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Redirecting Research in Business Policy and Strategy

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Summary
Because business policy research has primarily been a series of inductive generalizations of case studies; theories have been typically ambiguous and untested, and have not progressed swiftly. Deductive theorizing, by contrast, yields clear, often non-obvious conclusions that can be debated effectively and generalized slowly; so realism of current models can be sacrificed for progress towards realistic future models. Deductive theorizing, with more attention to a game-theoretic definition of equilibrium and to recent ideas from economics, should be one new direction for policy research. Of course, these deductive models will inevitably draw their inspiration from the richness of careful observation and exhaustive checklist-making that are the hallmarks of induction. Specific avenues for new research are described, and the importance of teaching non-obvious theories is defended.

In recent years, business school research and curriculum have become more rigorous. The study of institutions and capital budgeting in finance have yielded to rigorous modelling, especially of capital asset and options pricing. The mysterious arts of marketing have been replaced by an amalgam of sciences, including statistics and consumer psychology. Even accounting has been annexed by financial economics, agency theory and behavioural decision theory. While these revolutions have taken place, policy and strategy (and to some extent, organization theory; see Camerer, 1983) has remained methodologically unchanged. This manifesto explains the need for trying new directions in policy research and describes some promising directions.

My numerous biases will soon become apparent to the reader, but it is useful to announce them now: I believe that deductive use of mathematics and economic concepts is the best way to answer (and ask) corporate strategy questions. I have little faith in the usual techniques—inductive derivation of checklists based on an amalgam of metaphorical organization theory and misapplied economics—but much interesting work has been done this way. Therefore, I advocate the progress of research along *both* avenues, but my bets are placed on the deductive vehicle.

INTRODUCTION: THE STATE OF THE ART

Most policy and strategy texts contain 'definitions' such as the following from Thorelli (chosen randomly, and *not* taken out of context):

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> In principle, strategy is the primary means of reaching the focal objective. The focal objective is whatever objective is in mind at the moment. Strictly speaking, it is literally meaningless to talk about strategy without having an objective in mind. Viewed in this context, strategy becomes an integral part of the ends-means hierarchy (1977:6).

Typically, researchers and teachers use armchair analysis to derive such conceptual frameworks, taxonomies, typologies or 'approaches' to strategic and policy problems.

Many of these frameworks are useful and interesting, but ambitious policy researchers sense that checklist-making is not the hallmark of a scientific discipline, so there is (and should be) a queasy dissatisfaction with the state of the art. There are at least three symptoms of the disease causing the queasy dissatisfaction with policy research:

(1) concepts are often ambiguous and their definitions are not agreed upon;

(2) checklists or theories are rarely tested, and never tested directly against competing theories and

(3) theories do not 'cumulate' or build upon previous theories as they should. These three deficiencies are a result of the *way* policy research is typically done. Thus, this manifesto calls primarily for a *methodological* shift from induction to deduction and describes when (in the next section) and how (in later sections) this shift should be effected.

SHORTCOMINGS OF POLICY RESEARCH

Confusion about concepts

The 'approaches', checklists or typologies developed by policy researchers tend to be loosely constructed and are, therefore, ambiguous, fundamentally debatable and difficult to teach meaningfully. For example, by Thorelli's (1977) definition quoted above, is profitable acquisition a 'strategy', a 'focal objective' or both? There is clearly ambiguity in a classification such as Thorelli's, and this confusion can be illustrated with a classroom trick: ask students to define 'strategy' and 'policy'. Many will say that strategy is decided at top levels in a firm and policy is carried out at lower levels; and many will vehemently argue exactly the opposite! Clearly, the language is to be blamed, rather than misconception by half a class.

The usual approach to development of a new language of strategy and policy is an exhaustive 'literature review' or catalogue of definitions used by others; typically, reviewers then try to put out the fire with gasoline by synthesizing these diverse working definitions into a 'new', 'improved' super-definition—e.g. Leontiades (1982) proposes replacing 'strategy' with the crystal-clear 'planning'—ignoring the futility of the process of definition with the awkward grammar of English. A new, unambiguous, specialized vocabulary is needed. Using Friedman's (1956) analogy of theory to a 'filing system', it is important that we should be able to store ideas in neatly divided files (and into the same file each time), and that we should be able to find ideas filed by others. No business executive would consider using a filing system with files labelled 'strategy' and 'policy' because of the confusion that would result; so why do policy researchers persist in using such crude 'filing systems' in their research?

