037	0.047	-0.80	0.427

Panel D: Process 4

	Treatment Group		Control Group					
	N	Mean	N	Mean	Diff	SE	t-Stat	p-Value
Process Time	479	1.261	75	1.077	0.184	0.238	0.75	0.441
Bad Outcomes	479	0.034	75	0.026	0.006	0.022	0.30	0.760
Quality Assurance	479	0.055	75	0.014	0.041	0.026	1.55	0.126
Overall Efficiency	479	0.586	75	0.692	-0.106	0.079	-1.35	0.177

Panel E: Process 5

	Treatment Group		Control Group					
	N	Mean	N	Mean	Diff	SE	t-Stat	p-Value
Process Time	120	3.152	36	1.295	1.858	0.230	8.10	0.000***
Bad Outcomes	120	0.050	36	0.000	0.050	0.036	1.35	0.173
Quality Assurance	120	0.059	36	0.028	0.030	0.042	0.75	0.469
Overall Efficiency	120	-0.012	36	0.607	-0.618	0.089	-6.95	0.000***

Table 3: continued

Panel F: Process 6

	Treatment Group		Control Group					
	N	Mean	N	Mean	Diff	SE	t-Stat	p-Value
Process Time	26	4.088	9	3.939	0.149	0.564	0.25	0.793
Bad Outcomes	26	0.039	9	0.334	-0.295	0.116	-2.55	0.016**
Quality Assurance	26	0.039	9	0.111	-0.072	0.091	-0.80	0.434
Overall Efficiency	26	-0.281	9	-0.317	0.035	0.206	0.15	0.868

Panel G: Process 7

	Treatment Group		Control Group					
	N	Mean	N	Mean	Diff	SE	t-Stat	p-Value
Process Time	161	4.050	393	5.287	-1.238	0.096	-12.95	0.000***
Bad Outcomes	161	0.037	393	0.010	0.027	0.013	2.20	0.029**
Quality Assurance	161	0.006	393	0.196	-0.190	0.032	-6.00	0.000***
Overall Efficiency	161	-0.232	393	-0.845	0.614	0.049	12.5	0.000***

Table 4. Short-term Analysis: Experimental Evidence

This table presents results on the relation between the assignment of ABC information about randomly selected processes to the team leaders and four outcome variables: (1) Process Time, (2) Bad Outcomes, (3) Quality Assurance and (4) Overall Efficiency. This table is based on the most restrictive sample, with treated processes being benchmarked against identical control processes which are not treated during the experimental period. These control processes receive the treatment later in the post-experimental period. Across all specifications, we include time and team * process fixed effects. All tests are two-sided. T-statistics in parentheses are based on robust standard errors clustered in two ways by process and time blocks. All variables are defined in the Appendix. Significance is denoted by ***, **, and * for 1%, 5%, and 10%, respectively.

Test variables:	*(1) Process Time*	*(2) Bad Outcomes*	*(3) Quality Assurance*	*(4) Overall Efficiency*
Treated (Experimental Period)	0.524*	0.0022	-0.0001	-0.149**
	(2.115)	(0.207)	(-0.00410)	(-2.422)
Treated (Post-Experimental Period)	0.350	-0.0148	0.00373	-0.0915
	(1.705)	(-1.351)	(0.132)	(-1.022)
Observations	10,604	10,604	10,604	10,604
Adjusted R^2	0.406	0.014	0.027	0.333
Team * Process Fixed Effects	Yes	Yes	Yes	Yes
Time Fixed Effects	Time Block	Time Block	Time Block	Time Block

