Strategy Science — Call for Paper Proposals

Building a Science of the Particular: Methods for Strategy's Core Questions

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Proposal Deadline: November 15th, 2026

Motivation

Strategy research aspires to guide strategists—actors who seek to assemble sets of choices that combine to create and capture value on a sustained basis. What this decision maker needs is guidance about the particular: a diagnosis of the current problem, a theory of this firm, in this situation, at this moment—and ultimately guidance about a productive set of choices to make. These choices of a strategic decision maker are interdependent and rarely repeated, so their outcomes cannot be averaged or diversified away, and to create value they must be unique relative to peers.

A common claim is that the strategy field has strayed from providing such guidance and from engaging with core strategy questions, leaving research output often irrelevant for practitioners of strategy.

Some have linked this shift to the growing demand for causal identification in empirical work (Hambrick, 2004; Durand, Grant, & Madsen, 2017; Leiblein & Reuer, 2020). Over the past 30 years, strategy has joined economics in aggressively adopting research designs that enable causal claims (Angrist & Pischke, 2010). One estimate is that the share of papers using causal methods in leading outlets for strategy work has risen from roughly 10 percent in 1990 to more than half today (Frake, 2026).

But in both economics and strategy a debate has emerged about the impact of these methods. The complaint is not that the empirical designs are inherently wrong. Rather, both fields have come to prize confidence in answers over the strategic importance of the questions asked, pushing research to drift toward smaller questions with cleaner designs (Ruhm, 2019), and committing what Akerlof (2020) has called "sins of omission."

But what is a concerning problem in economics, is even more troublesome in strategy. The strategist makes a choice or set of choices for a single firm, and the estimated average effect is valuable only in so far as the estimated model fits well the specific context and situation in which the firm finds itself—which, for a choice that must be unique to create value, is precisely when it fits least well. For a strategist an estimated average effect across many firms is not just a noisy version of the guidance needed. Even with everything firm-specific controlled for as a confound, it may simply be bad advice. So the same methods that are concerning in economics should be of even greater concern in strategy—pulling our field even more strongly away from the questions strategic decision makers need answered.

Has the field built a research apparatus around analytical elegance instead of producing knowledge relevant to decision makers? When can we reconcile the uniqueness of strategic situations with the estimates of average effects that are outputs of causally identified estimates (see Manski, 2021;

Snyder, Wuebker, & Zenger, 2026)? When are alternative approaches needed? And what might they be?

This is a call for papers that provide guidance to the field about how our research can better reach new audiences, particularly strategists, including what new research methods, practices or norms we should follow to reach these audiences. For instance, under what conditions are deep studies of particular contexts relevant for academic study? When are they not? How and when should strategy scholars balance the academic's need for generality with the strategist's need for particularity? Can the application of experimental methods to strategy frameworks and decision tools yield rigorous and generalizable methods for strategists to address firm-specific problems? We are interested in descriptions of what makes a context acceptable (how broad, what country, a perfect laboratory), descriptions of what parameters we should be interested in, techniques for identifying (or partially identifying) them, and a discussion of how new research norms will broaden the impact of strategy research.

This special issue invites proposals for papers that consider these problems and diagnoses and importantly offers remedies as to how empirical strategy should proceed. The question is not whether strategy should be rigorous, but what rigor means when the object is advice about the particular. We construe methods broadly—research designs, formal and computational models, case and comparative-historical work, field experimentation, standards of evidence, AI-enabled tools, or other methods beyond this list.

We seek more than critique and especially encourage papers that deploy new methods—demonstrating, on a substantive strategy question, what an approach delivers that current empirical practice cannot. Work defending and extending identification focused approaches is equally welcome, provided it demonstrates value in addressing a core strategic question rather than asserting it. Illustrative questions that proposed papers might take up include:

- What would it mean to judge our work by the choices it supports rather than the precision of its estimates? How would this differentially shape what we estimate, report, and claim?
- If understanding unique context is central to strategy, what methods can characterize a firm's configuration, complementarities, and bottlenecks rather than partial them out? How might we provide better guidance about how to apply a finding inside a particular firm?
- How can a field whose tools aggregate across firms study outliers and deliberately contrarian positions—the regions where strategic value is likely concentrated?
- What findings that come from large scale empirical work are useful to the particular? What types are not?
- What should be the division of cognitive labor between a strategy scholar and a strategist? What must be done to map causal estimates to problems of the strategist? Also, if the strategist must complete the test locally, through piloting and experimentation, what must a researcher deliver so local testing can succeed?
- How should research evaluate the quality of a strategy—the quality of its underlying logic—before it is implemented? What can we learn about scientific decision making by strategic actors?

- Do large language models, machine learning, and synthetic data open paths to better firm-specific prediction, or do they compound the bias toward the average?
- Beyond statistical significance, what standards can calibrate confidence across quantitative, qualitative, and historical work? How should our approach to testing theories change?
- What changes in review, training, and journal practice would reward the production of work on core strategy questions?

Proposal Structure and Review Process

We invite proposals of no more than 3–4 pages (excluding references), due November 15th, that answer some or all of the following types of questions:

1. What core strategic question related to methods does the proposal take up, and why has prevailing practice failed to address it?
2. What method or shift in method is proposed—new, adapted from another field, or a revival of something strategy has set aside?
3. On what firms, settings, or data will the proposed method be shown to work, and what will that establish?
4. What do prevailing approaches fail to deliver on this question, and what does the proposed approach deliver that prevailing practice cannot?
5. By what standard should the results that proceed from this method be judged, and why is this standard appropriate?
6. What could a strategist do with access to your paper and proposed methods that differ from what is currently being done?

Purely conceptual contributions will be considered where they reconsider what the research product should be; priority among empirical proposals goes to methods demonstrated to explore strategy questions. **Timeline.** Proposal submissions (3-4 pages) due November 15th and will be provided feedback from editors by December 15th; the strongest proposals will be invited as full paper submissions (due by April 15th, 2027) for standard peer review, which does not guarantee publication. We will also organize an author workshop in the spring for the proposals selected. Submit proposals via the regular Strategy Science submission portal, flagging the submission for the Special Issue – Building a Science of the Particular: Methods for Strategy's Core Questions

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