Information, Incentives, and Attention:
A Field Experiment on the Interaction of Management Controls

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We study the profit effects and interplay of two core accounting practices in a field experiment in a large retail chain. In a 2 × 2 factorial design, we vary (i) whether store managers obtain decision-facilitating information on a profit metric and (ii) whether they receive performance pay based on the same metric. We find that both practices increase profits significantly. In contrast to reasoning based on standard economic theory, we do not find complementarity between both interventions. Rather, we detect evidence in line with an attention-directing role of both practices: the introduction of each raises attention to the underlying objective, which induces a countervailing substitution effect.

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

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1 Introduction

Exploring the performance effects of specific accounting practices is a key domain of accounting research. Due to potential interdependencies, scholars have advocated the study of systems of such practices to better understand their interplay.[1] In this paper, we investigate the performance effects of two core accounting practices, as well as their dynamic interplay, in a field experiment in a firm. We consider two interventions that represent core roles of managerial accounting information usage, both introduced to raise the profits of the firm: (i) the provision of decision-facilitating information and (ii) the use of performance pay to influence managers' decisions.

Decision-facilitating information helps employees by reducing uncertainty about the consequences of their actions and increasing the agents' knowledge about the decision problem. In contrast, decision-influencing information entails evaluating agents' behaviors to affect their incentives through, for instance, performance pay or supervisor monitoring (Demski and Feltham 1976, Baiman 1982, Evans et al. 1994, Narayanan and Davila 1998, Sprinkle 2003, Sprinkle and Williamson 2006, Wall and Greiling 2011).

We conducted a firm-level field experiment within a large German retail chain, randomly assigning 362 supermarket store managers to three different treatment groups and a control group, implementing a 2 × 2 factorial design. Prior to our study, stores had mainly been evaluated by their sales performance while all our interventions aimed at raising store profits.[2] To facilitate their decisions, store managers in the *INFORMATION* treatment group obtained access to information about the applied profit metric through an online training offer and learned about the profit margins of individual products. Store managers in the *BONUS* treatment group received performance pay based on the same profit metric. The third *BONUS* & *INFORMATION* treatment group received both, decision-facilitating information and performance pay. The control group remained unaffected by the changes. The random assignment of the interventions allows for the estimation of the causal effects of the two practices as well as their interaction effects in the same environment (e.g., Bandiera et al. 2011, Floyd and List 2016).

[1] 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, Martin 2020, Merchant and Otley 2020). 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." However, only little causal field-experimental evidence within firms on such interdependencies exists (for notable exceptions see, Lourenço 2016, Sandvik et al. 2020, Manthei et al. 2022)

[2] Moreover, several key components of store profits, such as personnel costs and inventory losses, were tracked systematically, but store profits were not at the forefront of performance assessments. As we explain below, the key reason is that before the interventions, store managers did not have access to profit margins of individual products.

Our study highlights two countervailing phenomena that affect the interplay between the two interventions. According to standard economic reasoning, performance pay should motivate higher efforts, and access to decision-facilitating information should make it easier for store managers to allocate these efforts more efficiently. From this perspective, both practices should thus be *complements* and this was our pre-registered hypothesis for the experiment. However, when the introduction of each practice also generates attention to the underlying objective (in our case, to raise profits), they may become *substitutes*.

We illustrate the interplay between these two mechanisms in a formal conceptual framework. In the first step, we incorporate the provision of decision-facilitating information into a standard multitasking moral hazard model (e.g., Holmström and Milgrom 1991, Feltham and Xie 1994) and show that a complementarity between performance pay and information provision naturally arises. We extend the framework to formalize the attention-directing role of both practices, capturing three key features stressed in the literature on attention: attention is triggered by stimuli, there is diminishing sensitivity to the strength of a stimulus, and attention tends to fade over time (e.g., Simon 1947, Kahneman 1973, Birnberg and Shields 1984, Ocasio 1997, Hirshleifer and Teoh 2003, Bordalo et al. 2021). The key idea of the extension is the following: when the introduction of each of the two practices communicates the importance of the objective to raise profits, then both generate attention for this underlying objective. Due to diminishing sensitivity, the effect of the two practices should be sub-additive in this respect; that is, the marginal additional attention effect of introducing one practice is smaller when the respective other practice is in place. This creates a countervailing substitution effect, potentially reversing the complementarity arising from standard agency considerations. To the extent that attention fades over time, the overall performance effects of each treatment, as well as the substitution effect, should be particularly pronounced directly after the introduction of the practices. By the same token, both should become weaker over time as the novelty of the stimulus fades.

Our empirical results show the following: First, when introduced separately, both the introduction of performance pay and the provision of decision-facilitating information significantly increase profits. With a return on investment of approximately 5500%, the decision-facilitating intervention, in particular, is highly profitable due to its low costs. Second, the combined intervention, in which both practices were introduced together, also increases performance above the level of the control group. However, in contrast to our ex-ante hypothesis based on standard economic reasoning, we do not find evidence of complementarity between the two practices. While in our main specification the *BONUS* treatment raises monthly

profits by about €1007 per store on average and the *INFORMATION* treatment by about €1170, the effect of the combined intervention of €1440 falls short of the sum of the individual effects of the two practices when introduced separately.

To understand the reasons for the absence of complementarity between both practices, we explore data from a post-experimental survey and time patterns in the observed profit effects in more detail. A first key observation in line with the interventions' attention-directing role is that managers' self-reported intention to raise profits is significantly stronger in all three treatment groups than in the control group. Yet, it hardly differs between the three treatments; thus, all treatments shifted the store managers' self-reported perceptions about the importance of store profits as a key objective in a similar manner.

Examining the time structure of the performance effects, we find that the separate interventions—*BONUS* and *INFORMATION*—have the strongest effects in the first month after their introduction. Moreover, we find time patterns consistent with the attention-directing behavioral channel formalized in the conceptual model. We observe clear evidence that the two practices are initially substitutes, as their interaction is significantly negative in the first month. However, this substitution effect vanishes over time. It appears that the practices indeed create an initial push in attention to the profit objective such that the two practices are substitutes directly after their introduction.

We find further evidence in line with the claim that the interventions also guided store managers' attention toward the underlying profit objective from the analysis of participation in the online training and its timing. For instance, we find that profits after the end of the intervention period are the larger, the smaller the time elapsed since the respective store manager had attended training. Still, we also find evidence that the information interventions affected the store managers' behaviors not only through the creation of attention but also through the provided decision-facilitating information, as store managers focused more intensively on the placement of higher margin products when they had access to information on profit margins.

The paper makes several contributions to the literature, which we explain in more detail below. We add to the large literature on the performance effects of management control systems (e.g., Bonner and Sprinkle 2002, Merchant and Van der Steede 2017) and to the study of interdependencies between specific practices (e.g., Ferreira and Otley 2009, Grabner and Moers 2013, Masschelein and Moers 2020). In particular, we contribute to the emerging field experimental literature on the causal evaluation of such interdependencies. In contrast to reasoning based on standard economic theory, but in line with some recent empirical findings

(Lourenco 2016, Sandvik et al. 2020), we do not find evidence for a complementarity between decision-facilitating and decision-influencing interventions. Our main novel contribution is the development of a theoretical explanation for this absence of complementarity and an empirical test of its implications. Our explanation is based on the idea already put forward by Simon (1947) that behavior in organizations is also guided by the direction of attention provided through stimuli. A key implication of our study is that the introduction of a specific management control intended to affect a certain objective directly creates attention for this objective. If, then, different controls are implemented, and these controls target the same objective through different means, they naturally become substitutes for guiding attention toward the underlying goal.

2 Literature Overview and Conceptual Framework

2.1 Literature Overview

The managerial accounting literature has a long history of studying the role of the organizational environment in the performance effects of specific management controls. As argued, for instance, by Otley (1980) and Chenhall (2003), the effect of a management control practice depends on the use of other practices in place. Similarly, the economic literature on complementarities in organizations (Milgrom and Roberts 1995, Ichniowski and Shaw 2003, Brynjolfsson and Milgrom 2013) has suggested that the performance effect of introducing a specific management practice is often contingent on using other practices. A set of management control practices is often categorized as a system if there are positive independencies between these practices (e.g., Ferreira and Otley 2009, Grabner and Moers 2013, Masschelein and Moers 2020).[3] While the recent literature on management control systems increasingly calls for experimental evaluation of the interdependencies (e.g., Choi 2020, Merchant and Otley 2020), studies on the dynamic relationship of the interdependencies of management controls and their effect on employees' performance are still scarce.[4]

[3] 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 using the term system only if there are interdependencies.

[4] A notable exception here, albeit focusing on how management control systems develop over time rather than how their performance effect develops over time, is Martin (2020).

2.1.1 Decision-Facilitating and Decision-Influencing

Two core roles of managerial accounting are (i) to provide information to facilitate managers' decisions and (ii) to influence managers' behavior through different types of incentives (Demski and Feltham 1976, Baiman 1982, Narayanan and Davila 1998). Both types of managerial accounting information usage have been shown to yield positive performance effects.[5] The provision of decision-facilitating information can increase learning and improve the quality of decisions (e.g., Ghosh 1997, Frederickson et al. 1999, Casas-Arce et al. 2017a, Anderson and Kimball 2019, Manthei and Sliwka 2019).[6] The use of decision-influencing information through the provision of performance pay has—in most studies—also raised performance (e.g., Bailey et al. 1998, Banker et al. 2000, Sprinkle 2000, Lazear 2000, Lourenço 2016, Friebel et al. 2017).[7]

Fewer studies have compared both practices directly. Van Veen-Dirks (2010) showed that firms tend to attach more importance to the decision-facilitating rather than decision-influencing use of a broad set of key accounting figures.[8] Indicating complementarity between both practices, Grafton et al. (2010) argued that organizational performance is correlated with the degree of commonality between decision-facilitating and decision-influencing information. Very much in line with the idea of the first part of our formal model, they assert that for managers to use decision-facilitating measures, these measures should also be part of the decision-influencing process.[9]

However, there is not much field evidence on the causal effect of both practices in the same environment, and only a few studies have investigated their interplay in affecting employees' behavior. Drake et al. (1999) provided some evidence for a complementarity between the implementation of activity-based costing information combined with incentives in an experiment with students from an MBA program. In a laboratory experiment, Sprinkle (2000) found an interdependency between feedback information to facilitate learning and performance incentives, as learning effects are greater when the information provided to facilitate learning

[5] For a summary of the literature, see, for instance, Sprinkle and Williamson (2006).
[6] 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. 2020).
[7] Specific environmental circumstances, however, such as task complexity, multitasking, different preferences, image concerns, or exhausted learning curves, can lead to a reduction or sometimes even a reversal of these effects (Holmström and Milgrom 1991, Bonner et al. 2000, Frey and Jegen 2001, Bénabou and Tirole 2006, Sliwka 2007, Manthei et al. 2021, Manthei et al. 2022).
[8] Together with our result that the provision of decision-facilitating information strongly outperforms that of performance pay, this suggests that often conflicts of interests between firms and their employees may be of less concern than a lack of information beneficial for decision-making.
[9] 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 (2006).

is also part of the performance incentive. In a later paper, Sprinkle (2003) himself pointed out the importance of studying the interaction effects between decision-facilitating and decision-influencing information. Only a few studies have provided causal evidence of the interplay between both practices in field experiments within firms. Notable exceptions are Lourenço (2016) and Sandvik et al. (2020), who also studied the interaction effects between performance pay and particular types of decision-facilitating information, such as feedback about past performance in Lourenço (2016) and knowledge-sharing conversations between co-workers in Sandvik et al. (2020). Consistent with our results, both studies found no evidence of complementarity between the respective decision-facilitating practice and performance pay.

