

[**Editor's Note:** The following two Comments were written to accompany the Robinson *et al.* paper on First Movers (this *Review*, Vol. 9, No. 1, January 1994). Because of editorial problems, they appear separately here.]

First-Mover Advantages from Pioneering New Markets: Comment

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The survey by Robinson, Kalyanaram, and Urban, is a valuable contribution on one of the most important concepts in the field of industrial organization. Unfortunately, “I-O” specialists have subjected the first-mover phenomenon to empirical research much less than proportional to the apparent power of entry order variables in explaining market structure and market participants’ profitability. Thus, we are in debt to business scholars for illuminating the relevant relationships, and we are in debt to Robinson *et al.* for bringing what has become a sizable literature into focus.

I have few complaints about the survey itself. My principal unease concerns the still-unsettled problem of semantics and its implications for inferring the profitability and risks of first-moving. Robinson *et al.* accept the definition of Lieberman and Montgomery: the market pioneer is the market’s first entrant, and a firm must have reached a competitive scale of commercialization to be recognized as an entrant. This seems a difficult concept to operationalize. As Robinson *et al.* note, there may be significant differences in the identity of first movers, depending upon whether one’s vantage point is the time when innovations are underway, or with 20–20 historical hindsight after the winners and losers have sorted themselves out. Also, by what combination of strategy and luck do the “early market leaders” of Golder and Tellis move ahead of the surviving market pioneers? Are the distinctions related to Alfred Chandler’s notion (1990, p. 35) that only by making “three-pronged investments” in large-scale production, distribution channels, and management structures can firms earn the status of first movers?

This is important, because if many try to innovate but only a few succeed, the handsome profits earned by those who *ex post facto* are called first movers may be offset by the losses of those who failed. See Glazer (1985). The pharmaceutical industry provides an illustration. Examining the profitability of 100 new chemical entities that entered the U.S. market (after FDA approval) during the 1970s, Grabowski and Vernon (1990) found that seven-tenths of the drugs had discounted

quasi-rents below, and usually well below, average R&D investments. Ten of the hundred – the big winners – generated the lion's share of sales and profits. Their gains were sufficient to let the cohort of new drugs as a whole earn a roughly normal return on investment, but with a skew distribution of gains and losses, this is by no means assured. There is reason to believe that the rewards in other branches of technological innovation are equally skewed. Thus, we need more research on expected profitability, taking into account the losses of those who do not graduate to first mover status along with the gains of those who do.

The Xerox example chosen by Robinson *et al.* shows how difficult the definitional task can be. They quote a *Washington Post* article asserting that the commercial potential of plainpaper copying was not recognized at first. Clearly, it was recognized by Chester Carlson, who was obsessed by the concept. Haloid (later Xerox) devoted 13 years to its realization, introducing xerographic mastering machines for duplicators along the way. When the 914 copier was introduced in 1959 (not 1958, as stated by the authors), I recall (as a student at Harvard Business School) immediate and enormous excitement over the innovation. By 1962, Harvard University, not exactly noted for its alacrity in adopting technological innovations, had made Xerox copies the only permissible alternative to original type-scripts or carbon copies for the Ph.D. dissertations filed in Widener Library. But it did take some time for xerography to realize its market potential. Blackstone (1968, p. 29) reports that xerography accounted for only 2% of total copier sale and rental revenues in 1960, 12% in 1961, and 55% in 1965. As late as 1968, we were still duplicating class notes at the University of Michigan by a messy stencil process because Xerox was considered too expensive. For Michigan economics faculty, at least, other processes *were* viewed as close substitutes. If determining *when* Xerox became a first mover in this unusually simple case is difficult, how much more difficult must the complex cases be?

