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ABSTRACT

Information Provision, Incentives, and Attention: A Field Experiment on Facilitating and Influencing Managers' Decisions*

The core role of managerial accounting is to provide information to facilitate managers' decisions and influence their behavior through incentives. We study the impact of these two roles of information on profits by implementing a field experiment in a large retail chain. In a 2 × 2 factorial design, we vary: (i) whether store managers obtain access to decision-facilitating accounting information on the profit margins of individual products and (ii) whether they receive performance pay based on an objective profit metric to influence their decisions. We find that both practices increase profits significantly, albeit through different behavioral channels. In particular, managers make use of the information provided by placing higher-margin products, thereby raising the gross profit margin. While we hypothesized a priori that both practices are complements, we find that the profit increases induced by the combined intervention do not significantly exceed those of the separate interventions. We attribute this finding to an attention-directing role of the interventions toward the objective of raising profits, thereby inducing a countervailing substitution effect. We show that this effect fades over time such that the combined intervention tends to induce more persistent profit increases.

JEL Classification: J33, M52, C93

Keywords: management controls, performance pay, monetary incentives, decision-facilitating, decision-influencing, accounting information, field experiment, complementarity

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1 Introduction

Standard principal–agent models assume that agents know the specific functional form linking effort to performance but that their interests differ from the interests of their principal (employer). From this perspective, accounting information is used to assess and reward performance to align interests. However, if agents lack information about the underlying production function, this may limit their possibility of yielding optimal work results, even when their interests are in line with the employer's objectives. Hence, accounting scholars have stressed the dual role of information in organizations: information is used to *influence decisions* for instance through performance pay – and it serves to *facilitate decisions*, helping managers make better decisions (Demski and Feltham 1976, Baiman 1982, Narayanan and Davila 1998). This study examines the effects of these two roles of information in a firm-level field experiment with a large German retail chain. In a 2 × 2 experimental design, each practice is implemented separately and both are implemented in combination. The study thus allows us to causally evaluate the impact of both practices and their interplay in the same organizational environment.

Decision-facilitating information helps employees make better decisions by providing ex-ante information to decrease uncertainty about specific actions and increase the agents' knowledge about the decision problem (Demski and Feltham 1976, Evans at el. 1994, Narayanan and Davila 1998, Sprinkle 2003, Wall and Greiling 2011). In contrast, decision-influencing information entails evaluating agents' behavior to affect their incentives through performance pay or supervisor monitoring (Demski and Feltham 1976, Baiman 1982, Narayanan and Davila 1998).[1] Research on principal-agent models has typically focused on the latter and has studied the asymmetry of interests in organizations and the resulting control problems (see, e.g., Jensen and Meckling 1976, Holmström and Milgrom 1991, Merchant 1985, Sunder 1997, Indjejikian 1999, Prendergast 1999, Lazear 2000, Lazear 2018).

To understand and compare the performance effects of both roles of managerial accounting information (facilitating and influencing), it seems essential to study their impact in the same environment. As, for instance, Sprinkle claims (2003, p.288): "*It is important to study empirically how both roles of managerial accounting information affect the behavior of individuals who compose organizations.*"[2] To do so, we first analyze the effects of decision-

[1] For a summary see Sprinkle (2003) and Sprinkle and Williamson (2006).

[2] The importance of studying interaction effects between different management controls has been stressed by various scholars (Milgrom and Roberts 1995, Holmström and Milgrom 1994, Ichniowski et al. 1997, Bonner and Sprinkle 2002, Grabner and Moers 2013, Hofmann and van Lent 2017, Bedford 2020, Choi 2020, Merchant and Otley 2020), However, only little causal field-experimental evidence on such interdependencies exists (Lourenço 2016, Manthei et al. 2019, Sandvik et al. 2020).

facilitating and decision-influencing information and their interaction in a theoretical model. Then, we provide causal evidence from a field experiment within the same organization on the effects of both roles of accounting information on performance and behavior. Specifically, we implemented a firm-level field experiment with a 2 × 2 factorial design within a large German retail chain. The field experiment was conducted over a period of three months. Overall, 363 store managers of discount supermarkets within a particular geographical region of the retail chain were randomly assigned to three different treatment groups and a control group.[3]

To facilitate their decisions, store managers in the *INFORMATION* treatment group obtained information about the underlying production function. The information comprised three ingredients: (i) novel information about profit margins of individual products, (ii) a short online training to brush-up knowledge on possible influencing factors to increase profits, and (iii) a monthly electronic performance report concerning store profits.[4] While store managers in the *BONUS* treatment group received monetary performance pay based on a simple profit metric, the third treatment group received both information and performance pay. The control group remained completely unaffected without any changes or information about the other groups. The randomly assigned provision of either decision-facilitating information, decision-influencing with performance pay, or their combination allows examining the precise causal effects of the interventions in the same environment (see e.g., Bandiera et al. 2011, Floyd and List 2016 for recent surveys on field experiments).[5]

We formalize the interplay between performance pay and the provision of decision-facilitating information by extending a standard multitasking moral hazard model (Holmström and Milgrom 1991, Baker 1992, Feltham and Xie 1994, Hemmer 1996). In the model, agents face uncertainty about the marginal returns of their efforts for different tasks. As performance pay raises efforts and providing agents with information about marginal returns helps them allocate efforts more efficiently across tasks, performance pay and the provision of decision-facilitating information are naturally complements in such a framework.

Our empirical results show the following: First, when introduced separately, both performance pay and the provision of decision-facilitating information have positive average performance effects. Interestingly, the effect of information provision surpasses that of

[3] The experiment was pre-registered at the American Economic Association's registry for randomized controlled trials. The experiments were approved by the workers' council serving as an IRB substitute (as our institutions did not have an IRB at the time the experiment was conducted).

[4] Before the intervention, store performance was mostly assessed by tracking single components of store profits such as sales and inventory losses. As a first step, a simple profit metric was introduced in all treatments as an aggregate accounting return measure. The online training and performance feedback assured that managers were provided with information on profit margins and helped to brush-up the managers' knowledge.

[5] For a discussion on endogeneity in managerial accounting research, see Chenhall and Moers (2007) and Van Lent (2007).

performance pay in nearly all specifications. Moreover, when subtracting the costs of the bonus, the information provision treatment clearly outperforms the performance pay treatment, implying that a lack of store managers' financial incentives to use the provided information tends to be a less severe limiting factor than a lack of information in our setting. With a return on investment of approximately 5,500%, the decision-facilitating intervention was highly profitable. The combined intervention, in which both practices are introduced together, also increases performance and. Yet, this increase is only approximately 21.63% larger than the performance effect in the treatment with information provision alone.

We then use more detailed financial data and self-elicited survey data to investigate how the treatments affected store managers' behavior.[6] First, we find that the performance effect in the decision-facilitating interventions mostly results from increases in the gross profit margin. Moreover, sizeable gains in the gross profit margin are achieved even when no bonus is paid. In the treatment where a bonus is implemented but no decision-facilitating information is provided, the treatment effect is to a larger extent driven by other profit components. In a detailed survey, managers were asked to state their activities during the time of the interventions. Here, we see considerable treatment differences, showing that when decision-facilitating information is provided, managers spend significantly more effort in placing high-margin products, showing that the information is indeed used productively. This effect again appears irrespective of whether the managers obtained performance pay.

A further key result is that in contrast to our ex-ante prediction based on standard agency theory, we do not find evidence for an overall complementarity between the two practices. We provide an explanation for this pattern by investigating the *attention-directing role of the interventions*. We argue that both practices create initial attention for the underlying objective to raise profits. In a first step we extend the formal model, incorporating an attention-directing role of interventions. The model captures key features stressed in the literature on attention (see, e.g., Simon 1947, Kahneman 1973, Birnberg and Shields 1984, Ocasio 1997, Hirshleifer and Teoh 2003), namely that attention is limited in capacity and tends to fade over time. The extension shows that when different interventions raise awareness of a performance objective, they serve a similar role in guiding attention limited by capacity constraints and thus are substitutes in this respect. This creates a countervailing effect, dampening, or even reversing the complementarity arising from standard agency considerations. The model extension

[6] The importance of developing a detailed understanding of the key result and the behavioral changes triggered by the interventions is discussed in, for instance, Hall (2010). For a further discussion on the benefits of understanding the environment in detail and analyzing additional quantitative along with qualitative data to explain causal effects, see Ichniowksi and Shaw (2003) and Ittner (2014).

furthermore shows that, as attention fades over time, the interaction effects moves in the opposite direction. As as the substitution effect induced by the newly created attention vanishes, the underlying complementarity driven by standard reasoning should prevail.

We then explore implications of the extended model empirically. As a first indication, we show that manager's self-reported aim to raise profits is significantly stronger in all three treatment groups than in the control group. However, the stated aim in the combined intervention hardly exceeds the level achieved when either of the practice is implemented alone supporting the view that the practices are substitutes in generating attention. Examining the timing of the treatment effects and their interplay, we find that the average treatment effect of the single interventions – information provision and performance pay – decreases throughout the experiment. Moreover, while in the first month after the intervention, the two practices are clear substitutes (i.e. the sum of the two separate effects significantly exceeded the effect of the combined intervention), this substitution effect vanishes over the course of the experiment. In turn, the performance gains of the combined intervention tend to be more persistent than the effects of either providing only information or implementing only a bonus.

Specifically, we make various contributions to the literature. First, we investigated the causal impact of decision-facilitating information and performance pay in the same environment. The approach combine the internal validity of a 2 × 2 experimental design with the external validity of a firm-level field experiment. We show that both practices can induce similar performance effects, but facilitating managers' decisions comes at lower costs. Second, we show that both types of interventions affect managers' behavior differently. Third, in contrast to the formal model using standard incentive considerations we observed no overall complementary between decision-facilitating information and performance pay. Fourth, we provide an explanation for the absence of a complementary based on the view that both practices also have an attention-directing effect (Simon 1954). As we show in an extension of the formal model, attention effects naturally dampen potential complementarities between different practices, particularly shortly after their implementation. Examining the timing of the treatment effects, we provide evidence in line with this idea. Fifth, we generally contribute to the small but growing literature using field experiments to investigate interactions of management controls in organizations (Lourenco 2016) as well as the recent conceptual literature in accounting on interdependencies between management practices/controls (see, e.g., Ferreira and Otley 2009, Brynjolfsson and Milgrom 2013, Grabner and Moers 2013).

2 Literature Overview

A core role of managerial accounting is the processing and provision of information to facilitate managers' decisions and influence their behavior through incentives. Information is used to influence decisions, for instance, through performance pay, and information serves to facilitate decisions, helping managers make better decisions (Demski and Feltham 1976, Baiman 1982, Narayanan and Davila 1998).

Although there is supportive empirical evidence for both roles of managerial accounting information, to our knowledge, the causal effect of both practices has not been evaluated in the same environment and time horizon in a field setting. The provision of decision-facilitating information can, for instance, increase learning and improve the quality of decisions (Ghosh 1997, Frederickson et al. 1999). Anderson and Kimball (2019) showed that providing school teachers with information about students' learning progress facilitates their diagnoses and possible focus to improve students' performance. In Casas-Arce et al. (2017a), the simple provision of customer lifetime value data to bank employees positively impacts the customer value and increases the employees' attention toward more profitable clients.[7] Manthei and Sliwka (2019) find in a field experiment, that providing supervisors with objective information of subordinates' performance raises profits in a retail bank. In a slightly different context, Farrell et al. (2008) showed that contracting on a forward-looking measure facilitates effort allocation across multiple periods.[8]

Using decision-influencing information to provide performance pay has also been the focus of very broad empirical literature in accounting and economics. Most investigated performance incentives and rewards have shown positive effects on performance (Bailey et al. 1998, Banker et al. 2000, Sprinkle 2000, Lazear 2000, Shearer 2004, Presslee et al. 2013, Lourenço 2016, Friebel et al. 2017). Nevertheless, different environmental circumstances, such as task complexity, multitasking, different preferences, image concerns, or exhausted learning curves, can reduce the positive effect (Holmström and Milgrom 1991, Bonner et al. 2000, Frey and Jegen 2001, Bénabou and Tirole 2006, Sliwka 2007, Manthei et al. 2021). Moreover,

[7] The literature also shows some countervailing effects. For instance, too frequent (performance) information can reduce positive effects at least if the employees lack the choice of receiving the information (Casas-Arce et al. 2017b, Holderness et al. 2019).

[8] The process of providing decision-facilitating information is also related to using trainings and knowledge transmission in organizations (see, e.g. Dearden et al. (2006) and Bassanini et al. (2007) for summaries of the training literature, and De Grip and Sauermann (2012) for a field experiment to estimate the effect of training on worker productivity). Work-related trainings can also be interpreted as filling a gap of knowledge (information) about a specific production function. Field experiments by Bloom et al. (2013) and Hanna et al. (2014) show, for instance, that managers are frequently unaware of the underlying production function and find substantial profit increases through the implementation of new practices.

performance pay may negatively influence the positive effects of other management controls (Manthei et al. 2019).[9]

Only a few studies, yet with different methodologies, have directly considered the interplay between the two roles of information or their relative performance. Using survey data, van Veen-Dirks (2010) showed that firms tend to attach more importance to the decision-facilitating rather than the decision-influencing use of a broad set of accounting key figures. In a laboratory experiment, Sprinkle (2000) investigated performance effects over multiple periods. He found that feedback information to facilitate learning and utilizing performance incentives are not independent. Importantly, learning effects are greater when the information provided to facilitate learning is also part of the performance incentive. Similarly, Grafton et al. (2010) argue that performance is correlated with the degree of commonality between decision-facilitating and decision-influencing information. They claimed that for managers to actually use decision-facilitating measures, these measures should also be part of the decision-influencing process (in their case performance evaluations). This is related to Indjejikian and Matejka (2006), who also emphasized that managers should focus on both decision-facilitating and decision-influencing, and Drake et al. (1999), who provided evidence for the complementarity between the implementation of activity-based costing information combined with incentives in an experiment with students from a M.B.A. program.[10] For both roles of accounting information to be complementary, it is generally important that there exists further leeway in both dimensions. That is, decision-facilitating information provides additional value, and performance bonuses can raise efforts above prior levels (Sprinkle 2003). Our results show a further interdependency between the two practices: When both management control practices are implemented with a similar objective they can become substitutes in guiding attention towards this objective.

