Rank Incentives

Evidence from a Randomized Workplace Experiment*

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Abstract

Performance rankings are a very common workplace management practice. Behavioral theories suggest that providing performance rankings to employees, even without pecuniary consequences, may directly shape effort due to the rank's effect on self-image. In a three-year randomized control trial with full-time furniture salespeople (n=1754), I study the effect on sales performance in a two-by-two experimental design where I vary (i) whether to privately inform employees about their performance rank; and (ii) whether to give benchmarks, i.e. data on the current performance required to be in the top 10%, 25% and 50%. The salespeople's compensation is only based on absolute performance via a high-powered commission scheme in which rankings convey no direct additional financial benefits. There are two important innovations in this experiment. First, prior to the start of the experiment all salespeople were

Introduction

Rankings and league tables, where people are ranked relative to others in terms of a performance measure, are a pervasive feature of life. Employers use them to measure employee performance and determine bonuses and promotions (Grote, 2005), and more recently the use of rankings is being extended to assess the performance of teachers and hospital employees. Beyond the monetary benefits that may go along with high rankings, it has also been argued that people may care about their ranking *per se*, even when rankings have no financial consequences, which I refer to as *rank incentives*, as they directly affect self-image (Maslow, 1943, McClelland et al, 1953, Benabou and Tirole, 2006, Koszegi, 2006) and convey status (Frank, 1985, Moldovanu et al, 2007, Besley and Ghatak, 2008).

These *rank incentives* open up an important cost-effective way to shape performance, given recent technological advances that make reporting rankings cheap and easy, as people might be motivated to put forth additional effort in order to rise in the rankings as a way to improve their self-image. Yet the response to being informed about one's rank is ambiguous as it can either be motivating or demoralizing.

I provide novel evidence on the effect of rank feedback using the context of full-time furniture salespeople. I have a clean and precise performance measure – sales data at the individual level over the span of three years – and in contrast to the laboratory, I study long-term responses to treatments that can abstract from transitory effects like learning.

Studying the impact of rankings on performance is, however, empirically very challenging, as several confounds have to be ruled out.

First, rank feedback has to vary separately from monetary incentives; otherwise, the behavioral response to rank feedback can be clouded by its financial aspect. This study deals with this challenge by using a natural field experiment (Harrison and List, 2004) with contemporaneous control and treatment groups where only the presence or absence of rank feedback is being varied, holding constant all monetary incentives.

Second, as is the case in any experiment, people may respond to changes in the environment by increasing performance irrespective of the nature of the treatment.[1] This effect is compounded in the case of rank feedback with learning behavior and experimentation: Telling people their rank induces a concern for relative standing, thus adding a new dimension to how they derive utility from their work. The critical point here is that as rankings become salient, people need to learn how much effort is required to change their rank leading to a transitory rise in performance. For this reason, *introducing* rank feedback leads to a short-term increase in effort, but it does not distinguish between learning about relative ability from rank incentives per se. This concern is handled in this paper by the sequence of treatments: Instead of adding, I

[1] This is referred to as the *Hawthorne Effect* even though a reexamination of the original Hawthorne data revealed no such effect at that site (Levitt and List, 2011).

removed rank feedback and then examine outcomes over several years. In the context of this paper, furniture salespeople have been told their rank in prior years so that this information is salient and they already had ample opportunity to learn how their effort affects their rank. Removing rankings can then separate the effect of rank-incentives from learning behavior.

Third, providing initial rank information affects employees' perceptions and beliefs about future compensation schemes, which by itself can raise performance. When a salesperson receives rank feedback, she could believe that the employer can and will link compensation to that rank in the future and this gives rise to performance improvements as employees want to signal ability to the employer. In this field experiment I distinguish rank incentives from this signaling effect by having two competing treatments: one with rank feedback and another that also induces the signaling mechanism without explicit rank feedback. This is implemented by a treatment arm where employees are given only benchmarks showing the current performance needed to be in the top 10%, 25%, and 50% of the sales distribution, allowing me to compare the effect of these benchmarks to rank incentives. I find that rank matters beyond the signaling mechanism as the rank feedback treatment lead to a larger treatment response compared to the benchmark treatment. I further corroborate the evidence with survey data revealing that for these employees rankings are more importantly used to shape self-image rather than to improve their chances for promotion on the external job market.

Fourth, tournament theory (Lazear and Rosen, 1981) predicts that employees might be affected by rank information not because they care about relative performance, but because rank data allows them to filter out the effect of common shocks to their productivity, enabling them to learn about current market conditions, and thus their current return to effort. Several results in my context make this mechanism less likely in my context as there are heterogeneous treatment effects, notably by the type of feedback and by gender, not predicted by tournament theory. Moreover my survey data confirms that employees are least likely to use rankings to learn about current market conditions.

Fifth, multi-tasking (Holmstrom and Milgrom, 1987, Bolton and Dewatripont, 2005) is a pervasive element of most jobs, as employees have some leeway in terms of how much attention they allocate across their various duties in addition to the trade-off between work and the satisfaction they can achieve outside the job. Multi-tasking is particularly relevant when people care about their rank yet can choose on which task they want to excel to improve their self-image. When the effort required to rank well in one task is too high, an employee might be better off pursuing a higher placement in the rankings of another task. This multi-tasking aspect in the response to rank incentives has not been addressed in the literature so far. This study can shed light on that phenomenon by testing a direct implication of the multi-tasking model. The multi-tasking problem is driven by how much the costs of effort are connected across tasks: Unless tasks are technologically independent, raising effort on one task raises the cost of effort on

the other. This so-called effort substitution problem has testable implications and I find, as predicted, that rank feedback has a stronger effect on those products with a high effort substitution parameter especially when the rank is lower than expected.

Sixth, the reaction to rank feedback could depend on how actionable the data is. When a furniture salesperson is told only that her rank is worse than expected, without telling her how much more she needs to sell to achieve a desired rank, she is more inclined to be demoralized and to shift her attention to other tasks. However, providing data not only on rank but also on how much additional performance is needed to rise in the rankings dampens this demoralization effect. This mechanism, also known as the path-goal model (House, 1971, 1996), improves motivation as it makes the connection between effort and reward clearer. A novelty of my study is to explore this directly by comparing rank feedback to another treatment where, in addition to rank feedback, salespeople are also told the current required sales performance necessary to place within the top 10%, 25%, and 50%.

Finally, another aspect to consider is that the taste for rank incentives and thus the behavioral response to rank feedback may be heterogeneous across people as some may care more about their rank than others. A natural place to explore this is to look at effects by gender. There is now a rich literature on gender differences in the response to incentives and competition (Bertrand, 2010, Gneezy, Niederle and Rustichini, 2003, Niederle and Vesterlund, 2007, Gneezy, List and Ludwig, 2009), which could be one reason for the persistent gender gap in compensation (Bertrand, 2010).[2] In line with gender differences in competition I find that rank incentives only affect men but not women adding a new result to literature on the gender gap. Empirically the challenge is to tease apart the gender effect from other characteristics that may be correlated with gender and workplace productivity, which here I can address with detailed survey and productivity data.

The context of the field experiment was a large office furniture company in North America between 2009 and 2011. The multi-tasking setting arises as the sales of these furniture products are outsourced to independent dealerships. Those selling the company's furniture products also can sell other products as long as they are not from a pre-specified list of competing brands. Salespeople are located in dealerships throughout the country. Their compensation is commission-based and depends on the value of

[2] In addition to gender differences in the response to incentives (Gneezy et al, 2009, Gneezy et al., 2003; Lavy, 2008, Niederle and Vesterlund, 2007, Bertrand, 2010), other reasons for the gender gap lie in differences in human capital (Blau and Kahn, 2010), stereotypes and discrimination (Spencer et al., 1999, Goldin and Rouse, 2000), and differences in preferences and identity (Bertrand, 2010). A recent field experiment by Flory et al. (2010), which tests for gender differences in job-entry decisions, shows that women disproportionately shy away from competitive work settings, yet the effect weakens when the job requires team work and, as in Gunther et al (2010), whether the task is female-oriented. Gill and Prowse (2010) find that the gender difference has to do with how men and women react to losses and the size of losses in tournaments. Men tend to respond particularly to large losses whereas women's response does not depend on the size of the loss. Cotton et al. (2010) find in an experiment using math competitions that gender differences only exist at the first experimental round of competitions and that it is absent in any subsequent periods.

their sales alone. Both before and during the experiment, all salespeople had access to a personalized and password-protected Website, which recorded their sales. The Website is updated daily and shows their commission rates and current payout. Historically, and prior to the experiment, all salespeople could view on their Webpage their performance rank in terms of year-to-date sales in North America.

In collaboration with the management of the furniture company, starting in 2009 I implemented a two-by-two randomized control trial with four treatment groups: (i) Employees in group one received no relative performance feedback; (ii) Employees in group two received rank feedback alone; they were privately only told their own rank; (iii) Employees in group three were given benchmarks informing them about the current sales-performance required to be in the top 10%, top 25%, and top 50%; and iv) Employees in group four were given rank feedback and benchmarks together.

Statistical power is of concern here as the variance in the sales performance across salespeople and months is very large. Furthermore after the pilot phase, I planned also to look at treatment by gender as well. For that reason, I spread the treatments over several years. After an initial pilot phase in 2009 with one treatment group with rank and another group without rank feedback, I had in 2010 one treatment group without rank feedback, another with rank feedback, and a third with rank feedback and benchmarks and finally in 2011 there was one treatment group without feedback, another with benchmarks only, and a third with rank feedback and benchmarks. To achieve balanced treatment groups across years, all salespeople were re-randomized to treatment groups at the beginning of year 2010 and 2011.

The field experiment yielded the following key results. First, I find that *removing* rank feedback *increases* sales-performance by 11% or one-tenth of a standard deviation. Second, I find some heterogeneity in the effects in that only men, but not women, exhibit a significant treatment response to rank feedback. Third, making feedback more actionable by adding benchmarks to rank feedback significantly raises performance compared to giving rank feedback alone, but this is not significantly different from the effect of not providing any relative performance feedback. Fourth, the result is driven by a demoralization effect as salespeople reduce their effort when they are informed of a lower than expected rank. Fifth, in line with a theoretical prediction of the multi-tasking model, there is evidence that the treatment effect is larger for those sales with high effort substitution across brands, i.e. when the effort to sell one brand raises the cost of effort to sell other brands, as salespeople switch to selling other brands especially when their rank is worse than expected.

Related Literature

Building on insights in sociology and social psychology (Festinger, 1954), there is now a rich theory in economics on the role of self-image (Benabou and Tirole, 2003, Koszegi, 2006), social status (e.g. Robson, 1992, Becker et al, 2005, Ellingsen and Johannesson, 2007, Frey, 2007, Moldovanu, et al, 2007,

Auriol and Renault, 2008, Besley and Ghatak, 2008, Dur, 2009, Ederer and Patacconi, 2010), equity theory (Adams, 1965) and identity (Akerlof and Kranton, 2005), which provide the underpinnings to this study.[3] More generally, a meta-analysis of psychology studies by Kluger and Denisi (1996) about feedback interventions, covering some 131 studies with over 13,000 subjects, revealed that the effect of feedback on performance is heterogeneous. Even though feedback across the studies improved performance on average, it reduced performance in one-third of the surveyed studies. [4]

There are now a number of papers that study the effect of rank feedback with field, laboratory, and quasi-experiments. I will focus on a few that are most closely related to mine.[5]

In a notable field experiment, Delfgaauw et al. (2012) collaborated with a Dutch retail chain and 128 of its stores to vary incentive pay and rank feedback based on store-level performance. Employees in each store are paid hourly with the store manager also receiving some performance-related pay. Prior to the start of the experiment, the stores received no rank feedback about store level sales performance. The researchers put stores into groups of five similarly performing outlets and implemented two treatments. In the first, stores were sent a poster every week containing cumulative sales growth figures for all five stores in their group, ranked in descending order. These posters were sent to the store manager and it was up to him or her to communicate it to the store's employees, who received no direct communication about the treatments. Store managers in the other treatment group also received those posters, but additionally they participated in a six-week tournament where the manager and all employees of the winning store received a reward of 75 Euro with a prize of 35 Euro for the runner-up. The average treatment effect of the poster and the poster plus prize treatment was an approximately five-percentage point increase in sales growth. Interestingly, adding prizes to rank feedback did not yield an additional improvement in performance. The treatment effect was stronger when the gender of the store manager was similar to the predominant gender in a store, which the authors interpret as evidence for improved communication and motivation channels in those stores.

