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# Cooperation and competition among employees: Experimental evidence on the role of management control systems

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### ABSTRACT

This article reviews experimental studies that investigate the influence of management control systems on competitive and cooperative interactions among employees. It begins by describing the role of experiments in improving theory specification, by improving construct definitions, documenting the causal processes that link management controls and performance, and identifying contextual factors that influence these processes. The article then analyzes experimental research on the role of management control systems in the social comparisons and tournament incentives that generate competition in organizations, and in the teamwork and reciprocity processes that support cooperation. A number of open questions and directions for future research, both experimental and non-experimental, are identified.

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### 1. Introduction

Much accounting research on management control systems (MCS)<sup>1</sup> has focused on hierarchical relations between superiors and subordinates or principals and agents; but managing horizontal relations of cooperation and competition among employees within an organization is also an important role for MCS. Researchers and managers are interested in questions like the following: How can internal competition be used as a motivating device without turning employees against each other (or against upper-level management)? Can MCS influence employees to cooperate more effectively with each other, other than by costly, detailed monitoring and enforcement?

Experimental research in recent decades has provided considerable insight into the complex processes by which MCS manage horizontal cooperation and competition and thus influence organizational performance. The goal of this article is to analyze what we have learned on this topic from the experimental literature in management accounting (MA), in a way that could be valuable for non-experimental MA researchers as well as experimentalists.

The experimental and non-experimental literatures in MA have become increasingly interconnected in recent years.<sup>2</sup> MCS experiments rely on formal principal-agent models (e.g., Evans et al., 2001; Towry, 2003; Rankin, 2004), while formal principal-agent models incorporate insights from experiments (e.g., Mittendorf, 2008; Fischer and Huddart, 2008). Experiments explore the individual and small-group processes that can explain organizational-level observations in archival and survey studies or widespread anecdotal evidence in the business press (e.g., Drake and Haka, 2008; Chen et al., 2012; Choi, 2014), while archival and survey studies use field settings to test the robustness of experimental results (e.g., Ittner et al., 2003; Chen and Sandino, 2012; Berger et al., 2013). Thus interconnections among these different research approaches have prompted significant research in recent years, but much unrealized potential remains.

The cooperation-competition theme offers opportunities for realizing more of this potential because it has generated a critical mass of high-quality experimental studies in accounting, and this critical mass reassures us about the robustness of the results in multiple ways. First, the existence of replications assures us that the results of single studies are robust in the laboratory.<sup>3</sup> Second,

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<sup>2</sup> This paper follows Chenhall (2003) in defining management accounting (MA) as “a collection of practices such as budgeting or product costing”, MA systems as “the systematic use of MA to achieve some goal”, and management control as “a broader term that encompasses MAS and also includes other controls such as personal or clan controls”.

<sup>2</sup> For empirical research, this is primarily true of quantitative, hypothesis-testing studies; connections with qualitative field studies remain relatively underdeveloped.

<sup>3</sup> Although replications are rare in the sense of new studies that do *nothing but* repeat a prior study, they appear fairly frequently in the experimental literature in the sense of new studies that replicate prior results incidentally, in the course of identifying limits to or influences on the original study's results. Quasi-replications

the basic phenomena addressed in the experiments—though not all their MCS implications—have been documented in natural environments (see the following sections for detailed evidence), increasing our confidence that the experimental results are not mere laboratory artifacts. Third, the critical mass means that there has been some (not complete) exploration of the boundaries within which specified effects are likely to occur.

The remainder of this article is organized as follows. Section 2 comments on what experiments can (and cannot) contribute to MCS research, with a view both to addressing some of non-experimentalists' concerns about the limitations of experimental research, and to offering suggestions for experimentalists about the kind of MCS questions that can (and cannot) best be answered with experimental methods. Section 2 also defines the scope of the particular experimental studies chosen for more detailed analysis in the remainder of the article. Sections 3 and 4 present in more detail what we have learned from experiments about performance effects of intra-organizational competition and cooperation; these sections also suggest questions for future research. Section 5 concludes.

## 2. Contributions and scope of experiments

What can experiments do to contribute to our understanding of the relations between MCS and organizational performance? Non-experimenters must ask this question when deciding when—if at all—to consider experimental studies as relevant to their own research.

Experimenters must also ask the question, first in order to determine whether a laboratory experiment is an appropriate way to address a particular research question, and then to decide what claims they can make about the generalizability of an experimental study.

Comparisons of experimental and archival or survey methods<sup>4</sup> are sometimes clouded by an assumption that these methods are primarily to be seen as (imperfect) substitutes for each other. I argue here that the more important role for experiments is to complement archival and survey studies: each method can provide an essential contribution that research using the other method often needs but cannot provide. An argument for the complementarity of the methods has three parts. First, both experiments and “real-world” empirical studies of MCS face serious questions about generalizability. Second, a primary reason that these questions remain unanswered is inadequately specified theory. Third, experiments and archival or survey studies provide different contributions to improving the specification of theory; and each approach can enhance the power of the other. These three steps in the argument are presented in more detail below.

### 2.1. Generalizability concerns

Questions about the generalizability of laboratory experiments to “real-world” settings arise because these experiments often use participants, tasks, or settings that differ in some respects from their counterparts in natural environments. Comparable concerns arise about the generalizability of archival and survey research,

that test identical theory in different settings are also fairly frequent. Examples appear in Sections 3 and 4 below.

<sup>4</sup> Archival and survey studies rather than qualitative field studies are used as the “real world” counterparts to compare with experiments in this section. Qualitative field studies are no less relevant to our understanding of MCS; but they are more difficult to compare to experiments because their theory base and methodological assumptions are often more distant from experiments than the theory base and methodological assumptions of quantitative hypothesis-testing studies using archival or survey data.

because studies conducted within a single organization or industry or region often cannot provide much assurance that their results will generalize to other organizations, industries, or regions. Studies based on more diverse samples aim at more assured generalizability but have often produced inconsistent—that is, non-generalizing—results (e.g., Shields and Shields, 1998; Otley and Fakiolas, 2000; Van der Stede et al., 2005).

It is reasonable to expect some degree of generalizability in MCS research, unless we believe that every instance of MCS and its effects is wholly unique and uninformative about other instances. Patterns of comparable behavior can be expected to exist; but it is often far from straightforward to identify exactly what constitutes a generalizable pattern of behavior or to identify the contextual factors that make the occurrence—and non-occurrence—of the pattern more likely.

### 2.2. Inadequately specified theory

When we are concerned about the generalizability of a laboratory study or a single-firm archival study, simply testing the same hypotheses over and over in different settings is not an efficient way of resolving the concern. Multiple instances of an  $x$ – $y$  association do not tell us very much about generalizability unless we are reasonably assured that (a)  $x$  and  $y$  are sufficiently well defined that other instances that fit the definition will actually be similar enough to behave in the same way; (b) there is a causal process explaining the  $x$ – $y$  association: it is not a spurious correlation created by some additional factor that may well not be present in other instances; and (c) the causal process linking  $x$  and  $y$  is not dependent on the presence of some contextual factor that may fail to appear in other instances.

