Better to have led and lost than never to have led at all? Lost leadership and effort provision in dynamic tournaments

Cédric Gutierrez
Bocconi University

Tomasz Obloj
HEC Paris

Douglas Frank
Analysis Group

Forthcoming*: Strategic Management Journal*

Abstract
In this paper, we develop and test a behavioral theory of lost leadership. Using insights from the literature on goals as reference points and prospect theory, we predict that former leaders exert more effort compared to otherwise identical competitors. We test this prediction using two contexts. The first, quasi-laboratory, data comes from an educational business game. The second setting draws on field data from a two-month banking sales contest. We find that provision of effort increases following the loss of leadership. We also explore whether past leaders exert more effort in general or simply shift effort from other, potentially less-salient goals. We find evidence of both mechanisms. Finally, we investigate the temporal effects and find that having been a leader has an attenuating effect on subsequent behavior.

Keywords: behavioral strategy, competitive dynamics, dynamic tournaments, lost leadership, reference points

Acknowledgements: The authors thank co-editor Connie Helfat, two anonymous SMJ reviewers, and Diane Burton, Russ Coff, Felipe Csaszar, Stefano DellaVigna, Henrich Greve, William Hesterly, Phanish Puranam, and Zur Shapira for their comments on earlier versions of this paper. For feedback, the authors are also grateful to various seminar and conference participants, especially to the attendees of the 2017 Consortium for Strategy Research and 2017 Strategic Management Society Conference.

INTRODUCTION

Dynamic tournaments, in which competition unfolds over time, are ubiquitous and include sporting events, sales contests, invitations to tender, promotion tournaments, and intra-firm idea sourcing contests (Fu and Wu, 2019). In such competitions, participants fall in and out of leadership positions as they observe interim feedback about their relative standing. Such changes in leadership during the tournament duration can be frequent. For example, the 1965 Italian Grand Prix auto race featured a Formula-One-record of 42 lead changes, while the 2010 Aaron's 499 NASCAR race featured 88 lead changes with 28 distinct leaders. Similarly, during the 2007 Netflix Prize (a competition to improve movie-suggestion algorithms), there were eight leadership changes and five distinct leaders (Bell, Koren, and Volinsky, 2007). Yet while the loss of leadership in dynamic contests is an important, widespread, and frequent phenomenon, little is known about its direct consequences. Using insights from the literatures on goals as reference points and goal-setting theory, we develop theory and present empirical evidence showing that former leaders try harder: losing a leadership position results in increased provision of effort. We also show evidence of effort substitution, wherein former leaders shift resources away from other goals towards the one from which they have been set back.[1]

Theoretically, we posit two interrelated behavioral mechanisms that cause contestants in dynamic tournaments to exert greater effort after losing the lead. The first mechanism relates to the perceived probability distribution over tournament outcomes, whereas the second relates to the perceived valuation of outcomes (as a consequence of the participant's reference point). Regarding

[1] While in many contexts being a leader requires holding the Number 1 position, in others it is sufficient to be part of a group of leading competitors. Indeed, many tournaments have a multi-prize structure, with either several identical prizes (Clark and Riis, 1998) or prizes that vary in value according to rank (Moldovanu and Sela, 2001). Sisak (2009) discusses multi-prize tournament applications to the domains of rent-seeking, patent and license races, tender, labor markets, universities, and sports. In this paper, we call a *leader* any participant that temporarily holds a prize-eligible position. We refer as a *former leader* to a participant who is not currently a leader but has been among the *leaders* and later lost leadership position before the end of the contest.

the first mechanism, we argue that the event of lost leadership is likely to affect former leaders' subjective beliefs about the probability of winning. Prior work on goal setting suggests that the level of commitment to different goals varies according to their perceived achievability (Koop and Johnson, 2012; Latham and Locke, 1991). Because former leaders may perceive the goal of leading as more attainable, it commands a greater share of resources, including effort.

The second mechanism is based on the observation that temporarily holding a leadership position may result in a shift of reference point, thus altering how decision makers frame their current (and all feasible) market positions. Reference points play a critical role in decision making because they divide "outcomes into gains or losses, thus creating a qualitative difference in the valuation of outcomes slightly above or below that reference point" (Allen et al., 2017, p. 1657). In particular, goals can act as powerful reference points against which achieved performance is evaluated. Obtaining and maintaining a leadership position is undoubtedly a particularly salient goal for any important performance metric (Ferrier, Smith, and Grimm, 1999; Boyle and Shapira, 2012; Ross and Sharapov, 2015). Thus, former leaders may update their reference point to an extreme degree when they fully integrate "being a leader" as a new reference point. Although much research on reference points focuses on risk-taking consequences (Kahneman and Tversky, 1979; Lim, 2015), an individual's or organization's position with respect to a reference point may also have important consequences for effort allocation, through the valuation of a prospect (cf. Abeler et al., 2011). Consequently, former leaders with a higher reference point would exert more effort compared to similarly placed competitors who have never led in the past. Like in the first mechanism, the increased effort may entail a greater total amount of effort or a diversion of effort from other tasks.

We test our predictions in two empirical settings: 1) an educational business simulation in which competing teams of students are evaluated, over multiple simulated years, based on the relative performance of firms that they manage, and 2) a proprietary dataset containing detailed records of 164 banking outlets during a two-month sales contest. Our empirical strategy, using two different types of dynamic tournaments, in essence compares effort allocations between two groups of non-leaders—those who have led in the past and those who have not—who are otherwise identically situated and therefore whose objective probability of winning and expected returns to effort should be the same. We therefore predict and document a purely behavioral effect of losing leadership. In the second setting, we additionally take advantage of a unique feature of the contest design that allows us to observe four parallel tournaments, each with a different number of prizes. This allows us to distinguish more fully the effect of losing leadership from the effects of other events, such as simply dropping in the tournament ranking.

In both empirical settings, we observe frequent leadership changes and link them to subsequent effort by, respectively, teams of students managing virtual organizations and real bank managers selling personal loans. Testing our theory in both contexts has several advantages. First, one setting allows us to benefit from the controllability typical of simulated environments, while the other setting provides the contextual reality typical of observational data. Second, these two contexts allow us to measure effort provision using inputs (time devoted to the business simulation) as well as outputs (realized performance in the banking sales contest). Finally, one setting allows us to observe behavior in a context where all decision makers follow a single goal (winning in a student business simulation), while the other offers evidence from the field, where managers' pursuit of multiple objectives is ubiquitous (Meyer and Gupta, 1994).

We find robust support for our main prediction—that provision of effort increases following the loss of leadership. In contrast, a mere decline in performance ranking, even from highly ranked positions but not involving a leadership change, does not have the same effect. We also investigate the temporal effects of lost leadership and find that having been a leader has an attenuating, though long-lasting, effect on effort. We further explore, in the banking context, whether former leaders exert more effort in general (and specifically towards the focal task that directly affects tournament outcomes) or simply shift effort from other, potentially less-salient goals. We find evidence of both mechanisms.

Our study contributes mainly to the competitive dynamics and behavioral strategy literatures. While the behavioral strategy literature has been growing rapidly in recent years (Levinthal, 2011; Powell, Lovallo, and Fox, 2011), scholars have only begun to understand, and develop theories explaining, the links between competition, micro-level drivers of decision making, and heterogeneity in firm conduct. Prior work in this area has focused on accounting for important micro-level mechanisms in organizations arising from, for example, inequity aversion (Obloj and Zenger, 2017), taste for control (Reitzig and Maciejovsky, 2015), biases in learning (Denrell and March, 2001), and loss aversion and incentive design (Wiseman and Gomez-Meija, 1988). In parallel, a large body of literature drawing on the Carnegie tradition has investigated how managers respond to performance feedback and alter their risk-taking behaviors (e.g., Greve, 1999; Joseph and Gaba, 2015). Similarly, recognizing the psychological and substantive importance of leadership as an individual and organizational goal, several papers have explored the antecedents of contest leadership, leaders' strategies to prevent displacement, and the actions of followers aimed at gaining it (e.g., Cabral, 2017; Wang and Shaver, 2014). In this paper, we add to these strands of literature and focus on the behavioral effect associated with the natural consequence of competition in dynamic tournaments: lost leadership.

We also contribute to the nascent literature on the use of goals as reference points (Allen et al., 2017; Markle et al., 2018). While this literature has so far focused on absolute levels of performance, we show the importance of reference points and temporary goal achievement in settings where relative performance determines rewards. In particular, our results suggest that former leadership in a contest may be associated with a shift in the reference point, leading to increased effort allocation. We show evidence that such increased provision of effort results both from substitution of effort away from leisure and from diversion of effort away from other goals. Thus, our results highlight important trade-offs that organizational designers face when considering a dynamic tournament that rewards performance on a single dimension.

Finally, we contribute to the literature on organizational design and on the use of relative rankings as a source of motivation. Wherever deviations from standard economic models of behavior affect outcomes, organizational performance may be improved if the design of structures and incentives takes these deviations into account. For example, extant tournament theory predicts the optimal number of prizes in contests such as the one we study (Boudreau, Lacetera and Lakhani, 2011; Connelly et al., 2014). However, our results show that there is an important behavioral effect associated with temporarily holding a prize-eligible position. This implies, for instance, additional benefits of multi-prize tournaments over single-prize contests, as the number of prize-eligible positions will affect the number, and frequency, of events of former leadership. Similarly, our results highlight the importance and additional consequences of providing tournament participants with interim performance feedback.

THEORY DEVELOPMENT

Effort allocation in (dynamic) tournaments

Tournament theory, originating with foundational work by Lazear and Rosen (1981), models behavior in contexts where relative, rather than absolute, performance levels among a well-defined set of peers determine rewards. The basic framework assumes standard preferences and predicts that in fair contests (i.e., in which participants' ex-ante chances of winning are identical), all rivals exert the same amount of effort, and the winner is determined randomly. A desire to predict contestants' actual levels of effort and find the optimal contest design—one that maximizes the net returns to the designer—has led researchers to focus on key tournament parameters such as the number of prizes, the reward structure, and the number of contestants (for a review, see Connelly et al., 2014).

The classical tournament model is a *static* one, in which contestants' choices are collapsed into a single effort allocation decision and the outcome is decided in one period. More recently, the classical approach has been extended to explicitly account for an important real-world feature of most contests: their *dynamic* structure, meaning that competition unfolds over time. In these types of contests, as in our empirical settings, participants can receive interim feedback about their own and/or their competitors' performance, which in turn can affect behavior in subsequent stages. Such dynamics, if present, could have important implications for predicting participants' effort levels and thus for designing optimal tournaments. Despite the obvious importance of tournament dynamics and the feedback mechanism, both theoretical and empirical "literature on feedback in contests is sparse" (Mihm and Schlapp, 2019, p. 561).