In general, the managerial accounting literature has a long history of studying the interplay of the organizational environment and management controls. As argued by Otley (1980) and Chenhall (2003), the effect of a management control practice may depend on using other practices in place. Similarly, the literature on complementarities in organizations in economics (Milgrom and Roberts 1995, Ichniowski and Shaw 2003, Brynjolfsson and Milgrom 2013) has revealed that the performance effect of introducing a specific management practice may be contingent on using other practices. A set of management control practices is often categorized

[9] The literature on decision-facilitating and decision-influencing information is also closely connected to the design of performance measurement systems (Banker et al. 1993, Ittner et al. 1997, Ittner and Larcker 2002).

[10] Potentially adverse effects of combining decision-facilitating information and decision-influencing using performance measures are discussed in Narayanan and Davila (1998) and Indjejikian and Matejka (2009).

as a system if there are independencies between these practices (Ferreira and Otley 2009, Grabner and Moers 2013, or Masschelein and Moers 2020). From this perspective, our study asks the question of whether the combination of providing decision-facilitating information and performance pay constitutes a "system" rather than a mere "package" of management control practices.[11]

Although the importance of studying interaction effects explicitly has often been pointed out, only a few field experiments have aimed to estimate causal complementarities in organizations and those who did, mostly have found little evidence for complementarities. Lourenco (2016) conducted a field experiment in a retail service organization, randomly varying the use of monetary incentives, feedback, and recognition as well as their interactions. While she did not find evidence of complementarities between these practices, she found that monetary incentives and recognition are substitutes. Manthei et al. (2019) provide evidence that regular meetings with a supervisor are only performance-enhancing if not combined with financial incentives for the employee. Blader et al. (2020) implemented different performance information conditions in a field experiment with a transportation company and found that the effect of publicly posting the drivers' performance depends on whether the corresponding worksite was already subject to a major reorganization toward more teamwork and empowerment. Sandvik et al. (2020) showed that the positive performance effect of structured meetings between coworkers to share knowledge is independent of additional monetary incentives.

3 Hypothesis Development

As described in the above, the empirical literature on providing decision-facilitating information or using decision-influencing information to set incentives mostly showed that both practices positively affect agents' performance. In order to derive hypotheses in particular on their interplay within the same theoretical framework we now adapt the classical framework of a multitasking principal agent model (Holmström and Milgrom 1991, Baker 1992, Feltham and Xie 1994, Hemmer 1996).

[11] Grabner and Moers (2013), for instance, distinguish between packages and systems of management control practices. Whereas a package describes the actual set of practices in place, irrespective of whether there are interdependencies, they advocate to use the term system only if there are interdependencies. See also Bedford (2020), Choi (2020), or Merchant and Otley (2020) for more recent discussions.

3.1 A Conceptual Framework

Consider the following multitasking principal agent model. A risk neutral agent is working on $j = 1, \ldots, k$ tasks and can exert a vector e of efforts e_j on task j at cost $\frac{1}{2}e_j^2$. Effort generates output for the principal, where the marginal returns of effort are given by r_j for task j, such that output is

$$\pi = \sum_{j=1}^{k} (e_j r_j + \eta_j),$$

where $\eta_j \sim N\left(0, \sigma_{\eta_j}^2\right)$ are independent noise terms. As in Bushman et al. (2000), marginal returns are ex-ante unknown, and the r_j are independently drawn from a normal distribution with $r_j \sim N(m_j, \sigma_j^2)$. The employer may either provide decision-facilitating information about marginal returns or not and chooses $I \in \{0,1\}$. The agent observes a vector $s(I)$ of individual signals $s_j = r_j + (1 - I)\varepsilon_j$ with $\varepsilon_j \sim N\left(0, \sigma_{\varepsilon_j}^2\right)$ for each task. Hence, when decision-facilitating information is provided ($I = 1$), the agent learns the marginal returns of effort for each task precisely, whereas without this information, only noisy signals on the marginal returns are observed.

The agent receives a performance contingent bonus $\beta \cdot \pi$ with $\beta \in [0,1]$. We allow for the possibility that even without bonus pay the agent internalizes the principal's well-being to some extent (due to social preferences or career concerns). The agent's objective function is

$$h\beta\pi + \theta\pi - \sum_{j=1}^{k} \frac{1}{2} e_j^2,$$

where h measures the marginal utility of money, and θ the degree to which the agent internalizes the effect of his actions on profits.

The agent maximizes

$$\max_e E_A \left[(h\beta + \theta) \left(\sum_{j=1}^{k} (e_j r_j + \eta_j) \right) \middle| s(I) \right] - \sum_{j=1}^{k} \frac{1}{2} e_j^2$$

and thus chooses

$$e_j = (h\beta + \theta) E_A[r_j | s(I)].$$

Ex-ante expected performance is thus

$$(h\beta + \theta) \sum_{j=1}^{k} E\left[r_j E_A[r_j | s(I)] \right].$$

Using that $E_A[r_j|s_j] = m_j + \frac{\sigma_j^2}{\sigma_j^2+(1-I)\sigma_{\varepsilon_j}^2}(s_j - m_j)$ and simplifying this expression becomes

$$\Pi(I,\beta) = (h\beta + \theta) \cdot \sum_{j=1}^{k} \left(m_j^2 + \frac{\sigma_j^4}{\sigma_j^2 + (1-I) \cdot \sigma_{\varepsilon_j}^2} \right).$$

Hence, both the provision of information (choosing $I = 1$) and using bonuses β increase expected profits. The bonus affects performance due to the standard incentive effect: as marginal returns to effort grow, the agent works harder. Information provision raises performance, as the agent can allocate his efforts more efficiently across tasks.

Moreover, there is a complementarity between both practices, i.e., the bonus has a stronger effect on performance if $I = 1$: To see that, note that $\frac{\partial \Pi(1,\beta)}{\partial \beta} > \frac{\partial \Pi(0,\beta)}{\partial \beta}$ as

$$\sum_{j=1}^{k} (m_j^2 + \sigma_j^2) > \sum_{j=1}^{k} \left(m_j^2 + \frac{\sigma_j^4}{\sigma_j^2 + \sigma_{\varepsilon_j}^2} \right).$$

The reason is that bonus payments raise efforts and concurrently access to decision-facilitating information allows the agent to allocate these efforts more efficiently to the different tasks. Thus, we can summarize:

Proposition 1. *Increasing bonuses and providing information both positively impact performance. Both practices are complements, as the marginal effect of one practice is higher when the respective other practice is used.*

Note that decision-facilitating information can only affect performance if there is some alignment of interest between principal and agent. If there is neither intrinsic alignment through employee identification or implicit incentives ($\theta = 0$) nor performance pay ($\beta = 0$), then decision-facilitating information is useless, as the agent lacks incentives to act on it.

3.2 Hypotheses

The reviewed literature and our stylized model lead to the following hypotheses for our research setting. The first two hypotheses have been studied separately in the empirical literature in accounting and economics to a certain extent before. The third hypothesis is based on our formal model.

Hypothesis 1: Providing information to facilitate decisions increases performance.

As illustrated by the formal model, the information about marginal productivities of the different tasks helps the agent to allocate efforts more efficiently across tasks, provided he has some incentives to use the information. This hypothesis is well in line with the empirical literature in accounting showing that decision-facilitating information through several channels tends to raise performance (frequent information; e.g. Frederickson et al. 1999), performance information (Holderness et al. 2019), and novel information (Casas-Arce et al. 2017a).

Hypothesis 2: Performance pay increases performance.

This hypothesis reflects the standard incentive mechanism illustrated in moral hazard models: performance pay raises the agent's marginal returns of effort and thus increases these efforts. Most empirical literature in both accounting and economics on the causal effects of performance pay supports the idea that performance pay positively influences the agent's performance (Banker et al. 2000, Lazear 2000).[12]

Hypothesis 3: Providing information to facilitate decisions and using performance pay are complements. That is, the impact of introducing performance pay is larger when decision-influencing information is provided and vice versa.

The key rationale for the hypothesis is illustrated in the formal model: when performance pay is in place, managers should have a stronger incentive to exert effort; that is, the conflict of interest between principal and agent is reduced. When decision-facilitating information provides them with more precise information on marginal returns of different tasks, managers can more effectively allocate these efforts across tasks. In turn, the provision of decision-facilitating information should have a stronger effect on performance when the manager's and the firm's interests are aligned to a stronger extent through performance pay.

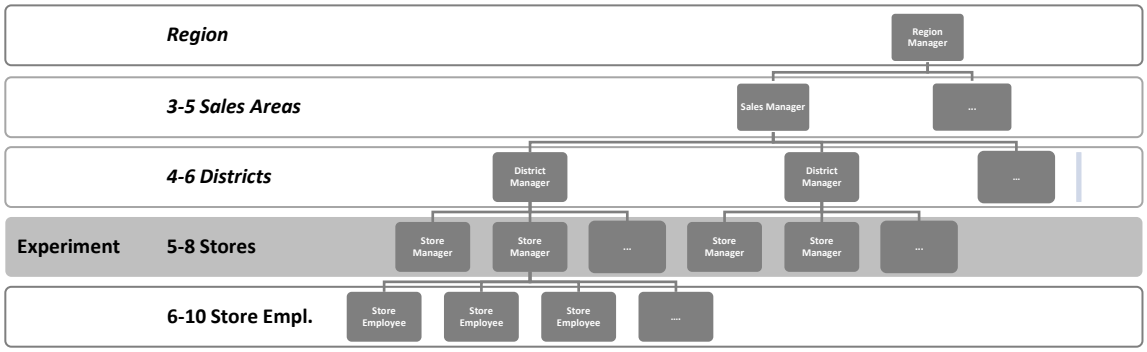
4 The Empirical Setting

The company in our study is a large nationwide discount retailer operating supermarkets in Germany with more than 2,000 stores. The supermarket chain is subdivided into several larger geographical regions that cover Germany and has a rather steep hierarchical structure with relatively small spans of control. The structure of the hierarchy is depicted in Figure 1. Each

[12] Limiting factors are, for instance, the lack of observability of important tasks (Holmström and Milgrom 1991), image concerns and motivation crowding-out (Bénabou and Tirole 2006), or exhausted learning curves (Manthei et al. 2021).

region has a regional top manager and is split into sales areas managed by sales area managers. The sales area managers supervise about 4–6 district managers, and the district managers, in turn, are responsible for 5–8 store managers. The average sales area per store is 695 square meters and store employ on average 6.6 fulltime equivalent employees (FTE). The average tenure of a store manager is 14.18 years.

Figure 1 - Illustrative Organizational Chart



In discount retailing, tasks and processes are typically highly standardized, and store managers only have limited leeway in the store procedures. The central office determines, for example, the store layout, product choices and most of the placements of goods within stores. Store managers' duties are mainly operational tasks, such as handling the presentation of (fresh) products, refill of shelves, cleanliness of stores, and efficient processes within the store (e.g., at the cashier desk). A computer system recommends order quantities based on an algorithm, but managers can overwrite the suggestions using their specific knowledge of local customer demand. They also have some leeway in temporary price reductions and special placements of goods within specific areas of the store. The store managers' main tasks as defined in job description used by the company and a classification of these tasks are shown in Appendix A1.

As illustrated in Figure 1, store managers directly report to district managers. District managers are usually former store managers and manage about six stores per district. Store and district managers receive weekly and monthly electronic performance feedback from the company accounting department. On their computer, the store managers have access to their main key performance indicators (KPIs): sales, number of customers, average sales per customer, personnel hours, personnel costs, overall inventory losses, sales of fresh items, inventory losses of fresh items, availability of items, and a mystery shopping score. Concerning these KPIs, the store managers see the absolute value of the week/month, the development regarding the previous year, the development regarding the planned KPI and the rank within

the region. The same is visible for the accumulated values of the KPI over the year. Thus, the store and district managers receive regular and detailed electronic performance feedback, which also allows district managers to closely monitor the store managers' performance.

Store managers did not receive any performance-dependent monetary bonuses in the past. However, most district managers are former store managers, and approximately 5% of store managers are promoted to become district managers in a given year, which leads to sizable salary increases (gross monthly salaries are about €6000 for district and €3000 for store managers). Hence, even in the absence of performance pay, career concerns most likely generate implicit performance incentives.

Prior to our study, the evaluation of the performance of stores and store managers was mostly based on the components of store profits, such as sales and inventory losses. As explained above, store and district managers obtain these figures weekly and monthly. One of the key conjectures arising from the discussions with the company was that using a broader profit metric should increase the scope for managers to raise performance (as, for instance, suggested in Bouwens and Van Lent 2007).[13] Although store managers were used to analyzing the components of profits due to their regular electronic performance feedback, they never focused on store profits as a combined metric. Moreover, an important issue at the outset was that the procurement prices for the goods sold were not publicly shared, as low procurement prices constitute a central source of competitive advantage in (very price competitive) discount retailing. Since store managers before our intervention did not know the actual margins for different products precisely, their leverage to raise profits were rather limited. Hence, we developed the idea of providing managers with information about profit margins, which constitutes the key element of our decision-facilitating information treatments.

5 The Experiment

From April 2017 to June 2017, we randomly varied whether store managers received decision-facilitating information, performance pay, or the combination of both among the 363 of the firm's stores in one region of Germany. For this, we used a simplified profit metric. This key figure was computed as follows:

Store Profit = sales – costs of goods sold (cogs) – personnel costs – inventory losses.

[13] Moreover, we decided to use the store's planned budget value as a threshold for receiving a bonus and not solely the managers' past performance to reduce possible ratchet effects (as for instance discussed in Bol and Lill 2015, Mahlendorf et al. 2015, Casas-Arce et al. 2018, see also Indjejikian et al. 2014).

Or even more simplified:

Store Profit = gross profit margin – personnel costs – inventory losses

The metric excluded costs that store managers can not affect (such as investment expenditures, store rents, costs of logistics, and overhead costs). Thus, we used one aggregated measure entailing all key elements of profits that store managers can influence to incentivize managers to use their full knowledge and set out possible actions.[14]

In the performance pay treatments, store managers received bonuses based on this key figure. In the information provision treatments, store managers obtained a brush-up concerning the performance metric and received information on the profit margins of all products but no financial incentives.[15]

5.1 Implementation

Overall, we implemented four different treatment groups in a 2 × 2 factorial design.

Table 1 – Treatments

		Decision-Facilitating	
		Information	No Information
Decision-Influencing	Bonus	N = 92	N = 88
	No Bonus	N = 92	N = 91

We used a stratified randomization (Athey and Imbens 2017) procedure depending on the prediction of the districts profits in the first treatment month. To construct the stratification groups, we used one year of past data through January 2017 and then predicted profits for the district in April 2017 with a simple time-series model.[16] Within groups of four with similar predicted values, we randomly assigned the treatments. We randomized at the district level (on average 7.06 stores) to avoid spillover effects and confusion due to possible communication

[14] To reduce personnel costs, store managers can actively manage their staff planning and also the usage of temporary employees and those employees on marginal part time work (in the German tax and transfer system, firms can rather easily employ people on the so-called "minijobs" for a few hours per week earning less than 480€ per month. Store managers have some leeway in employing such minijobbers.) Concerning inventory losses, store managers do have some influence on the ordering of products. They are also responsible for refilling the shelves of, for instance, fruits and vegetables, which also influence their shelf-life and thus inventory losses.

