

Strategy as Economics versus Economics as Strategy

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I.

Clearly, strategy and economics differ along one critical dimension. Strategy is concerned descriptively with firms and normatively with the tasks of managers, whereas economics is concerned descriptively with the entire economic system and normatively with the effective functioning of that system. Nonetheless, the overlap is considerable, and both management scholars and economists have ransacked economic thought for inspirations to strategic theory and managerial practice.

At one time, of course, there was little distinction between economics and management as intellectual disciplines. In the 1920s, one could find books like Dexter Kimball's *Industrial Economics*, which attempted to embrace both the descriptive and normative elements of industrial practice in the spirit of Smith, Babbage, and Marshall. But synthesis cannot long withstand the forces of the extent of the market. By the 30s, economics was already producing a specialized and highly abstract variant of price theory that was to become the basis of the field of industrial organization. Later, after World War II, management (and indeed *strategy*) also began to coalesce as a specialty. Like many other parts of the university, this new area came to suffer — and sometimes eagerly to seek — the “imperialism” of a more abstract and highly developed economics research program.

This essay will examine and evaluate the history of borrowings from economics by management scholarship. More interestingly, perhaps, it will suggest that perhaps the process ought to be reversed. Economics as it is now practiced would do well to learn from strategy.

II.

As Loasby (1976, 1989), Moss (1984), and others have argued, what we think of as “Marshallian” price theory today is in many ways more Pigovian than it is Marshallian. Marshall thought in population terms, and constructed a representative firm that reflects the characteristics of the population of firms as a whole rather than the characteristics of any particular firm. By contrast, the “theory of the firm” of price theory, which Joan Robinson, Edward Chamberlin, and others built during the 1930s on a Pigovian foundation, begins with identical idealized firms and then builds *up* to the industry by simple addition. Thus does modern-day price theory start with firms as production functions, each one identical, and each one transforming homogeneous inputs into homogeneous outputs according to given technical “blueprints” known to all. One effect of these assumptions has been to reduce the margins on which firms operate to two only: price and quantity. This in turn has led to the notion of “perfect”

competition, in which a technically desirable set of assumptions replaces the common-sense notion of competition¹ (Hayek 1948; McNulty 1968).

In this theory, the discipline of the market consists entirely in limiting the discretion producers have in setting the prices they will charge. One competes not by taking action but, in a real sense, by being unable to take action. Perfect competition limits discretion completely: no action a firm takes can affect the price it can obtain for its product. A theory in which managers are powerless would seem a poor basis for an imperialist assault on management. But the idea of perfect competition carries with it a set of theories in which firms do have discretion of a sort: theories of “imperfect” competition. It has been almost exclusively among these theories that management has rummaged.

The most famous version of imperfect competition, of course, is monopoly, in which there exists, for reasons unexplained, only one firm selling a given product in a given market. Unlike the perfectly competitive firm, which would lose fully all its customers if it charged more than the going rate, the monopoly has discretion over price. Nonetheless, the monopolist is still constrained by the assumption of rationality, and the theory offers the determinate prediction that the rational monopolist will set a price where

¹ As Loasby (1989) has pointed out, the theory of perfect competition is not some legacy of Smith and Marshall that was “corrected” by the imperfect-competition theory of Robinson and Chamberlin. Perfect competition was in fact *invented* by the theorists of the 1930s, and it is in fact quite different from the conceptions of Smith and Marshall.

marginal revenue equals marginal cost. This does not open up rich possibilities for offering advice to managers.

Which leaves the other principal version of “imperfect” competition, oligopoly. Here the number of firms involved ranges between two (duopoly) and perhaps a half-dozen, which is arguably a closer approximation to many markets we observe in the world. Unfortunately, however, that minimalist bit of verisimilitude is enough to destabilize the determinate neatness of the model. We know what the perfectly competitive firm will do, since it is completely constrained; and we know what the monopolist will do, since it is rational and dependent only on itself. But an oligopolist is not completely constrained by the situation in which it finds itself in. What is optimal for the oligopolist depends not only on variables subject to its control but also on variables subject to the control of its competitors. The oligopolist’s choice is governed by its *expectations* about the behavior of those competitors. And the nature and sources of expectations is a question beyond the scope of the neoclassical program. There is thus nothing in the logic of the oligopolist’s situation, and nothing in the kit bag of neoclassical optimization and equilibrium, that can close the model in anything but an *ad hoc* fashion (Latsis 1976).