One of the most remarkable things about the concept of first mover advantages is how late it emerged in the industrial organization literature and how rapidly it caught on once it did emerge. The earliest citation in the extensive bibliography provided by Robinson *et al.* is to a 1972 work (to which I shall return momentarily); the next two are dated 1977. The mean year is 1987. This right-hand skewness probably does reflect the state of the art fairly well. Although I have been impressed repeatedly at the rich insights into scale economies, pricing behavior, and much else displayed by economists such as Jeremiah Jenks, Arthur Hadley, Eliot Jones, Robert Liefmann, and others a century ago, I can recall no clear statement of what we now recognize as the order-of-entry phenomenon. In his classic work on barriers to entry, Joe S. Bain comes close, but does not quite ring the bell (1956, pp. 142–143):

Although the simple force of heavy advertising plays a significant role in most cases, the strategic underlying considerations in strong product differentiation seem . . . associated with poor consumer knowledge or ability to appraise products, and thus with dependence on "product reputation" . . . All of these things... suggest the existence of fundamental technical considerations . . . and more or

less fundamental consumer traits which make possible or even very probable the development of strong and stable product-preference patterns. They may also suggest that advertising *per se* is not necessarily the main or most important key to the product-differentiation problem . . . [W]e may need in general to look past advertising to other things to get to the heart of the problem.

The most important “other thing” - order of entry - was not explicitly identified.

A brief history of how my own thinking on the problem evolved might help fill in the chronological gap before 1977. I do not claim first mover status. As Wassily Leontief once observed, everything in economics is reinvented every 20 years, and although Leontief’s Law probably applies less strongly here than in other areas, I can claim at most to have been one of the pioneers who never quite consummated Chandler’s three-pronged investment.

At the Harvard Business School, I collaborated with eight other MBA students in conducting a survey of corporations’ views on the patent system and pulling the results together into a monograph. We were surprised to discover that for most corporations, patents played only a minor role in R&D decisions. Much more important was “the necessity of maintaining competitive leadership” and “profits resulting from customer belief in the company’s technological leadership.” Scherer *et al.* (1959, pp. 117–119). Shortly thereafter, I studied the economics of weapons research and development, where, it was clear, being first was accorded overwhelming importance. I also did some consulting on the Tetracycline antitrust cases, from which I learned that the original innovators held their market shares rather well at prices of \$51 per 100 capsules when generic knock-offs were selling at roughly \$20. When I received my Ph.D., my thoughts turned to modelling the role of lead time advantages in civilian sector R&D. One parameter of my model was a “permanent erosion coefficient,” which measured the rate at which an imitator’s market share deteriorated permanently over time with each year of lag in the introduction of its new product *vis a vis* the product of the first innovator. I was surprised to find that my model yielded R&D cost-increasing reactions among R&D rivals only when the permanent erosion coefficient was non-zero. The conclusion drawn was (Scherer 1967, p. 381):

We find then that the permanent share erosion coefficient ϵ has a critical influence on innovator reactions in both new market and market-sharing rivalries. The higher ϵ is, the more likely aggressive reactions will tend to be. This conclusion, while not obvious, is intuitively appealing. When permanent instead of only temporary slices of an existing or new market are the prize for technological leadership, innovators should naturally be more inclined to fight to preserve their leadership positions against the reactions of rivals.

Needless to say, it is disconcerting to learn that Robinson (1988) found no statistical support for my reaction hypothesis. I have observed positive reactions in case studies, but my own recent work on innovation rivalries across national borders reveals both positive and negative reactions which tend to cancel out in cross-sectional samples. Scherer (1992). The complexity of the reaction patterns may be the source of difficulty.

Given this background, my excitement was great when I was shown a draft of my

then-colleague Geoff Shepherd's (1972) paper – the earliest reference cited by Robinson *et al.* That market share was more important in explaining profitability than the four-firm concentration ratio seemed to say that having a large market share was accompanied by significant advantages. My own parallel research at the time suggested that economies of scale had pretty well petered out at the size levels realized by leading U.S. firms. Where then did the advantages lie? Product differentiation seemed the likely alternative, but what was the mechanism?

There matters stood until, as head of the FTC's Bureau of Economics, I was given a draft of the Bond and Lean report (1977) for review. It appeared to crack the unsolved riddle left by Shepherd's article. This supposition was reinforced when I participated in the conference at which Buzzell and Farris (1977) presented their pioneering paper. Those two works are the second- and third-oldest contributions in the list of references by Robinson *et al.* They indeed precipitated the intensive exploration of first-mover models both theoretically and (especially) empirically. The rest is history.

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