The Accounting Review
Vol. 91, No. 3
May 2016
pp. 793-810

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Vol. 91, No. 3
May 2016
pp. 793–810

American Accounting Association
DOI: 10.2308/accr-51202

Cost Estimates, Cost Overruns, and Project Continuation Decisions

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ABSTRACT: Cost overruns on multi-period projects are large and frequent in natural environments. Reasons for these overruns include cost understatements in initial project proposals and escalation of commitment to projects when initial actual costs turn out to be higher than expected. Prior literature has suggested, and some firms have implemented, a device that limits escalation and thus potentially reduces cost overruns: changing decision makers (superiors) so that the manager who approves continuation of a project is not the same individual who approved the project initially. We provide theoretical explanations and experimental evidence about how changing versus continuing superiors affect the underestimates of cost in initial project proposals. We find that although changing superiors, as expected, are more likely than are continuing superiors to react skeptically to continuation proposals when first-period cost overruns have occurred, this does not reduce initial cost understatements and overruns. On the contrary, in our setting it leads to greater initial understatements and overruns. Subordinates anticipate that new superiors will be more critical of their projects; hence, they discount later-period payoffs and focus on gaining initial funding by providing understated cost estimates.

Keywords: capital budgeting; decision-making; escalation; cost overruns.

I. INTRODUCTION

Budgets for multi-period capital projects commonly understate costs. A study of construction projects in 20 countries found that nine out of ten project budgets initially underestimated costs (Flyvbjerg, Holm, and Buhl 2002), and a study of information technology projects by an industry group found that 71 percent of projects exceeded their initially budgeted costs, with an average cost overrun greater than 50 percent (Hartmann 2004). Although the prevalence of cost underestimates in capital projects is such that organizations are likely to make allowances for them in planning, they still can pose a problem because the amount of the initial understatement is uncertain,[1] and this uncertainty makes it difficult to plan financing and resource allocation efficiently.

Two reasons have frequently been identified for cost overruns in capital projects. First, managers with private information about costs can often increase their chances of receiving funding by strategic understatement of these costs in project proposals; and because of the high levels of uncertainty and longtime horizons involved, control techniques like post-audits with penalties for overruns are difficult to implement and are not especially widespread (Haka 2007). Second, once a project has been funded, managers can be reluctant to "pull the plug" on the project even after evidence indicates that it will be excessively costly (or otherwise unprofitable) and that the organization would be better off terminating it and investing money elsewhere (escalation

We gratefully acknowledge helpful and constructive comments from Christoph Feichter, Lynn Hannan, Kathryn Kadous (editor), Frank Moers, Hun-Tong Tan, two anonymous reviewers, participants at the 2014 AAA Management Accounting Research Conference, and workshop participants at Nanyang Technological University, Tilburg University, and Maastricht University.

Editor's note: Accepted by Kathryn Kadous.

Submitted: June 2014
Accepted: June 2015
Published Online: July 2015

[1] In Flyvbjerg et al.'s (2002) construction project data, the standard deviation of cost overruns is 38.7 percent of budget. Even within construction projects of the same type and the same region, standard deviations remain high (most within a 25–65 percent range), indicating the difficulty of predicting the overrun amount on one project based on experience with similar projects.

of commitment; see Sleesman, Conlon, McNamara, and Miles [2012] for a review). Failure to terminate such projects can mean continuing cost overruns in future periods.

The literatures on these two sources of cost overruns have developed largely in isolation from each other. Studies of misrepresentation in budgeting typically do not address the escalation issue (see the review by Brown, Evans, and Moser [2009]), and studies of escalation typically do not address the problem of investment decision makers' reliance on possibly misrepresented information from others (Sleesman et al. 2012). We argue that the escalation and misrepresentation issues are related, and that *how* they are related is important to the effectiveness of control measures intended to reduce cost overruns.

Linked problems of escalation and misrepresentation are particularly important in settings where projects have distinct stages, with funding continuation decisions at each stage. When subordinates can benefit from receiving funding at earlier stages, even if the project is not continued, they are motivated to overstate the initial attractiveness of their projects (e.g., by understating expected costs). By the time later-stage decisions are made, however, the actual progress of the projects is likely to provide superiors with information about the reliability of subordinates' initial proposals. At least in the absence of escalation bias, superiors may refuse to continue projects with early-stage outcomes that are much less favorable than the outcomes predicted in subordinates' proposals, and this potential for reputation costs might influence subordinates' initial proposals.

In this setting, we investigate the connection between subordinates' cost estimates and a mechanism for reducing inappropriate escalation that has often been proposed in the literature and sometimes employed in practice:

> some practical advice scholars have offered to managers to combat escalation bias has been to shift escalation decisions to someone other than the initial decision maker so that the new individual is not so "attached" to the previously made decision. (Sleesman et al. 2012, 546)

Archival studies have provided evidence that new decision makers (hereafter, "changing superiors") are less likely to continue expenditure on investments with questionable performance records than initial decision makers are (McCarthy, Schoorman, and Cooper 1993; Staw, Barsade, and Koput 1997; McNamara, Moon, and Bromiley 2002).[2]

We argue that the value of changing superiors depends not only on the direct effect of such change on the quality of the continuation decisions made by superiors, but also on the indirect effect that changing superiors have on the quality of initial project proposals through subordinates' anticipation of how continuing funding decisions will be made. This indirect effect could, in principle, either reduce or exacerbate the problem of understated costs in initial proposals. On the one hand, the prospect of facing a new superior at the continuation stage could make subordinates more cautious about understating costs in their initial budget proposals. They might anticipate that superiors who originally funded the projects would be sufficiently committed to them to overlook moderate cost overruns when deciding about continuing funding, but new superiors would be more critical and more likely to cut off funding if the projects' performance diverges too much from the subordinates' predictions. Hence, when continuing funding is valuable, it could be worthwhile for subordinates with changing superiors to limit their understatements. The increased likelihood of continuing funding when overruns are low could compensate (in expected-value terms) for a modest reduction in the probability of initial funding. In this case, changing superiors would benefit the organization not only through better continuation funding decisions, but also through improvements in initial proposals (less understatement of costs) and, thus, reduced first-period cost overruns.

On the other hand, however, as we explain in more detail in the next section, the prospect of a new, non-committed, critical superior making the continuation decision could decrease subordinates' expectations of continuation funding and/or increase their uncertainty about this funding. As a result, subordinates under changing superiors could discount possible later-period reputation payoffs from accurate initial forecasting and focus primarily on gaining initial funding by understating costs. In this case, changing superiors would result in *greater* initial understatement of costs, which could offset the benefits of improved continuation decisions.

In our experimental setting, we find that understatement of costs by subordinates in their initial project proposals is significantly higher with changing than continuing superiors, even though cost overruns make it more difficult for them to obtain later-period funding by providing attractive later-period proposals. Initial cost understatement is associated with subordinates' stated concerns that new superiors will be more critical than continuing superiors would be. Thus, in our setting, although large initial cost overruns are more likely to harm subordinates' reputations for reliable budget forecasting and to have negative consequences for future funding, these overruns are also more likely to occur in the first place with changing superiors.

[2] Some escalation decisions appear to be driven by psychological commitment to projects, while others are driven by rational maximization of utility for wealth; for example, through fears that the project manager's reputation will suffer if the project is abruptly canceled, making the failure of the project visible and reducing the manager's future earnings (Kanodia, Bushman, and Dickhaut 1989; Camerer and Weber 1999). Inappropriate escalation due to either of these causes seems likely to be reduced by assigning continuation decisions to managers other than those who funded the project initially, because new decision makers do not incur the same psychological and/or reputation costs from canceling projects that initial sponsors of the projects do.

Subordinates' uncertainties and concerns about continuation funding lead them to discount the value of continuation and to focus on gaining initial funding by understating costs more aggressively in their initial proposals.

Our study contributes to the accounting literature by identifying connections between the problems of budget misrepresentation and investment escalation, which have been separated in the prior literature (cf. Brown et al. 2009; Sleesman et al. 2012). In practice, these problems are often not separated: the individuals who make continuing funding decisions are often dependent on others to gather information about projects and prepare proposals. Hence, well-designed control systems will take into account possible connections between the budget forecasting and escalation problems. By examining these connections, we identify a potential limitation on the benefits of the often-recommended practice of assigning responsibility to different superiors for initial funding and continued funding decisions. To the extent that this change in superiors increases the uncertainty and/or decreases the likelihood of future-period funding, it can reduce subordinates' incentives to secure this future funding by building a reputation for reliable cost forecasting in their initial proposals.

