

RESEARCH NOTES AND COMMENTARIES

THE PHILOSOPHY OF STRATEGY

THOMAS C. POWELL*

Australian Graduate School of Management, University of New South Wales, Sydney, New South Wales, Australia

The recent increase in philosophy of science articles in strategic management reflects researchers' rising concerns with understanding and securing the field's intellectual foundations. This article argues for a proactive approach to the philosophy of strategy, and for the rejection of conventional, 'off-the-shelf' philosophies that neither contemplated, nor can assimilate, the epistemological messiness and action-connectedness of strategic management. The article responds to Rodolphe Durand's critique, revisits the logic of competitive advantage, and makes the case for a pragmatist philosophy of strategy. Copyright © 2002 John Wiley & Sons, Ltd.

My earlier article (Powell, 2001) explored the epistemological and ontological foundations of strategy research. The task was necessarily analytical and critical, but the message was redemptive. The philosophical foundations of strategy research are not, in my view, either radical or conventional—they are neither objectivist nor subjectivist, rationalist nor empiricist, positivist nor post-modern. The paper told two stories: one of why strategy research cannot rely on conventional logical and philosophical justifications, and another showing the redemptive promise of pragmatist epistemology.

The article elicited a number of responses. Some of them, like Durand's, did not agree with its

conclusions, but they agreed unanimously on the importance of understanding the field's intellectual foundations. The recent upsurge of philosophy articles is encouraging, and attests to the willingness of strategy researchers to engage in this debate (e.g., Priem and Butler, 2001a, 2001b; Barney, 2001; Mir and Watson, 2000, 2001; Spanos and Lioukas, 2001; Brønn, 1998; Kwan and Tsang, 2001).

Durand raises four objections, and I thank the *Strategic Management Journal* editors for this opportunity to respond. The objections are:

1. *Logic and competitive advantage*—that it is possible to show that competitive advantages cause sustained superior performance by using the 'INUS' logic of non-deterministic necessary and sufficient conditions.
2. *Competitive disadvantage*—that competitive disadvantage is not a useful construct when reframed using non-negative propositions.

Key words: philosophy; logic; competitive advantage; pragmatism

*Correspondence to: Thomas C. Powell, Australian Graduate School of Management, University of New South Wales, Gate 11, Botany Street, Sydney, NSW 2052 Australia.

3. *Firm heterogeneity and tautology*—that, under Kantian reasoning, the assumption of firm heterogeneity is not tautologous.
4. *Strategy and pragmatist philosophy*—that pragmatism is relativistic and postmodern, reducing strategy research to semantic debates and subjectivity.

The following sections address these objections in order, and the final section adds further observations on philosophy and strategy research.

LOGIC AND COMPETITIVE ADVANTAGE

Durand argues that strategy researchers can, with certainty and without tautology, identify sustainable competitive advantages as the known causes of sustained superior performance. To demonstrate the point, he invokes the 'INUS' condition, i.e., a necessary but non-sufficient condition for a non-necessary but sufficient cause. He then applies INUS logic to the concept of competitive disadvantage, arguing that this concept amounts to nothing more than a negative restatement of the concept of competitive advantage.

These conclusions are, in my view, unjustified. In brief, if Durand's argument is intended as a purely logical one, then it reduces to a mere *assertion* (that competitive advantages cause superior performance), and is trivial; but if, as seems likely, it is intended as a set of empirical propositions (e.g., that every alternative cause has been identified and eliminated with certainty), then the argument falls victim to a host of empirical fallacies (including the fallacy of induction), and reduces ultimately to the Bayesian formulation presented in my earlier article.

The issues are complex and potentially confusing, and the formal syntax used in my article and Durand's response do not abate the confusion. In what follows, I restate my original argument using Venn diagrams, and then show why I believe Durand's objections do not succeed.

In the original article, I did not argue that sustainable competitive advantages do not cause sustained superior performance. They may, in fact, do so, even to the extent of constituting necessary and sufficient conditions. What I did argue is that we cannot merely *assert* that sustainable competitive

advantages cause sustained superior performance, and then rely on that assertion as proof.