Although some of the theoretical results from static contests carry over to dynamic ones (Aoyagi, 2010; Gershkov and Perry, 2009), dynamic tournaments also differ significantly from static ones in that participants' effort levels may vary in response to their changing relative positions over time.[2] For instance, Casas-Arce and Martínez-Jerez (2009) propose and test a model where a contestant's probability of winning depends on both her distance from the leader and the effort exerted in the current round. Ederer (2010) develops a similar model but allows for performance to depend not only on effort but also on ability. In a study on innovation contests, Mihm and Schlapp (2019) confirm results from earlier research predicting no difference in effort between leaders and followers in the case of *public* feedback, but the authors also show that providing contestants with *private* feedback about their own absolute performance generates heterogeneity in effort among competitors. Both Ederer (2010) and Mihm and Schlapp (2019) assume that contestants update their beliefs about their ability or chances of success according to Bayes' rule and that the feedback is non-deceptive (see Marinovic, 2015 for a model with deceptive feedback). These models thus do not explain a difference in behavior between two tournament competitors (one being a former leader) holding the same position, because of the assumed symmetry in updating following reception of positive and negative feedback.

Models incorporating asymmetry between players similarly find heterogeneity in effort among tournament participants, but focus on conditions that pre-date the tournament. For instance, Yildirim (2005) suggests that in an asymmetric tournament with a favorite and an underdog, the favorite acts more aggressively and directs more effort toward winning than the underdog does. While this result is driven by the assumed asymmetry in the players' probabilities of winning, Yildirim explicitly acknowledges that a similar "asymmetry between players might also arise if each player values the prize differently" (2005, p. 217). Kräkel (2008) proposes a similar model incorporating differences in the favorite's and the underdog's valuations of the prizes. In this case,

[2] In the paper, we focus on dynamic tournaments that are resolved based on the *aggregate level of output* over multiple rounds. This is the most common type of dynamic tournament in the literature and the one that directly matches our empirical settings. However, we acknowledge that related types of tournaments can also lead to heterogeneous levels of effort. This is the case, for instance, in a tournament in which winning depends on the number of rounds won and not on accumulated performance or when contests take the form of Stackelberg game (see for example Goltsman and Mukherjee, 2011).

optimal effort levels and best responses depend on the (public) value that each of the actors ascribes to the outcomes of the contest. Importantly, and unlike our context, these studies assume that the asymmetry between players is static and precedes the tournament, rather than emerging over time. Similarly, these theoretical models assume that tournaments have a "memoryless" quality: a contestant's effort choice is determined only by the current state of the tournament, and not by her past performance.

Taken as a whole, extant tournament theory—and in particular its formal extensions to multi-period, dynamic settings—may explain why former leaders' optimal effort allocations may differ from those of identically placed competitors with no history of leadership. Such differences may arise when the event of having led the tournament affects participants' *objective* probability of winning the contest or the *objective* value of the payoffs. However, in many dynamic contests, including the ones we study, being a former leader is associated neither with the objective probability of winning the contest nor the potential winnings (the value of the prize or the spread in prizes). In such contests, to the best of our knowledge, existing theory cannot predict a difference in behavior between two equally placed competitors, one of whom is a former leader. Commenting on this gap, Fu and Wu note that "it is imperative that contest modeling incorporate new elements (e.g., behavioral components)" (2019, p. 40). Below, we develop precisely such a behavioral account of lost leadership in dynamic contests.

Behavioral theory of lost leadership

Existing work provides some preliminary evidence that behavioral forces are likely to play an important role in explaining effort allocations in dynamic contests. For example, using a real-effort laboratory experiment, Gill and colleagues (2019) find that, in a series of consecutive static tournaments, subjects systematically depart from predicted levels of effort. In particular, the authors observe increased levels of effort for those ranked first or last, a behavior consistent with increased subjective valuations of the currently held top rank and increased aversion to the currently held bottom rank.[3] Boyle and Shapira (2012) used 48 editions of the *Jeopardy*! game show's Tournament of Champions as a natural experiment. The study exploits the fact that contestants can qualify for the second round either by winning in the first round or by being among the four non-winners with the highest scores. The authors show that leaders take excessive risks in narrowly focusing on the first qualification route, demonstrating what the authors refer to as the liability of leading. Finally, Casas-Arce and Martínez-Jerez (2009) allude to the possibility that equally placed followers may show heterogeneous levels of effort but "leave for future work the exploration of the theoretical underpinnings of this effect" (p. 1316).

Filling in this gap, and focusing on lost leadership (an event that can only happen in dynamic contests), we conceptualize two deeply related yet distinct mechanisms that lead to the prediction that former leaders will provide greater effort compared with rivals who are identical in all respects except for the history of temporary leadership. The first mechanism follows the intuition that contest participants behave according to reference-dependent preferences and that former leaders are likely to have a higher reference point on the relevant performance dimension. Because of loss aversion, such contestants would derive greater utility from achieving the leadership goal than would rivals whose reference point has not been adjusted to the same extent. This effect would prompt former leaders to exert more effort without requiring any change in the structure of subjective beliefs about the chances of winning the prize. A second, and related, mechanism arises if past leadership leads to biased beliefs—i.e., if otherwise identically situated

[3] The real effort task consisted of either word spotting or solving numerical tasks. The tournament examined by Gill et al. (2019) differs from ours as their experiment was designed as a multi-battle contest instead of a multi-round contest. In these types of contests, payoffs are determined based on the performance on each separate round and not on the aggregated performance (see Fu and Wu, 2019 for a classification of contests).

individuals vary in their subjective probability of achieving a goal. We discuss each of these mechanisms in turn and offer our theoretical prediction.

Reference points are a key behavioral construct that can affect how individuals and organizations evaluate their performance. Because reference points determine "the extent to which outcomes constitute gains […] or losses" (Holmes et al., 2011, p. 1072), prior work has focused on the role that they play in affecting individual and organizational risk taking, as well as an array of strategic decisions such as acquisitions (Kumar et al., 2015), R&D investments (Lim, 2015), and IPO pricing (Leitterstorf and Rau, 2014). Although the majority of prior work has focused on the risk consequences of comparing performance with a reference point, recent work has begun to extend theoretical predictions to the allocation of effort (Abeler et al. 2011; Hossain and List, 2012).

One of the key challenges of applying reference-dependence theories, such as prospect theory, is to identify the reference point that the decision maker under study uses to evaluate an outcome (for a review, see Barberis, 2013). This issue is independent of the actual behavior (e.g., risk-taking or effort allocation) that one aims to explain. While early empirical studies equated the decision maker's current position, also called the status quo, with the reference point (Tversky and Kahneman, 1991), recent studies have begun to consider non-status-quo reference points as key drivers of behavior (Abeler et al., 2011; Pope and Schweitzer, 2011). In particular, scholars have recently begun to explore the role of goals as reference points and their impact on performance. Indeed, goals often "inherit the properties of the [prospect theory] value function"—including, for example, loss aversion—and hence affect subsequent conduct accordingly (Heath, Larrick and Wu, 1999, p. 95).

Although reference points significantly affect individual and organizational decision making when rewards depend on absolute performance (e.g., Barberis, 2013), they may also play a particularly influential role in dynamic tournaments. In an experimental asymmetric contest, in which some participants receive a premium that makes them favorites, Chen, Ham and Lim (2011) find that effort allocation is well explained by a reference-dependence model; a favorite applies a higher reference point than an underdog and consequently experiences a disutility from losing that is greater than the extra utility an underdog would gain from winning. Similarly, Kräkel speculates that "a favorite who is more likely to win has a higher reference point than the underdog who is expected to lose" (2008, p. 206).

While the previous examples focus on asymmetric contests, this difference in reference points among competitors can also emerge over time in a fair contest, in which no competitor is favored. This is because an interim ranking can provide a substantial impetus for reference point adaptation (Bothner et al., 2007).[4] Importantly, this adaptation is likely to be systematically different for those who lose the leadership position compared with those who simply rise or fall in the rankings without a change in leadership status. Indeed, prior research has found that reference point adaptation is only partial and, in particular, stronger and faster following a gain than following a loss (Arkes et al., 2008; 2010). As an illustration, people tend to adapt immediately to a tax cut but take more time to adapt their reference point to an increase in the tax rate (Bernasconi, Corazzini, and Seri, 2014). The framing effect associated with a shift of reference point is "closely related to other behavioral anomalies, such as the endowment effect" (Hossain and List, 2012, p. 2151). That literature reports similar results in which the effect of an endowment persists once the object is no longer possessed. Such an asymmetry in reference point adaption explains why

[4] Note that in their paper, Bothner et al. (2007) used the current position in a tournament as a competitor's reference point. In other words, they assumed perfect and immediate adaption of the reference point.

"adaptation to losses takes longer than adaptation to gains" (Strahilevitz and Loewenstein, 1998, p. 281).

In our context, these results suggest that a competitor who reaches a leadership position should strongly and immediately update her reference point with respect to this goal. However, the loss of leadership should produce a smaller and slower downward shift (and potentially no shift at all), thus leaving a former leader with a higher reference point than that of an equivalently placed competitor with no history of leadership. Former leaders are thus more likely, on average, than non-former leaders to perceive any outcome other than leadership as a loss. Because of loss aversion—the fact that "changes that make things worse (losses) loom larger than improvements or gains" (Kahneman et al., 1991, p. 199)—former leaders will value winning the prize more than will identically situated competitors who have never led. This, in turn, will cause former leaders to exert more effort to achieve leadership.

In parallel, the literature on goal setting (see Locke and Latham, 2002 for a review) suggests that the perception of goal attainability is another important element that affects the effort exerted toward achieving a goal (Latham and Locke, 1991). In dynamic tournaments, this suggests that contestants' biased beliefs could be a second behavioral mechanism that should affect effort choices—i.e., if former leaders believe that winning the tournament is more attainable for them than for equally placed competitors who have never led. This could happen if competitors update their beliefs asymmetrically (Elfenbein et al., 2017) rather than symmetrically (Casas-Arce and Martínez-Jerez, 2009).

Prior literature indeed suggests that asymmetric updating occurs when information has a valence, i.e. when it can be interpreted as a good or bad news (Sharot and Garrett, 2016). In particular, positive information is likely to elicit a stronger reaction (Eil and Rao, 2011). Due to the “salience and uniqueness of the leader’s position” (Boyle and Shapira, 2012, p. 4), competitors who achieve interim leadership could therefore strongly update their beliefs about the chances of winning but not readjust them to the same degree after falling down the ranking and losing leadership. Indeed, “small victories,” such as the completion of intermediate goals, can disproportionately increase the perceived attainability of a goal (Gal and McShane, 2012). Similarly, past success has been shown to have a positive effect on the amount of effort directed toward a goal (Nunes and Dreze, 2006).