[3] Aoyagi (2007 and 2010) and Ederer (2010) characterize the assumptions required for feedback to lead to optimal effort provision which are what agents know about their ability and how that ability enters the production function and the shape of their cost of effort functions.

[4] See also Smither et al., 2005.

[5] An early paper on the effect of status on performance is Greenberg (1988), who published data from a field experiment of 198 employees in an insurance under-writing department. While the firm renovated its offices employees had to be relocated to other offices. The clever aspect of this paper is that this relocation was randomized, so that employees were moved to offices that corresponded to the same, lower, or higher pay-grades. Compared to those employees who were relocated to an office in line with their current pay-grade, those reassigned to higher-status offices increased their performance, whereas those assigned to lower-status offices reduced their performance. The effect of information about relative performance has also been studied experimentally in the context of electricity consumption (Costa et al, 2010) and job satisfaction (Card et al, 2010), and interpersonal wage comparisons (Charness and Kuhn, 2007).

One possible mechanism for the increase in performance is that the presence of those posters added a new dimension to that work environment and may have enticed those employees to experiment and learn how additional effort would raise their rank. As the treatments lasted only six weeks, this learning and experimentation might have persisted throughout the study.[6] In contrast, the subjects in my experiment have been given rank feedback for several years and the principal treatment was to remove that information, which is a more effective way of separating rank incentives from the effect of learning about how effort increases rankings.

In two studies, rank information was introduced at the same point in time to all employees. The first study was by Bandiera et al (2012), which studied fruit-pickers who worked in teams of five pickers. All pickers in a team were paid the same amount, and once a week pickers could themselves change the composition of their team at a team exchange. The first phase of the treatment consisted of posting histograms with team-level performance sorted in descending order. A later phase added weekly tournaments with a prize for the best team worth approximately 5% of the weekly wage. They find that posting those rankings reduces team performance. The mechanism behind the result was the endogenous change in team composition: Instead of forming teams with friends, the fruit-pickers began to form teams with people of similar ability which, given the skewed distribution of ability across pickers, lead to a drop in performance. The second study was by Blanes i Vidal and Nossol (2010), which involves performance data about grocery packers at a warehouse. The company chose to start posting the performance rank of employees, which the authors exploit as a quasi-experimental design. This study is very notable, as it finds in contrast to this paper that publically providing rank feedback increases sales.

The identification challenge in these two studies arises due to the absence of a contemporaneous control group, making it difficult to separate the treatment effects from time trends and general shocks to productivity, which my randomized control trial can address. More importantly, the fruit pickers in Bandiera et al. (2012) and the grocery packers in Blanes-i-Vidal and Nossol (2012) do not have flexibility in terms of what tasks they can work on except for the one job they are given to do. However, in my setting, which perhaps is more representative, furniture salespeople can either sell products from one brand or from other brands, which opens up a new and more realistic behavioral response: When salespeople learn that they rank poorly selling one brand, they could shift their attention to selling other brands to excel there.

The laboratory is a very useful environment to study the effect of performance rankings, as it allows for tight control of the sequence of events, the production functions, and the flow of information.

[6] The gender pattern in their results is also in line with that interpretation as it was up to the store manager to motivate the employees to work harder, which arguably is easier when there was gender alignment between them.

An important laboratory study of rankings[7] is by Kuhnen and Tymula (2011), who show that when the information about rank is worse than expected, experimental subjects subsequently increase their performance. This mechanism also inspired the analysis in my study wherirttlyt[xperim)-7((2011))]T 0.0077 Tc7-0.0-0.18-

invitation. The result was that those workers who were told their rank were less likely to return to work, and when they did return, they were less productive.

Theoretical Framework

To help organize the core results of this paper, I set up a framework building on the Holmstrom-Milgrom (1991) multi-tasking model, which is then extended with preferences over rank to illustrate how providing rank feedback can reduce effort. In my experiment, the incentive schemes are exogenously varied, so I focus on the agents' optimization problem and do not derive optimal contracts. Also, in line with the experimental design below, all feedback will be truthful.

Suppose salesperson i's payoff contains three components. First, she derives utility from the monetary benefit of her effort. This benefit $b_i(e_i) = be_i$ approximates the commission-based compensation scheme salespeople face and reflects how their effort maps into commission payouts. Second, each agent i can exert effort into one of two tasks $k = \{1, 2\}$, which captures that she can either sell *one* brand, denoted by "1", or *other* furniture brands, "2".

Second, exerting effort generates cost $c_i(e_1, e_1) = \frac{1}{2}\left(c_{i1}e_{i1}^2 + c_{i2}e_{i2}^2\right) + \delta e_1 e_2$, where $0 < \delta < \sqrt{c_{i1}c_{i2}}$ captures the complementarity in cost between the tasks. This is the key assumption generating *effort substitution* effects in multi-tasking models: Raising effort on one task, raises the marginal cost of effort on the other task.

The third element generates preferences for rank. Assume that the agent receives a *rank reward* $r_{ik} > 0$ from achieving a "high" rank. The rank needed to obtain this reward and the value of r_{ik} can vary across agents. Assume that the effort required to obtain this rank is $\widehat{e_{irk}}$. A critical assumption here is that $\widehat{e_{irk}}$ is not known by the agent and the agent either has to rely on feedback from the principal or on her beliefs about its level $\widetilde{e_{irk}}$.[8] Denote by I_{ir1} and I_{ir2} dummy variables equal to one when the effort exceeds the required level, $e_{ik} \geq \widehat{e_{irk}}$, and zero otherwise. The agent maximizes her utility with respect to e_{i1} and e_{i2}:

$$u_i = (b_1 + I_{ir1}r_{i1})e_{i1} + (b_2 + I_{ir2}r_{i2})e_{i2} - \frac{1}{2}\left(c_{i1}e_{i1}^2 + c_{i2}e_{i2}^2\right) - \delta e_1 e_2. \quad (1)$$

The agent equalizes the marginal return from effort for each brand and its marginal cost. Focusing on the case when the effort exceeds the amount required to obtain the rank reward, $e_{ik} \geq \widehat{e_{irk}}, I_{ir1} = I_{ir2} = 1$,[9]

[8] The rational for this informational assumption is for ease of exposition. Suppose all salesperson's performance is a function of effort plus a random walk shock where the agent only observes the shock after choosing effort. The agents form beliefs about the other agents' abilities and the current state of shocks.

[9] Otherwise $r_{i1} = 0$ and/or $r_{i1} = 0$.

we have the two first-order-conditions $b_1 + r_{i1} = c_{i1}e_{i1} + \delta e_{i2}$ and $b_1 + r_{i2} = c_{i2}e_{i2} + \delta e_{i1}$. The unique solution to the agent's problem of how much effort to put into selling the *main* furniture brand 1 is:

$$e_{i1}^{*} = \frac{(b_1+r_{i1})c_{i2} - \delta(b_2+r_{i2})}{c_{i1}c_{i2} - \delta^2} \quad (2)$$

Prediction 1: The effect of rank feedback is heterogeneous across agents.

The first prediction is driven by the intuitive assumption that preferences vary both in terms of the marginal cost of effort and by the effort required by an agent to obtain the rank reward r_{i1}: Some need to obtain a higher rank than others to have a perceived boost in their self-image. I will explore this empirically by testing for significant treatment effects by gender and other observable characteristics. The important point here will be to test if there are taste-based heterogeneities in rank incentives that are not driven by other mechanisms such as the responses to pecuniary benefits in financial tournaments.

When an agent does not get rank feedback, she has to rely on her beliefs $\widetilde{e_{irk}}$ in whether she has obtained the rank reward r_{ik}. Agents choose effort first and then, depending on the treatment, will or will not learn how the effort mapped into a rank. When they do get rank feedback, the achieved rank might be lower or higher than what they expected it to be based on their prior beliefs. Rank feedback can then decrease effort for three reasons:

Case i) *Complacency after overshooting ($\widetilde{e_{ir}}$ > $\widehat{e_{ir}}$).* A salesperson, using her beliefs, can end up putting in too much effort to obtain r_{i1}, and will adjust her effort downwards when she is told via rank feedback that she "over-shot."

Case ii) *Demoralization ($\widetilde{e_{ir}}$ < $\widehat{e_{ir}}$).* An agent, based on her beliefs will try to obtain r_{ik}, but in fact the effort required to obtain this rank reward is too high in that she is better off not pursuing the rank reward.[10] Rank feedback will then reduce her effort.

Case iii) *Multi-tasking.* An agent, upon getting rank feedback that she did not obtain the rank reward for task 1, r_{i1}, as $I_{ir1} = 0$, could reduce her effort on task 1 when it requires less effort to obtain the reward from the other task, r_{i2}. This is a new effect in a multi-tasking environment: When people hear they have low relative performance on task 1 they switch to excel on task 2.

These three cases lead to the second prediction.

Prediction 2: Rank feedback leads to a drop in performance under Cases i) to iii) .

[10] More precisely she will set $e_{i1}^{"} = \frac{b_1 c_{i2} - \delta(b_2+r_{i2})}{c_{i1}c_{i2} - \delta^2} < e_{i1}^{*} = \frac{(b_1+r_{i1})c_{i2} - \delta(b_2+r_{i2})}{c_{i1}c_{i2} - \delta^2}$.

The next prediction makes a subtler point that lies at the heart of the multi-tasking problem. When effort substitution is high, i.e. $\delta > 0$, then raising effort on one dimension of the task also increases the marginal cost of effort for the other task: In this case, selling products for the first brand makes it harder to sell products of the other brand. I can rewrite (2) as

$$e_{i1}^{*} = \frac{(b_1+r_{i1})c_{i2}-\delta(b_2+r_{i2})}{c_{i1}c_{i2}-\delta^2} = \frac{b_1c_{i2}-\delta(b_2+r_{i2})}{c_{i1}c_{i2}-\delta^2} + \frac{r_{i1}c_{i2}}{c_{i1}c_{i2}-\delta^2}. \tag{3}$$

When an agent no longer pursues the rank reward for product one, i.e. $I_{ir1} = 0$, her effort drops by $\frac{r_{i1}c_{i2}}{c_{i1}c_{i2}-\delta^2}$, which is increasing in δ.

Prediction 3: Rank feedback will reduce effort more for those sales that have a high effort substitution across brands.

Extensions

So far we abstracted from shocks to productivity. More generally productivity is driven by effort and current, period specific shocks to productivity. Even when agents learn how their effort maps into ranking in the long-run, they benefit from frequent feedback as it allows them to adjust their effort depending on the current state of those productivity shocks. This is at the principal rational behind using tournaments as a way to elicit efficient effort provision (Lazear and Rosen, 1981), as it filters out period specific shocks to productivity that are common to agents. In our context, when the rank of a salesperson turns out to be lower than expected, the agent could either reduce her effort and shift it to task 2 or she could increase her effort by the required amount to reach $\widehat{e_{ir}} > \widetilde{e_{ir}}$. This, however, requires that the principal informs the salesperson how much additional effort currently is required to obtain the rank reward, i.e. the measure of $\widehat{e_{irk}} - \widetilde{e_{irk}}$. This mechanism is also known as the path-goal model (House, 1971, 1996)

Prediction 4: Compared to receiving rank feedback alone, an agent will raise effort when she also receives information on benchmarks that help her gauge the required effort needed to obtain the rank reward.

