



# Using experiments in corporate strategy research

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## Abstract

Research in corporate strategy has come a long way since its early days as a purely case-based investigation. Today, corporate strategy theory draws not only on ideas from many disciplines, but has also developed its own concepts to better describe and predict strategic behavior. Methodologically, however, modern corporate strategy research relies primarily on observational methods using large data sets, developed either from primary or archival sources. We argue for an increased use of experiments to address the current integrative and interdisciplinary theory in corporate strategy and to complement existing methods. We review the few experiments in corporate strategy research, and describe the advantages and limitations of the experimental method.

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## Introduction

Research in corporate strategy began with a case-based approach in the 1960s and 1970s (Chandler, 1962, 1977; Learned *et al.*, 1969; Allison, 1971; Bowman, 1974), but it has evolved significantly since then. As a response to the recommendation of two influential ‘foundation reports’ (Gordon and Howell, 1959; Pierson, 1959), management scholars turned to related disciplines such as economics, psychology, and sociology for increased academic rigor (Agarwal and Hoetker, 2007). As a result, corporate strategy is now an inherently interdisciplinary endeavor, and has benefited, particularly on the theoretical front from the application of relevant concepts from related disciplines on issues of how corporations make, implement and benefit from strategic decisions.

On the methodological front, however, current research relies chiefly on the use of observational (empirical) data, developed either from archival or primary sources, in spite of the fact that the development, testing, and expansion of the relevant theories in related disciplines often rely on a multitude of research methodologies. As an example, game theoretic models in economics are often tested using experimental methodologies (e.g., Kagel and Roth, 1994). While corporate strategy research has benefited significantly from game theoretic insights (e.g., Camerer, 1991; Saloner, 1991; Khanna *et al.*, 1998), the concomitant

experimental methodology has not made inroads in strategy research.

In this paper, we suggest that corporate strategy would benefit from an expansion of its current set of research methodologies. In particular, we discuss the advantages and limitations of experimental methods in addressing core strategy theories and as a complement to current observational approaches. We provide some illustrative examples of how a few studies have successfully implemented experimental methodology in corporate strategy research, and also call attention to additional areas that may benefit from its application. This article thus is aimed at corporate strategy researchers who might be unfamiliar with experimental methods, but are interested in expanding their tool kit of research methodologies.

We define an experiment as a research methodology which involves collecting primary data from individual decision-makers who face real payoffs from their responses. Experiments can involve either individual decision-making or interactive (game-theoretic) tasks. Most importantly, experiments involve multiple *treatments*, that is, some participants see one set of materials, and other participants see another set (in a between-subject design) or all participants see multiple materials (in a within-subject design). The behavior of the participants is compared between the

treatments. Experiments can be run in the lab, in the field or within a firm. However, survey research (either in the lab or in the field), while empirical, typically does not involve multiple treatments, and is thus not experimental.

We argue that a greater use of laboratory experiments can address problems and limitations encountered in other methodologies, and thus aid both theory development and its testing. We highlight advantages that this method can bring, while also examining its limitations and potential pitfalls. To illustrate the concepts we introduce, we review some examples of how this methodology has been used to evaluate and extend theories of behavior in strategy settings, and provide suggestions for other areas in which it might be constructively applied.

We believe that an increased adoption of experiments in corporate strategy research will bring specific and significant innovations and rewards. The challenges and opportunities facing corporate strategy researchers often lies in the integration and reconciliation of multiple theories from different disciplinary perspectives. The experimental methodology can be useful in distinguishing among competing theories, as it provides the ability for creative designs to unconfound their predictions. As discovered in fields like psychology and economics, experiments are also helpful in addressing theories directly; testing them and estimating their parameters. Finally, experiments can also be helpful for theory-building in corporate strategy, suggesting modifications to existing disciplinary-based theories and exploring new theoretical directions efficiently. As a result, we believe that experiments have the potential to enliven the field and offer a valuable complement to the existing methodologies.

### Experimental philosophy

Experimentation has a rich tradition in the natural sciences, as well as the social sciences, especially psychology and economics. Psychology, and to a lesser degree sociology, has historically relied on laboratory experiments to develop and test theories of behavior. Within the management discipline, research in organizational behavior has drawn from these studies and relied heavily on the experimental method for developing and testing new theories in the organizational context. For example, numerous studies have investigated negotiations and small group decision-making using experimental methods (e.g., Adler *et al.*, 1992; Knez and Camerer, 1994; Pinkley and Northcraft, 1994; Polzer *et al.*, 1998; Flynn and Staw, 2004).