A well-specified theory provides the definitions of  $x$  and  $y$  that help us to determine whether different instances are sufficiently similar in ways that matter to the  $x$  –  $y$  relation. It also describes relevant causal processes—“plausible accounts of how the actions of real humans could produce the associations predicted and observed” (DiMaggio, 1995, 392). These accounts can guide the search for evidence to distinguish between causal and spurious correlations. Finally, a well-specified theory includes “careful attention to the scope conditions of [its] account” of how  $x$  relates to  $y$  (DiMaggio, 1995; 391): that is, it identifies contextual variables that are more or less likely to matter to the relation and thus helps to define the limits of generalizability.<sup>5</sup> In contrast, it is difficult to claim or evaluate generalizability when the theory underlying a study's predictions is inadequately specified—that is, when we are uncertain about the definition of constructs, the nature of the relevant causal processes, or the identity of important conditioning factors.

### 2.3. Toward improved theory specification

Experimental and archival or survey studies can complement each other in improving the specification of theories that link MCS and organizational performance. Specification is improved through testing theories, modifying theories as needed by (for example) checking their logic, extending (or shrinking) their domain, or adjusting their construct definitions, and then testing the modified theory. In this process we might say that archival

<sup>5</sup> The quote is from DiMaggio (1995)'s definition of “theory as narrative”. Alternative definitions identified by DiMaggio are “theory as covering law,” which specifies empirical regularities without attempting to explain why they occur, and “theory as enlightenment,” a more humanities-oriented approach that does not aim at generalization. The “theory as narrative” approach plays a particularly large role in social science of the kind that is represented in quantitative, hypothesis-testing MCS research (see also Sutton and Staw, 1995).

or survey tests provide the “proof of the pudding” with respect to MCS—organizational performance relations, while experiments provide analyses of the pudding contents that help us to understand why the “proofs” come out well or badly.

Archival and survey studies provide important large-sample evidence that associations actually exist (or do not exist) between MCS and organizational performance, but they often are limited in their ability to provide evidence of how and why the associations occur (or fail to occur). We can infer, from an observed association between MCS and performance, that a particular set of management controls prompted employees to choose more productive actions because they were better informed or better motivated. But this is only an inference: archival and survey studies often do not allow us to directly observe these individual action choices and the reasons for them. Moreover, because of natural confounds in real-world data, it is often difficult in archival and survey studies to be sure which of the characteristics that are often (not always) bundled together in MCS or in contextual factors is really driving the observed results. Thus when there is a real-world association between control system or contextual factor  $x$  and organizational performance, we cannot always tell exactly what it is about factor  $x$  that creates the results. (Not all “balanced scorecard usage” or every “highly competitive environment” is alike in the respects that matter for particular effects, for example.)

Thus archival and survey studies are often not efficient tools for checking and improving the causal-story and construct-definition components of relevant theory. As the next sections will illustrate, experiments can provide more direct evidence of causal processes and can unbundle loosely defined constructs to tease out the specific characteristics of MCS and of contextual factors that account for their effects. Better construct definitions can support generalizability in archival and survey research because they assist in judging whether other instances of the “same” MCS or “same” type of environment should be expected to yield similar results. Better understanding of causal processes can also support generalizability, because the better we understand the causal processes underlying  $x$ – $y$  relations, the more readily we can identify contextual factors that support or impede these processes, and the more efficiently we can search for difficult-to-find evidence that an observed association is not a spurious correlation.

Conversely, archival and survey studies support other kinds of improvements in theory specification that are not well supported by experiments. For example, experiments are not as likely as archival and survey studies to provide some of the surprises that alert us to the need for better theory specification. When a well conducted real-world study provides a result that differs from its theory-based predictions, this can alert us to shortcomings in the relevant theory: perhaps, for example, it has failed to include an important variable. Laboratory experiments can help by providing clean tests of proposed modifications to the theory; but they are less likely to discover the need for modifications in the first place, because they are likely to eliminate supposedly irrelevant variables from the experimental setting.

Moreover, experiments often provide evidence only on specific portions of the causal processes that link MCS and performance, not on the whole process from beginning to end. Survey and archival studies, in contrast, can connect the beginning (MCS) and the end (organizational performance), albeit sometimes without a clear view of some portions of the middle. Experiments are best at providing evidence about actions by individuals and small groups, and often tell us less about how individual and small-group actions aggregate into large-scale organizational effects; this latter part of the process is often not easily addressed in the laboratory. For example, an experiment can show how a particular control increases a particular kind of cooperation among peers. But even if this result replicates perfectly in natural environments, experi-

menters need to be cautious about claiming *general* implications for organizational performance. The particular kind of cooperation demonstrated can be important for overall performance in some types of organizations and not very important in others.

Thus experiments do not substitute for archival and survey studies in documenting the overall MCS – organizational performance link, and they typically should not attempt to do so. Testing a black-box relation between MCS and performance, without pinning down at least some part of the mediating causal process, is unlikely to be a useful experimental project. If the experiment is not designed to provide some considerable confidence about *how* a difference in  $x$  brings about a difference in  $y$ , then it is unlikely to provide adequate confidence that its results are not simply a laboratory artifact. However, if the experiment—like many of the experiments analyzed in the following sections—does provide some confidence about the specific characteristics of MCS that bring about particular (relevant) individual actions in particular (relevant) settings, then it helps to build the well-specified theory that enables more generalizable research.

#### 2.4. Selection of experiments to be reviewed

The following sections examine experiments that meet two criteria. First, they address questions about the effects of MCS in settings in which outcomes (measured performance and/or pay-offs) for one employee depend on the actions of peer employees. For example, under a competitive bonus system, an employee’s bonus depends not only on her own skill and effort (and the realization of relevant uncertainties) but also on the skills and efforts of the peers who are competing against her; or in a teamwork setting, the influence of one employee’s actions on organizational performance depend crucially on whether his actions are supported or undermined by other employees.

The second criterion is that the experiments appear in the last twenty-five years of the four major international journals that typically publish experiments (*Accounting, Organizations and Society*; *The Accounting Review*; *Contemporary Accounting Research*; and the *Journal of Accounting Research*) or the two principal specialty journals in management accounting—*Journal of Management Accounting Research* and *Management Accounting Research*.

The intent of this review is not to cover in detail every experiment that meets the criteria but to identify major themes in which clusters of studies have provided related results, and to discuss what we have—and have not—learned from these clusters of studies. Thus for some studies that are included in the present article, only portions of the study are analyzed, because only portions of the study are relevant to the themes under discussion.