Lastly, one very different behavioral explanation for the effect of rank feedback on performance is that the agent may perceive relative performance feedback as a signal about the principal's intentions and technical capability to condition rewards on relative performance in the future (Benabou and Tirole, 2003). This then leads to an increase in effort as agents want to signal their ability and motivation to the

principal. To separate this effect we need a mechanism that triggers this updating of beliefs without rank incentives. We will test for this mechanism by having one treatment arm where agents only receive benchmarks on what it takes to be in the top 10%, 25% and 50%, the idea being that this treatment should bring about a change in beliefs about the principals future linking of relative performance to compensation but is not triggering rank incentives, as agents are only told benchmarks instead of their rank.

Context and Methods

The firm I study is a leading office furniture company in North America. The natural field experiment (Harrison and List, Rasul and List, 2011, Levitt and List, 2009, Bandiera et al, 2011a) was designed and implemented in collaboration with the company's incentive scheme management team. The company designs, manufactures, and distributes the furniture, but they outsource the sales to independent dealerships. Those independent dealerships hire their own salespeople who then sell the office furniture directly to companies. The final clients are predominantly companies rather than private individuals.

Importantly for my paper, salespeople in the independent dealerships can sell other furniture products, as long as they are not from a set of specified direct competitors. I do not have direct data on their sales for other products, but I administered a survey in 2012 to elicit more information about how they allocated their time across tasks. Data from the 2012 survey shows the percentage of time allocated across the different tasks and the total hours worked per week. The survey had 617 respondents and reveals that on average they work 41-50 hours per week in total,[11] and they spend on average 61.54% of their work time selling the furniture company's brand, 17.6% selling other manufacturers' products, and the remaining 20.7% of their time on other tasks not involving sales. Only 2% of salespeople report that they exclusively sell furniture by the company.[12] All taken together, each salesperson and dealership thus faces a multi-tasking problem, whereby they need to decide how to allocate their time and effort between selling furniture from the main brand or from the other brands.

As is common for salespeople, their compensation is commission-based, whereby they earn a percentage of the dollar value of their sales.[13] The commission rates are the same for everyone in North

[11] Of the respondents, 20% work 30 hours of less, 16% work 31-40 hours, 46% work for 41-50 hours, 25% work 51-60 hours, and 9% work more than 60 hours per week.

[12] That survey also asked how the commission rates compared to those of the other brands. 52% stated that the commission rates were comparable to those offered by the other brands. Of the others, 21% said other brands offered more generous commission rates, and 18% said other brands offered less generous commission rates, 9% did not sell furniture from the other brands.

[13] As is common in commission-based compensations, the commission system resets at the start of each calendar or fiscal year and the commission rate rises at specific thresholds based on year-to-date total sales. The exact commission rates of the scheme are confidential. To make an illustrative example, however, the commission rate is

America. As is typical for such an outsourced sales arrangement, the furniture company gives salespeople very precise price-discount guidelines. It is important to stress two points here: First, the fact that compensation is purely commission-based with no guaranteed fixed payments implies a very high-powered incentive scheme. Second, conditional on these commissions, performance rankings do not convey any additional material benefits.

The task of the salespeople is primarily to get current clients to buy new products rather than to identify new clients. Most of the clients are medium to large companies that have well-established relationships with the dealerships. There is no competition for clients within or across dealerships as they are operating in geographically separated markets. The 1,754 salespeople in the data work in 204 dealerships across the U.S. and Canada and comprise the universe of all salespeople who sold furniture under the furniture company's commission-based system between 2009 and 2011. Salespeople are typically full-time employees in the respective dealerships with a median tenure of seven years. In this setting, the furniture salespeople's production technology is such that there is no scope for cooperation between salespeople within a dealership, and each salesperson exclusively focuses on the sales aspect in the furniture business. For instance, the detailed choice of patterns and colors of a client's order is completed by other employees and not by the salesperson. Each salesperson thus has to decide how much effort to exert in selling products from this or from other furniture brands.

The demand for office furniture products varies greatly over time for two reasons. First, the experiment spans the time of the aftermath and the recovery from the 2008 financial crisis, which led to a sharp decline in furniture sales in the beginning of 2009 prior to the start of the experiment followed by a slow recovery in the ensuing years. Second, furniture is sold directly to companies, which tend to group orders in time, so that sales volume varies from month to month for a salesperson. These features of the context are addressed in the experimental design and the data analysis. First, as explained further below, I used contemporaneous treatment groups, which allow me to control for seasonal variations like the Great Recession with period dummy variables. Second, I study long-run treatment effects that will smooth out the intermittent nature of sales.

A key element of the work environment and the platform for experimentation is a personalized Webpage the salespeople can access, which existed well before the start of the experiment. The purpose of the Webpage is twofold. First, it allows the salespeople to verify whether all their sales have been correctly recorded and attributed to them. Second, this Webpage contains a wealth of data about their absolute performance to date by listing their current and year-to-date sales, the commissions they earned, and their current commission rates. This Webpage remained available to all salespeople throughout the

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x% below $10,000 and (x+0.5)% between $10,000 and $25,000 etc. These steps are small so they don't give rise to gaming and ratcheting during the year.

experiment. Based on login data, salespeople on average access that Webpage twice a month. In the experiment, as explained in detail further below, I randomized the content of that webpage.[14]

Perception and Usage of Rankings

The firm gave rank feedback for many years to their salespeople and I was interested in what the sales people thought the effects of the rankings were. In a survey in April 2012, I asked them how much they agreed or disagreed with the following two statements: “Companies that rank sales performance try to pit their employees against each other.” Only 15.69% of salespeople agreed with this statement.[15] Next I stated, “Companies that rank sales performance try to instill healthy competition to entice employees to

As highlighted in the tournament literature, a benefit of rankings is that it allows people to filter out the effect of their effort on productivity from time-specific productivity shocks that are common across salespeople (Lazear and Rosen, 1981). Since rankings are independent of factors affecting performance that are common to all sales people, they make it easier to learn about real changes in ones own productivity. To see if this is of importance to salespeople we asked them how much they agree with the following statements.

[D] "Knowing my sales rank helps me evaluate my own performance." (56.25% agreed)

[E] "Knowing my sales rank helps me determine the market demand for [brand name] products and thus whether it is worth my while to try harder to sell [brand name] furniture." (37.43%)

Even though rank is used for performance evaluation, when asked explicitly, the salespeople do not use rankings to filter out common shocks to their productivity.

In sum, salespeople primarily use rank feedback as a way to shape self-image rather than to learn about market conditions or to further their career.

Timing of Events and Treatments

Prior to the start of the experiment in August 2009, *all* salespeople saw their rank on their personalized Webpage. Specifically, they saw next to their name their rank in terms of year-to-date sales in North America.[17] Showing employees their rank is common in the sales business and is practiced by many other companies as well (Grote, 2005).[18]

To be clear, *all* salespeople had access to that Webpage throughout the experiment and I did not remove or alter information about their absolute performance, e.g. commission payouts and rates. The treatments of this field experiment comprised the randomized manipulation of information about relative performance that was displayed on the Webpage.

As is common practice in field experiments, I first had a relatively short pilot phase from August to December 2009 that allowed for the testing of whether the reprogrammed Webpages displayed correctly. During that pilot phase, I randomized salespeople into three groups. The first group could continue to view their rank in terms of year-to-date sales on their personalized Webpage, but the second group no longer saw that ranking information. In the third group, I experimented with a different way to

[17] Based on my survey in April 2012, salespeople on average know the correct number of salespeople actively selling the company's products in North America.

[18] For instance GE, Google, Microsoft, Whirlpool, but also the hospital at the University of Pennsylvania give ranked performance feedback to their employees. The frequency of this feedback varies and in some firms employees are told their actual rank whereas in other companies they give bands, e.g. middle 50%. The variance in the way rank feedback is implemented is testimony to the need for a better understanding of its effect on performance.

calculate rank, taking into account state-level differences in market conditions. This was called the "Normalized Rank" and only used during the pilot phase.[19]

Based on this pilot phase where I noticed a difference in treatments by gender, I implemented a two-by-two randomized control trial. For reasons of statistical power, I had to span the experiment due to the large variance in sales performance: As I wanted to be able to test the difference across treatments and by gender, I could only have three treatment groups per year to achieve the required statistical power. To deal with possible selection effects, differential attrition, and unbalanced treatment groups, all people were *re-randomized* into treatments at the start of 2010 and 2011.

The experiment randomized whether salespeople saw their rank on the Webpage and whether benchmarks were also displayed. The benchmarks were the current sales-performance of those just inside the top 10%, top 25%, and top 50%. The data was updated daily on the Webpage.

Let me describe the sequence of treatments. Up until August 2009 all salespeople saw their rank but no benchmarks. In the pilot phase from August to December 2009, one group continued to see their rank which I call the "Rank Feedback" treatment, the second group did not see their rank ("No Rank Feedback") and a third saw a "Normalized Rank" (see footnote 19).

In 2010, one group saw their rank on the Webpage, the "Rank Feedback" treatment, but the second group did not see their rank ("No Rank Feedback"), and the third group saw their rank together with the benchmarks ("Rank Feedback & Benchmarks").

In 2011, the two-by-two matrix was completed by having one group be assigned to "No Rank Feedback," another to "Rank Feedback & Benchmarks," and the third to "Benchmarks" alone.

Following the discussion in the theoretical section, the benchmark treatments served two purposes. First, it allows me to distinguish rank incentives form another mechanism whereby employees interpret it as a signal by the principal that she has the IT capabilities to easily construct rankings, and that explicit rewards based on rank will come. The second purpose is to give those who are told their rank additional information of what it takes to make a difference to their rank. This mechanism, also known as the path-goal model (House, 1971, 1996), predicts that providing a path – the distance to cover to get to the next benchmark – is an effective organizational leadership tool to help employees achieve their goals.

[19] More precisely, I regressed each salesperson's rank onto state fixed effects and then used the residual to calculate the "normalized rank." Those salespeople who saw this ranking were informed on the Webpage that "the normalized rank employs a statistical method that takes into account differences in market condition across regions so that all dealerships in North America are comparable to each other." This treatment was only used during the pilot phase as it was not significantly different from the "Rank Feedback" treatment.

Discussion of the Context

There are several noteworthy features in this context that permit the study of whether people care about their rank *per* se. First, salespeople are only compensated for their absolute performance. They have a very high-powered incentive scheme based on commissions. For a given level of absolute sales, a change in rankings does not affect their compensation. This is important as otherwise preferences for rank might be confounded with a concern for the direct pecuniary benefits associated with rankings. Second, all salespeople had easy access to a Webpage where they could review data on their absolute performance as measured by their own sales history. Thus, rankings were not needed to proxy for information about absolute performance and ability, as that was always available to all employees. Third, rankings were based on year-to-date sales, which made them less volatile and a more reliable representation of relative performance. Fourth, even if no claim can be made that the subjects were representative of the overall U.S. labor force, they exhibited a broad range of demographic backgrounds in terms of age, job experience, demographic location and had a balanced gender mix with 56% of the salespeople being female.

I considered randomizing whether to show them the rank *within* the dealership, but after visiting several stores, it became clear that all salespeople readily had access to this information as it was prominently displayed and communicated in their offices. So I would lack the experimental control to estimate the effect of dealership rankings. As the study uses national rank, which based on my survey in April 2012 is the second most important reference group after that in a dealership, I induced an attenuation bias in the estimated treatment effects.

