# AVERAGE TREATMENT EFFECTS AND UNIQUE STRATEGIES

Jason Snyder
Robert Wuebker
Todd Zenger

What explains the increasing disconnect between empirical strategy research and real-world strategy? While empirical methods have evolved, notably in causal identification, this paper argues that empirical strategy's focus on population-level average treatment effects is fundamentally misaligned with the strategist's need for firm-specific insights. This misalignment poses a critical problem because strategic decisions are typically one-shot, non-diversifiable, and deeply interconnected with other firm-specific choices—thus, strategists require localized, firm-specific causal estimates rather than generalized averages. Through an extended empirical example, this paper demonstrates the limits of strategy's current approach of ever-more-precise identification and offers new methodological approaches to bridge the relevance gap.

*Most people regard clarity and precision as more or less the same. But in my opinion, there is a big difference between the two… The clash between clarity and precision means that as you become more and more precise, fewer and fewer people will be able to understand what you are saying*. – Karl Popper[1]

The strategy field today confronts a growing chorus of complaints that its research output has become rather irrelevant to strategy practice. As early as 2001, Michael Porter claimed that "strategy had lost its intellectual currency" and was "losing adherents" (Hammonds, 2001). More recently, scholars suggest the field has "lost its way…[and] has strayed from its primary focus on efficient and effective management practice" (Drnevich, Mahoney, & Schendel, 2020: 35). Surprisingly, this decline in real-world application coincides with significant growth in empirically sophisticated causal estimation approaches—approaches that, in other fields such as labor economics, development economics, and finance, have fueled a surge of real-world impact and popular press attention. In the strategy field, not only has this push toward causal identification not heightened real-world application, but it has arguably coincided with a decline.

Some suggest the remedy to this crisis of relevance is for strategy scholars to more effectively communicate their ideas—for example, to add managerial translations of findings, to conduct better outreach, and to more broadly make findings available and understandable to those engaged in practice. The groundswell of manager-focused abstracts, translations, podcasts, and video summaries all targeting broader and more effective access for practitioners are examples of such remedies. These are certainly worthy endeavors, but the problem our field confronts is more endemic and arguably unsolvable with such remedies. The essence of the problem is that while empirical methods that generate causal estimates of average treatment effects work well in situations where policy makers can diversify the influence of their policy

[1] Drawn from Lecture 1, "Introduction to the Scientific Method", Karl Popper's first-year lectures at London School of Economics, transcribed and organized by Mark Amadeus Notturno (2012).

decisions across a population, such estimates are of less value in settings where the influence of the policy choice cannot be diversified—in settings where the decision-maker is interested in a decision's impact on a particular organization at a particular time.

Consider a typical causal empirical finding from a field like development economics. Here it is well-known that subsidies for life-saving products such as bed nets increase willingness to pay for the product (Dupas, 2014). While such subsidies work on average, a subsidy will not increase willingness to pay for everyone. For some people, the subsidy increases willingness to pay, while for others, willingness to pay is unmoved. When policymakers look at the evidence, they know that—on average—a subsidy will increase bed net usage. And, if increasing bed net usage is one of their goals, then such subsidies make sense. On average the policy shift changes willingness to pay and thus this estimate is useful to these policy makers.

In contrast to such broad policy interventions where the response to the decision is felt and measured across many individuals or firms, *strategic decisions* shape a single organization and must be orchestrated to complement a host of other decisions inherent to that unique organization (Leiblein, Reuer, & Zenger, 2018). These decisions are also often irreversible and there is thus one shot at getting it right and thus limited means of diversifying the influence of such decisions across time[2]. Accordingly, strategists have only limited interest in the average treatment effects of an intervention or policy change across many organizations. Instead, the interest is in the hyper-local effect of a choice. This perspective implies a dramatically different conception of empirical strategy than the one the field has currently converged on, which we argue is fundamentally incapable of solving the relevance problem. Indeed, our view is that the

[2] In a letter to Amazon shareholders, Jeff Bezos characterized this attribute of strategic decision-making as a one-way door: "Some decisions are consequential and irreversible or nearly irreversible – one-way doors – and these decisions must be made methodically, carefully, slowly, with great deliberation and consultation. If you walk through and don't like what you see on the other side, you can't get back to where you were before" (Bezos, 1998).

trend in empirical strategy towards ever more accurate identification and estimation of average treatment effects can only exacerbate the relevance problem. Said another way, in a field where the core insight is that unique strategies drive heterogenous performance, estimating population-level average treatment effects is necessarily of limited practical use.

We first examine the unique empirical challenges inherent to strategy research and the central questions strategists face. Next, we review advancements in causal identification and their unintended limitations for practical application. Against this backdrop we illuminate the disconnect between developing causal results and the strategist's central question and discuss the central paradox—that efforts to increase causality mostly undermine, rather than elevate, practical relevance—and offer an empirical example that illustrates this paradox. The final section offers a path forward, illuminating how the field of strategy can both continue its pursuit of causality, recognize its limitations, and elevate the relevance to strategists of the research that it presents.

## WHAT IS STRATEGY?

To situate the dilemma that the field faces, it is useful to briefly review what strategy as a field is. The strategy field, like most others, has expanded into a wide range of topics as scholars have specialized their skillsets and focused their attention. Although this evolution makes the task of describing the field of strategy something of a moving target, the core questions of the field remain connected to the central questions of interest to the strategy practitioner. Most centrally, the field of strategy seeks to understand what explains heterogeneous performance across firms and how strategists can craft decisions that deliver advantage and sustained valuable growth.