We also contribute to a growing literature in accounting on the fragility of reputation-building as a solution to control problems (DeJong, Forsythe, and Lundholm 1985; Mayhew 2001; Mayhew, Schatzberg, and Sevcik 2001; Hales and Williamson 2010). Although profitable reputations for reliable forecasting and productive action do sometimes form, reputation formation appears vulnerable to myopia and uncertainty, sometimes to a greater extent than can be accounted for by plausible levels of time discounting (Mayhew 2001) or risk aversion (Hales and Williamson 2010). Prior literature has investigated reputation-building problems in auditing (Mayhew 2001; Mayhew et al. 2001) and effort provision and compensation in agency relations (DeJong et al. 1985; Hales and Williamson 2010). We extend the investigation of these problems to the multi-period budgeting context.

The remainder of the paper proceeds as follows. Section II provides a literature review and hypothesis development, Section III describes the experimental method, and Section IV presents results. Section V concludes.

II. LITERATURE REVIEW AND HYPOTHESIS DEVELOPMENT

Initial understatement of the costs of capital projects is a non-trivial phenomenon. Accenture estimated in 2012 that cost overruns and delays in capital projects in the oil and gas and utility industries would result in a $5 trillion "overspend" on these projects compared to budget (Accenture 2012). A study of 258 infrastructure projects finds large initial cost underestimates and concludes, "Underestimation cannot be explained by error and is best explained by strategic misrepresentation, that is, lying" (Flyvbjerg et al. 2002).

Concerns about cost understatement in capital budgeting contrast with the typical operating budget concern about overestimation of costs, or slack creation, especially when slack increases subordinates' expected payoffs via easier performance targets (e.g., Dunk and Nouri 1998; Fisher, Maines, Peffer, and Sprinkle 2002; Indjejikian and Matějka 2006). Capital and operating budgets tend to create different incentives for misrepresentation, thus resulting in different biases in cost prediction.[3]

Managers are often rewarded for achieving actual operating costs below budget and, thus, they have an incentive to overestimate budgeted operating costs. In contrast, rewards based on achieving actual costs less than budget for capital projects appear to be rarer. Because of the higher level of uncertainty inherent in long-term (sometimes unique) projects, rewarding risk-averse managers based on differences between actual and budgeted performance would require high—perhaps prohibitively high—risk premiums. Moreover, with multi-year capital projects, there can be a considerable delay before initially predicted costs can be compared to actual costs, especially for later stages of a project. Delayed compensation can be unattractive to subordinates and/or difficult to administer (for example, if the subordinate leaves the organization before final realized costs are known). Hence, formal controls based on accuracy in capital budget forecasting (e.g., rewards based on post-audits) are likely to be more costly than comparable operating budget controls and are far from universal (Haka 2007).

At the same time, managers' incentives for initially understating capital project costs are strong. Receiving capital for a project, rather than being refused, often provides private benefits to subordinates even when subordinates do not expect the projects to be very profitable (Jensen 1986). An additional capital project means a larger asset base and potentially larger business unit income, which can result in greater visibility in the organization and higher likelihood of promotion. Managers whose projects are funded are "winners," while those whose projects are not funded are "losers." Additional projects can provide attractive opportunities to hire friends, buy new equipment and office furniture, travel to desirable locations, and so on.[4]

[3] Managers can also overstate expected revenues from projects; motivations and opportunities are similar to those for cost understatement. For simplicity, we refer to costs only in the remainder of the paper.

[4] Receiving larger operating budgets can be attractive for similar reasons and can provide motivation for subordinates to understate (or at least, not overstate) costs in order to look efficient and attract funding (Fisher et al. 2002). But incentives in the capital budget setting can be particularly strong, because capital project funding is often all-or-nothing, while operating budgets of zero are unlikely. Thus, in game theory terms, the prize spread can be larger in the competition for capital project funding, thereby increasing the magnitude of incentives to take costly actions such as cost understatement (cf. findings that both productive effort and sabotage of competitors' work increase with the size of prize spread; Harbring and Irlenbusch 2011).

Although superiors are likely to anticipate cost understatements on the part of their subordinates and adjust for them—for example, by having contingency funds available for budget overruns—large understatements can nevertheless be costly to organizations because superiors are unlikely to know exactly *how much* subordinates understate costs of any given project. Hence, contingency funds will sometimes be insufficient or remain unused, and either of these outcomes can be costly to organizations.

Moreover, and perhaps more importantly, superiors' uncertainty about the exact magnitude of understatements leads to suboptimal project choice, especially when the magnitude of understatements can be large. If superiors do not know exactly how much subordinates understate, but know that the amount is relatively small, then projects with substantially lower predicted costs (all else equal) are likely to be superior projects in fact, and superiors can make good funding decisions. But if the range of possible understatements is large (e.g., many subordinates make large understatements, while others are relatively honest), then predicted costs are relatively uninformative and, absent other sources of information, superiors have little basis for determining which projects are, in fact, superior.

Of course, limited formal controls in capital budgeting do not mean that subordinates can understate costs with impunity or that initial proposals are entirely uninformative. Large discrepancies between predicted and actual costs in initial project stages can be taken by superiors as indicators that subordinates are unreliable or incompetent. Hence, such discrepancies can damage subordinates' reputations and reduce the likelihood that their projects will receive continuing funding at the next decision stage.[5] Superiors may refuse continuing funding to subordinates with large cost overruns for a variety of reasons. First, they may discount the subordinates' later-period cost predictions so much that—contrary to these predictions—they do not believe that continuing the project would be more profitable than making alternative investments. Second, independent of the project value, superiors may prefer not to continue involvement with a subordinate whom they believe has lied to or betrayed them. (See Evans, Heiman-Hoffman, and Rau [1994] and Bohnet, Greig, Herrmann, and Zeckhauser [2008] for evidence of disutility of being lied to or betrayed, independent of the monetary consequences.) Third, superiors may want to penalize subordinates for apparent misrepresentation, either to punish violations of the norm of honesty (Ohtsubo, Masuda, Watanabe, and Masuchi 2010) or as an act of negative reciprocity (Berg, Dickhaut, and McCabe 1995).

If self-interested subordinates expect such reactions from superiors, then their decisions about whether and how much to understate costs initially will be based on a trade-off between the positive effects of understatement on initial funding and the negative effects on continuation funding.[6] An organization's choice of using continuing or changing superiors to make funding continuation decisions can change the magnitude of initial understatement because it changes the terms of this trade-off.

The larger the difference in p(continuation funding) between projects with and without substantial overruns, the higher is subordinates' expected reputation payoff for reliable initial forecasting and (all else equal) the more likely it is that subordinates will limit their understatements in order to gain this payoff. Changing versus continuing superiors could influence this expected payoff difference in either of two ways. First, subordinates could anticipate that when continuing superiors decide on continuation funding, they will be reluctant to cancel a project after they have initially approved it, even if substantial cost overruns have occurred. Thus, with continuing superiors, the later-period reputation costs of initial understatement will be low and will provide relatively less deterrent against initial understatements. In contrast, as both experimental and archival studies have documented (McCarthy et al. 1993; Staw et al. 1997; Sleesman et al. 2012), third parties, such as changing superiors (or outside consultants asked for continuation recommendations in Kadous and Sedor [2004]), will be more critical of poor performance (i.e., in this case, large cost overruns) than continuing superiors and more likely to cut off funding when projects have bigger discrepancies between actual and predicted costs. Hence, reputation costs would be higher and subordinates should be more cautious about making large understatements of early-period costs in their budget proposals when changing superiors make the continuation decision.

However, it is also possible that changing superiors could have the opposite effect and increase, rather than decrease, the prevalence of initial cost understatements. Introducing a changing superior as an additional decision maker—that is, yet another player whose behavior cannot be predicted with certainty—adds to the uncertainty and complexity of subordinates' decision problem. Two effects on subordinates' trade-offs between initial and continuing funding could result. First, increased uncertainty about continuation funding would make later-period payoffs less attractive to risk-averse subordinates. They would then discount expected later-period payoffs more heavily and focus more on gaining initial funding through substantial understatement.

[5] In presenting the role of subordinates' reputation here, we focus on the updated, project-specific contribution to reputation that initial project outcomes provide and not the reputation subordinates may bring with them from previous projects in natural environments.