It is important in an experimental study to distinguish between planned comparisons and those that are invoked after the end of the experiment. Apart from the comparisons across treatment arms, I also planned on comparing treatments by whether agents receive positive or negative feedback, i.e. “good” versus “bad” news. The interest in results by gender emerged at the end of the pilot phase in 2009 when I discovered evidence for gender specific effects. This was the principal reason why I spaced out the experiment across two years to gain sufficient statistical power.

Level of Randomization

The randomizations were done at the salesperson level. Balancing tests are reported at the top of Table 1. Here I regressed the sales performance in a year on the treatment dummies of the prior year and tested for the joint significance of those dummy variables. I repeat the same regressions with gender as the dependent variable. The treatment dummies are not jointly significant, so I cannot reject the null hypotheses of balanced treatment groups in line with evidence for a successful randomization.

As I randomized at the salesperson level, the design effect and the scope for contamination needed to be addressed. The design effect, which is the inflation to the sample size required due to correlated shocks in each dealership to achieve a pre-determined power in the statistical tests, was negligible here as the group size, which here is the average number of salespeople per dealerships, was small [mean 6.34, standard dev. 4.80] as was the intra-class correlation coefficient at the dealership level (0.12631). So the sample sizes used were large enough to identify the treatment effects.

Beyond correlated shocks to productivity at the dealership-level, a possible concern was contamination of the control group by the treatment group within a dealership. There are two reasons that allow for the argument that contamination was not generating important biases in the estimated treatment effects. First, sales-people worked on accounts, i.e. clients, by themselves so there was no teamwork involved within a dealership. Second, the design of the randomization was such that contamination can be tested. Note that the randomization had been conducted at the salesperson level and therefore the share of salespeople in each dealership who were being informed about their rank was random as well. I then tested whether the treatment effect varied with the share of salespeople in a dealership who were receiving the other treatments. The test of these local interaction effects fails to reject the null of no local contamination effects.[20]

Results

In this section, I report and interpret the results of the field experiment. The presentation starts by reviewing time-series graphs at the month-treatment level. I then review averages and differences in means at the treatment-salesperson level followed by panel regressions estimating across and within salesperson treatment effects. Finally, I test for heterogeneous treatment effects depending on the type of rank feedback and across different product categories to explore an implication of the multi-tasking model. In each case, I present the results for the overall effect and split by gender.[21]

Time series graph

Figure 1a shows the time-series from 2009 to the end of 2011 of the monthly average of those salespeople who had sales. I drew separate lines for each treatment group. Note also the vertical lines indicating the points in time when salespeople were randomized or re-randomized to the different treatment groups. The sharp decline in sales in the beginning of 2009 is due to the aftermath of the recession. As the commission rate system resets at the beginning of each year, many salespeople switch to selling other brands at the

[20] Results omitted due to space constraints but available upon request.

[21] I also tested if the treatments had an effect on attrition, i.e. on whether salespeople remained in the sample but I find no evidence for it (results omitted but available upon request).

start of the year, so only those with large and steady sales remain active then. This explains the peak in this graph in January of each year. Note that these period effects will be filtered out in the regression analysis below.

The dashed line is the "Rank Feedback" treatment and has the lowest sales for most months in the pilot phase and for all months during 2010. This treatment was no longer in place in 2011. The dark solid line is the "No Rank Feedback" treatment, which for most months in the pilot phase of 2009 and 2010 led to the highest sales. Interestingly, the treatment "Rank Feedback & Benchmarks" was as success as the "No Feedback" treatment in raising performance. Finally, the three remaining treatments performed equally well during 2011.

The next two graphs draw the times series separately by gender. Figure 1b, shows that for women there is no clear pattern of treatment response. In contrast, Figure 1c shows very accentuated differences for men: Male salespeople who saw their rank had the lowest sales compared to those without rank feedback, as well as those in the "Rank Feedback & Benchmarks" treatment who both had higher sales.

In sum, the graphs highlight two results. First, only showing rank led to the lowest sales and second, the results are gender specific with larger differences for men and no discernable treatment effects for women.

Treatment-person Level Effects

As a first cut at the data, I now turn to formal tests of differences in means by treatment and year using data for all salespeople from the start of the experiment in August 2009 to the end of 2011. In Table 2, I show the weighted average of monthly sales by treatment where the weights are the number of months a salesperson had sales. At the edges of the table, I then report the tests of differences in means. I constructed the means by first taking averages at the salesperson-treatment level and then calculating the means for each treatment across salespeople. In the Appendix and in Table A1, I also test for differences in means by treatment separately for each year and gender.

Turning to Table 2A there are three significant differences means in the full sample of all salespeople. First, removing rank feedback increases sales (Prediction 2): In the top line with "No Benchmarks," the difference in means between the "No Rank," cell [1] in Table 2A, and "Rank Feedback" [2] treatment was an increase of 0.201 from 8.577 to 8.778 log of average monthly sales. This difference is significant at the 1% level. This is a seemingly large effect but note that the standard deviation associated with these means is large, so that the treatment effect is about one fifth of a standard deviation.

Second, making feedback data more actionable increases sales (Prediction 3): Looking into the first column with rank feedback, adding Benchmarks [3]-[1], increases sales by 0.276, about a quarter of a standard deviation, which is precisely estimated and significant at the 1% level.

Third, I also find that removing rank feedback and instead only giving benchmarks increases sales by 0.292, about a third of a standard deviation, significant at the 1% level, but there is no difference in means between Rank and Benchmarks [3], No Rank and Benchmarks [2], and No Rank and No Benchmarks [4]. Thus all treatments raised sales over the status quo in the company of only showing rank by some 20%.

In the next two panels, I separately test for differences in means for women, panel 2B, and men, panel 2C, and overall the same pattern of result emerges.

Across and Within Estimates: Treatment-Salesperson-Month Level Data

To effectively control for period effects, like the recession, I need to run panel regressions. As discussed above there are contemporaneous treatment groups so I can control for any period effects with year-month dummies ruling out any trends and time specific confounds that could bias my estimates. Specifically I estimate:

$$s_{imy} = \beta_{NR} NR_{iy} + \beta_{RB} RB_{iy} + \beta_B B_{iy} + \tau\lambda \tag{4}$$

where s_{imy} is the log of monthly sales, measured in thousands of USD, for a salesperson, *NR* is a dummy equal to one when a salesperson is being shown neither rank feedback nor the benchmarks. *RB* is a dummy equal to unity for those who received both rank feedback and benchmarks, and finally *B* denotes whether a salesperson only received benchmarks. The omitted category is to receive rank feedback only, which was the status quo for all salespeople prior to the start of the experiments. This regression also includes a set of dummies λ for each month in each year of the experiment.

In column (1) Table 3, I use the full sample from 2009 to 2011. All standard errors are clustered at the salesperson level. I find, in line with Prediction 2, a positive and precisely estimated treatment effect of removing rank compared to displaying ranking on the Webpage, β_{NR} = 10.9%**, significant at the 5% level, which is about one tenth of a standard deviation of the dependent variable. I can also see that showing rank feedback together with benchmarks has the same positive and precisely estimated effects as “No Rank Feedback”, β_{RB} = 12.5%, significant at the 5% level. Benchmarks alone are not raising productivity compared to not giving rank information, β_B = 6.37%, not significant at conventional levels, and the effect is smaller than providing Rank Feedback with Benchmarks even though the difference in the coefficients, $\beta_{RB} - \beta_B = 0.125 - 0.064 = 0.061$ is not significant at conventional levels. Nevertheless this is first evidence that rankings have an effect over and above benchmarks alone. This is important to tease apart the effect of rank incentives from the mechanism whereby salespeople respond to the change in feedback, as they update their beliefs about future changes to compensation schemes.

Next, I test for gender differences in treatment response. The motivation to study gender differences is twofold. First, I wanted to provide evidence that the gender differences in attitudes towards competition (Niederle and Vesterlund, 2007) extends to rank incentives as well. Second, robust gender differences would go towards providing additional evidence that rank incentives are a taste-based phenomenon and not primarily driven by economic incentives as would be the case for feedback in tournaments prizes.

In column (2) I am estimating:

$$s_{imy} = \beta_{NR}^{m} NR_{iy} + \beta_{RB}^{m} RB_{iy} + \beta_{B}^{m} B_{iy} + \beta_{NR}^{m-f} NR_{iy} + \beta_{RB}^{m-f} RB_{iy} + \beta_{B}^{m-f} B_{iy} + \tau \tag{5}$$

where the subscript "m" denotes the treatment effect for men and "$m -$ " the difference in treatment effects between men and women. To make the table easier to read, I also reported the marginal effects separately by gender.

In line with Prediction 1, the treatment effects of "No Rank Feedback" are gender specific. Removing rank increases monthly sales by 16.0% for men, which is precisely estimated at the 5% level. There is, however, no significant treatment effect for women, (0.0633, p-value = 0.279).

Showing both rank together with benchmarks increases sales for men by 20.3%, which is significant at the 1% level, but again there is no such effect for women, (0.0544, p-value = 0.369). In contrast, benchmarks alone do not affect performance compared to other treatments, as the effect is very small and insignificant both for men, 0.00975, and for women (0.101, p-value = 0.245).

I can also confirm that the mechanism behind rank feedback results for men is in line with *rank incentives*, and not a change in their beliefs over future compensation schemes. If beliefs were the driving factor then showing benchmarks alone should generate similar treatment response. This is not the case as the difference in the coefficients for men of $\beta_{RB}^{m} - \beta_{B}^{m} = 0.203 - 0.00975$ is significant at the 5% level. This also rules out the Hawthorne effect. Therefore rank has an effect on performance for men over and above benchmarks alone.

I have a subsample of 889 salespeople who are present in all three years, some of which witnessed several treatments depending on their randomization outcome. For those salespeople, I can estimate the within treatment effects with salesperson dummy variables by estimating:

$$s_{imy} = \beta_{NR}^{m} NR_{iy} + \beta_{RB}^{m} RB_{iy} + \beta_{B}^{m} B_{iy} + \beta_{NR}^{m-f} NR_{iy} + \beta_{RB}^{m-f} RB_{iy} + \beta_{B}^{m-f} B_{iy} + \tau\lambda + \kappa \tag{6}$$

where κ is a vector of salesperson fixed effects. In column (9), I can confirm that the effect of not showing rank increases performance for men by 11.7%, which is significant at the 5% level, but the effect for women again is not significant.

Columns (4) to (9) report the estimates separately for each year 2009[1], 2010, and 2011, and also by gender. Note that in columns (1)-(4) and (7)-(8) the omitted category is “Rank Feedback”, but in columns (5) and (6) the omitted category is “No Rank Feedback,” as the “Rank Feedback” treatment was no longer in place in 2011. In the Appendix, Table A2, and Figure A1a-c, I discuss how the main and gender specific effects are robust across the conditional distribution of productivity using quantile regressions, so that the result is neither driven by the tails of the distribution nor by mean reversion.

Table A3 provides further robustness checks of the gender effects. One possible confounding effect for these gender results is that male and female salespeople may be different from each other apart from their sex. However, I find that even after controlling for other types of observable heterogeneity, there is still a significant gender difference in treatment response.

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However, looking at the bottom set of coefficients, a result emerges which is in line with Prediction 2. First, not giving rank feedback when it would convey bad news increases sales by over 28%, which is precisely estimated and significant at the 5% level or about a quarter of a standard deviation. Second, compared to rank feedback alone, adding benchmarks does not have a significantly different treatment effect when the news is bad, as the coefficient $\hat{\beta}_{RB}^{b-g} = 0.1848$ is not significant. This is in line with Prediction 4, as having more actionable data allows salespeople to raise their sales even when they fall short of expectations. Third, compared to rank feedback, providing benchmark data alone increases sales significantly more with bad feedback ($\beta_B^{b-g} = 0.3603^{***}$) than with good feedback.