### 3. Competition, accounting, and management control

Because MCS are typically intended to foster cooperative behavior in organizations, it can seem—and sometimes is—counterproductive for MCS to prompt members of the organization to compete against each other. MCS nevertheless do prompt intraorganizational competition, for two reasons. First, competition is sometimes an unavoidable side-effect of controls that have other purposes. For example, sharing performance information among business units may be intended to improve performance by enabling managers to imitate more successful units and avoid the mistakes of less successful units. But shared performance information can also prompt competition among units, even in the absence of relative-performance-based incentives, due to social comparison effects described in Section 3.1. As the experiments indicate, this competition can create additional benefits for the organization or additional costs, depending on contextual factors.

Second, explicitly competitive monetary incentives such as tournaments<sup>6</sup> are sometimes used to solve problems with the incentive systems that are intended to encourage productive behavior. For example, when the level of common uncertainty is high, tournaments can significantly reduce the risk of incentive compensation by filtering out this uncertainty and thus enabling the organization to provide incentives without imposing excessive risk on employees. Similarly, when performance targets are difficult to set accurately and inaccurate targets are likely to reduce motivation, tournament incentives increase efficiency by eliminating the need for target-setting (Lazear and Rosen, 1981; Connelly et al., 2014). As the experiments presented in Section 3.2 illustrate, tournaments can be more or less effective in supporting organizational performance, depending on other MCS components and on context.

### 3.1. Social comparison effects and dissemination of performance information

Social comparison theories in psychology argue that people have an innate drive to compare themselves with others and are averse to “looking bad” in these comparisons, even when there are no consequences beyond the individuals’ private awareness of the comparisons (Festinger, 1954; Suls and Wheeler, 2000). Thus, the desire to look better—or at least not worse—than others motivates additional costly action (e.g., effort, expenditure of scarce resources, or cheating), even when looking better has no tangible consequences.

A number of experimental MCS studies have documented that even when relative performance is not used as a basis for monetary compensation, the desire to compare favorably with others can be a powerful motivator (Frederickson, 1992; Tafkov, 2013; Hannan et al., 2013a; Brown et al., 2014). Because MCS can prompt and guide social comparisons via the dissemination of relative performance information (RPI)<sup>7</sup> as well as through other control choices (see below for examples), social comparisons can play an important role (sometimes positive, sometimes negative) in the effects of MCS on performance.

What factors influence the sign and magnitude of social comparison effects? First (perhaps unsurprisingly) public RPI, in which the performance ranking of each individual in a group is provided to all group members, has more powerful effects than private RPI, in which individual knows his or her own rank only (Tafkov, 2013; Hannan et al., 2013a).

Second, social comparison effects are stronger for performance that is signaled as valuable by other components of the MCS. For example, in Tafkov (2013), participants are more strongly motivated by RPI when their compensation is dependent on individual (not relative) performance than when their compensation is a flat wage. They interpret measured performance as a more important signal of their ability when compensation depends on it, and the resulting increase in the psychological value of high performance magnifies the effect of the RPI.

Third, the sign of social comparison effects—whether they prompt actions beneficial or detrimental to organizational performance—depends on a combination of task, setting, and MCS factors. For example, when the only way to increase comparative

performance (as measured) is to choose actions that benefit the organization, prompting social comparisons is unproblematic. But if the task, setting, and performance measurement system are such that individuals can improve their relative performance by misreporting, misallocating effort across activities or sabotaging the performance of others, then the dissemination of information that prompts social comparisons is likely to be problematic. For example, Hannan et al. (2013a) predict and find that in a multi-task setting, public RPI has both positive effects on performance through its encouragement of additional effort and negative effects through its prompting of effort misallocations across multiple tasks. Individuals allocate more effort to the task on which they have higher abilities and thus a better chance of being seen as a “star”, even though this is not the best allocation for the organization or for their own compensation (which, in the experiment, is designed to provide the highest payoffs in return for organizationally optimal effort allocations).

Fourth, when there are significant opportunities to “look good” via organizationally detrimental activities, additional components of the control system can limit these activities. In Hannan et al. (2013), a simple action control that limits opportunities to misallocate effort eliminates the negative effects of public RPI while leaving the positive effects largely intact.

In Brown et al.’s (2014) budget-negotiation setting, subordinates can take advantage of their private information to create slack, which decreases business-unit profits but increases their own compensation (which is dependent on their own budget but not on RPI). Compared to a baseline condition without meaningful RPI, participants create *more* slack when the control system provides them with (private) information about how their compensation ranks relative to others, because they are reluctant to look like poor performers, even only to themselves. When rankings of business-unit profits are added to the compensation rankings, however, participants create *less* slack than in the baseline condition. Brown et al. (2014) suggest that when both rankings are provided, the antisocial nature of budget slack creation—the fact that slack reduces profit for the business *in order to* increase one individual’s compensation—is more salient. Thus the interpretation of high compensation shifts away from connoting success and toward connoting greed, making slack creation less attractive.<sup>8</sup>

Taken together, the studies in this subsection suggest a variety of factors that can be bundled with RPI dissemination in order to keep its effects on organizational performance positive. High-quality performance measurement or strong organizational cultures can prevent the undesirable actions (e.g., dishonesty, effort misallocation) that RPI might otherwise motivate. These prevention mechanisms may not be feasible, however, in settings with (for example) particularly difficult measurement problems, intractably high levels of uncertainty, or lack of consensus about organizational values. In such settings, other controls like the action control in Hannan et al. (2013a) or the interpretation-shifting additional information in Brown et al. (2014) might make the effects of RPI more positive. To the extent that options like these are also not feasible (for example, when lack of observability precludes action controls), organizations are more likely to need to limit social comparisons via information restrictions and/or organizational cultures that de-emphasize competitive comparisons.

<sup>6</sup> A tournament is a specific kind of competitive incentive in which only the rank order of performance matters to payoffs: the top performer gets the top prize regardless of the absolute level of his or her performance. In other kinds of competitive incentives, rank order is not the only determinant of payoffs: for example, the top performer’s payoff may also depend on the absolute level of his or her performance and/or the amount by which it exceeds others’ performance.

<sup>7</sup> Individuals can also gather information for comparison purposes outside the formal MCS, of course.

<sup>8</sup> Results in Brown et al. (2014) are similar when only the profit rankings are provided and when both profit and compensation rankings are provided. This contrasts with other studies in which the focus on self tends to dominate when conflicting (self-oriented and social-oriented) cues are provided, resulting in selfish behavior (Rowe, 2004; Bloomfield and Tayler, 2011). It is possible that in these earlier studies the social-oriented cue simply conflicts with the self-oriented cue (and loses in the conflict), rather than giving the self-oriented cue a different meaning.