Failure to test models properly

Even if variable definitions are agreed upon, most approaches or taxonomies in policy research are never subjected to scientific criteria of proof, such as measurement and statistical testing. Instead many policy researchers appeal only to an intuitive sense of correctness or plausibility, as do historians, artists and fiction writers. If the checklist 'seems sensible' it is judged acceptable; but this criterion of acceptability is not stringent enough. Predictive ability should be the fundamental test of a theory, or at least of a 'mature' theory (Blaug, 1980), often at the expense of surface realism or truth of assumptions (Friedman, 1956). Prediction is stressed because *ex-post* explanation of events is too easy for facile minds and glib tongues; after the fact, even random occurrences can be easily 'explained' (Fischhoff, 1975). Thus, appeal to historical cases, although possibly illustrative and useful, is hardly 'proof' of a theory's adequacy. Furthermore, it is not generally enough to show that a theory predicts well; an adequate theory must predict better than *competing* theories (Platt, 1964). Most policy research, however, does not involve any predictive tests, and clear tests of relative superiority are unheard of.

Take the 'product life cycle' (Biggadike, 1981): at a given stage in the 'cycle', it is unclear when the next stage will begin, or how long it will last. The concept lacks strong predictive capability. Indeed, these 'cycles' resemble the fake pseudo-cycles that are visible, *ex-post*, in almost any statistical series (such as stock market data; see Fama, 1976). I am especially sceptical that the product life cycle concept predicts product sales more accuractly than predictions which product managers generate from their own intuitive models of sales growth—that is, I guess the formal model is not *worse*, but is probably no better than 'competing' common-sense models.

Most models or frameworks in policy research, if tried before the stern judge of predictiveness and her sterner cousin, relative predictiveness, would be convicted and be sentenced to perish rather than be published.

A frequent objection among social scientists to the practice of empirically testing models mercilessly is voiced by Jemison (emphasis mine):

> Recently, many authors have been applying highly sophisticated statistical techniques to AB-related (administrative behavior) strategic management research questions—probably owing in equal measure to the requirements of journals for 'rigorous' research and to their own methodological training. Although quantitative studies can be useful and information, they can also be futilely applied in a *managerial environment that is inherently more complex than the degrees of freedom available to the researcher* (1981a:640).

The thrust of the argument is a red herring (cf. Cronbach, 1975): the 'too complex for testing' defence is an objection to any predictive test. These conscientious objectors would rather switch than fight: their complex, but fragile, theories would fare better if allowed to take the less demanding tests of aesthetic appeal and apparent completeness, etc., which have traditionally been used (with notable recent exceptions), in scientific arts such as history, anthropology and sociology.

However, two counterarguments to the importance of rigorous predictive tests are worth noting: first, sometimes the rigorous standards of testing are rejected because a discipline or theory is only infantile and should not be subjected to the empirical scrutiny appropriate for mature theories or disciplines. However, the protectionist view can be carried too far—at some point, the infant must be able to fend for itself. It is important gradually to develop a theory towards empirical testability, rather than always defending a theory from merciless testing. Secondly, prediction does not always *guarantee* understanding—any observer can 'predict' that the sun will rise tomorrow, with no help from physics. But only a physical model enables us to make predictions in a slightly altered universe—e.g. with two suns. Thus, predictive ability is not the *only* test of a good theory.

When extensive empirical work *has* been done in policy research—e.g. research using the PIMS data—it has often resulted in generation, not testing, of hypotheses. This may be appropriate at early stages of research but such empirical 'data-mining' must not be mistaken for definitive theory testing. Furthermore, it is especially difficult to draw causal implications—necessary for making normative prescriptions, one important activity of policy research—from regression results and other popular tests. For instance, the well-known market-share-ROI correlation (Schoeffler, Buzzell and Heany, 1974) might be due to a 'direct effect' of share on return, or due to a shared correlation with some third variable such as 'luck' (Rumelt and Wensley, 1981). Separating these competing hypotheses—which are both consistent with the 'first-stage' correlation—is difficult without sophisticated tests and careful specification of the underlying hypothetical structures (Camerer and Fahey, forthcoming). However, few second steps of this sort have been taken.

Business policy research: art or science?