Table 5. Short-term Analysis: Cross-Sectional Heterogeneity in Treatment Effects

This table presents results on the relation between the assignment of ABC information about randomly selected processes to the team leaders and four outcome variables: (1) Process Time, (2) Bad Outcomes, (3) Quality Assurance and (4) Overall Efficiency. The sample is split by three variables: (1) Priority of Processes, (2) Management Time Estimates and (3) Managerial Ability. Columns (1) to (8) are based on the most restrictive sample, with treated processes being benchmarked against identical control processes which are not treated during the experimental period. Columns (9) to (12) also include medium-ability managers, who were non-randomly treated in the experimental period. The inclusion of medium-ability managers does not affect the control group or the coefficients of the other teams. All control processes receive the treatment later in the post-experimental period. Across all specifications, we include time and team * process fixed effects. All tests are two-sided. T-statistics in parentheses are based on robust standard errors clustered in two ways by process and time blocks. We also report p-values from Wald tests assessing the statistical significance of differences across select coefficients. All variables are defined in the Appendix. Significance is denoted by ***, **, and * for 1%, 5%, and 10%, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Split variable:	Priority of Process				Management Time Estimates				Managerial Ability			
Test variables:	*Process Time*	*Bad Outcomes*	*Quality Assurance*	*Overall Efficiency*	*Process Time*	*Bad Outcomes*	*Quality Assurance*	*Overall Efficiency*	*Process Time*	*Bad Outcomes*	*Quality Assurance*	*Overall Efficiency*
[1] Treated & Low Split (Experimental Period)	0.588**	0.0104	-0.0143	-0.153**	0.647**	0.00584	-0.00799	-0.175**	0.917***	0.0213**	-0.0163	-0.256***
	(2.495)	(0.966)	(-0.874)	(-2.625)	(2.445)	(0.498)	(-0.539)	(-2.749)	(5.900)	(2.484)	(-1.606)	(-16.75)
[2] Treated & Low Split (Post-Experimental Period)	0.417	-0.00616	0.00265	-0.116	0.478	-0.0120	-0.000330	-0.124	0.570	-0.00229	-0.000110	-0.118
	(1.640)	(-0.501)	(0.0974)	(-1.112)	(1.863)	(-0.944)	(-0.0145)	(-1.221)	(1.265)	(-0.203)	(-0.00434)	(-0.981)
[3] Treated & High Split (Experimental Period)	0.114*	-0.0227***	0.0376	-0.0697	-0.119	-0.0164**	0.0387	-0.0113	0.0966	-0.0180*	0.0195	0.0202
	(2.351)	(-3.775)	(1.126)	(-1.476)	(-0.618)	(-3.208)	(0.769)	(-0.0960)	(1.016)	(-2.161)	(0.682)	(0.434)
[4] Treated & High Split (Post-Experimental Period)	0.0224	-0.0406***	0.0109	0.0123	-0.236	-0.0286***	0.0268	0.0505	0.0806	-0.0284**	0.0144	0.0609
	(0.187)	(-4.823)	(0.300)	(0.161)	(-0.900)	(-5.518)	(0.532)	(0.331)	(0.851)	(-2.364)	(0.420)	(0.863)
P-value: [1] = [3]	0.0634*	0.0120**	0.0284**	0.1519	0.0137**	0.0436**	0.2971	0.1619	0.0002***	0.0012***	0.1015	0.0000***
P-value: [1]+[2] = [3]+[4]	0.1173	0.0037***	0.1169	0.1453	0.0309**	0.0792*	0.3461	0.1943	0.0457**	0.0110**	0.2519	0.0009***
Treated & Medium Split (Experimental Period)	-	-	-	-	-	-	-	-	0.231	0.00220	-0.00126	-0.0567**
									(1.209)	(0.575)	(-0.108)	(-3.246)
Treated & Medium Split (Post-Experimental Period)	-	-	-	-	-	-	-	-	0.138	0.00192	0.0282	-0.0591
									(1.449)	(0.115)	(1.202)	(-1.060)
Priority	-0.737***	0.00814	-0.00137	0.203***	-	-	-	-	-	-	-	-
	(-6.646)	(1.676)	(-0.107)	(7.654)								
Treated & High Priority	0.312	0.0140**	0.0323*	-0.149*	-	-	-	-	-	-	-	-
	(1.034)	(2.621)	(2.340)	(-1.963)								
Observations	10,604	10,604	10,604	10,604	10,604	10,604	10,604	10,604	19,970	19,970	19,970	19,970
Adjusted R^2	0.426	0.015	0.031	0.342	0.408	0.014	0.027	0.334	0.556	0.018	0.046	0.353
Team * Process Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Time Block	Time Block	Time Block	Time Block	Time Block	Time Block	Time Block	Time Block	Time Block	Time Block	Time Block	Time Block

Table 6. Short-term Analysis: Two-Way Partitioning by Priority and Ability

This table presents results on the relation between the assignment of ABC information about randomly selected processes to the team leaders and four outcome variables: (1) Process Time, (2) Bad Outcomes, (3) Quality Assurance and (4) Overall Efficiency. The sample is split by both Priority of Processes and Managerial Ability. The analysis is based on the most restrictive sample, with treated processes being benchmarked against identical control processes which are not treated during the experimental period. In addition, the table includes medium-ability managers, who were non-randomly treated in the experimental period. The inclusion of medium-ability managers does not affect the control group or the coefficients of the other teams. All control processes receive the treatment later in the post-experimental period. Across all specifications, we include time and team * process fixed effects. All tests are two-sided. T-statistics in parentheses are based on robust standard errors clustered in two ways by process and time blocks. All variables are defined in the Appendix. Significance is denoted by ***, **, and * for 1%, 5%, and 10%, respectively.