Hence, the previous literature has found mixed results concerning the complementarity between decision-facilitating information and the incentives used to influence decisions. While some studies find evidence in line with the standard economic reasoning of complementarity, others, especially field experimental studies in firms, do not find such evidence. Our main contribution is that we add a novel explanation that helps reconcile these outcomes. In particular, we show that the attention-directing role of management practices can explain the absence of complementarity seen in previous studies.[10]

2.1.2 Attention and Accounting

In his classical treatise on behavior in organizations, Simon (1947) has already stressed the role of attention to guide behavior and laid out that "Organizations and institutions provide the general stimuli and attention-directors that channelize the behavior of the members of a group, and that provide the members with the intermediate objectives that stimulates actions" (pp. 100–101). That is, in contrast to standard economic reasoning, choices are often also affected by less volitional and more automatic processes, and this has substantial effects on behavior in organizations.

The literature on the role of attention in psychology (Kahneman, 1973), organizational theory (Simon 1947, Ocasio 1997), accounting (Birnberg and Shields 1984, Libby and Trotman 1993, Hirshleifer and Teoh 2003) and more recently in economics (see Bordalo et al. 2021 for a survey) has stressed several crucial aspects which we discuss in more detail below in section 2.2.2: First, attention is not only allocated through voluntary choices but also guided

[10] In fact, Sandvik et al. (2020, p.1658) speculate about reasons for the absence of a complementarity in their case, stating that it "may indicate crowding out of monetary incentives […] or reduced salience when incentives are presented in conjunction with instructions to change other behavior," where the latter may hint at a mechanism closely related to the one we develop in this paper.

involuntarily through specific stimuli, and organizational practices generate such stimuli. Second, individuals have a limited capacity for attention, and responses to stimuli exhibit diminishing sensitivity—that is, there are decreasing marginal returns in the strength of a stimulus. Third, if a certain intervention or event provides a stimulus that generates attention without further reminders, attention tends to fade over time.[11]

Our study is also related to research on the attention and effort-directing role of accounting information. In the context of performance feedback, Casas-Arce et al. (2018) and Eyring (2021), for instance, find evidence supporting the view that decision-makers have limited information processing capacities and that the relative salience of the presented information has a strong effect in guiding attention. Relatedly, studies investigating the processing of accounting information, for instance, in the design of performance reports or balanced scorecards, have investigated how these have to be designed in order to create stimuli guiding managers' attention and, in turn, their actions (e.g., Banker et al. 2004, Cardinaels and van Veen-Dirks 2010, Chen et al. 2016, Cardinaels et al. 2022).

In line with these findings, we argue that the introduction of any management control practice, in addition to its intended incentive or decision-facilitating effects, will also affect the allocation of attention toward specific objectives. These attention-directing effects interact with the directly intended effects, making it essential to study their dynamic interplay over time.

2.2 A Conceptual Framework

We start by describing a novel conceptual framework that allows us to describe the interplay between the provision of decision-facilitating information and the use of performance pay. To do so, we extend the classical framework of a multitasking principal-agent model (Holmström and Milgrom 1991, Baker 1992, Feltham and Xie 1994). In the first step, we introduce uncertainty about marginal returns to efforts to study how information about these marginal returns affects the (rational) allocation of efforts. In the second step, we introduce (boundedly rational) attention effects into the framework.

[11] Analogous patterns are described in the literature on visual attention where attention is defines as "a selective process, which is usually conceptualized as being related to limited cognitive and brain resources" (Carrasco 2011, pp. 1486). Similarly to the literature in social psychology, the field also distinguishes two attention systems where "one corresponds to our ability to willfully monitor information […]" while the other "is an involuntary system that corresponds to an automatic orienting response" to a stimulus and is transient in nature (Carrasco 2011, pp. 1488).

2.2.1 The Basic Model

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 gross profits are equal to

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

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, (i.e., $I \in \{0,1\}$). 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. Note that $\sigma_{\varepsilon_j}^2$ captures the role of prior uncertainty about the task that is resolved through information provision: if $\sigma_{\varepsilon_j}^2$ is small for a specific task j, the agent already has precise information ex-ante for this task.

The agent internalizes the effect of his actions on overall profits, and the parameter θ determines the extent to which he does so. This parameter captures the effects of reputational incentives but also—as we illustrate below—the attention created for the objective to raise profits. On top of that, the agent may receive a performance contingent bonus $\beta \cdot \pi$ with $\beta \in [0,1]$. The agent's objective function is

$$(\beta + \theta)\pi - \sum_{j=1}^{k} \frac{1}{2} e_j^2.$$

The agent thus maximizes

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

and therefore optimally chooses

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

Using thus, the ex-ante expected performance is given by

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

When 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

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

Hence, both the provision of information (choosing $I = 1$) and implementing bonuses $\beta > 0$ increase the 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.

Let now $\Delta_{\mathrm{B}}\Pi(\mathrm{I}) = \Pi(\mathrm{I}, \mathrm{B}) - \Pi(\mathrm{I}, 0)$ be the profit effect of introducing a bonus $B > 0$ such that

$$\Delta_{\mathrm{B}}\Pi(\mathrm{I}) = \mathrm{B} \cdot \sum_{j=1}^{k} \left(m_j^2 + \frac{\sigma_j^4}{\sigma_j^2 + (1-I) \cdot \sigma_{\varepsilon_j}^2} \right),$$

which is strictly larger when $I = 1$. Hence, both practices are complements. The reason is that bonus payments raise efforts, and the decision-facilitating information allows the agent to allocate these efforts more efficiently.

Thus, we can summarize:

Proposition 1. *When the agent is fully rational, (i) the introduction of a bonus and the provision of decision-facilitating information both raise profits. (ii) Both practices are complements in this case, i.e., the effect of the bonus is larger when the agent has access to decision-facilitating information, i.e.,* $\Delta_{\mathrm{B}}\Pi(\mathrm{I}) > \Delta_{\mathrm{B}}\Pi(0)$.

2.2.2 The Role of Attention

Basic agency models, such as the one developed above, depict the voluntary allocation of efforts to different tasks through a volitional process to achieve an objective. The pre-registered hypotheses at the start of this project were based on *Proposition 1*, which describes the interplay between both practices from this standard economics perspective.

However, choices are often also affected by less volitional and more automatic processes (see, e.g., Kahneman 1973). Bordalo et al. (2021), for instance, state in a recent survey: "*In standard economics, attention is either unlimited or, if costly, optimally deployed 'top down' given current goals and expectations. This approach has proved very useful, but does not recognize that goals often compete with bottom up stimulus-driven attention.*" (p. 2).

In the next step, we therefore extend the framework to illustrate the interplay between the rational process analyzed above and a less volitional and more automatic *attention-directing effect* that can be triggered when a management practice is introduced. The key idea of the extension is simple: whenever a practice is implemented that aims at affecting a certain objective, this objective in itself becomes more salient—and the increased attention in turn also guides behavior toward the objective.

To incorporate this into our formal framework, consider a dynamic version of the above model in which the agent works over $t = 1,..T$ periods. In Period 1, the respective practices are implemented and then remain in place so that $t - 1$ is the time elapsed since the practice has been introduced. Recall that in the above model, the parameter θ captures the extent to which the agent internalizes the effect of her actions on the profits of the firm. We now—while keeping the structure of the model otherwise unchanged—assume that θ depends on the *salience* of profits as an objective in the agent's mind. In particular, we assume that

$$\theta_t = \Theta \cdot S_\pi(I, \beta, t),$$

where Θ determines the strength of implicit incentives and the function $S_\pi(I, \beta, t)$ captures the salience of the profit metric π. Hence, the agent's objective function becomes

$$\big(\beta + \Theta \cdot S_\pi(I, \beta, t)\big) \cdot \pi - \sum_{j=1}^{k} \frac{1}{2} e_j^2$$

such that his effort choices are affected both by his material returns and the extent to which a stimulus-driven process guides his attention toward the profit objective.

The salience function $S_\pi(I, \beta, t)$ now describes how the interventions contribute to the strength of the overall stimulus that generates attention for the objective of raising profits. The extension now incorporates two key characteristics stressed in the literature on attention and salience into the basic framework:

1. There is *diminishing sensitivity* to the strength of a stimulus.
2. Without further stimulus, attention tends to *fade over time*.

Diminishing sensitivity is a central feature of fundamental models of perception (such as prospect theory). As laid out, for instance, in Thaler (2015, p. 32), it reflects the Weber-Fechner law of sensory perception, one of the earliest findings in psychology: a given variable difference is more salient at lower values of the variable. In the words of Kahneman and Tversky (1979, p. 278) "*the psychological response is a concave function of the magnitude of physical change*". More specifically, the Weber-Fechner law stipulates that the perception of a stimulus is proportional to the logarithm of the physical change.[12] This effect is described analogously in marketing research and is expressed in a standard concave advertisement response function.[13] To illustrate this concept applied to our setting, think of salience as being driven by how often one is reminded of considering the profit effects of one's actions. Both the bonus and the information intervention contribute to the strength of the overall stimulus. The concept of diminishing sensitivity now implies that the salience of the objective exhibits decreasing marginal returns in the strength of the stimulus.

Fading attention is another crucial element in the study of salience. 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 typically 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, found that wage increases lead to higher efforts, which then fade over time. Madsen and Niessner (2019) studied how advertising triggers investor attention, which vanishes over time.[14] Rubin and Wenzel (1996) conducted a meta-study on time patterns in forgetting and found patterns of retention that were decreasing and concave over time.

To incorporate the Weber-Fechner law as well as the fading of attention into the model, we apply the following salience function:

$$S_\pi(I, \beta, t) = \eta^{t-1} \cdot ln\left(\frac{s_i(I) + s_b(\beta) + S_0}{S_0}\right),$$

where $s_i(I)$ and $s_b(\beta)$ are strictly increasing functions that capture the contributions of both practices to the strength of the overall stimulus toward the profit metric π. Let $s_i(0) = s_b(0) = 0$, and S_0 be a strictly positive constant that determines the baseline level of the stimulus. The constant $\eta < 1$ determines the decay of attention over time.

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

[12] See, e.g., https://dictionary.apa.org/fechners-law.
[13] This is widely supported by empirical research (e.g., Simon and Arndt 1980 or Vakratsas and Ambler 1999).
[14] Another recent application of the consequences of fading memory is Nagel and Xu (2021), who study asset pricing when investors have a fading memory.

$$\Pi(I,\beta,\mathrm{t}) = \left(\beta + \Theta\eta^{t-1} ln\left(\frac{s_i(I) + s_b(\beta) + S_0}{S_0}\right)\right) \cdot \sum_{j=1}^{k}\left(m_j^2 + \frac{\sigma_j^4}{\sigma_j^2 + (1-I)\cdot\sigma_{\varepsilon_j}^2}\right).$$

Note that attention guidance thus reinforces both the direct effects of the bonus and the provided information, as salience increases in I and β. However, the guidance of attention weakens the complementarity between the two practices. To see that, consider how information provision changes the profit effect of introducing a bonus $\Delta_{\mathrm{B}}\Pi(\mathrm{I}) =$

$$\underbrace{\left(\beta + \Theta\eta^{t-1}\left(ln\left(\frac{s_i(I)+s_b(\beta)+S_0}{S_0}\right) - ln\left(\frac{s_i(I)+S_0}{S_0}\right)\right)\right)}_{\text{smaller when } I=1}\underbrace{\left(\sum_{j=1}^{k}\left(m_j^2 + \frac{\sigma_j^4}{\sigma_j^2+(1-I)\cdot\sigma_{\varepsilon_j}^2}\right)\right)}_{\text{larger when } I=1}.$$

When $\Theta = 0$, both practices are complements by *Proposition 1*. But when $\Theta > 0$, there is a countervailing substitution effect, as the first factor in the above expression is decreasing in I. The following result shows that, indeed, the two practices can become substitutes rather than complements if this effect is sufficiently strong. However, it also shows that this substitution effect weakens over time:

Proposition 2. *If the stimulus effects $s_i(1)$ and $s_b(\beta)$ are sufficiently strong, then*
(i) both practices are substitutes, i.e., $\Delta_{\mathrm{B}}\Pi(\mathrm{I}) < \Delta_{\mathrm{B}}\Pi(0)$,
(ii) and this substitution effect weakens over time, i.e., $\frac{\partial(\Delta_B\Pi(I)-\Delta_B\Pi(0))}{\partial t} > 0$.