More recently, experimental methods have been applied to economics, resulting in significant contributions and the 2002 Nobel Prize for the area (see Davis and Holt (1993), and Kagel and Roth (1994) for reviews). In competitive strategy (and its economics cousin, industrial organization), experiments have been used to investigate decisions about pricing, monopoly power, market entry, and other issues (see Holt (1994) for a review). The advantages of experimental methodologies have been recognized (and the methodology adopted) in fields related to corporate strategy; however, it has not yet received widespread attention in our own field. The gap in adoption may likely be due to insufficient knowledge of the advantages and limitations of experimental methodology when applied to

corporate strategy research, and the potential complementarities with traditional empirical designs based on secondary data analysis. We turn to these issues next.

### Advantages of experiments

The relationship between theory and data in the sciences has been hotly debated (see, e.g., Kuhn, 1970). Most researchers agree, however, that a combination of *induction* (observing regularities or facts in the world and generalizing them to form theories) and *deduction* (using the theories to make predictions in new environments and then testing those predictions) is the basis of scientific advancement (Camerer, 1985). Data of all kinds can be used in this process, observational, self-reported, and experimental. However, observational data, as typically used in strategy research, faces a number of challenges. Appropriate data are often not available to test the theories being considered. When data are available, it is not always cleanly measured. Finally, the causality of relationships between naturally occurring measures is difficult to identify.

Experiments provide a unique opportunity to address these challenges. First, they provide clean, observable, dependent measures, unlike real-world data that can be noisy.<sup>1</sup> Second, experiments can be designed without confounds, much as a physics experiment might control air pressure in measuring an atomic reaction, yielding clean independent measures as well. These two advantages make experiments useful in testing the predictions of theories (either point-predictions or comparative-static predictions) or in estimating theories' parameters. Third, this additional control can be used to construct laboratory conditions to separate alternate theories and predictions that might not otherwise be separable with naturally occurring data. Fourth, experiments can establish causality more convincingly than empirical data. Finally, experiments are replicable; other researchers can reproduce the experiment and verify the findings independently. We discuss each of these issues in turn below.

First, experiments can provide dependent measures that are easy to interpret and directly address the theories being tested. For example, in field studies evaluating coordination between recently merged firms, scholars often use stock prices as the dependent variable as a proxy for the amount of coordination that the merged firm has achieved (Anand and Singh, 1997). However, it is difficult to identify what information the market is responding to and what has already been factored in. Using experimental methodology, these problems are circumvented, since output measures (like firm profitability or coordination) can be directly captured, without confounding factors like stock-market valuations or incentive problems within the firm.

Second, experiments can provide a direct test of theories by isolating the independent measures on which the theories rely. Many theories designed to explain field observations abstract away from other phenomenon that are present in the field. For example, incentive problems between managers and firms' owners are not represented by many acquisition theories, while they certainly exist in natural settings. An experiment can control for these, confounding factors and test the theory itself (as we will discuss below, this is also a limitation of experiments, when

the factor of interest interacts with the one being controlled). Because of this control, experiments can be used to test a theory's point-predictions, its comparative-static predictions or to estimate parameters of a theory.

A third advantage of the experimental methodology is in its ability to reconcile competing theories. Corporate strategy researchers often assimilate theories from different source disciplines to develop new theories that integrate these multiple perspectives. Testing competing and integrative theories require data that control for many factors (see above), and *situations where these theories make competing predictions*. These situations are difficult to find in the field, but can be constructed in the lab. Multiple theories often make predictions that are collinear in the field, but in the lab we can construct situations that disentangle the theories' predictions and allow us to draw conclusions as to their validity and applicability in the corporate strategy setting. Lab experiments can implement unusual or rarely observed parameter values or treatments in which the competing theories make opposite (or at least different) predictions, in a way which would not be possible using naturally occurring data. Thus a clever experimental design can separate theories that could not be distinguished otherwise.