And what progress has the field made in the exploration of its central questions? What we know is that valuable strategies are unique and firm-specific, and that they allow firms to be distinct from competitors in valuable ways. These unique firm-level strategies are achieved through managers' strategic decisions (Leiblein et al., 2018). Strategic decisions differ from routine managerial choices by their cascading effects on other decisions, stakeholders, and future outcomes. Said another way, strategic decisions are a class of managerial decisions that are important in shaping all other decisions (Van den Steen, 2017) and the strategist generally only has one shot to get it right (Rumelt, Schendel, & Teece, 1991). It is therefore essential that the firm-specific actions and strategies reflect well-composed firm-specific theories (Felin & Zenger, 2017) about how assets, resources, and activities can be orchestrated in unique, complementary ways to compose value (Barney, 1986, 1991; Brandenburger & Stuart, 1996; Montgomery & Wernerfelt, 1988; Porter, 1985, 1996; Rajan, 2012; Rivkin, 2000; Rumelt, 1984).

What real-world strategists therefore ultimately seek from the academic field of strategy is guidance about how to compose their unique, firm-specific theories or build firm-specific models (or production functions) by which to guide the orchestration of various policies and choices about assets, activities, and resources into value creating patterns. This central objective of the strategy practitioner has profound implications for the type of research that strategy practitioners find relevant. With this central interest of the strategy practitioner in mind, we now evaluate the capacity of the important empirical trends that have transformed economics, and more recently the field of strategy, to inform the strategist's fundamental task.

## EXPORTING THE CREDIBILITY REVOLUTION IN ECONOMICS TO STRATEGY

Providing useful empirical guidance to any decision-maker from available data requires scholars to generate reliable estimates that strategic actors can use to envision and predict the outcome of their choices. Thus, a large literature in economics has focused on developing and refining estimates of the wage elasticities of labor supply, fiscal multipliers, aggregate productivity estimates, and other policy relevant parameters. The importance of reliable estimation of economic models cannot be overstated, as sound economic policies hinge on the accuracy of such estimates. Yet, despite the commitment to the estimation of such parameters, prior to the late 1980s and 1990s, the credibility of those estimates remained subject to doubt—in particular, by those most familiar with them (Leamer & Leonard, 1983).

Beginning in the late 1980s and early 1990s, the field of economics witnessed significant developments in the estimation of economic parameters—dubbed the “credibility revolution” (Angrist & Pischke, 2010). Central to advances in parameter estimation were the application of quasi-experimental methods to non-experimental data. Difference-in-differences, regression discontinuity, and instrumental variables were among the methods that gained widespread use. These methods were applied to critical public policy questions, such as the relationship between increases in minimum wage and employment levels.

Within the sphere of public policy, the impact of quasi-experimental approaches in estimating economic parameters was profound. For instance, Card and Kruger’s (1995) paper on minimum wage increases that showed no significant relationship between minimum wage increases and employment called into question long standing economic policy logic. This paper, and a host of others employing these quasi-experimental methods, catalyzed sustained interest by policymakers in non-experimental methods (Findley, Kikuta, & Denly, 2021; Finkelstein & Hendren, 2020; Glied, 2021; Jackson & Mackevicius, 2024).

The use of these quasi-experimental methods in strategy has gained substantial popularity as well. With these tools strategy scholars are generating reliable causal estimates of the influence of a singular managerial choice on performance. For example, the field of strategy often tests its theories by analyzing how an organizational choice —after controlling for a host of factors including firm and industry characteristics—correlates with and causes differences in performance. This is the *average treatment effect paradigm*:

$$(1)\ Performance_i = \alpha + \boldsymbol{\beta} * Strategy_i + \varepsilon_i$$

Much of the attention in empirical strategy is now paid to leveraging the innovations spawned from the "credibility revolution" in economics to develop more and more precise and causal estimates of β. So, what kinds of audiences would value insights generated from Equation 1, and implicitly also value further methodological advances that more precisely estimate and identify β? To answer that question, let's briefly return to the field of public policy—the birthplace of the "credibility revolution" and the methodological rootstock for modern empirical strategy. In public policy, further refinements in estimating β are—and will likely remain—quite valuable. This is due to the nature of the decision—public policy decisions are generally diversifiable across broad outcome metrics. Policymakers are interested in how choices affect an entire economy, or a sector of that economy. Accordingly, public policy finds great value in understanding the average treatment effect on a population because the field can use that information to guide optimal decision-making.

Consider the public policy question "Does a year of additional education cause an increase in wages?" Using a variety of methods and settings, economists have found that the answer to this question is "yes". On average, an additional year of education causes wages to increase by 10 percent. This well-established correlation between education and wages is likely not driven solely by self-selection—it is *causal*. While it is true that for some individuals, an additional year of education may increase wages by 25 percent, for others it may reduce wages by 5 percent, a policymaker sees the average treatment effect of 10 percent and correctly concludes to use public policy instruments to encourage investments in education. Given the large number of potential students in a state or country, the policy maker diversifies risk associated with an education-encouraging policy change across this population and can sleep well knowing that while not *all* students are being helped equally, *on average* there is a 10 percent increase in wages for each additional year of education.

But there is a critical difference between public policy and firm-specific strategy making. In contrast to such policy decisions, the strategic decisions critical to a firm's success are one-shot, and in an important sense not diversifiable. Accurate firm-specific causal estimates are central to generating actionable insight from findings in empirical strategy. While public policy makers are well informed by estimates of average treatment effects, strategists who seek to compose unique, firm-specific strategies must seek to understand what explains the variance of estimates.

