

RESEARCH NOTES AND COMMENTARIES

STRATEGY WITHOUT ONTOLOGY

THOMAS C. POWELL*

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

Professor Arend's critique raises fundamental epistemological issues for strategy research, particularly the concern that pragmatist philosophy forecloses access to objective truth and scientific progress. This response discusses the empiricist underpinnings of pragmatism, and addresses Professor Arend's specific concerns about the connections between competitive advantages and firm performance. Copyright © 2003 John Wiley & Sons, Ltd.

STRATEGY AND OBJECTIVE TRUTH

Professor Arend's critique raises important issues, and I appreciate the opportunity to respond. I intended my article as a discussion paper on strategy and philosophy of science, and have been encouraged by the many public and private commentaries.

Arend's paper raises numerous objections, but saves its crucial and most passionate objection for last: that I would deny strategy researchers access to objective truth. In Powell's world, the critique suggests, strategy researchers might as well abandon the quest for truth and follow 'the lure of rich consulting rewards ... without any worry about having underlying theories proven wrong.' Arend then concludes on a note of despair:

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*Correspondence to: Thomas C. Powell, Australian Graduate School of Management, Gate 11, Botany Street, Sydney, NSW 2052, Australia. E-mail: thomasp@agsm.edu.au

If ... a science is defined by an ability to predict and control the dependent variables of interest then strategy research cannot ultimately fare well. Perhaps a new definition is needed to provide a fairer measure of progress in strategy research, but that, unfortunately, brings us back to the language game where we began.

I do not share this pessimism or despair. I believe strategy research can fare quite well, that control and prediction are not beyond the reach of our work, and that our particular 'language game' can be as productive as any in the social sciences. My original paper was, in large part, an attempt to develop the epistemological foundations for this belief.

But what emerges in Arend's critique, as in Durand's earlier commentary, is that many strategy researchers want something more than to 'fare well.' They want to fare well *and* to preserve their common-sense intuitions about reality, truth, and scientific progress.

I do not think this is possible. For any empirical discipline, epistemological beliefs have theoretical and methodological consequences, and habitual beliefs can lead to dogmatism, illusion, or despair. In a world where common sense and conventional wisdom often mislead us, philosophy should, in Wittgenstein's words, 'show the fly out of the fly-bottle.' Our philosophy of strategy should show us the way, even if it means relinquishing the comforts of well-worn epistemological beliefs.

These wider epistemological concerns go beyond the scope of my original paper, which dealt specifically with the competitive advantage hypothesis. But they are relevant to Arend's comments on objective truth and scientific progress, and therefore the following section addresses them, providing the empiricist underpinnings supporting the earlier paper. In the second section, I address Arend's more specific concerns on competitive advantage.

STRATEGY AND EMPIRICISM

Philosophers agree on very little, but nearly all of them believe that human experience originates, at least in part, in sense impressions. Philosophers disagree about whether experience originates *entirely* in sense impressions (Locke's 'tabula rasa'), or is supplemented by preexisting sense-making machinery (e.g., Kant's 'categories'), but these debates—though essential in epistemology—have little direct relevance for research in natural or social science. The primacy of empirical experience is noncontroversial, and nearly all researchers (and nonresearchers alike) are, in the broadest sense, empiricists.

For research, the telling differences concern the origins and consequences of sense impressions—or, more properly, the *ontology* of sense impressions. Empiricism itself provides no ontological guidance. Sense impressions may be literal copies of 'real' objects and phenomena, or they may be private, solipsistic dreams, or shared cultural imaginings. Whatever it is that 'causes' or 'stands behind' sense impressions—if anything—does not impress itself independently upon us. Empiricism is ontologically silent.

Philosophers and scientists, however, have not remained ontologically silent. 'Realists' believe in the hard reality of sense impressions, 'idealists' believe in their transcendental reality, and 'theists'

believe in their divine reality; the 'correspondence' view says that propositions copy reality; the 'coherence' view says they connect only with other propositions; and the 'conceptualist' view says propositions 'shadow' but do not copy reality; 'monists' believe reality is unified under a single principle; 'pluralists' believe reality is distributed; and 'dualists' believe reality is bifurcated. And while the ontological 'isms' proliferate, a pure empiricism, clear of the ontological fray, has proven elusive.