I can conclude that receiving positive news has no significant effect on performance, as none of the treatment effects are significant then. Giving bad news with rank feedback, however, significantly reduces sales compared to not giving any feedback or giving benchmarks alone. So the driving factor behind the result of both Table 2 and Table 3 is the demoralization effect of being informed of an unexpectedly low rank.

Multi-Tasking

When salespeople learn that they are ranked worse than expected they might be particularly inclined to shift their sales to other brands where they won't receive such negative feedback. As discussed in the theoretical section, see Prediction 3, this reduces sales even more when effort substitution is high – i.e. when increasing effort to sell one brand raises the cost of effort on selling the other brands.

Studying the extent of multi-tasking in this context is empirically challenging as the salespeople work in over 200 independent dealerships. According to a survey I conducted in April 2012, 98.3% of salespeople also sell other furniture brands. The contract they have with the main brand permits them to sell other brands as long as they are not from the four direct competitors. I have very precise data about their sales activities for the main brand but do not have data on their sales for the other brands, as this would involve the release of personnel records by over 200 independent dealerships.

The empirical strategy pursued here instead tests a direct implication of the multi-tasking model. The key insight of the multi-tasking set-up is the effort substitution problem, and I have a direct measure of the extent of the effort substitution parameter δ at the product line level allowing me to test whether the treatments vary with the size of this parameter.

Before I describe the empirical implementation, just one more note on the multi-tasking model: The fact that salespeople can engage in selling more than one brand is clearly undesirable for the main brand. There are three ways to limit this problem. First, as long as the effort substitution is high, raising the commission rate on the main brand will effectively lower effort on the other brand. Second, when the

effort substitution is low then the company could simply hire the salespeople to stop them from selling other brands. Third, it could give the dealerships a direct incentive for not selling other brands.

The furniture company pursued the third path: it gives dealerships a direct financial incentive to keep the share of sales of other brands below a certain threshold. Not all product sales qualify for this reward though. Instead, they categorized – prior and independently from the experiment - each individual product line into two categories depending on whether they induce *high*- or *low-effort substitution* and, in line with the multi-tasking model, they only pay the rebate for the sale of low-effort substitution products. This scheme relies on each dealership providing information about the share of sales of other brands.[24]

To sum up, I can categorize all sales into whether they induce high- or low-effort substitution. More precisely, from the data archives I retrieved, sales at the person-month-product level are categorized by whether they were of the low- or the high-effort substitution type.

Recall from the theoretical section (Prediction 3) that when a salesperson receives bad news and no longer pursues the rank reward, the drop in effort is increasing in the degree of effort substitution.

In terms of identification, the causal interpretation is still available as the company categorized their products into high- or low-effort substitution prior to the start of the experiment.

The results are reported in Table 4, in columns (3) and (4) for the low-effort substitution products and in columns (5) and (6) for the high-effort substitution types. Comparing columns (3) and (5) shows, in line with Prediction 3, that the treatment is only significant for the high-effort substitution types and then only for the "No Rank" treatment.

More important are the results in columns (4) and (6) where I split the results by whether salespeople hear good or bad news. In column (4), focusing on the "No Rank Feedback" treatment, when the salespeople were not told bad news, their low-effort substitution sales increase by 0.1509* (=0.2133-0.0625; P = 0.085) compared to 0.2449*** (=0.2866-0.0417; P = 0.007) for their high-effort substitution sales. Even if the difference between these two coefficients is not significant in a statistical sense ($\Delta = 0.0940, P = 0.300$), the direction and the economic effect are in line with Prediction 3.

These results indicate how the multi-tasking environment is relevant for our understanding of how rank incentives affect performance when the feedback given to agents is better or worse than expected: When people receive bad feedback they could switch to selling other brands to improve their self-image. This result is novel and could not be shown in an environment with only one task.

[24] Clearly, there is an incentive for misreporting by the dealerships of their sales by other brands, which is mitigated by the fact that to qualify for the rebate the dealerships have to consent to random auditing of their sales.

Discussion

Over the last decades, incentive schemes based on behavioral theories have been put forth (Thaler and Sunstein, 2003) to address some of the unintended consequences of purely monetary incentives. They also have the scope to be more cost effective than monetary incentives. Rankings are a plausible candidate for a behavioral incentive scheme as they speak to well-established theories of interpersonal comparisons (Festinger, 1954) and self-image (Benbou and Tirole, 2006).

The field-experimental results presented in this paper confirm that rankings have an important impact on behavior, but given the multi-tasking aspect of the context a new result emerged: People may switch their attention to other tasks when they are being informed that their rank is lower than they expected it to be. I also find significant gender effects which suggests, together with other treatment effects and complemented by survey evidence, that rank incentives may be a taste-based phenomenon and not driven by financial incentives. This study is also novel in that it varied the way rank feedback was shown. More actionable feedback, with the addition of benchmarks, could diminish the negative effect of rank feedback in a multi-tasking environment.

This horse race between several treatment effects within the same experiment is still rather rare in field experimentation, as it requires much larger data sets and longer time-horizons, in this case three years to achieve the required statistical power. Future work should emphasize such experimental designs.

The results from this study can be informative for companies, but also for public institutions where the ranking of teachers and medical professionals is becoming more prevalent.

Given the significant yet intricate behavioral response generated by such a trivial and cost-effective intervention and its long-run consequence on behavior, this topic seems deserving of more field experimentation to extend our understanding of its impact on performance.

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Appendix (not for publication) for "Rank Incentives: Evidence from a Randomized Workplace Experiment"

Treatment-person-year Level Effects

In Table A1 panel A1a, I show the weighted average of monthly sales by treatment and year where the weights are the number of months a salesperson had sales. At the edges of the table, I then report the tests of differences in means. Note that testing differences in means by year is somewhat underpowered given the large variance in performance across salespeople.

In 2009, the difference in means between the "No Rank" and "Rank Feedback" treatment was an increase of 16.4% (p = 0.080) and the same year-level comparison for 2010 reveals an increase of 15.4% (p = 0.024). At the bottom, I can see that adding benchmarks to rank feedback increases sales by 12.0% (p = 0.079). Notably, there is no difference between giving actionable data with rank feedback and not providing any relative feedback. The other year-level comparisons of treatments were not significant.

In the next panel A1b, I only use data for female salespeople. I find no significant differences in means for any contemporaneous pairwise test of differences in means. Lastly in panel A1c, I only use data from men. Here, I find again two results. First, not showing rank information increases sales by 27.5% (p = 0.055) in 2009 and by 17.8% in 2010. This effect is large, but recalling the large standard deviation in sales it is one-fifth of a standard deviation. Second, again there is no significant difference between the full information treatment "Rank and Benchmarks" and the treatment with the least information "No Rank and No Benchmarks."

These results also rule out the theoretical prediction that rank feedback changes the culture of the workplace or induces an update in beliefs that rank will be the basis for future compensation schemes. If that were the case then I should see a change in performance when I add benchmarks to the "No Rank" treatment, but I see no evidence for it in the full sample in 2011 (Δ = -0.0219, p = 0.760), for women (Δ = 0.0629, p = 0.507), or for men (Δ = -0.1279, p = 0.245).

Dispersion of Productivity

The evidence so far indicates that compared to only showing salespeople their rank, either not showing them their rank or adding benchmarks to the rank feedback increases their productivity. The theoretical discussion explains that the response to these treatments could be heterogeneous across salespeople. Informing people that their rank is low may convince them to focus on their other tasks. However, this effect is mitigated by the fact that people with low rank may also have low performance and therefore a poor rank on other tasks. Hence it is an empirical question whether the dispersion of the treatment effect varies across the performance distribution.

To check for this, I use quantile regression methods to estimate the conditional distribution of log productivity at different quantiles . I estimate:

$$\text{Quant}_\theta\left(s_{imy}|.\right) = \beta_\theta^{NR} NR_{iy} + \beta_\theta^{RB} RB_{iy} + \beta_\theta^{B} B_{iy} + \tau_\theta \lambda$$

When compared to showing the rank, the omitted category, I estimate the treatment effect of not showing rank (NR), showing rank and benchmarks (RB), and only showing benchmarks (B) by quantiles . As before, λ are period fixed effects ruling out that any of these effects are capturing month-to-month changes in performance.

In Table A2 report the simultaneous estimates at various quantiles first for all salespeople and then separately by gender. The top panel in Table A2 shows that when estimating quantiles for all salespeople, the effect of not providing rank incentives is positive and flat up to the 90th quantile at which point it is still positive, but only weakly significant (p = 0.109). Not giving rank feedback increases productivity homogeneously by 15.9%, 14.1%, 12.1%, 15.1%, and 7.1% at the 10th, 25th, 50th, 75th, and 90th quantile. There is a no significant difference between these effects except for the drop between the 75th and the 90th quantile as reported at the bottom panel of Table A2.

The effect of adding benchmarks to rank information is also positive and precisely estimated except for the 10th quantile where significance is weaker (p = 0.054). Adding benchmarks to rank feedback increases productivity by 13.1%, 13.0%, 12.4%, 16.1%, and 17.7% at the 10th, 25th, 50th, 75th, and 90th quantile. There is again no significant difference between any of these effects as reported at the foot of first panel. Replacing rank feedback with only giving benchmarks increases productivity solely at the 75th and the 90th percentile by respectively 11.4% and 12.4%, but the size of the treatment effect is the same for all quantiles as confirmed by tests at the bottom of the table.

This study also confirms that gender differences are present in the quantile regressions. For women, removing rank feedback only increases productivity at the 50th quantile by 10.8% and at the 75th quantile by 10.2%. Adding benchmarks to rank feedback does not increase productivity at any quantile. Notably, taking away rank feedback and providing benchmarks increases productivity at the median (16.8%) and farther up by 14.9% (75th quantile) and 16% (90th quantile) even though significance hovers around the 5% level and these treatment effects are not different from those at lower quantiles as reported at the bottom of the panel.

For men, I can again confirm prior results. The effect of not showing rank information is positive and precisely estimated at the 10th, 25, and 75th percentile, the p-value dropping to 0.102 for the 50th percentile. The effect is between 17-25% and several percentage points larger than for the full sample. Adding benchmarks to rank feedback is positive and precisely estimated except at the median. The effect

is stronger than for the full sample at 25-27%. Only providing benchmarks does not raise performance compared to rank feedback for men.

Figures A1a-c provides a graphical representation of the results by plotting the estimates of $\beta_\theta^{NR}, \beta_\theta^{RB}$, and β_θ^{B} at every $\in (0, 1)$, and the associated bootstrapped 90% confidence interval. This emphasizes that the pattern described above holds throughout the distribution of conditional productivity. In particular, the figure shows that: (i) $\beta_\theta^{NR}, \beta_\theta^{RB} > 0$ and is flat for the full sample and for men, but not for women, (ii) I cannot reject $\beta_\theta^{B} = 0$ for the full sample and for men, (iii) $\beta_\theta^{B} > 0$ for women.

The homogeneous treatment refutes the prediction that providing rank feedback will particularly dissuade those at the bottom of the distribution, as then I would have expected to see significant and positive effects only at lower quantiles. It also does not support the notion that rank feedback is essential to those at the top of the distribution, as that would have implied negative treatment effects of not showing rank at the top quantiles, which is not the case. Finally, the gender difference is notable: Rankings do not affect performance for women, but benchmarks alone do. This is consistent, among other mechanisms, with the notion that benchmarks are perceived to be a signal to women that relative performance matters to the principal and might be a preview of changes in the compensation system. It could also be that women find benchmarks alone more motivating than men.