[15] Different to the other KPIs, store managers never receive a ranking (relative performance information) of their store profit within their region.

[16] Unfortunately, we had to randomize three months in advance as the data on profits come with a delay of one month and the central office needed the group composition early to implement the required operational processes.

within districts.[17] Table A2 in the appendix shows summary statistics and balancing of treatment groups.[18]

Store managers in the treatment groups were notified about the respective treatment and the duration of the project, with a personalized letter sent to the address of their private home in the last week of March. The letter contained information about the treatment, which started on April 1st, 2017. Importantly, letters were in the corporate design of the company, signed by the HR responsible and the regional manager and sent from the company's post office. The control group did not receive any notification. District managers were briefed in written form on how to react to questions concerning the experimental design.[19]

To complement the treatments, we also conducted two large online surveys with store and district managers before and after the experiment. We sent personalized letters to their private home address in February 2017 and in the last week of June 2017.[20] With the letter, each manager received an individual code for online registration, allowing us to match each responder to the other data.

Throughout the experiment, neither the district nor the store managers knew that we, as researchers from a university, were involved in this project or that the project was a designed experiment. The only event in which we communicated directly to the managers was the survey. Here, we maintained the managers' anonymity as a research institute. Importantly, managers could not connect the surveys directly with the experiment.

5.2 Treatment BONUS

Managers in this group received bonus payments based on the profit metric explained above. Bonuses were calculated as follows:

$$\text{Bonus (in €)} = [\text{Stores Profit} - (0.8 \cdot \text{Planned value of Stores Profit})] \cdot €0.05$$

Store managers, hence, receive €0.05 for every €1 profit they yield above a threshold of 80% of the planned budget value. The planned budget was determined by the accounting department at the beginning of the year based on a prediction algorithm. Bonuses were accumulated, and

[17] Contamination is a relevant concern in a field experiment. Therefore, it was a key aim in the design to minimize contamination issues. Importantly, essential lines of communication were performed within the same district but store managers hardly communicated (or even know each other) across district boundaries.
[18] We handled the randomization. However, we detected some differences between treatment and control groups. Controlling for these differences in a simple OLS regression induced no notable differences in the treatment effects (see Appendix Table A3). Moreover, differences are time constant and should not affect the fixed effects regressions.
[19] Exemplary letters to store and district managers are provided in the online Appendix.
[20] As surveys were sent out on June 26th, there was an overlap with the experimental period of at most 2 days.

cumulative bonuses were paid out after three months (capped at zero) together with the store managers' salary. Note that it was possible to receive a negative bonus for a month, thereby reducing the amount gained in the bonus months. Also, there were no individual performance bonuses for store managers in this region before.

For each of the three months from April to June 2017, store managers in this treatment group also received a personalized letter sent to the address of their private home.[21] The letter reported the achieved profit and all its components of the previous month and the initially planned value. Moreover, managers received feedback on the bonus for the respective month.

5.3 Treatment INFORMATION

The provision of information to facilitate decisions comprised an online training tool (a video explaining the profit metric and a quiz), information about the profit margin of individual products (which was unavailable to store managers before the intervention), and monthly electronic feedback on the profits of the respective store. The online training tool was a 10-minute online video clip, explaining the different profit components, how to influence them, and how they interact with each other.[22]

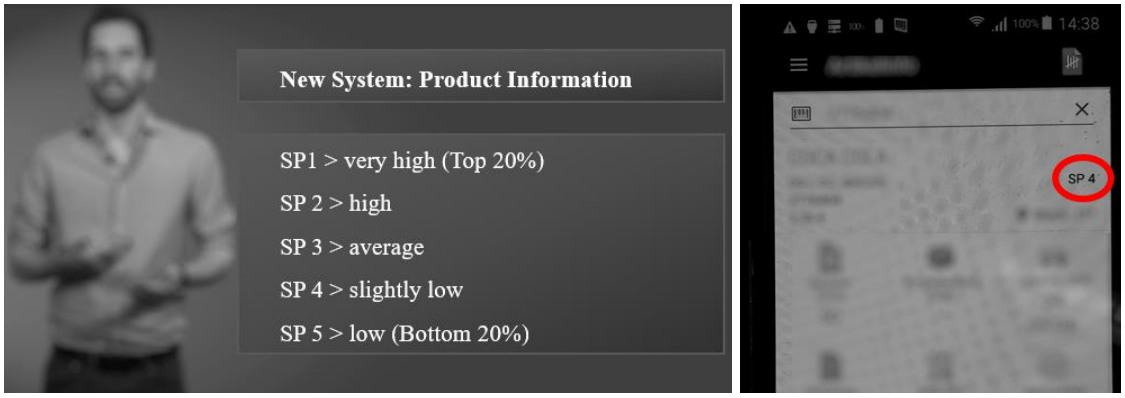
Importantly, the video also explained the novel information managers obtained on profit margins in detail (see Figure 2 for a screenshot). As specified above, the costs of goods sold for specific products are highly confidential in the competitive business of discount food retailing. Hence, the company had never disclosed specific margins before the experiment to store managers. To provide information about margins without giving precise information leakable to competitors, we devised a system classifying all products according to their relative margin on a 5-point scale, where "1" meant that a product belonged to 20% of products with the highest margins and "5" meant that it belonged to the quintile with the lowest margins. The intermediate steps were set accordingly. This margin rating was made accessible to store managers on their portable data terminals (PDT). PDTs are technical devices like smartphones with barcode scanners that are commonly used in retailing to immediately provide all product-related

[21] More precisely, due to a delay in calculating staff costs, profit data were always delayed by one month. Hence, for instance, by the end of May, we sent out the letter with the calculations for April. However, as explained in Section 3, store managers received their weekly and monthly electronic performance feedback from which they could directly infer how changes in their behavior induced changes in the financial KPIs. The letter is provided in the online Appendix.

[22] As one of the authors was the trainer in the video clip and we scripted it, we had full control on the content and the transmission of the video. Store managers were unaware that the trainer was part of the research team. We carefully ensure that it remained a video to transfer and brush-up knowledge and not to motivate employees. A screenshot of the video is displayed in Figure 2. An excerpt of the video script is provided in the online Appendix.

information and allow for quick ordering. Store managers therefore had instant access to the information whenever scanning a product.

Figure 2 – Information on Profit Margins



Note: The left panel shows a screenshot of the video (pixelated and translated from German). The right panel shows the pixelated screen of the portable data terminal, where the margin category was displayed (see circle).

The electronic performance report informed store managers about the achieved profits for a given month and overviewed the components (sales, costs of goods sold, personnel costs and inventory losses) and planned values. Managers could access these reports directly after the beginning of the treatment to inspect the planned values for their store. Moreover, the reports also contained a reminder of the definition of margin categories. As information about margin categories was an essential part of the training video (see the screenshot in Figure 2), for managers who watched the video, it was very much assured that they knew the margin categories. Those who did not watch the video could still inform themselves about the margin categories accessing the performance reports, but this required (slightly) more effort.

The key idea of this information intervention was thus to inform store managers about the store's production function and, with this, facilitate a store manager's decision toward profit increases.

5.4 Treatment BONUS&INFORMATION

This treatment combined individual monetary performance pay and information provision. It was conducted along the lines described above.

6 Results

6.1 Empirical Approach

We estimate our main results on the full sample of managers originally assigned to the treatment (however, excluding managers who switched stores during the treatment time) using a difference-in-difference estimation, including fixed effects for months and stores:

$$(1)\ Y_{s,t} = \beta_0 + \beta_1 \cdot TreatmentBONUS_{s,t} + \beta_2 \cdot TreatmentINFORMATION_{s,t} + \beta_3 \cdot TreatmentBONUS\&INFORMATION_{s,t} + \gamma \cdot X_{s,t} + a_s + \delta_t + \varepsilon_{s,t},$$

where $Y_{s,t}$ is the profit in month *t* for store *s*. $X_{s,t}$ includes time-variant controls, which are the planned budget value of the store's profits and dummy variables indicating an ongoing or past refurbishment of the store. $\varepsilon_{s,t}$ is an idiosyncratic error term clustered at the district level (the store belonged to at the beginning of the experiment). a_s are store fixed effects and δ_t are monthly time fixed effects. In some specifications, we also include district manager and store manager fixed effects. *TreatmentBONUS*, *TreatmentINFORMATION*, and *TreatmentBONUS&INFORMATION* are dummy variables equal to 1 for the respective treatment group during the experimental period and 0 otherwise.

To investigate the interdependency of our interventions more directly, we also use a specification in which we use dummies for the use of the practices and an interaction term for the combined use instead of treatment dummies. For instance:

$$(2)\ Y_{s,t} = \beta_0 + \beta_1 \cdot Bonus_{s,t} + \beta_2 \cdot Information_{s,t} + \beta_3 \cdot Bonus \times Information_{s,t} + \gamma \cdot X_{s,t} + a_s + \delta_t + \varepsilon_{s,t}.$$

In this case, *Bonus* and *Information* are dummy variables equal to 1 in case a store manager received a bonus or information during the experimental period and 0 otherwise. *Bonus* × *Information* is the estimate of the interaction between the bonus and information provision. Thus, an estimate below zero indicates that both practices are substitutes, and an estimate above zero indicates complementarity. Note that we therefore use capital letters to indicate the treatments (which may combine two practices as in the case of *BONUS&INFORMATION*) and lower case letters to indicate the use of a practice.

We use the periods from the beginning of the previous year to the end of the experiment (e.g., January 2016 through June 2017, 18 months) for estimating fixed effects. Table A3 in the Appendix provides robustness checks with ordinary least squares regressions.

6.2 Main Results

The key results are reported in Table 2. Column 1 displays outcomes of a fixed effects model with profits regressed on the treatment dummies, controlling for planned values of the store profit, store refurbishments, and including store and time fixed effects. Column 2 includes fixed effects for district managers and store managers. Columns 3 and 4 use similar specifications, but include dummies for the different practices and an interaction term between these practices rather than treatment dummies.

The first key observation is that providing decision-facilitating information has a sizable average treatment effect on profits, irrespective of whether it is combined with a bonus or not. In fact, the *INFORMATION* treatment raised profits on average by about €1.000–€1.200 (about 2%) per month per store.[23] As the costs of the intervention were very small (costs of shooting the video and minor personnel costs of supplying the information), the intervention was highly beneficial for the firm with an approximate return on investment over the three months of the experiment of roughly 5500% just for the *INFORMATION* group (using the estimates from Table 2, column 2). Hence, store managers productively used the decision-facilitating information, even without performance pay.

Second, while point estimates for the *BONUS* treatment are also positive, they tend to be smaller in magnitude than those for *INFORMATION*. However, they are never statistically distinguishable from the effects of the information intervention (Wald test, $p > 0.1$). Actual bonus payments are sizeable as store managers with performance pay received an average bonus payment of €322.12 per month (SD = 309.74), which is approximately 10% of their monthly salary. In the first month of the experiment, only 3.31% of store managers (12 managers in total) failed to pass the threshold of 80% of the planned budget profit above which increasing performance was rewarded, and after the experiment, only 1.93% did not receive a bonus at all. Above this threshold bonuses varied substantially (see Figure A1 in the Appendix which shows the distribution of bonus payments per treatment group).

[23] Regressions using log gross profits as dependent variable are provided in the Appendix Table A4.

Table 2 – Main Treatment Effects on Gross Profits

	(1) Profits	(2) Profits		(3) Profits	(4) Profits
Treatment BONUS	581.2 (393.3)	1050.2** (448.1)	Bonus	581.2 (393.3)	1050.2** (448.1)
Treatment INFORMATION	998.6** (450.3)	1223.0** (515.0)	Information	998.6** (450.3)	1223.0** (515.0)
Treatment BONUS&INFORMATION	1295.3** (534.0)	1487.5** (604.4)	Bonus × Information	-284.5 (701.1)	-785.8 (788.8)
Planned Profits	0.424*** (0.0488)	0.420*** (0.0488)	Planned Profits	0.424*** (0.0488)	0.420*** (0.0488)
Refurbishment Ongoing	-2767.8*** (596.6)	-2783.4*** (614.8)	Refurbishment Ongoing	-2767.8*** (596.6)	-2783.4*** (614.8)
After Refurbishment	-639.0 (409.8)	-642.5 (431.8)	After Refurbishment	-639.0 (409.8)	-642.5 (431.8)
Time FE	Yes	Yes		Yes	Yes
Store FE	Yes	Yes		Yes	Yes
District Manager FE	No	Yes		No	Yes
Store Manager FE	No	Yes		No	Yes
N of Observations	6472	6296		6472	6296
N of Stores	363	363		363	363
Cluster	56	56		56	56
Within R^2	0.3492	0.3674		0.3492	0.3674
Overall R^2	0.8343	0.7479		0.8343	0.7522

Note: The table reports results from fixed effects regressions with the profits on the store level as the dependent variable. The regression accounts for time and store fixed effects (columns 1–4) and adds fixed effects for district and store managers in columns 2&4. The fixed effects regressions compare pre-treatment observations (January 2016–March 2017) with the observations during the experiment (April 2017 – June 2017). All regressions control for possible refurbishments of a store (dummy variable equals 1 if the shop is currently refurbished, dummy variable equals 1 after the time of refurbishment, and dummy variables are 0 otherwise) and the companies' planned value. Observations were excluded once a store manager switched the store during the treatment period. *Treatment effect* thus refers to the difference-in-difference estimator. Robust standard errors are clustered at the district level of the treatment start and displayed in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$.

The third key result is that the combined intervention *BONUS&INFORMATION* increased profits by about €1200–€1400 per month and store. However, these performance gains do not exceed the effects of the pure information intervention substantially (Wald test, $p > 0.1$).[24]

Importantly, when studying the interplay between performance pay and the information intervention, we find no evidence for the complementarity between both practices that the model predicted. In all specifications, the sum of the point estimates of *INFORMATION* and *BONUS* surpasses the *BONUS&INFORMATION* estimate.[25] A direct test of the Hypothesis 3

[24] When using net profits, that is, when subtracting bonus payouts from gross profits, the dominance of the information intervention becomes all the more obvious (see Table A6).