Even if models contain ambiguous variables, and are not (or cannot be) rigorously tested, we certainly expect policy frameworks to pass the test of time. That is, if they are not tested explicitly, models should at least be timeless (see Meehl, 1978, on Freud), or should get subsumed by later, more general models—e.g. Newtonian physics is a special case of Einstein's physics; and the core of a co-operative game includes, in the limiting case where the number of players is large, the competitive equilibrium outcome which is the focus of traditional economics (Debreu and Scarf, 1963; see also Schotter and Schwodiauer, 1980). Unfortunately, policy approaches do not seem to pass these tests of time; knowledge in policy analysis is neither timeless nor cumulative. New texts replace old texts with non-progressive faddishness, just as Picasso 'replaces' Rembrandt without having necessarily incorporated Rembrandt's theories or aesthetic ideals. The failure of policy research to progress in this way might be reflected by non-cumulative changes in consulting wisdom. As *Fortune* wrote about the business-portfolio matrix:

> These (*Fortune 1000*) companies may be disturbed to learn that many of the matrix's original champions now view it as outmoded, if not dangerously wrong (1981:148).

This is more than the natural death of an idea which has lost its novelty, and thus lost its appeal to firms seeking competitive edges: instead, many of the original champions of the simple growth-share matrices have denounced them (according to *Fortune*) but the tools that have sprung up to replace them may be similarly misguided, and certainly owe no intellectual debts to what was learned from the 'outmoded' matrices. (A case in point may be the 'value curve' championed by Strategic Planning Associates, which seems to rest on intelligent assumptions of capital market efficiency while advocating a reallocation of resources within the firm that would only make sense if the current allocation is clearly suboptimal.)

The failure of the policy arts to progress is not too surprising, since most arts do not *need* to progress (Shweder, 1979) though they may do so (at least technologically). However, most policy researchers probably think, or wish, that they are doing science rather than art. But the lack of tests comparing models (replaced by 'does this seem right?' judgement) and the failure of knowledge to cumulate swiftly, identify policy research as more an art than a science. This may not be such an awful fate for our field; probably social sciences should combine some elements of science and art (and blindly trying to use only the methods of physics or chemistry is hardly called for), but in a proper balance.

INDUCTION AND DEDUCTION

For all the energetic research on strategy and policy, the state of the art is disappointing. Theories are ambiguous, untested and tend to replace other theories with little apparent progress. I believe this malaise can be traced to the way research is typically done: specifically, the almost exclusive use of armchair induction encourages the creation of ambiguous ill-specified theories with little comparability to other theories (cf. Mitroff and Mason, 1982). In induction, conclusions are derived from observation of cases. Examples include Peters and Waterman's (1982) provocative contention, based on careful case studies, that a strong corporate culture is an important ingredient of 'excellence' in corporate performance; and Bower's (1978) claim, based on anecdotal case evidence, that use of growth-share matrices by conglomerate management for internal capital allocation 'is a far more disciplined resource allocation process than the capital markets provide'. In contrast to these debatable conclusions induced from evidence, deductive modelling yields conclusions, logically and irrefutably, from a set of assumptions. Unfortunately, the logical consistency of deductive techniques is invariably accompanied by a lack of realism, since theorems can usually only be proved by restricting the realism of assumptions.

Of course, this is the pristine textbook view of induction and deduction. In practice, the bases for deductive theorizing are often found in careful observations. In macroeconomics, for instance, theorists observe facts (e.g. correlations between short-run changes in money supply and in GNP) then concoct artificial 'stories' (sets of assumptions) and deduce conclusions from these stories which match the facts. Thus, a good economic model is one which reaches virtually pre-determined (induced) conclusions, but does so in a deductive, rigorous, debatable way. The beauty of such a deductive formalization of what we induce is that we can argue more clearly with deduction, since assumptions are explicit, and differences in assumptions can be carried to their logical conclusions via the deductive structure of the model. But we cannot argue effectively with the 'justification' for an inductively derived 'checklist', since the justification is usually locked into the theorist's psyche. Thus, induction often results in unproductive debate about variable definitions, petty semantic sniping, ambiguity about variables, lack of testability and failure of theories to cumulate.