A fourth benefit of this methodology is that it can establish causality more conclusively than observational research or case studies. Observational research provides evidence of association between phenomena but has difficulty establishing a cause-effect relationship. While observational research has become increasingly sophisticated in this domain (see, e.g., Granger causality and instrumental variables analyses) and leads to valuable conclusions, experimental methods can establish a more compelling case for causal relationships that are hypothesized from existing research. Often experiments involve one treatment in which an outcome or relationship is demonstrated, and a second in which it is made to disappear. This 'make it go away' objective is an important one; if we remove the hypothesized cause and the effect remains, this is clear evidence against a causal link. In contrast, if the effect disappears when the cause is removed (all else held constant) this is evidence in favor of the theorized causal relationship. Analyses of moderation and mediation effects can also help in identifying causal relationships (Baron and Kenny, 1986); although these analyses can be (and often are) used on observational data, they are especially appropriate for experimental data.

Finally, experiments are replicable; that is, researchers in different labs can implement similar procedures and collect independent validating (or dissenting) data. While this kind of replication is possible using observational data from the field, it is often difficult to collect similar data from multiple sources.<sup>2</sup>

In summary, the experimental methodology allows for clean measures of dependent variables. Experiments can be designed to avoid confounds and provide clean measures of independent variables as well. The methodology allows us to disentangle competing hypotheses that would be confounded in the field, provides strong evidence for causality as well as correlation, and allows for replication of results. These advantages make experiments a useful tool in the dialectic between corporate strategy theories and evidence. However, experiments have limitations as well.

### Limitations of experiments

The most salient limitation for corporate strategy is that of *external validity* (how much does the experiment tell us about the real world). Critics of experiments argue that since the laboratory situation is abstract and unrealistic (in that it contains fewer considerations, dimensions and confounds than the real world), no results from the lab can be used to predict behavior in the world.

We disagree. Zelditch (1961) discusses this issue in depth, in his amusingly titled article 'Can You Really Study an Army in the Laboratory?' His argument acknowledges that the laboratory setting is different from any naturally occurring, real-world setting one is likely to find. However, he argues, the bridge between the lab and the real world is the theory being developed to explain real-world behavior and being tested in the laboratory design. Theories are developed to predict and explain real-world observations. These theories should also predict and explain behavior in laboratory settings. If they do not, it is not the fault of the experiment, but a lack in the theory.<sup>3</sup>

This argument is also made by Plott (1991) in the context of market experiments. He argues that the theory of market equilibrium, if true, should predict behavior in a laboratory market just as it should predict behavior on the London Stock Exchange. In fact, it should perform *better* in the lab, since confounding factors not incorporated in the theory are absent in that setting. If the theory does not predict in the clean, uncomplicated environment of the lab, how likely is it to predict in the cluttered, confounded environment of the field? Note that this argument holds only if the conditions and assumptions of the theory are implemented in the lab (if *internal validity* is high).

There is one serious limitation from the lack of external validity, however, and that involves interaction effects. Imagine that an experiment identifies the impact of a main effect in a controlled setting; for example, the advantages of learning-by-doing within an organization in a setting without principal-agent conflicts. If the main effect (learning-by-doing) interacts with one of the omitted institutional details (principal-agent conflicts), then the results from the lab would not be applicable in the field. So, for example, learning-by-doing advantages might be present in the absence of principal-agent conflicts, but in their presence, learning-by-doing advantages might disappear. This limitation can be addressed only by carefully thinking about possible and reasonable interactions, and testing them in further experiments. Many experiments do just this; demonstrate interaction effects rather than focusing on main effects. Including the appropriate interactions in the initial design is crucial to overcoming this limitation.

A second major limitation has to do with the level of analysis typically used in experiments. Experiments usually involve a single decision-maker, while in most models of corporate strategy we envision groups, or teams, making strategic decisions. How much can experiments involving individuals explain or predict outcomes generated at the level of the firm?<sup>4</sup>

The answer depends on the type of experiment, and the type of result. Firms are made up of groups of individuals, and firm's decisions are made by people. If the experiment illuminates a universal, systematic, and persistent bias (e.g., that individuals tend to chase sunk costs; Arkes and

Blumer, 1985), this bias will likely be observed at the level of the firm as well as the level of the individual, with appropriate consequences. If the experiment illuminates strong individual differences (e.g., that some individuals overweight low probabilities, and others underweight low probabilities (Fehr-Duda *et al.* (2006)), the predictions for firm decision-making are not clear.

Furthermore, some experiments explicitly examine group decision-making (e.g., experiments in the risky shift, showing that groups of individuals tend to make more risk-loving choices than their constituent individuals (Cartwright, 1971). These experiments, of course, relate more directly to theories of firm (group) decision-making.

### Complementarities between experimental and observational research

We believe that experimental methodology can serve as an important complement to observational research. There are three ways that experiments and observational research can complement each other.