[25] Using a Wald-test to test the sum of the isolated effects of *INFORMATION* and *BONUS* against the combined treatment effect of *BONUS&INFORMATION* do not yield a statistically significantly difference (Wald test, $p > 0.1$) in any specification. Even the upper bound of the 95% confidence interval of *BONUS&INFORMATION* is with an estimate of 2698.817, only marginally above the sum of *INFORMATION* and *BONUS* (2273.2).

is provided in the specification where we use dummies to indicate the use of practices and an interaction term between the practices rather than a treatment dummies (Columns 3 and 4). The model predicted a strictly positive interaction term. However, the interaction effect of performance pay and information provision is always negative (although not statistically significant), and thus even tends to suggest a substitutional relationship between the two practices – a finding which we explore in more detail below.

When dropping observations of store managers who did not watch the online training video during the treatment period[26], point estimates for the effects of the *INFORMATION* treatment increase and become very similar in magnitude to treatment effects of the combined intervention (see Table A5 in the Appendix).

To summarize our main results, we find empirical support for Hypotheses 1 and 2. Both information to facilitate decisions and using performance pay positively impact performance. Particularly, we show that managers reacted to decision-facilitating information even without performance pay.[27] However, we do not find evidence for Hypothesis 3 that the combined intervention increases profits more than the sum of both single interventions implemented separately. The added value of performance pay appears to be rather small, and point estimates even tend to indicate a substitutional relationship.

In the following section, we first investigate behavioral changes triggered by the treatments. In the next step, we extend our formal model to provide a potential rationale for our finding and then provide further evidence.

6.3 What Did the Store Managers Do to Increase Profits?

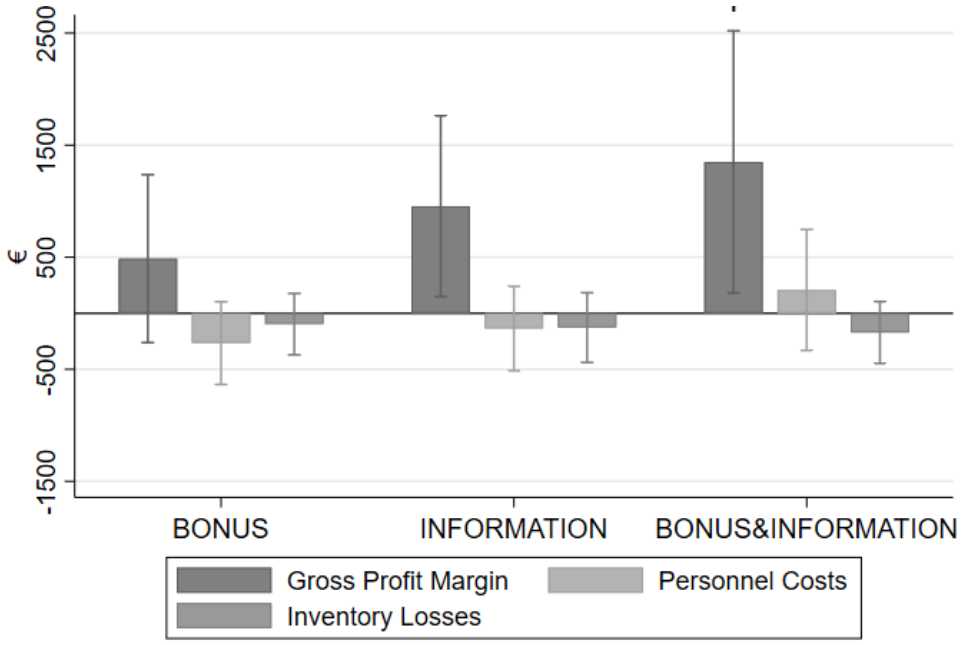
Following, for instance, Ichniowksi and Shaw (2003), Hall (2010), and Ittner (2014), we first analyze the behavioral changes induced by our interventions in more detail. First, we use detailed financial data on the different components of our profit metric (e.g., *gross profit*

[26] For this specification, we set store profit to missing during the treatment period in case a store manager did not watch the training video. We caution that training participation is affected by the treatment and thus endogenous. Hence, the estimates show the profit increases in the group of store managers who were sufficiently motivated to participate in the training. However, these estimates have a causal interpretation (interpreted as the effect of the treatment on the store managers who were sufficiently motivated to watch the video), if we assume that the counterfactual time trends (hypothetical time trends in the absence of the treatment) are uncorrelated with the motivation to participate in the online training.

[27] An alternative interpretation for our results might be that the *INFORMATION* treatment affected the implicit incentives for store managers by communicating the importance of store profits relative to sales as key performance metric tracked by management without generating attention for the different profit margins. However, store profits were not a completely new KPI as store managers were already confronted with the components of store profits in their weekly/monthly reports. Moreover, the *BONUS* treatment should carry the same signal about the importance of store profits with an additional explicit incentive. Thus, incentives should be stronger in the BONUS treatment but, point estimates are nearly always below those from the *INFORMATION* treatment. We explore this topic further in the next section when analyzing differences in managers' behavior.

margin, personnel costs, and inventory losses) to investigate through which channel managers increased profits in the different treatments. To analyze the treatment effects on the different profit components, we conducted separate regressions with each of these components as dependent variables. The results are displayed in Figure 3. As the figure shows, the intervention raised profits mostly through increasing the gross profit margin, particularly in the two treatments in which managers received decision-facilitating information. This supports the hypothesis that providing information helps store managers achieve more profitable sales.

Figure 3 – Treatment Effects on Profit Components



Note: The figure displays the treatment effects from three separate regressions. The regressions dependent variable is either the gross profit margin, personnel costs, or inventory losses. The remaining specification is the same as in our main table (Table 2, Column 2). 90% confidence bands are displayed.

While the growth in the gross margin is larger in the combined treatment as compared to the *INFORMATION* treatment, the difference is not too large and not statistically significant on any conventional level. Hence, managers made substantial use this information, even without formal incentives, indicating that implicit incentives were sufficiently strong. In the *BONUS* treatment, where managers lacked explicit information on the relative profitability of different products, profit growth is driven rather by a combination of the different profit components, and the share of the profit increases achieved through higher gross margins is comparably low. In fact, while in the *BONUS* treatment, the gross profit margin effect is only about 46% of the

magnitude of the overall profit effect, this ratio is 78% in the *INFORMATION* condition and 91% in *BONUS&INFORMATION*, respectively.[28]

We also use data on sales and the number of products sold for each of the five margin categories displayed to store managers in the two treatments with decision-facilitating information. While we cautioned that statistical power is limited here, regressing sales and the number of products sold in the different margin categories separately on the treatment dummies indicates that sales grow predominantly in the top and middle but not the bottom margin categories in the *INFORMATION* and *BONUS&INFORMATION* treatments (see Table A7 in the Appendix). We do not find any evidence that store managers in *BONUS managed to* increase profits substantially in these categories.

In the next step, we investigate which actions the managers actually undertook to raise profits by their own account using responses to a post-experimental questionnaire. We invited store managers to participate in an online questionnaire close to the end of the experiment (participation rate 56.20%). The questionnaire, for instance, included open questions asking store managers what they had actually done to increase profits in the previous months. We used a task classification developed and applied by the firm for formal job descriptions of store managers (Table A2), and two research assistants independently mapped the statements to this task classification.[29]

We displayed the results categorizing these tasks into the seven general task dimensions used by the firm.[30] Figure 4 shows the fraction of stores for each task dimension in which at least one of the RAs assigned a statement to a specific task in the respective dimension.[31] A first observation is that the placements of goods is the most important dimension store managers mention when asked about activities implemented to raise profits. Frequently, store managers stated that they made secondary placements of high-margin products (products typically have specific locations in the store, but store managers can also display products on a second prominent spot, for instance, on a specific desk close to the cash desk). Exemplary statements of store managers in the survey are as follows:

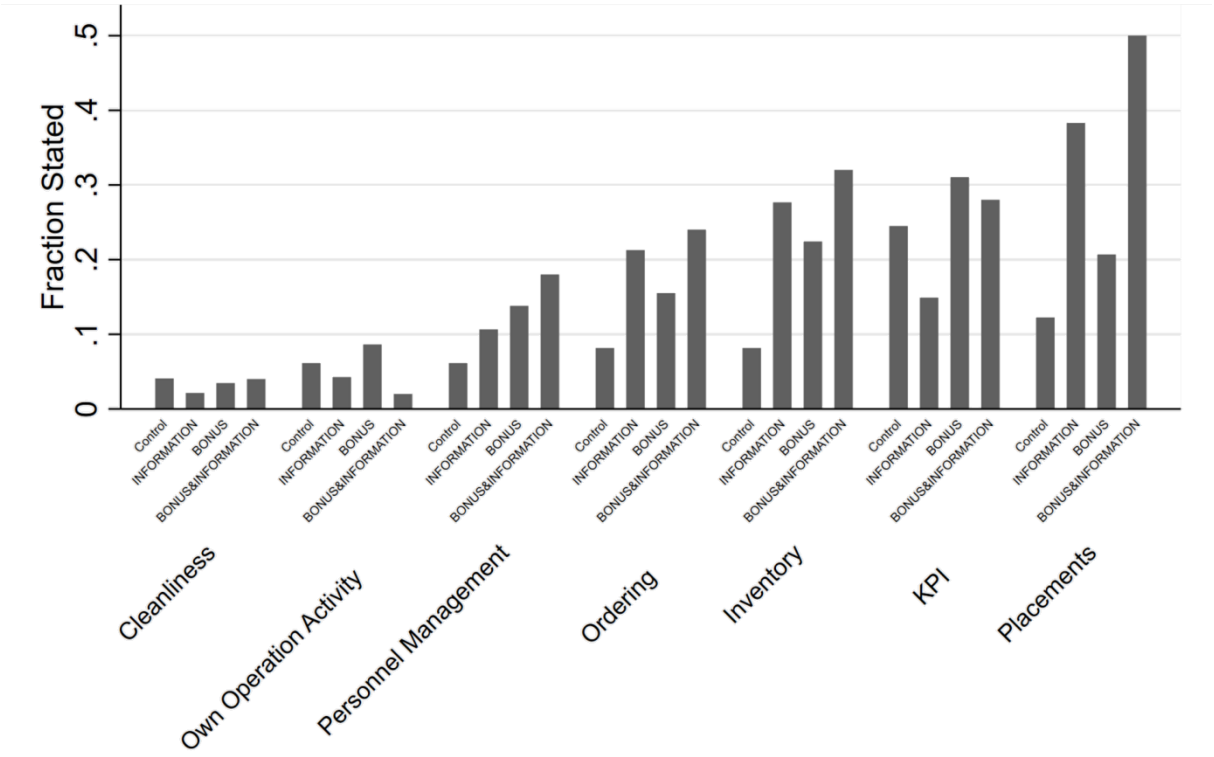
- *"I tried to prominently place articles with a high-margin category (SP1 or SP2). Furthermore, I pushed sales of bakery products with secondary placements."*

[28] Note that the estimates are not directly additive as the respective models estimate different time trends and store fixed effects when different outcome variables are used.
[29] The average Cohen's Kappa is 0.64, and can thus be interpreted as substantial (McHugh 2012).
[30] Importantly, the research assistants could categorize all statements into the task classification used by the firm. Figure A2 in the appendix shows the more detailed split into finder subclasses of tasks.
[31] This procedure prevents possible subjective opinions when classifying the statements. While the specific task might leave room for interpretation, the task dimension should reduce this. Figure A4 in the Appendix illustrates the results of a keyword analysis (counting of the most relevant keywords) and supports the classification done by our RAs.

- *"Paid attention to the margins of different articles and consequently made secondary placements."*
- *"Secondary placements in front of the cash desk. More focus on ordering meat and bread."*
- *"Worked with the product margins and secondary placements of high-margin products"*

Notably, placements stand out only in the treatments with additional information on product margins. In fact, 38% of the survey respondents in the *INFORMATION* and 50% in the *INFORMATION&BONUS* groups mentioned a placement activity, while placements were only mentioned by 21% of respondents in the *BONUS* group (see also the regressions displayed in the Appendix Table A8). The same picture arises when we include only statements that explicitly mention the placement of high margin products (see Figure A3 in the Appendix). We observe a similar pattern for activities related to product ordering. Hence, managers reacted to the novel information on profit margins and did so in particular through ordering and placement of high-margin products. Moreover, even without a bonus, managers in the *INFORMATION* treatment reported a sizable number of activities undertaken in these categories.

Figure 4 – Task Focus to Increase Profits (Open Questions)



Note: The figure displays the fraction of stated task dimensions to increase profits obtained from open questions of an ex-post questionnaire. A task dimension counts as soon as one of the underlying tasks is mentioned and identified by at least one research assistant. N = 204.

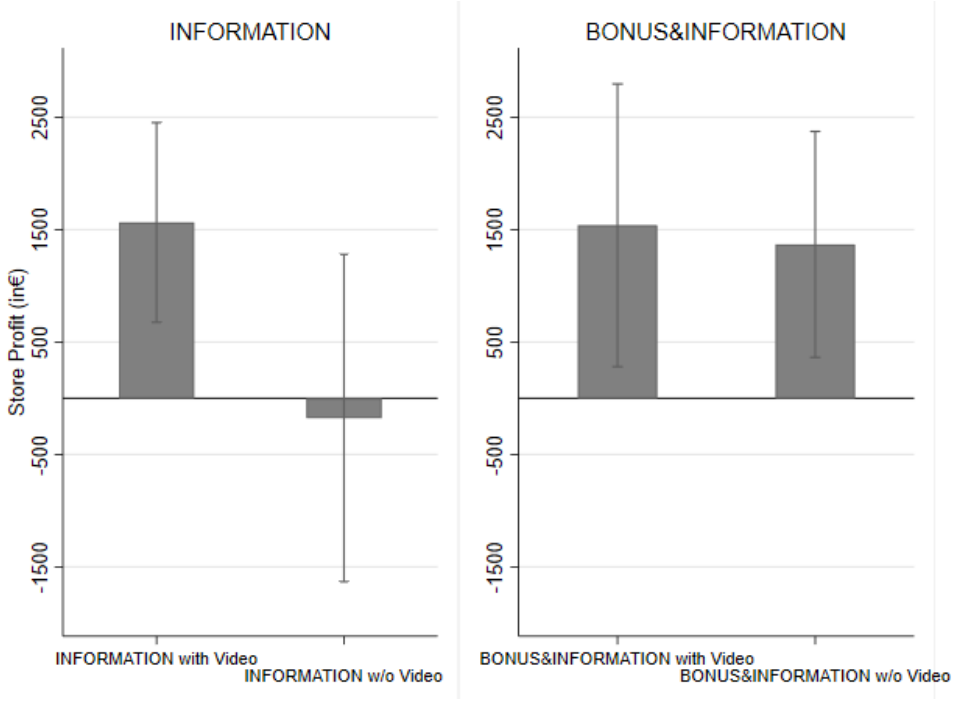
Interestingly, managers in the control group also stated some activities to increase profits, indicating that thinking in terms of profit increases was not completely new to the managers and also part of their daily routine. However, the three treatments have clearly raised the awareness of managers about driving profits through different activities.