Proof: See Appendix.

The reason for the substitution effect is simple: when both practices increase the salience of the performance objective, they serve a similar role in guiding attention to it. As there are decreasing returns to further stimuli, both practices are substitutes in this respect. This effect dampens the complementarity driven by standard incentive considerations. When the stimulus effects are large, the direct profit effect of each practice will be large and, at the same time, the marginal attention effect of adding the respective other practice will be small. As the formal result shows, these two mechanisms together can outweigh the complementarity established above.

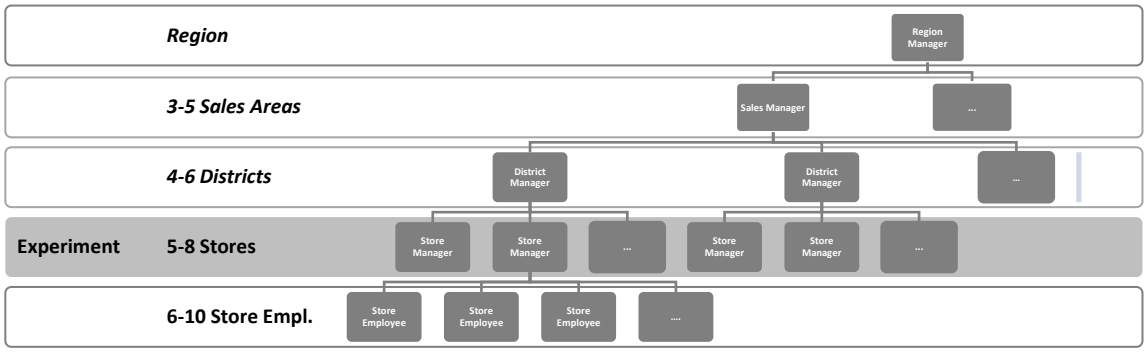
On a more general level, the results show that when two management practices increase the salience of the same specific underlying objective, diminishing sensitivity to the stimulus naturally makes the practices substitutes with respect to the attention generated for the

objective. However, the result also shows that this substitution effect should tend to vanish when time elapses as the "push" in attention generated by the introduction fades.

3 The Empirical Setting

The company in our study is a large nationwide discount retailer that operates 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 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 a store employs on average 6.6 full-time equivalent employees (FTE). The average tenure of a store manager is 14.17 years.

Figure 1 – Illustrative Organizational Chart



In discount retailing, tasks and processes are highly standardized, and store managers have only limited leeway in 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, refilling 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 store areas. The store managers' main tasks are defined in the job description used by the company, and a classification of these tasks is shown in Appendix A1.

As illustrated in Figure 1, store managers report directly 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's 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. The store managers see the absolute value of these KPIs per week and month as well as the respective planned values and their rank within the region. Thus, the store and district managers receive regular and detailed electronic performance feedback, which also allows district managers to monitor the store managers' performance closely.

Store managers have not received 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, stores were mostly evaluated by their sales performance and specific key figures, such as inventory losses. As explained above, store and district managers obtain weekly and monthly reports on these figures. One of the key conjectures arising from discussions with the company was that using a profit metric should increase the scope for managers to raise their performance (as, for instance, suggested in Bouwens and Van Lent 2007).[15] Although store managers were used to analyze the components of profits due to their regular electronic performance feedback, they rarely 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 precisely know the actual margins for different products, their leverage to raise profits was rather limited. Hence, we developed the idea of providing managers with information about

[15] 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 avoid 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).

profit margins, which constitutes the key element of our decision-facilitating information treatments.

4 The Experiment

From April 2017 to June 2017, store managers in the 362 stores in one region of Germany were randomly allocated to a control group and three treatment groups; they received decision-facilitating information, performance pay, or a combination of both. The underlying objective was to raise the following simplified profit metric which was communicated to the managers in the respective treatments[16]:

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

The metric excluded costs that store managers could not affect (such as investment expenditures, store rents, costs of logistics, and overhead costs). Thus, it provides 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.[17]

4.1 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 = 91	N = 88
	No Bonus	N = 92	N = 91

We used a stratified randomization (Athey and Imbens 2017) procedure based on the prediction of the districts' profits in the first treatment month. To construct the stratification

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

[17] To reduce personnel costs, store managers can actively manage their staff planning and the usage of temporary employees and employees on marginal part-time work (in the German tax and transfer system, firms can relatively easily employ people on the so-called "mini-jobs" for a few hours per week earning less than 480€ per month. Store managers have some leeway in employing such mini-jobbers.) 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.

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.[18] 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 within districts.[19] Table A2 in the Appendix shows the summary statistics and balancing of the treatment groups.[20]

Store managers in the treatment groups were notified about respective treatments and 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 respective treatments, which started on April 1, 2017. Importantly, the letters were in the corporate design of the company, signed by the HR responsible as well as the regional manager, and sent from the company's post office. The control group did not receive any notifications. District managers were briefed in written form on how to react to questions concerning the experimental design.[21]

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 addresses in February 2017 and in the last week of June 2017.[22] 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, the managers could not connect the surveys directly to the experiment.

4.2 Control Group

The control group was completely unaffected by our experiment and received no additional information. Thus, as described above, store managers in this group continued to receive the

[18] We had to randomize three months in advance as the data on profits came with a delay of one month and the central office needed the group composition early to implement the required operational processes.
[19] 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 knew each other) across district boundaries.
[20] We handled the randomization. However, we detected some differences between the 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.
[21] Exemplary letters to store and district managers are provided in the online Appendix.
[22] As surveys were sent out on June 26th, there was an overlap with the experimental period of at most 2 days.

weekly and monthly electronic performance feedback from the company accounting department but nothing in addition. They could access all relevant KPIs with their absolute value of the week/month and the development regarding the previous year. The only thing they were missing in comparison to the treatment groups in this respect was a direct report of a profit metric. Moreover, they saw the development regarding the planned KPI, the planned KPI for future months, and their rank within the region.

4.3 Treatment BONUS

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

$$\text{Bonus (in €)} = [\text{store profit} - (0.8 \cdot \text{planned value of store profit})] \cdot €0.05$$

Store managers, hence, received €0.05 for every €1 profit 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. The 80% threshold in the bonus formula thus assures that most store managers received a bonus and that increases in the profit metric linearly translated into bonus increases.[23]

Bonuses were accumulated, and cumulative bonuses were paid out after three months (capped at zero) together with the store managers' salaries. Note that it was possible to receive a negative bonus for a month, thereby reducing the amount gained in the bonus months. There had been no individual performance bonuses for store managers in this region before.[24]

The initial letter describing the treatment stated the bonus formula together with an extensive written explanation of this formula and an additional page showing an illustrative example of how the bonus would be calculated. For each of the three months, from April to June 2017, the store managers in this treatment group also received a personalized letter sent to the address of their private home.[25] The letter reported the achieved profit with all its components (sales, costs of goods sold, personnel costs, and inventory losses) of the previous

[23] Note that our conceptual framework does not entail a lower threshold in the bonus contract. In the experiment, we have tried to come as close to the linear contracting setting as feasible. The store managers' bonus was positive and increased linearly once they achieved 80% of the planned profits. Beyond this point, the bonus function had a constant slope (with no kink at the 100% and no cap). In total, more than 95% of managers achieved profits above the 80% threshold. Thus, the managers' marginal incentives were constant and strictly positive as in the formal model. Note also that all store managers knew the planned values of the components of the profit metric, even in the control group. Therefore, it is unlikely that non-monetary goal setting incentives affected the results.

[24] This is a crucial difference to other projects with this organization in different regions (see, e.g., Manthei et al. 2021, Manthei et al. 2022).

[25] 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 a 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.

month and the initially planned value. Moreover, managers received feedback on the bonus for the respective month and the already-accumulated bonus.

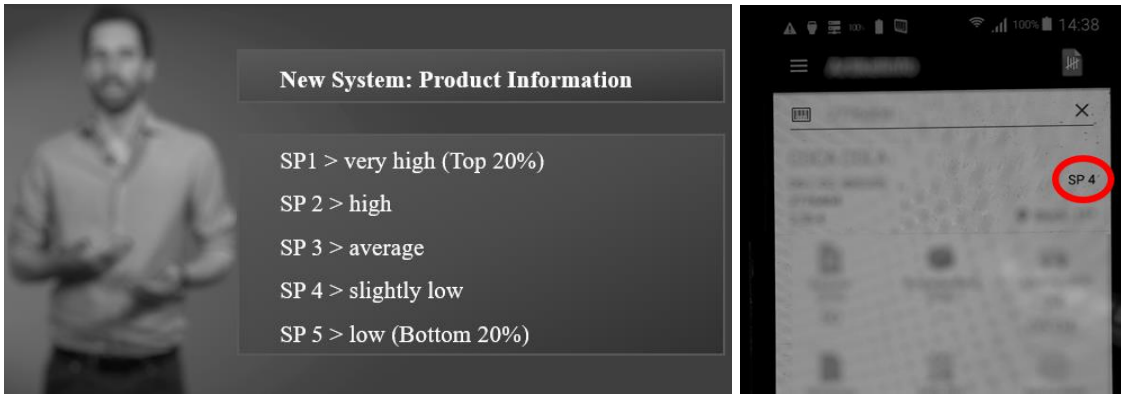
4.4 Treatment INFORMATION

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.[26] The initial letter describing the treatment stated the availability of an online training tool, the different possibilities to access the online training tool and the additional electronic feedback on the profit components.

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 never disclosed specific margins to store managers before the experiment. To provide information about margins without giving precise information that may then leak 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, such as smartphones, with barcode scanners that are commonly used in retailing to immediately provide all product-related information and allow for quick ordering. Store managers, therefore, had instant access to information by scanning a product.

[26] As one of the authors was the trainer in the video clip and we scripted it, we had full control over the content and the transmission of the video. Store managers were unaware that the trainer was part of the research team. We carefully ensured that it remained a video to transfer and brush-up knowledge and not to motivate employees. Hence, the training entirely consisted of information and explanations and avoided specific appeals. We further avoided to additionally motivate employees by ensuring not to use motivational language or motivational gestures. A screenshot of the video is displayed in Figure 2. An excerpt of the video script is provided in the online Appendix.

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 the circle).

The electronic performance report informed store managers about the profits achieved for the previous month and its 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 stores. Different from the *BONUS* treatment (and the control group), in which managers received their regular reports containing the planned values for sales, costs of goods sold, personnel costs, and inventory losses separately, managers in the *INFORMATION* group thus received one single overview of these figures as well as the resulting store profit. 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), it was ensured that managers who watched the video knew the margin categories. Those who did not watch the video could still inform themselves about the margin categories in 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.

4.5 Treatment BONUS&INFORMATION

This treatment combined individual monetary performance pay (*BONUS*) and information provision (*INFORMATION*). It was conducted along the lines described above.

5 Results

5.1 Empirical Approach

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

$$Y_{s,t} = \beta_0 + \beta_1 \cdot Bonus_{s,t} + \beta_2 \cdot Information_{s,t} + \beta_3 \cdot Bonus \times Information_{s,t} + \gamma \cdot X_{s,t} + a_s + \delta_t + \varepsilon_{s,t} \quad (1)$$

where $Y_{s,t}$ is the profit in month *t* for store *s*, $X_{s,t}$ includes time-variant controls (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. As we drop observations of store managers who are not eventually assigned to a treatment, store fixed effects can also be considered store manager fixed effects. In some specifications, we also include district manager fixed effects. *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* indicates the interaction between the bonus and information provision. Therefore, a positive estimate of β_3 shows a complementarity between both practices, and a negative estimate indicates that both are substitutes. We used the periods from the beginning of the previous year to the end of the experiment (January 2016 through June 2017, 18 months) to estimate the fixed effects.