Although I do not wish to appear over-critical of the power of human intuition or of the power of inductive theorizing, I believe the time is ripe for *some* effort to theorize deductively about business policy issues. Of course, the bases for such theorizing will be concepts, issues, and variables which have become recognized as important through careful induction. Imagination, inspiration, 'the human element', aesthetic appeal, experimentation, careful observation and other hallmarks of 'soft' sciences, are undoubtedly crucial at certain stages of model-building. My conviction is simply that deductive models can, ideally, express hypotheses in a language that is more amenable to progressive debate than is the language of checklists. Furthermore, the biggest weakness of deductive models—their lack of realism—can be defended by appealing to an obscure passage in Kuhn's classic work on the sociology of science, and to a well-known fable.

Kuhn's 'postulate' and scientific progress

If preferences in the apparent trade-off between logical consistency and realism of models were the only reason to choose between induction and deduction, then faith in either approach could be justified by taste—wordy humanists concerned with realism and the 'big picture' can study policy whereas narrow-minded pedants concerned with unrealistic mathematical details can study economics or game theory. However, for all its narrowness and lack of realism, deduction might lead to faster progress toward realistic theories than induction. As Thomas Kuhn wrote (emphasis mine):

> Bringing a normal research problem to a conclusion is achieving the anticipated in a new way, and it requires the solution of all sorts of complex instrumental, conceptual, and mathematical puzzles... Puzzles are, in the entirely standard meaning here employed, that special category of problems that can serve to test ingenuity of skill in solution .... *It is no criterion of goodness in a puzzle in that its outcome be intrinsically interesting or important*. On the contrary, the really pressing problems, e.g. a cure for cancer or the design of a lasting peace, are often not puzzles at all, largely because they may not have any solution... A paradigm can, for that matter, even insulate the community from those socially important problems that are not reducible to the puzzle form, because they cannot be stated in terms of the conceptual and instrumental tools the paradigm supplies. Such problems can be a distraction, a lesson brilliantly illustrated by several facets of seventeenth-century Baconianism and *by some of the contemporary social sciences*. One of the reasons why normal science seems to progress so rapidly is that its practitioners concentrate on problems that only their own lack of ingenuity should keep them from solving (1970:36–37).

Kuhn's (empirically based) 'postulate' is that progress is made when scientists worry about deductive puzzles rather than realistic, important problems. Thus, perhaps strategy researchers interested in 'implementation' problems of a political or behavioural sort should study these problems with deductive techniques from mathematical political science (including co-operative game theory), agency theory and elsewhere, rather than exclusively producing induced checklists of factors that inhibit correct implementation of a brilliantly formulated strategy.

Although deductive models relevant to strategy are currently unrealistic, researchers building such models realize that a model is merely a means to an end, namely to build better models with more realistic sets of assumptions. In contrast, inductive thinkers—and especially traditional policy researchers—seem to feel that their models are final, complete products. Seeing models as intermediate steps in ongoing model-building makes it clear that the realism of today's model is relatively unimportant, and blatantly unrealistic models may be better 'building blocks' than realistic inductive frameworks.

Indeed Kuhn's claim that deduction progresses faster than induction recalls the fable about the tortoise and the hare: by analogy, deductive thinking starts far behind in the race toward realism, but plods steadily toward the finish. Hare-like inductive thinking has a head start because of its inherent realism, but little progress is made—theories do not cumulate, and the hare never advances. Betting on our tortoise does require a measure of faith, as in all matters methodological: but there is no reason to *not* try thinking deductively in policy and strategy research in tandem with creative checklist-making.

Fancy mathematics is not necessarily part of deduction (e.g. Akerlof's, 1970, 'lemons' model is developed with and without mathematics), but to reach interesting—i.e. non-obvious—conclusions often requires mathematics, which also makes models easily discussable. Because of limits on the capacity of the human mind to synthesize experiential data into new, coherent, fòrms, induction is not an efficient way of producing strikingly non-obvious conclusions, but mathematics can be astonishingly powerful. For instance, in the Hotelling-style 'sequential location' problem studied by Prescott and Visscher (1977), three firms locate their products on some finite continuum (e.g. represented by a line from 0 to 1 in some dimension), along which product demand is uniformly distributed. If the costs of relocating are very high, and if firms locate in a prespecified order, where should firms optimally locate? (The usual objection of policy researchers to the study of such a puzzle is its lack of realism, but Sears, Roebuck purportedly worries fairly explicitly about this problem.) Our intuition clearly has trouble 'inducing' an exact solution, though we expect to see some spread among the three firms along the continuum. The deductively derived answer is that the first two firms locate at 1/4 and 3/4, and the third firm locates in the middle at 1/2. In this brief illustration the process of deduction leads us to a non-obvious, albeit simple, conclusion which was just beyond the reach of our inductive capabilities.