Findings generated from Equation (1) are of more limited value to most real-world strategy-makers because for their most consequential, one-shot strategic decisions, they are not interested in the average effect of the choice across a sample of firms. Rather, they seek to know the impact of a particular policy on *their* unique firm with its own unique, firm-specific model. To use the example above, in strategy the decision-maker is not the policy maker trying to decide how to motivate educational investments, but rather an individual trying to decide whether an additional year of education would add to their personal income. Put differently, real-world strategists are interested in an Equation (2) that is quite different in substance from Equation (1):

$$(2)\ Performance_i = \alpha + \boldsymbol{\beta_i} * Strategy_i + \varepsilon_i$$

Equation (2) is a firm-specific model, derived from a firm-specific theory about how to elevate performance.

Strategy is distinct from other disciplines such as finance and public policy in how diversifiable the decisions are and thus how firm-specific its empirically generated guidance needs to be. In fields like finance or public policy, decisions are often made with the understanding that their impact can be spread or diversified across a broad population or a set of investments, allowing for more generalized conclusions. For example, in finance, portfolio theory allows for diversification across a range of assets, mitigating the risk associated with any single decision.

Indeed, the field of finance has not experienced a "crisis of relevance" the way the field of strategy has, *precisely because* estimates of Equation (1) are generally sufficient to create value for its audience. But the pursuit of perfection in estimating Equation (1) will typically not make the findings from empirical strategy any more valuable to strategists.

In fact, as we will argue, chasing perfection in Equation (1) may make findings from empirical strategy even less relevant.

## BUILDING AND ESTIMATING COMMON MODELS

When IO economics was imported into a newly emerging strategy field decades ago by Michael Porter (1981, 1985, 1996) great efforts were made to ensure that the resulting tools informed a firm-specific strategy formation process. In contrast, the econometric tools of the "credibility revolution" have essentially been dropped into the field of strategy without fully grasping the implications of doing so. Consequently, modern empirical strategy has left an important question unaddressed and unanswered: how, *exactly*, do methodological improvements to the average treatment effects paradigm advance the field of strategy's most pressing questions and help to resolve the growing relevance gap?

The fundamental challenge in elevating the relevance of empirical strategy rests on a rather simple fact. Our empirical work essentially aims to build a common model, one that is applicable to all firms and all strategic decision-makers—a model that provides a common prediction of outcomes for various strategic choices, treatments, or even resources across all firms and decision-makers. Tom Sargent has commented on this common model challenge in empirically examining rational expectations in macroeconomics (Evans & Honkapohja, 2005). As Sargent argues, this "communism of models" problem is that "All agents inside the model, the econometrician, and God share the same model" (Evans & Honkapohja, 2005: 566). In strategy, our implicit empirical modeling assumption is that while strategic actors across firms possess differing resources or information, they all share a common model through which decisions are reached and performance determined.

To illustrate the problem and its implications for the field of strategy more clearly, consider the following empirical illustration. Suppose that entrepreneur-strategists are trying to uncover the causal effect on long term profitability of eponymous entrepreneurship, i.e., naming their firms after themselves. This is an important decision that is hard to reverse and is not diversifiable. The entrepreneur likely has one chance to get it right and lacks the luxury of simultaneously naming multiple new ventures. Thankfully for our fictive entrepreneur, relevant research has been done. The well-estimated Belenzon, Chatterji, & Daley (2017) paper on eponymous entrepreneurship answers this precise question. We chose this particular paper because of its strong, well-identified results on a large sample. They find that self-naming a firm is causally connected to improved performance within a large sample of European firms. By all standards of research, this study is extremely well done. By employing rigorous econometric techniques, Belenzon, Chatterji, and Daley (2017) isolate the relationship between eponymy and firm performance through robust controls for confounding factors. While the authors are cautious in their willingness to describe the results as causal, the estimated parameter on eponymy is a precisely (if not perfectly identified) average treatment effect. Using their data, we replicate the main finding of the paper in column (1) of Table 1. Column (1) suggests that eponymous entrepreneurship causes a three-percentage point increase in a firm's return on assets. The t-statistic on this finding is 49.43, suggesting very strong confidence in the estimated effect.

Yet the precision of the effect and their credible claim of it being causal does not mean that the treatment effect for *all* firms is positive. Consider the following simple example: There are five firms, and we somehow know that the treatment effect of eponymy is 0.4 for 3 of the firms, and -0.1 for two of the firms. The average of these individual firm treatment effects is 0.2 with an associated standard error of 0.12. Now suppose that there are 300,000 firms with a firm-specific treatment effect of 0.4, and 200,000 firms with a firm-specific treatment effect of -0.1. This large sample would dramatically increase the precision of the estimated average treatment effect, reducing the standard error around the estimate to 0.0003. The precision of the estimated average treatment effect increases as the sample size increases, but this elevated precision does nothing to reduce the fact that 40 percent of the firms still have a negative individual firm treatment effect.

*** INSERT TABLE 1 ABOUT HERE***

While the authors provide strong evidence of a positive, causal effect of eponymy, what precisely should the entrepreneur-strategist do with this well-identified causal result? These very strong average treatment effects simply mask the potential for substantial heterogeneity in the individualized treatment effects. Fortunately, there are numerous techniques to estimate such individualized treatment effects, allowing us to examine firm-specific heterogeneity in this estimate. To explore this, we utilize the Generalized Random Forest method (Athey, Tibshirani, & Wager, 2016) which uses machine learning approaches to estimate heterogenous treatment effects. In this case, the Generalized Random Forest estimates the treatment effect of eponymy for each firm in the sample.[3] This sub-sample is represented in column (2) of Table 1. This 2010 sub-sample shows that eponymy is correlated with a 2.4 percentage point increase in ROA in the cross-section and again is highly significant. Using this sub-sample with the Generalized Random Forest technique we estimate the treatment effect of eponymy for *each individual firm* in the 2010 sub-sample. Figure 1 shows the distribution of individualized firm-specific treatment effects of eponymy.