Further Evidence on Treatment Heterogeneity by Gender

In the April 2012 online survey, I collected a set of characteristics and report summary statistics in Table 1B. Indeed, it is apparent that even though men and women are not different in terms of sales-performance, job and life satisfaction, and emotional stability, they differ significantly in terms of age, tenure, marital status, risk attitude, whether they provide their household's principal income, extraversion, conscientiousness, and openness to experiences.

In Table A3, I explore whether the treatment effects significantly vary across these characteristics and whether at the same time the heterogeneous treatment effects by gender remain significant. To read this table, note that at the top it is denoted which interaction terms are included: e.g. in column (1) age is added linearly and as interaction terms in addition to the gender effects, in column (2) it includes marital status, etc.

These samples do not provide enough statistical power to separately estimate all coefficients. Instead, the key statistics of interest are at the bottom of the table where joint tests for treatment heterogeneity were performed for each additional characteristic to show whether the gender effects remain jointly significant. For example in column (1), I test jointly

H_0: age = 0, age*(Nor Rank Feedback) = 0, age*(Rank & Benchmarks) = 0, age*Benchmarks = 0

which has an F-statistic of 3.824 with an associated p-value of 0.00194. The same process is followed across the columns.

Two results emerge. First, in addition to the gender effects, I find treatment heterogeneity (significant at the 5% level) by age, life and job satisfaction. Second, even after controlling for these additional characteristics, the gender effects remain jointly significant.

This exercise gives additional support to the results in Table 3 that the results are gender specific.

Table 1 - Summary Statistics: Balancing Tests and Gender Differences in Observable Characteristics

Table 1A - Balancing tests

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+6$"#6$(#7$)#&-O4'1.6)3#.6$&%(-$%/	9=8> '9=A>AB/	8=;8 '9=;89B/	9=;? '9=BA?:/	9=@A '9=A@CA/
!$7(-$&'16(5#3O)/	9=:8 '9=C99;/	9=CC '9=>C;A/	9=;? '9=BB;8/	9=B8 '9=@@A@/

Table 1B - Gender Differences in Observable characterstics overall and by gender

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-O4'7O)#I-<&%(-$%/ 899:"89;;	B=@88 '9=:>9/	B=@>; '9=::?/	B=C:8 '9=:??/	9=;?>A
!$7(-$&'16(5#3O)/	9=@A9 '9=C:@/			
G$).6$	;AC@=9B ';9B:=AB/	;B@@=:C ';9B9=>?/	;>>;=CB ';9BB=>A/	9=9;?BJJ
D4$	CB=8 ';9=9A/	@9=9C ':=:A8/	C>=A> ':=:8C/	9=9999JJJ
K3)4-$ 'O7 73##$*&5(#L&7(663$*&()*&*3,O65$*M%$+(6(#$*/	9=;;? '9=?;A/	9=9@: '9=8?>/	9=;@@ '9=?>8/	9=9999JJJ
N3,O65$*MK$+(6(#$* 'O7 73##$*&5(#L&%3)4-$&()*&*3,O65$*M%$+(6(#$*/	9=9BB '9=8BC/	9=9@? '9=88@/	9=;;> '9=?8;/	9=998CJJJ
O3%P&D##3#.*$ '%$$&QO#$%/	B=8B: ';=B;C/	B=CA; ';=A@A/	B=;CC ';=BC:/	9=9;?;JJJ
ROH&N3%%(#3%1(5#3O)	;=:CB ';=9;C/	;=:9> '9=::C;/	;=:BC ';=9?9/	9=8:9>
S31$%&N3%%(#3%1(5#3O)	;=@@B '9=A@>;/	;=@@ '9=AC:/	;=@>A '9=A>?/	9=AB?8
TO.%$IO-*U%&7(3)&3)5O7$&%O.65$	9=>;>: '9=CB>@/	9=A;: '9=C@;/	9=@?A '9=C::/	9=9999JJJ
Big Five VW#6(,$6%3O)	@=C:> ';=8@8/	@=8:> ';=8@>/	@=>@@ ';=8BB/	9=999;JJJ
D46$$(H-$)$%%	@=C8A ';=9>:/	@=8C> ';=9C:/	@=@A9 ';=9>@/	9=9999JJJ
XO)%53$)#3O.%)$%%	>=;CA '9=:;9/	>=9CB '9=:8B/	>=88? '9=B:9/	9=99B;JJJ
V7O#3O)(-&K#(H3-3#<	@=@C; ';=;?9/	@=@@ ';=;98/	@=@?C ';=;?;/	9=BC8:
Y+$))$%%&#O&VW+63)5$%	@=BB; '9=:9:/	@=ACA '9=:;?/	@=:B> '9=B:?/	9=999?JJJ

QO#$L&G(H-$&;Z&6$+O6#%&2O3)#&!"#$%#%&#O&#$%#&1O6&H(-()5$&(56O%%&#I$&#6$(#7$)#&46O.+%&[3#I&#I$&(%%O53(#$*&+",(-.$%&3)&+(6()#I$%$%=&GI$&*$+$)*$)#&
,(63(H-$&3%&5O--(+%$*&(#&#I$&(-$%+6O)&-$,$-&()*&3%&#I$&6$46$%%$*&O)&(&%$#&O1&*.77<&,(63(H-$%&3)*35(#3)4&[I(#&#6$(#7$)#&I(#&(-$%+$6$O)&I(%
H$$)&3)&1O6&$(5I&O1&#I$&#6$(#7$)#&<$(6&899:&#O&89;;&%$+$6(#$-<&()*&3)&#I$&-(%#&5O-.7)&(&(56O%%&(--&#I6$$&<$(6%&O3%P&D,$6%3O)&3%&H(%$*&O)&#I$
()%[$6&#O&#I$&\.$%#3O)&]D6$&<O.&4$)$6(--<&(&+$6%O)&[IO&3%&1.--<&+6$+(6$*&#O&#(P$&63%P%&O6&*O&<O.&#6<&#O&(,O3*&#(P3)4&63%P%^&_-$(%$&5IOO%$&(&
).7H$6&16O7&9&#O&;9&[I$6$&9&7$()%&L`)O#&+6$+(6$*&#O&#(P$&63%P%a&()*&#I$&,(-.$&;9&7$()%&L`1.--<&+6$+(6$*&#O&#(P$&63%P%=a&K$$&NOI7$)&$#&(-&
'RVVD&89;;/&1O6&(,(-3*(#3O)&O1&#I3%&3)%#6.7$)#&[I$6$&#I$&7$()&'7$*3()/&3)&(&6$+6$%$)#(#3,$&%(7+-$&O1&c$67()<&3%&C=C8&'@/=

Table 2 Natural log of monthly sales - means and differences in means by treatment groups

Table 2A All salespeople

Treatments		**Rank** mean (std. dev.)	**No Rank** mean (std. dev.)	*Differences in means (robust standard errors)*
No Benchmarks	mean (std. dev.)	[1] 8.577 (1.021)	[2] 8.778 (0.9494)	*[2]-[1] = 0.201*** (0.053)*
Benchmarks	mean (std. dev.)	[3] 8.852 (0.967)	[4] 8.869 (1.046)	*[4]-[3] = 0.017 (0.064)*
Differences in means (robust std. err.)	*[3]-[2] = 0.075 (0.050)*	*[3]-[1] = 0.276*** (0.056)*	*[4]-[2] = 0.091 (0.062)*	*[4]-[1] = 0.292*** (0.066)*

Table 2B Female salespeople

Treatments		**Rank** mean (std. dev.)	**No Rank** mean (std. dev.)	*Differences in means (robust standard errors)*
No Benchmarks	mean (std. dev.)	[1'] 8.530 (0.999)	[2'] 8.696 *(0.932)*	*[2']-[1'] = 0.166*** (0.068)*
Benchmarks	mean (std. dev.)	[3'] 8.756 (0.905)	[4'] 8.879 (1.017)	*[4']-[3'] = 0.123 (0.083)*
Differences in means (robust std. err.)	*[3']-[2'] = 0.059 (0.064)*	*[3']-[1'] = 0.226*** (0.070)*	*[4']-[2'] = 0.182** (0.081)*	*[4']-[1'] = 0.349*** (0.086)*

Table 2C Male salespeople

Treatments		**Rank** mean (std. dev.)	**No Rank** mean (std. dev.)	*Differences in means (robust standard errors)*
No Benchmarks	mean (std. dev.)	[1''] 8.638 (1.048)	[2''] 8.880 *(0.962)*	*[2'']-[1''] = 0.242*** (0.084)*
Benchmarks	mean (std. dev.)	[3''] 8.972 (1.028)	[4''] 8.858 (1.081)	*[4'']-[3''] = -0.114 (0.099)*
Differences in means (robust std. err.)	*[3'']-[2''] = 0.092 (0.079)*	*[3'']-[1''] = 0.333*** (0.089)*	*[4'']-[2''] = -0.022 (0.095)*	*[4'']-[1''] = 0.220** (0.103)*

Note: Tables contain weighted averages of the log of monthly sales by treatment where the weights are the number of months a salesperson in a treatment had sales between August 2009, the start of the experiment, to 2011. The edges of the tables report tests of differences in means between treatment groups with robust standard errors in parantheses. Salespeople in the "Rank" treatment could see their rank in terms of year-to-date sales in North America on their personalized web-report. Salespeople in the "Benchmark" treatment saw the year-to-date sales in North America required on a given day to be in the top 10%, 25%, and 50%. Web-reports were updated daily.

Table 3: Main and Gender Specific Treatment Effects on log of monthly sales by salesperson

Dependent Variable: Log or monthly sales

Ommitted category: Rank Feedback

	(1)	*(2)*	(3)	*(4)*	(5)	*(6)*	(7)	*(8)*	*(9)*
	09-11: Main Effect	*09-11: by Gender*	*09-11: Diff-in-Diff*	2009: Main Effect	*2009: by Gender*	2010: Main Effect	*2010: by Gender*	2011: Main Effect	*2011: by Gender*
Ommitted category:	Rank Feedback	*Rank Feedback*	*Rank Feedback*	Rank Feedback	*Rank Feedback*	Rank Feedback	*Rank Feedback*	*No Rank Feedback*	*No Rank Feedback*
No Rank Feedback	0.109**	*0.160***	*0.117***	0.150*	*0.328****	0.148**	*0.173**		
	(0.0529)	*(0.0723)*	*(0.0527)*	(0.0765)	*(0.117)*	(0.0668)	*(0.104)*		
Female*(No Rank Feedback)		*-0.0965*	*-0.1470***		*-0.306***		*-0.0508*		
		(0.0766)	*(0.0657)*		*(0.132)*		*(0.136)*		
Gender: Female		*-0.0885*	*(omitted)*		*-0.0895*		*-0.0900*		*-0.171**
		(0.0579)			*(0.0606)*		*(0.0981)*		*(0.0949)*
Rank Feedback & Benchmarks	0.125**	*0.203****	*0.0499*			0.113*	*0.168*	0.0532	*0.114*
	(0.0582)	*(0.0779)*	*(0.0611)*			(0.0676)	*(0.108)*	(0.0685)	*(0.107)*
Female*(Rank Feedback & Benchmarks)		*-0.149**	*-0.0381*				*-0.108*		*-0.110*
		(0.0820)	*(0.0709)*				*(0.137)*		*(0.138)*
Benchmarks only	0.0637	*0.00975*	*0.0247*					-0.0280	*-0.123*
	(0.0777)	*(0.0996)*	*(0.0832)*					(0.0702)	*(0.107)*
Female*(Benchmarks only)		*0.0911*	*0.0597*						*0.171*
		(0.102)	*(0.0896)*						*(0.142)*
Marginal treatment effects for female salespeople									
No Rank Feedback (women)		*0.0633*	*-0.0299*		*0.0216*		*0.123*		
		(0.0585)	*(0.0484)*		*(0.0850)*		*(0.0864)*		
Rank Feedback & Benchmarks (women)		*0.0544*	*0.0117*				*0.0597*		*0.00462*
		(0.0641)	*(0.0546)*				*(0.0844)*		*(0.0872)*
Benchmarks only (women)		*0.101*	*0.0844*						*0.0480*
		(0.0868)	*(0.0723)*						*(0.0926)*
Adjusted r-squared	0.0455	*0.0483*	*0.2553*	0.0393	*0.0430*	0.0210	*0.0229*	0.0309	*0.0342*
Month-year fixed effects	**yes**	**yes**	**yes**	**yes**	**yes**	**yes**	**yes**	**yes**	**yes**
Salespeople fixed effects	no	no	**yes**	no	no	no	no	no	no
Number of salespeople (clusters)	1754	*1754*	*1754*	960	*960*	1285	*1285*	1434	*1434*
Number of observations	29722	*29722*	*29722*	7780	*7780*	10360	*10360*	11063	*11063*

Note: Standard errors in parentheses clustered at the salesperson level. $p < 0.10$ (*), $p < 0.05$ (**), $p < 0.01$ (***). Rank is calculated based on year-to-date sales in North America. "No Rank Feeback" provides neither rank nor benchmarks, Benchmarks is the treatment where we showed salespeople what it currently takes to be in the top 10%, 25%, and 50% in North America.