First, experiments can be combined with observational research in theory-building (inductive) exercises. In this situation, observational research identifies regularities that are not predicted by existing theory. Experiments can then be designed to capture what researchers believe are the relevant dimensions of the field and to replicate the regularity in controlled conditions. Then, one by one, the real-world features can be eliminated (or relaxed or changed) until the regularity observed disappears or significantly weakens. This exercise identifies the cause (or causes) of the observationally observed regularity, and can result in theory construction.

Second, experiments can complement observational research by suggesting places in the field to look. Collecting proprietary or archival data is often expensive and complicated, requiring years of investment and effort. Fast, inexpensive pilot experiments can be used in conjunction with a researcher's intuition to suggest what data are worth collecting.

Finally, observational research can point experimenters in the right direction. Observational research can provide firm-level outcomes, and experiments can be designed to isolate the drivers of decisions. For example, if observational research shows that firms often make bad acquisition decisions or overpay for their acquisitions, then competing theories related to escalation of commitment, hubris hypotheses and principal-agent forces can be tested in a lab setting to explain such an observation (see, e.g., Haunschild *et al.*, 1994).

In summary, experiments have both advantages and limitations. We argue for the use of multiple methodologies to further inform theory development in strategy (and other areas), including experiments. When research methods that emphasize internal validity and those that emphasize external validity provide a consistent set of results, it increases not only the reliability of the outcome, but also leads to a stronger set of conclusions.

### Existing strategy experiments

While the set of experiments in strategy is small, a few papers have applied this methodology to our field. This

section discusses previous experiments in strategy. Our review will necessarily be incomplete, and the boundary between experiments in strategy and its reference disciplines are somewhat arbitrary. Nonetheless, we hope that these papers can serve to illuminate what the experimental method can contribute to corporate strategy research.

#### Uncertainty and vertical integration: Sutcliffe and Zaheer (1998)

This paper describes a 'horserace' experiment, which tests the predictions of competing models in a controlled setting. Theoretical research in strategy has made competing predictions of the impact of uncertainty on the probability of vertical integration. Some models suggested that uncertainty would increase the likelihood of integration (Walker and Weber, 1984, 1987; Anderson, 1985; John and Weitz, 1988), while others suggested it would decrease the likelihood (Porter, 1980; Harrigan, 1985; Balakrishnan and Wernerfelt, 1986). The authors identify three different types of uncertainty, which might have different effects, *primary uncertainty* (about the state of the world), *competitive uncertainty* (about the actions of one's competitors), and *supplier uncertainty* (about the actions of one's suppliers). They run an experiment to test the impact of these types of uncertainty on the likelihood of vertical integration.

Participants in the experiment are students enrolled in an executive MBA strategic management class. All were executives or managers working full time. Participants read a scenario describing a hypothetical situation where a firm must decide whether to vertically integrate or to outsource. The authors manipulated the extent (low/high) and the type(s) of uncertainty (primary, competitive, and supplier) faced by the firm. They then recorded the recommendations of their participants.

The authors found that the primary uncertainty and competitive uncertainty were negatively related to the likelihood of vertical integration. In contrast, supplier uncertainty was positively associated with vertical integration.

This experiment provides a nice illustration of some of the advantages of the experimental design. The authors used the experiment to distinguish between competing strategic theories in ways that observational data could not. In particular, they independently manipulated types and levels of uncertainty, and could thus identify the impact of each.

But the authors went further than simply 'proving' one theory and 'disproving' another. They used the experiment to *reconcile* the existing theories, showing conditions (here, types of uncertainty) under which one theory or another made more accurate predictions. Thus this experiment contributes to theory development (and refinement) as well as providing a simple test of the theories.

#### Compensation and incentives: Schotter and Weigelt (1992)

This paper tests principal-agent theories that have been used to explain and predict profitability of firms. In particular, managers are often compensated based on the investments (and returns) they make in the short run, thus they have insufficient incentives to make long-term investments that might be in the firms' best interests. The authors constructed four treatments designed to mimic varying 'horizons' of managers.

The authors found that participants whose incentives were more closely aligned with the firms made more efficient investments than those whose interests were not aligned, thus providing evidence in support of the principal-agent model's predictions.

While this experiment tests (and finds support for) theoretical predictions of incentive contracts, its motivation stems not from theory testing but from practice. Concerns about optimal managerial investment are often faced in the field, and firms have been creative in constructing contractual arrangements to attempt to align incentives. This paper asks if these creative arrangements are really necessary, and concludes that they are.

