

Edwin Mansfield: An Appreciation

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In January of 1985, I received from the committee in Sweden an invitation to nominate candidates for the Nobel Prize in Economic Science. The instructions made it clear that my nominations were to be strictly confidential. But nothing should be kept secret for more than two decades, so now I tell my tale. I nominated two persons, listed in alphabetical order: Edwin Mansfield and Robert Solow. As a preface to my exposition of reasons why those two should receive the Nobel Prize, I observed that "It is now generally accepted that in advanced economies, the growth of material well-being has depended critically upon technological change." Solow's 1957 article, I have observed frequently, was a shot heard 'round the world, elevating the study of technological change to a status far above what it had been — a neglected sideshow attraction in economics. My rationale for Mansfield was as follows:

"Professor Mansfield has painstakingly collected and analyzed within-the-firm microdata to illuminate the nature of industrial research and development, revealed in numerous scholars' studies to be a key, if not the most important, institution through which technological change is effected. His work on uncertainty, decision-making criteria, market structure, the speed of diffusion, R&D — productivity links, and much else is collected in 1968, 1971, 1977, and 1982 books."

In 1987, Bob Solow received the Nobel Prize, which pleased me greatly. But in its infinite wisdom the committee chose not to have Ed Mansfield share the prize. Perhaps the fault was mine

for saying that Mansfield's work was summarized in books, ignoring the scores of articles on which those books were based. The committee may have been showing its change of heart toward blockbuster books and its increasing emphasis on articles with an important but sharply focused contribution to knowledge. Whatever the reason, I continue to believe that the committee made a mistake in not honoring Ed Mansfield and his research.

1. Beginnings

It remains somewhat of a mystery, at least to me and many who were close to him, how Mansfield was inspired to begin his research on the economics of technological change. In tributes to Eugen von Boehm-Bawerk and later to Carl Menger, Joseph A. Schumpeter characterized the third decade of a scholar's life as "that period of sacred fertility which, in the case of every thinker, creates what is subsequently worked out." Mansfield (1961) was in substantive respects a disciple of Schumpeter, but he did not conform to the master's biographical paradigm. His first publication on the economics of technological change appeared in *Econometrica* in 1961, when he had passed through his third decade to the age of 31.

Mansfield's first publications, extending his Duke University dissertation on city size and per capita income, were in urban and labor economics. When he arrived at Carnegie-Mellon University (called at the time the Carnegie Institute of Technology) in 1955, he joined senior colleague Harold H. Wein in a study of decision-making in railroad switching yards. Wein's specialty was public utility economics, which may explain the railroad focus and the supporting grant from a Pittsburgh railroad equipment supplier. The

research, however, exhibited classic Carnegie-Mellon methodology of the time, emphasizing in an operations research framework managerial risk aversion and adaptive expectations.

The first visible indication that Mansfield had shifted his focus to technological innovation was his participation as a discussant at the famous spring 1960 National Bureau of Economic Research conference at the University of Minnesota, leading to the classic 1962 compendium (edited by Richard R. Nelson), *The Rate and Direction of Inventive Activity*. That conference marked a beginning point for scholarly interaction among economists on technological change. In what would prove to be a Mansfield hallmark, he devoted only the first four paragraphs of his commentary to the William Fellner paper he was assigned to discuss and then turned to his own work modelling company research and development expenditure patterns, “only the first parts [of which] have been completed.” The research was conducted under a grant from the National Science Foundation Office of Special Studies, which was responsible for the first systematic surveys of research and development expenditures in American industry. Much of Mansfield’s later work was supported under grants from the successors to that organization as well as the Economics branch of the NSF Social Sciences division.

From the timing of that presentation and the assertion that it was in its early stages, one infers that Mansfield turned to technological innovation and imitation questions in 1958 or at the latest in early 1959. At the time Carnegie-Mellon had no tradition of work on technological change. One might guess that he was introduced to the field’s research agenda by Richard Nelson, a pioneering contributor, but Nelson said in an interview that Mansfield was already well underway on his new research by the time Nelson first visited Carnegie-Mellon in 1959, joining the faculty formally in 1960. In the prefaces to his first two books on innovation, Mansfield thanks George Leland Bach for encouraging him to work in the area. At the time Bach, a monetary economist with no professional publications in the field of technical change, was dean at Carnegie-Mellon. It is possible that he participated in a Ford Foundation committee concerned with

technological change during the 1950s. We remain in doubt as to how much of Mansfield’s agenda was self-initiated and how much came from Bach’s suggestions. Mansfield’s wife Lucile reported that a relevant element in Ed’s change of field was his desire to be his own master and not to play a supporting role under another scholar’s grant. The grant supporting Mansfield’s work with Wein had run out, so the availability of research funds from the National Science Foundation may have influenced his choice. I know from later conversations that he maintained close relationships with several National Science Foundation grant officers. But it is impossible in hindsight to disentangle demand-pull influences from the science-push that came inter alia from Leland Bach and Mansfield’s reading of the nascent literature. What is clear is that he thereupon persisted in pursuing his technology agenda for nearly four decades.

2. Methodologically pioneering

Mansfield’s typical research methodology differed by several robust standard deviations from what has been conventional in economics, and especially for fellows of the Econometric Society, which he was. His initial work was conventional enough. For the preliminary work he presented at the 1960 *Rate and Direction* conference, Mansfield drew data on company R&D expenditures from an M.I.T. dissertation by Charles Langenhagen, applying to them an adaptive expectations analytic framework. Langenhagen obtained his data in part from published corporate reports, but in those pre-1975 days before the Financial Accounting Standards Board required companies to disclose “material” R&D expenditures in their annual reports, he filled in data gaps by asking companies for the needed figures. It is not unreasonable to infer that Mansfield recognized he could do the same thing, and so for the extension of that research published as Mansfield (1964), he asked companies for supplemental data. These included R&D data and also, from face-to-face interviews with a subset of companies, estimates of the frequency distribution of expected profit returns from R&D, how difficult it was to expand research efforts, and how actual R&D