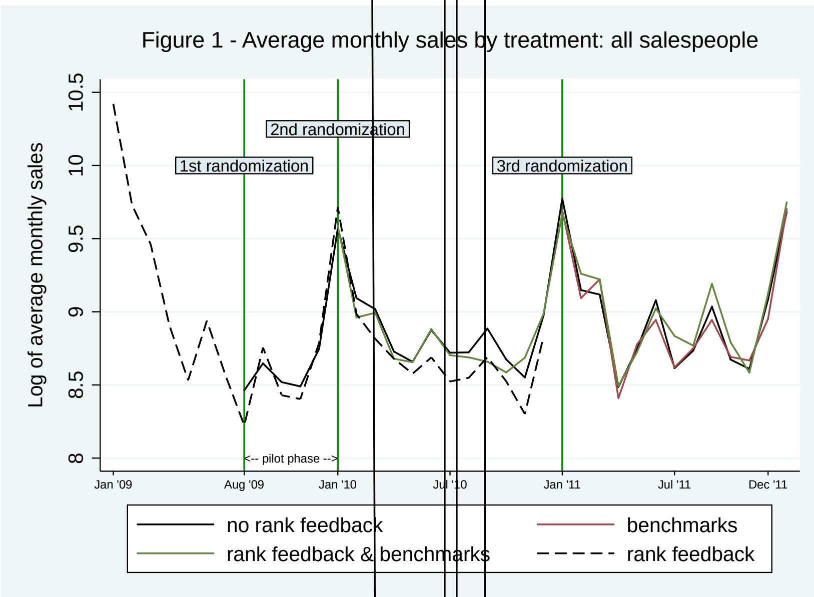
Table 4: Good versus Bad News - All products and by low and high effort substitution products

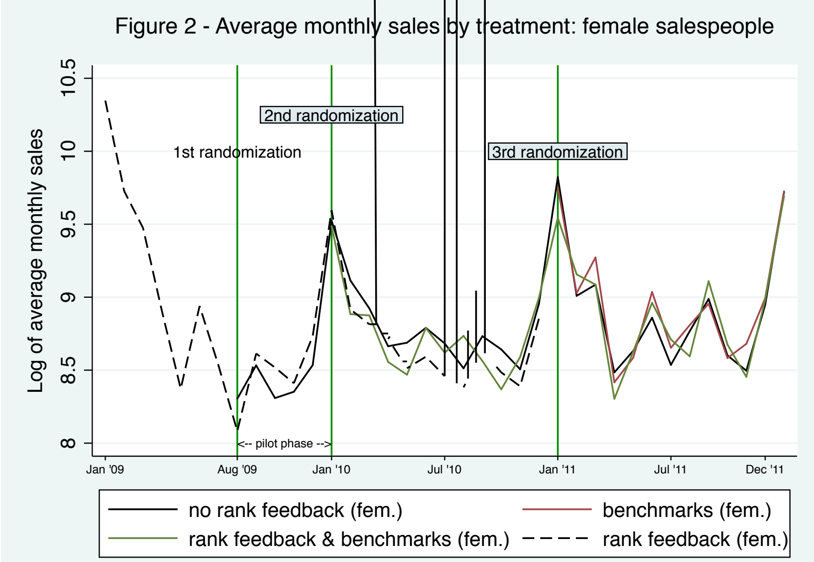
Ommitted category: Rank Feedback

Dependent variable	log(monthly sales) on all products		log(monthly sales) of those furniture products with LOW effort substitution across brands		log(monthly sales) of those furniture products with HIGH effort substitution across brands	
	baseline	Good vs Bad News	baseline	Good vs Bad News	baseline	Good vs Bad News
	(1)	(2)	(3)	(4)	(5)	(6)
No Rank Feedback	0.1250**	-0.027	0.0754	-0.0625	0.1440**	-0.0417
	(0.0629)	(0.0941)	(0.0645)	(0.1004)	(0.0693)	(0.0998)
Rank Feedback & Benchmarks	0.1370**	0.0622	0.0992	-0.0454	0.1617	0.0687
	(0.0633)	(0.0956)	(0.0642)	(0.1047)	(0.0712)	(0.0987)
Benchmarks only	0.0779	-0.0691	0.0864	-0.0559	0.1268	-0.0566
	(0.0841)	(0.1170)	(0.0847)	(0.1184)	(0.0949)	(0.1247)
(No Rank Feedback)*(Bad News)		0.2846**		0.2133*		0.2866**
		(0.0941)		(0.1169)		(0.1206)
(Rank Feedback & Benchmarks)*(Bad News)		0.1848		0.0992		0.1547
		(0.1148)		(0.1277)		(0.1220)
(Benchmarks only)*(Bad News)		0.3603***		0.3012**		0.3240**
		(0.1374)		(0.1423)		(0.1560)
Bad News (=1 when current rank lower than at end of prior year)		-0.8301***		-0.6610***		-0.7353***
		(0.0901)		(0.1095)		(0.0985)
Month-year fixed effects	yes	yes	yes	yes	yes	yes
Adjusted r-squared	0.0339	0.0647	0.0145	0.0369	0.0121	0.0353
Number of salespeople (clusters)	1623	1341	1508	1243	1502	1233
Number of observations	21942	18097	16740	14020	16945	14160

Note: Standard errors in parentheses clustered at the salesperson level. $p < 0.10$ (*), $p < 0.05$ (**), p , 0.01 (***). 'Bad News' is a dummy variable equal to one when the rank in the current month is lower than the rank in the prior year. The sample is therefore restricted to years 2010 & 2011. The dependent variable in columns (1) and (2) is the log of monthly sales; in columns (3) and (4) it is the log of monthly sales on those products that are subject to low effort substitution with products by other brands; in column (5) and (6) it is the log of monthly sales on those products that are subject to high effort substitution with products by other brands. Rank is calculated based on year-to-date sales in North America. The ommitted treatment is with Rank Feedback; No Rank Feeback provides neither rank nor benchmarks, Benchmarks is the treatment where we showed salespeople what it currently takes to be in the top 10%, 25%, and 50% in North America.

Figure 1-3: Timeline of Average Monthly Sales for all salespeople and by gender





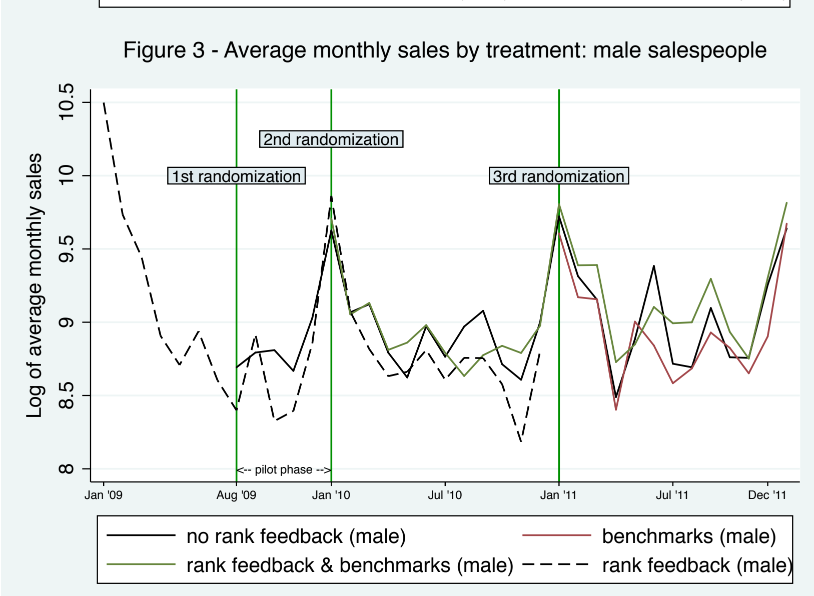


Table A1 Natural log of monthly sales - means and differences in means by year and treatment groups

Table A1b All salespeople

Treatments				Rank mean (std. dev.)			No Rank mean (std. dev.)		*Differences in means (robust standard errors)*		
			[1]	[2]		[3]	[4]	[5]	*[3]-[1]=*	*[4]-[2]=*	
		year	Aug-Dec 2009	2010		Aug-Dec 2009	2010	2011	*Aug-Dec 09*	*2010*	
No Benchmarks		mean	8.403	8.681		8.566	8.834	8.866	*0.1637**	*0.1535***	
		(std. dev.)	(1.2743)	(0.9252)		(1.124)	(0.927)	(0.987)	*(0.093)*	*(0.068)*	
				[6]	[7]			[8]			*[8]-[7]=*
		year		2010	2011			2011			*2011*
Benchmarks		mean		8.801	8.937			8.844			*-0.093*
		(std. dev.)		(0.9321)	(1.027)			(1.072)			(0.073)
	[6]-[4]=	*[7]-[5]=*		*[6]-[2]=*				*[8]-[5]=*			
year	*2010*	*2011*		*2010*				*2011*			
Diff. in means	*-0.033*	*0.071*		*0.1203**				*-0.0219*			
(robust std. err.)	*(0.068)*	*(0.070)*		*(0.069)*				*(0.072)*			

Table A1c Female salespeople

Treatments				Rank mean (std. dev.)			No Rank mean (std. dev.)		*Differences in means (robust standard errors)*		
			[1']	[2']		[3']	[4']	[5']	*[3']-[1']=*	*[4']-[2']=*	
		year	Aug-Dec 2009	2010		Aug-Dec 2009	2010	2011	*Aug-Dec 09*	*2010*	
No Benchmarks		mean	8.320	8.640		8.404	8.769	8.787	*0.084*	*0.129*	
		(std. dev.)	(1.307)	(0.864)		(1.045)	(0.927)	(0.972)	*(0.122)*	*(0.087)*	
				[6']	[7']			[8']			*[8']-[7']=*
		year		2010	2011			2011			*2011*
Benchmarks		mean		8.709	8.809			8.850			*0.042*
		(std. dev.)		(0.872)	(0.945)			(1.041)			(0.093)
	[6']-[4']=	*[7']-[5']=*		*[6']-[2']=*				*[8']-[5']=*			
year	*2010*	*2011*		*2010*				*2011*			
Diff. in means	*-0.060*	*0.021*		*0.069*				*0.063*			
(robust std. err.)	*(0.088)*	*(0.089)*		*(0.085)*				*(0.095)*			