FOUNDATIONS OF A NEW TRADITION IN STRATEGY RESEARCH

Although a new tradition in strategy research *could* spring from current writings, it is probably healthy to borrow ideas and methods from other disciplines. Decison theory, game theory, industrial organization and microeconomics are prime fields for poaching. Slightly less fertile fields—where deductive theorizing has taken a back seat to inductive description—include military strategy, science, organization theory, anthropology, psychology, sociobiology and perhaps sociology.

The most useful important concept is the idea of equilibrium, which is central in game theory and economics. However, strategy researchers must eventually look beyond equilibrium, or to a broader conception of equilibrium, in theorizing about business behaviour.

Importance of equilibrium

Perhaps the most overworked tool in the economists' toolbag is the concept of equilibrium, a state in which everyone is happy and nobody can improve their lot (obviously, there is a typically strong reliance on assumptions of individuual rationality here). A good heuristic test of whether an idea or model is one of 'equilibrium' or 'disequilibrium' is this: if everyone found out about the idea or model and believed it, would their behaviour change? If behaviour would change, then the model is a disequilibrium model, because it contends that people are currently behaving in a suboptimal way. (Implicit in my thought-experimental definition of equilibrium is the assumption of 'rational expectations'—managers know the basic economic theory generating profits, market shares, etc.)

Underlying this heuristic test is some mix of undying faith in the intelligence of businessmen, and humility among academics. A dose of the latter is sorely needed in traditional policy research (Wensley, 1982).

A classic case of disequilibrium has allegedly been uncovered in the PIMS data (Schoeffler, Buzzell and Heany, 1974; cf. Camerer and Fahey, forthcoming). After observing that market shares and profitability of, businesses are correlated, researchers began making qualified prescriptions like '"Get market share or get out" is too strongly put, but not much too strongly put' (Gale and Branch, 1979:32). This advice is wrong if all firms understand that larger share leads to larger profits (i.e. if firms have rational expectations), but Biggadike claimed:

> perhaps there is still a widespread lack of awareness of the relationship between profitability and market share (1979:109).

This belief is clearly at odds with the rational expectations assumption. Perhaps firms do suboptimize, or do not have rational expectations about determinants of business performance, but policy researchers should then be testing models with these assumptions made explicit, rather than invoking these assumptions *after* studying the data, to justify normative advice to firms.

Game-theoretic equilibrium

In a strategic interaction where the actions of any firm or person affect the status of others, the species of equilibrium discussed in game theory becomes useful. Usually these equilibrium concepts rely on the assumption of 'mutual expected rationality' (Harsanyi, 1977), namely that your opponent is rational, and knows that you are rational too. As Harsanyi wrote:

> As experience shows, even though people do not always act very rationally, in economic and other strategic situations rational behavior is sufficiently common so as to make it imperative for all of us to understand what strategies are open to a rational opponent, and so as to make it extremely dangerous to underestimate an opponent's ability to act rationally (1982:125).

The mutual expected rationality assumption allows construction of game-theoretic models of strategic interaction in which competitive reaction is anticipated and *explicitly* modelled, not considered as an afterthought. Even if competitors react to, rather than anticipate actions, as critics of game theory often argue, calculating the equilibrium that will eventually result is useful (and probably saves firms the costs of adjusting iteratively to that equilibrium).

Inevitably, mathematical tools are needed to solve the thorny problems raised by 'I think he thinks I think' guesswork which is the heart of strategic interaction. However, since most game theorists are mathematicians, strategy researchers cannot expect theorists to prove theorems of direct relevance to the problems of corporate strategists. Rather, we must actively distill and apply the principles of game theory—and especially the central ethic of mutual expected rationality—to business strategy settings (see the discussion of 'reputation' in the next section; and Prescott and Visscher, 1977; Karnani, 1982; Rao and Rutenberg, 1981).

Admittedly, the active development of a useful applied game theory will involve some weakening of the basic structure of game theory (see Kadane and Larkey, 1982, and the ensuing debate with Harsanyi, 1982), especially the typical assumption that all players have 'complete information' about the game. Progress is being made in this direction, but it is slow (Harsanyi, 1967–1978; Myerson, 1980).