#### Leadership: Weber *et al.* (2001)

This paper moves closer toward a group decision-making setting, examining how groups react to leadership and what leaders can do to lead in organizations. The authors argue that a large part of what leaders do is to coordinate the actions of the members of the organization. In their experiment, participants play an impure coordination game, known as the minimum effort game. Here, each participant chooses an effort level, but the payoff is based on the minimum effort chosen by any member of the group. Choosing higher levels is costly for the individual, but high-effort outcomes are more profitable for the group as a whole. High efforts are also risky, as high-effort individuals pay the costs of these efforts, and may get a low-effort outcome if one participant chooses the lower (and safer) option.

In this experiment, before playing the game the experimenters randomly select a leader, and allow them to make a speech exhorting participants to make an efficient investment. They use both large groups (which often fail in inducing high effort) and small groups (which often succeed in inducing high effort). The authors show that small groups were successful, and that this success was attributed to the abilities of the leader. In contrast, large groups were unsuccessful and these failures were also attributed to the abilities of the leader. In a follow-up experiment the same type of task was given, but the materials included more context and structure, with identical results.

This experiment inductively asks (and answers) the question of the impact of leaders in coordination settings, and of the perception of leaders, and attributions made about them. It suggests that leaders are often held responsible for outcomes that they could not control.

#### Norms and corporate culture: Bettenhausen and Murnighan (1991); Knez and Camerer (1994); Weber and Camerer (2003)

Three experiments in strategy investigate the impact of corporate culture and norms in organizations. In Knez and Camerer the authors use the minimum effort coordination game described above. Participants begin in small groups (of three) who then merge into larger groups (of six). The authors use two treatments, one in which the new, larger, group is informed of the history of the previous groups and the other in which they are not. As in the previous study, this experiment finds that large groups do worse than small groups. But large groups do *more* worse when the previous history is public than when it is private information.

The authors argue that this experiment contributes to resource-based theories of the firm by identifying a new type

of assets that firms have; expectation assets. These assets represent others' beliefs about the firm, which are affected in their experiment by the communication of the group's history of decisions. Groups whose decisions were cooperative and efficient benefited from this communication, while groups whose decisions were not were harmed by it.

In a second study, Bettenhausen and Murnighan (1991) examined whether groups imported norms of behavior from previous experiences. They used a repeated prisoner's dilemma game with communication between the parties. Participants played 'for practice' with a confederate who was either cooperative or competitive, and who presented arguments in favor of their actions. Then participants were rematched into new groups of two based on their previous experiences (excluding the confederates). Three treatments of cooperative/cooperative, competitive/competitive, and cooperative/competitive were constructed. Participants again made decisions, but their discussion was also recorded and coded.

The authors found that groups who had previously been exposed to cooperative cultures acted more cooperatively, and groups who had previously been exposed to competitive cultures acted more competitively. Groups made up of heterogeneous cultures fell in between, but interestingly individuals tended to use arguments in favor of actions that reflected their previous experiences.

A third paper by Weber and Camerer (2003) examines the impact of corporate culture in merger success using a slightly different paradigm. Here, participants are organized into small groups and given a communication task. Two participants see the same set of pictures on separate computer terminals. One (the manager) needs to describe one of the pictures to the other (the employee) so that the employee quickly and accurately chooses this picture out of the set. The positions of the pictures are randomized on the two screens. In this task, a specialized language quickly develops between the two parties; shortcuts for identifying features of the picture that is desired. After a few rounds of this, firms are merged, such that one manager now describes the pictures to two employees. This creates a clash in corporate culture, as the language used by the new employee is not likely to be the same language as was developed by the existing employee. The authors measure performance degradation in response to the merger.

The authors find that the time to completion (selection of the correct picture) decreases significantly with repetition in the original (premerged) firms. After a merger, time to completion significantly increases, then decreases over the course of the next few periods. Thus this experiment documents both the transitions that mergers engender, and the recovery that can occur to create a functioning merged organization.

More generally, this stream of research illuminates the impact of corporate culture on behavior, especially when firms merge or acquire other firms. The norms or typical modes of behavior or communication in one organization (and knowledge about them) significantly influences behavior in the new organization.