By extension, temporarily holding a leadership position in a dynamic tournament may cause contestants to revise their subjective beliefs about the attainability of ultimate leadership, leading to greater provision of effort.[5] This mechanism, which relates to the perceived probability distribution over outcomes, is distinct from reference point adaptation, which relates to the valuation of outcomes (the marginal value of winning the contest, which depends upon the reference point). Both mechanisms, however, generate a consistent theoretical prediction:

> **Hypothesis:** *Former leaders exert more effort toward achieving ultimate leadership in a dynamic contest compared with equally placed contestants who did not achieve temporary leadership.*

Our hypothesis can help explain differences in effort allocations in a dynamic contest between former leaders and comparable rivals who have never led in the past. However, by itself it does not predict where this effort comes from—i.e., whether it is “new” effort (i.e., working harder in general) or diverted effort (i.e., substituting effort away from other organizational goals). While the literature to date has generally focused on a single reference point and on a single goal more generally, some scholars have modeled, and tested empirically, multiple, co-existing reference

[5] Following prior work on cognitive biases, we assume that tournament participants are, to some extent, boundedly rational and rely on two modes of thinking and deciding (Kahneman, 2003). Thus, we explicitly assume away a possibility that other tournament participants integrate our predicted response to lost leadership into their best response functions.

points. Indeed, individuals or organizations often pursue multiple goals simultaneously (Ethiraj and Levinthal, 2009; Obloj and Sengul, 2020). In such instances, given constrained resources, actors must prioritize across goals. An extension of our hypothesis is that, in the presence of multiple goals, the predicted increase in former leaders' effort can result from greater aggregate provision of effort, substitution of effort away from other goals, or both. We have no theoretical prediction about the relative importance of changes in aggregate effort versus substitution but leave it as an empirical question.

EMPIRICAL APPROACH

Observing and identifying actual behavioral mechanisms in the field is often prohibitively difficult. Therefore, ascribing documented heterogeneity in behaviors to the underlying cause hinges on a careful identification strategy, precise measurement of hypothesized effects, and refutation of alternative explanations. Still, as is common with non-laboratory data, identification of the true mechanism driving some of the data may be hard to attain, a limitation that applies to our work. At the same time, showing empirical evidence of predicted behavioral effects outside of the lab is a crucial endeavor to which we aim to contribute. Indeed, there is an ongoing debate in which some scholars question the applicability of purely experimental lab-based evidence to real-life decision making and call for more tests of the importance of behavioral mechanisms in the field (e.g., Barberis, 2013). Underscoring this need are recent results from Esteves-Sorenson (2018), showing that workers who exhibited strong prosocial behaviors in the lab did not actually behave prosocially in the field. Consequently, we test our hypotheses in two different studies: one using data from an educational business simulation and another relying on observational data from a retail bank. The two contexts are complementary in addressing some of the difficulties just discussed. Study 1 allows us to observe the effort consequences in a controlled, but single-task environment. Study 2, although introducing some noise associated with data from the field, allows us to test directly for effort substitution across diverse tasks as well as better rule out alternative explanations for the empirical patterns that we document.

Study 1

We use a dataset containing decisions and performance outcomes of 295 teams competing in 49 distinct computer-assisted business simulation games. In each of the games, an average of six teams of 4 to 6 students managed virtual firms and made complex strategic decisions over multiple time periods (simulated years). The decisions spanned major functional areas such as marketing and sales (e.g., setting prices and predicting sales for a range of products), R&D (e.g., technology development and certification), HR (e.g., hiring and wages), and finance (e.g., debt). All firms were identical at the start of the simulation. As the game unfolded, the participants could choose to develop new technologies, exit or enter different consumer segments, and internationalize their operations. After each round of decisions, teams' performance was calculated based on three criteria: profits (return on capital employed), people (employee morale index), and planet (environmental sustainability index). This composite metric, set initially to 100 for all teams, determined the interim rank and ultimate winners.

The average duration of each simulated year (period) was close to 1.5 hours and a modal game lasted for seven periods played over two consecutive days. After dropping eight observations due to erroneous data entry by students or missing time-stamp information, and accounting for the lagged structure of our models (performance outcomes of decisions from round 1 are observed at the start of round 2), the final dataset comprises 1,717 firm-year observations. Figure 1 presents a sample screenshot from an actual in-class debriefing presentation whereby each team could follow its performance.

----- Insert Figure 1 about here -----

Measures

Dependent variable

One strength of the simulation data is that we observe all actual managerial decisions in a controlled environment. For instance, participants were randomly assigned to teams, and all firms were strictly identical at the beginning of the game. This reduces potential confounds that could be due to historical performance, unobservable internal firm politics, or heterogeneity in team composition. Other potential confounds due to differences in the external environment or demand characteristics were also reduced as all teams had access to the exact same sources of market intelligence. Finally, using controlled-environment data allows us to observe and measure each team's effort directly by focusing on input rather than output-based proxies. We use two measures of effort. The first, time spent on each round, is a commonly used proxy for cognitive effort (Garbarino and Edell, 1997).[6] It is calculated as the number of minutes from the beginning of the round until the team closed its decision-making panel (thus sending its final set of decisions to the central system). Most rounds lasted for 90 minutes, but participants had an option to finish the round earlier (or later, as explained below). On average, teams used 86 minutes to complete each round. The second measure of effort is a binary variable denoting whether a team decided to extend its decision-making period into its free time (break). Technically it was possible to substitute leisure time for work, and some teams (with a frequency of 20 percent) chose this option. This measure is a strong proxy for effort as it denotes "overtime" work that is particularly costly.[7]

[6] Previous research has found that the time spent working on a given task correlates with other measures of cognitive effort (Bettman et al., 1990). It has also been frequently used as a measure of effort in experimental work in both economics (Abeler et al., 2011) and psychology (Heyman and Ariely, 2004).
[7] In 85 cases, the timestamp did not allow us to determine this variable unambiguously. We drop these observations from our analyses. Again, drawing on the notion of loss aversion, we can expect that the utility lost from having 1 less minute of leisure time was greater than the utility gained from having 1 more minute of leisure time (for an example of asymmetric behavior for gains and losses of time, see Abdellaoui, Gutierrez and Kemel, 2018).

Independent variables and controls

Each round, we coded as leader the highest-ranking team on the composite performance metric in a given business simulation game. We define *former leader* as a binary variable that takes the value of 1 for a team that does not hold a leadership position in any given round but has held one in the past, and 0 otherwise. To study the decay of the lost leadership effect, we split the former leader variable into *old former leader* (equal to 1 for former leaders whose last leadership was more than 2 rounds in the past) and *recent former leader* (equal to 1 for former leaders who have led in the two rounds preceding the observation).[8] To account for dynamic tournament incentive effects we control for *trailing distance*. This variable is equal to 0 for the current leader and, for all other teams, is measured as the absolute value of the difference between their performance and the performance of current leader. Finally, in all of our models, we include firm (team) and period fixed effects.

Results

Table 1 presents fixed-effects regression results of the impact of former leadership on effort. The errors are clustered by simulation game (Cameron and Miller, 2015). These analyses provide preliminary support for our prediction, that former leaders exert more effort than their comparable competitors who have never led. Keeping all other variables at their means, former leaders are 16 percent more likely to substitute work for leisure (β=0.66, 95% CI: [0.21,1.11]). Their predicted decision time is also longer (on average by over 3 minutes against an average round duration of 86 minutes, β=3.40, 95% CI: [0.38,6.42]). Although our time series is relatively short, we also observe a decay effect of effort allocations. On average, the estimated effects for recent former

[8] We opted for such dichotomization as the variable former leader and a continuous measure of time since lost leadership were too highly correlated to include them jointly in our models. We keep this approach for Study 2 to maintain consistency of analyses.

leaders are larger and more precisely estimated than for old former leaders. Indeed, recent former leaders' decision-making time is almost 7 percent longer than the sample average. In other words, the behavioral effect of lost leadership seems to fade away.

---- Table 1 about here ----

Overall, these results provide some preliminary evidence that losing leadership is associated with increased provision of effort. The data are also consistent with the attenuation effect. This said, the results presented above must be treated with caution due to some limitations of our empirical context. First, the evidence comes from a simulation game played by students and, although we do observe intense competition between teams, the actual stakes are relatively low. Second, the duration of the contest is relatively short. In any given simulation we could observe a maximum of five changes in leadership. Third, this empirical context is one in which participants maximize a single, well-defined performance metric, and hence it does not allow us to study possible effort substitution between two different performance metrics. Finally, there is growing evidence that participants may behave differently when making simulated business decisions than when making decisions in the field (Gneezy and List, 2006). Thus, we would like to show that our results extend to real organizations, which is the purpose of our second study.

Study 2

In our second study, we rely on a proprietary dataset containing daily decisions of all branches of a European retail bank throughout a two-month, cross-branch sales tournament. The bank we study employs several thousand people and serves hundreds of thousands of customers yearly. Its focus is on the sale of simple financial products, such as deposit accounts and small personal loans, to mass-market customers. The bank operates through a network of branches (also called outlets) located in mid-size to large towns. A typical outlet employs three to four salespeople.

The bank therefore has a typical multi-unit structure. Consequently, our level of analysis is the small team, which constitutes a business unit of the focal organization. This dataset is well suited to test our theory for three main reasons. First, it contains daily business unit decisions and outcomes, enabling us to observe changes in effort across outlets as the tournament unfolds. Second, it contains information on *all* outlets of the bank over the *entire* period of the sales tournament. Hence, it captures all longitudinal and cross-sectional variation without suffering from sample selection bias, attrition, or censoring. Third, the information about relative ranking in the tournament was communicated on a daily basis to all outlets, ensuring a feedback structure in which the loss of leadership event was observable not only to the researchers but also salient to the tournament participants.[9]

There were 164 branches participating in this two-month sales contest. Throughout the contest period, outlets were ranked each day according to the cumulative number of "primary" personal loans sold. These loans had to be sold to a first-time customer (hence their designation as primary) in order to count towards the tournament metric. Outlets also concurrently sold "secondary" loans to returning customers. These secondary loans were much less incentivized, a feature that we exploit in some of our supplementary analyses. At the end of the tournament, all employees at the top-ranked outlets received a prize of a one-week holiday at an exotic resort, paid by the bank. To level the playing field among contestants, outlets were "handicapped" in the sense that each outlet's performance was measured as the quantity of loans it sold during the tournament divided by its monthly average in the four months preceding the contest. Because the bank announced the contest just four days before it began, outlets had little opportunity to influence their performance benchmark and hence the handicapping algorithm. These are essential features

[9] A fuller description of this data and sales contest is available in Obloj and Zenger (2017).

of our context as they conform to theoretical dynamic tournament models that posit fair competition (i.e., where participants have an equal ex ante chance of winning).[10]

In practice, the contest consisted of four parallel tournaments: each outlet was assigned to one of four 41-outlet groups according to its pre-tournament performance benchmark. The number of prizes available (i.e., the number of outlets that could win the holiday) varied by group: Group 1 (outlets with the lowest benchmark) competed for one prize, Group 2 for two prizes, and so on up to four. Competition for prizes was solely within, not between, tournament groups. Each day, one hour before the official earliest opening time, the interim contest results (rank and performance of all outlets) were electronically distributed to outlet managers. Based on these rankings, each day, we coded as leading those outlets that occupied one of the prize-eligible positions in their respective tournament group. For example, we defined leading outlets as those holding ranks 1-3 in tournament Group 3, and those holding ranks 1-2 in tournament Group 2. The main data set consists of 7,959 outlet-day observations. For some of the robustness checks described below, we also use available data from the period preceding the tournament.