### 3.2. Tournament incentives and the problem of losers' behavior

The experiments discussed in the previous subsection focus on settings in which relative performance is not a basis for compensation. Competitive monetary incentives, including the tournaments that are focus of this section, are common in organizations, however. For example, promotions are tournaments; and some bonus systems are tournaments, providing bonuses only to employees who rank in the top X% on specified performance measures. Similarly, when an organization's financial resources are limited, competition for these resources is a tournament in which even valuable projects may fail to win funding if other projects appear more valuable.

Because the RPI provided in the social comparison studies in the previous section is typically rank-order information, the social comparisons are much like a rank-order tournament for the non-monetary payoff of "being number one" (or at least not being "number N"). Thus the effects documented in social-comparison studies are likely to be relevant also to tournaments with monetary payoffs; but such tournaments also raise an additional set of issues that have been salient in the economics literature on tournaments (Dechenaux et al., 2015).

First, this literature has stressed the importance of prize structure (the number of tournament prizes and the distance in value between high and low prizes) in influencing behavior under tournaments.<sup>9</sup> Second, this literature has pointed out that tournaments pose more cognitively difficult decision problems than incentives based only on individuals' own (non-relative) performance (Bull et al., 1987).<sup>10</sup> Thus tournaments are likely to create more confusion and stress and greater variance of action choices than non-competitive incentives. In consequence, when tournaments are used an important objective of MCS may be to limit this confusion and stress as well as to motivate additional effort.

In addressing this set of issues, MCS experiments have highlighted the importance of the behavior of tournament losers in influencing overall organizational performance. Tournament competitors often differ in abilities and other resources, in the costs they incur for performance-improving actions, and/or in the favorability of their environments. Competitors who are disadvantaged in one or more of these respects and therefore believe they are likely to lose may simply give up and therefore perform particularly badly; or they may compensate for their disadvantages by cheating (e.g., sabotage, misreporting) if the situation allows it. Even if they do not give up or cheat, the stress of expecting to be a loser can reduce their performance in cognitively difficult tasks (see below for evidence). Such behavior can drag down overall organizational performance, especially when the number of tournament prizes is small relative to the number of competitors, thus producing numerous probable losers.

Four main observations about the effectiveness of tournaments can be drawn from experiments in the MA literature. First,

although individuals who are far enough behind in a competition do sometimes give up, this reaction appears to be less likely than simple rational-maximizing models would predict. The classic experimental-economic study by Bull et al. (1987) finds that disadvantaged competitors choose higher than equilibrium levels of effort.

In tournament experiments by Hannan et al., 2008 and Newman and Tafkov (2014), competitors who receive interim feedback indicating that they are not likely to win the tournament do not reduce their effort, even though the feedback has lowered the expected value of these actions. Because tournaments can prompt above-equilibrium effort, they can provide more motivational "bang for the buck" than non-competitive incentives and thus can be an attractive form of incentive if their negative side effects are limited.

The second observation is that the errors in individual action choice that result from the cognitive difficulty of tournaments (that is, errors from the point of view of conventional utility maximization) may or may not be a problem for organizations, depending on the task environment. In task environments like Bull et al.'s (1987) experiment or the real-world sales contest in Casas-Arce and Martinez-Jerez (2009), where performance-increasing effort is fairly straightforward for employees to identify and their choice is primarily *how much* of this effort to exert, the typical individual error is a choice of above-equilibrium effort, which is likely to create value for the organization.

In contrast, if employees need to choose the *type* as well as the magnitude of their efforts, and if these choices are not simple, then tournaments are more problematic because losers' decision errors are not so organizationally benign. Hannan et al. (2008) and Newman and Tafkov (2014) compare the behavior of individuals who learn that they are low performers when working under either a tournament or a non-competitive (profit-sharing) incentive in a cognitively difficult task with many possible action choices.<sup>11</sup> In both studies, participants working under tournament incentives tend to choose less effective actions than their profit-sharing peers: they choose dominated strategies more frequently, their choices exhibit more random variance, and they are more likely to say in post-experiment questions that they were distracted from the task by thinking about their performance rank.

The third observation that can be drawn from this set of experiments is that changes in the MCS—either incentive-structure or information changes—can increase the value of tournaments by improving "loser" choices in task settings where these choices are likely to pose problems for organizational performance. In Newman and Tafkov (2014), the remedy consists of restructuring the tournament incentive, replacing the two-tier tournament (all participants receive a fixed payment and the top performer receives an additional prize) with a three-tier tournament in which all participants receive a somewhat larger fixed payment, the top performer receives a modest additional prize, and the bottom performer receives the worst payoff. The total prize money provided by the organization and the ex ante expected value of compensation to individuals is the same in both tournaments, but the three-tier prize structure provides better motivation and less stress and decision error for non-top performers. Even if they have little hope of gaining the top prize, the possibility of gaining the middle prize rather than the lowest provides continuing motivation; it also provides a reasonable likelihood of avoiding the stress-producing role of "absolute loser". In Hannan et al. (2008), the remedy for performance-reducing stress is a change in information that makes interim RPI feedback coarser: it gives individuals a general idea of

<sup>9</sup> Prize structure is more easily controlled with monetary incentives than with purely psychological effects of social comparisons. For example, *how much* less attractive it is to be ranked fifth rather than fourth in a group of five depends entirely on individual preferences in social-comparison studies but can be manipulated via the prize structure in monetary-payoff settings.

<sup>10</sup> To see why tournaments are more difficult, consider a stylized production-task example. Under a piece-rate incentive, individuals can decide whether to produce an additional piece by comparing their payoff for the additional item (as determined by the incentive system) and the costs they personally incur to produce the item. But under a tournament incentive, in which their rewards depend on producing more than their competitors do, individuals' estimates of their payoff for producing an additional piece depend on their predictions of how much competitors will choose to produce, and competitors' choices will in turn depend on the competitors' predictions of the focal individuals' action choices. This mutual dependence makes the production-quantity decision more cognitively complex.

<sup>11</sup> The task is taken from Sprinkle (2000) who documents the difficulty of the task: participants can learn how to perform better, but the progress of learning is slow.

whether they are performing well or not, but it does not so explicitly identify so many individuals as likely losers in the tournament.<sup>12</sup>

The fourth observation, following from the information manipulation in Hannan et al. (2008) is that information requirements for effective MCS—and thus, implications for accountants—can be quite different under tournament and non-competitive incentives. In Hannan et al. (2008), fine and coarse RPI have similar performance-improving effects under an individual profit-sharing plan, while coarse RPI supports significantly higher mean performance improvements than fine RPI under the tournament incentive.

A limitation of the studies described above is that they use, and therefore are relevant to, settings in which there is little scope for individuals to win the tournaments through illicit actions like dishonest reporting or sabotage of competitors. The effects of competitive incentives on organizational performance are likely to differ in some but not all respects when illicit actions are more likely, however. Brügger and Luft (2011) provide an example in a study of misrepresentation in capital budgeting proposals.