*** INSERT FIGURE 1 ABOUT HERE***

[3] To limit the computational burden of the Generalized Random Forest we take a single-year cross-sectional sample from the data. We choose 2010 since it was the year with the most observations in the data set. The result is approximately the same as column (1) in Table 1. Further we reduced the industry codes from three digit to single digit and eliminated industries or countries where less than 100 firms were present. Again, these are efforts to reduce computational burden.

While Figure 1 shows that, on average, this effect is positive, using our best techniques to estimate the individual firm effect from the original data from Belenzon, Chatterji, & Daley (2017) we also find that there is a broad distribution of outcomes—both positive, and negative. Approximately 39 percent of the firm-level estimates are negative, and 61 percent are positive estimates. On average there is a positive effect, but even in a large sample, many firms are estimated to be negatively impacted by eponymy.

Of course, the strategy econometrician may infuse the model with moderators to explain such heterogeneity. But the challenge remains that unless moderators can capture a significant portion of the variation in Figure (1) we are left with the same problem. Sometimes the moderating effect will be positive and sometimes negative, and the individual strategist is left with little basis by which to know which effect occurs when. While it is impossible to say paper by paper how much of the variation in the distribution of treatment effects is explained by moderators, within the context of the eponymy exercise we find that very little of the variation in the treatment effects is explained by the moderators provided. Column (1) of Table 2 predicts the individualized treatment effects based on the control (and potential moderating) variables, showing that less than 5 percent of the variation in *treatment effects* is explained by these moderators. More generally, to render our empirical estimates of a common model relevant to individual entrepreneurs and strategists through the addition of moderators is a tremendously tall order. The burden for the empiricist is to capture the full breadth of firm-specific strategic nuance regarding the treatment effect of interest—essentially to develop a grand model and then empirically demonstrate that such a model encompasses all firm-specific models related to this particular treatment.

*** INSERT TABLE 2 ABOUT HERE***

Within this empirical example, we find it quite unlikely that the field of strategy could build such a common model to provide entrepreneurs thinking of naming their firms after themselves meaningful guidance on what to do. Could these entrepreneurs simply use the average treatment estimate in isolation to make this strategic decision? Given the variance we demonstrate, doing so has a high likelihood of yielding poor results. Therefore, the empirical exercise, while tremendously fascinating to the academic field and perhaps even the general population, provides limited guidance to the strategist-entrepreneur beyond "this is an important decision to think about". So much is unexplainable by the data alone. This isn't because the research conducted by strategy scholars is bad, or even that the entrepreneur is necessarily unaware of the research. The results in this paper (and perhaps much of our empirical work in strategy) are valuable, but perhaps not for the purpose of informing a practitioner strategist about what to do.

For example, Belenzon, Chatterji, & Daley (2017) has been used by Guzman and Stern (2020) as an important component to estimate changes in the quality of new ventures started in the United States—leading to relevant findings for policymakers.[4] However, for our fictive entrepreneur, these findings from Belenzon, Chatterji, & Daley (2017) have limited application precisely because the study uses a public policy method rather than an individualized, context-specific method.

[4] Guzman and Stern (2020) find that high-growth potential ventures may be concentrated in certain periods, driven by favorable institutional and economic conditions. The research underscores the role of institutional and regional conditions in shaping both the quality and quantity of entrepreneurship and contributes to a literature that points to the need for targeted policies that can foster high-quality entrepreneurial ventures, rather than merely increasing the number of startups (Stenholm, Acs, & Wuebker, 2013).

All the above leads to the central conclusion that empirical strategy faces a relevance problem precisely because it seeks to estimate a common model—one that implicitly or explicitly seeks to provide guidance to all agents—to all strategists or entrepreneurs. While our standard empirical work seeks to address relevant variance in response through moderators or discussions of boundary decisions, the fundamental assumption is, as Tom Sargent has articulated, that all agents are inside the model and share the same model with the econometrician, and that somehow this common model can capture all relevant variation in the effect of strategic choices.

Our contention is that this approach is necessarily destined to provide practitioners—entrepreneurs and strategists—with limited insight. Before turning to a discussion of how empirical strategy might address its relevance gap, we first review what we see as the entrepreneur-strategist's aspirational task—to build a highly firm-specific model.

## BUILDING FIRM-SPECIFIC AND PROBLEM-SPECIFIC MODELS

While empirical strategy seek to build a common model of strategic choices applicable across firms and new ventures, the central task of an entrepreneur or strategist is to build a firm-specific causal model— one that predicts how various decisions and choices will affect performance for their business (Felin & Zenger, 2016, 2017; Nickerson & Zenger, 2004; Wuebker, Zenger, & Felin, 2023). Thus, while the strategy scholar often seeks to generate a general performance equation, the entrepreneur-strategist seeks to build a firm-specific equation.

Of course, for any model to be useful, it must simplify the world and provide direction and focus to a set of choices. Therefore, the model that a firm seeks to build is not a representation of a firm's entire surrounding performance landscape (Gavetti & Levinthal, 2000) but rather is something far more focused. It often begins with problem identification, and proceeds to problem formulation, and a theory of how to solve it. At a high level, a strategist believes that solving this problem will enable the firm or venture to achieve higher performance. The model that results reflects the theory and presents a hypothesized causal path to solving the problem—essentially a firm-specific model of the path to value creation. The problem finding and problem-solving literature in strategy closely links to this approach, as it examines how economic actors identify and formulate problems as a path to value creation (Felin & Zenger, 2016, 2016; Gavetti, 2012; Nickerson & Zenger, 2004). This work particularly emphasizes the initial diagnostic phase as crucial in the strategy-making process, suggesting that problem identification is not merely a precursor to solving but a significant part of the strategizing process itself (Cummings & Nickerson, 2021; Leiblein & Macher, 2009; Nickerson, Wuebker, & Zenger, 2017; Nickerson, Yen, & Mahoney, 2012). The aspirational result is a model that guides strategic decision-making.