This is a logical problem, one that can’t be fixed. Herbert Simon (1976, p. 140) recognized this when he declared oligopoly “the permanent and ineradicable scandal of economic theory.” There are only two choices. Either (1)

abandon the model of identical production functions interacting in price and quantity space or (2) ignore the problem and push on undaunted into ad hocery. Most have chosen the latter path, with an ironic result. Because of its indeterminacy, a situation of oligopoly offers wide scope for advice to managers. But price theory is powerless to generate any of that advice.

The irony applies as sharply to antitrust advice as it does to managerial advice. The former is of course the principal normative focus of what is somewhat misleadingly called industrial organization. By limiting its field of vision to price, quantity, and the number of firms in the industry, the price-theory model funneled the economics of antitrust into an obsessive concern with market structure, interpreted to mean, well, the number of firms in the industry (Coase 1972). Interestingly, this obsession was strongest within the traditional of analysis most comfortable with the *ad hoc* and least concerned with the niceties of theory, namely the “structural” or Harvard School of Edward Mason and Joe Bain (1959). This school was always anxious to count the number of firms in the industry and keen to detect “barriers to entry” at the slightest provocation. Speaking of economists in this tradition, Coase long ago jibed that “if an economist finds something — a business practice of one sort or another — that he does not understand, he looks for a monopoly explanation” (Coase 1972 [1988, p. 67]). Instantiated in case law and administrative policy, this presumption became what Williamson (1985) calls the “inhospitality tradition.”

And here of course we find the first major attempt to formulate strategy from economic ingredients. Michael Porter (1980) made a considerable splash by turning the rough-and-ready Harvard School view of what imperfect competitors ought not do (for the sake of economic efficiency) into an account of what the smart manager ought to do (for the sake of profit). Much has been written about Porter's work. But two points are in order. First, economic theory arguably played a "deep background" role in Porter's analysis, whereas much of the action in his framework actually comes from the eclectic outer core — both his own and that of the Harvard School — rather than from anything flowing directly from the logic of neoclassical industrial organization. But, second, where Porter's framework ultimately proved lacking was in its basic orientation arising out of the economic deep background. The book on Porter among students of management is that (especially in his early work) he pays far too much attention to the environment facing the firm and to how it should position itself in that environment — and almost no attention to the firm itself. Like the neoclassical firm, Porter's firm is ultimately a faceless mannequin whose fate is determined by (a somewhat manipulable) market structure.

Within management, the response to Porter took the form of the resource-based perspective on the firm. Rather than emphasizing the firm's outer environment as a source of profit, the resource-based view stresses the choices managers can make to learn, to develop new competences, and otherwise to

change the strengths the firm possesses. In this view, competitive advantage is a matter not of positioning vis-à-vis competitors but of building and creating within the firm. And the advantage conveyed is not some amorphous and ill-defined “profit” but rather a rent (or quasirent) to the possession of some distinctive competence or other resource.² This development on the management side parallels an earlier development within economics itself, namely the attack on the Harvard School by the Chicago School.³ Among Chicago’s ripostes to Harvard was the observation that instances of supranormal returns are not automatically evidence of undesirable market power but are most often rents to superior efficiency (Demsetz 1973).