This elusiveness is illustrated in the history of positivist epistemologies. Positivism arose in the nineteenth century as an attempt to provide an ontology-free epistemological defense for emerging scientific theories—particularly the theory of natural selection, which was under attack from existing theories, religious interests, and popular opinion; and later, for sociological functionalism, neoclassical economics, and behaviorist psychology. Like 'logical positivism,' its twentieth-century descendent, nineteenth-century positivism attempted to establish a complete logic of science by distinguishing factual from value-laden propositions, codifying the elements of scientific method, establishing criteria for empirical investigation, and defining the requirements of good scientific theory (Menand, 2001).

At its early twentieth-century peak, 'logical positivism' was constructed on three platforms: (1) *Logic*—distinguishing analytic (logically necessary) from synthetic (empirically meaningful) propositions; (2) *Verifiability*—the insistence that propositions be, in principle, testable, i.e., contain observable or measurable terms; and (3) *Meaning*—that the meaning of a proposition is its means of verification; and unverifiable ('metaphysical') propositions are not wrong, but meaningless. These platforms, founded in part on a (mistaken) reading of Wittgenstein's *Tractatus*, were promulgated in Vienna by Schlick, Carnap, and Feigl, and in Cambridge by Ayer and the 'analytical school' (see Ayer, 1946; Reichenbach, 1951; Kraft, 1953).

Positivism succeeded insofar as its vocabulary and analytical methods survive in the philosophical mainstream. But there are few remaining positivists, and positivism's decline is one of the more dramatic chapters in recent philosophy. Although positivism began with ontology-free aspirations, its increasingly dogmatic insistence on observability and the absurdity of metaphysics opened logical positivism to attacks from every quarter—realists,

theists, idealists, philosophers of language, social constructionists, Kuhnians, neo-pragmatists, post-modernists, and even analytical philosophy itself. Quine (1953), for example, showed that the analytic–synthetic distinction, though useful in some contexts, is often undecidable; and, along with Pierre Duhem (1954), emphasized the ambiguity of the verifiability criterion, and the improbability of decisive empirical tests.

Philosophers of science may eventually abandon the idea of a complete, consistent empirical system, as many have already abandoned the notion of a comprehensive logic of science (Kincaid, 1996). But this brief history shows that neither philosophers nor empirical researchers can afford the luxury of carrying dogmatic ontological beliefs on the backs of their epistemologies. We may never achieve an entirely consistent empirical system, but we can at least learn the most important epistemological lesson: *of what we cannot know, we must remain silent* (Wittgenstein, 1922: 151).

The essential premises of a broadly construed empiricism are straightforward: we receive information from our sense impressions; we do not know what ‘stands behind’ these sense impressions (if anything); and our ‘sense-making machinery’ (which may or not preexist sense impressions) deploys language and concepts to help us organize, interpret, and communicate our sense impressions. In this way, a family of sense impressions (describable adjectivally as ‘short,’ ‘brown,’ ‘furry,’ ‘loud’) may give rise to a sense-making sign (the grammatical substantive ‘dog’), or to a proposition about sense-making relations (‘The dog barked because he was hungry’), as an analytical and discursive convenience. These conveniences, of course, do not give rise to an independent entity (a *dog*) or relation (a *cause*)—there may or may not be a *real dog* feeling hungry, barking behind our sense impressions.

These ideas are widely misunderstood, and even prominent empiricist philosophers have attached ontological beliefs to their empiricism—Cambridge analytical philosopher G. E. Moore insisted his pen was independently real because he could see and feel it (Moore, 1959: 33); and Karl Popper, while rejecting common-sense realism, insisted on the reality of objects such as planets because we perceive them intersubjectively, and through artificial devices like telescopes (Popper, 1963: 117). But nothing in the empiricist epistemology either accepts or denies

these claims. There may be *real planets* there. But we cannot be sure, and more to the point for empirical research—*it doesn’t make any difference*. The planet’s (or the manager’s, or the firm’s) ontological status has no research consequences.¹

What does have research consequences, and pernicious ones, is the interjection of ontological *beliefs*, which tend to run in escalating chains of ideology: a hard reality exists; *thus*, our sense impressions correspond to that reality; *thus*, our language corresponds to that reality; *thus*, the terms in our propositions are real; *thus*, we are approaching truth; etc., etc.; or conversely: reality is transcendental; *thus*, reality is perfect; *thus*, sense impressions are imperfect; *thus*, truth is accessible only by thought; etc., etc.

Under a broad empiricist epistemology, beliefs about ultimate ‘truth’ and ‘reality’ have no research consequences. They are, to quote William James (1890: 1264), ‘altars to an unknown god’—we can believe in them or not, or be agnostic, as we are inclined. But introducing ontological beliefs into the research context seems a strange and nefarious intrusion—like bringing political opinions into weather predictions. Unless someone had an ulterior motive, it isn’t clear why they would do it.