This type of problem-specific model building also resonates with the practitioner-oriented work of Richard Rumelt, who observed that "a great deal of strategy work is trying to figure out what is going on. Not just deciding what to do, but the more fundamental problem of comprehending the situation" (Rumelt, 2011). In many ways, this process of diagnosis for the strategist is analogous to approaches employed by other knowledge-intensive professions like medicine or engineering. Just as a doctor cannot prescribe a treatment without first understanding a patient's condition, a strategist or entrepreneur must first diagnose the current state—the environment, competition, internal capabilities, and unique challenges faced by a particular firm—before making informed decisions about how to proceed. With a well-diagnosed problem, the physician or engineer composes a theory or model about how to solve it. We see this as the central task of economic actors—to build models about how to solve problems and thereby elevate performance.

## HOW CAN STRATEGY RESEARCH CONTRIBUTE TO FIRM-SPECIFIC MODEL BUILDING?

Given strategists' focus on firm-specific model building, how can strategy research most effectively contribute to this model building process? In the discussion below we highlight four forms of strategy research that are of notable value to the firm-specific, model-building manager. These are meant solely to be exemplary, not exhaustive. First is research that develops firm-specific model building processes, especially work that empirically establishes their efficacy. Second is research that provides a deep understanding of the mechanisms that drive causal results, that explain heterogeneity in effects and highlight the boundary conditions that constrain them. Third is empirical research that reveals the structure of complexity that underlies strategic decisions (e.g., Leiblein et al., 2018)—research that helps managers understand which choices are complements and which are substitutes or where paradoxes or tradeoffs in strategic decision-making lie. Fourth is empirical case work that illustrates firm-specific model building efforts. We briefly discuss each of these forms of empirical research and provide examples.

### Firm-Specific Model-Building Tools and Processes

Research that crafts model-building tools, techniques and frameworks and establishes their efficacy are of considerable value to the strategist. Our field has a history of developing such tools—tools which have proved highly impactful to strategists (Barney, 1991; Christensen & Bower, 1996; Porter, 1981; Rivkin, 2000). Many of these frameworks are featured in classrooms and deployed in case discussions, serving as conceptual devices that help structure the diagnostic process and offer a lens through which to analyze competitive dynamics and organizational positioning. Popular press books that elaborate on these frameworks are regularly cited by real-world strategists as having influenced their thinking (Christensen, 1997; Christensen, Hall, Dillon, & Duncan, 2026; Helmer, 2016; Porter, 1985, 2008; Prahalad & Hamel, 1990; Rumelt, 2011). These tools have frequently been built from foundational academic theories such as IO economics, rational expectations, or Schumpeterian innovation, and then translate these theories for practitioner application.

That said, these tools remain largely diagnostic of the industry or of resources, or they are taxonomies of strategy types or frameworks that explain industry dynamics. As such, they provide useful inputs to model building rather than a model building process itself. And, despite the enduring value of many of these classic frameworks, we have seen little progress on framework development coming from the field of strategy since the late 1990s.[5] Instead, the field's attention has largely shifted to generating fine-grained insights about these classic frameworks without challenging or evolving the underlying theoretical assumptions that guide them (Leiblein & Reuer, 2020). Early-career researchers, rather than being encouraged to develop new frameworks, are steered toward safe, data-driven projects that may result in precise—but ultimately narrow—practical contributions to the literature. We wonder: is the lack of development of new, influential frameworks a result of lower expected returns to developing

[5] Imagine that you are an alien dropped into an academic institution charged by its superiors to provide a report on human progress in the domain of strategic management. Such an observer might reasonably conclude that the core theoretical work in the field was completed in the 1990s and that everything since has been an exercise in refinement. After conducting a comprehensive review of the literature, our dutiful alien visitor could be forgiven for reporting back to the mothership that, in their view, the field of strategy's intellectual output today now revolves around validating and refining existing models, rather than breaking new ground in theory or practice.

them because such groundbreaking work is a high-risk career choice, or because new frameworks offer low intellectual returns?

We encourage work that helps managers make diagnoses and progress toward developing firm-specific theories. We applaud the burgeoning empirical work from scholars (and their associates) at the Bocconi University ION Lab that trains founders to build theories and conduct theory-guided experiments and documents the efficacy of these treatments (Camuffo et al., 2024a; Camuffo, Cordova, Gambardella, & Spina, 2020; Coali, Gambardella, & Novelli, 2024; Novelli & Spina, 2024a) and studies like Yang, Christensen, Bloom, Sadun, and Rivkin (2024) that use strong causal methods to document how the adoption of specific strategy practices, including problem formulation and hypothesis development, elevate performance. This style of research, which uses strong causal empirical methods to test the efficacy of tools or various forms of conceptual training in firm-specific model building, seems highly relevant to practicing managers.

The development of firm-specific model building tools is of course also quite valuable to strategists. Nickerson and colleagues' tools on framing and formulating problems (Baer, Dirks, & Nickerson, 2013; Cummings & Nickerson, 2021; Heiman, Nickerson, & Zenger, 2009; Nickerson et al., 2017, 2012), Rumelt's work on diagnosing the crux of problems and formulating firm strategy (Rumelt, 2011, 2022), and frameworks and other tools designed for model and theory-building for both established firms (Felin & Zenger, 2017; Sorenson, 2024; Zenger, 2013) and new ventures (Felin, Gambardella, Novelli, & Zenger, 2024; Felin, Gambardella, & Zenger, 2020) are all focused on providing this model-building guidance. Tools that help managers examine their firm-specific context and move from diagnosis to model building also resonate quite deeply with managers. What we might call a theory or a model (and what Rumelt (2011) might describe as a *guiding policy*) acts as a bridge between a firm's diagnosis of its environment and the concrete actions it takes. These theories, models, or guiding policies are not detailed plans. Rather, they provide an underlying causal logic that helps managers make consistent, adaptive decisions that steer the firm toward its strategic goals. They also provide a principled approach that helps managers make coherent decisions about a host of "downstream" choices (Nickerson et al., 2017; Novelli & Spina, 2024b; Wuebker et al., 2023).