Consider a simple example. If Mary claims to go to the cinema whenever it rains, and only when it rains, then this claim is as shown in Figure 1(a). In logical parlance, the events 'rain' and 'Mary goes to the cinema' are *functionally equivalent*. Clearly, raining and going to the cinema are different events, so that the events 'rain' and 'Mary goes to the cinema' are not *definitionally equivalent* (tautological). Moreover, we could test Mary's claim simply by observing her cinema-going behavior, and alternative hypotheses readily suggest themselves (see Figure 1(b)). If our tests corroborate Mary's claim (Figure 1(a)), then the hypothesis of functional equivalence is not disconfirmed, and we tentatively accept that the events 'rain' and 'Mary goes to the cinema' are *empirically equivalent*.

By analogy, Joe claims that the events 'firm *x* has competitive advantages' and 'firm *x* has superior performance' are functionally equivalent (Figure 2(a)). Clearly, competitive advantages (locations, technologies, product features, etc.) are not the same thing as superior performance (market share, profit, share price, etc.), so Joe's events are not definitionally equivalent.¹ It should be possible, by observation of firms, to determine whether competitive advantages and performance are also empirically equivalent.

To conduct empirical work on competitive advantage, we develop null hypotheses. Figure 2(b), for example, parallels Mary's Figure 1(b), and allows four possibilities: (1) firm *x* has competitive advantages but not superior performance; (2) firm *x* has superior performance but not competitive advantages; (3) firm *x* has both; and (4) firm *x* has neither.

Figure 2(b) is not the only alternative model, but it is interesting for two reasons: (1) it is logically possible, and (2) it has never been tested, or even proposed, in strategy research. Researchers occasionally refer to underperformance in spite of competitive advantages—area 1 (e.g., Coff, 1999; Coyne, 1986)—but area 2 never arises as an empirical possibility. In Mary's case, we would

¹ As noted in Powell (2001), 'competitive advantage' is sometimes *defined* as superior performance—e.g., 'the ability of the firm to outperform rivals on the key performance goal' (Grant, 1998). This usage is clearly tautological. What I show above is that the relation between performance and 'competitive advantage(s)' (often in the plural) need not be tautological, but is, in the logic of strategy research, reduced to tautology.

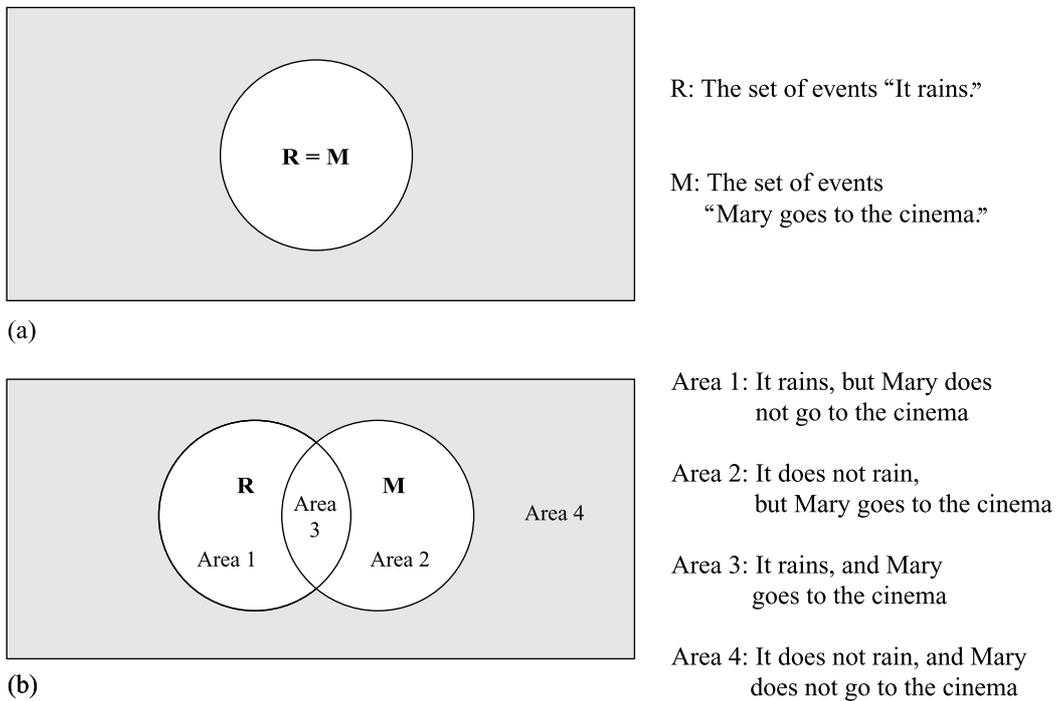


Figure 1. (a) The functional equivalence hypothesis. (b) An alternative hypothesis