None of this impugns scientific methods or scientific contributions to society. But it does impugn the scientist’s ontological pretensions. As Wittgenstein put it: ‘What a Copernicus or a Darwin really achieved was not the discovery of a true theory, but of a fertile new point of view’ (Wittgenstein, 1977: 18). Our discoveries are not *inferior* by virtue of solving scientific problems—this is exactly what good theories do. But it is self-serving for scientists to insist, over and above solving human problems, that science transports us into the transcendental realm of reality and objective truth.

Among the many available ontologies, empirical researchers seem particularly attracted to common-sense realism, and often vindicate it with proofs: intersubjective agreement, the accumulation of evidence, the involuntary character of sense impressions, etc. But these ‘proofs’ strike me very much like the ontological and design proofs of God’s existence. The conclusion may someday be proven true, but people invoke the ‘proofs’ for other reasons—to persuade, encourage, control,

¹ In logic, this idea is sometimes presented in ‘redundant’ or ‘disquotational’ form—that the proposition ‘p is true’ adds nothing to the proposition ‘p.’

cajole, or to justify habitual beliefs. In any case, my accepting or rejecting them has no consequences for research, other than making it more complicated. Empiricism, without the ontology attached, already stipulates that observers cannot freely choose their sense impressions, that different observers experience similar sense impressions, that these impressions may 'accumulate,' and that 'intersubjective agreement' makes for good science. But empiricism remains silent on what it does not know—the origins of experience.

Moreover, common-sense realism might be wrong. Our sense impressions might stem entirely from shared unconscious imaginings, or from the Cartesian evil demon, or we might all be—as the philosophy professors say—'brains in a vat,' under the control of some diabolical scientist. In one sense, intersubjective agreement should not astound us—similarly constructed, similarly trained beings observing in the same way will give similar reports. Certainly, the convergence of observations says nothing about what lies outside those observations. None of us, for example, will report observing in four spatial dimensions and, in general, our sense faculties and shared experiences impair our imaginations. All of us may look through a telescope, perceive a white sphere, and call it a fact of science—but we need not get carried away about 'reality' or 'truth': an alien using the same telescope might hear music and call the moon a fugue in B-flat.

We may not feel *subjectively* as though we are inventing ordinary experience, but that belief does not imply that we are *discovering* an objective reality independent of perception—we may, in fact, be discovering ourselves, in the sense of becoming blindingly familiar with our shared frame of reference, i.e., the peculiarities of human sense impressions and sense-making machinery. In the nineteenth century, William James put it as follows:

The mind is not a mirror floating with no foothold, passively reflecting what it comes upon. Mental interests, hypotheses, postulates help *make* the truth the mind declares. There belongs to mind a spontaneity, a vote. It is *in the game*. (James, 1878: 3)

Clearly, some researchers do believe that science holds a mirror to objective reality. From his comments on objective truth and scientific progress, I infer that Professor Arend believes this, as is his

right. But as for myself, I wonder what researchers hope to gain by mixing strategy with ontology. Indeed, Arend's critique, which drives itself to a vague and despairing conclusion, demonstrates the consequences of clinging to a common-sense realism that neither hears nor answers back. Whatever our ontological beliefs, they can only pull us deeper into the fly-bottle.

I believe Arend's despair is unfounded. Under any reasonable empiricism, we can theorize, gather data, make discoveries, establish careers, maintain our research integrity, and be 'scientific,' and all without the ontological baggage. Moreover, we can free ourselves from enervating tribal disputes (e.g., 'objectivist vs. subjectivist' debates), and admit a wide and inclusive array of theories, methodologies, and perspectives, from functionalist to interactionist to postmodern. And along the way, we can gain a more profound appreciation of our work; that our research is not *reduced* by its detachment from myths of ontological 'reality,' but *enhanced* by its authentically human character; that we are not 'passive mirrors,' but active explorers, discoverers, and organizers of experience.

ON COMPETITIVE ADVANTAGE AND FIRM PERFORMANCE

Professor Arend's critique raises a number of issues specific to the hypothesis of competitive advantage. The main objections are as follows: (1) that firm performance should be defined in relative, not absolute, terms; (2) that my concept of competitive disadvantage has antecedents in strategy research; (3) that abductive inference and the pragmatist view deny the scientific value of strategy research; and (4) that my account of competitive advantage neglects key features of the advantage–performance relation: e.g., the origins of competitive advantage, endogeneity of advantages, changes in advantages, and attacks on rival advantage. The remainder of the paper addresses these concerns.