If one reads the accounts of Chicago School insiders (Posner 1979), one learns that Chicago’s victory over Harvard in the marketplace of antitrust ideas came as the result of a more assiduous application of neoclassical price theory. It would certainly have been hard to find a less assiduous application of price theory than the work of Bain. Nonetheless, one could even more plausibly argue that the Chicago victory was based on *moving beyond* the simple price theory of the 1930s (Langlois 1989, p. 836; Meese 1997). Consider for example George Stigler’s (1964) theory of oligopoly. Rather than casting oligopoly in the form of (what is ultimately) a game among players who know everything except one

² For a careful analysis of the difference between returns to output restriction and returns to the possession of a distinctive resource, see Winter (1995).

another's expectations, Stigler transforms the problem to one of cartel theory. Firms wish to collude, but collusion is costly because of the incentive to defect from the cartel. The extent of successful collusion — and thus the ability of an oligopoly to approximate the behavior of a monopolist rather than that of a competitive industry — depends on the costs of policing the behavior of the players. What was a problem in price theory becomes a problem of information or transaction costs. One can see the same pattern, perhaps even more clearly, in the analysis of “vertical restraints,” which the Harvard School assumed to be obvious mechanisms of monopolization and which the Chicago School showed to be in the main efficient solutions to transaction-cost or related problems.

There is of course a tradition in economics centrally concerned with the role of information and transaction costs in economic organization. Although inspired by the early work of Coase (1937), this approach — sometimes called transaction-cost economics or, more broadly, the New Institutional Economics (Williamson 1975; Langlois 1986) — gained momentum only in the last two decades or so. It is significant that transaction-cost economics is in most minds not a more assiduous version of neoclassical price theory but precisely a response to the inadequacy of price theory. Coase's fundamental insight was that the price theory he saw developing in the 1930s not only failed to explain the interesting features of the real-world institution of the firm but actually failed to

³ Indeed, some would list the Chicago School as an inspiration to the resource-based view

explain why there should be firms at all. Coase's own explanation, that there must be "a cost to using the price system" (Coase 1937, p. 390), put the focus on costs of transacting. This left conventional price theory undisturbed, creating instead a kind of parallel theory that ignored production almost entirely in favor of exchange (Langlois 1997; Langlois and Foss 1999).

Transaction-cost economics is, of course, another area in which economic ideas have flowed into strategy. The standard-bearer has been Oliver Williamson. But even Williamson's conception of the contribution to strategy of transaction-cost economics is relatively narrow (Williamson 1991). Like the resource-based approach, he is critical of an idea of strategy based entirely on positioning against competitors. The best strategy, he says, is economizing, by which he appears to mean minimizing costs. Transaction-cost economics contributes to this endeavor by suggesting how alternative organizational structures can best govern transactions, especially in the face of opportunism and highly specific assets. And the work of Williamson and his followers has indeed proved illuminating in a number of areas of management, including corporate governance and supply-chain management.

As a far-reaching influence on strategy research, however, transaction-cost economics has its limitations. Writers in the management literature (Conner and Prahalad 1996; Kogut and Zander 1992; Ghoshal and Moran 1996), and even a

(Rumelt, Schendel, and Teece 1991, p. 8; Foss, Knudsen, and Montgomery 1995, p. 4).

few renegades in the economics literature ([Langlois and Foss 1999](#)), have criticized transaction-cost economics for its fixation on opportunism and asset specificity to the exclusion of learning and coordination as determinants of organizational form and boundaries.⁴ Despite Williamson's frequent mention of "adaptability," transaction-cost economics is in the end grounded in the same process of static optimization as neoclassical theory. It has little to say about organization — let alone strategy — in a world of rapid change ([Langlois and Robertson 1995](#)).

Indeed, some aspects of the Coasean legacy have been reabsorbed into (what has become) mainstream price theory. But this is getting ahead of the story. I have already disputed the proposition that Chicago claimed victory over Harvard because it wielded more powerfully the bright sword of price theory. What is less subject to dispute, however, is that the opponents of Chicago counterattacked with mathematical game theory, an even more high-tech weapon forged of the steel of 1930s price theory. The post-Chicago approach, as it is now called (Hovenkamp 1985; Baker 1988), could be understood as having made formal the basic approach of the Harvard School, namely, that anticompetitive effects — and, by implication, competitive advantage and

⁴ Much of the problem, in my view, stems from Coase's early (implicit) decision to cede the realm of production costs to conventional price theory, thus militating against a unified examination of how limitations on knowledge simultaneously play themselves out in both producing and transacting (Langlois 1997).

profit— may be had by “strategic” positioning in price and quantity space⁵ (Tirole 1988). Since strategic positioning is difficult in a world of perfect information, the “strategy” animating many of these models has been the strategic use of information. To the extent that models of asymmetric information imply some of the concerns (though not at all the approach) of Coase, then, it is here that some parts of the economics of organization have been absorbed within mainstream price theory. Although mathematic modelers of organization reject Williamson’s labels, there is in fact considerable overlap with transaction-cost economics, especially in the realm of opportunism and highly specific assets.