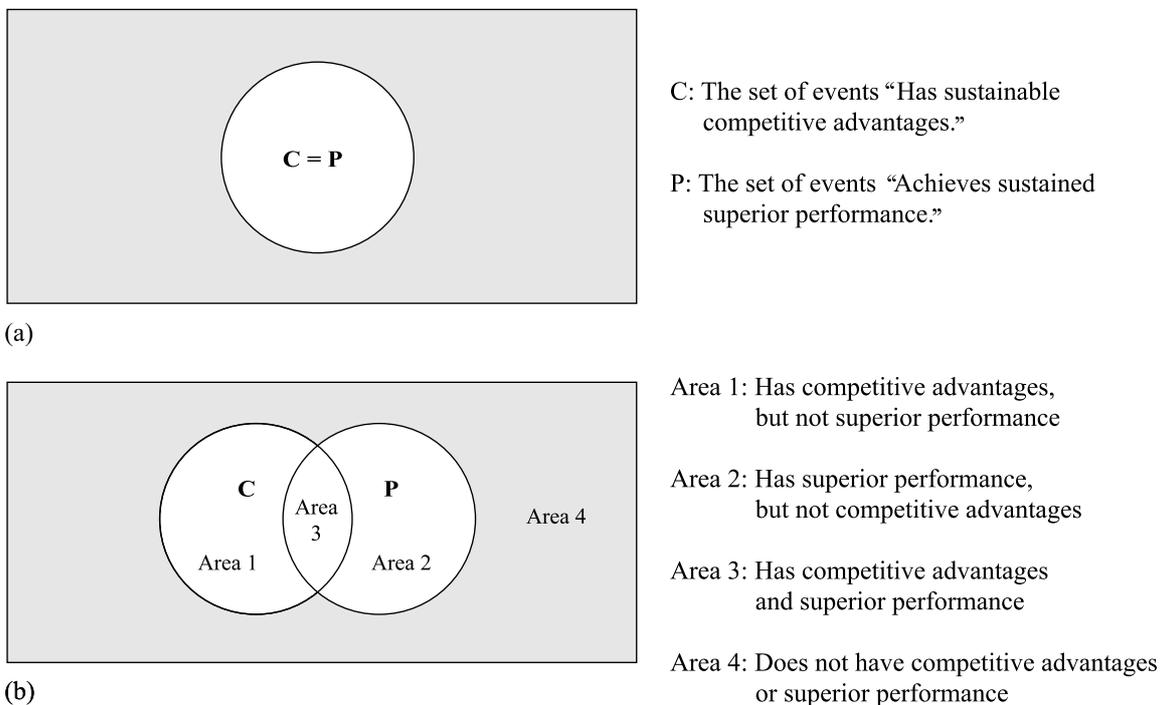


Figure 2. (a) The functional equivalence hypothesis. (b) An alternative hypothesis

never allow such an oversight—area 2 (going to the cinema on a non-rainy day) is a *logical* possibility that Mary must recognize as an *empirical* possibility. She cannot dismiss area 2 with a clever definition, e.g., ‘It may be sunny outside, but it is always raining in my heart.’ Similarly, if we remind Mary that she avoided the cinema on a rainy day, the reply ‘I think of my home as a cinema’ would arouse epistemological suspicion—‘rain’ and ‘Mary goes to the cinema’ would then be definitionally equivalent.

This is precisely what occurs in competitive advantage propositions, and nothing in Durand’s comments either contradicts or resolves this state of affairs. It is not enough to assert that competitive advantage and performance are functionally equivalent—the logical alternatives must be taken seriously and shown empirically to be false, not merely *defined* as false. As things stand, strategy research is burdened with an immense body of obfuscatory grammar—‘intangible-invisible assets,’ ‘causally-ambiguous causes,’ etc.—that, while conveying a kind of common-sense truth, obscures the fact that competitive advantage and performance are related definitionally, not functionally.

I deal directly with Durand’s INUS conditions in Exhibit A. The argument is fairly technical, but is clearer in the diagrams than in the formal expressions. As shown in Exhibit A, using Durand’s ‘house fire’ example, the INUS conditions show that, at best, competitive advantages are neither necessary nor sufficient, but rather stochastic causes of firm performance. The INUS approach is interesting, but ultimately unnecessary—the conclusion is equivalent to the Bayesian analysis presented in my original article.