Measures

Independent variable

Consistent with Study 1, we define *former leader* as a binary variable equal to 1 if an outlet does not currently hold one of the prize-eligible positions but has done so in the past, and 0 otherwise. As discussed above, we observe four distinct tournament groups, with the number of identical prizes in each group varying from 1 to 4. Therefore, for example, in two of the tournament groups a Number 3 position denoted leadership, while in one group, only the Number 1 outlet could lead.

[10] In the Online Appendix, we show that outlets' ranking according to their historical performance benchmarks does not predict the final ranking in the tournament (see Table A3). In other words, the handicapping algorithm worked, and the winners of the contest were not the outlets with the highest pre-tournament performance.

As in Study 1, we further refine the former leader variable into *old former leader* and *recent former leader*, with "recent" here defined as leadership within the past 14 days.[11]

Dependent variable

Following common practice in labor economics (e.g., Lazear, 2000, Casas-Arce and Martínez-Jerez, 2009; Hossain and List, 2012), we operationalize the dependent variable, *effort,* using daily productivity.[12] As mentioned before, the tournament's objective function was to maximize sales of personal loans relative to an outlet-specific pre-tournament benchmark. We thus use as our dependent variable the number of loans sold divided by the benchmark. We further take advantage of some specificities of our empirical context to test for a possible substitution effect between the leadership goal and alternative goals. First, we separately measure the effect of lost leadership on sales of *large primary loans* and *small primary loans*. The tournament did not replace but rather supplemented an existing incentive regime. Alongside the tournament, outlets continued to operate under a system whereby they received a per-loan piece-rate bonus once they passed a performance threshold. In other words, among a multitude of goals that outlets faced, three were particularly important: 1) being eligible for a prize in the tournament, 2) achieving the monthly bonus, and 3) selling other products. The performance threshold used to compute the piece-rate bonus was based on the dollar *volume* of loans sold. In contrast, tournament performance was measured exclusively in terms of the *number* of loans sold, independent of their actual monetary value. In the absence of the piece-rate bonus goal, an outlet's optimal strategy would be to sell the maximum possible number of very small personal loans in order to maximize its chances of winning the tournament. This is because sales of larger loans, on average, entail greater effort than sales of smaller loans.

[11] 14 days is the mean amount of time since lost leadership in our sample, for former leaders. We check the robustness of our results to all periods between 7 and 14 days and find consistent results.

[12] Lazear (2000) used daily productivity to test the proposition that average effort increases after introduction of a piece rate incentive schemes. Similarly, Casas-Arce and Martínez-Jerez (2009) use several measures of output (e.g., sales) to test the proposition that effort in dynamic contests is affected by distance from winning position.

However, in doing so, the outlet risked not being eligible for the piece-rate bonus and thus receiving neither the performance-based compensation nor the tournament prize in the event of not winning the contest. Although, for confidentiality reasons, the bank did not share with us the exact value of each loan, it categorized them into five groups, based on their size. We define a small loan as belonging to group 4 or 5.[13] We expect the effect of former leadership on effort to be particularly strong for small loans as they contribute more to the tournament goal and less to the alternative goal.

Second, we analyze the effect of lost leadership on secondary loans (loans to customers who have already been granted a primary loan in the past). These loans were incentivized with a separate piece-rate plan, but their sales did not contribute to the tournament goal. Additionally, the piece rate for these loans was much weaker than that for primary loans, reflecting the bank's focus on extending the consumer base rather than "milking" the existing base.[14] Indeed, at the time of the study, primary loans accounted for over 50 percent of total sales but over 70 percent of pre-tax profits from personal loans. If the effects of lost leadership generate spillover effects across tasks, we expect that lost leadership would increase the substitution effect between the tournament goal and the secondary loan sales goal, in the sense that former leaders should sell fewer secondary loans compared with equally positioned outlets that have never led.

Control variables

Our main controls aim to account for the influences of monetary incentive structures on bank managers' effort. First, as mentioned above, the bank's sales contest was a temporary incentive plan that supplemented an ongoing incentive program in which outlet employees received piece-

[13] Every loan is categorized by the bank's risk department as belonging to one of five size categories, from 1 (largest) to 5 (smallest). We define "small loans" as categories 4 and 5. Our results are robust to defining small loans as category 5 only or as categories 3 through 5 inclusive.

[14] The average piece-rate bonus was approximatively 10 times larger for primary loans than for secondary loans.

rate bonuses for sales of personal loans. The bonus rate varied with the level of performance against a monthly target, with corresponding variation in the immediate marginal returns to selling loans. To control for possible confounding effects of this second incentive scheme, we therefore include the variable *piece rate,* measured as the outlet's exact position with respect to its sales target on a given day, which fully determines the bonus rate for the marginal loan sold.

Second, we also wish to control for dynamic tournament incentive effects. For example, branches with a performance too far below the winning position (or with a significant lead over followers) may also reduce their effort since their chances of winning (losing) the lead are small (see Casas-Arce and Martínez-Jerez, 2009). To account for such potential effects, we measure outlet *i'*s *trailing distance* as its performance distance behind the nearest prize-eligible competitor on day t: i.e., $trailing\ distance_{i,t} = max\ \{P_{j,t-1} - P_{i,t-1}\ ,\ 0\}$, where P is the tournament performance measure described earlier and j indexes the outlet occupying the lowest prize-eligible rank. This distance is zero for the lowest-ranked current leader and all other current leaders. The *leading distance,* corresponding to performance above that necessary to secure a prize, is defined analogously for outlets already occupying prize-eligible positions.[15] Leading distance is always equal to zero in a tournament group that has only one possible winner, as it was in the business simulation game. As a robustness test, we alternatively measure the distance according to ordinal rank rather than cumulative loan sales. Our models also include several variables related to the timing of the tournament. First, we include month and day-of-month fixed effects to account for possible demand fluctuations over the calendar year and over the course of a month. Second, we include a time trend measured as the number of days remaining in the tournament, as the level of effort may vary as the tournament progresses.

[15] Leading distance was not included as a control in the simulation game regressions as there was only one leader per game.

All of our specifications include outlet-level fixed effects (see estimation details below) to allow for outlet-specific unobservable characteristics (such as innate ability) that could drive the responses to organizational incentives, including personal characteristics of outlet employees. Finally, we control for several available time-varying outlet characteristics such as total employment (*employment*) and total expenditures (*costs*). Table 2 reports summary statistics for all variables as well as pairwise correlations.

---- Insert Table 2 about here ----

Results

The data patterns observed in our sample are consistent with our predictions and with results from the simulation game. Table 3 reports the results of fixed-effects OLS regressions explaining effort allocation. Standard errors are block-bootstrapped on individual branches with 200 repetitions due to data interdependence within blocks (Lahiri, 2003). Column 1 reports the baseline model with controls only. In column 2 we report our main results, testing our prediction. As expected, lost leadership is associated with increased levels of effort. Controlling for current distance from the first prize-eligible position, former leaders' daily tournament performance is, on average, 14 percent higher than that of their comparable competitors who have never led. In column 3 we report results from specifications that include time-separated measures of former leadership. Unlike Strahilevitz and Loewenstein (1998), who could only observe potential decay over a few minutes, we find evidence of attenuation in the effect of lost leadership on effort—the estimated coefficient for recent former leader is 50 percent larger than for old former leader. This result must, however, be treated with caution. Similar to the results from the business simulation game, although the estimated coefficients represent large economic differences, their 95 percent confidence intervals overlap (95% CI for recent former leader [0.004,0.013], 95% CI for old former leader [-0.000,0.012]). Finally, consistent with prior work, we find that effort decreases as contest participants fall farther behind the prize-eligibility threshold.

Finally, we explore the potential substitution effect between the effort allocated toward the tournament goal and alternative goals. As mentioned before, the objective of the tournament was to maximize the total number of primary loans sold, irrespective of the loan value. Thus, bank managers' increased performance in the tournament is likely to result to some degree from the (relative) neglect of the high-effort and lengthy process of negotiating large loans. Indeed, in column 1 of Table 4, we report only a small effect of former leadership on the sales of large primary loans. Although the estimated coefficient is positive (β=0.03, 95% CI: [-0.07,0.13]), the 95 percent confidence interval is wide and includes zero, indicating that there is little evidence of the substitution effect across primary loan size. In contrast, in column 3, we observe a stronger positive effect of former leadership on the output of small primary loans (β=0.21, 95% CI: [0.12,0.31]). This is consistent with our prediction that former leaders will put more effort into selling loans that contribute the most to reaching a prize-eligible position. In addition, we observe a negative effect of former leadership on sales of secondary loans (see column 5 of Table 4). This provides evidence of effort substitution away from loans that contribute to the alternative goal but not to the tournament goal.

In addition, in columns 2, 4, and 6 of Table 4, we separately analyze the effect of recent and old former leadership on effort allocation toward large primary loans, small primary loans, and secondary loans. We find some evidence of a decaying effect of former leadership. The estimated effect of recent former leadership on sales of small primary loans is 20 percent greater than that of old former leadership. In terms of large primary loans, we only find some evidence of increased effort for recent former leaders. Again, however, the statistical precision of our estimates for sales of large primary loans is relatively low and we hence do not discuss the magnitude of these effects. Interestingly, we also document a strong decay in effort substitution away from the task that is unrelated to tournament performance. The negative economic effect of recent former leadership on sales of secondary loans (β=-0.39, s.e.=0.09) is over five times greater than that of old former leadership (β=-0.07, s.e.=0.11). This evidence indicates that both increased overall provision of effort and substitution of effort from alternative tasks are likely to be driving our main results.