Beyond equilibrium

As powerful as the notion of equilibrium is, progress in strategy research may involve stretching or discarding that notion. As Rumelt (1981) points out:

> (the) complete absence of any interaction between business policy and economic theory...came about because the neoclassical theory of the firm was created by assuming away the very existence of those phenomena which most concern students of business policy (1981:1).

(The interesting phenomena, Rumelt explains, include things such as firm heterogeneity and entrepreneurship.) One way to study these phenomena in equilibrium models is to assume that firms have private information or special, unreplicable skills. Fortunately, recent advances in the economics of uncertainty (Diamond and Rothschild, 1978) and in the 'new industrial organization' (Encaoua, Geroski and Jacquemin, 1982) provide solid cornerstones for the study of strategic issues in an equilibrium setting.

By contrast, game theory is especially tongue-tied about many live strategic issues—threats, credibility, reputation, imagery, timing, 'politics', propaganda, incomplete information and commitment (cf. Schelling, 1960). Unfortunately, the situation resembles the story about the man who looks for his lost keys under a bright lamp-post, though the keys were lost in a dark place nearby, simply because the lamppost sheds light. Game theory sheds light, but probably not in places relevant to strategy research, so policy researchers must build new lampposts, extending the concept of equilibrium and the appropriate branches of game theory, in order to shed light on (lost-) key policy issues.

SOME SPECIFIC DIRECTIONS FOR RESEARCH

The succession of 'new' approaches to policy has been endless, but these approaches are rarely *methodologically* innovative: their 'newness' represents quibbling about the direction or thickness of arrows connecting boxes, the number of items in a checklist, or whether strategy causes structure or vice versa. The reorientation advocated here is methodologically new—calling for greek letters in place of boxes and arrows—and more extreme than most (cf. Caves, 1980; Jemison, 1981b; Porter, 1981; Wensley, 1982). I propose adopting the methodology and some substance of disciplines that are rigorous—game theory, industrial organization and decision theory—and especially emphasizing interdependent strategic thinking in the game-theoretic sense.

Since talk about methodology is invariably both difficult to argue with and boring, it is best to illustrate my ideals with some specific directions for research.

(1) In Porter and Spence's (1982) sophisticated case study of innovation in corn wet milling, they studied the capacity expansion decisions made by firms after the sudden development of high-fructose corn syrup (HFCS) as a viable sugar substitute. Along with extensive case details, they describe a game-theoretic model in which firms' fates are entangled: if other firms build too much, one firm suffers by building extensively (industry over-capacity); but if firms build too much, one firm can profit by building extensively (industry under-capacity). All firms are assumed to have rational expectations, so they recognize this incentive structure (and believe that the others recognize it too), and collectively 'decide' on some moderate level of capacity. (Their model thus makes a

prediction about the equilibrium industry capacity level.) However, their paper violates the norms of economic theory when necessary, to make strategic behaviour realistic (e.g. firms do not merely maximize expected value).

(2) The 'new theory of the firm' or 'new institutional economics', revived by Williamson (1975, 1979) and others (Klein, Crawford and Alchian, 1978) and traceable to Coase (1937) has exciting relevance for policy research (e.g. Dundas and Richardson, 1980). In this paradigm, the economic transaction is the fundamental unit of analysis, and researchers ask whether market deals or risk-sharing with monitoring (e.g. in hierarchically organized firms) are cheaper ways of governance transactions. The relative efficiency of firms and markets depends on the nature of the transactions. Is it frequent? Standardized? Do parties build up non-marketable knowledge in making such transactions? The 'transactions cost' paradigm provides a clear, logical approach to the 'supply side' of business strategy. With badly needed mathematical extensions, it should be part of a new tradition in policy research.

(3) Balas's (1980) obscure game-theoretic analysis of the optimal size of the U.S. strategic petroleum reserve (SPR) seems a useful analogy—perhaps a direct one—to the problem of modelling entry barriers. Balas considers both the *deterrent* effect of an SPR (exporting nations will not embargo if the SPR is large) and the obvious benefits of having a large SPR if an embargo does occur. Surprisingly, the deterrent effect is the more important of the two effects, and he is able to calculate exact estimates of how big the SPR must be to completely deter embargo.