Our broad concern with current empirical work is that rather than providing insights that help managers develop a coherent, firm-specific strategy to guide choices, empirical strategy has essentially taken the opposite approach—attempting to document every possible effect and consideration across a wide range of contexts. The result of all this effort ultimately leaves the strategist to sift through a haystack of regressions hoping to find a firm-specific insight.

**Mechanisms, Boundary Conditions, and Generalizability**

Firm-specific model builders are far from disinterested in causal identification. Rather, they are interested in more than the population-level average treatment effects that result. Their interest is in something more refined—knowing the firm-specific average treatment effect. The strategist's ultimate goal is to not merely diagnose problems or craft a guiding policy, but to ensure that these steps lead to coherent action—actions that collectively create and capture value in a consistent, focused manner (Rindova & Courtney, 2020; Rindova & Martins, 2021; Rumelt, 2011; Zellweger & Zenger, 2023). The field of strategy can also support real-world strategy formulation through work that bridges the gap between abstract models and frameworks and/or the empirical recommendations grounded in the average treatment effect to produce a context-sensitive, firm-specific plan of action.

The firm-specific model builder ideally also wants a causal model, but the data to estimate this model precisely is unavailable. However, the strategist or model-builder does appreciate all information that helps calibrate these firm-specific average treatment effects and builds confidence in the precision of such estimates. What types of useful information might an econometrician committed to both precision and managerial relevance provide?

Clearly, work that highlights substantive interactions with the treatment variable are of use in these efforts—as is related empirical work that estimates effects for sub-samples. For example, empirical work that documents boundary conditions, or even provides an econometrician's speculation about boundary conditions, are useful to the firm-specific modeler. Complier analysis, and more broadly a more sincere focus in empirical strategy on heterogeneity rather than identification (e.g., Atanasov & Black, 2021; Camuffo et al., 2024a; Gaessler, Harhoff, Sorg, & von Graevenitz, 2024; Yildirim, Simonov, Petrova, & Perez-Truglia, 2024) may also help address the strategist's need for insights that transcend the average treatment effect paradigm. Work that carefully pinpoints the underlying mechanisms that drive estimates of average treatment effects are of particular interest to firm-specific model builders. Understanding mechanisms helps firm-specific modelers consider whether the identified mechanisms have application in their specific setting. However, from the example presented above, the ability to make prescriptive recommendations from much empirical work in strategy to actual firm decision-making is limited. Indeed, Table 2 reveals that the ability to predict the firm-specific treatment effects, even in an extremely large dataset, is quite limited.

A common push from journal editors to elevate the practical application of authors' work encourages authors to elevate the generalizability of their empirical findings, perhaps by developing a more broadly representative sample. But such advice is of uncertain value to the model builder and may even reduce the utility of empirical estimates. To illustrate this point, consider Blake, Nosko and Tadelis (2015) in the marketing field. The authors show that in the context of eBay, randomly eliminating search engine spending in certain markets has no causal impact on eBay's profitability. For a decision-maker at eBay, this research is important, supported by clear causal evidence, and has clear strategic application. Of course, it is much less clear that this incredibly precise estimate using eBay data produces findings that apply anywhere other than eBay (and, perhaps, only this version of eBay and not a past version or a future version). In evaluating the usefulness of these results, any other firm is left to consider how close their strategic setting is to the circumstances at eBay. The paper provides useful guidance to estimate the firm-specific effect at eBay, but it would be foolish to think the eBay estimates directly apply to their own circumstances.

Now let's imagine an alternate branch of reality where, during the refereeing process, the authors of Blake, et al., (2015) are asked to generalize their findings to improve their relevance. Let's further imagine that the authors agree to do so, conducting the same experiment across one hundred other firms, and find—on average—that advertising on search engines has a modest, positive, causal impact on firm profitability. While these findings might be viewed as more generalizable, they are—if anything—*even less useful* to a model builder. The results of the paper are not any more directly valuable to a manager outside of eBay, but now even eBay does not know where it falls in the authors' sample! If these methodological changes are implemented, a decision-maker at eBay would no longer be able to use the results to guide their strategic decision-making, and those model builders outside eBay can no longer simply ask how similar their business is to eBay and thus evaluate the relevance of the estimated results. Put differently, the field of strategy's commitment to precisely estimating Equation 1 in a way that is causal, precise, and increasingly generalizable—while perhaps quite informative to the academic field that seeks a broad understanding of empirical patterns—has, in this pursuit of generalizability, become less relevant to the real-world entrepreneur or strategist.

Many opportunities exist for empirical strategy to fine-tune its approach in ways that improve its relevance. For example, rather than solely focusing on aggregate patterns across multiple firms, empirical work might simply present firm-specific results as a use case, and then offer guideposts or templates that guide firms in how to conduct internal experiments on their own data, test hypotheses on relevant metrics, and interpret results within their context. Again, another relatively low-effort adaptation would be to address and highlight mechanisms, interactions, and boundary conditions, thereby aiding interpretation and firm-specific application. Through delineating the contexts in which empirical findings are most applicable, empirical strategy can offer more tailored guidance to strategists. Of course, this approach necessitates a shift in perspective, away from seeking universally applicable "truths" to acknowledging—and emphasizing—the contingent nature of strategic effects. Rather than using methodological advances to obsess over identification, empirical strategy could use its tools to evaluate the heterogeneity of effects and seek to understand the underlying variance.