[6] In this argument, for the sake of simplicity, there is no possibility for further reputation payoffs after the continuation decision. This simplification is consistent with the minimal influence of later reputation payoffs in the experimental literature (Mayhew 2001), and with natural settings in which post-project reputation payoffs are likely to be small, perhaps because major projects do not occur frequently, reputations are uncertain or are not widely diffused in the long run, the outcome of one project is not regarded as highly relevant for other, different projects, etc.

Second, judgment and decision research has shown that when individuals face higher levels of uncertainty and complexity, they are more likely to break down multi-period decision problems into a sequence of single-period decisions, solve the first-period problem as if there were no inter-period trade-offs, and then proceed to consider the next-period problem only after knowing the first-period outcomes (see Luft and Shields [2009, 260–265] for a summary of this research). If subordinates in our setting behave in this way, then they will be more likely under changing superiors to abandon the attempt to trade off initial funding and continuation funding benefits. They will be more likely to make large initial understatements in order to maximize the probability of initial funding, and will not consider the continuation decision in detail until it occurs.

So far as we are aware, the literature does not provide a basis for choosing between these alternative responses to changing versus continuing superiors in the setting employed in our experiment. Therefore, we state our hypothesis in the null form:

Hypothesis: Understatement of first-period costs by subordinates in capital budget proposals will not differ depending on whether superiors continue or change for the funding continuation decision in the second period.

III. METHOD

Participants

Participants were 84 undergraduate business students at a European university. Their average age was 23.5 years, and 28 percent of the participants were female.[7] All participants had a basic familiarity with laboratory experiments in the business domain, having had prior experience with at least two experiments in the same university laboratory, but with different experimental tasks. They received performance-dependent compensation that averaged €8.38. (See below for details of the compensation system.) Participants interacted over a computer network (z-Tree software; Fischbacher 2007) to ensure anonymity.

Design and Procedure

Participants were randomly assigned to either the subordinate or the superior role and remained in this role throughout the seven rounds of the experiment. A firm was composed of two subordinates and one superior, and firm members were rematched after each round. All participants knew that the experiment would consist of several rounds and that each round would consist of two periods, but they did not know the exact number of rounds. Participants were also randomly assigned to one of the two continuation decision conditions (the same or a different superior made the decision to continue funding a project in the second period) and remained in this condition throughout the experiment.[8] (See Table 1 for a summary of the experimental procedure.)

Participants began by reading instructions that described the experimental task and its setting. They then took a quiz about the information and incentive structure of their task and had to answer all questions correctly before they could proceed.

In the first period of each round, each subordinate could propose a project for funding. Both subordinates and superiors knew that each project would generate revenue between 90 and 110 units of experimental currency per period, and that realized project revenues followed a uniform (integer) distribution (90, 91,. . .,110). In the absence of private information, both subordinates and superiors knew that the costs of a project in each period were uniformly distributed between 70 and 90 units of experimental currency. (Thus, consistent with settings in which control systems work well enough to deter subordinates from presenting negative-NPV [net present value] projects, all projects that were actually proposed had non-negative values, but some were substantially more profitable than others.)

Each subordinate received private information about the first-period costs of his or her project, in the form of a statement that the costs would be X with a probability of 75 percent; the remaining 25 percent probability was uniformly distributed across the 70–90 range.[9] Values of the cost signal X were random draws from the 70–90 uniform distribution that were matched across conditions in each period. For example, in the first period of the first round, the two subordinates in each firm received cost signals of 79 and 83 in both Change and Stay. A new pair of random draws was made at the beginning of each round.

[7] Gender was initially included as a control variable in all models reported in Section IV (Tables 3–6). Gender effects did not approach conventional levels of significance and, thus, gender was omitted from final versions of the models.

[8] Thus, in the experiment, the difference between changing versus continuing superiors is exogenous, corresponding to a natural setting in which the organization has a general policy of assigning or not assigning continuation reviews to the original decision maker. In other natural settings, the difference can be endogenous; for example, continuation reviews for projects with satisfactory progress remain with the original decision maker, while projects with disappointing initial outcomes are reviewed by other managers.

[9] Research indicates that misrepresentation behavior differs, depending on whether private information is probabilistic or deterministic. For example, Schweitzer and Hsee (2002) find significantly less self-serving misrepresentation of performance with deterministic information. We believe that probabilistic private information is more characteristic of budget settings; therefore, we provided a probabilistic, rather than deterministic, private signal. Thus, subordinates in our experiment had, and superiors knew they had, naturalistic opportunities to provide cost forecasts that were improbably low, but were not outright lies.

TABLE 1

Sequence of Experimental Procedures

1. Participants received instructions and had to pass a quiz about the information and incentive structure of the task before continuing.
2. Each subordinate received private information about the first-period costs of his or her projects.
3. Each superior received a private first-period cost forecast from each of two subordinates and nonfinancial information about the two projects.
4. Superiors decided which (if any) project to fund in the first period.
5. Subordinates learned whether their projects were funded.
6. Superiors and subordinates with funded projects learned the realized first-period revenues and costs of their projects
7. Subordinates with funded projects received private information about second-period costs and privately reported a cost forecast to their superiors (either the same superior who funded the project in the first period or a different superior, depending on experimental condition).
8. Superiors received a profit forecast for an alternative one-period project, as well as the cost forecast and nonfinancial information for the second period of the subordinate's project.
9. Superiors decided whether to fund the alternative project or a second-period continuation of the subordinate's project.
10. Subordinates learned the superior's decision, and both superiors and subordinates learned the realized profit of the project funded in the second period.
11. Subordinates and superiors were rematched for the next round.
12. At the end of seven rounds, all participants answered post-experiment questions (demographics, manipulation checks, etc.).

Participants were informed that each cost draw was independent; thus, information about the cost of one subordinate's project in the first period did not provide information about the costs of the other subordinate's project in that period or about project costs in other periods or rounds. Each subordinate privately provided a first-period cost forecast for his or her project to the superior. Both subordinates and superiors were told that subordinates "can decide themselves whether to submit a cost report that is equal, more, or less than the actual cost prediction" that they received as private information. Thus, at this point, subordinates had no individual reputation for reliable forecasting, and in the first period of each round, superiors had to judge the forecasts they received based on general *ex ante* expectations and experience with other subordinates. This absence of first-period reputations enables us to isolate more clearly the subsequent reputation effects of cost overruns.

Superiors then decided which project (if any) to fund; they could not fund both projects. Funding a project meant covering its actual costs after these were known, even if they were more (or less) than the costs predicted by subordinates. Superiors based their decisions on the cost forecasts and on nonfinancial information about each project's level of innovativeness and the likelihood that the project would improve the brand image of the firm. For each of the nonfinancial measures, every project had a score from 1 (very low) to 10 (very high), which was also known to (but not provided by) the subordinates. These scores remained the same for a given project across the two periods of a round. The nonfinancial information provided superiors with some additional basis for making decisions and, thus, a basis for feeling committed to the projects they chose. Observations in a pilot test of the experimental materials, in which only the two cost forecasts were provided to superiors, suggested that the superiors' task was not sufficiently information-rich to generate interest in and potential commitment to the chosen proposal. (Analyses in the "Results" section control for this information, which influenced both superiors' funding decisions and subordinates' understatements.)

At the end of the first period, subordinates and superiors received information about realized revenues and costs. Realized costs for funded projects were drawn from a distribution in which there was a 75 percent probability that realized costs would equal the subordinate's private signal, and the remaining 25 percent probability was distributed uniformly across the 70–90 interval.[10] In one of the experimental conditions ("Change"), superiors were rotated randomly so that each subordinate had a new superior in the second period of each round. In the other condition ("Stay"), superiors remained in their firms in the second period. Subordinates who had received first-period funding now received private information about their project's second-

[10] Unlike the private signals, the realized cost draws were not the same for all firms and conditions in a period. Such matching was not feasible because the realized cost draws were tied to the private signals of the subordinate who won funding. For example, in one firm, the winning subordinate might be the one with a private signal of 79, while in another firm, the winner was the subordinate with a private signal of 83; hence, their actual costs were drawn from different distributions. This did not result in any systematic difference between conditions in the realized costs, however; the mean of realized cost draws across periods was 79 in both Change and Stay.

period costs and provided cost estimates to their second-period superiors. Superiors then decided whether to provide continuing funding for the project in the second period or to fund an alternative one-period project instead.