That game theory places central emphasis on “strategic” behavior raises the issue of its relevance to management scholarship, where the word “strategy” is equally central. But even the proponents of game theory have to work hard to find reasons why this approach might be of use to managers (Saloner 1991; Camerer 1991). Many would immediately point to the abstract and even rarefied character of these models as disqualifying them from serious consideration by scholars of management. In my view, however, the real problems lie in two other areas.

⁵ I don’t want to imply that the development of game-theoretic and asymmetric-information models in economics arose primarily out of a desire to attack the Chicago School. In fact, the burgeoning of such models reflects the growth in the extent of the market for the product of economists as well as the playing out of the mother lode of basic Arrow-Debreu theory. An increasing number of economists were all dressed up with high-powered techniques and

For one thing, game theory has not solved — and, of course, logically cannot solve — the problem of indeterminate expectation. In the words of Garth Saloner:

The degree of rationality assumed in game-theoretic models is often much greater than in other economic models. In game-theoretic models, each firm's optimal action depends on what it believes its rivals will do. In order to decide what to do itself, the firm must put itself in its rival's shoes and analyze the situation from its rival's perspective. The analysis therefore requires assumptions about the rival's rationality, as well as the assessment of the rival's belief about one's own rationality, and so on (Saloner 1991, p. 120).

The “and so on” is known as Morgenstern's paradox (Morgenstern 1935), best expressed by the Batman of the 1960s TV series when he explained himself to his young assistant: “yes, Robin, but I knew that he knew that I knew that he knew that I knew... .” The practice community of mathematical modelers has addressed this problem by adopting a strong methodological conventionalism in which the *ad hoc* and unjustified character of the assumptions — almost always the Cournot-Nash assumption — go entirely undiscussed.

There is a related, but larger and more important, problem of adhocery embedded in the culture of normal science that has grown up in the practice community of mathematical modelers. This trait is often encapsulated in the phrase “MIT-style theorizing,” which Eric Rasmussen characterizes this way:

had nowhere to go but into the unexplored realm of institutions that Coase had long earlier mapped out.

“the ... heart of the approach is to discover the simplest assumptions needed to generate an interesting conclusion — the starkest, barest, model that has the desired result. This desired result is the answer to some relatively narrow question” (Rasmussen 1994, p. 3). The trouble is that the “answer” such models propose are sufficient but not necessary ones, and there are an infinite number of possible sufficient explanations (Camerer 1991, p. 146; [Foss and Foss 2000](#)). Worse, the culture of practitioners relies entirely on mathematical elegance rather than empirical evidence or plausibility to sort among models.⁶ I view this as not merely inappropriate but actually *unrigorous* in the context of explaining real-world institutions. Like most things, rigor is subject to a conservation law, and the more rigor along mathematical dimensions, the less of it along other, perhaps more important, dimensions.⁷ In other disciplines, rigor would mean immersing oneself in the institution, considering and sorting through all possible alternative explanations, and delivering a measured and nuanced argument backed by a preponderance of all evidence. Think about historian David Landes (1986) explaining the factory system, or any number of examples in management.

The second problem with game theory and related models applied to management is that, since these models are essentially price-theoretic formalizations of the Harvard-Porter approach, they suffer from the deficiencies

⁶ My favorite example of a clever but screamingly hokey and implausible model is Rotemberg and Saloner (1994). Characteristically, it appeared in the *American Economic Review*.