As in previously described tournament studies, the prize structure (number of funding opportunities per group of competitors) makes a significant difference to the results—in this case, the magnitude of misrepresentation—in Brügger and Luft's (2011) experiment, and the behavior of likely losers drives these results. However, the nature of likely losers' behavior in this study differs from the other tournament studies, in that "giving up" is a more likely response to high likelihoods of losing in the capital budgeting setting. It seems likely that the difference between the settings is due to the fact that "giving up" in the other tournaments means withdrawing effort, which has negative social/ethical connotations. In the capital-budgeting competition, in contrast, "giving up" means not misrepresenting—it means giving up the attempt to make a mediocre project look like an excellent project—and the positive connotations of this behavior make giving up more attractive.

Another budget-misrepresentation study (Fisher et al., 2002b) underlines the importance of the exact combination of multiple controls for determining the magnitude of illicit actions in competitive settings. In Brügger and Luft's (2011) setting, as in many long-term capital project settings, the proposed project budget is not a basis for managers' later rewards (although the actual project outcomes are). Hence there is an incentive for managers to overstate the future profits of their projects in order to gain funding. In Fisher et al. (2002b), the use of budgets for performance evaluation (as is common in operating budgets) provides a countervailing incentive to understate profits. This effect of countervailing incentives from multiple budget uses is so powerful in the experiment that it leaves no room for an incremental effect of an otherwise useful misrepresentation-reducing device, the dissemination of information about peers' budgets.<sup>13</sup>

### 3.3. Competition and MCS: experiments, natural environments, and open questions

This section addresses three related questions that can arise from the presentation of the experimental literature in the two previous sections. First, what are the grounds for expecting that

<sup>12</sup> Specifically, the coarse interim feedback tells participants whether they are currently in the top or bottom half of the performance distribution, while the fine feedback gives them their performance decile. Tournament prizes are awarded to individuals who are in the top decile at the end of the task.

<sup>13</sup> Other tournament experiments involving illicit or at least questionable behavior (Drake et al., 1999; Hannan et al., 2013b) involve additional issues of how MCS supports intra-organizational cooperation. These studies are therefore included in the analysis of cooperative behavior in Section 4.

social-comparison and tournament effects like those documented in laboratory experiments are nontrivial in natural environments? Second, what (if any) implications does the experimental literature have for archival or survey studies of MCS? Third, what (if any) implications do archival or survey studies of MCS have for experimental research on social comparisons and tournaments?

A number of studies have documented significant social-comparison effects outside the laboratory, in the absence of monetary payoffs for relative performance. For example, social comparisons have been shown to affect costly energy use in households (Schultz et al., 2007), donations to charity (Shang and Croson, 2006), and job attitudes and turnover intentions in a large sample of employed adults (Brown et al., 2007).<sup>14</sup> Similarly, excess tournament effort has been documented in the field: Casas-Arce and Martinez-Jerez (2009), using archival evidence from sales contests at a commodity company, find that interim feedback indicating to individuals that they are not likely to win often increases rather than decreases their efforts—without, however, making it likely that they will win. Negative effects of tournaments, such as ineffective effort by individuals whose performance falls behind competitors' performance, also appears in natural environments (Berger et al., 2013; Grund et al., 2013; Kolev et al., 2015).<sup>15</sup>

These studies document basic social-comparison and tournament phenomena, but most are not primarily MCS-focused, and thus they provide only limited information on how MCS components influence these basic phenomena. Archival and survey literature in the management accounting domain, which is more likely to take an interest in MCS issues, has thus far given only limited attention to intra-organizational competition. While the content of MA information (e.g., financial, nonfinancial, customer and external competitor information) and the basis of performance pay (individual vs. group or budget-based vs. not budget-based) are common variables in this literature, competition-related variables like the breadth of dissemination of information that prompts social comparisons, or the structure of competitive incentives (e.g., relative numbers of competitors and prizes in tournaments) appear much less frequently.<sup>16</sup>

Internal dissemination of RPI (separate from the choice of relative-performance-based incentives) does vary across organizations, however, and appears to matter: some organizations treat performance information as highly confidential, while others disseminate it more widely within the organization, for example in the interests of publicizing "best practice".<sup>17</sup> There is also considerable variation across organizations in the presence and structure of tournament incentives. Thus one implication of experimental research in this area is that there is considerable scope for more archival and survey investigation of the role of intra-organizational competition and of how MCS and contextual features can leverage the positive and mitigate the negative effects of such competition.

Experimental studies, in turn, could benefit from a more explicit consideration of some of the contextual variables that appear in archival and survey research but are less often manipulated in

<sup>14</sup> Here and elsewhere in this paper, references to studies in natural environments are samples rather than exhaustive lists.

<sup>15</sup> Theoretical and empirical work in behavioral economics suggests that not only tournaments but also other small-numbers competitive settings are likely to pose challenging decision problems. (Camerer et al., 2004; Brown et al., 2012; Crawford et al., 2013).

<sup>16</sup> The lists of MCS variables widely used in the literature that Chenhall (2003) and Bedford and Malmi (2015) provide do not include variables related to the dissemination of RPI. A search on "tournament" in the last ten years of six accounting journals (AOS, CAR, JAR, JMAR, MAR, and TAR) yields only two empirical, non-experimental studies that address tournament issues.

<sup>17</sup> See Mahlendorf et al. (2014) for survey evidence of the positive effects of RPI dissemination of this kind, and of the dependence of these effects on employees' social identity.

experiments, such as task and environment characteristics. For example, as noted above, the negative performance effects of fine information in Hannan et al. (2008) and the two-tier tournament in Newman and Tafkov (2014) are due to the ineffective (dominated or random) action choices made by individuals who are unlikely to win the tournament. Studies in natural environments indicate that such ineffective action choices are not confined to the laboratory; but it seems possible that they are dependent on contextual variables in ways that have not yet been adequately explored. In routine tasks or stable environments, in which people *know* what they have to do in order to improve performance, poor action choices of this kind are less likely and the performance effects of fine information and the two-tier tournament might well differ from those in recent experiments.<sup>18</sup>

Another contextual factor that matters to the performance effects of tournaments but has received only limited attention in MCS research (either experimental or otherwise) is the heterogeneity of the contestants in terms of abilities and other resources that can help them win. Some organizations are more homogeneous than others (a characteristic that can be related to the typical contingency-theory variables of diversification, differentiation, and size); and within a relatively heterogeneous organization, employees or business units can be allocated to more or less homogeneous subgroups for purposes of internal competition. More heterogeneity tends to reduce individual and aggregate effort in contests through two paths (Dechenaux et al., 2015). First, although people do not “give up” in tournaments and other contests as often as conventional economic theories predict, a large enough disadvantage will be sufficient to discourage them, thus reducing effort from weak players (see Schotter and Weigelt, 1992; Casas-Arce and Martinez-Jerez, 2009 for evidence from the laboratory and natural environments, respectively). Second, individuals who are already in very strong positions know they do not need to work very hard to win against much weaker players, and therefore their performance is lower than it would be if they faced more nearly equal contestants.