Alternative projects were designed to be reasonably attractive, but not to dominate the subordinates' projects. For each firm in each round, the predicted profits of the alternative project were programmed to be one unit higher than the actual second-period profits of the subordinate's project (unknown at that point to both subordinate and superior). Subordinates were likely to overstate when predicting the profits of their own projects in order to make these look highly attractive and, thus, their projects would look more attractive than the alternatives, but superiors were also likely to discount these predictions sufficiently that they would seriously consider the alternative investment. Participants were informed that actual profits from the alternative project would be the predicted amount plus a noise term drawn from a uniform distribution of integers between −5 and +5. Thus, the returns of the alternative projects were less uncertain than the returns of subordinates' projects, adding to the attractiveness of the alternatives.

Superiors decided whether to fund the alternative project or to provide continued funding for the subordinate's project in the second period. Subordinates learned the superiors' decision, and both parties then learned the realized revenues and costs of whichever project was funded. Next, subordinates and superiors were rematched into new firms for the next round.

After seven two-period rounds, the experiment finished with an exit questionnaire that included manipulation checks and questions on demographics.[11] The whole experiment took about 50 minutes.

Compensation

Compensation for both subordinates and superiors was common knowledge and was based on the realized profits of a single round, which was randomly chosen at the end of the experiment.[12] For this round, compensation for both subordinates and superiors was calculated as:

25 percent of realized profit per funded project + fixed wage.

For periods in which a subordinate's project was not funded, he or she received only the fixed wage. The realized profit of a project was the difference between the project's realized revenue (a draw from the [90, 91,. . .,110] uniform distribution) and the realized investment costs (a draw from the [70, 71,. . .,90] distribution, in which the subordinate's private information had a 75 percent chance of being the realized cost and the remaining probability was uniformly distributed over the range). For alternative second-period projects, realized profit (which contributed only to the superior's payoff, not the subordinate's) was the profit prediction provided to superiors plus a noise term drawn from a uniform distribution of integers between −5 and +5.

Superiors' variable compensation depended only on the decisions they made themselves. Thus, in the condition in which superiors rotated in the second period of each round, superiors' compensation for a round depended on the first-period profits of the projects they chose in the first period and the second-period profits of the (different) projects they chose in the second period.[13]

The exact amount of the fixed wage was set *ex ante* for each experimental condition to result in approximately equal mean (not individual) payments across roles and conditions. During the experiment, participants did not know the amount of the fixed wage, but (consistent with the incentives described in the hypothesis motivation) they knew that their payoffs would always be higher if they succeeded in receiving funding for their projects (subordinates) or choosing the most profitable projects (superiors) than if they did not.[14]

IV. RESULTS

Dependent Measures and Descriptive Statistics

We use three measures for the dependent variable in the model employed to test our hypothesis, subordinates' first-period understatements of cost. Understatement (a) is a subordinate's private cost signal minus his or her cost forecast. Understatement

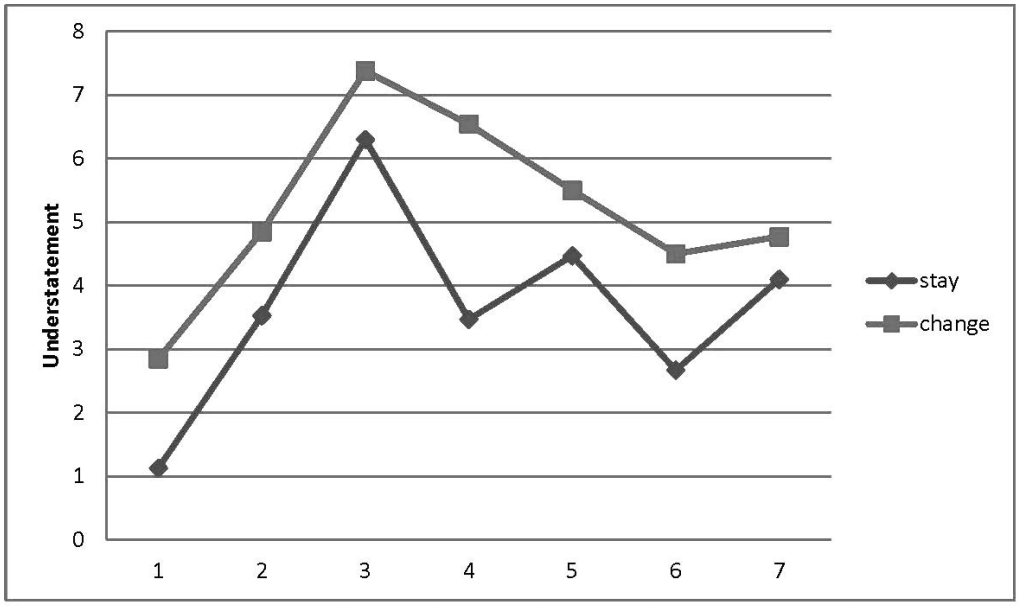
[11] One participant with the role of a subordinate and two participants with the role of superior did not complete the exit questionnaire. This reduces the number of analyses in which we use variables from the exit questionnaire.

[12] Consistent with analytic models of capital budgeting (e.g., Reichelstein 1997) and evidence from practice (Haka 2007), we make individuals' compensation depend on the realized profits of projects rather than on a comparison between realized profits and original proposals.

[13] For simplicity, we restrict superiors' monetary incentives in the experiment to the payoffs they could earn from successful project choice. Thus, for example, they could not earn additional payoffs from acquiring a good reputation for how they manage subordinates (e.g., being loyal or, conversely, not being deceived by unreliable subordinates).

[14] Incentives of this kind, which include *ex ante* adjustments of various kinds to equalize mean (not individual) payoffs across conditions, are often used in the experimental literature in finance and accounting (e.g., Nelson, Bloomfield, Hales, and Libby 2001; Bloomfield, O'Hara, and Saar 2005; Hales 2009). In such systems, participants know they will always earn more by performing better, thus providing incentives consistent with the theory employed in the studies.

FIGURE 1
First-Period Cost Understatement by Round



Round	1	2	3	4	5	6	7	Mean across rounds
Change minus Stay	1.72*	1.32	1.08	3.07***	1.03	1.83*	0.67	1.54***

*, *** Indicate significance of the difference in mean understatements between Change and Stay at $p < 0.10$ and $p < 0.01$, respectively.

Understatement = subordinate's private information about cost minus the first-period cost prediction provided by subordinate; Stay = the same superior makes the decision to fund a project in the first period and to continue it in the second period; and Change = a new superior makes the second-period continuation decision.

The experiment consists of seven rounds, with two periods in each round.

(b) is the expected value of costs given the private signal minus the subordinate's cost forecast. This measure allows for the fact that, when private signals are extreme, expected values can differ substantially from the private signal. For example, if the private signal is 88, then the expected value is 85.9. For participants who believe that they should provide something like an expected value rather than their private signal, a forecast of 86 in this case is not an understatement. Understatement (c) is a percent understatement measure: subordinates' actual understatement (a) divided by the maximum possible understatement (a).

We use understatement (a) as the primary measure in our reporting of results; hypothesis tests using the alternative measures are footnoted briefly. In general, understatement by measure (b) is only trivially different from (a). Understatement (c) presents a directionally similar pattern of results, but is not as close to (a) as (b) is. Although we report measure (c) because similar measures have been used in the literature (e.g., Evans, Hannan, Krishnan, and Moser 2001; Zhang 2008), we do not believe it is as suitable for our study as the other two measures, because it treats understatements as equal that are not, in fact, equal in important respects. For example, understatement by this measure is 100 percent, both when a subordinate with a private signal of 71 forecasts costs of 70 and when a subordinate with a private signal of 90 forecasts costs of 70. Subordinates are unlikely to regard these two understatements as equal, either ethically or practically, and their consequences for superiors who rely on the cost forecasts are not the same.[15]

Figure 1 depicts mean first-period cost understatements, using measure (a), in the Stay and Change conditions in each round of the experiment. Subordinates understate more in Change than in Stay: mean first-period cost understatement across all

[15] The intent of the percent measure is to allow for the fact that subordinates who receive low signals cannot understate as much as subordinates who receive higher signals. In our analyses using understatement measures (a) and (b), we address this issue by including the subordinates' private signal as a control variable.