(4) Recent theoretical work by Kreps *et al.* (1982), has given new, much-needed formality to the old idea of 'reputation' of repeated game-playing. Assuming that players have incomplete information—either about their opponent's utility payoff (Kreps and Wilson, 1982) or behaviour (Milgrom and Roberts, 1982)—they show that co-operation in the early periods of a finitely repeated prisoner's dilemma is rational. This has never been proved before; but the *generality* of the 'reputation' concept is its real strength. They show how reputation can lead to effective entry deterrence by incumbent firms, but reputation for effectiveness in flexing any sort of competitive muscle—distribution networks, marketing advantages, 'culture'—in many settings could presumably be established by firms, and studied by theorists, with similar techniques.

(5) Klein and Leffler (1981) show how premiums above competitive prices can yield a stable, equilibrium 'quality-assuring' market price when quality is unknown to consumers before purchase. Their model is rich with strategic implications: they discuss the role of repurchase frequency (producers of durables will have more trouble assuring quality than deodorant-makers); 'economies of scale' in quality-assurance (thus providing some rationale for selling unrelated products under a single umbrella) and the importance of advertising and brand-name capital as a quality-assuring force.

(6) The notion of market 'contestability' formalized by Baumol, Panzar and Willig (1982) provides one new foundation—albeit an empirically debatable one—for theories of industrial organization. Markets are contestable when entry and exit are free, so that 'hit-and-run' entrants can build plant, produce and sell to price-sensitive consumers, and shut down while salvaging plant value. In contestable industries (which may have any number of firms), an incumbent must choose 'sustainable' prices which deter entry. However, a set of sustainable prices for different time periods may be very hard to find—they show how a natural economic 'changing of the guard' can happen over time even if all firms act rationally with foresight (see chapter 13) due to the economics of intertemporal production.

Their result is rare for its combination of shallow algebra and deep implications, and their model provides a useful new way to study entry without contrived assumptions about technological discoveries, new information, or irrationality.

(7) Psychology may tell us something about the genesis and persistence of strategic 'blind spots' (Porter, 1980:60), which are a cognitive element of economic disequilibrium. For example, conjectures about environmental relationships between variables may be systematically wrong when the 'cues to causality' that direct causal perception are incorrect (Einhorn and Hogarth, 1983); so it may be 'well known' that selling panty hose in supermarkets is foolish, but Hanes has been successful doing just that with L'Eggs. Work in problem 'framing' (Tversky and Kahneman, 1981) is also relevant, since many corporate innovations are made by outsiders who *see* business situations differently. In general, psychological concepts such as 'imagination' and 'creativity' probably correspond better with the important observable qualities of successful entrepreneurs than do economic concepts such as unusual risk preferences or utility functions.

(8) The concept of 'economies of scope', long understood and only recently formalized (Baumol, Panzar and Willig, 1982), has brought new rigour to the study of the strategically important problems of the multi-product firm. Economies of scope are cost reductions that result when resources can be shared across product lines. Since these cost reductions are much of the basis for 'synergies' sought by diversified firms, their study is crucial (Teece, 1982). Since many economies of scope arise from sharing of information (about details of consumer tastes, for instance) across product lines, the calculus of such economies will require a better understanding of how information moves within, and across, organizations; but with such an understanding, many of the complex decision problems facing multi-product firm managers could conceivably be reduced to solvable maximization problems.

(9) Observers of financial markets have often noted the 'herd behaviour' or 'market psychology' that seems to explain much of the variation in asset prices in such markets, and similar forces often seem to determine industry-wide strategic decisions. Usually such behaviour is attributed to bad cognition (e.g. 'groupthink'); to conformity pressures; to reduction of regret (Bell, 1982, 1983) if projects fail, since everyone else failed also, etc. Although these hypotheses undoubtedly explain many episodes of mindless herd behaviour, agency problems resulting from incentive structure may provide a more parsimonious, and much more testable, explanation. Suppose managers cannot be evaluated solely on absolute performance since such an evaluation imposes too much uncontrollable risk on a manager; so it becomes sensible to use measures of performance *relative* to the performance of an appropriate peer group. Then, managers have a slight disincentive to avoid actions that are different from actions their peers are taking, and this disincentive may wholly discourage any departures from the 'herd' behaviour in some cases. The 'man-in-the-street' claim that media coverage is 'biased' or certainly very fickle may be explained by these incentive effects: performance of a newspaper reporter, for instance, is essentially impossible to evaluate absolutely, but it is possible to evaluate relative performance. Then, reporters are somewhat discouraged from covering stories other than those their peers are covering, since such renegade action introduces variance into the renegade's relative performance, and variance lowers expected utility. Thus, reporters all end up covering similar stories, and shifting dramatically from story to story when it appears the 'herd' is changing direction. This logic can be formalized (e.g. Holmstrom and Weiss, 1982), applied to industry-wide strategic decisions, and probably tested more definitively than the appealing 'behavioural' hypotheses specified above.