#### Reputations: Camerer and Weigelt (1988); Weigelt and Camerer (1988)

Two papers identify the importance of reputations as an asset that can generate future rents for firms. Weigelt and

Camerer (1988) describe corporate strategy settings where reputation is important, including product quality, reputations in the service sector, screening borrowers and employees, and as a feature of corporate culture. Camerer and Weigelt (1988) report an experiment investigating the development and use of reputations.

In their experiment, entrepreneurs borrow money from bankers and then decide whether to pay it back. Some are honest types, who prefer to pay back their loans. Others are dishonest types who do not. However, because the interaction is repeated, dishonest types have the incentive to pay back their loans early in the game in order to develop a reputation for honesty. The authors run multiple treatments with different parameters and different equilibrium predictions. They find results consistent with the comparative static predictions of the equilibria, as treatments vary the proportion of loans made and repaid varies accordingly. Nonetheless, they found that individuals in the experiment were 'too trusting,' in deciding to make loans they ignored the prior probability that the entrepreneur was the dishonest type and instead made more loans than was optimal.

These experiments are deductive, testing formal (equilibrium) theories of reputation formation and examining the similarities between observed behavior and those predictions. These deviations are then used to explain and predict firm behavior in settings where reputations form an important and strategic asset.

Cognitive biases: Bateman and Zeithaml (1989); Schoemaker (1993); Hodgkinson *et al.* (1999); Burmeister and Schade (2007) A larger stream of research identifies cognitive biases that might affect managers making strategic decisions. This research both identifies biases and suggests debiasing techniques that can be used in organizations to improve strategic decision-making.

Schoemaker (1993) investigates the bias of *overconfidence*, individuals tend to believe that their estimates of strategically important variables are more accurate than they actually are. The authors use a within-subject design with MBA students and executives as subjects. First participants describe a strategically important issue, identified target variables that would be relevant for their decision and provided estimates and confidence intervals for their estimates of the target variables. Estimates and confidence intervals are also collected from a colleague at work. Participants then conduct a detailed scenario analysis, and are again asked for their estimates and confidence intervals. The author finds that scenario planning significantly reduced overconfidence, both for the target individual and for the colleague.

Hodgkinson *et al.* (1999) investigate the bias of *framing*, individuals tend to make different strategic decisions when the choices are framed positively or negatively. The authors present participants with positively or negatively framed scenarios. The scenarios involved a vignette of a future investment decision, with a choice between a safe strategy (staying in a domestic market) and a risky strategy (investing overseas). There were two versions of the vignette, one framed positively (profits greater than zero) and the other framed negatively (profits less than target profits). Some participants were given a pre-choice cognitive mapping task, while others did the task post-choice.

The authors found the traditional framing effect in the post-choice treatments; individuals chose the more risky option when outcomes were framed as losses, but the less risky option when outcomes were framed as gains. However, they found that when individuals engaged in the cognitive mapping task pre-choice, the framing effect was significantly meliorated. A second experiment with managers (as opposed to undergraduates) yielded the same results.

Both of the above studies not only identified the impact of cognitive biases in strategic decisions, but also identified analysis tools that could meliorate or eliminate the negative effect of these biases on behavior.

Bateman and Zeithaml (1989) investigate the bias of escalation of commitment, individuals do not ignore sunk costs as they should and overinvest in projects to which they have previously committed resources, even though the marginal benefit of these new investments is relatively low. These authors ran lab experiments with both college students and practicing managers. Participants were given a scenario describing either a successful project and financial windfall, or an unsuccessful project and tight financial situation. Future profits were described in a positive or negative frame. Participants were then asked how much they wanted to invest in the existing project. The results are consistent with the sunk cost fallacy; the highest investment is when the project's future prospects are positive but when the history of the project (and availability of money) is low. Individuals invest significantly more in this treatment than when the project's future prospects are positive and the history of the project (and availability of money) is high. In a follow-up study, Bateman and Zeithaml (1989) asked executives to predict the results in their previous experiment, and found that executives did not predict the patterns actually observed.

Burmeister and Schade (2007) investigate the *status quo bias*, individuals prefer options that are described as the current situation. This paper begins with the maintained (and popular) assumption that entrepreneurs are more biased than others in the population. Burmeister and Schade asked students, bankers, and entrepreneurs to make a series of decisions, both on the consumer purchasing side (e.g., buying a digital camera) and on the business side (e.g., deciding on which new markets to enter). The experiment involved treatments with different *status quos*, or none, and measured the likelihood of a given individual to switch their choice from the *status quo*. The authors found that entrepreneurs are equally as biased as students, but less biased than bankers, who were the most biased.