COMPETITIVE DISADVANTAGE

Durand argues that, if we convert the expression ‘avoids competitive advantage’ to its presumed equivalent—‘achieves a capable organization’—then we find that ‘competitive disadvantage’ adds nothing to our existing pool of strategy concepts and relations.

The previous discussion foreshadows the weaknesses in this argument. If the causal relation between competitive advantages and performance is not definitional, then there exist, as a logical possibility, other potential causes of firm performance.

In other words, for any firm *x*, there exists a set of ‘factors’ (or ‘resources,’ ‘activities,’ etc.), some of which are competitive advantages, but others of which are *not*. A strategy theory might distinguish the two sets by their attributes. Thus, for example, competitive advantages (C) may be valuable, scarce and imperfectly imitable; whereas all other factors (A) would lack one or more of these attributes.

A performance hypothesis might assert that some subset of A-factors causes superior performance. This would contradict the competitive advantage hypothesis, but would account for area 2 in Figure 2(b)—firms with superior performance without competitive advantages. Strategy research has no such hypotheses, since prevailing theories define area 2 out of existence. But researchers can only corroborate prevailing theories by recognizing the logical possibility of alternative causes, and showing empirically—not definitionally—that these areas have no membership.

Now consider area 2—firms with competitive advantages, but without superior performance. As I noted in the earlier paper, this space would contain firms under any reasonable theory of performance. A firm’s presence in that space would *not* imply that the firm lacked competitive advantages—by assumption, the firm has competitive advantages—but that it suffered deficiencies sufficient to vitiate the effects of its competitive advantages. These deficiencies are what I called ‘competitive disadvantages.’

The expression ‘avoidance of competitive disadvantages’ does not at all resemble Durand’s ‘capable organization,’ which ‘may mean cost efficient (economic perspective), transaction efficient (transaction cost perspective), able to learn (evolutionary perspective), able to manage agency relationships (agency theory), able to play with selective pressures, etc.’ From my reading, such capabilities—even the term ‘capabilities’—are precisely what existing theories have in mind in defining ‘competitive advantage.’ As I noted in the earlier paper, competitive disadvantage, whatever it may be, is not merely the dark side of competitive advantage.

In the critique, Durand compares the role of competitive advantage avoidance to that of flammable materials in a house fire, both being necessary but not sufficient conditions (see Exhibit A). I have no objection to this comparison, as it merely

Using Durand’s ‘house fire’ example, the ‘INUS’ argument is as follows:

1. Assume the occurrence of a ‘house fire’ (f), and that flammable materials (m) are a necessary but not sufficient condition for f.
2. Assume that ‘electrical short circuit’ (e₁) is a possible and suspected cause of f, but neither a necessary nor sufficient condition for f—not necessary because there are alternative causes (arson, lightning, etc.); not sufficient because flammable materials are required.
3. Assume a finite number of other possible causes of f (e₂, e₃, . . . e_n); assume that none of these causes is either necessary or sufficient for f, and that all these causes are known and observable.
4. Assume that the conjunction of any e with flammable materials (m) produces fire, e.g., a short-circuit in conjunction with flammable materials must produce a house fire.

From the above argument, Durand concludes that, by eliminating all possible causes other than e₁, we can infer with logical certainty that a short-circuit caused the house fire, even though neither a necessary nor sufficient condition for f (or only a necessary condition ‘post factum’). By analogy, we can know with logical certainty that an identifiable competitive advantage caused firm performance.

Graphically, the argument is as follows:

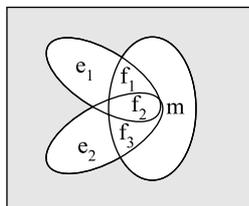


Figure A1

f: fire
 m: flammable materials
 e₁: electrical short-circuit
 e₂: arson, lightning, all other causes

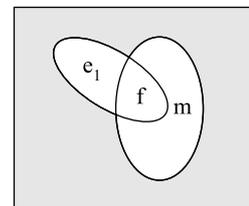


Figure A2

In A1, if a house fire (f) is observed, we can infer there were flammable materials; but we do not know which cause (e₁ or e₂) was working in conjunction with the flammable materials. As such, f₁ (short-circuit only), f₂ (short circuit and other causes), and f₃ (other causes only) are all possibilities *ex ante*. But if we can eliminate e₂ (Figure A2), then we can logically infer that the conjunction of e₁ and m caused the fire. This, I believe, is an accurate representation of Durand’s argument.