Table A1d Male Salespeople

Treatments				Rank mean (std. dev.)			No Rank mean (std. dev.)		*Differences in means (robust standard errors)*		
			[1'']	[2'']		[3'']	[4'']	[5'']	*[3'']-[1'']=*	*[4'']-[2'']=*	
		year	Aug-Dec 2009	2010		Aug-Dec 2009	2010	2011	*Aug-Dec 09*	*2010*	
No Benchmarks		mean	8.509	8.732		8.784	8.91	8.965	*0.275**	*0.178**	
		(std. dev.)	(1.226)	(0.996)		(1.192)	(0.923)	(1.000)	*(0.143)*	*(0.106)*	
				[6'']	[7'']			[8'']			*[8'']-[7'']=*
		year		2010	2011			2011			*-0.260***
Benchmarks		mean		8.905	9.097			8.837			*(0.115)*
		(std. dev.)		(0.987)	(1.103)			(1.041)			
	[6'']-[4'']=	*[7'']-[5'']=*		*[6'']-[2'']=*				*[8'']-[5'']=*			
year	*2010*	*2011*		*2010*				*2011*			
Diff. in means	*-0.005*	*0.132*		*0.172*				*-0.128*			
(robust std. err.)	*(0.103)*	*(0.109)*		*(0.110)*				*(0.110)*			

Note: Tables contain weighted averages of the log of monthly sales by treatment and year where the weights are the number of months a salesperson in a treatment had sales in the years 2009 to 2011. The edges of the tables report tests of differences in means between treatment groups with robust standard errors in parantheses. Salespeople in the "Rank" treatment could see their rank in terms of year-to-date sales in North America on their personalized web-report. Salespeople in the "Benchmark" treatment saw the year-to-date sales in North America required on a given day to be in the top 10%, 25%, and 50%. Web-reports were updated daily. The "year" rows shows which treatments were in place in a what year. Salespeople were randomly allocated to treatment groups in August 2009, January 2010, and January 2011 and the randomization occured for all salespeople at each of these dates so that treatment groups memberships were uncorrelated across year.

Table A2: Main and Gender Specific Quantile Treatment Effects

Dependent Variable: log of sales by month and salesperson
Ommitted category: Rank Feedback

All Salespeople	(1)	(2)	(3)	(4)	(5)
Quantile	q10	q25	q50	q75	q90
No Rank Feedback	0.159**	0.141***	0.121***	0.152***	0.0709
	(0.0622)	(0.0489)	(0.0387)	(0.0368)	(0.0442)
Rank Feedback & Benchmarks	0.131*	0.130**	0.124***	0.161***	0.177***
	(0.0683)	(0.0518)	(0.0395)	(0.0368)	(0.0485)
Benchmarks only	0.114	0.0727	0.0558	0.114**	0.124**
	(0.0800)	(0.0615)	(0.0490)	(0.0524)	(0.0588)
Period fixed effects	yes	yes	yes	yes	yes
N	29722	29722	29722	29722	29722
Female Salespeople					
Quantile	q10	q25	q50	q75	q90
No Rank Feedback	0.0983	0.0813	0.108**	0.102*	0.0317
	(0.0770)	(0.0628)	(0.0532)	(0.0521)	(0.0571)
Rank Feedback & Benchmarks	0.0750	0.0493	0.0769	0.0745	0.0618
	(0.0846)	(0.0672)	(0.0594)	(0.0542)	(0.0664)
Benchmarks only	0.147	0.0719	0.168**	0.149**	0.160**
	(0.105)	(0.0871)	(0.0740)	(0.0760)	(0.0791)
Period fixed effects	yes	yes	yes	yes	yes
N	16177	16177	16177	16177	16177
Male Salespeople					
Quantile	q10	q25	q50	q75	q90
No Rank Feedback	0.249**	0.196***	0.105	0.171***	0.0429
	(0.0979)	(0.0739)	(0.0643)	(0.0597)	(0.0679)
Rank Feedback & Benchmarks	0.256**	0.251***	0.132*	0.266***	0.245***
	(0.0994)	(0.0803)	(0.0681)	(0.0652)	(0.0746)
Benchmarks only	0.125	0.0234	-0.135*	0.0465	0.0298
	(0.131)	(0.100)	(0.0817)	(0.0835)	(0.0921)
Period fixed effects	yes	yes	yes	yes	yes
N	13545	13545	13545	13545	13545

Note: Quantile regressions with robust standard errors based on 5000 replications.

A C
D

A	(1) A	(2)	(3) D	(4) A	(5)	(6) D	(7) D	(8)	(9) A	(10) C	(11)	(12)
No Rank Feedback	0.379*	0.150	0.161	-0.0534	0.213*	0.129	0.0788	-0.0164	0.483*	-0.0861	0.0736	0.208
	(0.229)	(0.102)	(0.102)	(0.266)	(0.122)	(0.150)	(0.149)	(0.264)	(0.291)	(0.377)	(0.260)	(0.336)
Rank & Benchmarks	0.779***	0.238**	0.250**	0.442*	0.331**	0.251	0.175	0.159	0.570*	-0.330	0.625**	0.321
	(0.272)	(0.111)	(0.111)	(0.264)	(0.135)	(0.158)	(0.161)	(0.257)	(0.304)	(0.418)	(0.273)	(0.361)
Benchmarks	0.384	-0.106	-0.0894	-0.537	-0.231	-0.215	-0.275	-0.599*	-0.179	-0.406	0.00877	-0.593
	(0.311)	(0.141)	(0.140)	(0.350)	(0.160)	(0.190)	(0.193)	(0.329)	(0.400)	(0.502)	(0.393)	(0.485)
Female*(No Rank Feedback)	-0.131	-0.148	-0.150	-0.145	-0.163	-0.163	-0.153	-0.158	-0.132	-0.153	-0.150	-0.148
	(0.0909)	(0.106)	(0.104)	(0.104)	(0.103)	(0.104)	(0.104)	(0.103)	(0.106)	(0.104)	(0.104)	(0.104)
Female*(Rank & Benchmarks)	-0.166*	-0.191*	-0.214*	-0.225**	-0.236**	-0.212*	-0.227**	-0.217*	-0.186*	-0.239**	-0.219*	-0.213*
	(0.0998)	(0.114)	(0.114)	(0.113)	(0.114)	(0.112)	(0.112)	(0.113)	(0.112)	(0.115)	(0.113)	(0.114)
Female*Benchmarks	0.151	0.168	0.141	0.237*	0.195	0.190	0.191	0.134	0.184	0.162	0.186	0.171
	(0.120)	(0.143)	(0.143)	(0.141)	(0.141)	(0.142)	(0.141)	(0.145)	(0.141)	(0.145)	(0.141)	(0.145)
Female	-0.110	-0.0549	-0.0780	-0.0589	-0.0548	-0.0690	-0.0712	-0.0847	-0.0695	-0.0778	-0.0688	-0.0748
	(0.0720)	(0.0811)	(0.0820)	(0.0812)	(0.0816)	(0.0808)	(0.0801)	(0.0799)	(0.0800)	(0.0812)	(0.0814)	(0.0813)
Interaction Variable (INTVAR)	-0.00240	-0.264**	0.0588	0.0339	0.0881	-0.102***	-0.176***	0.0334	-0.00745	0.0346	0.0459	0.0132
	(0.00359)	(0.116)	(0.133)	(0.0224)	(0.0829)	(0.0390)	(0.0588)	(0.0351)	(0.0394)	(0.0499)	(0.0344)	(0.0438)
INTVAR*(Nor Rank Feedback)	-0.00407	0.0756	0.0271	0.0257	-0.0659	0.0188	0.0594	0.0327	-0.0621	0.0398	0.0159	-0.00821
	(0.00450)	(0.145)	(0.176)	(0.0295)	(0.103)	(0.0554)	(0.0723)	(0.0437)	(0.0515)	(0.0599)	(0.0429)	(0.0547)
INTVAR*(Rank & Benchmarks)	-0.0118**	-0.103	0.0222	-0.0217	-0.105	0.00193	0.0618	0.0174	-0.0632	0.0962	-0.0672	-0.0121
	(0.00508)	(0.184)	(0.186)	(0.0291)	(0.113)	(0.0545)	(0.0705)	(0.0445)	(0.0523)	(0.0667)	(0.0446)	(0.0592)
INTVAR*Benchmarks	-0.00826	0.169	0.287	0.0517	0.232*	0.0700	0.127	0.0993*	0.0163	0.0538	-0.0167	0.0873
	(0.00590)	(0.212)	(0.219)	(0.0382)	(0.137)	(0.0651)	(0.104)	(0.0583)	(0.0685)	(0.0811)	(0.0668)	(0.0815)
A A			A	B	A B							
F-statistic	3.824**	2.085*	0.533	1.869*	1.221	3.918***	3.553***	1.619	1.373	1.492	1.012	0.302
p-value	0.00194	0.0653	0.751	0.0975	0.297	0.00163	0.00349	0.152	0.232	0.190	0.409	0.912
				B	B							
F-statistic	3.480***	2.216*	2.667**	2.819**	2.845**	2.925**	2.938**	2.787**	2.367**	2.995**	2.758**	2.701**
p-value	0.00399	0.0510	0.0212	0.0156	0.0148	0.0126	0.0123	0.0167	0.0381	0.0110	0.0177	0.0198
Adjusted r-squared	0.0457	0.0463	0.0448	0.0471	0.0457	0.0475	0.0474	0.0468	0.0457	0.0465	0.0452	0.0446
Number of salespeople (clusters)	1043	770	770	771	770	771	771	771	771	771	771	771
Number of observations	21430	16508	16508	16530	16508	16530	16530	16530	16530	16530	16530	16530

the interaction variable is gender, in column (2) it is age, etc. The regression also includes a constant and a dummy for a treament only used during the pilot phase in 2009 where we provided normalized rank feedback in one treatment arm. See Table 1 for definition of variables

Figure A1: Quantile Regression Estimates

Figure A1a: All salespeople

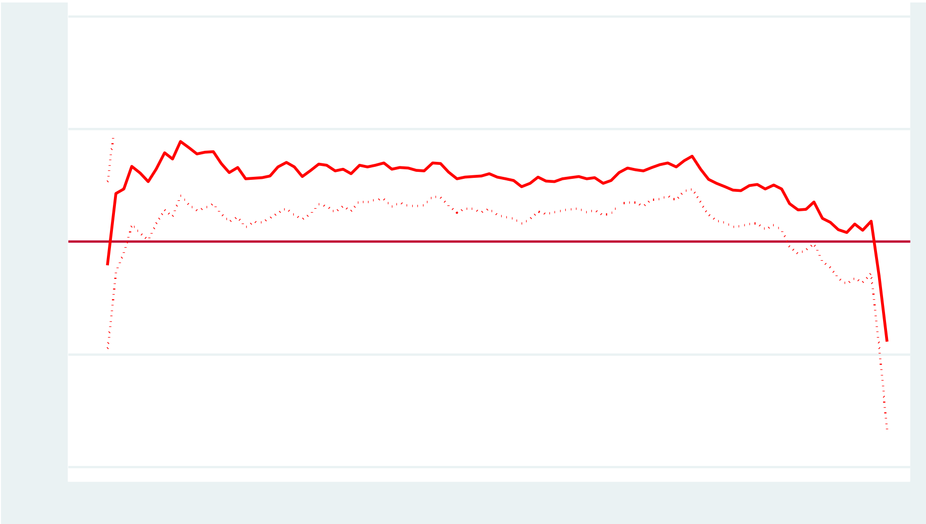


Figure A1b: Female Salespeople

Figure A1c: Men

Note: Quantile estimates of treatment effects compared to "Rank Feedback without Benchmarks". Bootstrapped standard errors with 90% confidence intervals based on 5000 replications.