----- Insert Tables 3 and 4 about here -----

Alternative explanations and robustness tests

Given that the loss of leadership is not an exogenous event in either of our empirical contexts, one concern is that we identify an effect that is correlated with lost leadership but not caused by it. Within the confines of our data we cannot claim causality. We are therefore careful to discuss and, to the extent possible, rule out a set of alternative explanations for our findings. As an initial matter, we note that any alternative explanation based on differences in ability—for example, that both leadership (and, hence, former leadership) and daily average performance would be correlated with branches' or teams' productivity—can be ruled out, for multiple reasons. First, our specifications in both studies contain outlet-level (team-level) fixed effects, which would absorb any such average differences. Second, the bank assigned branches to tournament groups according to their pre-tournament performance, and branches' performance in the tournament was measured relative to their pre-tournament performance. Finally, as noted above, the bank's handicapping algorithm worked; pre-tournament performance rankings do not predict tournament outcomes (see Online Appendix Table A3). Remaining alternative explanations largely fall into one of four broad categories of mechanisms. We start by presenting these mechanisms before describing a series of robustness tests and additional analyses that reveal that our main results are inconsistent with these alternative explanations (Bettis et al., 2014).

Mean reversion. One possible explanation for our results is that the variable *former leader* is correlated with some (unobservable to us) mechanism that affects provision of effort but is not tournament-induced, thus resulting in spurious correlations. In particular, one might be concerned that sales patterns of individual outlets are subject to mean-reverting tendencies. Indeed, "when performance is measured repeatedly, the disturbance term will differ each time, and the observed performance will regress toward the mean: unusually high values will tend to be followed by lower values, and unusually low values will tend to be followed by higher values" (Greve, 1999, p. 592). Consequently, a tournament contestant who loses leadership due to underperformance in one round is likely to perform better in the following rounds. This could result in an apparent positive effect of lost leadership on performance. We note here that this mechanism does not apply to the business simulation game that we analyze. This is because, in that context, we measure effort with provision of time, rather than realized outcomes. This measure should, in principle, be more immune to a possible mean reversion pattern.

Voluntary self-displacement. In some situations, it may be rational for a leader to decide not to maintain leadership and to "self-displace." Anecdotally, this occurs for example in the video game console business, where a generational leader may have incentives to postpone the launch of a new generation of hardware. Similarly, this is the case in hypercompetitive markets, in which any competitive advantage "decays rapidly due to intense competition, [and] leader firms can sustain superior performance only by concatenating a series of (short-lived) advantages" (Pacheco-de-Almeida, 2010, p. 1502). In these conditions, and when fast-paced innovation is particularly costly, it may be rational for the leader to reduce effort even though this could imply losing leadership (Pacheco-de-Almeida, 2010). In a similar vein, it may be optimal for a leader to wait and observe its competitors' moves in order to update information and potentially better perform in the next rounds.

Temporal differentiation of goals. When winning the tournament is not the only goal, but instead participants face multiple goals, participants might address different goals sequentially, rather than contemporaneously. Consequently, contestants might periodically alternate the level of effort that they exert toward the different goals (Ethiraj and Levinthal, 2009). A leader could therefore purposely decide to allocate more effort toward the alternative goal in some rounds, which in turn could lead to losing the leadership position. In subsequent rounds, this former leader would reallocate the effort back towards winning the tournament, and potentially more effort than equally positioned competitors. This mechanism is distinct from mean reversion (because it is based on deliberate, rather than random, variation in performance), but it would produce similar data patterns.

Performance feedback. This mechanism has traditionally been applied to study the determinants of alternative modes of search and organizational change as consequences of performance against aspirations (e.g., Greve 1998; Sengul and Obloj, 2017). While rarely applied to the empirical context of tournaments, this theory would predict that effort allocation is driven by a general change in the tournament ranking (Greve, 1999), including a change that carries no leadership consequences, rather than actual loss of leadership specifically. In other words, this mechanism would imply that we identify an effect that is not due to lost leadership *per se* but rather to a general tendency of rivals to increase their provision of effort following a drop in performance against aspiration levels.

To ensure that our findings are not driven by any of these four alternative mechanisms, we run a battery of additional empirical tests (see Online Appendix Tables A1 and A2). Table 5 summarizes both the attributes of our data and empirical tests that help alleviate these concerns. We start by presenting one analysis that speaks broadly across alternative explanations before introducing analyses tailored to each mechanism.

----- Insert Table 5 about here -----

We empirically exploit the fact that banking outlets were assigned to different contests with different numbers of winners and, hence, different minimum performance rankings needed to win a prize. We examine a specification that simulates the structure of the four-prize tournament for all other tournament groups. We define the variable *simulated former leader,* which equals one if an outlet would be a former leader under the rules of the four-prize tournament but is not currently an actual former leader. Including this variable alongside the former leader variable permits us to compare the behaviors of similarly placed outlets across tournaments operating under different rules. For example, an outlet losing a Number 3 position in a tournament group with only two prizes would be defined as *simulated former leader* but not as *former leader*. Joint inclusion of these two variables tells us whether the effect of falling out of second place in a three-prize tournament differs from the effect of falling out of second place in a one-prize tournament (as our theory would predict). If the effect we identify arises simply from being "near the top," then the *simulated former leader* variable will also predict behavior when it appears in the regression alongside the *former leader* variable. In contrast, if it is the sharp event of lost leadership that gives rise to the changes in conduct, then only crossing the *actual* prize threshold at any point will have effort consequences.

Consistent with our prediction, in these new regressions, the coefficient on *former leader* continues to be positive (β=0.008, s.e.=0.002). The coefficient on *simulated former leader* is indistinguishable from zero (β=0.001, s.e.=0.003). These results are inconsistent with the predictions of the four alternative mechanisms. Mean reversion would predict a similar pattern of outcomes for simulated and actual former leaders. Similarly, if the patterns that we observe were driven by a voluntary, temporary effort reduction by highly ranked participants (either because of self-displacement or temporal differentiation of goals), the effect of *simulated former leader* should be similar to that of *former leader*.[16] Finally, performance feedback would also predict that a fall from any top-4 position (whether or not it is a prize-eligible one) should affect effort in the subsequent round.

To address the first alternative mechanism (i.e., mean reversion) in more detail, we exploit the fact that we observe all banking outlets before the start of the tournament, by analyzing the two-month period preceding the tournament as if the tournament were taking place then. If effort allocations during the tournament were driven by the mechanical structure of the production process, then former leaders of the fictitious tournament should behave similarly to the former leaders of the actual tournament. In contrast, the absence of such similarities would favor the conclusion that our main results are driven by our theorized behavioral mechanisms. As expected, in our "placebo" test, the *former leader* variable does not predict subsequent levels of effort. The model fit is also substantially diminished, as would be expected under our theory.

[16] As discussed below, the self-displacement and temporal differentiation of goals strategies are available to *all* contestants, not just those of a certain rank and not just formal leaders. At most, one would expect these mechanisms to produce only *marginal* differences in behavior between similarly situated contestants. Yet our test here reveals *qualitative* differences in behavior between outlets with nearly identical peak historical rankings in the same tournament, and with *identical* peak historical rankings in different tournaments, depending on whether they achieved formal leadership.

The second alternative mechanism, self-displacement, critically depends on contestants' ability to observe their rivals' innovations. In Pacheco-de-Almeida (2010), contestants can externalize the cost of innovation by imitating leaders—which under certain circumstances creates a disincentive to retaining leadership. In contrast, in the bank setting, all outlets sell the same products, meaning that if any innovation occurs it is in sales techniques, not product attributes. These techniques are not public knowledge, and outlets have few incentives to disclose them. Moreover, because learning in the bank is *introspective*, branches must fully internalize the costs of innovation. Hence, it is not clear that contestants in the bank tournament have any incentive at all to self-displace.[17] While branches might sacrifice short-run performance in order to discover innovative sales techniques that increase their long-run performance, such a strategy would be available to *all* branches, not just leaders. In that case, a fall in the rankings from *any* level—not just leadership—would be associated with enhanced future performance. Furthermore, to the extent that the incentives to innovate depend on tournament position, we would expect only marginal differences between branches with similar tournament histories. Thus, the self-displacement mechanism is inconsistent with the qualitative differences we find above between former leaders and simulated former leaders.

Nonetheless, to test the self-displacement hypothesis further, we take advantage of the fact that we observe the tournament over a period of two months. If the population of former leaders at any given time is partially composed of those self-displacing, and our results are driven by increased effort among these outlets, then the effect should be weaker as the proportion of self-displacing units decreases. That proportion should decrease over time, because such a strategy

[17] While the mechanism could, in principle, be operative in the management simulation, it is unlikely that teams would have voluntarily relinquished the lead given the contest's compressed time frame. Moreover, to be preferred to our theory, the voluntary self-displacement hypothesis would need to explain the results in both of our empirical settings, which—as we show above and here—it is unable to do.

becomes riskier as the end of the tournament nears; leaders who sacrifice short-run performance for long-run gain may not have enough time to reclaim the leadership position. We therefore perform an analysis using a variable, *late former leader*, that is defined in an analogous way to *former leader* but restricted to those outlets that first lose leadership in the second half of the tournament. Importantly, a *late former leader* is also a *former leader*; in other words, the coefficient *late former leader* captures the difference in the effect of having lost leadership before and after the midpoint of the tournament. If self-displacement drives our main results, we should observe a smaller effect of losing leadership in the second half of the contest, compared with the first half. Inconsistently with this hypothesis, after the midpoint of the tournament, there is no decline in the average effect of having lost leadership (see column 1 of Table A2).

Regarding the third alternative mechanism (i.e., temporal differentiation of goals), we first note that it cannot explain our results from the business simulation, as competing teams of students had only one goal. Similarly, and as with the self-displacement hypothesis, nothing about this mechanism inherently confines it to tournament leaders; hence it is unable to explain the qualitative differences in behavior that we observe between former leaders and other branches in the bank tournament. To further examine whether this mechanism can explain our findings in the bank tournament, we take advantage of the fact that the ongoing per-loan piece-rate bonus was conditioned on passing a performance threshold. In other words, competing outlets had a strong incentive to put enough effort toward this alternative goal until they reached the bonus threshold, but a weaker motivation thereafter, reducing the strength of the piece-rate goal as compared to the tournament goal. We therefore perform an additional analysis using the binary variable *bonus achieved* that is equal to one if the focal outlet has qualified for the piece-rate bonus, and zero otherwise. We observe that the variable *former leader* still explains additional provision of effort, and that there is no difference in its effect before or after reaching the piece-rate bonus threshold (see column 5 in Table A1). This pattern is therefore inconsistent with temporal differentiation of goals as an alternative explanation of our findings.

Finally, to rule out the fourth alternative mechanism (i.e., performance feedback), we perform additional analyses to examine if effort allocation was driven by losing a leadership position or by a general change in ranking. Indeed, to become a former leader, a given outlet had to fall in the ranking. Thus, our results could be driven by a more general effect of dynamic position adjustments rather than the mechanisms we posit. Following standard practice, we perform a spline analysis incorporating separate variables for the increase and decrease, respectively, in the outlet's tournament rank from the period preceding to the focal period. These additional variables add little explanatory power to our models, and the effects of lost leadership remain robust, indicating that we observe a mechanism that is distinct from the one described in prior literature. In other words, we show that the effect of lost leadership is distinct from the effect of dropping in the ranking. We also obtain similar results using variables that measure the change in performance in absolute (i.e., distance), instead of relative (tournament rank) terms (see columns 3 and 4 in Table A1). Importantly, none of the coefficients of gained/lost performance predicts subsequent performance.