Test variables:	*(1) Process Time*	*(2) Bad Outcomes*	*(3) Quality Assurance*	*(4) Overall Efficiency*
High Priority	-0.734***	0.00823	-0.00150	0.137***
	(-6.407)	(1.428)	(-0.118)	(5.949)
High Priority & High Ability	-0.0139	0.00416	0.0378*	-0.0405
	(-0.0597)	(0.525)	(2.124)	(-1.327)
High Priority & Medium Ability	0.846***	-0.0330***	0.152***	-0.204***
	(3.684)	(-6.234)	(4.825)	(-3.868)
High Priority & Low Ability	0.757***	-0.0146	0.0272	-0.142***
	(4.814)	(-1.592)	(0.863)	(-5.642)
Treated & High Ability & Low Priority (Experimental Period)	0.0207	-0.0123	-0.00421	0.0377
	(0.0893)	(-1.004)	(-0.176)	(0.542)
Treated & High Ability & Low Priority (Post-Period)	0.0553	-0.0168	0.0143	0.0297
	(0.297)	(-1.055)	(0.426)	(0.349)
Treated & High Ability & High Priority (Experimental Period)	0.110	-0.0236***	0.0415	0.0173
	(0.838)	(-3.686)	(1.099)	(0.429)
Treated & High Ability & High Priority (Post-Period)	0.0272	-0.0432***	0.0142	0.119
	(0.175)	(-4.053)	(0.331)	(1.857)
Treated & Medium Ability & Low Priority (Experimental Period)	0.261	0.00158	0.00901	-0.0696**
	(1.136)	(0.234)	(0.405)	(-2.732)
Treated & Medium Ability & Low Priority (Post-Period)	0.230	0.00491	0.0406	-0.0989
	(1.768)	(0.244)	(1.321)	(-1.599)
Treated & Medium Ability & High Priority (Experimental Period)	-0.122	0.00599**	-0.0262	0.0288
	(-0.750)	(2.784)	(-1.599)	(0.870)
Treated & Medium Ability & High Priority (Post-Period)	-0.225**	-0.00629	-0.00246	0.0714
	(-2.445)	(-0.665)	(-0.130)	(1.481)
Treated & Low Ability & Low Priority (Experimental Period)	0.862***	0.0226*	-0.0159	-0.248***
	(5.418)	(2.284)	(-1.020)	(-12.98)
Treated & Low Ability & Low Priority (Post-Period)	0.581	-0.00260	-0.000641	-0.119
	(1.279)	(-0.231)	(-0.0235)	(-0.975)
Treated & Low Ability & High Priority (Experimental Period)	0.106	0.00959	-0.162***	0.0766
	(0.351)	(1.498)	(-7.881)	(1.542)
Treated & Low Ability & High Priority (Post-Period)	-0.528**	0.0363*	-0.121***	0.0980***
	(-2.532)	(1.956)	(-4.511)	(3.470)
Observations	19,970	19,970	19,970	19,970
Adjusted R^2	0.566	0.019	0.048	0.358
Team * Process Fixed Effects	Yes	Yes	Yes	Yes
Time Fixed Effects	Time Block	Time Block	Time Block	Time Block

Table 7. Long-term Analysis

This table presents results on the relation between the assignment of ABC information about selected processes to the team leaders and four outcome variables: (1) Process Time, (2) Bad Outcomes, (3) Quality Assurance and (4) Overall Efficiency. This table provides a long-term analysis. Thereby, treated processes are benchmarked against different untreated control processes, which never receive a treatment. The experimental period is identical to previous analyses and spans from May 01, 2018 until August 14, 2018. The medium-term period spans from August 15, 2018 until March 11, 2019 and the long-term period spans from March 12, 2019 until June 27, 2019. Across all specifications, we include time and team * process fixed effects. All tests are two-sided. T-statistics in parentheses are based on robust standard errors clustered in two ways by process and time blocks. All variables are defined in the Appendix. Significance is denoted by ***, **, and * for 1%, 5%, and 10%, respectively. The experimental period is identical to previous analysis and spans from May 01, 2018 until August 14, 2018.

Test variables:	*(1) Process Time*	*(2) Bad Outcomes*	*(3) Quality Assurance*	*(4) Overall Efficiency*
Treated (Experimental Period)	-0.0567	-0.00449	0.00806	0.0171
	(-0.327)	(-0.950)	(1.262)	(0.476)
Treated (Medium-Term Period)	0.0554	-0.0274	0.0162**	0.0316
	(0.517)	(-0.908)	(2.781)	(0.660)
Treated (Long-Term Period)	-0.234**	-0.0325	-0.00352	0.124*
	(-2.235)	(-0.926)	(-0.589)	(1.785)
Observations	41,210	41,210	41,210	41,210
Adjusted R^2	0.538	0.075	0.033	0.410
Team * Process Fixed Effects	Yes	Yes	Yes	Yes
Time Fixed Effects	Time Block	Time Block	Time Block	Time Block