It is also interesting to consider the activities managers undertook in the *BONUS* group (relative to the control group). Here, if anything, the survey data indicates shifts in the focus on personnel management and inventory losses, which agrees with the above findings that managers focused considerably on more easily manageable profit components without information on profit margins.

Finally, we can also make use of the fact that we are able to track whether store managers actually watched the training video in the treatments *INFORMATION* and *BONUS&INFORMATION*. The video's purpose was to explain the profit metric, and in particular, the decision-facilitating information on profit margins in detail. As specified in the above, in both treatments, managers could also inform themselves about the margin categories, for instance, accessing the performance reports, but it required more effort.

Interestingly, we find a treatment difference in the likelihood that managers actually watched the video. While 80.43% of the store managers watched this video in the *INFORMATION* treatment, only 67.74% of those in the *BONUS&INFORMATION* treatment did so (MWU, $p = 0.0495$). This finding seems to indicate that the two practices to some extent "competed" for the store managers' attention. When being informed about the practices, managers in *INFORMATION* learned about the online training. Managers in the *BONUS&INFORMATION* condition were also informed about the bonus, and thus the online training was only one part of the overall package of two practices implemented. As attention is limited in capacity (Simon 1949, Kahnemann 1973), the broader package may have diverted attention from accessing the video.

Figure 5 – Treatment Effects by Video Training Participation



Note: The figure displays treatment effects from a fixed effects regression, with the profits on the store level as the dependent variable. The regression follows our main specification (Table 2), including store, time, store manager and district manager fixed effects. Treatment dummies are included for the *BONUS* group, the *INFORMATION* group who watched the video, the *INFORMATION* group who did not watch the video, the BONUS&INFORMATION group who watched the video, and the BONUS&INFORMATION group who did not watch the video. 90% confidence bands are displayed.

Furthermore, it is interesting to explore whether and how performance effects differ between those store managers who watched the video and those who did not. Figure 5 displays the respective treatment effects split by training participation. In the *INFORMATION* group, the

treatment effects are fully driven by the store managers who watched the video. In *BONUS&INFORMATION*, treatment effects are very similar irrespective of whether the store managers watched the video or not. This picture lends further support to the role of attention: Hence, in the INFORMATION treatment, the video apparently created attention for the profit margins—and only then profits increased. In the BONUS&INFORMATION, attention was created by either the bonus or the video. This pattern suggests that both are substitutes for generating attention for the objective of the intervention—a mechanism we explore in more detail in the next section.

7 The Role of Attention and the Timing of Treatment Effects

Our key hypothesis at the outset was that providing decision-facilitating information and using performance pay are complements. In the formal framework presented above, we have shown that a straightforward incorporation of decision-facilitating information into a standard multitasking principal agent model induces this prediction.

The results on the access to the training video and the differential treatment effects conditional on watching this video have suggested a potential countervailing effect, namely that the two practices are substitutes in guiding *attention* toward the aim to focus on profits. To explore this more extensively and investigate whether an attention-directing role of management practices can rationalize our main findings, we incorporate the role of attention in our formal framework and then explore further data on self-reported attention on raising profits and on the timing of the treatment effects and the interdependency between the two practices.

7.1 Modeling the Role of Attention

The key idea of the extension presented in the following is that both interventions generate *attention* for the underlying profit metric. The model incorporates several key characteristics stressed in the literature (see e.g. Simon (1949, pp. 54), Kahneman (1973), or Birnberg and Shields (1984), who provide a review on the psychology of attention from an accounting perspective) into our basic framework: First, attention is not only guided by voluntary choices but also involuntarily through specific stimuli. Second, individuals have a *limited capacity* for attention (Libby and Trotman 1993, Ocasio 1997, Hirshleifer and Teoh 2003). Third, without

further stimuli, attention tends to *fade over time* due to forgetting or interference of other events.[32]

Note that basic agency models, such as the one we explained above, can be understood as modeling the voluntary allocation of attention to different tasks through a volitional process to provide effort to achieve an objective. The extension we develop here aims to capture automatic shifts in attention triggered by the incidence that a novel practice is implemented. As shifts in attention are naturally a dynamic phenomenon, we now consider a dynamic version of the model in which the agent works over $t = 1,..T$ periods. In period 1, the respective practices are implemented and then remain in place. Hence, the material incentives and access to information remain constant. Attention generated through the new practices may change over time.

In the above, we assumed that the agent is motivated not only by the bonus but also through implicit incentives. Indeed, Our empirical results have shown that store managers productively use decision-facilitating information, even without performance bonuses. We now assume that the strength of these implicit incentives is determined by the level of attention $a \in [0,1]$ for the key figure stimulated through the novel implementation of the practices. Furthermore, we assume that the level of attention a for the profit metric in period t is determined by

$$a = \eta^{t-1} \cdot A(I, \beta)$$

with $0 < \eta < 1$ such that attention is highest in period 1 and then becomes lower over time.[33] The function $A(I, \beta)$ reflects the guidance of attention induced by the introduction of the two considered practices. We assume that providing information raises attention for the profit metric such that $A(1, \beta) \geq A(0, \beta)$. Moreover, attention is also increasing in the size of the bonus. As attention is limited in capacity, bonus increases have decreasing returns such that

$$\frac{\partial A(I,\beta)}{\partial \beta} > 0, \frac{\partial^2 A(I,\beta)}{\partial \beta^2} \leq 0 \text{ and } \frac{\partial A(1,\beta)}{\partial \beta} < \frac{\partial A(0,\beta)}{\partial \beta}.$$

That is, when attention for the key figure is created through decision-facilitating information, the bonus has a weaker additional effect on attention and vice-versa. An example is the following functional form:

[32] Madsen and Niessner (2019), for instance, showed that advertising triggers investor attention, which then fades over time. A number of studies have shown that the effect of nudges and incentives for specific types of behavior such as exercising, quitting smoking, academic performance or job choices can decay over time (Charness and Gneezy 2009, Giné et al. 2010, Levitt et al. 2016, Coffman et al. 2017). Relatedly, Gneezy and List (2006) or Sliwka and Werner (2018), for instance, showed that wage increases trigger higher efforts which then fade over time. See Rubin and Wenzel (1996) for a meta study on time patterns in forgetting.

[33] Note that the functional form corresponding to that used in the Nerlove and Arrow (1962) framework is often applied in Marketing and Economics to formalize the decay of customer attention over time.

$$A(I,\beta) = \begin{cases} \beta^{\lambda} & if \quad I = 0 \\ 1 & if \quad I = 1 \end{cases}$$

with $\lambda \in]0,1[$ such that $I = 1$ generates full attention and otherwise attention is increasing in β. Note that when λ is close to zero, even small bonuses generate close to full attention.

Hence, the agent's objective function becomes:

$$h\beta\pi + \theta\eta^{t-1}A(I,\beta)\pi - \sum_{j=1}^{k}\frac{1}{2}e_j^2.$$

We can now proceed as in the above and obtain the expected profits

$$\Pi(I,\beta) = \left(h\beta + \theta\eta^{t-1}A(I,\beta)\right) \cdot \sum_{j=1}^{k}\left(m_j^2 + \frac{\sigma_j^4}{\sigma_j^2 + (1-I)\cdot\sigma_{\varepsilon_j}^2}\right).$$

Attention guidance thus reinforces both the direct effects of the bonus and the provided information as $\frac{\partial A(I,\beta)}{\partial \beta} > 0$ and $A(1,\beta) > A(0,\beta)$. However, attention effects weaken or even reverse the complementarity between the two practices. To see that, consider how information provision affects the marginal effect of a higher bonus:

$$\frac{\partial \Pi(I,\beta)}{\partial \beta} = \underbrace{\left(h + \theta\eta^{t-1}\frac{\partial A(I,\beta)}{\partial \beta}\right)}_{\textit{lower when } I=1} \underbrace{\left(\sum_{j=1}^{k}\left(m_j^2 + \frac{\sigma_j^4}{\sigma_j^2 + (1-I)\cdot\sigma_{\varepsilon_j}^2}\right)\right)}_{\textit{higher when } I=1}.$$

Hence, attention naturally creates a substitution effect, which dampens the complementarity. If this attention effect is sufficiently strong (for instance of θ is sufficiently large), then the two practices are substitutes rather than complements.[34] The reason for the substitution effect is simple: When both practices trigger awareness of the performance objective, they serve a similar role in guiding attention limited by capacity constraints and thus become substitutes. The model shows that this effect naturally counteracts the technological complementarity driven by standard incentive considerations.

Still, the nature of the interdependency between both practices shifts over time when attention effects fade. In fact, if t is sufficiently large,

[34] To illustrate this result, consider the parametric example of the attention function described above. Note that with this functional form we have that

$$\frac{\partial \Pi(1,\beta)}{\partial \beta} - \frac{\partial \Pi(0,\beta)}{\partial \beta} = h \cdot \sum_{j=1}^{k}\sigma_j^2\left(1 - \frac{\sigma_j^2}{\sigma_j^2 + \sigma_{\varepsilon_j}^2}\right) - \theta\lambda\beta^{\lambda-1} \cdot \sum_{j=1}^{k}\left(m_j^2 + \frac{\sigma_j^4}{\sigma_j^2 + \sigma_{\varepsilon_j}^2}\right),$$

which is strictly negative if implicit incentives θ are not too weak.

$$\frac{\partial \Pi_t(1,\beta)}{\partial \beta} - \frac{\partial \Pi_t(0,\beta)}{\partial \beta}$$

will always be positive, and both practices will again be complements, as in the basic framework based on standard agency theory. In other words, the more time has expired after introducing the practices, the stronger should be their complementarity. We can summarize these considerations:

Proposition 2. *When the introduction of performance pay and the provision of decision-facilitating information both guide attention, then*

(i) *the direct effects on profits are reinforced in the short term;*

(ii) *the complementarity between both practices is weakened or even reversed in early periods such that both may become substitutes; and*

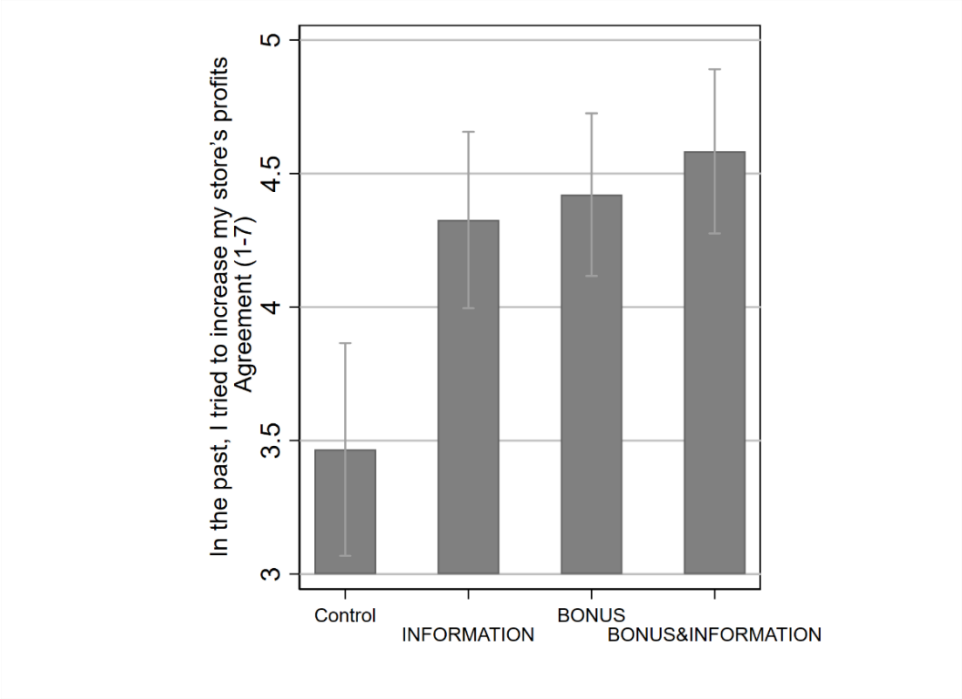
(iii) *the complementarity becomes stronger over time.*

In the following, we explore these implications by analyzing further data from our experimental setting on the timing of the treatment effects.

7.2 Self-reported Attention

Our questionnaire included an item asking store managers about their own perceived intensity of activities for increasing profits. The specific item reads, "*In the past, I tried to increase my store's profits.*" and store managers responded on a scale from 1 (not agree) to 6 (fully agree). The means of this variable for the different treatment groups are displayed in Figure 6.

Figure 6 – Attention on Profit Increases



Note: The figure displays mean agreement (on a scale from 1 = not agree at all to 7 = completely agree) to the statement "In the past, I tried to increase my store's profits.". N = 204. Error bars are displayed.

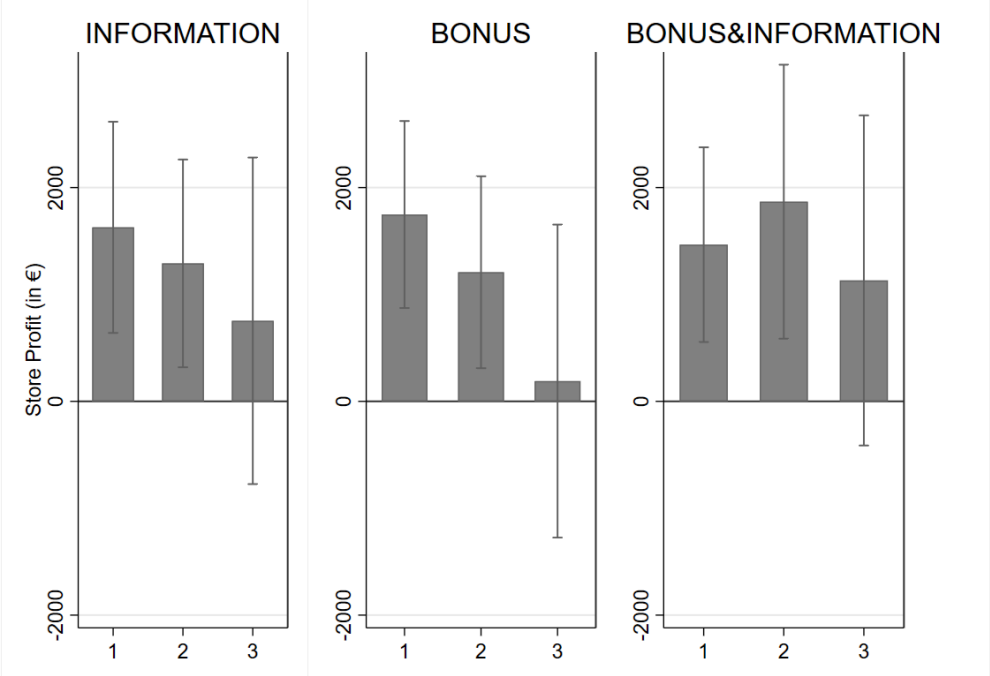
Figure 5 shows that all three interventions thus created substantial self-reported attention for increasing store profits as all treatment effects highly significantly differ from the control group (MWU, all $p < 0.01$). In line with the above reasoning, however, the treatment effects do not vary substantially among the three treatments. In other words, although the focus on store profits is arguably the highest in the *BONUS&INFORMATION* treatment, attention only slightly and insignificantly exceeds attention created in both the mere *BONUS* and the mere *INFORMATION* treatments. Thus, the pattern supports the view that the treatments were substitutes rather than complements in generating attention for the aim to increase profits, which may have induced a countervailing effect, reducing the complementarity between bonuses and decision-facilitating information.