TABLE 2

Descriptive Statistics
Three Measures of Mean Cost Understatement by Subordinates
(Standard Deviations in Parentheses)

	Stay Condition		Change Condition	
Variable	**Funded n = 105**	**Not Funded n = 105**	**Funded n = 91**	**Not Funded n = 91**
Understatement (a) Period 1	4.37	2.95	6.37	4.02
	(4.02)	(4.03)	(4.17)	(4.25)
Understatement (b) Period 1	4.36	2.79	6.38	3.85
	(3.68)	(3.85)	(3.77)	(4.09)
Understatement (c) Period 1	0.41	0.26	0.62	0.38
	(0.34)	(0.41)	(0.34)	(0.41)

	Stay Condition	Change Condition
	n = 105	n = 91
Understatement (a) Period 2	3.04	6.76
	(4.95)	(4.87)
Understatement (b) Period 2	3.00	6.10
	(4.62)	(4.69)
Understatement (c) Period 2	0.23	0.52
	(0.56)	(0.37)

Understatement (a) = private cost signal − cost forecast.
Understatement (b) = expected value given private signal − cost forecast.
Understatement (c) = actual understatement as a percent of possible understatement = (cost forecast − private signal)/(70-private signal).

rounds (using a cross-round average per participant as the unit of analysis) is significantly greater in Change (mean understatement (a) = 5.20, std. deviation = 4.36) than in Stay (mean understatement (a) = 3.66, std. deviation = 4.08), $t_{54} = 2.65$, $p = 0.01$. Understatement amounts vary from round to round not only because of learning effects, but also because of differences across rounds in private cost signals, which provide more scope for understatement in some rounds than others; when the rounds are analyzed individually, the difference between conditions is significant in rounds 1, 4, and 6. We use data from all rounds in the analyses reported below, but control for effects of round and its interactions with variables of interest, as well as effects of private cost signal magnitudes.

Table 2 presents descriptive statistics for subordinates' cost understatements in Periods 1 and 2, using all three measures and distinguishing between funded and unfunded projects in Period 1. With all three measures and in both periods, understatements are directionally higher in Change than in Stay. In the first period, when subordinates' projects are competing with each other for funding, understating costs appears to be advantageous: understatements are larger for funded than for unfunded projects. (See below for statistical tests of these comparisons and more detail on second-period behavior.)

Hypothesis Test

In order to test the effect of changing superiors on subordinates' first-period cost understatements, with controls for other influences on understatements, we estimate the model presented in Table 3:

$$Under1 = \alpha_0 + \alpha_1 Change + \alpha_2 Costinfo1 + \alpha_3 Round + \alpha_4 Round \times Change + \alpha_5 Inno + \alpha_6 Image + \alpha_7 Distributive\ Fairness + \varepsilon$$

The dependent variable, *Under1,* is subordinates' first-period understatement of costs, measured as subordinates' private cost signals minus their cost forecasts (understatement measure (a)), and the main independent variable is the experimental treatment (*Change*). We control for the subordinates' private signals of cost (*Costinfo1*) in each round, because less understatement is possible when the signal is nearer the lower bound of plausible costs. We also control for learning effects over the seven rounds of the experiment (*Round*) and possible differential learning in the Change and Stay conditions (*Round* × *Change* interaction); the scores of a subordinate's project on innovation (*Inno*) and reputation (*Image*); and the participants'

TABLE 3

First-Period Cost Understatement by Subordinates
OLS Regression

Independent Variable		Estimate	t-statistic	p-value
Intercept	α_0	−37.70	−6.09	0.00
Change	α_1	2.12	1.96	0.05
Costinfo1	α_2	0.61	9.05	0.00
Round	α_3	0.41	2.91	0.00
Round × *Change*	α_4	−0.12	−0.55	0.58
Inno	α_5	−0.81	−4.15	0.00
Image	α_6	−0.72	−3.22	0.00
Fairness	α_7	−0.21	−1.27	0.21
Adj. R^2		0.25		

All p-values are two-tailed. n = 385 cost forecasts = 56 subordinates × 7 rounds − 7 missing observations (7 rounds × 1 participant who did not provide a fairness judgment).
The dependent variable is the first-period subordinate's private cost signal minus the cost prediction provided by subordinate to superior.

Variable Definitions:
Change = 0 if the same superior makes the decision to fund a project in Period 1 and to continue it in Period 2, and coded as 1 if a new superior makes the Period 2 continuation decision;
Costinfo1 = cost signal a subordinate receives before providing a forecast to the superior;
Round = round of the experiment;
Inno = innovation score of a subordinate's project;
Image = reputation score of a subordinate's project; and
Fairness = response on a seven-point Likert scale (1 = strongly agree, 7 = strongly disagree) to the statement: "I found the distribution of profits of an accepted project between the superior and me fair."

views on the fairness of profit distribution between superior and subordinate (*Fairness*).[16] Standard errors are clustered to allow for dependence in multiple observations from the same participants across rounds.

The significant coefficient on *Change* in Table 3 indicates that, on average, subordinates understate their first-period private information about costs to a greater extent in the Change than in the Stay condition ($\alpha_1 = 2.12$, $p = 0.05$). As expected, subordinates who receive higher private cost signals understate significantly more ($\alpha_2 = 0.61$, $p < 0.01$). Understatement also increases significantly over rounds ($\alpha_3 = 0.41$, $p < 0.01$), but this experience effect does not differ across experimental conditions ($\alpha_4 = -0.12$, $p = 0.58$); that is, the Change versus Stay difference is not systematically larger or smaller in later rounds than in earlier ones.

Positive coefficients on the projects' innovation and image scores indicate that subordinates understated less when these scores were high. Apparently, subordinates were aware of superiors' tendency to fund projects with higher scores on these nonfinancial factors (see Table 5 and accompanying discussion); thus, higher scores on these factors reduced subordinates' "need" to understate costs to gain funding.[17]

These results are not consistent with an argument that subordinates in our setting limit their first-period cost understatement more when they expect a different, potentially more critical individual to make the renewal decision. The results are more consistent with an argument that *Change* increases subordinates' concerns about the likelihood of receiving second-period funding after an overrun and/or increases their uncertainty about the second period, and in consequence, subordinates focus more on gaining first-period funding and less on the uncertain benefits of developing a reputation for reliable prediction in the first period.

The hypothesis test addresses the *effect* of Change versus Stay on first-period understatement; the remaining analyses provide additional evidence on the *process* by which this effect occurs. We verify that (1) subordinates experience incentives

[16] *Fairness* is included because prior literature has shown it to have a significant effect on subordinates' misrepresentations of private information (e.g., Zhang 2008; Brüggen and Luft 2011). The feeling of being treated fairly results in a sense of positive reciprocity (Folger and Kass 2000) and triggers more honest behavior. In this model, we omit the interaction of *Costinfo1* and *Change,* which was tested in a larger model, but was not significant ($p = 0.92$). Omitting the variable *Fairness* does not change the results of the hypothesis test materially ($\alpha_1 = 1.99$, $p = 0.06$).

[17] Results are the same in direction and significance if we use our measure (b) of cost understatement (subordinates' cost forecast minus expected cost given private signal) as the dependent variable. Results are directionally the same for our measure (c), percent understatement, but do not reach conventional levels of significance ($\alpha_1 = 0.14$, $p = 0.21$, two-sided).

TABLE 4

Superior Decisions
Subordinate Projects Funded and Not Funded, Mean Cost Forecasts
(Standard Deviations in Parentheses)

	Stay Condition		Change Condition	
Variable	**Funded**	**Not Funded**	**Funded**	**Not Funded**
Mean cost forecast in Period 1	n = 105	n = 105	n = 91	n = 91
	75.58	77.70	73.60	76.67
	(3.65)	(3.79)	(3.31)	(4.31)
Mean cost forecast in Period 2	n = 43	n = 62	n = 36	n = 55
	77.33	77.06	77.08	75.51
	(4.16)	(4.28)	(4.60)	(4.38)

for understating first-period costs that do not differ between Change and Stay conditions, (2) they experience second-period disincentives for understating first-period costs that *do* differ between Change and Stay conditions, and (3) subordinates' concerns about more critical second-period superiors in Change are associated with the magnitude of their first-period understatements.[18]

First-Period Funding Decisions by Superiors

In principle, the difference between subordinates' first-period cost understatements in Stay and Change could be due to differences in perceived first-period incentives, as well as the second-period incentives that we focus on in this study. Therefore, we verify that subordinates in both experimental treatments have similar, and reasonably founded, beliefs about the Period 1 incentives for understatement, so that differences in behavior are primarily due to different expectations about Period 2 (changing versus continuing superiors).