Table 8. Long-term Analysis: Partitioning by Ability

This table presents results on the relation between the assignment of ABC information about selected processes to the team leaders and four outcome variables: (1) Process Time, (2) Bad Outcomes, (3) Quality Assurance and (4) Overall Efficiency. This table provides a long-term analysis. Thereby, treated processes are benchmarked against different untreated control processes, which never receive a treatment. The table includes medium-ability managers, who were non-randomly treated in the experimental period. The inclusion of medium-ability managers does not affect the control group or the coefficients of the other teams. The experimental period is identical to previous analyses and spans from May 01, 2018 until August 14, 2018. The medium-term period spans from August 15, 2018 until March 11, 2019 and the long-term period spans from March 12, 2019 until June 27, 2019. Across all specifications, we include time and team * process fixed effects. All tests are two-sided. T-statistics in parentheses are based on robust standard errors clustered in two ways by process and time blocks. All variables are defined in the Appendix. Significance is denoted by ***, **, and * for 1%, 5%, and 10%, respectively.

Test variables:	*(1) Process Time*	*(2) Bad Outcomes*	*(3) Quality Assurance*	*(4) Overall Efficiency*
Treated & High Ability (Experimental Period)	-0.200	-0.0262***	0.0330	0.0782
	(-1.051)	(-3.963)	(1.468)	(1.474)
Treated & High Ability (Medium-Term Period)	0.0621	-0.0426	0.0155	0.0612
	(0.529)	(-1.428)	(0.949)	(1.161)
Treated & High Ability (Long-Term Period)	-0.464***	0.0173	0.0177	0.0634**
	(-4.656)	(0.722)	(1.161)	(2.966)
Treated & Medium Ability (Experimental Period)	-0.149	-0.00512	0.00772	0.0407
	(-0.898)	(-0.941)	(1.209)	(1.274)
Treated & Medium Ability (Medium-Term Period)	0.00633	-0.0271	0.0228**	0.0381
	(0.0554)	(-0.885)	(2.696)	(0.757)
Treated & Medium Ability (Long-Term Period)	-0.258**	-0.0390	-0.00511	0.144
	(-2.251)	(-0.899)	(-0.874)	(1.685)
Treated & Low Ability (Experimental Period)	0.556**	0.0200	-0.00840	-0.167***
	(2.311)	(1.583)	(-1.267)	(-4.065)
Treated & Low Ability (Medium-Term Period)	0.171	-0.0179	-0.0237	0.0119
	(0.571)	(-0.558)	(-1.226)	(0.145)
Treated & Low Ability (Long-Term Period)	-0.0193	-0.0402	-0.00850	0.0922
	(-0.123)	(-1.036)	(-0.375)	(1.121)
Observations	41,210	41,210	41,210	41,210
Adjusted R^2	0.539	0.076	0.034	0.411
Team * Process Fixed Effects	Yes	Yes	Yes	Yes
Time Fixed Effects	Time Block	Time Block	Time Block	Time Block

Table 9. Learning effect

This table presents results on the relation between the assignment of ABC information about selected processes to the team leaders and three outcome variables: (1) Process Time, (2) Bad Outcomes and (3) Overall Efficiency. All untreated control processes are included. We differentiate between managers with and without ABC experience. “No ABC Experience” captures adoption effects for managers without previous cost accounting information. This coefficient is estimated during the initial adoption period during the experimental period. “ABC Experience” captures adoption effects for managers with previous cost accounting information. Hence, this coefficient is estimated during the post-period. This analysis is based on a sample spanning from January 19, 2018 until August 14, 2019. Across all specifications, we include time and team * process fixed effects. All tests are two-sided. T-statistics in parentheses are based on robust standard errors clustered in two ways by process and time blocks. All variables are defined in the Appendix. Significance is denoted by ***, **, and * for 1%, 5%, and 10%, respectively.

Panel A: Process time

Adjusted initial effect	High Ability	Low Ability	Δ Manager	p-Value
No ABC Experience	-0.147	0.611*	-0.758	0.0001***
ABC Experience	-0.161	-0.251**	0.090	0.4653
Δ Experience	0.014	0.862		
p-Value	0.9233	0.0086***		

Panel B: Bad outcomes

Adjusted initial effect	High Ability	Low Ability	Δ Manager	p-Value
No ABC Experience	-0.018	0.014	-0.032	0.0262**
ABC Experience	-0.022	-0.028	0.006	0.8236
Δ Experience	0.004	0.042		
p-Value	0.9088	0.275		

Panel C: Efficiency

Adjusted initial effect	High Ability	Low Ability	Δ Manager	p-Value
No ABC Experience	0.077	-0.204**	0.281	0.0000***
ABC Experience	0.089*	0.129*	-0.040	0.5952
Δ Experience	-0.012	-0.333		
p-Value	0.9033	0.0130**		

TRR 266 Accounting for Transparency

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