At some points in the paper, particularly in graphical illustrations, more accessible interpretations can be provided by estimating treatment effects. In these cases, we used capital letters indicating the bonus treatment *BONUS*, the information treatment *INFORMATION*, and the treatment in which store managers received a bonus and information *BONUS&INFORMATION*.[27]

[27] Hence, *BONUS* and *INFORMATION* are equal to one only if an observation is part of the respective treatment where either only the bonus is used or only the information is provided but are equal to zero when an observation is from the treatment where the respective store manager received both.

5.2 Main Results

The key results are reported in Table 2. Column 1 displays the outcomes of a fixed-effects model according to Equation (1), with profits regressed on the *Bonus* and *Information* dummies and their interaction, 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.

The first key observation is that providing decision-facilitating information has a sizable average treatment effect on profits. In fact, the *Information* intervention raised profits on average by about €1000–€1200 (about 2%) per month per store. Hence, store managers productively used the decision-facilitating information, even without performance pay. 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% for the group receiving *Information* (using the estimates from Table 2, Column 2).[28]

Second, while point estimates for the *Bonus* intervention are also positive and significant, they tend to be smaller in magnitude than those for *Information*. However, they are never significantly different 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 overall bonus payment of €952.11 (SD = 758.75), which is approximately 30% 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 these thresholds, bonuses varied substantially (see Figure A1 in the Appendix, which shows the distribution of bonus payments per treatment group).

The third key result is that when studying the interplay between performance pay and the information intervention, we find no evidence for complementarity between both practices. In contrast, in all specifications, the point estimate of *Bonus*×*Information* is negative. Calculating the treatment effect of *BONUS&INFORMATION* as the sum of the point estimates of *Bonus*, *Information* and *Bonus*×*Information*, yields €1440 and, thus, falls short of the sum of the individual effects of the two practices when introduced separately. Hence, while the initial model based on standard economic reasoning predicts a strictly positive interaction term, we – if anything – find the opposite pattern.

[28] With a point estimate of € 1223 per month per store and a 3-month experiment with 92 stores participating, the overall effect would be € 337548. The cost of shooting the video was €6000. Hence, the return on investment is about 337548/€6000* 100 = 5625.8%.

To summarize our main empirical results, we find empirical support for both, (i) the importance of providing decision-facilitating information and (ii) the performance effects of bonus payments when both practices are implemented separately. In particular, we show that managers react to decision-facilitating information, even without performance pay.[29] However, we do not find evidence of complementarity between the two practices. The added value of performance pay appears to be rather small, and point estimates even tend to indicate a substitutional relationship. As discussed above in the conceptual framework, a potential explanation for this finding is that both practices are substitutes in generating attention for the overall objective of raising profits. We explore this idea in the following section in more detail.

[29] 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' behaviors.

Table 2 – Main Effects on Gross Profits

	(1) Profits	(2) Profits
Bonus	835.9**	1006.6**
	(410.0)	(431.2)
Information	1085.0**	1170.1**
	(469.7)	(497.4)
Bonus × Information	-604.0	-737.4
	(723.8)	(777.2)
Planned Profits	0.411***	0.414***
	(0.0487)	(0.0493)
Refurbishment Ongoing	-2809.9***	-2781.6***
	(609.7)	(613.4)
After Refurbishment	-605.0	-616.1
	(419.0)	(425.7)
Time FE	Yes	Yes
Store FE	Yes	Yes
District Manager FE	No	Yes
N of Observations	5958	5958
N of Stores	362	362
Cluster	56	56
Adj R^2	0.9309	0.9306

Note: The table reports results from fixed effects regressions with profits on the store level as the dependent variable. The regression accounts for time and store fixed effects in Column 1 and adds fixed effects for district managers in Column 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 (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 values. Observations were excluded once a store manager switched to the store during the treatment period or from store managers who were not assigned to a treatment. Robust standard errors are clustered at the district level of the treatment start and displayed in parentheses.* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

5.3 Attention Guidance or Information Provision?

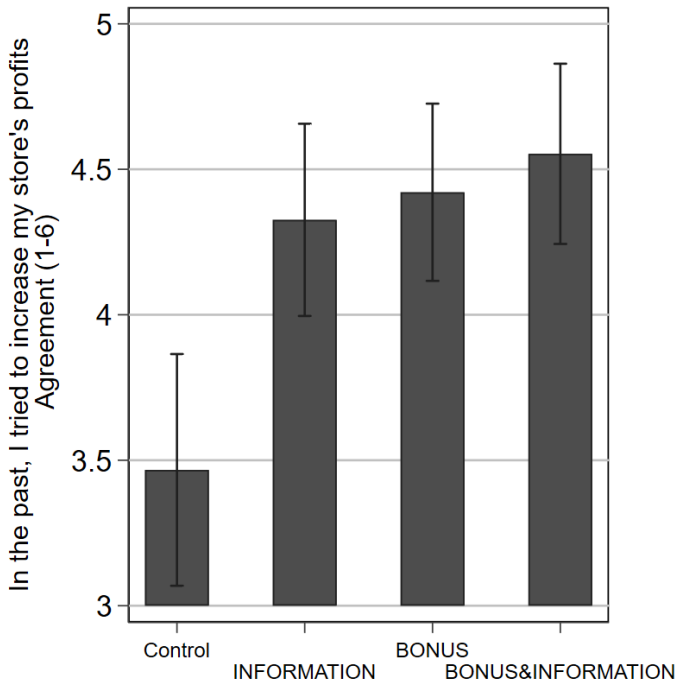
The conceptual framework illustrated different channels through which the treatments affected performance: (i) by providing information and incentives and (ii) by guiding attention toward the profit metric. As illustrated in the model, the latter mechanism can rationalize the absence of complementarity between the two practices. Therefore, we now explore this in more detail. In particular, we investigate store managers' perceptions measured by a post-experimental survey, the timing of the treatment effects, as well as differential effects depending on participation in the online training.

5.3.1 Attention to the Overall Objective of Raising Profits

It is important to recall that prior to the intervention, store managers' performance had been mainly assessed through tracking revenues as well as specific cost key figures rather than overall profitability. Hence, all three treatments may have drawn their attention to the store's profit contribution as a key objective. As shown in our conceptual framework, such a process can have a naturally generated substitution effect, at least in the short term.

As a first step, we thus investigate a direct but subjective measure of a manager's attention on profits as a salient objective. Our online post-experimental questionnaire (participation rate 53.87%) included an item that asked store managers about their own perceived strength of intention 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). As Figure 3 shows, all three interventions create substantial self-reported attention for increasing store profits. Responses in all three treatments are highly significantly different from the control group's responses (MWU, all $p < 0.01$). More importantly, and in line with the idea developed in Section 2.2.2 that treatments are substitutes for generating attention due to diminishing sensitivity, the treatment effects do not vary substantially. Although subjective attention to store profits is the highest in the *BONUS&INFORMATION* treatment, it only slightly and insignificantly exceeds attention in both the mere *BONUS* and the *INFORMATION* treatments. Therefore, the pattern supports the view that the treatments are substitutes rather than complements in generating attention for the aim of increasing profits.

Figure 3 – Attention on Profit Increases



Note: The figure displays mean agreement (on a scale from 1 = not agree at all to 6 = completely agree) with the statement "In the past, I tried to increase my store's profits." depending on the different treatment groups. N = 195. Error bars are displayed.

5.3.2 Timing of the Treatment Effects

The previous section already provided evidence in line with the idea that the interventions also affected performance by generating attention for the objective of raising profits and that both are substitutes in this respect. The study of the dynamics of the treatment effects allows for a further exploratory test of this mechanism: as *Proposition 2* shows, if there is indeed a substitution effect driven by attention guidance, (i) it should be particularly prevalent shortly after the introduction of the treatments and (ii) it should weaken over time.

In this section, we investigate the dynamic nature of interdependency. We start by estimating time trends in the effects of the *Bonus* and *Information* intervention, as well as in their interaction *Bonus*×*Information*. Table 3 displays estimates from our baseline regression further interacting the interventions with the variable *Treatment Time* measuring the time elapsed since the start of the respective intervention (i.e., running from 0 in the first month to 2 in the last). The intervention dummies hence estimate the respective effects in the first month and the time interactions' respective time trends in the intervention effects.

Table 3 – Effects over Time

	(1) Profits	(2) Profits
Bonus	1562.6***	1733.5***
	(457.0)	(453.2)
Bonus × Treatment Time (0,1,2)	-729.8	-730.1
	(451.4)	(453.8)
Information	1560.0***	1644.1***
	(551.8)	(544.4)
Information × Treatment Time (0,1,2)	-475.9	-474.8
	(443.0)	(445.8)
Bonus × Information	-1666.5**	-1798.2**
	(772.9)	(800.3)
Bonus × Information × Treatment Time (0,1,2)	1066.3*	1064.7*
	(621.7)	(625.5)
Planned Profits	0.411***	0.414***
	(0.0486)	(0.0492)
Refurbishment Ongoing	-2808.6***	-2780.1***
	(606.3)	(609.7)
After Refurbishment	-598.2	-609.0
	(418.6)	(425.0)
Time FE	Yes	Yes
Store FE	Yes	Yes
District Manager FE	No	Yes
N of Observations	5958	5958
N of Stores	362	362
Cluster	56	56
Adj R^2	0.9309	0.9306

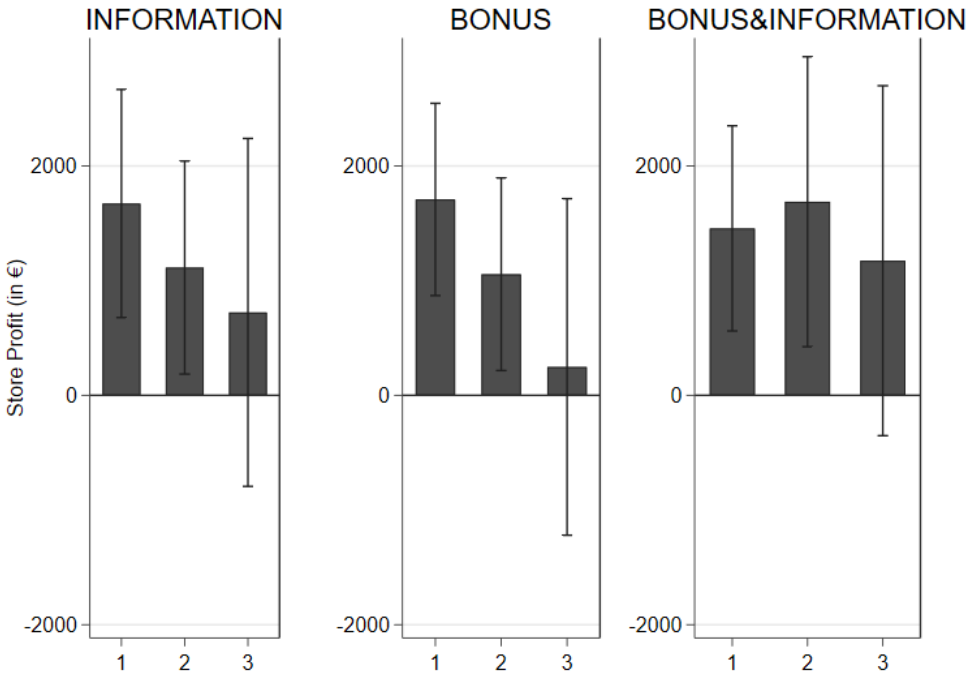
Note: The table reports results from fixed effects regressions with profits on the store level as the dependent variable. The regression accounts for time and store fixed effects in Column 1 and adds fixed effects for district managers in Column 2. *Treatment Time (0,1,2)* is a variable equal to 0 for the 1st treatment month, 1 for the 2nd treatment month, and 2 for the 3rd treatment month. 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 values. Observations were excluded once a store manager switched to the store during the treatment period or from store managers who were not assigned to a treatment. Robust standard errors are clustered at the district level of the treatment start and displayed in parentheses.* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

First of all, both the *Information* and the *Bonus* interventions have sizeable effects in the first month. Moreover, the coefficient of *Bonus*×*Information* shows that there is indeed a large and significant substitution effect between the two practices early on. This is consistent with claim (i) in *Proposition 2* of our conceptual framework, according to which the two practices can become substitutes when their attentional stimulus is strong enough (i.e., $\Delta_B \Pi(I) - \Delta_B \Pi(0) < 0$ in the notation of our conceptual framework). Moreover, the (insignificantly)

negative time interactions for the two intervention dummies are in line with a fading of attention after the initial attention push. Finally, in line with claim (ii) in *Proposition 2* (according to which $\frac{\partial(\Delta_B \Pi(I) - \Delta_B \Pi(0))}{\partial t} > 0$), the interaction term becomes less negative over time: the coefficient of *Bonus*×*Information*×*Treatment Time* is significantly positive. Hence, the nature of the interdependency between the two practices also changes over time, consistent with *Proposition 2*: 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 toward being complements.