Defining competitive advantage and firm performance

Professor Arend is concerned that, in discussing the logic of competitive advantage, my propositions do not define the terms 'competitive advantage' and 'performance.' However, as noted in

the paper, my argument addresses only the logical structure of competitive advantage propositions, not their content—we could express *any* competitive advantage hypotheses using these forms. In later sections, my paper specifically presented a resource-based definition of competitive advantage, and analyzed its epistemological properties in relation to other definitions (e.g., competitive advantage as market power). But the conclusions in the logic section did not depend on our agreement about definitions—to quote the paper, these conclusions ‘would apply to any propositions p and q so long as they were arranged in parallel logical forms’ (Powell, 2001: 881).

Arend seems particularly adamant that ‘performance’ be defined not in absolute terms, but as performance *relative to competition*. I do not see how we disagree here, or how my own construal could be misunderstood: all of the propositions in the paper used the term ‘*superior performance*.’

But Arend takes this objection a step further, claiming that the notion of *relative* performance vitiates the need for a concept of competitive disadvantage. The argument is as follows: consider Powell’s hypothetical industry in which firms perform differently, but no firm has sustainable competitive advantages; then the firms differ only in their competitive disadvantages; and thus relative performance is a function of differences in competitive disadvantages. But, Arend argues, why not ‘redefine the baseline,’ and simply refer to firms with fewer competitive disadvantages as firms with competitive advantages?

The problem is that however we operationalize competitive disadvantage—and my paper suggested some of the possibilities—it *cannot* be the inverse of competitive advantage. We obviously do not need a new concept if we are only going to ‘redefine the baseline’ to rescue an existing concept. As Arend seems to recognize, this lands us ‘squarely on the second problem of tautology.’ But much worse, it lands us back on the first problem, leaving us without an inferential foundation for connecting firm-specific attributes with superior performance—we can eliminate competitive disadvantage, but we still need an alternative that is not a disguised attempt to rescue the hypothesis of competitive advantage.

The concept of competitive disadvantage

From the above critique of competitive disadvantage, Professor Arend takes the unusual step

of praising the concept of competitive disadvantage, and showing how the concept was foreshadowed in earlier strategy research—e.g., the ‘W’ in SWOT, inertia, inefficiency, core rigidities, and strategic liabilities. He then concludes that, in prior research, ‘negative causes have not been well integrated with positive causes to explain overall effects, positive, negative, and neutral. There is a vacuum for sophisticated propositions on how competitive advantages and disadvantages combine, on how each combines with similar causes, and on how advantages may become disadvantages and vice versa.’

I agree with most of this, since it echoes my own paper. But as a point of clarification, my concept of competitive disadvantage was a logical construct—a propositional fiction designed to suggest how we might improve our inferences about superior performance. Empirical researchers can interpret it variously, including the ways suggested above and those suggested in my own article, but in my paper competitive disadvantage played a formal rather than empirical role. Logically, the only restriction is that competitive disadvantage not be redefined as equivalent to other constructs in the same propositions—i.e., it is not anti-performance, and not the mirror image of competitive advantage.

Abductive inference and pragmatism

Having commented on these subjects in two previous papers (Powell, 2001, 2002), I will not repeat those arguments here. In any case, I find very little to disagree with in Professor Arend’s comments on abduction and pragmatism. He asserts his disagreement, but then devotes a substantial part of his critique to exploring the reasons why strategy research may not converge to objective truth, before finally concluding that the analysis ‘provides room for debate.’ As part of this discussion, he reflects on the desire for objective truth and scientific progress, as I addressed earlier.

On the other hand, I have found that, in defending a pragmatist interpretation of strategy research, one runs the risk of being misunderstood. In Professor Arend’s critique, I am concerned about three apparent misunderstandings. First, Professor Arend suggests that pragmatism and abductive inference degrade the ‘scientific value’ of strategy research, reducing strategy to the ‘poor brother of economics; a dismal science without the science.’ As

noted earlier, this conclusion is unwarranted. Pragmatism is founded on an inclusive empiricism, and makes no judgment on strategy's scientific value in relation to economics or any other discipline.

Second, although Professor Arend concedes that the resource-based view suffers to varying degrees from all the epistemological shortcomings noted in my article (tautology, unfalsifiability, etc.), he is at pains to show that theoretical advance must represent more than inference to the best explanation. In this connection, he provides the following scenario: 'consider a firm that has created, at a cost, a strategic asset—a resource that is valuable, rare, inimitable, non-substitutable, appropriable, etc. . . . Now assume that the value is trend-dependent and it lasts for one day, increasing firm revenues; not long enough to recoup the investment. The firm is likely to have inferior performance even though it has fulfilled RBV requirements of competitive advantage.'