Notably, this call for an increased focus on firm-specific estimates in empirical analysis does not necessitate a wholesale change of approach—rather, it is a re-direction of attention away from a muscular elaboration of average treatment effects and, instead, focuses on using those same tools to identify relevant subgroups and illuminate tailored strategies that connect to unique organizational or contextual characteristics. Progress in elevating the practical relevance of strategy research, we suggest, will not come from the field's current obsessions with causal identification. Rather, progress will occur through leveraging its empirical toolkit to formulate and refine theories that accurately describe the reality that strategists face. Real-world strategists—interested in interacting with and manipulating the firms they lead in ever more sophisticated ways—will likely be quite interested in empirical work that has clear, specified boundary conditions, and offers direct guidance as to how and when to apply strategy's theoretical insights.

**Complements, Substitutes, Tradeoffs and Paradoxes in Internal Choices**

Firm-specific model builders are also interested in understanding more than the influence of an isolated treatment or effect. They are particularly interested in the structure of interdependence that underlies entire sets of choices. They are interested in understanding choices which are complements and those which are substitutes, or settings where there are significant tradeoffs in strategic decision making. In contrast to policy recommendations aimed at broad populations, coherent action in strategy demands an understanding of the firm's specific environment and the coordination of actions that reinforce each other. Aided by a firm-specific theory, coherent action illuminates a pattern of interlocking moves that reinforce each other and collectively strengthen the firm's position (Camuffo, Gambardella, & Pignataro, 2024b; Felin & Zenger, 2016; Zenger, 2013). Strategies as models or theories should guide sequences of choices or treatments (e.g., Van den Steen, 2017)—choices and treatments that are interdependent or complementary in generating performance.

There is abundant work in the strategy and organization theory fields that highlights concepts of fit or documents principles of fit. Much of the work in transactions cost economics is fundamentally about highlighting principles of fit and how fit elevates performance (David & Han, 2004; Nickerson & Silverman, 2003; Williamson, 1975, 1985). This literature describes principles to guide sets of decisions. For instance, this work suggests that a decision to pursue a strategy that demands the composition of assets and activities that are unique or co-specialized benefits from organizational design choices that involve vertical integration or perhaps alliances (Argyres & Zenger, 2012; Dyer, 1997; Wuebker et al., 2023). Milgrom and Roberts' theoretical work and descriptive empirics (1990, 1995; 1992) highlighting inherent complementarities among choices is of clear relevance to firm-specific model builders. The management practices literature (Bandiera, Guiso, Prat, & Sadun, 2011; Bloom, Genakos, Sadun, & Van Reenen, 2012; Bloom, Lemos, Sadun, Scur, & Van Reenen, 2014; Bloom, Sadun, & Van Reenen, 2010) is similarly of clear relevance to the model builder, as is Ichinowski, et. al.'s (1997) work highlighting clusters of practices within steel firms that affect productivity outcomes. These efforts need not compromise empirical rigor. Empirical methods by Athey & Stern (1998) to document evidence of complementarities are highly useful. As evidenced by the work of Blader, Gartenberg & Prat (2020), Hong, Kueng & Yang (2019) and Sandvik, Sauoma, Seegert & Stanton (2020), studies providing evidence of complementarity among choices need not sacrifice the use of strong causal methods. Documenting complementarities among choices that support elevated performance is of value to the model builder, who can use this information to develop a deeper understanding of how bundles of factors may shape the performance of their own enterprise.

Empirical work that documents empirical tradeoffs or paradox is also of value to the model builder. For instance, the literature on organizational ambidexterity that explores both the complementary relationship between exploration and exploitation and the tradeoffs and tensions that exist in attempting to generate both through organizational design and leadership is highly useful (e.g., Raisch, Birkinshaw, Probst, & Tushman, 2009). Again, while such research does not deliver precise guidance, it provides the strategist with a clearer understanding of tradeoffs and relationships among strategic choices. Similarly, work that explores the tradeoffs that model builders face in strategically pursuing uniqueness can also be useful. Uniqueness in positions or resources is central to essentially all stories of value creation, so understanding the tradeoffs that uniqueness presents in the form of imposing higher information costs, paying a higher cost of capital, or enjoying beneficial knowledge spillovers (Fan, Litov, Yang, & Zenger, 2024; Litov & Zenger, 2011) is also highly relevant to the model-builder.

**Case Work That Supports Firm-Specific Model Building**

There are at least two ways that case work could be useful to the strategist. The first is to to develop cases that capture simple examples of models that could be analogous to the model the strategist seeks to build—i.e., capturing broad archetypes that aid strategists in developing analogical reasoning and pattern matching skills. The second would be case examples of the model building process itself.