Table 4 presents descriptive statistics on superiors' project funding decisions. When choosing a proposal to fund in the first period, superiors had only subordinates' forecasts and nonfinancial indicators as a basis for their decisions and, hence, cost forecasts played an important role. (Because subordinates and superiors are rematched in each round, subordinates did not yet have any individual reputation in the first period that could affect superiors' decisions.) Mean cost forecasts were 75.58 for funded projects versus 77.70 for unfunded projects in Stay ($t_{28} = 3.98$, $p < 0.01$), and 73.60 for funded and 76.67 for unfunded proposals in Change ($t_{24} = 3.90$, $p < 0.01$).

The logistic regression reported in Table 5 analyzes first-period incentives for understatement, controlling for additional factors that could affect superiors' decisions. Because only one of the two projects in a firm can be funded in each round, observations from two subordinates in the same firm and the same round are radically non-independent: if one subordinate is funded, then the other subordinate necessarily is not. Hence, each observation used in this analysis consists of data from *one* randomly selected subordinate in each firm in each round. Independent variables are the experimental manipulation (*Change*), the subordinate's understatement of costs (*Under1*), the interaction of these two variables, and the control variables used in the analysis in Table 3.[19]

The significant coefficient on *Under1* in Table 5 ($\alpha_1 = 0.17$, $p < 0.01$) indicates that, other things equal, understating costs increased subordinates' chances of receiving funding for their projects in the first period. The non-significant coefficient on the *Under1* × *Change* interaction ($\alpha_3 = 0.05$, $p = 0.47$) provides evidence that the first-period cost understatements did not have differential effects on first-period funding in Stay versus Change.[20] The coefficient for *Costinfo1* is negative and significant ($\alpha_4 = -0.22$, $p < 0.01$), indicating that, controlling for the magnitude of understatement, a subordinate with a lower private cost

[18] As a manipulation check, a post-experiment question asked subordinate participants to choose which of the following alternatives described the way Period 2 funding decisions were made: (1) "The superior who funded my project in Period 1 also made the Period 2 decision;" or (2) "A new superior made the Period 2 decision." All subordinates answered this question correctly.

[19] Subordinates' fairness judgments, which are included in the analysis of subordinates' behavior in Table 3, are naturally not included in the analysis of superiors' decisions in Table 5.

[20] To provide assurance that the results of this random sample-based analysis are not due to sampling error, we also perform a bootstrapping analysis with 150 replications. Results are similar to those reported in Table 5. The key result in the analysis—the lack of significant difference between first-period incentives for understatement in Change and Stay—is stronger in the bootstrapping analysis ($p = 0.71$). Coefficients that are significant in Table 5 have somewhat higher p-values in the bootstrapping analysis ($0.05 < p < 0.10$).

TABLE 5

First-Period Funding Decisions
Logistic Regression

Independent Variable		Estimate	χ^2	p-value
Intercept	α_0	10.80	2.55	0.11
Under1	α_1	0.17	9.66	0.00
Change	α_2	−0.41	0.84	0.36
Under1 × *Change*	α_3	0.05	0.51	0.47
Costinfo1	α_4	−0.22	8.14	0.00
Round	α_5	−0.01	0.02	0.89
Inno	α_6	0.62	7.28	0.01
Image	α_7	0.53	5.98	0.01

n = 196 first-period funding decisions (= 28 firms × 7 rounds). All p-values are two-tailed. $R^2 = 0.20$. The dependent variable is an indicator of whether a subordinate's project was funded in the first period (1 = yes, 0 = no).

Variable Definitions:
Under1 = subordinate's private information about cost minus the first-period cost forecast provided by subordinate to superior;
Change = 0 if the same superior makes the decision to fund a project in Period 1 and to continue it in Period 2, and coded as 1 if a new superior makes the Period 2 continuation decision;
Costinfo1 = cost signal a subordinate receives before providing a forecast to the superior;
Round = round of the experiment;
Inno = innovation score of a subordinate's project; and
Image = reputation score of a subordinate's project.

signal has a higher probability of funding in the first period. The coefficients for the two nonfinancial information variables, *Inno* and *Image,* are positive and significant, indicating that nonfinancial information also influences superiors' funding decisions.[21]

The analysis in Table 5 provides evidence on *actual* incentives for understatement, but in principle, subordinates' *perceived* incentives could have been different. We check for perceived incentives with a post-experiment question that asked subordinates to agree or disagree (1 = strongly agree; 7 = strongly disagree) with the statement: "I believed that understating the predicted costs of my project in Period 1 would *increase* my chances of getting my project funded *in Period 1*." Mean responses do not differ between experimental conditions ($t_{53} = 0.49$, $p = 0.62$) and are significantly on the "agree" side of the scale (mean [Change and Stay pooled] = 2.71, $t_{54} = 5.14$, $p < 0.01$). Thus, subordinates' perceived first-period incentives are consistent with superiors' actual funding decisions.

Second-Period Disincentives for First-Period Cost Understatement

Table 4 provides descriptive statistics on funded and unfunded projects in the second period.[22] In both conditions, only about 40 percent of the proposals funded in the first period were continued in the second, since the alternative project was designed to be reasonably attractive to superiors and make the likelihood of non-continuation a non-trivial threat to subordinates.

As indicated in the hypothesis motivation, differential *second-period* consequences of first-period understatement in Stay versus Change are likely to be important in explaining differences in first-period understatement. Large first-period cost overruns could reduce the probability of second-period funding by making the subordinates' second-period proposals less credible, and/or they could affect funding independently of second-period proposals (e.g., if superiors retaliate for perceived dishonesty of first-period proposals, even when second-period proposals are attractive).

[21] No interactions were expected between the experimental variables and the control variables *Inno* and *Image,* nor were interactions expected between *Round* and other variables. These interactions were tested, but they were not significant and did not change inferences about the experimental variables; for simplicity, these interactions are omitted from the final model reported in Table 5.

[22] Note that the "funded" and "not funded" proposals in the second period, as shown in Table 4, are not competing with each other and, thus, a comparison of the cost forecasts for these two project categories would not be informative. In the second period, as explained in the "Methods" section, each subordinate's project is competing with an alternative project that has been programmed to have a slightly higher expected value than the subordinate's project.

TABLE 6

Second-Period Funding Decisions
Logistic Regression

Independent Variable		Estimate	χ^2	p-value
Intercept	α_0	3.17	0.04	0.85
Change	α_1	7.78	3.14	0.08
Overrun1	α_2	−0.03	0.00	0.97
Expprofit2	α_3	−5.85	4.47	0.03
Expprofit2^2	α_4	3.00	10.07	0.00
Change × *Overrun1*	α_5	1.66	5.50	0.02
Change × *Expprofit2*	α_6	−6.93	2.35	0.13
Change × *Expprofit2*^{2}	α_7	0.60	0.16	0.69
Overrun1 × *Expprofit2*	α_8	−0.73	0.75	0.39
Overrun1 × *Expprofit2*^{2}	α_9	0.45	4.34	0.04
Change × *Overrun1* × *Expprofit2*	α_{10}	−1.01	4.42	0.04
Inno	α_{11}	0.89	3.74	0.05
Image	α_{12}	−0.80	0.47	0.49
$R^2 = 0.36$				

n = 48. These models are estimated using only observations for which the second-period proposal from the subordinate appeared more attractive than the alternative investment. The dependent variable is an indicator of whether a subordinate's project was funded in the second period (1 = yes, 0 = no).

Variable Definitions:
Change = 0 if the same superior makes the decision to fund a project in Period 1 and to continue it in Period 2, and coded as 1 if a new superior makes the Period 2 continuation decision;
Overrun1 = realized first-period cost minus the subordinate's forecast of first-period cost;
Expprofit2 = amount by which the subordinate's project would be expected to out-earn the superior's alternative project *if* the subordinate's forecast equaled his or her private cost signal;
Inno = innovation score of a subordinate's project; and
Image = reputation score of a subordinate's project.

In order to examine the joint influences of first-period cost overruns and second-period cost forecasts, we calculate a measure *Expprofit2,* the amount by which the subordinate's project would be expected to out-earn the superior's alternative project *if* the subordinate's cost forecast was an honest statement of his or her private signal.[23] The smaller the coefficient on *Expprofit2,* the less superiors' funding decisions are influenced by the apparent attractiveness of subordinates' second-period proposals. An appropriately signed two-way interaction between *Expprofit2* and *Overrun1* (first-period cost overrun) would indicate that higher cost overruns in the first period decrease the effectiveness of attractive-looking second-period proposals in inducing superiors to provide continuation funding. An appropriately signed three-way interaction between *Expprofit2, Overrun1,* and *Change* would indicate, as we expect, that changing superiors are more likely than continuing superiors to discount attractive-looking second-period proposals when first-period cost overruns are high (see analysis below for identification of the "appropriate" signs).