Homogeneity can also matter to social-comparison effects, because individuals tend to compare themselves more intensively with others, and therefore to be more motivated by social comparisons, when they regard the others as more similar to themselves. Frederickson (1992) provides an interesting example in which, holding the *level* of uncertainty common, the presence of more common *sources* of uncertainty among participants results in higher effort under RPE contracts because the greater similarity increases the saliency of “rivalrous comparisons”. Subsequent experiments have tended to examine groups of individuals that are *ex ante* likely to compare themselves to each other; and thus these experiments bypass the important question of who does and does not compare themselves to whom in an organization, and why. In large and diverse organizations, employees do not all compare their performance, resources, etc. to all other employees. Factors like information and organizational structure might guide comparisons and thus guide competitive behavior, but as yet we know little about these processes.

A further set of questions that could usefully be addressed in both experiments and archival or survey studies combines competition issues with task and environmental characteristics. For simplicity, studies like Hannan et al. (2008) and Newman and Tafkov (2014) conceptualize organizational performance as the sum or mean of individual performances, but different relations

between individual and organizational performance can imply different effects of competition and different optimal MCS. For example, in high-risk settings, a single individual in a large group who either withdraws effort or makes poor action choices might change the *mean* action choice in the organization only trivially, but could nevertheless have significant organizational-performance effects—for example, in the extreme, by setting off a serious industrial accident or financial crisis. In such settings, powerful tournament incentives and difficult tasks with poorly understood input-output relations are likely to be a particularly hazardous “misfit”, because they reduce the mean quality and increase the variance of likely losers’ action choices.

In other settings, the action choices of *top* performers matter more to organizational outcomes than the action choices of the low performers on whom much of the MA experimental literature has focused. For example, in an innovation-dependent industry like pharmaceuticals or software, where research teams often compete for internal funding for projects, what matters to the organization is the quality of the projects that *win* funding and ultimately provide the production and marketing units of the organization with products that will succeed (or not) in the marketplace. In such settings, the quality of the projects that fail to win funding is not so important. These settings suggest that there is scope for research that investigates how competitive incentives and other controls can influence not just the level of *mean* performance but also the level of *top* performance in creative tasks.

#### 4. Cooperation: teamwork, collusion, and controls

In the experiments analyzed in the previous section, the nature of the interdependence among employees is relatively simple: the *payoffs* (monetary or psychological or both) that individuals receive for a given level of performance depend on their peers’ performance as well as their own, but the individual’s *performance* level typically does not depend on peers’ action. Possibilities for cooperation (or “negative cooperation” in the form of sabotage) can create performance interdependencies, in which individuals’ own performance varies depending on peers’ actions. These interdependencies can be further complicated when payoffs may depend not only on an individual’s own performance, but also on the difference between her own and her peers’ performance (competitive incentives), or the sum of her own and her peers’ performance (team incentives). The experiments analyzed below focus on three (sometimes related) components of MCS that influence cooperation among employees in these complex-interdependency settings: incentive compensation form, the provision of higher-quality or more widely disseminated information, and the promotion of trust and reciprocity among employees that can enable more effective cooperation when cooperative actions are not contractually enforceable.

##### 4.1. Teams, innovations, and information

Recent decades have seen an increased emphasis on the need for MCS to support innovation and/or creativity from employees (Chenhall and Moers, 2016), and an increased awareness of the value of teamwork, for example in creating innovations that increase efficiency across multiple segments of a supply-chain process. It may seem straightforward to predict that effective teamwork and the resulting profit will be higher with team-based incentives and better information about the relevant processes, compared to individual (especially competitive) incentives and/or poorer information; and often these predictions can

<sup>18</sup> This possibility is consistent with results in a non-competitive experiment by Webb et al. (2013), in which the pressure of challenging targets and target-based pay reduce the discovery of production efficiencies via increased stress, more mistakes, and slower discovery, but are successful in motivating more routine effort.

be supported.<sup>19</sup> Experiments suggest some important nuances to these expectations, however.

First, the negative side effect of intra-organizational competition in this setting is not merely that individuals fail to put better information to use when the resulting innovation might benefit a competitor. Rather, individuals *do* put the information to use, in order to actively resist or avoid innovations that benefit others more than themselves; and as a consequence, “better information” in some settings not merely fails to generate a gain in organizational performance but actually creates a loss. Drake et al. (1999) found that the provision of finer cost information to a supply-chain team—information that identifies specific sources of cost in the production and logistics process—actually decreases cooperative innovations and production efficiency relative to coarse information when participants work under an individual tournament incentive, though not when they work under a team incentive.

Second, the absence of explicitly competitive incentives does not altogether solve the problem of the misuse of finer information, because individuals may still be concerned about social comparisons and “unfair” divisions of the common surplus produced by team innovations. Drake and Haka (2008) examine a setting in which supply-chain partners are paid based on their own profits only and have potentially valuable private information about their own costs. When they can choose whether or not to share their private information as a basis for creating joint efficiency improvements, they are less likely to share fine than coarse information because of concerns about unequal surplus sharing. The reduced level of sharing, on average, cancels out the potential benefits of fine information in helping to identify valuable innovations.<sup>20</sup>

Third, whether competitive incentives have a negative effect on team performance depends on the exact nature of the team task. In the task used by Drake and colleagues, individuals can increase their own payoffs by making innovations that benefit only themselves and can withhold information and ideas that would generate innovations benefiting others as well (and thus creating larger benefits for the organization as a whole). Not all tasks have equal scope for this kind of passive sabotage by withholding ideas or information, however. In Chen et al. (2012) for example, small groups develop solutions for local problems; an independent panel then rates the creativity of both the individual inputs and the group solution. Individual contributions cannot be evaluated without being shared in the group, so individuals cannot readily improve their own measured performance by withholding ideas and information. Hence, although an intra-group tournament condition in the experiment offers motivation for lowering the performance of other group members, the limited opportunity for such actions in this setting means that the intra-group tournament results in no worse group performance than non-competitive bonuses based on either individual or group performance (piece rates).<sup>21</sup>

<sup>19</sup> For example, Rankin and Sayre (2000) find that, in a teamwork setting, effort is lowest when the compensation system is a tournament and the performance measurement system provides relatively little certainty about how much each individual has contributed to team performance. In contrast, performance is higher and is unaffected by the quality of the performance measurement system when incentive pay is based on team performance; outcomes in an individual (non-competitive) compensation setting lie between these extremes.

<sup>20</sup> Relationship-fairness concerns also appear in an experiment by Masschelein et al. (2012) which finds that the effect of more precise cost information in supporting efficiency improvements depends on which party is the source of an existing inefficiency and thus on the perceived fairness of arguments for changes in existing supply-chain arrangements.