7.3 Timing of the Treatment Effects

To analyze further implications in our data, we now explore how the treatment effects develop over time. For this purpose, we interact dummies for treatment months with treatment groups.

Figure 7 displays the respective regression coefficients (The regression estimates are shown in Table A9 in the Appendix).

Figure 7 – Treatment Effects Over Time



Note: The figure displays treatment effects from a fixed-effects regression with the profits on the store level as the dependent variable. The regression accounts for time and store fixed effects and fixed effects for district and store managers (the specification is the same as in our main table, Table 2 Column 2). Dummies for the different treatment groups were included separately for the three different months of the experiment. 90% confidence bands are displayed.

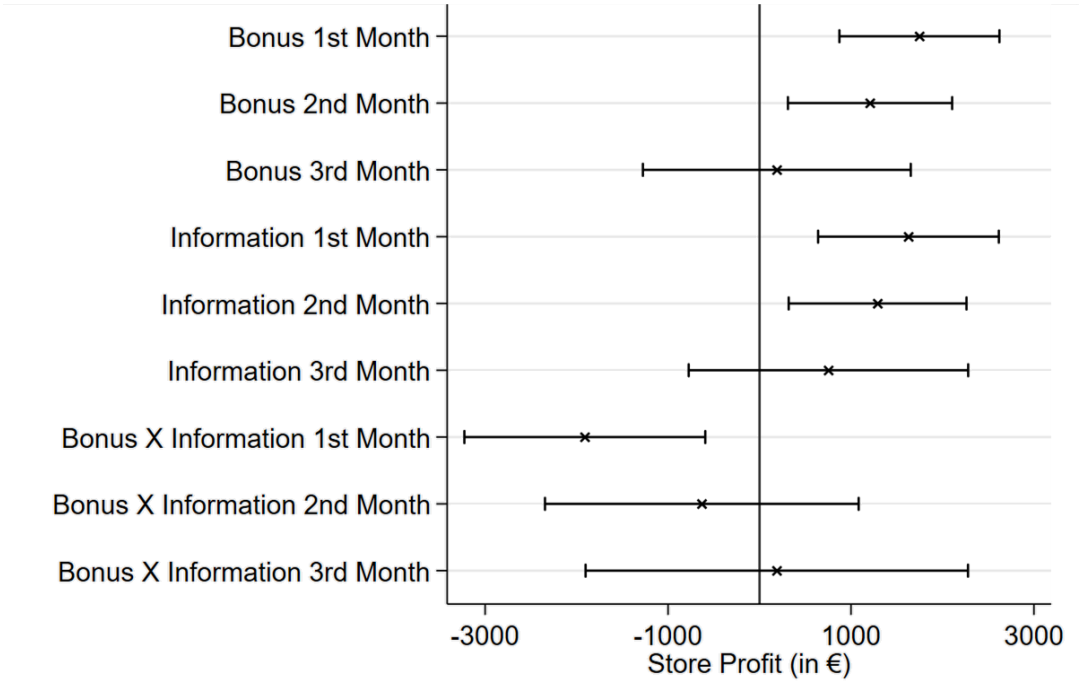
The treatment effects of *BONUS* and *INFORMATION* thus decrease over the three months period.[35] For the *INFORMATION* treatment, a short-term increase in attention might have been expected as managers' awareness of the profit metric and its implications may naturally fade the longer the time passed since the initial intervention. For the performance pay treatment, the effect seems more surprising, as marginal incentives to raise profits in month 1 of our treatment were the same as in months 2 and 3. Still treatment effects became weaker over time, supporting the view that introducing the bonus triggered attention as well.

However, note that the combined intervention appears to have stabilized the performance gains. To investigate the interdependency between performance pay and information provision over time in more detail, we again now switch our main independent variables from estimates

[35] While in the last month of the experiment the treatment effect in *INFORMATION* is not statistically significantly different from the first treatment month (Wald test, p = 0.3252, Table A9 column 2), the *BONUS* treatment shows a significant decline in the effect size between the first and the third treatment month (Wald test, p = 0.0926, Table A7 column 2).

of the treatment effects to estimates of using the practices as well as their interaction (as in column 3&4 of Table 2). We thus include dummies equal to one if store managers received a bonus or received the additional information in the respective period and an interaction term, which is 1 if both practices are implemented in that period. This interaction effect then measures the size of the complementarity/substitution effect. Figure 8 shows the respective estimates (with the corresponding regression output reported in Appendix A10).

Figure 8 – Interdependency Between the Practices Over Time



Note: The figure displays treatment effects from a fixed effects regression with the profits on the store level as the dependent variable. The regression accounts for time and store fixed effects, and fixed effects for district and store managers (the specification is the same as in our main table, Table 2 Column 4). Dummies for the different practices (Bonus and Information) were included separately for the three different months of the experiment. 90% confidence bands are displayed.

The effects of implementing the single practices reflect those displayed in Figure 3 and are monotonically decreasing over time. Importantly, the nature of the interdependency between the two practices also changes over time. In the first period, there is a sizeable and significant substitution effect. The more time has elapsed, the weaker this substitution effect becomes, and the two practices move closer to being complements.

Hence, the timing of the treatment effects supports the view that attention effects play an important role in guiding behavior in our setting. Both practices have strong initial effects on performance, but these effects become weaker over time. Concurrently, their interdependence

moves in the opposite direction. This pattern is well in line with the extended formal model, which has illustrated that attention effects naturally generate substitution effects counteracting complementarities between management control practices. But our results show that the counteracting force becomes weaker over time.

8 Conclusion

We report a firm-level field experiment to study and compare the performance effects of providing decision-facilitating information and implementing performance pay as well as their interaction. In particular, we studied the hypotheses that: (i) the provision of decision-facilitating information raises profits, (ii) using decision-influencing information by introducing performance pay raises profits, and (iii) both practices are complements. To do so, we implemented a field experiment with a 2 × 2 factorial design in a retail chain.

Investigating the average treatment effects, we provide evidence in the same field setting for the importance of both providing decision-facilitating information and using bonuses to influence decisions. Both interventions substantially increase profits. Notably, point estimates of the impact of the information provision treatment exceed those for the performance pay treatment and the intervention came at much lower costs.

Exploring survey data, we studied the underlying behavioral mechanisms in more detail. In particular, we show using survey data that providing information led managers to focus their efforts much more on the placement of (higher margin) products—and this occurred irrespective of whether store managers received performance pay or not. Following this observation, analyzing more detailed financial data showed that providing decision-facilitating information raised profits, mostly through increasing the gross profit margin. Hence, we provide empirical support that our intervention indeed facilitated managers' decisions.

In contrast to our ex-ante hypothesis based on standard agency considerations, we found no overall complementarity between the two practices. Moreover, the effects of the separate interventions (performance pay or information provision) were particularly strong in the first month and then decreased over time. At the same time, the interdependency between the two practices moved in the opposite direction, stabilizing the performance effects. As we have shown, these patterns are well in line with a formal model that has incorporated key insights from the literature on attention into the basic agency framework used at the outset: When different management controls are used to foster the same objective, they are substitutes in guiding attention toward this objective. This effect thus naturally counteracts potential

complementarities. However, as attention fades, so does the substitution effect. In turn, the system of practices that provides both decision-facilitating information and performance pay tends to generate the most persistent performance increases.

These results have several implications for the design of management practices. First, merely providing better information to employees on the relative profitability of specific tasks can be highly profitable. Second, this can work even in the absence of explicit incentives. Employees productively use the provided information, even when this generates no direct monetary payoffs for themselves. Third, however, these effects tend to vanish over time. That is, introducing management control practices can entail substantial attention-directing effects that decline in the longer term. Our results also indicate that performance pay, while being prone to vanishing attention effects itself, may stabilize the fading of attention for the provided information.

Our results thus show that introducing new management practices influences behavior not only directly through the intended channel. A new practice will also create attention for the underlying purpose of its introduction. That is, when a firm establishes a new management practice to achieve a specific objective, some part of the induced behavioral effect is driven by generating salient attention for this objective.

As attention effects are often short term, it is thus a key challenge in the design of management practices to counteract the fading of attention. The literature on attention suggests potential remedies, such as reminders that trigger new stimuli guiding attention. For instance, it is conceivable that the redesign of a management control can have value in itself—as it may renew awareness for the underlying objective. It will be an important topic for further research to evaluate instruments for renewing managers' attention to crucial performance objectives over longer time frames.

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10 Appendix

10.1 Additional Tables and Figures

Table A1 - Classification of Store Manager Tasks

Task	Classification
Ordering of fruits and vegetables, plants	Ordering
Ordering of baked goods	
Ordering of meat	
Additional Ordering	
Baking of bakery articles	
Preparation of secondary placements	Placements
Presentation and maintenance of special-offer tables (Non-Food/ Food/ end of aisle)	
Maintaining product positioning plans	
Quality checks fruits, vegetables, and plants	Cleanliness
Cleanliness of the baked goods stations	
Preservation and maintenance of the condition of the furnishings and the inventory (e.g., shelves, bumpers, freezers, cash desks)	
Guaranteeing the cleanliness and orderliness inside and outside the store	
Analysis of Spoilage	KPI
Analysis of Sales	
Analysis of Personnel Costs	
Analysis of Hourly Output	
Analysis of Inventory	
Checking the minimum durability date (meat, dairy, convenience)	Inventory
Process left overs	
Stocking of goods and maintenance of shelves (colonial goods, frozen goods, load)	
Incoming goods inspection	
Security of goods	
Working on gap listing and inventory care	
Training of cashier employees	Personnel Management
Appraisal interviews / leadership	
Staff planning	
Communication with customers and processing of customer requests	Own Effort
Own cashier work	
(Temporary price reductions)	

Table A2 – Balancing Table

	(1) Descriptives Overall	(2) Descriptives Control	(3) Descriptives Information	(4) Descriptives Bonus	(5) Descriptives Bonus&Information
Profits Jan-Mar '17	34244.12	32535.37	33261.79	35116.27	36102.65
	(14444.84)	(13805.71)	(12525.05)	(15051.03)	(16141.77)
Planned Profits Jan-Mar '17	34437.85	32880.53	33586.91	35642.64	35690.4
	(13635.98)	(12873.05)	(11890.28)	(15244.92)	(14363.22)
Female Store Manager (Y/N)	0.68	0.76	0.65	0.60**	0.72
	(0.47)	(0.43)	(0.48)	(0.49)	(0.45)
Walking Customers (Y/N)	0.12	0.10	0.18	0.15	0.05
	(0.33)	(0.30)	(0.39)	(0.36)	(0.23)
FTE	6.63	6.45	6.69	6.84*	6.55
	(1.38)	(1.17)	(1.39)	(1.58)	(1.33)
Age of Store	16.37	17.63	16.57	17.44	13.89**
	(9.75)	(10.47)	(1.39)	(10.10)	(8.11)
Age Store Manager	43.15	44.57	43.52	41.15**	43.25
	(10.84)	(10.05)	(10.55)	(10.79)	(10.99)
Tenure Store Manager	14.18	15.51	14.23	13.01**	13.96
	(8.44)	(8.43)	(8.64)	(7.73)	(8.82)
Store Space	695.89	701.70	679.03	693.33	709.45
	(134.09)	(112.95)	(143.24)	(121.67)	(154.03)
Observations	363	91	92	88	92

Note: The table reports means of the respective variables for the different treatment groups and their standard deviations in parentheses. Asterisks display significance levels from t-tests (fisher exact test for binary variables) of the respective treatment group against the control group. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$.