Figure 4 displays the estimated monthly effects of the treatments *INFORMATION*, *BONUS*, and *BONUS&INFORMATION* (the respective regression estimates are reported in Table A4 in the Appendix), again showing that the treatment effects become weaker over time in *INFORMATION* and *BONUS*, but the fading effect tends to be weaker in the combined intervention *BONUS&INFORMATION*. Hence, the timing of the treatment effects is in line with the patterns predicted by an attention-guiding effect. Both practices have strong initial effects on performance, but these effects become weaker over time. Concurrently, their interdependence moves in the opposite direction.

Figure 4 – Treatment Effects Over Time

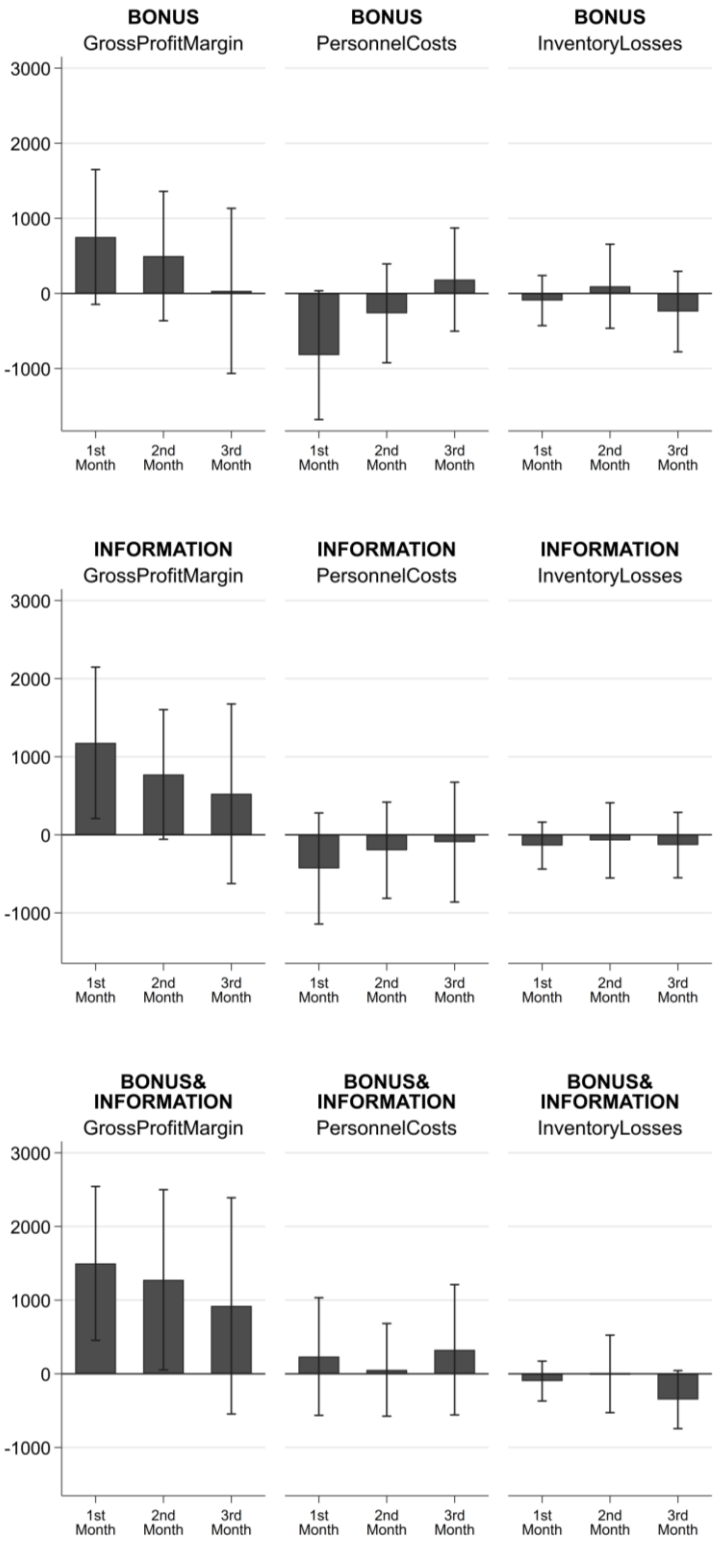


Note: The figure displays treatment effects from a fixed-effects regression, with the profits on the store level as the dependent variable. The regression accounts for time and store fixed effects and fixed effects for district managers (the specification is the same as in our main table, Table 2, Column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

While the fading of attention for the *Information* intervention seems very natural and well in line with results from other information interventions studied in the literature, the strong fading effect that we also observe for the bonus (where store managers received monthly reward feedback) may appear a bit puzzling. While it is consistent with some previous findings—such as Sandvik et al. (2020), who also found bonus effects decreasing over time—it appears to contradict results, such as, for instance, from the classical study on performance pay by Lazear (2000), who even detected increasing performance effects. To investigate this in more detail, we estimate the treatment effects for the three profit components (gross margin, personnel expenses, and inventory losses) separately over time. As Figure 5 shows, roughly half of the profit effect of *BONUS* in the first month is driven by a substantial reduction in personnel expenses. Therefore, it seems likely that the combination of monetary incentives and the attention push on profits induces store managers to reduce personnel costs substantially (as they did not receive decision-facilitating information on profit margins that may have appeared as the natural lever to raise profits), and this backfires in subsequent months. In line with this explanation, we, for instance, find that when regressing gross margins in a given month t on

personnel costs in $t-1$ in a fixed-effects model, lagged personnel costs exhibit a strongly significant negative sign (Table A5 in the Appendix). Hence, it appears likely that the strong negative time trend in the *BONUS* treatment is not entirely due to a fading of attention but also driven by an initial (and rather short-sighted) reduction in personnel costs, which then backfired.

Figure 5 –Effects Over Time



Note: The figure displays treatment effects from a different fixed-effects regression, with the gross profit margin, personnel costs, or inventory losses at the store level as the dependent variable. The regression accounts for time and store fixed effects and fixed effects for district 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.

5.3.3 Training Participation and Treatment Effects

As described above, the *INFORMATION* intervention consisted of different elements, one of which was online training on the measurement of profit and its components. Not all store managers participated in this online video training, and it seems likely that some felt already informed about profits beforehand or collected information about the profit metric independently (the profit margin information on the portable data terminals was also accessible for non-participants in both *INFORMATION* and *BONUS&INFORMATION*).

The actual participation in the online training can have affected store managers' performance through the two mechanisms laid out in the above conceptual framework. First, the training may have communicated further decision-facilitating information. In addition, it may have provided a stimulus, directing managers' attention toward the profit objective. We now make use of the fact that we can track whether and when store managers participated in this training to explore the relevance of the two mechanisms.

From both, the decision-facilitating and the attention-guidance perspectives, training participation should come along with larger treatment effects among training participants in the *INFORMATION* treatment. In the *BONUS&INFORMATION* treatment the bonus itself should have already created substantial attention for the profit objective. From an attention guidance perspective, actual training participation should therefore matter less for performance in *BONUS&INFORMATION* due to diminishing sensitivity. However, if the training affects performance mainly through the provision of decision-facilitating information, we should see sizeable profit differences between participants and non-participants of the online training, even when there is a bonus.

To study this, we replicate our baseline regression interacting the *Information* and the *Bonus*×*Information* dummies with another dummy variable, indicating whether the respective store managers watched the online video or not.

Table 4 – Effects by Video Training Participation

	(1) Profits	(2) Profits
Bonus	836.1**	1006.9**
	(410.1)	(431.3)
Information	-218.0	-231.2
	(866.3)	(860.2)
Information × Video Participation	1627.3*	1743.2*
	(915.5)	(913.6)
Bonus × Information	492.1	544.7
	(966.1)	(1003.7)
Bonus × Information × Video Participation	-1328.0	-1572.8
	(1170.2)	(1169.5)
Planned Profits	0.408***	0.412***
	(0.0485)	(0.0489)
Refurbishment Ongoing	-2780.1***	-2749.2***
	(590.9)	(590.6)
After Refurbishment	-594.8	-610.8
	(423.2)	(429.6)
Time FE	Yes	Yes
Store FE	Yes	Yes
District Manager FE	No	Yes
N of Observations	5958	5958
N of Stores	363	363
Cluster	56	56
Adj R^2	0.9310	0.9306

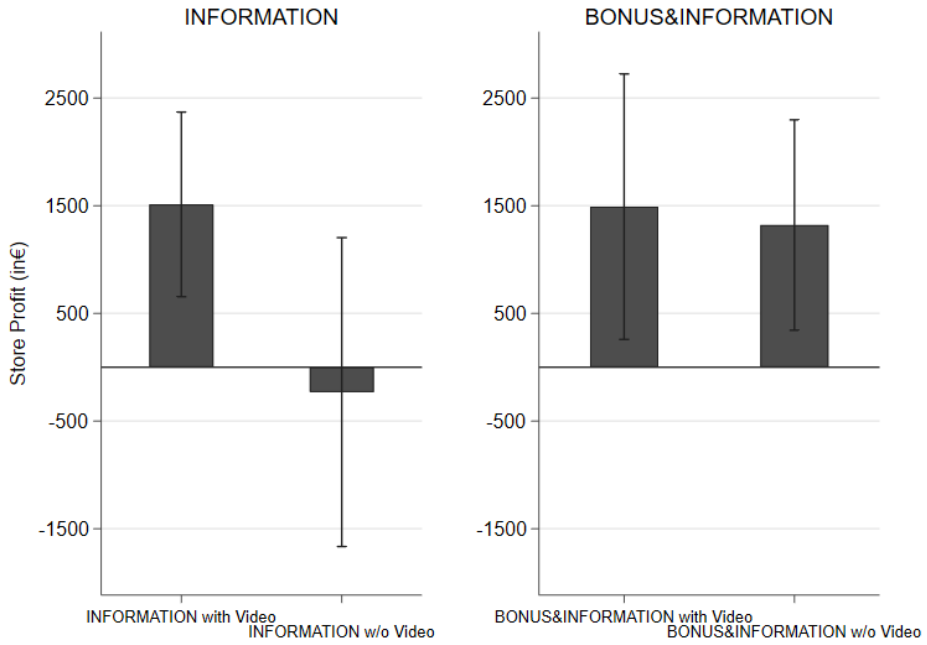
Note: The table reports results from fixed effects regressions with the profits on the store level as the dependent variable. The regression accounts for time and store fixed effects in Column 1 and adds fixed effects for district managers in Column 2. *Video Participation* is a dummy variable equal to 1 if the store manager participated in the video training and 0 otherwise. 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 values. Observations were excluded once a store manager switched the store during the treatment period or from store managers who were not eventually assigned to a treatment. Robust standard errors are clustered at the district level of the treatment start and displayed in parentheses.* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

As Table 4 shows, the *Information* intervention increase profits only among the store managers who participated in the training, which is in line with both explanations. Importantly, when store managers receive the *Bonus*, profits increase irrespective of whether the manager watched the online video or not.