Logically, this argument assumes its own conclusion. The conjunction with flammable materials is not, in fact, necessary to the argument, and obscures its circularity. In the competitive advantage context, the above assumptions amount to the following: (1) If we observe an instance of superior performance, and (2) we assume that (only) competitive advantages cause superior performance, and (3) we eliminate all causes except competitive advantage C, then (4) C caused superior performance. We cannot deny the conclusion, but assumptions 2 and 3 betray the argument’s circularity.

Empirically, the argument also falls victim to weak assumptions (e.g., we can neither identify nor eliminate all possible alternative causes). However, it is correct insofar as it concedes that some competitive advantages may not produce superior performance, and that the advantage–performance relation is, at best, stochastic, or ‘weak implication’ (Boudon, 1974). My earlier article formulated this using Bayesian probabilities (Powell, 2001: 879ff).

Exhibit A. The INUS condition

restates my original argument. I am also sympathetic to characterizing ‘avoidance of competitive disadvantage’ as an affirmation—in a separate paper, Jeremy Davis and I proposed what I regard as a more suitable construct (Davis and Powell, 2001). Where I part ways with Durand is in his redefinition of competitive disadvantage avoidance as ‘capable organization,’ and in his apparent belief that specific competitive advantages can be known, with certainty, to cause superior performance. As

noted earlier, and as shown in Exhibit A, this belief is either logical or empirical, but is, in either case, unjustified.

FIRM HETEROGENEITY AND TAUTOLOGY

The third section of Durand’s critique makes several claims—that analytic propositions and

analytic judgments are different; that the firm heterogeneity assumption is not an analytic judgment; that if perfect competition is an axiom, then so is firm heterogeneity; that heterogeneity is falsifiable—all from a Kantian philosophical perspective.

I will deal only briefly with these objections. In using the analytic–synthetic distinction, my article made one passing reference to Kant, from which Durand seems to infer that the subsequent argument was grounded in Kantian rationalism. This was not the case. Following Frege’s advances in modern logic, the analytic–synthetic distinction has been applied almost exclusively to propositions (or ‘statements’), rather than to Kant’s more ambiguous ‘judgments’ (see, for example, Quine, 1953). Moreover, like nearly all philosophers trained outside rationalist philosophical traditions, I am deeply skeptical of most of what Kant has to say about synthetic *a priori* judgments.

I mention this because our disagreement on heterogeneity is essentially about Kantian philosophy, not competitive advantage. When Durand argues, contrary to my claims, that ‘firms are heterogeneous’ is not a tautology, he invokes Kant’s synthetic *a priori*; i.e., he asserts that this ‘judgment’ can be known as empirically true and certain without (or prior to) empirical investigation. We could debate the matter further, but it involves epistemological differences between rationalists and empiricists that have not been resolved in philosophy itself.² My response is that, like most non-rationalist philosophers, I am skeptical about any possibility of synthetic *a priori* ‘judgments,’ including those involving firm heterogeneity.

I would quickly add that my remarks on heterogeneity were not intended to undermine strategy theory, but rather to help us understand the basis for its philosophical legitimacy. In my view, strategy research cannot rest either on rationalist or positivist–falsificationist foundations—too many strategy concepts are unobservable, too many strategy propositions are untestable or tautologous, and we cannot fall back on the Kantian synthetic *a priori*. We have two choices: we can either continue defending our propositions as synthetic and verifiable, or we can look elsewhere for philosophical justification. For all the reasons discussed

here—and for others not raised here, from constructivist, philosophy of language, critical, existentialist and postmodern perspectives (see, for example, Parker, 2000; Mir and Watson, 2000; Calas and Smircich, 1999)—I believe falsificationism is a losing battle. In my view, a more attractive and proactive approach would insist on philosophical foundations adapted to the messy, ill-behaved, complex art and science of strategy research and practice.

STRATEGY AND PRAGMATIST PHILOSOPHY

I have no objection to Durand’s titular statement that ‘competitive advantages exist.’ My article was essentially a logical and philosophical analysis of what such statements might mean. Durand believes they mean that we can know with certainty that some perceptible entity called a ‘competitive advantage’ has a demonstrable cause–effect relation with the performance of a unique firm. I, on the other hand, am not so sure—‘know’ and ‘certainty’ seem very peculiar language when the relation is true by definition, the constructs and relations are intangible, and alternative hypotheses have not been seriously proposed or tested. I suspect the propositional attitude is closer to *belief*, perhaps even *hope*.