This expectation applies only to cases in which subordinates' second-period proposals have some likelihood of appearing attractive to superiors. When the value of *Expprofit2* is less than 0—that is, when the expected profits from subordinates' proposals are lower (and the risk higher) than the alternative project, even without discounting subordinates' predictions—we expect that superiors are unlikely to provide continuation funding, regardless of the treatment and of first-period overruns. To confirm this expectation about unattractive second-period proposals, we estimate a logistic regression (not tabulated) with continuation funding as the dependent variable, using only observations with *negative Expprofit2*. (Standard errors are clustered to allow for dependence in multiple observations from the same participants across rounds.) As expected, the Stay versus Change treatment is not significant ($p = 0.82$), nor is the overrun magnitude ($p = 0.44$), their interaction ($p = 0.48$), or the magnitude of the (negative) *Expprofit2* ($p = 0.43$).

In contrast, when superiors' second-period budget proposals appear more profitable than the alternative project, superiors' funding decisions are influenced by both the magnitude of *Expprofit2* and the first-period overrun, and the shape of these influences differs between Stay and Change treatments. Table 6 presents results of a logistic regression that captures these

[23] *Expprofit2* = (Expected revenue of subordinate's project − 0.75 × subordinate's cost forecast − 0.25 × 80) − expected profit of alternative project presented to superior.

effects, using only observations with *Expprofit2* > 0. Inspection of the data revealed a marked nonlinearity in the effect of *Expprofit2* on superiors' decisions, which is represented in the model by the *Expprofit2*^{2} term. The nonlinearity is intuitive: superiors are attracted by forecasts of higher profits up to a point, but not when the forecasts are so high as to lose credibility:

$$\begin{aligned} Funding2 = {} & \alpha_0 + \alpha_1 Change + \alpha_2 Overrun1 + \alpha_3 Expprofit2 + \alpha_4 Expprofit2^2 + \alpha_5 Change \times Overrun1 \\ & + \alpha_6 Change \times Expprofit2 + \alpha_7 Change \times Expprofit2^2 + \alpha_8 Overrun1 \times Expprofit2 \\ & + \alpha_9 Overrun1 \times Expprofit2^2 + \alpha_{10} Change \times Overrun1 \times Expprofit2 + \alpha_{11} Inno + \alpha_{12} Image + \varepsilon \end{aligned}$$

The dependent variable is superiors' second-period funding decision (coded as 1 if superiors continued the first-period project, and 0 if they discontinued it and funded an alternative project). Independent variables, in addition to those in the first-period logistic regression, are *Expprofit2* (defined above) and *Overrun1* (realized first-period cost minus subordinate's prediction of first-period cost).[24] Standard errors are clustered to allow for dependence in multiple observations from the same participants across rounds.

As Table 6 indicates, the three-way interaction of *Change, Overrun1,* and *Expprofit2* is significant ($p = 0.04$). Because of the multiple interactions and nonlinear terms in the model, the inference to be drawn from the significant three-way term may not be immediately transparent. The primary goal of the analysis is to determine whether, as expected, changing superiors are more likely than continuing superiors to discount attractive-looking second-period proposals, especially when first-period cost overruns are high. To determine whether the analysis is consistent with this expectation, we take the partial derivative of *y* with respect to *Expprofit2,* the measure of the attractiveness of the subordinate's second-period funding proposal. Using all relevant coefficients with $p \leq 0.05$ in the model in Table 6, the partial derivative is:

$$\frac{dy}{dExpprofit2} = -5.85 + 6.00 + 0.9{*}Overrun1 - 1.01{*}(Change \times Overrun1)$$

The first two terms in the partial derivative come from the coefficients on *Expprofit2* and *Expprofit2*^{2}. Together, they indicate that a marginal increase in *Expprofit2* increases the likelihood of receiving continuation funding (*y* increases by $-5.85 + 6.00 = 0.15$). The remaining two terms include first-period overrun effects. With continuing superiors (*Change* coded as 0), the effect of a first-period overrun is positive, consistent with escalation, but the larger negative coefficient on the *Change* $\times$ *Overrun1* interaction in the derivative indicates that the positive effect of *Expprofit2* is reduced in the Change condition, and the magnitude of this reduction is larger for larger first-period overruns.[25] This result is consistent with the argument in our hypothesis motivation that overruns will lead changing (not continuing) superiors to be more critical of second-period proposals.

Supplementary Evidence on Causes and Consequences of Understatement

Our primary concern in the paper is simply with whether subordinates understate first-period costs more in Stay or in Change as a result of trade-offs between first- and second-period incentives. Additional evidence gathered in the experiment, however, enables us to supplement the hypothesis test with information supporting both the causal mechanism and the negative consequences of understatement that are proposed in the hypothesis motivation.

The first of our supplementary analyses uses evidence from subordinates' responses to a post-experiment question to support the argument that greater first-period cost understatement in Change was the result of concerns about the likelihood of losing funding in the second period. Subordinates rated their agreement with the statement: "I believe that outsiders are often more critical about a project and less optimistic about its prospects than are people who have been involved in selecting and managing the project." Subordinates typically agreed with this item, especially if they had actually experienced change in superiors (92 percent of subordinates in the Change condition and 72 percent in the Stay condition chose responses on the "agree" side of the scale midpoint). The difference in mean responses between the Stay and Change conditions did not reach

[24] We expected no interactions between the experimental variables and the control variables *Inno* and *Image*. These interactions were tested, but they were not significant and did not change inferences about the experimental variables; for simplicity, these interactions are omitted from the model reported in Table 6. *Round* was also omitted, both because it was insignificant when included and because it was partially confounded with *Expprofit2* through the common revenue draws that helped to create both *Expprofit2* and the (insignificant) *Round* effects. Including a three-way interaction of *Change, Overrun1,* and *Expprofit2*^{2} in the model along with the linear three-way interaction was not practicable because of multicollinearity problems. If the three-way interaction with the quadratic term was included without the linear three-way, to avoid multicollinearity, the coefficient on the quadratic interaction did not reach conventional levels of significance ($p > 0.10$).

[25] If the partial derivative included all coefficients with $p < 0.15$, then it would also include a $-6.93 * Change$ term, indicating that *Expprofit2* has a less positive effect in Change than in Stay regardless of first-period overrun magnitude. Note, also, that caution should be used in interpreting the magnitude of the coefficients, as these represent effects on *y* in the logistic regression, which would need to be transformed via the logit equation to estimate effects on the probability of continuation funding.

TABLE 7

First-Period Cost Understatement
OLS Regression Including Subordinates' Concerns about Second-Period Funding
(n = 385)

Independent Variable		Estimate	t-statistic	p-value
Intercept	α_0	−37.08	−5.92	0.00
Change	α_1	0.37	0.41	0.68
Costinfo1	α_2	0.61	8.91	0.00
Round	α_3	0.35	3.09	0.00
Second-period concern	α_4	0.19	1.28	0.20
Second-period concern × *Change*	α_5	−0.79	−1.66	0.10
Inno	α_6	−0.81	−4.17	0.00
Image	α_7	−0.72	−3.26	0.00
Fairness	α_8	−0.26	−1.72	0.09
Adj. R^2		0.26		

All p-values are two-tailed. n = 385 cost reports (calculated as in Table 3). The dependent variable is subordinates' first-period cost understatement.

Variable Definitions:
Change = 0 if the same superior makes the decision to fund a project in Period 1 and to continue it in Period 2, and coded as 1 if a new superior makes the Period 2 continuation decision;
Costinfo1 = cost signal a subordinate receives before providing a forecast to the superior;
Round = round of the experiment;
Second-period concern = response to a post-experiment question on concerns about not receiving second-period funding from a new superior (centered around the mean); higher scores indicate less concern;
Inno = innovation score of a subordinate's project;
Image = reputation score of a subordinate's project; and
Fairness = response on a seven-point Likert scale (1 = strongly agree, 7 = strongly disagree) to the statement: "I found the distribution of profits of an accepted project between the superior and me fair."

conventional levels of significance ($t_{53} = 1.47$, two-tailed $p = 0.14$). This non-significant difference between conditions suggests that the expectation of more critical attitudes from new superiors is based in large part on participants' general knowledge of the world, not purely on their specific (different) experiences in the experiment.