This is illustrated in Figure 6, which shows the estimated treatment effects for the *INFORMATION* and *BONUS&INFORMATION* treatment groups split-up in the effect among training participants and non-participants. Although performance effects hinge on training participation in the *INFORMATION* treatment, they do not in *BONUS&INFORMATION*. These

results support the view that training participation indeed mostly affected performance by generating attention to the overall objective.

Figure 6 – 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, 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.

Although we caution that statistical power is limited here, it is also interesting to observe that the positive point estimate of the *Bonus*×*Information* interaction term in Table 4 indicates a complementarity between the two practices among those store managers who did not watch the video. A possible interpretation in light of our conceptual framework is that in the case of non-participation, the stimulus contribution of the *Information* intervention (denoted as $s_i(I)$ in the notation of the model) is not sufficiently strong to reverse the complementarity between the two practices, which is only reversed among the store managers who watched the video.

Interestingly, we also find a treatment difference in the likelihood that managers participate in the training. While 80.43% of the store managers participated in the *INFORMATION* treatment, only 68.13% did so in *BONUS&INFORMATION* (MWU, p = 0.0575). A likely explanation is that the salience of the online training was larger in the *INFORMATION* treatment compared to *BONUS&INFORMATION*, where store managers were informed about

both elements in the same letter and thus at the same point in time.[30] Hence, when introduced in isolation the training not only induces more attention for the profit metric but also receives more attention in itself.

As a final piece of evidence on the question of whether the training affected performance in the *INFORMATION* treatment through the generation of attention, we explore the association between the point in time a store manager undertook the training and later performance. To this end, we study the association between store profits in the two months after the experiment and the number of days elapsed since the respective store managers had watched the training video. It is conceivable that, generally, more motivated managers tended to watch the training video earlier, which may lead to larger long-term performance gains for timely training participants. If, however, training participation generated attention, which then faded over time, we should observe the opposite pattern: the earlier the manager had attended the training, the smaller should be the long-term performance. Indeed, we find that in the *INFORMATION* group, profits above the planned profits in the months after the experiment are significantly negatively correlated with the number of days elapsed since the store manager attended the training (Spearman rank correlation, rho = -0.1714, p = 0.0367). Interestingly, we do not find such a correlation in the *BONUS&INFORMATION* group (Spearman rank correlation, rho = -0.0129, p = 0.8885), which is in line with the patterns discussed in the above indicating that the training itself induced stronger attention on the profit metric when introduced in isolation.[31]

5.3.4 The Use of Decision-Facilitating Information

The previous findings support the view that the interventions influenced performance by generating attention for the objective of raising profits. The question remains whether the interventions affected performance beyond this channel and, in particular, whether the *Information* intervention still provided decision-facilitating information that was actually used by managers. Recall that an important element of the *Information* intervention is the provision of knowledge about profit margins of individual products, which should help managers to focus on more profitable products.

[30] The timing of participation provides additional support. Note that store managers could access the video at the end of March 5 days before the bonus period started. While the participation rate in the month immediately before the bonus period was slightly and insignificantly larger in *BONUS&INFORMATION* than in *INFORMATION* (24.44% versus 20.88%, Fisher's exact p-value = 0.598), significantly more store managers in *INFORMATION* than in *BONUS&INFORMATION* watched the video when the bonus period officially began (57.61% versus 43.96%, Fisher's exact p-value = 0.077). This is the case even though the material incentive to watch the video should have been larger in *BONUS&INFORMATION*.

[31] See also Table A6 for a corresponding regression analysis.

The first piece of evidence was given above when exploring financial data on the different components of our profit metric (e.g., *gross profit margin*, *personnel costs*, *and inventory losses*). Figure 5 shows that the different treatments led to a focus on different channels through which managers increase profits. In the *BONUS* treatment, where managers lacked explicit information on the relative profitability of different products, profit growth is driven by a pronounced reduction in personnel costs, while the share of the profit increases achieved through higher gross margins was comparably low. In the other two treatments, where store managers were provided with knowledge about profit margins, profit growth is driven by increases in the gross margin.

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 in the bottom margin categories in the *INFORMATION* and *BONUS&INFORMATION* treatments (see Table A7 in the Appendix). We did not find any evidence that the store managers in *BONUS* managed to increase profits substantially in these categories.

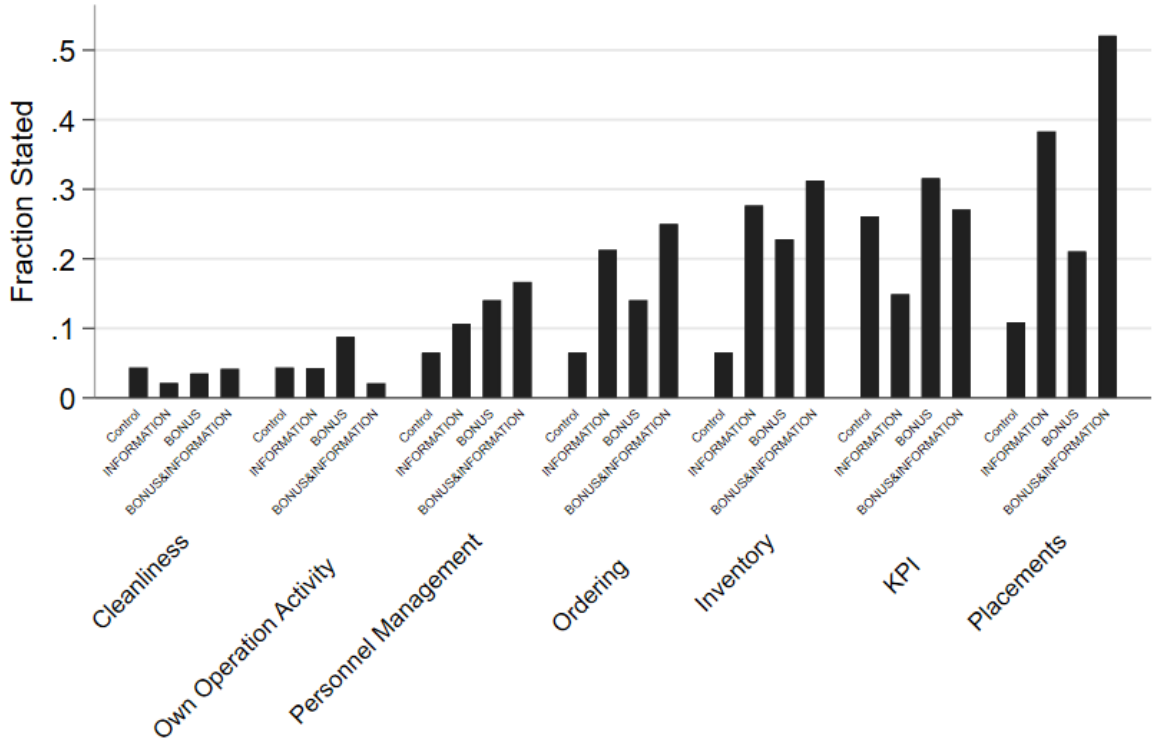
In the next step, we investigate which actions the managers actually undertook to raise profits by their own accounts using responses to a post-experimental questionnaire. We had invited store managers to participate in an online questionnaire close to the end of the experiment (participation rate 53.87%). The questionnaire, for instance, included open questions asking store managers what they had actually done to increase profits in previous months. We used a task classification developed and applied by the firm for the formal job descriptions of store managers (Table A2), and two research assistants independently mapped the statements to this task classification.[32] We display the results of categorizing these tasks into seven general task dimensions. Figure 7 shows the fraction of stores for each task dimension, in which at least one of the research assistants assigned a statement to a specific task in the respective dimension.[33] A first observation is that the placement of goods is the most important dimension that store managers mention when asked about activities implemented to raise profits. Frequently, store managers stated that they made secondary placements of high-

[32] Importantly, the research assistants were asked to categorize all statements into the task classification used by the firm. Figure A2 in the Appendix shows the more detailed split into finer subclasses of tasks. The average Cohen's Kappa is 0.64 and can thus be interpreted as substantial (McHugh 2012).

[33] 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.

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).

Figure 7 – Task Focus to Increase Profits (Open Questions)



Note: The figure displays the fraction of stated task dimensions to increase profits obtained from the open-ended 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 = 198.

Notably, placements stand out only in treatments with additional information on product margins. In fact, 38% of the survey respondents in the *INFORMATION* and 52% 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 react to the novel information on profit margins and do so particularly 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.

Finally, it is also interesting to consider the activities managers exerted in the *BONUS* group (relative to the control group). Here, if anything, the survey data indicate shifts in the focus on

personnel management and inventory losses, which complements the above finding that managers focus considerably on more easily manageable profit components when they do not have access to information on profit margins.

Hence, the *Information* intervention not only pushed attention toward the profit metric (and in this respect was a substitute for the *Bonus* intervention), but it also raised performance by providing relevant decision-facilitating information.

6 Conclusion

We report a firm-level field experiment to study the performance effects of providing decision-facilitating information and implementing performance pay as well as their interaction. Investigating the average treatment effects, we provide evidence for the importance of providing decision-facilitating information and using bonuses to influence decisions in the same field setting. Both interventions substantially increase profits. Notably, point estimates of the impact of the information provision treatment exceed those for the performance pay treatment, although the intervention came at much lower costs.

In contrast to our ex-ante hypothesis based on standard economic reasoning, we find no overall complementarity between the two practices. Moreover, the effects of the separate interventions (performance pay or information provision) are particularly strong in the first month and then tended to decrease over time. At the same time, the interdependency between the two practices moves 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 a 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 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 about the relative profitability of specific tasks can be highly beneficial. Second, this can work even in the absence of explicit incentives. Employees productively used the information provided in our setting, although this generated no direct monetary payoffs for themselves. Third, however, these effects tend to fade over time. That is, introducing management control practices can entail substantial attention-directing effects that decline in the long term.

Hence, our results 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 can be 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 that guide attention. For instance, it is conceivable that a redesign of management control can have value in itself, as it may renew awareness of 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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8 Appendix

8.1 Proof of Proposition 2

Let $Q_1 = ln\left(\frac{s_i(1)+s_b(B)+S_0}{S_0}\right) - ln\left(\frac{s_i(1)+S_0}{S_0}\right)$ and $Q_2 = ln\left(\frac{s_b(B)+S_0}{S_0}\right)$ and consider $\Delta_B\Pi(I) - \Delta_B\Pi(0)$, which is then equal to

$$(\beta + \Theta\eta^{t-1}Q_1) \cdot \sum_{j=1}^{k}\left(m_j^2 + \sigma_j^2\right) - (\beta + \Theta\eta^{t-1}Q_2) \cdot \sum_{j=1}^{k}\left(m_j^2 + \frac{\sigma_j^4}{\sigma_j^2+\sigma_{\varepsilon_j}^2}\right) \qquad \text{(A1)}$$

First, note that Q_2 is strictly increasing in $s_b(B)$ and $\lim_{s_b(B)\to\infty} Q_2 = \infty$. Now note that

$$\frac{\partial Q_1}{\partial s_i(1)} = \frac{S_0}{s_i(1)+s_b(B)+S_0} - \frac{S_0}{s_i(1)+S_0} < 0$$

and

$$\lim_{s_i(1)\to\infty} Q_1 = \lim_{s_i(1)\to\infty}\left(ln\left(\frac{s_i(1)+s_b(B)+S_0}{S_0}\right) - ln\left(\frac{s_i(1)+S_0}{S_0}\right)\right) = 0$$

such that for any given value of $s_b(B)$, we have Q_1 strictly decreasing in $s_i(1)$ and converging to 0. Hence, we have $\Delta_B\Pi(I) - \Delta_B\Pi(0) < 0$ whenever both individual stimuli are sufficiently large, which establishes claim (i).