These last two papers focus not on debiasing individuals but instead on identifying the extent of the biases observed in different populations. Both found that students as well as professionals exhibited these biases, and that individuals did not anticipate these biases in others.

Alliances and acquisitions: Agarwal *et al.* (2007a, b)

Two experimental papers examine the performance of alliances. Both model alliance behavior as a co-optetition task, where the alliance is successful when enough resources are committed to it, but where individuals have an incentive to free ride and let others contribute. In Agarwal *et al.* (2007a) the authors compare the impact of economic incentives and communication on successful alliances.

In this experiment, participants allocate resources either toward their own private production, or toward joint production activities in the alliances. Incentives are either aligned (all alliance members have a financial incentive to contribute), mis-aligned (all alliance members do not), or mixed (some alliance members have an incentive to contribute, and others do not). They are either allowed to communicate (via a chat box) or not. The authors find that incentive alignment is a necessary but not sufficient condition for alliance success. No alliances are successful when the incentives are mis-aligned. However, communication significantly improves the success of alliances when incentives are aligned or when incentives are mixed.

This experiment serves to unify competing models of alliance activity. Theories from the economic perspective argue that incentives are everything, and that if incentives are aligned then alliances will be successful. In contrast, theories from the sociological perspective argue that communication is key to a successful alliance. This paper demonstrates the role for each factor in alliance success.

Agarwal *et al.* (2007b) investigate the impact of an alliance on a subsequent acquisition between the firms. Previous observational studies suggest that acquisitions that have been preceded by alliances are more successful than those that have not. However, there are multiple (competing) theories that have been generated to explain this link. For example, the *real options* approach suggests that there is some uncertainty in the potential benefit of an acquisition, and that an alliance allows the acquirer to resolve the uncertainty, and to acquire only if it is profitable to do so (e.g., Kogut, 1991). Note, this relationship is not causal – the alliance reveals already-existing information, rather than causing improved performance. A second approach claims that the *experience* of interacting in an alliance changes the behavior of the firms and causes superior coordination post-acquisition (e.g., Gulati, 1995). A third perspective suggests that alliance partners develop *sticky routines* to aid in alliance interaction but which may not help post-acquisition coordination *per se* (e.g., Zollo *et al.*, 2002).

In this experiment two divisions of a firm interact with each other, sharing profits. A third firm exists and earns its own (independent) profits. When the experiment begins, the third firm is involved (or not involved) in an alliance with the two divisions. After a few initial quarters, the second firm is acquired and becomes a third division. This third division's payoffs are now also shared among the other divisions. Post-acquisition, the experience is the same for all participants in all treatments (with and without alliances). Note that this design eliminates the real options mechanism, as all treatments result in an acquisition, regardless of the success of the alliance experience. If, on the other hand, the arguments underlying *experience* as the causal mechanism are valid, one would observe a positive effect of alliances on post-acquisition performance. Similarly, if *sticky routines* cause suboptimal behavior, we might observe a negative relationship in the experiment. Thus the experimental design provides the ability to distinguish the predictions of competing theories.

The authors find a negative relationship between alliance activity and post-acquisition performance, providing support for the sticky routines explanation of behavior. In the field, the real option approach is likely operating in

conjunction with these routines, leading to positive returns from pre-acquisition alliances overall.

### Summary

In this section, we have described a series of experiments in corporate strategy. While we cannot provide a comprehensive review, we hope that this section has provided an introduction to the types of experiments that have been (and could be) run. In describing each experiment, we have tried to go beyond the substantive contribution and highlight the different types of experiments that one can run. Some experiments are horse races between competing theories (e.g., Sutcliffe and Zaheer, Agarwal, Anand, and Croson), others test a given theory (e.g., Camerer and Weigelt, Schotter and Weigelt), others are motivated by observations from the field (e.g., Weber and Camerer, Weber, Camerer, Rotenstreich, and Knez), and still others inductively explore and identify anomalies for further research (e.g., Schoemaker, Burmeister, and Schade). We believe that experiments can contribute to research in corporate strategy in all these ways, and more.

### Future research directions and applications of experimental methodology

This paper has outlined some advantages and limitations of the experimental method. This method has been used in many related fields, but has not yet been widely adopted in corporate strategy. In this section, we discuss four areas of corporate strategy research where we believe experiments could be productively applied. These areas often involve a discussion or debate among perspectives, and we believe that experiments can make an intellectual contribution in addressing these debates.