⁷ Loasby (1976) formulates this as a rigor-relevance tradeoff.

identified by theorists of the resource-based school. Writing from the perspective of organizational economics rather than management, [Langlois and Foss \(1999\)](#) have lately criticized transaction-cost economics — in both its Williamsonian and mathematical formulations — on exactly these grounds. Following Coase, the mainstream of the economics of organization today expends considerable effort considering the limitations on knowledge and information in the process of transacting, but is content with the assumptions of perfect knowledge of production contained implicitly in the price-theoretic account of production costs. Brian Loasby makes a similar point. In his recent review essay on Milgrom and Roberts (1992), a widely cited textbook treatment of the modern economics of organization, Loasby notes that, “despite their ready acknowledgement of [Alfred] Chandler’s work, Milgrom and Roberts prefer the transaction as the unit of analysis, and do not enquire into the productive activities which a firm undertakes. The final chapter, of only ten pages, skims over technical change, team production, the creation of capabilities and organizational entrepreneurship” (Loasby 1995, p. 475).

III.

In part, the attraction of economics for management lay in its unity and standardization. The message of Thomas Kuhn (1970) is that scientific disciplines enjoy substantial “network effects,” since standardizing on a single

worldview brings benefits in lower communication costs and in a more effective coordination of the division of labor. Economics is not so much the queen of the social sciences as it is the Microsoft of the social sciences. By contrast, management theory is pluralistic, perhaps excessively so (Foss 1996). Nonetheless, many scholars concerned with business practice have tended quite sensibly to resist the intrusion of neoclassical economics. Actually existing competition fundamentally involves differences among firms and products, and neither “perfect knowledge” nor “equilibrium” makes much sense in the real world.

One can always debate the rate at which verisimilitude trades off against theoretical elegance and coherence. But, whatever the tradeoff, there remains the question of *how* to abstract. The choices that economists made in the early part of the century have created a “theoretical trajectory” akin to the kinds of technological trajectories one observes in the history of technology. In such trajectories, past choices narrow today’s options and constrain future choices. A trajectory that started out from choices appealing to economists may not be so much *too* abstract as *wrongly* abstract. Not only may the wrong choice of starting point have led down a path that is not of great relevance to strategy, a wrong choice at the beginning may also be responsible for the dead-end of oligopoly theory.

If one were to pick theoretical building blocks of relevance to strategy, what would they be? If we imagine ourselves in the same position today with respect to theory as economics found itself in the 1920s, what kind of theory would we build? A half-century ago, Armen Alchian (1950, p. 221) suggested just how to pick up the other end of the stick. The price theory of the 1930s began with the assumption of perfect knowledge and asked how firms solve a known and well-defined problem optimally. Since perfect knowledge is normally not a plausible assumption — and, as in the case of multi-player interaction, sometimes not even a meaningful assumption — economists soon found themselves allowing in homeopathic drabs of “imperfect” knowledge. This trajectory is the one I’ve just criticized. But consider the opposite approach. Start with the assumption that agents are completely ignorant and that the world is too complex for “optimization” to make much sense. And then slowly blend in some features of learning and intention.

As Alchian suggests, the starting point of this approach would be a model of blind evolutionary selection. No economist has actually ever started there, although the population ecologists have (Hannan and Freeman 1989). Early on, Sidney Winter (1964) pointed out that, for selection to work, there must be an analogue of genes in biology; and the proper analogue has in fact to do with what firms and other economic actors know — their routines. As part of their development of the evolutionary idea, Nelson and Winter (1982) refined the

notion that routines embody knowledge. The result was the beginnings of a theory of economic capabilities. Their work dovetailed with the earlier but theretofore largely unnoticed work of Edith Penrose (1959) and G. B. Richardson (1972), and began to influence a “capabilities” theory of the firm closely allied with the resource-based approach. Here, of course, we have another area in which economics has strongly influenced management theory. And we also have another irony. The main influences on the resource-based approach to strategy have been economists like Penrose, Richardson, Nelson, and Winter. But, unlike the other instances of borrowing catalogued above, the economic theory that influenced the resource-based approach to strategy has had almost no influence on *economics*.

To the extent that capabilities theory proves to be a more successful economic import into strategy than its predecessors — one more accurately tuned to the need of strategy — perhaps its success in the business school will seep over into the economics department. This is probably unlikely. But if it were to happen, economists could comfort themselves with the knowledge that they had learned not only from strategy but ultimately from themselves.

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