Again I cannot disagree, since this conclusion is consistent with my own (a 'quadrant 2' firm). But it does not, as Arend implies, *refute* abductive inference by showing that new strategy theories correct 'holes' in previous theories. If anything, this process *illustrates* abductive inference in action—though it hardly seems a decisive example, since the resource-based view has a ready menu of responses to the 'quadrant 2' scenario (that the asset was not valuable; that its rents were not appropriable, etc.).

Finally, in an earlier section of his critique, Professor Arend makes the following statement: 'Research that either uses relative measures or explicitly considers both positive and negative causes of performance is logically valid. However, it is not, as Powell and others (e.g., Kirzner, 1973) assert, necessarily meaningful as a prescription for future action.'

To clarify, I do not (and did not) assert that propositions of logic have any prescriptive value whatsoever, and I have no idea how my paper could have given that impression. I affirm most vehemently: 'is' does not imply 'ought.' Pragmatism and abductive inference do not supersede the logic of the propositional calculus, and they do not epistemologically connect strategy research with management practice.

Moreover, I have not argued that researchers should *try* to be pragmatic or instrumental. At best, pragmatism and abductive inference *describe* what we do as researchers, and I have not found

a better epistemological portrayal of our work. They also recognize that, in a world of warring and incommensurate philosophies of science, researchers need to get on with their work, and that an inclusive philosophical foundation can help them do so. As philosopher–scientist Ernst Mach put it: 'Imagine the position scientists would be in if they had to refute all the philosophical systems one by one' (Mach, 1906: 368).

Pragmatism is, in that narrow sense, an epistemological 'justification' for our work—it describes what we do, enables us to act, and tries to stand clear of ontology. Of course, pragmatism itself is not perfect, complete or value-free, and I could imagine abandoning it if a more convincing justification came along. Professor Arend may believe that the discovery of 'objective truth' is more convincing, but on this we disagree.

Complexities of the advantage-performance relation

Professor Arend correctly points out that there is more to strategy research than connecting competitive advantage to firm performance; and more to firm performance than a simple connection with competitive advantage. I fully agree, and I endorse his sentiments on the importance of developing more sophisticated accounts of firm performance. His examples effectively illustrate the possibilities, although I would caution that our 'sophisticated' theories should strive for consistency—it is not consistent, for example, to insist that competitive advantage is both uncontrollable and endogenous. In any case, my own article did not attempt to develop a comprehensive theory of strategy, but rather to establish the *philosophical* foundations for the research connecting competitive advantages with sustained superior performance.

A FINAL NOTE

At several points in his critique, Professor Arend expresses concern about 'language games,' a term I borrowed from Wittgenstein's later works (Wittgenstein, 1953, 1958), and used in my paper to denote (as I explained it): 'a way of seeing that directs scholars to remove problems that lie in the way of discovery about firm performance' (Powell, 2001: 885). But Arend uses this expression idiosyncratically: 'Strategy may be as much

the pursuit of superior returns as a *game of language*'; 'The *'game of language'* Powell accurately accuses some strategy research of being also applies to Powell's article'; 'If strategy is to become more scientific then the *language games* must stop'; 'Putting *language games* and implicit assumptions aside, we can now explore whether competitive disadvantage has received its just analysis'; and, 'Perhaps a new definition is needed to provide a fairer measure of progress in strategy research, but that, unfortunately, brings us back to the *language game* where we began.'

As a point of clarification, I do not think of the term 'language game' as an epithet or an accusation, nor do I believe it entails frivolity or lack of seriousness, nor do we exit and enter language games at will. A language game, in my understanding, is a shared verbal and cultural context that guides perception, interpersonal communication, and human action—not unlike a 'paradigm,' 'world view,' or 'frame of reference.'

As already discussed, I do not believe science gives us privileged access to objective truth or reality, and I find it self-serving and unproductive to claim that it does. But I do think our ways of perceiving and getting along in the world—our 'language games'—provide essential contexts for human understanding, and for what I would call 'science.' Science is not arbitrary, as some would claim, but neither is it value-free—it is an ever-changing picture of experience, and an artifact of our peculiar perceptual and sense-making equipment. Like other language games, strategy is not to be lamented or escaped or venerated, but to be lived and understood, as a habitat in which we discover and communicate human experience.

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