DISCUSSION

Many individual- and organization-level competitive contexts can be modeled as finite-time-horizon dynamic tournaments. Indeed, at an individual level, interim performance feedback and relative performance assessment characterizes most sporting events, promotion and hiring decisions, and student evaluations. At an organizational level, such reward structures are also ubiquitous in cases of internal sales or idea sourcing contests, standards wars, and competitive tendering. One important feature of such dynamic tournaments is that, over time, participants rise and fall in the interim rankings. In particular, those at the top often lose this position as rivals overtake them. With losses looming larger than gains, such lost leadership events attract ample attention and are hence likely to trigger behavioral responses.

Yet, despite the importance accorded to lost leadership, we still know relatively little about its consequences for managerial decisions and actions. In this paper, we make a step towards closing this gap, by developing a theory and providing empirical tests of the effect of lost leadership on the provision of effort in dynamic tournaments. Building on the literatures on goals as reference points and goal-setting theory, we predict that former leaders will exert more effort relative to comparable rivals who have not led in the past. We test this prediction, and provide corroborating evidence, using a proprietary dataset from a two-month sales contest among 164 banking outlets and an educational management simulation game. Although we reject several plausible alternative explanations of our findings, our results must be treated with caution, pending research with stronger causal identification of the underlying mechanisms.

Our results contribute to a better understanding of micro-level responses to competitive outcomes, and have important implications for firm governance and competitive interactions. We join a growing stream of literature in strategy focusing on the psychological foundations of decisions in firms and markets (Powell et al. 2011; Roach and Sauerman, 2015). By theorizing and empirically demonstrating a purely behavioral effect of lost leadership, our study responds to a recent call by Connelly and colleagues (2014) for stronger integration of tournament and behavioral theories. Knowing that a management team is likely to exert increased levels of effort and substitute attention away from other tasks in order to reclaim leadership may optimally require governance structures that explicitly account for such cognitive patterns in decision making. Indeed, internal governance mechanisms and design structures can serve the important role of cognitive repairs in firms (Heath, Larrick, and Klayman, 1998), and future research could fruitfully examine if organizations do, in fact, use their design elements strategically to prevent potentially costly behaviors such as those we document.[18]

Our study also highlights an unrecognized tradeoff in the prescriptions of the organizational design and goal setting literature. Sitkin and colleagues (2011) argue that incentives in the form of stretch goals have heterogeneous effects on performance and learning, depending on past performance. In particular, the authors posit that the stronger the past performance, the more beneficial the use of stretch goals becomes. Our results highlight the limitations of stretch goals. If too ambitious, they create a reference point that is never achieved. This can hurt organizational performance in two ways: through a direct, demotivating incentive effect (Casas-Arce and Martínez-Jerez, 2009) and also by preventing the mechanism of crossing the performance threshold that our study depicts. Accordingly, our study also speaks to the literature on the optimal prize structures in tournaments (Boudreau, Lacetera and Lakhani, 2011). If temporarily occupying a prize-eligible position has positive effort consequences, then the optimal number of prizes may be greater than traditional tournament theory indicates.

Our findings also have direct implications for incentive systems that rely on dynamic tournament structures. In an uncertain environment, where frequent changes among leaders occur, such incentive systems could lead to over-escalation and unintended substitution of effort that can eventually be detrimental to firms' performance. In some circumstances however, organizational designers can strategically use the behavioral mechanism we study to their advantage. For instance, a firm that organizes an innovation contest to solve a given problem may provide frequent interim feedback on the ranking of the participants to foster increased effort, which could potentially lead

[18] Heath, Larrick, and Klayman (1998, p. 1) coined the term cognitive repairs to refer to "organizational practices that may effectively repair the cognitive shortcomings of individuals," such as confirmation or small-sample biases.

to the discovery of more innovative solutions. Absent such information, the behavioral effect that we document is unlikely to emerge. Future work could fruitfully study the implications of our findings for the two key dimensions of dynamic tournament design: the number of prizes and the frequency of feedback. Both of these levers are likely to influence the mechanisms we document.

Finally, our study contributes to the growing literature on the use of goals as reference points (Allen et al., 2017; Markle et al., 2018). While this literature has been predominantly concerned with absolute performance levels, we extend it to settings in which outcomes are assessed based on relative performance. Moreover, the characteristics of our empirical settings allow us to study a novel mechanism: the temporary achievement of a goal. While prior studies have found that completing sub-goals increases persistence toward the goal (Gal and McShane, 2012), we suggest that temporarily achieving an objective in a dynamic contest increases subsequent provision of effort toward this goal. Although decreasing with time, the behavioral effects of having temporarily achieved a goal are long-lasting.

We test our predictions in the context of finite-time-horizon dynamic tournaments with well-defined prize structures. An exciting avenue for future research would be to study the effects of lost leadership in other contexts where the tournament structure is less obvious. For example, competition between mutual funds for capital has often been modeled as a dynamic tournament (Kempf and Ruenzi, 2008). A similar theoretical framework has also been applied to competition among financial analysts (Yin and Zhang, 2014). Open-ended competition among firms for market share or inclusion in an index (e.g.: S&P 500 or Fortune's World's Most Admired Companies) may also take the form of dynamic contests. Indeed, anecdotal evidence suggests that in such contexts lost leadership may trigger a similar behavioral response to the one we document.[19]

Generalizability of our results to such contexts, however, awaits future research.

ACKNOWLEDGEMENTS

We thank co-editor Connie Helfat, two anonymous SMJ reviewers, and Diane Burton, Russ Coff, Felipe Csaszar, Stefano DellaVigna, Henrich Greve, William Hesterly, Phanish Puranam and Zur Shapira for their comments on earlier versions of this paper. For feedback, we are also grateful to various seminar and conference participants, especially to the attendees of the 2017 Consortium for Strategy Research and 2017 Strategic Management Society Conference.

REFERENCES

Abdellaoui, M., Gutierrez, C., & Kemel, E. (2018). Temporal discounting of gains and losses of time: An experimental investigation. *Journal of Risk and Uncertainty, 57*(1), 1–28.

Abeler, J., Falk, A., Goette, L., & Huffman, D. (2011). Reference Points and Effort Provision. *The American Economic Review, 101*(2), 470–492.

Agence France-Presse. (2012). Oracle aims to dethrone IBM in business hardware. Retrieved from http://news.abs-cbn.com/business/05/31/12/oracle-aims-dethrone-ibm-business-hardware

Allen, E.J., Dechow, P.M., Pope, D.G., & Wu, G. (2017). Reference-dependent preferences: Evidence from marathon runners. *Management Science, 63*(6), 1657–1672.

Aoyagi, M. (2010). Information feedback in a dynamic tournament. *Games and Economic Behavior, 70*(2), 242–260.

Arkes, H.R., Hirshleifer, D., Jiang, D., & Lim, S. (2008). Reference point adaptation: Tests in the domain of security trading. *Organizational Behavior and Human Decision Processes, 105*(1), 67–81.

Arkes, H.R., Hirshleifer, D., Jiang, D., & Lim, S.S. (2010). A cross-cultural study of reference point adaptation: Evidence from China, Korea, and the US. *Organizational Behavior and Human Decision Processes, 112*(2), 99–111.

Barberis NC. (2013). Thirty years of prospect theory in economics: A review and assessment. *Journal of Economic Perspectives, 27*, 173–196.

Bell, R., Koren, Y., & Volinsky, C. (2007). Chasing $1,000,000: How we won the Netflix progress prize. *ASA Statistical and Computing Graphics Newsletter*, *18*(2), 4–12.

Bernasconi, M., Corazzini, L., & Seri, R. (2014). Reference dependent preferences, hedonic adaptation and tax evasion: Does the tax burden matter? *Journal of Economic Psychology, 40*, 103–118.

Bettis, R., Gambardella, A., Helfat, C., & Mitchell, W. (2014). Quantitative empirical analysis in strategic management. *Strategic Management Journal, 35*(7), 949–953.

Bettman, J. R., Johnson, E. J., & Payne, J. W. (1990). A componential analysis of cognitive effort in choice. *Organizational Behavior and Human Decision Processes*, *45*(1), 111–139.

[19] For example, EDS's CEO Mike Smith once vowed to "fight to the bitter end" to regain the company's leadership position in IT services, which it had lost to IBM (ComputerWorld, 2004). Similarly, after Oracle lost its leading position in databases and middleware to IBM, Oracle's CEO Larry Ellison used a public speech to describe his plans for reclaiming the top spot (Agence France-Presse, 2012) and General Motors made an "aggressive $12 billion investment plan to regain position as market leader" (Forbes, 2014) after having been overtaken in 2014 by Volkswagen in China.

Bothner, M. S., Kang, J. H., & Stuart, T. E. (2007). Competitive crowding and risk taking in a tournament: Evidence from NASCAR racing. *Administrative Science Quarterly*, *52*(2), 208–247.

Boudreau, K. J., Lacetera, N., & Lakhani, K. R. (2011). Incentives and problem uncertainty in innovation contests: An empirical analysis. *Management Science*, *57*, 843–863.

Boyle, E., & Shapira, Z. (2012). The liability of leading: Battling aspiration and survival goals in the Jeopardy! Tournament of Champions. *Organization Science, 23*(4), 1100–1113.

Cabral, L. (2017). We're Number 1: Price Wars for Market Share Leadership. *Management Science, 64*(5), 2013–2030.

Cameron, A.C., & Miller, D.L. (2015). A practitioner's guide to cluster-robust inference. *Journal of Human Resources, 50*(2), 317–372.

Casas-Arce, P., & F. Martínez-Jerez. (2009). Relative performance compensation, contests, and dynamic incentives. *Management Science, 55*(8), 1306–1320.

Chen, H., Ham, S. H., & Lim, N. (2011). Designing multiperson tournaments with asymmetric contestants: An experimental study. *Management Science, 57*(5), 864–883.

Clark, D. J., & Riis, C. (1998). Competition over more than one prize. *The American Economic Review*, *88*(1), 276–289.

ComputerWorld. (2004). EDS fights for top ranking. Retrieved from https://www.computerworld.com.au/article/6940/eds_fights_top_ranking/

Connelly, B. L., Tihanyi, L., Crook, T. R., & Gangloff, K. A. (2014). Tournament Theory: Thirty Years of Contests and Competitions. *Journal of Management, 40*(1), 16–47.

Denrell, J., & March, J.G. (2001). Adaptation as information restriction: The hot stove effect. *Organization Science, 12*(5), 523–538.

Ederer, F. (2010). Feedback and motivation in dynamic tournaments. *Journal of Economics & Management Strategy, 19*(3), 733–769.