The more strongly Change subordinates agreed with this statement, the more they understated first-period costs, consistent with the argument that concerns about not receiving second-period funding from a new superior increased first-period understatement. Table 7 presents the results of the following model, which adds participants' responses to the post-experiment question, and its interaction with the experimental manipulation, to the hypothesis-testing model presented in Table 3:

$$Under1 = \alpha_0 + \alpha_1 Change + \alpha_2 Costinfo1 + \alpha_3 Round + \alpha_4 Second\text{-}period\ \ concern + \alpha_5 Second\text{-}period\ concern \times Change + \alpha_6 Inno + \alpha_7 Image + \alpha_8 Fairness + \varepsilon$$

(Standard errors are clustered as in previous analyses.) The marginally significant interaction coefficient ($\alpha_5 = -0.79$, $p = 0.10$, two-tailed) provides some evidence that more concern about a new decision maker's critical views is associated with more first-period understatement in Change, but not in Stay. (Because higher numbers on the response scale indicate *disagreement* with the statement, the negative interaction coefficient indicates that in Change, lack of concern about new decision makers is associated with less first-period understatement.)[26]

Our remaining supplementary tests examine consequences of subordinates' first-period understatements. We first check for effects on subordinates' *second*-period understatements—to verify, for example, that subordinates in Change do not compensate for their greater understatements in the first period by being unusually honest in the second period. Then we examine the effects of subordinates' understatements on superiors' success in choosing profitable projects.

We find no evidence that the tendency to greater first-period understatement in Change than in Stay reverses in the second period. Using only observations from individuals who receive funding in the first period, understatements are insignificantly greater in the second period (6.76) than in the first (6.37) ($t_{25} = 0.32$, $p = 0.75$) in the Change treatment. Mean understatements in the Stay treatment decrease in the second period (4.37 in the first period versus 3.04 in the second period) ($t_{29} = 2.06$; $p =$

[26] As would be expected when this mediating process variable is included in the model, the coefficient on *Change* itself is no longer significant ($p = 0.68$).

0.05). Thus, understatements in the second period are also significantly higher in the Change treatment than in the Stay treatment ($t_{26} = 4.34$, $p < 0.01$), and total understatement across the two periods is higher in Change than in Stay ($t_{26} = 4.19$, $p < 0.01$).

We then turn to effects of subordinates' first-period understatements on their superiors' decisions. Greater cost understatements by subordinates can have a variety of negative consequences for organizations; for example, expenses of raising additional funds to cover larger cost overruns, reduction of trust between subordinates and superiors, and suboptimal project choice by superiors. Our experiment is not designed to capture all the possible effects of subordinates' understatements, but it does enable us to examine superiors' success in choosing the more profitable of the two projects available to them in each period.

We estimate a logistic regression (untabulated) in which the independent variable is the experimental treatment, Change versus Stay, and the dependent variable is an indicator of whether the superior chose the *ex ante* optimal project (1) or not (0)—that is, the project that would have been chosen by an expected value maximizer who had the private information possessed by the subordinates. (Standard errors are clustered to allow for within-subject dependence across periods.) When we estimate this regression for superiors' first-period decisions, including data from all seven rounds, the coefficient on treatment is negative (−0.31)—that is, superiors are less likely to make optimal project choices in Change (Wald Chi-square = 1.60, $p = 0.10$, one-sided). If we examine only the last three rounds, after subordinates and superiors have had opportunities to learn and behavior has stabilized, then the effect is larger, in terms of both coefficient magnitude (−0.65) and significance (Wald Chi-square = 3.20, $p < 0.05$, one-sided).

Continuation funding decisions in the second period are influenced by two countervailing effects. The increased understatement by subordinates in Change compared to Stay should lower the relative quality of decisions in Change, and the lower potential for escalated commitment in Change should raise the relative quality of decisions. We estimate a logistic regression where the independent variable is the experimental treatment, Change versus Stay, and the dependent variable is an indicator of whether the superior in the second period chose the *ex ante* optimal project (1) or not (0)—that is, either continuing funding or choosing the alternative investment project that would have been chosen by an expected value maximizer who had the private information possessed by the subordinate. With data from all seven periods, the treatment effect is positive (0.11), but insignificant (Wald Chi-square = 0.15, $p = 0.70$). The effect becomes negative if we include only the last three periods, but remains insignificant (Wald Chi-square = 0.29, $p = 0.59$). These results are consistent with countervailing effects of worse decision making in Change due to more understatement in proposals and worse decision making in Stay due to more escalation.

These results indicate that a regime of changing superiors does not lead to better capital budgeting decisions in our setting. Indeed, the evidence is more consistent with their making worse initial (first-period) funding decisions. In continuation (second-period) decisions, even though changing superiors are more skeptical of subordinates' proposals than continuing superiors and are at no disadvantage in terms of private information about the proposals, the greater tendency of subordinates to understate their second-period costs to changing superiors makes it more difficult for the superiors to distinguish between more and less profitable projects and appears to negate any decision-making advantages that might accrue to changing superiors from their greater skepticism.

V. CONCLUSION

Our results provide an important caveat to the previous literature's observation that the practice of changing decision makers improves capital-budgeting performance by limiting the continuation of poorly performing projects (e.g., those with excessive cost overruns). In our experiment, as in prior literature, changing superiors are more skeptical than continuing superiors: they are less likely to be influenced by high second-period forecasts to continue funding underperforming projects. We document a countervailing effect, however. In our setting, large first-period cost overruns are also *more likely to occur* in the first place, because subordinates' concerns about the second period increase their first-period understatements. It is also the case (although this is not the primary focus of our study) that second-period understatements (and subsequent cost overruns) are more likely with changing superiors. When cost overruns have been high, changing superiors are less influenced by an additional unit of predicted second-period profit than are continuing superiors. Hence subordinates "need" to promise more units of higher profit (i.e., understate costs more) to changing superiors in order to achieve a given probability of receiving second-period funding.

Further research will be needed to identify the boundaries between settings in which the presence of critical changing superiors *increases* initial cost understatement, as in our experiment, and settings in which changing superiors might *decrease* initial cost understatement. At very low levels of uncertainty about superiors' decisions, presumably, the effect observed in our experiment would not occur. For example, if subordinates were nearly certain that continuing superiors would tolerate cost overruns of 15 percent without cutting off future funding, while changing superiors would always cut off funding after any cost overrun greater than 5 percent, then it is highly likely that cost overruns would be smaller under changing than continuing

superiors. How much higher subordinates' uncertainty would need to be in order to generate results like those in the experiment would depend on a variety of other factors, such as subordinates' risk aversion and aversion to misrepresentation, the magnitude of the expected difference between changing and continuing superiors' willingness to provide later-period funding to underperforming projects, the complexity of the inter-period trade-offs involved in subordinates' decisions, and their ability to cope with this complexity. Subordinates' uncertainty about their superiors' capital budgeting decisions is higher when there is a larger subjective component to these decisions; hence, organizations in which subjectivity plays a larger role in funding continuations are more at risk of a negative side effect from changing superiors.[27]

Some of the simplifications employed in our experiment create potential limitations to generalizability and opportunities for future research. For example, subordinates in our experiment cannot conceal first-period cost overruns; at the end of the first period, subordinate and superior are equally well informed about actual costs. In natural environments, some information asymmetry between subordinate and superior is likely to remain after the first period, although it is also likely that the asymmetry is reduced by first-period experience. The greater the extent to which first-period cost overruns can be concealed, the less relevant such reports of realized first-period costs are to decisions about later-period funding. A setting in which these first-period cost reports had little credibility might display less difference between Change and Stay conditions than our experimental setting.

Another important simplification is that, in our experiment, in order to provide a clean test of forecasting reputation effects, the realized costs of one period provide no information about other periods, *except* insofar as they provide information about the credibility of subordinates. A large first-period cost overrun in our setting, for example, cannot result from exogenous factors that will continue in the second period, such as unexpectedly high input prices. In settings where first-period cost overruns do indicate that second-period costs are also likely to be high, the first-period overruns might prompt more skepticism about rosy second-period proposals even among continuing superiors—or might not, if these superiors are subject to significant escalation bias. Investigating the effects of such variations in the setting is a task for future research.

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[27] For evidence of the importance of subjectivity in capital budgeting decisions in large firms, see Block (2005) and Alkaraan and Northcott (2006).

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