<sup>21</sup> Chen et al. (2012) test only the comparison between individual tournament and individual piece-rate performance; but it is evident from the descriptive statistics that performance under the group piece-rate is not significantly superior to performance in the individual-incentive conditions.

Interestingly, an inter-group tournament results in significantly better group performance than the alternative incentive plans. Chen et al. (2012) argue, and provide detailed supporting evidence, that the incentive type alters intragroup work processes, because competition between groups increases intragroup cohesion and information sharing. Members of more cohesive groups are less apprehensive about sharing ideas, because they believe the members of their group are more open to their ideas, and they are less likely to free-ride because they are more committed to group success (and perhaps expect others to be so as well).

The kind of informal give-and-take that occurs within the groups in the studies described above—private information sharing, assistance in developing ideas, and other kinds of mutual support—plays an important role in organizations. Formal MCS can hardly specify all these mutually supportive actions through rules, measure them (or their results) precisely, and enforce them through rewards and penalties. Effective cooperation is often dependent on small-scale “chain reactions” of trust and reciprocity, in which one individual’s cooperative (or uncooperative) action prompts other similar actions in response. Although the MCS does not control these action chains in detail, it can provide some prompting and guidance for them, as the experiments in the next section illustrate.

#### 4.2. Trust, reciprocity, collusion, and MCS

Effective cooperation in the workplace often occurs when one individual contributes time, information, or other resources to assist another individual, trusting that the favor will be approximately returned at some future time. Norms of reciprocity often make it likely that the favor will in fact be returned, even when it is not in the economic interest of the recipient of the favor to return it. Reciprocity experiments in accounting often address employer–employee relations, but reciprocity can also play a role in the relations among employees that are the primary focus of this review. Basic findings about reciprocity from the employer–employee studies, which can be extended to inter-employee settings, include the following.

First, reciprocity can be either positive (returning helpful actions) or negative (returning harmful actions). Negative reciprocity has particularly powerful effects (Hannan, 2005; Kuang and Moser, 2009, 2011; Christ, 2013; Brown et al., 2015). Second, the sign and magnitude of reciprocity effects depend not only on observable and measurable features of others’ behavior, but on the focal individual’s *interpretation* of that behavior. For example, the imposition of a more restrictive control or a lower wage level by one party has different effects, depending on whether it is interpreted by the other party as a fairly reasonable act under the circumstances or an act of deliberate selfishness or arbitrary mistrust (Hannan, 2005; Kuang and Moser, 2009; Christ, 2013). Third, people are not always good at predicting others’ interpretations and responses, and thus they sometimes inadvertently choose actions that prompt strong negative reciprocity (Hannan, 2005; Hales and Williamson, 2010; Kuang and Moser, 2011; Brown et al., 2015).<sup>22</sup>

Experiments that address reciprocity among peers have incorporated these insights and stressed the role of the formal contracting system within MCS in creating a setting within which

<sup>22</sup> Factors that reduce positive and/or stimulate negative reciprocity among some substantial portion (but typically not all) of the participants in an experiment include penalty incentives rather than economically equivalent bonus incentives (Christ et al., 2012); subjective adjustments to (profit-based) compensation that are made via adjustments to the profit-sharing ratio rather than the target (Liu and Leitch, 2013); salient imposition of formal controls by a superior, compared to external or uncertain sources of the control (Christ, 2013); or unyielding positions in wage or budget negotiations that are not driven by external pressures (Fisher et al., 2000, 2002a, 2006; Kuang and Moser, 2011; Arnold, 2015).



unenforceable, trust- and reciprocity-based cooperation flourishes. Formal MCS can support such cooperation through the provision of additional information and/or through actions that signal and build trust. Importantly, either of these paths can lead to more pro-employer cooperation or more anti-employer collusion, depending on other components of the control system and setting.

Several themes occur repeatedly in recent experiments on this topic. First, reciprocity mechanisms are information-intensive and communication-intensive, because employees often need additional task knowledge in order to cooperate effectively. Rankin (2004) provides analytic and experimental evidence on the knowledge issue, showing that team incentives and cooperative goodwill do little to improve performance in low-information settings, where employees are unsure when it is more beneficial for an individual to set aside his own work to help other team members with their work, and when the individual would contribute most to overall team performance by focusing on his own job. Thus effective reciprocity in a team-incentive setting requires more information about others' jobs.<sup>23</sup>

Second, reciprocity mechanisms are also information-intensive and communication-intensive because communication of information influences trust levels in a variety of ways. One of the most straightforward ways is the provision of information that reduces uncertainty about whether other employees have chosen cooperative actions in the past. Such information reduces free riding in team projects in Kelly and Tan (2010) and increases trust- and reciprocity-based collusion among employees to conceal misrepresentations in cost reports in Evans et al. (2016). (Neither collusion nor productive cooperation is sustainable based on monetary payoffs alone in these experiments.)

Another way of influencing trust through information is illustrated in an experiment by Rowe (2004), in which alternative presentations of what is essentially the same information make common team payoffs more or less salient relative to individual payoffs. When teams work face-to-face, trust is higher and team performance is higher (free-riding is lower) when the team payoff is more salient. However, when team members work in separate locations and thus there is, so to speak, no human face on the common surplus, the accounting presentation has no effect.

In Rowe (2004), communication among team members is held constant at a minimal level across experimental conditions; but other experiments provide evidence that communication opportunities are important in generating or enhancing cooperation. In both Towry (2003) and Zhang (2008), increased communication among team members increases their tendency to collude against the employer when the formal incentive compensation system provides occasions for such collusion. (Employees cannot make binding commitments to collude, but they can intensify each other's dissatisfaction with the employer and promote collusion as a remedy.)

A third main theme in this literature is the role of formal MCS in signaling to employees about the cooperative or non-cooperative atmosphere of the workplace. Setting a relatively high wage even when high effort cannot be enforced is a signal of trust by the employer that can prompt positive reciprocal actions by employees in return (e.g., Hannan, 2005; Choi, 2014). Generous compensation can also affect reciprocity among employees. In Kelly and Tan (2010), cooperation among employees to reduce free-riding is higher when the contract the employer imposes includes more

generous surplus-sharing terms. (This is the case even though free riding remains the conventional-economic equilibrium, and even when no feedback about other employees' prior actions is provided: the trust signal provided by generous wages substitutes for the (imperfect) record of others' actions provided by a feedback system in other conditions.) In Zhang (2008), the choice of a more generous fixed wage by employers results in less collusion to conceal misreporting among employees.

Formal MCS can signal an overall atmosphere of trust in a variety of ways. For example, two alternative incentive compensation plans in Towry (2003), which have the same economic equilibrium, can be seen as differing with respect to the signals they send about trust within the organization. One ("horizontal") plan uses a team incentive and leaves it up to the team to manage potential free-rider problems as they see fit, while the other ("vertical") plan rewards employees for informing on each other to the employer. In these settings, stronger team identity (which can be encouraged or discouraged in a variety of ways by organizations) leads to more productive cooperation among team members under the horizontal incentive and more collusion against the employer under the vertical incentive.