Eil, D., & Rao, J. M. (2011). The good news-bad news effect: asymmetric processing of objective information about yourself. *American Economic Journal: Microeconomics, 3*(2),114–138.

Elfenbein, D. W., Knott, A. M., & Croson, R. (2017). Equity stakes and exit: An experimental approach to decomposing exit delay. *Strategic Management Journal*, *38*(2), 278–299.

Esteves-Sorenson, C. (2018). Gift exchange in the workplace: Addressing the conflicting evidence with a careful test. *Management Science*, *64*(9), 4365–4388.

Ethiraj, S. K., & Levinthal, D. (2009). Hoping for A to Z while rewarding only A: Complex organizations and multiple goals. *Organization Science, 20*(1), 4–21.

Ferrier, W.J., Smith, K.G., & Grimm, C.M. (1999). The role of competitive action in market share erosion and industry dethronement: A study of industry leaders and challengers. *Academy of Management Journal, 42*(4), 372–388.

Forbes, 2014. General Motors' Aggressive $12 Billion Investment Plan To Regain Position As Market Leader In China, 18 June, Retrieved from https://www.forbes.com/sites/greatspeculations/2014/06/18/general-motors-aggressive-12-billion-investment-plan-to-regain-position-as-market-leader-in-china

Fu, Q., & Wu, Z. (2019). Contests: Theory and topics. In *Oxford Research Encyclopedia of Economics and Finance*. Oxford University Press.

Gal, D., & McShane, B.B. (2012). Can small victories help win the war? Evidence from consumer debt management. *Journal of Marketing Research, 49*(4), 487–501.

Garbarino, E. C., & Edell, J. A. (1997). Cognitive effort, affect, and choice. *Journal of Consumer Research, 24*(2), 147–158.

Gershkov, A., & Perry, M. (2009). Tournaments with midterm reviews. *Games and Economic Behavior*, *66*(1), 162–190.

Gill, D., Kissová, Z., Lee, J., & Prowse, V. (2019). First-place loving and last-place loathing: How rank in the distribution of performance affects effort provision. *Management Science*, *65*(2), 494–507.

Gneezy, U., & List, J.A. (2006). Putting behavioral economics to work: Testing for gift exchange in labor markets using field experiments. *Econometrica, 74*(5), 1365–1384.

Goltsman, M., & Mukherjee, A. (2011). Interim performance feedback in multistage tournaments: The optimality of partial disclosure. *Journal of Labor Economics*, *29*(2), 229–265.

Greve, H. R. (1998). Performance, Aspirations, and Risky Organizational Change. *Administrative Science Quarterly, 43*(1), 58–86.

Greve, H. R. (1999). The Effect of Core Change on Performance: Inertia and Regression toward the Mean. *Administrative Science Quarterly*, *44*(3), 590–614.

Heyman, J., & Ariely, D. (2004). Effort for payment: A tale of two markets. *Psychological Science*, *15*(11), 787–793.

Heath, C., Larrick, R.P., & Klayman, J. (1998). Cognitive repairs: How organizational practices can compensate for individual shortcomings. *Review of Organizational Behavior*, *20*, 1–37.

Heath, C., Larrick, R.P., & Wu, G. (1999). Goals as reference points. *Cognitive Psychology, 38*(1), 79–109.

Holmes Jr, R.M., Bromiley, P., Devers, C.E., Holcomb, T.R., & McGuire, J.B. (2011). Management theory applications of prospect theory: Accomplishments, challenges, and opportunities. *Journal of Management, 37*(4), 1069–1107.

Hossain, T., & List, J.A. (2012). The behavioralist visits the factory: Increasing productivity using simple framing manipulations. *Management Science*, *58*(12), 2151–2167.

Joseph, J., & Gaba, V. (2015). The fog of feedback: Ambiguity and firm responses to multiple aspiration levels. *Strategic Management Journal, 36*(13), 1960–1978.

Kahneman, D., Knetsch, J. L., & Thaler, R. H. (1991). Anomalies: The Endowment Effect, Loss Aversion, and Status Quo Bias. *The Journal of Economic Perspectives, 5*(1), 193–206.

Kahneman, D., & Tversky, A. (1979). Prospect Theory: an Analysis of Decision Under Risk. *Econometrica, 47*(2), 263–291.

Kahneman, D. (2003). Maps of bounded rationality: Psychology for behavioral economics. *American economic review*, *93*(5), 1449–1475.

Kempf, A., & Ruenzi, S. (2008). Tournaments in mutual-fund families. *The Review of Financial Studies*, *21*(2), 1013–1036.

Koop, G. J., & Johnson, J. G. (2012). The use of multiple reference points in risky decision making. *Journal of Behavioral Decision Making, 25*(1), 49–62.

Kräkel, M. (2008). Emotions in tournaments. *Journal of Economic Behavior & Organization*, *67*(1), 204–214.

Kumar, M. S., Dixit, J., & Francis, B. (2015). The impact of prior stock market reactions on risk taking in acquisitions. *Strategic Management Journal*, *36*(13), 2111–2121.

Lahiri, P. (2003). On the impact of bootstrap in survey sampling and small-area estimation. *Statistical Science, 18*(2), 199–210.

Latham, G. P., & Locke, E. A. (1991). Self-regulation through goal setting. *Organizational Behavior and Human Decision Processes, 50*(2), 212–247.

Locke, E. A., & Latham, G. P. (2002). Building a practically useful theory of goal setting and task motivation: A 35-year odyssey. *American psychologist*, *57*(9), 705-717.

Lazear, E. P., & Rosen, S. (1981). Rank-order tournaments as optimum labor contracts. *Journal of Political Economy, 89*(5), 841–864.

Lazear, E. P. (2000). Performance pay and productivity. *American Economic Review*, *90*(5), 1346–1361.

Leitterstorf, M. P., & Rau, S. B. (2014). Socioemotional wealth and IPO underpricing of family firms. *Strategic Management Journal, 35*(5), 751–760.

Levinthal, D.A. (2011). A behavioral approach to strategy—what's the alternative?. *Strategic Management Journal, 32*(13), 1517–1523.

Lim, E. N. (2015). The role of reference point in CEO restricted stock and its impact on R&D intensity in high-technology firms. *Strategic Management Journal, 36*(6), 872–889.

Locke, E.A., & Latham, G.P. (2002). Building a practically useful theory of goal setting and task motivation: A 35-year odyssey. *American Psychologist, 57*(9), 705–717.

Markle, A., Wu, G., White, R., & Sackett, A. (2018). Goals as reference points in marathon running: A novel test of reference dependence. *Journal of Risk and Uncertainty, 56*(1), 19-50.

Marinovic, I. (2015). The credibility of performance feedback in tournaments. *Journal of Economics & Management Strategy*, *24*(1), 165–188.

Meyer, M. W., & Gupta, V. (1994). The performance paradox. *Research in Organizational Behavior, 16*, 309-369.

Mihm, J., & Schlapp, J. (2019). Sourcing innovation: On feedback in contests. *Management Science*, *65*(2), 559–576.

Moldovanu, B., & Sela, A. (2001). The optimal allocation of prizes in contests. *American Economic Review*, *91*(3), 542–558.

Nunes, J.C., & Dreze, X. (2006). The endowed progress effect: How artificial advancement increases effort. *Journal of Consumer Research*, *32*(4), 504–512.

Obloj, T., & Zenger, T. (2017). Organization design, proximity, and productivity responses to upward social comparison. *Organization Science, 28*(1), 1–18.

Obloj, T., & Sengul, M. (2020). What do multiple objectives really mean for performance?
Empirical evidence from the French manufacturing sector. Forthcoming: *Strategic Management Journal.*

Pacheco-de-Almeida, G. (2010). Erosion, time compression, and self-displacement of leaders in hypercompetitive environments. *Strategic Management Journal*, *31*(13), 1498–1526.

Pope, D.G., & Schweitzer, M.E. (2011). Is Tiger Woods loss averse? Persistent bias in the face of experience, competition, and high stakes. *The American Economic Review, 101*(1), 129–157.

Powell, T.C., Lovallo, D., & Fox, C.R. 2011. Behavioral strategy. *Strategic Management Journal, 32*(13), 1369–1386.

Reitzig, M., & Maciejovsky, B. (2015). Corporate hierarchy and vertical information flow inside the firm—a behavioral view. *Strategic Management Journal, 36*(13), 1979–1999.

Roach, M., & Sauermann, H. (2015). Founder or joiner? The role of preferences and context in shaping different entrepreneurial interests. *Management Science, 61*(9), 2160–2184.

Ross, J.M., & Sharapov, D. (2015). When the leader follows: Avoiding dethronement through imitation. *Academy of Management Journal,* 58(3), 658–679.

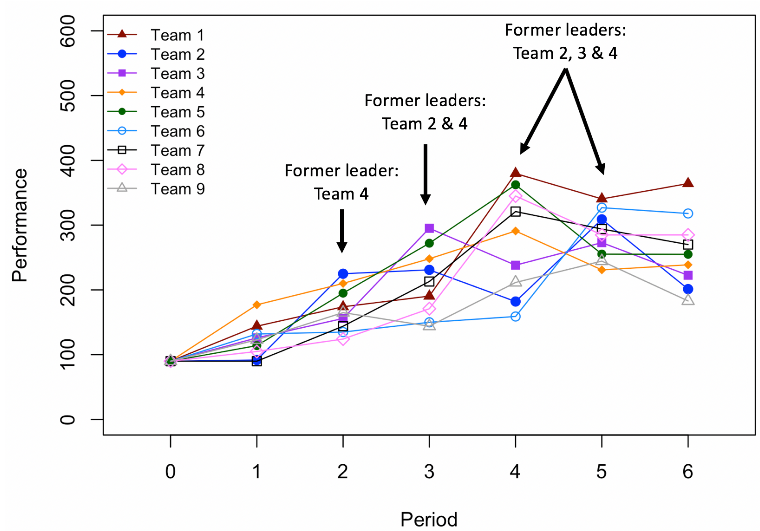
Sengul, M., & Obloj, T. (2017). Better safe than sorry: Subsidiary performance feedback and internal governance in multiunit firms. *Journal of Management, 43*(8), 2526–2554.

Sisak, D. (2009). Multiple-Prize Contests the Optimal Allocation of Prizes. *Journal of Economic Surveys*, *23*(1), 82–114.