Although business school curricula have often been slow in responding to changes in basic research orientation (e.g. in finance), it is important to change the teaching of policy and strategy; and a teaching shift may be at least as important (and more dramatic) than a shift in research orientation.

The conventional way of teaching policy wisdom (or the conventional models and texts) is only marginally helpful to students, mostly because it is not intellectually challenging. Conventional wisdom about policy may be correct, but like knowledge gleaned from case study, it may be better learned from actual business experience. Many MBAs are phenomenologically equivalent to 3 months of work experience crammed into 2 years.

The idea of 'comparative advantage' is crucial in determining what topics should be taught, and what information should be conveyed through media other than the classroom. For instance, Buchholz reports the following statement, 'typical of the response of these executives (to a survey)':

> Learning to understand the external environment, and to consider its impact in making management decisions, has become a most necessary skill for every successful manager... No business decision today can be based solely on traditional business rationale and be successful (1982:xii).

Businessmen are undoubtedly correct that such issues are important to managers, but the argument that 'the subject matter is so important to management education that it deserves to be a fundamental piece of the curriculum' (Buchholz, 1982:xii) involves an invisible step of logic: namely, to say that an issue is important to *management* does not necessarily imply that it is important to formal management *education*, since business school teachers may not be especially well equipped to efficiently teach students about such issues. I repeat: my quarrel is not with the importance of fuzzy issues such as 'social responsibility', but with the naïve presumption that traditional case study courses are the best vehicle for imbuing students with an understanding of these fuzzy issues.

In defending the new approach to teaching policy we can invoke Whitehead:

> ...Whatever be the detail with which you cram your student, the chance of meeting in after-life exactly that detail is almost infinitesimal; and if he does meet it, he will probably have forgotten what you taught him about it. The really useful training yields a comprehension of a few general principles with a thorough grounding in the way they apply to a variety of concrete details. In subsequent practice they (students) will have forgotten your particular details; but they will remember by an unconscious common sense how to apply principles to immediate circumstances (1963:62).

Traditional policy analysis is largely concerned with case details and broad principles, and teachers seem to feel that students will at least remember these principles when managing firms (perhaps because they have taught themselves the principles—see Gragg, 1940). However, little is taught about truly *new* ways to conceptualize strategy—i.e. the 'few general principles' of Whitehead. Whereas philosophy courses teach people to think like philosophers, and physics courses teach people to think like physicists, policy courses have only 'taught' (or reminded) people to think, when they are policymakers, with a modicum of logic and common sense as they normally would. In the new approach to research and teaching, students will forget details and principles but remember (usually unconsciously) new, non-obvious ways of thinking strategically.

This highbrow classical justification for liberal arts training may be tainted by the grubby needs of professional schools: not much theory is taught in law schools or medical schools because students are busy learning the myriad facts they need to know to practice their professions. It could be argued that business schools should function similarly, cramming students' minds with mundane facts needed for a managerial career, and this argument resurrects the need for case-method and other atheoretical approaches. But I am sceptical that a body of myriad essential details exists currently: the fact that many successful businessmen are not formally educated attests to the lack of such a necessary body of facts. By contrast, there are virtually no self-taught doctors or lawyers.

CONCLUSION

Because its 'checklists' are usually drawn from inductive synthesis of cases, rather than from deductive construction of conclusions, policy research has been ambiguous, untested, and only barely progressive. Fortunately, recent advances in economic theory have pushed that field beyond its obsession with a drab, non-strategic equilibrium; so recent economic concepts provide a useful basis for deductive thinking about corporate strategy.

I argue that deductive theorizing may be one useful way to approach policy issues (although hardly the only way), and the inherent lack of realism of such theories should dissipate, since the great strength of deductive models is how they can be extended and made more realistic, as assumptions are relaxed, over time.

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