Rumelt himself approaches the diagnostic process by grounding his insights in observations of managerial activity and generalizing through cases or analogies (Rumelt, 2011, 2022). Real-world strategy-makers often follow a similar path, using analogical reasoning to draw parallels between seemingly unrelated situations to inform their decision-making (e.g., Gavetti, Levinthal, & Rivkin, 2005; Gavetti & Menon, 2016; Lovallo, Clarke, & Camerer, 2012; Schilling, 2018). Indeed, a signature strength of a case studies is that they offer managers insight into strategic decision-making by catalyzing analogical reasoning, allowing them to identify firms that look like their own and facing problems like those that they are facing. They can use insights from the lessons elucidated in the case to develop their own firm-specific model and, thus, engage in firm-specific diagnosis fine-tuned to their particular setting.[6]

For example, the complementary nature of strategic choices is exemplified in the Lincoln Electric case, which demonstrates how a carefully aligned set of organizational practices—such as internal ownership, internal promotion, high bonuses, and flexible work rules—fostered significant productivity and performance gains. Similarly, Porter (1996) highlights how Ikea and Southwest Airlines employ strategies built on coherent, interdependent choices to achieve sustained competitive advantage. Siggelkow (2002) provides a comparable example with Vanguard, where a low-cost structure is bolstered by complementary choices, including conservatively managed index and fixed-income funds, direct distribution channels, and transparent customer communications. Siggelkow's (2017) examination of Liz Clairborne also offers insight into how a set of interdependent choices—when aligned properly—creates a superadditive effect.

The development of new cases—those that support the development of analogical reasoning and those that illuminate the process of model-building itself are likely to be quite valuable to real-world strategy makers. This case-based combination of a firm-specific problem and a framework we describe here balances "managerial relevance and academic rigor…making academic solutions tangible to managers" (Hoopes, Madsen, & Teece, 2022: 11). Indeed, one of our field's primary touchpoints with real-world strategic decision-making is the cases that have been written and deployed in the classroom. Unfortunately, this task has disproportionately fallen on one institution—Harvard Business School. If we are indeed serious about influencing real-

[6] Rumelt's case-based diagnostic process has been described as "process of inquiry leading to trenchant observations" (Hoopes, Madsen and Teece, 2024: 13). However, as noted by Teece, one of the challenges of this ground-up, "…"Rumeltian" case-specific approach is that you have to be as clever as Dick to get full purchase from it. A framework, on the other hand, provides more guidance." (Hoopes, Madsen and Teece, 2024: 13).

world strategy, the field may find making broad-based investments in a case-based diagnostic toolkit more of a priority and rewarding case writing that contributes to advancing analogical reasoning in real-world strategy makers.

In an important sense, coherent action is where the academic and practical sides of strategy meet. While empirical research may offer insights into potential effects of various choices, these insights are only meaningful if they can be woven into a coherent, actionable framework tailored to the firm's unique situation. By refining methods that support firm-specific experimentation, hypothesis testing, and iterative problem-solving (Agarwal et al., 2023; Camuffo et al., 2024a; Coali et al., 2024) strategy research can offer valuable guidance not only on what works in general, but on how individual firms can achieve their unique goals through coordinated and mutually reinforcing actions.

## CONCLUSION

Unlike labor or development economics, where the policy-making practitioners seek advice on designing interventions intended to help a broad swath of the population, the strategy practitioner has a different aim. In contrast to broad interventions informed by the average treatment effect paradigm, the strategist is singularly focused on building sets of complementary choices that uniquely position a particular firm to create and capture more value relative to their competitors. For a real-world strategist, estimating a population-level average treatment effect of a singular choice or treatment provides little value. The strategist is not interested in average treatment effects, but highly localized firm-specific treatments—treatments that interact with many sets of other treatments and choices that in many cases have yet to be made and thus depend on how other choices are made. It is this feature of strategic actors that complicates the empirical challenge faced in rendering strategy research useful to the strategist.

Our contention is that there is little hope of providing firm-specific guidance from papers that estimate population-level treatment. In this paper we have argued that the field of strategy's pursuit of empirical precision explains—at least in part—its lack of relevance to real-world strategists. This perspective implies a dramatically different conception of empirical strategy than the one the field has converged on—an approach that we argue is fundamentally incapable of solving the relevance problem. In our view, the field's proposed remedy—an increased commitment to empirical precision—only exacerbates the problem, because this approach represents a fundamental misalignment with the needs of practitioners. Even if an estimate of the effect of such a choice on performance is precise and well-identified, those estimates are only more of what they were: weighted averages of massively varying firm-specific treatment effects. In a field where the core insight is that unique strategies drive heterogenous performance, estimating population-level average treatment effects is of limited usefulness. Indeed, our view is that the trend in empirical strategy towards ever more accurate identification and estimation can only exacerbate the problem. Progress in elevating practical insights from strategy research, we suggest, will not come from the field's current obsession with causal identification. Rather, progress will occur through leveraging strategy's empirical toolkit to help strategists formulate and refine their own theories that accurately describe the reality that these strategists face.

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## TABLES AND FIGURES

**Table 1**

| Dep Var: Return on Assets | (1) | (2) |
|---|---|---|
| Eponymy Dummy | 0.030*** | 0.024*** |
| | (49.43) | (24.57) |
| Log(Assets) | 0.011*** | 0.013*** |
| | (50.74) | (65.86) |
| Log(Num Shareholders) | -0.025*** | -0.026*** |
| | (-69.64) | (-34.94) |
| Equity Dispersion | -0.020*** | -0.021*** |
| | (-32.95) | (-16.37) |
| Adjusted $R^2$ | 0.091 | 0.069 |
| Country FE | YES | YES |
| SIC 3 Digit FE | YES | NO |
| SIC 1 Digit FE | NO | YES |
| Year FE | YES | YES |
| Sample | 2002-2012 | 2010 |
| Observations | 6193610 | 916642 |

$t$ statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

**Table 2**

| Dep Var: Predicted Tau | (1) |
|---|---|
| Log(Assets) | -0.000050 |
| | (-1.06) |
| Log(Number of Shareholders) | 0.000041 |
| | (0.23) |
| Equity dispersion | 0.000038 |
| | (0.13) |
| Adjusted $R^2$ | 0.043 |
| Country FE | YES |
| 1 Digit SIC SIC FE | YES |
| Observations | 916642 |

$t$ statistics in parentheses

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

**Figure 1**