Sitkin, S.B., See, K.E., Miller, C.C., Lawless, M.W., & Carton, A.M. (2011). The paradox of stretch goals: Organizations in pursuit of the seemingly impossible. *Academy of Management Review, 36*(3), 544–566.
Sharot, T., & Garrett, N. (2016). Forming beliefs: Why valence matters. *Trends in Cognitive Sciences, 20*(1), 25–33.
Strahilevitz, M.A., & Loewenstein, G. (1998). The effect of ownership history on the valuation of objects. *Journal of Consumer Research, 25*(3), 276–289.
Tversky, A., & Kahneman, D. (1991). Loss aversion in riskless choice: A reference-dependent model. *The Quarterly Journal of Economics, 106*(4), 1039–1061.
Wang, R.D., & Shaver, J.M. (2014). Competition-driven repositioning. *Strategic Management Journal, 35*(11), 1585–1604.
Wiseman, R.M., & Gomez-Mejia, L.R. (1998). A behavioral agency model of managerial risk taking. *Academy of Management Review, 23*(1), 133–153.
Yildirim, H. (2005). Contests with multiple rounds. *Games and Economic Behavior*, *51*(1), 213–227.
Yin, H., & Zhang, H. (2014). Tournaments of financial analysts. *Review of Accounting Studies*, *19*(2), 573–605.

FIGURES AND TABLES

Figure 1. Illustration of competition dynamics from a simulation game



Notes: Performance index on the vertical axis, decision periods on the horizontal axis. To improve the readability of the figure, we slightly modified some data points to avoid overlap.

Table 1. Effect of lost leadership on effort allocation —Simulation game data

	(1)	(2)	(3)	(4)
Dependent variable	Time Spent		Probability of substituting leisure for work	
Former leader	3.40		0.655	
	(1.54)		(0.225)	
Recent Former leader		5.73		0.669
		(2.50)		(0.235)
Old Former leader		2.84		0.581
		(1.61)		(0.360)
Trailing distance	0.004	0.004	0.001	0.001
	(0.006)	(0.006)	(0.001)	(0.001)
Firm f.e.	yes	yes	yes	yes
Period f.e.	yes	yes	yes	yes
Observations	1717	1717	1632	1632
Number of firms	295	295	270	270

OLS estimates for 1-2, Logit for 3-4. Robust standard errors clustered by simulation game in parentheses. Constant included, not reported.

Table 2
Descriptive statistics and pair-wise correlations — Retail bank data

		Mean	S.D.	1	2	3	4	5	6
1	Trailing distance	0.57	0.54	1.00					
2	Leading distance	0.005	0.03	-0.16	1.00				
3	Former leader	0.22	0.39	-0.04	-0.10	1.00			
4	Tournament Performance	0.05	0.04	-0.06	0.01	0.04	1.00		
5	Costs	0.327	0.109	-0.15	0.17	0.09	0.00	1.00	
6	Employment	4.50	1.53	0.08	0.03	0.05	-0.03	0.49	1.00
7	Piece rate	0.44	0.31	-0.11	0.13	0.12	0.11	0.12	-0.01

Table 3
Effect of lost leadership on effort allocation — Retail bank data

	(1)	(2)	(3)
Dependent variable	Tournament Performance		
Former leader		0.007	
		(0.002)	
Recent former leader			0.009
			(0.002)
Old former leader			0.006
			(0.003)
Leading distance	-0.019	-0.031	-0.032
	(0.022)	(0.025)	(0.024)
Trailing distance	-0.005	-0.004	-0.004
	(0.001)	(0.001)	(0.001)
Piece rate	0.016	0.017	0.017
	(0.002)	(0.002)	(0.002)
Costs	0.020	0.022	0.022
	(0.016)	(0.017)	(0.017)
Employment	0.001	0.001	0.000
	(0.001)	(0.001)	(0.001)
Outlet f.e.	yes	yes	yes
Time controls	yes	yes	yes
Observations	7,959	7,959	7,959
Number of units	164	164	164

OLS estimates. Errors block-bootstrapped by unit in parentheses. 200 repetitions. Constant included, not reported. All right-hand-side side variables that are available on outlet-day level are lagged one period.

Table 4
Substitution of effort across tasks — Retail bank data

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	Large primary loans		Small primary loans		Secondary loans	
Former leader	0.03		0.21		-0.33	
	(0.05)		(0.05)		(0.09)	
Recent former leader		0.05		0.23		-0.39
		(0.06)		(0.05)		(0.09)
Old former leader		-0.03		0.19		-0.07
		(0.07)		(0.07)		(0.11)
Leading distance	-0.34	-0.36	-1.03	-1.04	-1.52	-1.45
	(0.57)	(0.58)	(0.44)	(0.49)	(0.86)	(0.86)
Trailing distance	-0.07	-0.09	-0.06	-0.11	0.03	0.08
	(0.04)	(0.05)	(0.04)	(0.04)	(0.06)	(0.07)
Piece rate	0.46	0.46	0.32	0.30	-0.02	-0.02
	(0.05)	(0.05)	(0.044)	(0.045)	(0.08)	(0.08)
Costs	0.01	-0.02	1.11	1.11	-2.34	-2.27
	(0.53)	(0.53)	(0.45)	(0.45)	(0.80)	(0.79)

Employment	-0.03 (0.03)	-0.03 (0.03)	0.03 (0.03)	0.03 (0.03)	0.01 (0.05)	-0.00 (0.05)
Outlet f.e.	yes	yes	yes	yes	yes	yes
Time controls	yes	yes	yes	yes	yes	yes
Observations	7,959	7,959	7,959	7,959	7,959	7,959
Number of units	164	164	164	164	164	164

Notes: OLS estimates. Errors block-bootstrapped by unit in parentheses. 200 repetitions. Constant included, not reported. All right-hand-side side variables that are available on outlet-day level are lagged one period.

Table 5

Summary of the empirical tests and data properties that help addressing alternative explanations

		Alternative Mechanisms			
Data structure/ Empirical tests	Detail and Implementation	Mean reversion	Self-displacement	Temporal differentiation of goals	Perf. feedback
Context: bank	Competitors' innovations are unobservable.		X		
Context: simulation game	Input-based measure of effort.	X			
	Single-task environment (i.e., no switching between two goals).			X	
Simulated tournament structure	Using the fact that outlets were assigned to different contests with different numbers of winners, we test whether having been "near the top", but not a former leader, has productivity consequences.	X	X	X	X
Placebo test	Placebo test using the two-month period preceding the tournament as if the tournament were actually taking place.	X			
Late former leader test	Analyses focusing on *late former leaders*, defined as outlets who first lose leadership in the second half of the tournament.		X		
Bonus achieved test	Analyses using *bonus achieved* variable, which takes value 1 if the focal outlet has reached the volume of primary loans making it eligible for a piece-rate bonus.			X	
Spline analyses on distance/rank lost	Spline analysis incorporating separate variables for the increase and decrease in the absolute/relative performance levels.				X

Online Appendix

Better to have led and lost than never to have led at all? Lost leadership and effort provision in dynamic tournaments

Table A1
Robustness tests 1

	(1)	(2)	(3)	(4)	(5)
Dependent variable	Tournament Performance				
Former leader	0.007	0.008	0.007	0.006	0.008
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Former leader × Bonus achieved					0.001
					(0.003)
Simulated former leader		0.001			
		(0.003)			
Leading distance	-0.031	-0.032	-0.029	-0.035	-0.032
	(0.025)	(0.022)	(0.023)	(0.025)	(0.019)
Trailing distance	-0.004	-0.004	-0.003	-0.003	-0.005
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Distance lost			-0.030		
			(0.021)		
Distance gained			-0.007		
			(0.005)		
Rank lost				-0.001	
				(0.001)	
Rank gained				0.001	
				(0.002)	
Piece rate	0.017	0.017	0.018	0.017	0.026
	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
Bonus achieved					-0.010
					(0.002)
Costs	0.022	0.022	0.024	0.019	0.029
	(0.017)	(0.018)	(0.018)	(0.014)	(0.017)
Employment	0.001	0.001	0.001	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Outlet f.e.	yes	yes	yes	yes	yes
Time controls	yes	yes	yes	yes	yes
Observations	7,959	7,959	7,959	7,959	7,959
Number of units	164	164	164	164	164

Notes: OLS estimates. Errors block-bootstrapped by unit in parentheses. 200 repetitions. Constant included, not reported. All right-hand-side side variables that are available on outlet-day level are lagged one period. *Simulated former leader* is a binary variable equal to one if a focal outlet is not currently a former leader but would have been one if all tournaments had four prizes. *Distance lost* is equal to the absolute value of the daily change in performance distance from the nearest prize-eligible competitor if this distance has: i) increased for outlets trailing behind the first prize-eligible position and ii) decreased for outlets occupying one of the prize-eligible positions. It is equal 0 otherwise. *Distance gained* is equal to the absolute value of the daily change in performance distance from the nearest prize-eligible competitor if this distance has: i) decreased for outlets trailing behind the first prize-eligible position and ii) increased for outlets occupying one of the prize-eligible positions. It is equal 0 otherwise. *Rank lost is* equal to the number of positions lost compared to the previous day. It is equal 0 if the focal outlet has gained ranks. *Rank gained* is equal to the number of positions gained compared to the previous day. It is equal 0 if the focal outlet has lost ranks. *Bonus achieved* is a binary variable taking value of 1 if the focal outlet has reached the volume of primary loans making it eligible for a piece rate bonus.

Table A2
Robustness tests 2

	(1)	(2)	(3)
Dependent variable	Tournament Performance		
Former leader	0.007	0.007	0.007
	(0.002)	(0.002)	(0.002)
Late former leader	0.002	0.002	0.001
	(0.002)	(0.003)	(0.002)
Leading distance	-0.032	-0.028	-0.027
	(0.022)	(0.021)	(0.021)
Trailing distance	-0.004	-0.005	-0.005
	(0.001)	(0.002)	(0.002)
Piece rate	0.018	0.017	0.017
	(0.002)	(0.002)	(0.002)
Costs	0.019	0.020	0.019
	(0.017)	(0.018)	(0.017)
Employment	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)
Outlet f.e.	yes	yes	yes
Time controls	yes	yes	yes
Time restriction	No	7 days	14 days
Observations	7,959	7,959	7,959
Number of units	164	164	164

Notes: OLS estimates. Errors block-bootstrapped by unit in parentheses. 200 repetitions. Constant included, not reported. All right-hand-side side variables that are available on outlet-day level are lagged one period. *Late former leader* is defined in an analogous way to *Former leader* but restricted to those outlets that have first lost leadership after the end of the first month of the tournament. Note that a *Late former leader* is also a *Former leader*; in other words, the coefficient on *Late former leader* captures the differential effect of having lost leadership before and after the midpoint of the tournament. To account for the possibly confounding attenuation effect we report an unrestricted specification (Model 1) as well as specifications limiting the time during which an outlet is coded as *Former leader* and *Late former leader* to one (Model 2) and two (Model 3) weeks following the event of lost leadership.

Table A3
Pre-contest rank effects on final contest rank

	DV: Final Contest Rank
Pre-contest rank	0.08
	(0.07)
Constant	20.08
	(2.64)
Contest group dummies	Included
F-statistic	0.40

Notes: N=164. OLS regression results, robust standard errors in parenthesis.