Table A3 – Regressions only using Treatment Period

	(1) OLS	(2) log OLS	(3) OLS	(4) log OLS
	Profits	Profits	Profits	Profits
Treatment BONUS	426.3	0.0148	544.9	0.0209*
	(356.1)	(0.0102)	(374.5)	(0.0107)
Treatment INFORMATION	829.9**	0.0162	846.4**	0.0308***
	(352.5)	(0.0110)	(347.3)	(0.0108)
Treatment BONUS&INFORMATION	1082.8***	0.0236**	939.5**	0.0255**
	(381.7)	(0.0106)	(413.4)	(0.0106)
Planned Profits	0.122	0.105	0.142	0.714***
	(0.0922)	(0.0849)	(0.105)	(0.0657)
Refurbishment Ongoing	-4249.0***	-0.123***	-3740.6***	0.00833
	(1008.0)	(0.0331)	(999.5)	(0.0344)
After Refurbishment	-260.0	-0.00927	-64.54	0.0104
	(367.2)	(0.0104)	(389.8)	(0.0124)
Time FE	Yes	Yes	Yes	Yes
Store FE	No	No	No	No
Further Controls	No	No	Yes	Yes
N Observations	1086	1086	1068	1068
N Stores	363	363	356	356
N Cluster	56	56	56	56
Within R^2				
Overall R^2	0.9259	0.9170	0.9273	0.9079

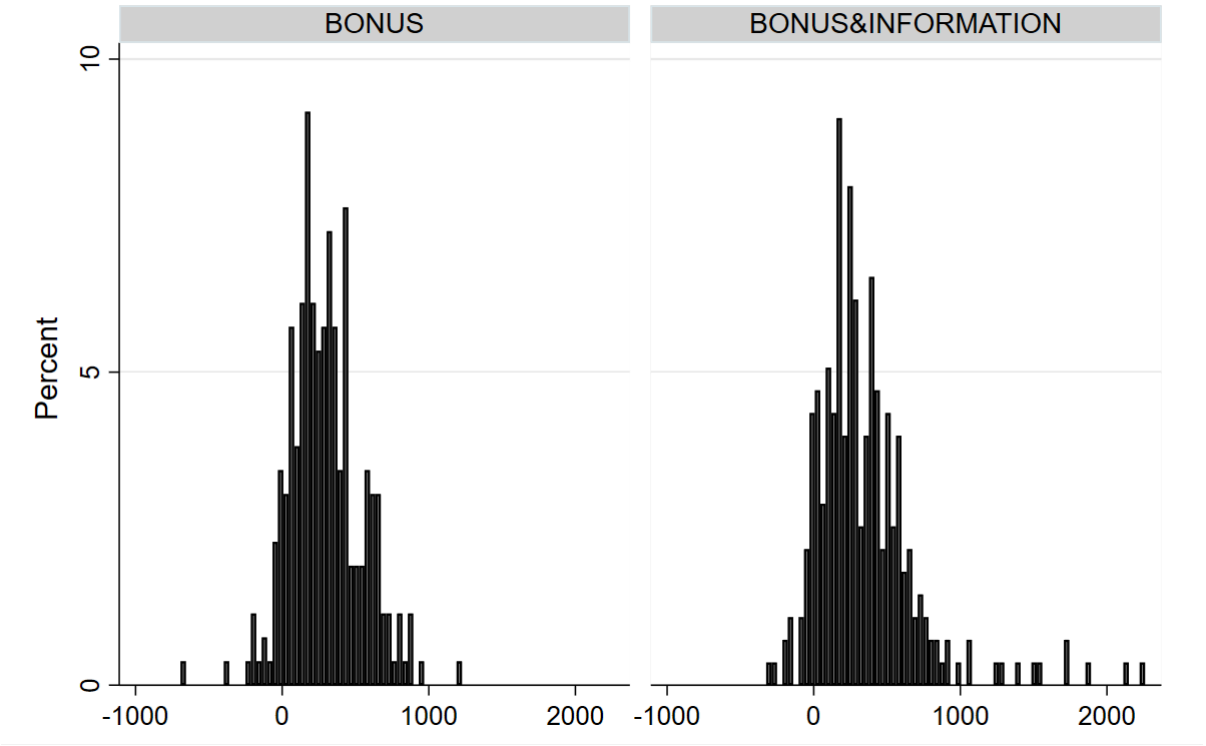
Note: The table reports results from ordinary least squares estimations with profits at the store level as the dependent variable in columns 1&3 and the log value in columns 2&4. Regressions control for the mean of profits from January 2016– March 2017 and the randomization pair. All regressions further control for possible refurbishments of a store and the companies planed profits. Columns 3&4 further control for variables with slight imbalance between treatments (gender, FTE, age of the store, age of the store manager, tenure of the store manager). Observations were excluded once a store manager switched the store during the treatment period. Robust standard errors are clustered at the district level at the start of the experiment and displayed in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$.

Table A4 – Main Treatment Effects on Log Gross Profits

	(1) Profits	(2) Profits		(3) Profits	(4) Profits
Treatment BONUS	0.0148	0.0276**	Bonus	0.0148	0.0276**
	(0.0115)	(0.0126)		(0.0115)	(0.0126)
Treatment INFORMATION	0.0173	0.0231	Information	0.0173	0.0231
	(0.0143)	(0.0162)		(0.0143)	(0.0162)
Treatment BONUS&INFORMATION	0.0257*	0.0295*	Bonus × Information	-0.00637	-0.0212
	(0.0137)	(0.0161)		(0.0190)	(0.0214)
Planned Profits	0.349***	0.387***	Planned Profits	0.349***	0.387***
	(0.0790)	(0.0789)		(0.0790)	(0.0789)
Refurbishment Ongoing	-0.0783***	-0.0734***	Refurbishment Ongoing	-0.0783***	-0.0734***
	(0.0185)	(0.0191)		(0.0185)	(0.0191)
After Refurbishment	-0.0190*	-0.0194*	After Refurbishment	-0.0190*	-0.0194*
	(0.00983)	(0.0101)		(0.00983)	(0.0101)
Time FE	Yes	Yes		Yes	Yes
Store FE	Yes	Yes		Yes	Yes
District Manager FE	No	Yes		No	Yes
Store Manager FE	No	Yes		No	Yes
N of Observations	6470	6296		6470	6296
N of Stores	363	363		363	363
Cluster	56	56		56	56
Within R^2	0.3246	0.3503		0.3246	0.3503
Overall R^2	0.7902	0.7102		0.7902	0.7119

Note: The table reports results from fixed effects regressions with log profits on the store level as the dependent variable. The regression accounts for time and store fixed effects (columns 1–4) and adds fixed effects for district and store managers in columns 2&4. The fixed effects regressions compare pre-treatment observations (January 2016–March 2017) with the observations during the experiment (April 2017–June 2017). All regressions control for possible refurbishments of a store and the companies' planned value. Observations were excluded once a store manager switched the store during the treatment period. *Treatment effects* thus refer to the difference-in-difference estimator. Robust standard errors are clustered at the district level of the treatment start and displayed in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$.

Figure A1 – Histogram of Monthly Bonus Payments



Note: The figure displays the monthly bonus payment to managers in the BONUS and BONUS & INFORMATION treatment.

Table A5 – Main Treatment Effects on Gross Profits w/o managers who did not watch the training video

	(1) Profits	(2) Profits		(3) Profits	(4) Profits
Treatment BONUS	581.2 (400.0)	1051.1** (452.2)	Bonus	581.2 (400.0)	1051.1** (452.2)
Treatment INFORMATION	1324.9*** (470.8)	1574.0*** (535.0)	Information	1324.9*** (470.8)	1574.0*** (535.0)
Treatment BONUS&INFORMATION	1292.7* (686.2)	1533.6** (748.9)	Bonus × Information	-613.3 (836.3)	-1091.5 (914.1)
Planned Profits	0.431*** (0.0487)	0.423*** (0.0494)	Planned Profits	0.431*** (0.0487)	0.423*** (0.0494)
Refurbishment Ongoing	-2686.4*** (530.0)	-2618.1*** (557.8)	Refurbishment Ongoing	-2686.4*** (530.0)	-2618.1*** (557.8)
After Refurbishment	-539.0 (435.3)	-585.9 (466.9)	After Refurbishment	-539.0 (435.3)	-585.9 (466.9)
Time FE	Yes	Yes		Yes	Yes
Store FE	Yes	Yes		Yes	Yes
District Manager FE	No	Yes		No	Yes
Store Manager FE	No	Yes		No	Yes
N of Observations	6328	6154		6328	6154
N of Stores	362	362		362	362
Cluster	56	56		56	56
Within R^2	0.3514	0.3690		0.3514	0.3690
Overall R^2	0.8377	0.7564		0.8377	0.7609

Note: The table reports results from fixed effects regressions with log profits on the store level as the dependent variable. The regression accounts for time and store fixed effects (columns 1–4) and adds fixed effects for district and store managers in columns 2&4. Columns 1-4 drop observations for store managers who did not watch the training video during the treatment time. The fixed effects regressions compare pre-treatment observations (January 2016–March 2017) with the observations during the experiment (April 2017 - June 2017). All regressions control for possible refurbishments of a store and the companies' planned value. Observations were excluded once a store manager switched the store during the treatment period. *Treatment effects* thus refer to the difference-in-difference estimator. Robust standard errors are clustered at the district level of the treatment start and displayed in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.001$.

Table A6 – Main Treatment Effects on Net Profits

	(1) Profits	(2) Profits		(3) Profits	(4) Profits
Treatment	297.7	759.0*	Bonus	297.7	759.0*
BONUS	(392.7)	(445.1)		(392.7)	(445.1)
Treatment	999.3**	1224.1**	Information	999.3**	1224.1**
INFORMATION	(450.5)	(515.1)		(450.5)	(515.1)
Treatment	957.6*	1146.9*	Bonus × Information	-339.4	-836.2
BONUS&INFORMATION	(512.5)	(583.6)		(683.7)	(769.0)
Planned Profits	0.422***	0.417***	Planned Profits	0.422***	0.417***
	(0.0483)	(0.0485)		(0.0483)	(0.0485)
Refurbishment Ongoing	-2788.6***	-2805.0***	Refurbishment Ongoing	-2788.6***	-2805.0***
	(588.2)	(606.4)		(588.2)	(606.4)
After Refurbishment	-625.7	-626.2	After Refurbishment	-625.7	-626.2
	(408.7)	(430.5)		(408.7)	(430.5)
Time FE	Yes	Yes		Yes	Yes
Store FE	Yes	Yes		Yes	Yes
District Manager FE	No	Yes		No	Yes
Store Manager FE	No	Yes		No	Yes
N of Observations	6470	6296		6470	6294
N of Stores	363	363		363	363
Cluster	56	56		56	56
Within R^2	0.3246	0.3503		0.3475	0.3656
Overall R^2	0.7902	0.7102		0.8350	0.7431

Note: The table reports results from fixed effects regressions with the net profits (profits minus bonus costs) on the store level as the dependent variable. The regression accounts for time and store fixed effects (columns 1–4) and adds fixed effects for district and store managers in columns 2&4. The fixed effects regressions compare pre-treatment observations (January 2016–March 2017) with the observations during the experiment (April 2017 - June 2017). All regressions control for possible refurbishments of a store and the companies' planned value. Observations were excluded once a store manager switched the store during the treatment period. *Treatment effects* thus refer to the difference-in-difference estimator. Robust standard errors are clustered at the district level of the treatment start and displayed in parentheses. * $p<0.1$, ** $p<0.05$, *** $p < 0.001$.

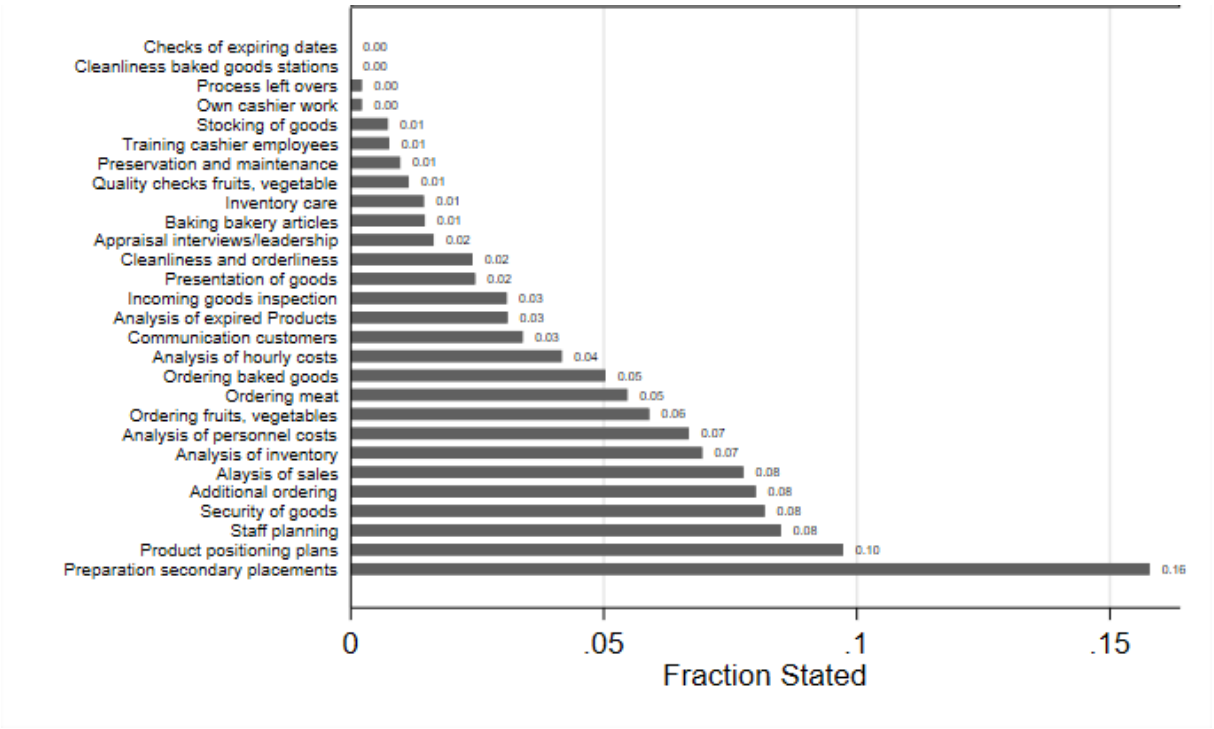
Table A7 –Treatment Effects depending on Product Margin Categories

Panel A –Sales	(1) 1st Cat	(2) 2nd Cat	(3) 3rd Cat	(4) 4th Cat	(5) 5th Cat
Treatment BONUS	164.4 (479.4)	107.8 (285.3)	287.5 (405.6)	-453.3 (932.2)	66.69 (311.1)
Treatment INFORMATION	682.9 (435.2)	595.7** (259.3)	945.3** (370.5)	77.81 (941.4)	373.8 (264.1)
Treatment BONUS&INFORMATION	715.3 (543.4)	597.4 (437.1)	950.2* (557.4)	499.7 (1171.5)	407.4 (318.7)
Refurbishment Ongoing	453.9 (506.6)	-226.8 (336.2)	-919.6** (358.0)	-676.5 (815.8)	46.56 (309.3)
After Refurbishment	1201.5*** (376.5)	193.2 (236.1)	-278.6 (305.6)	514.9 (619.4)	153.8 (256.4)
Fixed Effects (Store, Time, Store and District Manager)	Yes	Yes	Yes	Yes	Yes
Planned Values	Yes	Yes	Yes	Yes	Yes
Observations	6297	6297	6296	6297	6296
N Store	363	363	363	363	363
N Cluster	56	56	56	56	56
Within R^2	0.6000	0.5618	0.5781	0.7916	0.8646
Overall R^2	0.7717	0.8537	0.8080	0.7433	0.7160