Coletti et al. (2005) illustrate yet another way in which the formal MCS can prompt unenforceable cooperation. In their experiment, when a formal control can enforce a relatively high level of cooperation, employees tend to give their peers credit for trustworthiness (i.e., they do not correctly partial out how much of their peers' cooperative behavior is due to trustworthy character traits and how much is due to the control system). Hence—since a belief in the trustworthiness of others inspires trust—employees behave in a trusting and cooperative way, even beyond what the formal control system can enforce.

A number of the studies above indicate that the sign of inter-employee cooperation effects and the influence of MCS on them are highly context-dependent. Hannan, Towry, and Zhang (2013b) provide a particularly clear demonstration of this point. In their experiment individuals bring either a competitive or collusive approach to a tournament setting. (In principle, these initial approaches can be prompted by a variety of factors such as organizational culture or incentives.) When an initial competitive approach is prompted, participants compete more vigorously (choose more costly, performance-increasing effort) when they have information about their competitors' actions than when they do not. But when an initial collusive approach is prompted, participants collude more effectively (choose less costly effort) when they have such information. The information acts as an intensifier—it "turns up the volume"—of whatever behavior is prompted by other factors in the control system or environment.

#### 4.3. Cooperation and MCS: experiments, natural environments, and open questions

As with the competition effects examined in Section 3, the basic theory underlying the laboratory experiments described above has been supported by studies in natural environments. Mechanisms of reciprocity—for example, higher effort or greater honesty in return for higher fixed compensation—have been documented in a variety of field settings (e.g., Greenberg, 1990; Fehr and Gächter, 2000; Bellemare and Shearer, 2009; Chen and Sandino, 2012; Kube et al., 2013). Similarly, Bandiera et al. (2005) find collusion effects in a field setting that are reminiscent of the results of Towry's (2003) and Hannan et al.'s (2013b) laboratory experiments, in which collusion is higher when social identity among work-group members is high and information conditions are favorable.

The experimental studies analyzed above suggest two avenues for enriching archival and survey research on MCS and cooperation. First, archival and survey research could do more to investigate

<sup>23</sup> This evidence is consistent with the results of an experiment by Young et al. (1993), who find no net gain from opportunities to cooperate. Participants in their experiment apparently miscalculate the gains from mutual assistance and spend time trying to help each other that would be more productively spent on their own tasks.

the effects of MCS components, not just in supporting or failing to support productive behavior, but also in prompting or preventing actively destructive behavior such as the interdepartmental feuds (negative reciprocity) and negotiation impasses and sometimes subtle forms of sabotage that can undermine organizational performance.

For example, in the theory that underlies many real-world studies, the effects of “better” information are expected to be either positive or zero before considering the costs of system implementation. It is possible that one of the (several) difficulties in finding consistent results in these studies is their failure to take into account the possibility that under some conditions “better” information will have negative effects, reducing employee efforts, discouraging teamwork, and supporting collusion against organizational objectives.<sup>24</sup> The more accurately these settings can be characterized (in part through experimental research), the better the chances are for identifying real-world indicators for them and thus identifying settings in which the benefits of apparently superior incentives or information will be limited.

Second, experiments suggest interactions of formal and informal components of MCS that appear plausible but have not yet received extensive attention in archival and survey studies in the accounting literature. Elements of the formal MCS—independent of their value as contracting devices—can “grease the wheels” for uncontractible reciprocity mechanisms to work effectively; they also provide signals that prompt these mechanisms to work for or against the employers.

Archival and survey studies have called attention to changes in organizational structures—specifically, shifts in the extensiveness and strength of vertical and horizontal relations—which have implications for experimental research. Experimental studies have tended to ask different specific questions about cooperation in horizontal (e.g., team) and in vertical (subordinate-superior) relations. In principle, however, many questions could be asked about either type of relations, and it would be valuable to know if theories and findings in horizontal relations also apply to vertical relations and vice versa. Formal properties (e.g., decision rights, information, rewards) can differ between horizontal and vertical relations; and even when these are held constant, social relations differ. Thus questions arise: is negotiating with a peer (for example, in transfer pricing) subject to the same influences as negotiating with a superior (for example, in budgeting)? Similarly, are horizontal collusion (employees at the same level colluding against some higher level of the organization) and vertical collusion (employees at different levels within one organizational unit colluding against employees in a different unit or against the organization as a whole) equally robust, and are their causes and effects similar? Possibly not, because of differences in the social situation; and if not, then there are implications for organizational structuring—for example, for the likely effects of flatter versus steeper hierarchies, since the former is likely to have more peer relations and the latter more subordinate-superior relations.

## 5. Conclusion

Recent experimental studies of MCS have expanded our understanding of some of the ways in which MCS can influence

<sup>24</sup> In addition to the negative effects of information on cooperative effort and honesty documented by the literature presented in this section, experiments have also identified other organizational frictions resulting from the availability of information that is interpreted in different ways by different individuals within the organization. These frictions can reduce the efficiency of transfer pricing (Luft and Libby 1997; Kachelmeier and Towry, 2002; Chang et al., 2008) and subjective performance evaluation (Luft et al., 2016).

organizational performance by influencing peer relations among employees. Specific causal processes that create these influences, and contextual factors that block or intensify these processes are identified in recent experiments that address four main themes.

The first theme is that the distribution of performance information can play an important role in stimulating and guiding competitiveness among employees. Social comparisons can be a powerful force (either positive or negative) even without tangible incentives for relative performance, and it seems likely that choices about what performance information is distributed and to whom it is distributed can influence employees' behavior by influencing the terms on which, and the peers with whom, they compare themselves.

Second, the effect of adding tangible incentives for competition is crucially dependent on a combination of incentive design (e.g., tournament prize structure), information characteristics (e.g., coarse or fine), and task or control-system characteristics (e.g., opportunities for improving relative performance via productive actions vs. cheating). An important consideration in tournament effects is the different effects that a particular tournament type will have on the actions of likely winners and likely losers, and the consequences of these different actions for the organization.

Third, many components of MCS (e.g., information, incentives) are two-edged swords: they can prompt either constructive or destructive actions on the part of employees, depending on context and on other MCS components. Preventing or halting destructive chain reactions of negative reciprocity among employees or between employees and employers is an important problem for MCS to address, as well as the more traditional MCS objectives of motivating effort and reducing information asymmetry.

Finally, the formal, contractual components of MCS play an important role, not only in enforcing the behavior that they can contractually enforce, but also in influencing the behavior they cannot enforce. Formal components can set a tone or atmosphere (trusting or otherwise) that encourages or discourages informal reciprocity among employees and directs that reciprocity toward or against organizational goals.

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