For proof of claim (ii), note that $\frac{\partial(\Delta_B\Pi(I)-\Delta_B\Pi(0))}{\partial t}$

$$= \Theta\eta^{t-1}\ln(\eta)\, Q_1 \cdot \sum_{j=1}^{k}\left(m_j^2 + \sigma_j^2\right) - \Theta\eta^{t-1}\ln(\eta)\, Q_2 \cdot \sum_{j=1}^{k}\left(m_j^2 + \frac{\sigma_j^4}{\sigma_j^2+\sigma_{\varepsilon_j}^2}\right),$$

which is strictly positive whenever $s_i(1)$ is sufficiently large and thus Q_1 sufficiently small (as $\ln(\eta) < 0$). ■

8.2 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 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	34204.45	32674.83	33200.34	35052.03	35937.58
	(14556.21)	(14049.27)	(12568.43)	(15076.67)	(16308.45)
Planned Profits Jan–Mar ’17	34517.16	33103.80	33652.66	35738.41	35626.21
	(13757.99)	(13246.95)	(11953.66)	(15292.48)	(14402.47)
Refurbishment Ongoing Mar ’17	0.04	0.05	0.04	0.06	0.02
	(0.21)	(0.23)	(0.21)	(0.23)	(0.15)
After Refurbishment Mar ’17	0.62	0.66	0.54*	0.68	0.59
	(0.49)	(0.48)	(0.50)	(0.47)	(0.49)
Female Store Manager (Y/N)	0.69	0.76	0.65	0.60*	0.73
	(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	6.60	6.45	6.65	6.80*	6.55
	(1.38)	(1.17)	(1.33)	(1.56)	(1.42)
Age of Store	16.41	17.63	16.57	17.44	14.04**
	(9.72)	(10.47)	(9.86)	(10.10)	(8.02)
Age Store Manager	43.09	44.57	43.52	41.15**	43.03
	(10.81)	(10.95)	(10.55)	(10.79)	(10.86)
Tenure Store Manager	14.17	15.51	14.23	13.01**	13.91
	(8.70)	(8.43)	(8.64)	(7.73)	(8.86)
Store Space	695.49	701.70	679.03	693.33	708.80
	(134.22)	(112.95)	(143.24)	(121.67)	(154.76)
Observations	362	91	92	88	91

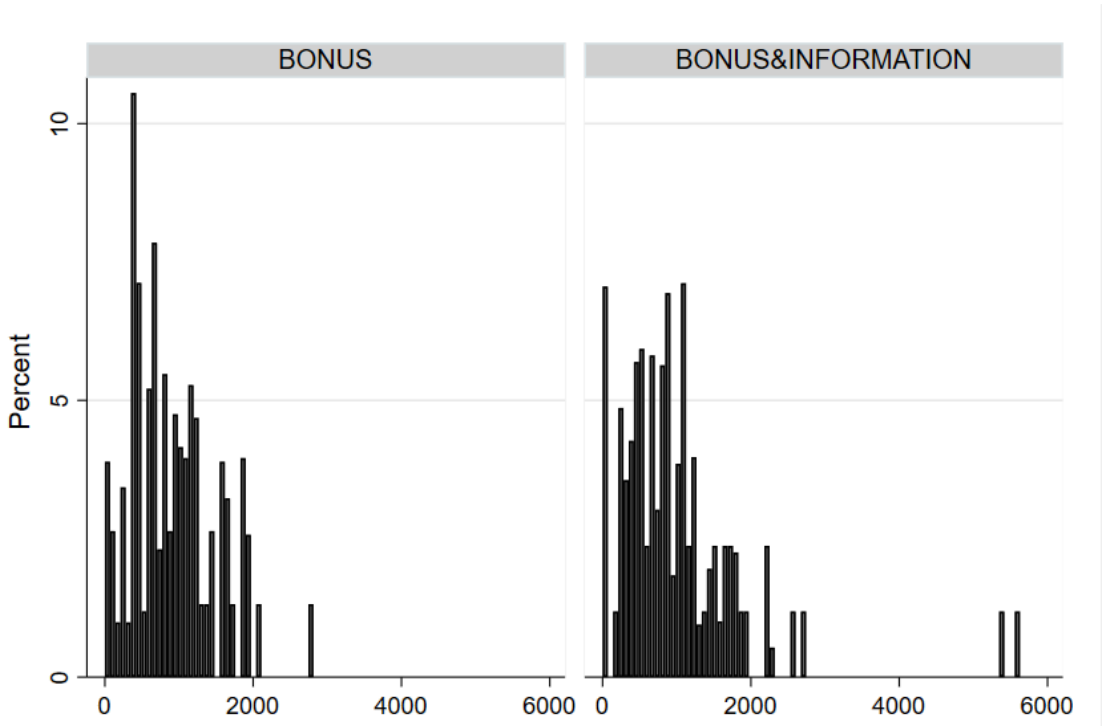
Note: The table reports means of the respective variables for the different treatment groups and their standard deviations in parentheses. Asterisks display significance levels from t-tests (fisher’s exact test for binary variables) of the respective treatment group against the control group. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A3 – Regressions only using Treatment Period

	(1) Profits	(2) Profits
Bonus	1013.4**	921.0**
	(396.1)	(387.4)
Information	1029.2**	1143.9***
	(481.7)	(364.6)
Bonus × Information	-473.0	-804.8
	(682.7)	(562.3)
Planned Profits	0.122	0.153
	(0.0928)	(0.102)
Refurbishment Ongoing	-4439.9***	-3656.8***
	(1022.1)	(1020.7)
After Refurbishment	-33.90	-4.325
	(336.7)	(377.3)
Time Controls	Yes	Yes
Further Controls	No	Yes
N Observations	1075	1059
N Stores	362	356
N Cluster	56	56
Overall R^2	0.9227	0.9282

Note: The table reports results from ordinary least squares estimations with profits at the store level as the dependent variable. 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' planned profits. Column 2 further controls for variables with slight imbalances 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 or from store managers who were not eventually assigned to a treatment. Robust standard errors are clustered at the district level of the treatment start and displayed in parentheses.* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure A1 – Histogram of Final Bonus Payments



Note: The figure displays the total bonus payment amounts over the three month period paid to managers in the *BONUS* and *BONUS&INFORMATION* treatments.

Table A4 – Monthly Treatment Effects Using Interactions

	(1) Profits	(2) Profits
BONUS 1st Month	1536.6***	1707.9***
	(472.4)	(501.0)
BONUS 2nd Month	885.5	1055.6**
	(543.7)	(501.7)
BONUS 3rd Month	76.86	247.4
	(832.0)	(877.0)
INFORMATION 1st Month	1587.8***	1672.3***
	(574.2)	(594.5)
INFORMATION 2nd Month	1029.3*	1113.7**
	(585.9)	(555.0)
INFORMATION 3rd Month	636.4	722.9
	(849.2)	(906.5)
BONUS&INFORMATION 1st Month	1331.8**	1455.4***
	(510.3)	(534.5)
BONUS&INFORMATION 2nd Month	1566.4**	1688.1**
	(754.9)	(755.5)
BONUS&INFORMATION 3rd Month	1051.6	1173.6
	(839.8)	(911.8)
Planned Profits, Refurbishments	Yes	Yes
Fixed Effects (Store, Time)	Yes	Yes
Fixed Effects (Store Manager, District Manager)	No	Yes
Observations	5958	5958
N Store	362	362
N Cluster	56	56
Overall R^2	0.9309	0.9305

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 in Column 1 and adds fixed effects for district managers in Column 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 values. Observations were excluded once a store manager switched the store during the treatment period or from store managers who were not eventually assigned to a treatment. Robust standard errors are clustered at the district level of the treatment start and displayed in parentheses.* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A5 – Correlation between Gross Product Margin and Personnel Expenses

	(1) Gross Profit Margin	(2) Gross Profit Margin
Personnel Expenses_t-1	0.0972**	0.0935**
	(0.0429)	(0.0431)
Refurbishment Ongoing	-2740.3***	-2718.1***
	(482.5)	(489.0)
After Refurbishment	679.4*	696.5*
	(405.5)	(412.3)
Time FE	Yes	Yes
Store FE	Yes	Yes
District Manager FE	No	Yes
N Observations	6354	6353
N Stores	369	369
N Cluster	56	56
Overall R^2	0.9685	0.9685

Note: The table reports results from a fixed effects regression with the gross profit margin at the store level as the dependent variable. The regression accounts for time and store fixed effects in Column 1 and adds fixed effects for district managers in Column 2. The fixed effects include lagged personnel expenses and use data from January 2016–August 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 values. Robust standard errors are clustered at the district level and displayed in parentheses.* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A6 – Time Elapsed since VTP

	(1) INFORMATION	(2) BONUS& INFORMATION
Time Elapsed	-64.35*	11.27
	(36.07)	(67.06)
Planned Profits	0.895***	0.972***
	(0.0551)	(0.0313)
Refurbishment Ongoing	-1512.1	-383.5
	(3142.1)	(2076.3)
After Refurbishment	-475.6	5336.8**
	(1155.6)	(2243.3)
Time FE	Yes	Yes
Further Controls	Yes	Yes
N Observations	137	110
N Stores	69	56
N Cluster	16	16
Overall R^2	0.8678	0.8566

Note: The table reports results from ordinary least squares estimations with profits at the store level as the dependent variable. *Time Elapsed* refers to the number of days elapsed since the respective store managers had watched the training video. All regressions further control for possible refurbishments of a store and the companies' planned profits. Moreover, they control for store space, the age of the store manager, and the performance evaluation of store managers. Observations were excluded once a store manager switched the store during the treatment period or from store managers who were not eventually assigned to a treatment. Robust standard errors are clustered at the district level of the treatment start and displayed in parentheses.* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

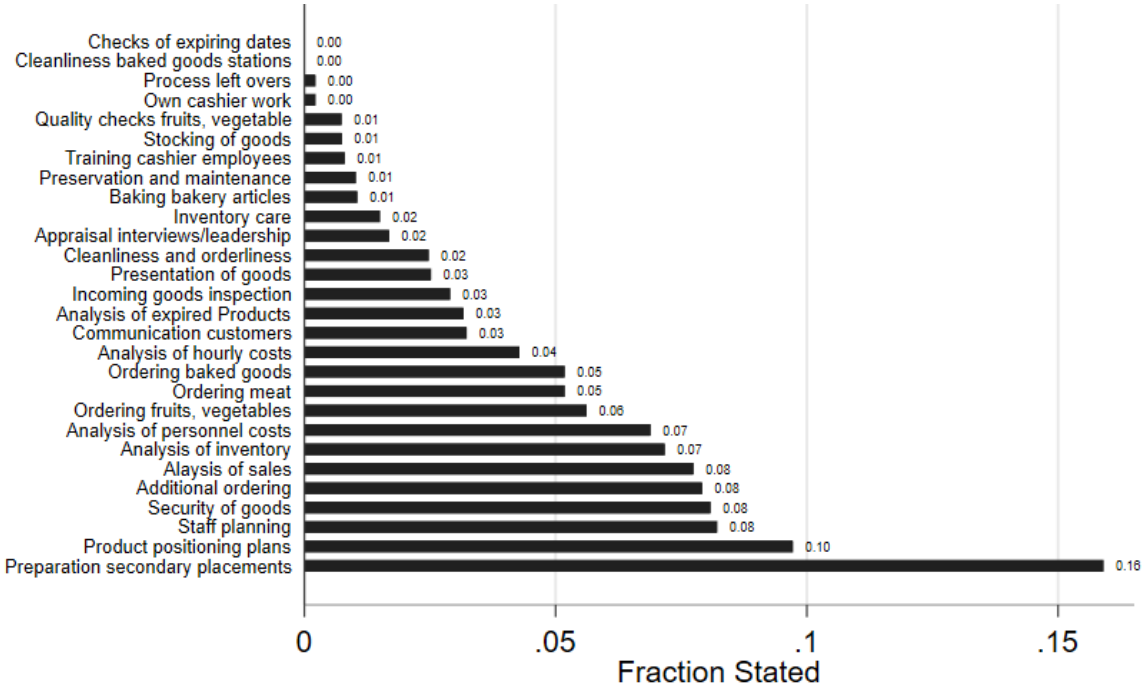
Table A7 – Treatment Effects depending on Product Margin Categories