Panel B – Quantity of Products (in units)	(1) 1st Cat	(2) 2nd Cat	(3) 3rd Cat	(4) 4th Cat	(5) 5th Cat
Treatment BONUS	-62.30 (537.8)	199.0 (157.9)	316.3 (324.5)	-329.4 (614.3)	10.87 (145.7)
Treatment INFORMATION	423.5 (498.1)	383.4** (172.5)	885.8*** (328.2)	29.52 (605.6)	132.7 (127.7)
Treatment BONUS&INFORMATION	957.0* (565.3)	614.9* (325.4)	1234.5** (597.9)	814.1 (636.5)	186.4 (163.3)
Refurbishment Ongoing	-1521.8*** (468.1)	-815.7*** (215.5)	-2002.5*** (308.3)	-1583.8*** (391.0)	-308.0*** (112.6)
After Refurbishment	1464.0*** (497.8)	442.1** (210.4)	-274.0 (295.6)	405.8 (437.5)	224.8** (103.5)
Fixed Effects (Store, Time, Store and District Manager)	Yes	Yes	Yes	Yes	Yes
Observations	6264	6264	6263	6264	6263
N Store	361	361	361	361	361
N Cluster	56	56	56	56	56
Within R^2	0.4938	0.4469	0.3975	0.6116	0.8309
Overall R^2	0.0311	0.0540	0.0283	0.0730	0.2131

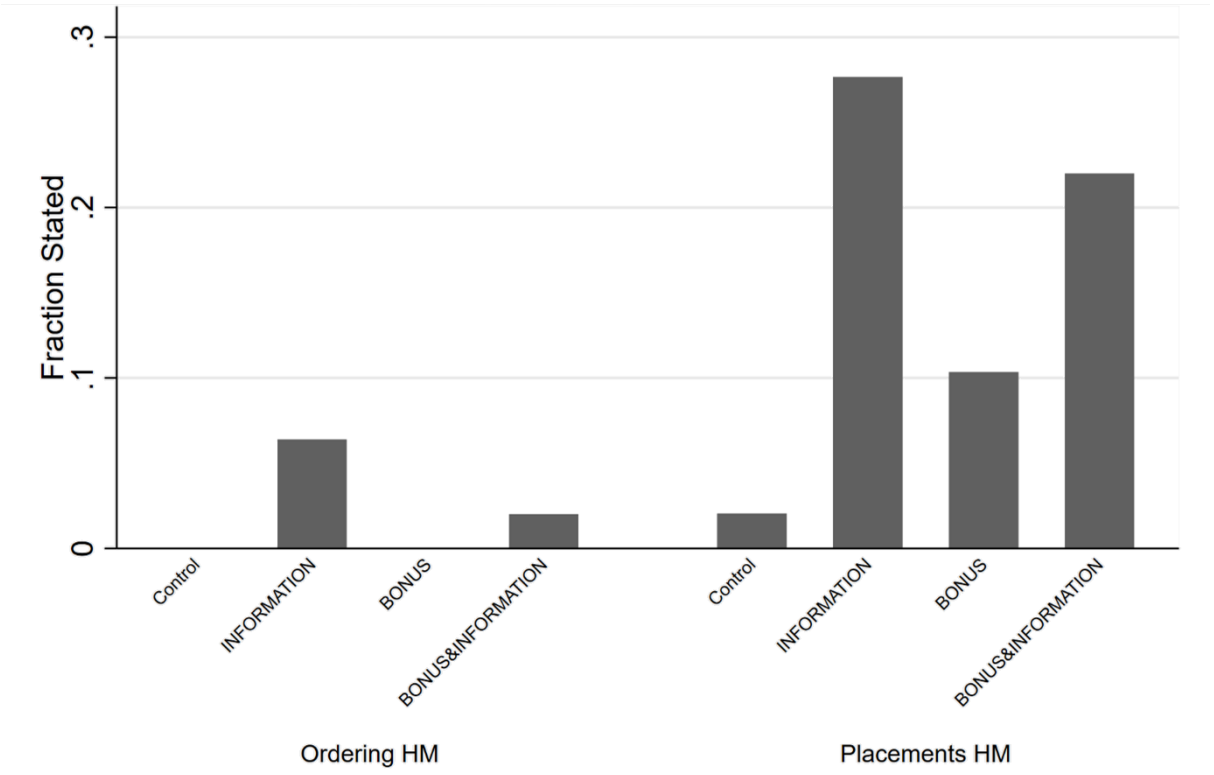
Note: The table reports results from fixed effects regressions with sales on the store level as the dependent variable. The different columns represent the different margin categories (e.g. 1st category = sales of the 20% of the products with the highest margin, 5th category = sales of the 20% of the products with the lowest margin). The regression accounts for time, store, store manager and district manager fixed effects. The fixed effects regressions compare pre-treatment observations (January 2016–March 2017) with the observations during the experiment (April 2017 - June 2017). All regressions control for possible refurbishments of a store and the companies' planned value for all profit components. Observations were excluded once a store manager switched the store during the treatment period. Observations were further excluded for store managers who did not watch the training video during the treatment time. *Treatment effects* thus refer to the difference-in-difference estimator. Robust standard errors are clustered at the district level of the treatment start and displayed in parentheses. * $p<0.1$, ** $p<0.05$, *** $p < 0.001$.

Figure A2 –Task Focus to Increase Profits (Open Questions)



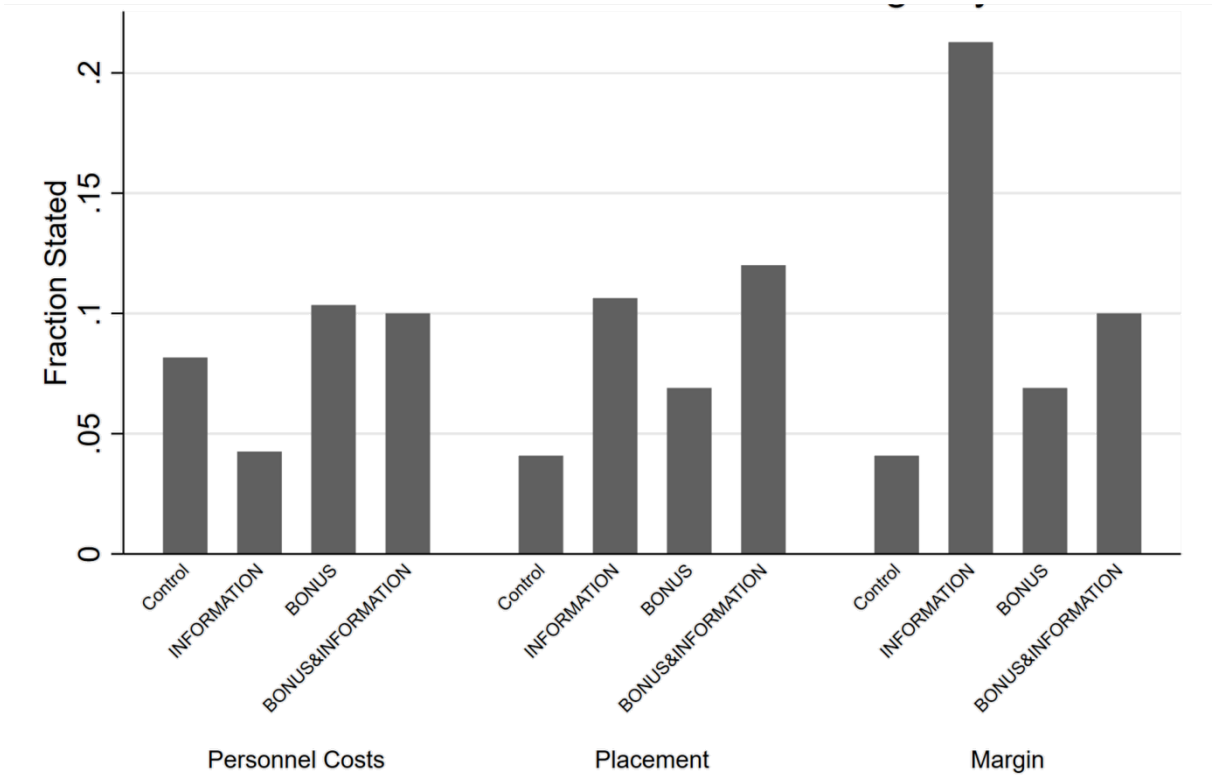
Note: The figure displays the average rating of focus on specific tasks (1 = low focus, 6 = high focus) obtained from an online questionnaire. N=204.

Figure A3 – Focus on High-Margin Products



Note: The figure displays the fraction of explicitly stated task dimensions with a focus on high-margin products to increase profits obtained from open questions of an ex-post questionnaire. N=204.

Figure A4 – Text (Keyword) Analysis of Open Question



Note: The figure displays the fraction of stated keywords when we asked store managers what they did to increase store profits in an open question from an ex-post questionnaire. N=204.

Table A8: Self-Stated Actions to increase profits (open questions)

Panel A	(1) Ordering	(2) Placements	(3) Cleanliness	(4) KPI	(5) Inventory	(6) Personnel Management	(7) Own Effort
Treatment BONUS	0.0735	0.0844	-0.00633	0.0654	0.143*	0.0767	0.0250
	(0.0729)	(0.0850)	(0.0356)	(0.0840)	(0.0801)	(0.0637)	(0.0440)
Treatment INFORMATION	0.131*	0.261***	-0.0195	-0.0960	0.195**	0.0452	-0.0187
	(0.0768)	(0.0894)	(0.0375)	(0.0884)	(0.0842)	(0.0670)	(0.0463)
Treatment BONUS&INFORMATION	0.158**	0.378***	-0.000816	0.0351	0.238***	0.119*	-0.0412
	(0.0756)	(0.0881)	(0.0369)	(0.0871)	(0.0829)	(0.0660)	(0.0456)
Controls	No	No	No	No	No	No	No
Observations	204	204	204	204	204	204	204
R^2	0.025	0.102	0.002	0.007	0.044	0.017	0.012
Panel B	Ordering	Placements	Cleanliness	KPI	Inventory	Personnel Management	Own Effort
Treatment BONUS	0.0468	0.0167	-0.0530	0.0564	0.114	0.127*	0.0117
	(0.0858)	(0.0968)	(0.0370)	(0.0956)	(0.0929)	(0.0761)	(0.0516)
Treatment INFORMATION	0.136	0.249**	-0.0262	-0.113	0.190**	0.0547	-0.0257
	(0.0848)	(0.0956)	(0.0366)	(0.0944)	(0.0918)	(0.0752)	(0.0510)
Treatment BONUS&INFORMATION	0.132	0.350***	-0.00751	0.00703	0.220**	0.0948	-0.0483
	(0.0833)	(0.0939)	(0.0359)	(0.0927)	(0.0901)	(0.0738)	(0.0501)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	181	181	181	181	181	181	204
R^2	0.050	0.158	0.019	0.046	0.053	0.017	0.018

Note: The table reports results from OLS regressions with the focus of different classified tasks from an online questionnaire as the dependent variable. The dependent variable is equals 1 if a mentioned task falls into the respective category and 0 otherwise. Panel B controls include the size of the store, amount of full-time equivalent employees (FTE), age of the store manager, and the annual subjective performance evaluation. Observations were excluded once a store manager switched the store during the treatment period. Robust standard errors are clustered at the district level of the treatment start and displayed in parentheses.* $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$.

Table A9 – Monthly Treatment Effects

	(1) Profits	(2) Profits
Treatment BONUS 1st Month	1319.8*** (469.7)	1747.9*** (522.9)
Treatment BONUS 2nd Month	681.2 (526.2)	1208.0** (536.5)
Treatment BONUS 3rd Month	-257.6 (807.3)	189.3 (875.2)
Treatment INFORMATION 1st Month	1532.0*** (567.0)	1628.0*** (590.4)
Treatment INFORMATION 2nd Month	995.7* (573.5)	1290.7** (581.0)
Treatment INFORMATION 3rd Month	470.9 (819.0)	753.4 (913.2)
Treatment BONUS & INFORMATION 1st Month	1437.4*** (521.7)	1466.0*** (544.5)
Treatment BONUS & INFORMATION 2nd Month	1605.3** (748.3)	1867.8** (766.0)
Treatment BONUS & INFORMATION 3rd Month	844.2 (826.8)	1131.3 (923.1)
Planned Profits	0.424*** (0.0487)	0.420*** (0.0487)
Refurbishment Ongoing	-2759.4*** (594.0)	-2782.0*** (610.6)
After Refurbishment	-633.0 (409.5)	-635.5 (431.3)
Fixed Effects (Store, Time)	Yes	Yes
Fixed Effects (Store Manager, District Manager)	No	Yes
Observations	6472	6296
N Store	363	363
N Cluster	56	56
Within R^2	0.3496	0.3679
Overall R^2	0.8345	0.7455

Note: The table reports results from fixed effects regressions with the profits on the store level as the dependent variable. The regression accounts for time and store fixed effects and adds fixed effects for district and store managers in columns 2. The fixed effects regressions compare pre-treatment observations (January 2016–March 2017) with the observations during the experiment (April 2017–June 2017). All regressions control for possible refurbishments of a store and the companies' planned value. Observations were excluded once a store manager switched the store during the treatment period. *Treatment Effect* thus refers to the difference-in-difference estimator. Robust standard errors are clustered at the district level of the treatment start and displayed in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$.

Table A10 – Monthly Treatment Effects using Interactions

	(1) Profits	(2) Profits
Bonus 1st Month	1319.8***	1747.9***
	(469.7)	(522.9)
Bonus 2nd Month	681.2	1208.0**
	(526.2)	(536.5)
Bonus 3rd Month	-257.6	189.3
	(807.3)	(875.2)
Information 1st Month	1532.0***	1628.0***
	(567.0)	(590.4)
Information 2nd Month	995.7*	1290.7**
	(573.5)	(581.0)
Information 3rd Month	470.9	753.4
	(819.0)	(913.2)
Bonus × Information 1st Month	-1414.39*	-1909.96***
	(521.7)	(787.00)
Bonus × Information 2nd Month	-71.62	-630.96
	(958.00)	(1024.88)
Bonus × Information 3rd Month	630.92	188.61
	(1169.35)	(1249.53)
Planned Profits	0.424***	0.420***
	(0.0487)	(0.0487)
Refurbishment Ongoing	-2759.4***	-2782.0***
	(594.0)	(610.6)
After Refurbishment	-633.0	-635.5
	(409.5)	(431.3)
Fixed Effects (Store, Time)	Yes	Yes
Fixed Effects (Store Manager, District Manager)	No	Yes
Observations	6472	6296
N Store	363	363
N Cluster	56	56
Within R^2	0.3496	0.3679
Overall R^2	0.8345	0.7438

Note: The table reports results from fixed effects regressions with the profits on the store level as the dependent variable. The regression accounts for time and store fixed effects and adds fixed effects for district and store managers in columns 2. The fixed effects regressions compare pre-treatment observations (January 2016–March 2017) with the observations during the experiment (April 2017–June 2017). All regressions control for possible refurbishments of a store and the companies' planned value. Observations were excluded once a store manager switched the store during the treatment period. Robust standard errors were clustered at the district level of the treatment start and displayed in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$.