First, an interesting conversation has emerged between the capability, evolutionary, and knowledge-based perspectives that emphasize employees' *ability* to work and the transactions costs, agency and game-theoretic perspectives that emphasize employees' *willingness* to work (e.g., see critiques of Kogut and Zander, 1993; Love, 1995; McFetridge, 1995). While these theories rely on different mechanisms, ability and willingness are exceptionally difficult to measure in the field. Where they are measurable, they are often not separable. An experiment could induce incentives for work (or not), provide information needed by employees to work (or not) and thus help in distinguishing between these alternative causes. Experiments can productively address this debate, since they circumvent some of the measurement problems encountered with the use of observational data.

Second, in the context of cooperative settings such as alliances, there is an interesting discussion between perspectives that emphasize social capital and networks and those that emphasize capabilities and incentives. In the world, social networks or social positions are likely to be correlated with real differences in capabilities of firms, which makes it difficult to conclude whether observed outcomes reflect effects of these capabilities or of social positions. Experimental methods can be used to tease apart the effects of incentives (present or not) and social capital (present or not) in alliances.

A third area involves corporate governance. Experiments could examine a recurring question of why boards of

directors and top management teams undertake strategies that are seemingly value-destroying rather than value-creating. Experimental methodology can simulate incentives and contexts faced by managers, and examine agency costs and managerial hubris as competing (or complimentary) motivations for the observed decisions.

A fourth topic involves corporate strategies related to international diversification. For multi-national firms, the success of corporate strategies may depend on the 'mental model' used by managers, which differ across nations. A controlled experiment run in different cultures would allow for the identification of these mental models. Note that this suggestion does not involve distinguishing between competing theories (as the other examples) but is instead more inductive; identifying the mental models in use and using those models to construct a theory of when international diversification is likely to be successful, and identifying strategies to improve its success.

### Conclusion

We have emphasized the potential contributions that experimental methods can make in the field of corporate strategy. We began with the observation that experiments are already well accepted in disciplines closely related to corporate strategy. But the use of experiments need not be limited to only those domains that directly build on these disciplines; corporate strategy theories that are not typically associated with psychology or economics are also fertile territory for the use of experiments. We believe that experiments can be used to address theories, both by testing them directly (point-predictions, comparative static predictions), by distinguishing among competing theories, and by convincingly establishing causality. Experiments have limitations as well, particularly external validity, and thus must be used in combination with theory and observational research to triangulate on the truth. We have identified a number of ways that experimental and observational methods can reinforce each other as well, including theory-building and identifying promising areas to pursue. We have provided a review of previous experiments that have addressed corporate strategy topics, and discussed more briefly four other areas of corporate strategy where we believe experiments can be productively applied. We encourage interested scholars to consider these (and other) areas in corporate strategy as those ripe for further experimental research.

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### Notes

1 Of course, no data are perfectly clean. Individual differences in risk preferences, other-regarding preferences, or even confusion, can make experimental data noisy. However, these sources

of variation can be measured and controlled. Risk preferences can be induced (e.g., by using the binary lottery procedure) or measured using a risk elicitation mechanism and added as a control to the analysis. Most experiments include a questionnaire as part of the instructions or in as an exit survey after the experiment, and participants who are confused can be identified and eliminated from the data set. Other-regarding preferences can similarly be identified and controlled for, and instructions can be written so as to minimize (or maximize) fairness concerns, as the researcher chooses.

- 2 Note that while the ability to replicate is an advantage of the experimental method, many have decried the lack of replication which exists, primarily due to the lack of incentives for replication (see, e.g., Rubinstein, 2001).
- 3 Note that Rubinstein (2001) takes an opposing view, arguing that economic theories do not make testable predictions about the real world, and thus that experiments cannot evaluate them. He writes that he considers it 'hopeless, and, more importantly, pointless to test the predictions of models in economic theory.' In contrast, theories in strategic management are presumed to make testable predictions about firms' behavior and thus (presumably) could be tested in a laboratory setting as well as in the field. Rubinstein also notes that 'experiments serve as a test of the plausibility of assumptions and not conclusions' of economic theories. Thus he sees an important role for experiments, even when the theories being tested are not designed to predict real-world behavior.
- 4 Experimental research in industrial organization has dealt with a similar concern, see Holt (1994) for a discussion.

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