Panel A –Sales	(1) 1st Cat	(2) 2nd Cat	(3) 3rd Cat	(4) 4th Cat	(5) 5th Cat
BONUS	108.1	39.64	195.1	-496.7	8.911
	(469.0)	(282.7)	(403.6)	(920.9)	(310.7)
INFORMATION	630.0	539.7**	872.7**	55.21	335.5
	(421.7)	(250.9)	(361.1)	(927.4)	(257.4)
BONUS&INFORMATION	665.9	543.5	882.7	483.5	372.0
	(535.5)	(435.4)	(555.0)	(1159.6)	(313.8)
Refurbishment Ongoing	459.0	-213.6	-903.1**	-653.5	44.61
	(511.9)	(341.5)	(366.8)	(829.8)	(313.3)
After Refurbishment	1211.5***	210.9	-255.8	541.1	179.1
	(372.6)	(233.9)	(302.0)	(619.8)	(258.1)
Fixed Effects (Store, Time, and District Manager)	Yes	Yes	Yes	Yes	Yes
Planned Values	Yes	Yes	Yes	Yes	Yes
Observations	5959	5959	5959	5959	5958
N Store	362	362	362	362	362
N Cluster	56	56	56	56	56
Overall R^2	0.9740	0.9755	0.9735	0.9663	0.9547

Panel B – Quantity of Products (in units)	(1) 1st Cat	(2) 2nd Cat	(3) 3rd Cat	(4) 4th Cat	(5) 5th Cat
BONUS	387.8	347.6*	842.4**	27.03	124.4
	(493.6)	(177.9)	(327.3)	(601.3)	(126.6)
INFORMATION	-91.37	159.3	212.9	-334.2	-3.041
	(528.2)	(164.9)	(340.2)	(606.6)	(146.4)
BONUS&INFORMATION	922.1	578.4*	1188.7*	810.5	178.0
	(560.6)	(327.8)	(596.8)	(632.2)	(162.4)
Refurbishment Ongoing	-1530.1***	-815.2***	-1988.1***	-1583.0***	-309.0***
	(472.0)	(218.9)	(312.8)	(399.9)	(113.7)
After Refurbishment	1462.4***	448.2**	-251.8	412.6	224.7**
	(493.8)	(206.8)	(291.5)	(437.5)	(102.3)
Fixed Effects (Store, Time, and District Manager)	Yes	Yes	Yes	Yes	Yes
Observations	5930	5930	5929	5930	5929
N Store	360	360	360	360	360
N Cluster	56	56	56	56	56
Overall R^2	0.9655	0.9685	0.9683	0.9671	0.9526

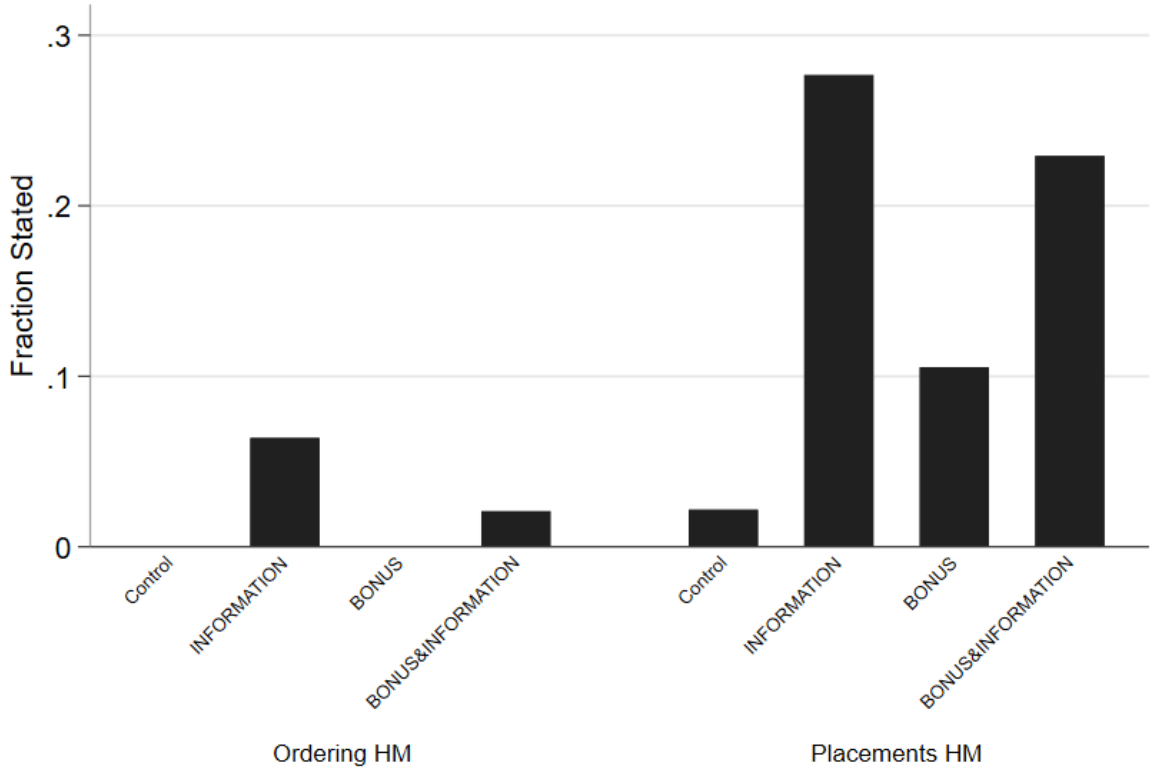
Note: The table reports results from fixed effects regressions with sales on the store level as the dependent variable. The different columns represent the different margin categories (e.g., 1st category = sales of the 20% of the products with the highest margin and 5th category = sales of the 20% of the products with the lowest margin). The regression accounts for time, store, 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 values for all profit components. Observations were excluded once a store manager switched the store during the treatment period or from store managers who were not eventually assigned to a treatment. Robust standard errors are clustered at the district level of the treatment start and displayed in parentheses.* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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



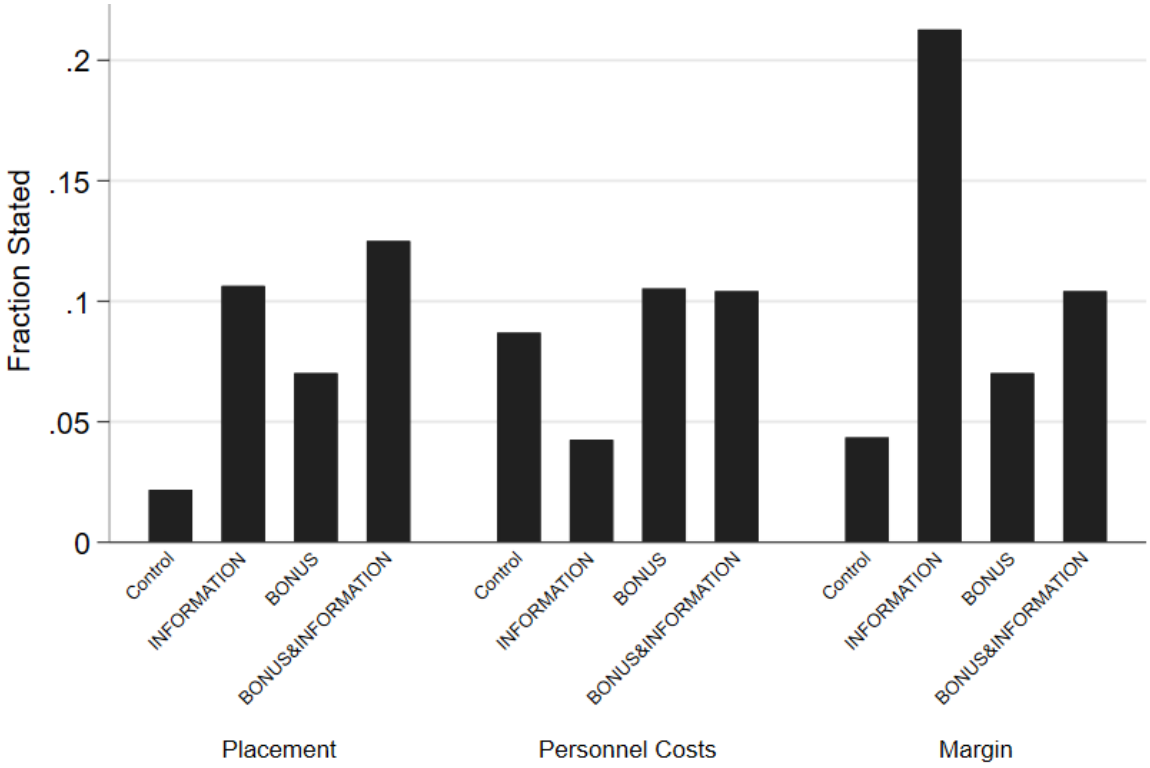
Note: The figure displays the average rating of focus on specific tasks (1 = low focus, 6 = high focus) obtained from an online questionnaire. N = 198.

Figure A3 – Focus on High-Margin (HM) Products



Note: The figure displays the fraction of explicitly stated task dimensions with a focus on high-margin products to increase profits obtained from the open-ended questions of an ex-post questionnaire. N = 198.

Figure A4 – Text (Keyword) Analysis of an Open-Ended 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-ended question from an ex-post questionnaire. N = 198.

Table A8 – Self-Stated Actions to Increase Profits (Open-ended Questions)

Panel A	(1) Ordering	(2) Placements	(3) Cleanliness	(4) KPI	(5) Inventory	(6) Personnel Management	(7) Own Effort
BONUS	0.0315	0.102***	-0.00740	-0.0181	0.0193*	0.00301	-0.000231
	(0.0288)	(0.0291)	(0.0134)	(0.0322)	(0.0100)	(0.0218)	(0.0113)
INFORMATION	0.0387	0.0302	0.000127	0.0500	0.0191**	0.0185	0.00667
	(0.0276)	(0.0278)	(0.0128)	(0.0308)	(0.00957)	(0.0208)	(0.0108)
BONUS&INFORMATION	0.0597**	0.141***	-0.00408	0.00208	0.0223**	0.0198	-0.00393
	(0.0287)	(0.0289)	(0.0133)	(0.0320)	(0.00996)	(0.0217)	(0.0113)
Controls	No	No	No	No	No	No	No
Observations	198	198	198	198	198	198	198
R^2	0.022	0.138	0.002	0.028	0.031	0.007	0.005
Panel B	Ordering	Placements	Cleanliness	KPI	Inventory	Personnel Management	Own Effort
BONUS	0.0355	0.101***	-0.00876	-0.0233	0.0182*	0.00154	-0.00169
	(0.0302)	(0.0305)	(0.0114)	(0.0353)	(0.00977)	(0.0244)	(0.0117)
INFORMATION	0.0256	0.0117	-0.0179	0.0708*	0.0106	0.0318	0.000516
	(0.0308)	(0.0310)	(0.0116)	(0.0359)	(0.00994)	(0.0248)	(0.0119)
BONUS&INFORMATION	0.0567*	0.129***	-0.00612	-0.00175	0.0191*	0.0100	-0.00514
	(0.0299)	(0.0302)	(0.0113)	(0.0349)	(0.00967)	(0.0242)	(0.0116)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	178	178	178	178	178	178	178
R^2	0.061	0.185	0.023	0.071	0.052	0.028	0.009

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 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 or from store managers who were not eventually assigned to a treatment. Robust standard errors are clustered at the district level of the treatment start and displayed in parentheses.* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.