But we *might* have meant something truly important. We might have meant that the statement ‘competitive advantages exist’ opens avenues of intellectual discovery heretofore inaccessible, or that it prompts actions—data-gathering, seminar-organizing, speaking, writing, advising—that would otherwise go unperformed. ‘Competitive advantages exist’ might stimulate new questions, measurement tools, statistical methods, and lead to further refinements of theory and methodology. The statement might lead managers to attend our seminars, to see the world in a new light, to undertake surprising, daring, and successful ventures. And if ‘competitive advantages exist’ achieves these ends better than rival statements *thus far*, we might justifiably adopt it as our own.

I am not proposing that statements like ‘competitive advantages exist’ need have no connections with empirical reality—clearly, this would render strategy statements implausible, useless and relativistic. But neither must we insist that strategy propositions conform slavishly to the mandates of

² When A. J. Ayer asked Cambridge philosopher Alfred Ewing what he most looked forward to in the afterlife, Ewing replied: ‘God will tell me if there is a synthetic *a priori* proposition’ (Edmonds and Eidinow, 2001).

neo-positivist philosophy of science. If a strategy proposition stimulates results on the dimensions we value, then we make it our own. That, in capsule, is the pragmatist theory of truth—in a complex world where facts are messy and disrespectful of our theories about them, we need not take the desperate course of constructing defenses and pretences to ‘correspondence.’ Our truth is not correspondent, but *instrumental*—the better theory is the one that stimulates better research, better teaching, better learning, better practice. The pragmatist criteria are clear, specific, and not even remotely relativistic. And in the epistemologically ill-behaved world of strategy, it is not unreasonable to call instrumental propositions *true*.

These ideas are not conventional, but neither are they, as Durand suggests, ‘fashionable’ or ‘post-modern’ (!). They originated with Charles Sanders Peirce, the enigmatic late-nineteenth-century Boston philosopher–mathematician (Peirce, 1877, 1903, 1906a, 1906b) and spread largely through the philosophical work of his Harvard friend and colleague William James (1897, 1907a, 1907b, 1907c). Although pragmatism’s origins are distinctly American, and it remains influential there (e.g., Rorty, 1989), pragmatists of various persuasions have arisen in academic departments in Europe and elsewhere.

From the pragmatist line of reasoning arose Peirce’s notion of ‘abduction,’ or ‘inference to the best explanation.’ Clearly, abduction does not satisfy the demands of formal logical inference, and an abductively inferred proposition is not ‘true’ under a correspondence theory of truth. To *abduct* a proposition is to acknowledge its capacity to withstand competition among rival theories, while recognizing the formal and empirical complexities of knowledge-claiming—and showing respect for the fragility and instability of human knowledge. In my view, to claim anything more—e.g., that our strategy propositions are undeniably and certainly true—reflects a vast overestimate of the veracity of human judgment, rationality, perception, and language, and a futile clinging to the false security of empirical ‘certainty.’ Truths in strategy are neither certain nor final, and our wishing cannot make them so. Whatever philosophical foundations strategy may build upon—pragmatist or otherwise—they must surely incorporate this difficult truth.

Strategy is an experiential arena where philosophy matters, and strategy research is beginning to

recognize this connection. But there is much work to be done. Beyond ethics and philosophy of science, philosophy has been slow to enter strategy research, even when clearly relevant. For example, normative strategy assumes a strong voluntarist orientation, and a clear rejection of philosophical determinism—managerial action must make a difference. But managerial volition undermines or directly contradicts a large body of management and economic theory—population ecology, resource dependence, industrial organization, Marxist theories, self-organized criticality—and the philosophical conflicts have not been resolved. Similarly, strategy assumes mind–body dualism (Ryle’s ‘ghost in the machine’), but the philosophical issues remain virtually undiscussed—and I imagine that many provocative and powerful empirical possibilities (e.g., relating strategic decision-making to brain processes and hormonal response) could arise from such a discussion.

Professor Durand and I agree that strategy’s philosophical foundations are worthy of further exploration, and I very much appreciate his thoughtful comments on my article. His response was timely, and his arguments very much to the point. I had intended the article to provoke debate, and I am pleased that it has done so. I look forward to further explorations at the interface of strategy and philosophy, in the *Strategic Management Journal* and elsewhere.

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