Technology and Technology Transfer: Mansfieldian Inspirations and Subsequent Developments

David J. Teece

ABSTRACT. This paper discusses the foundational work and ideas of Edwin Mansfield to the economics of technological change and innovation, and introduces some of the recent work in the field. I argue that much of the recent work on patenting, technology strategy and the economics of knowledge has roots to the early Mansfield contributions, and that he should be recognized as a pioneer for these recent developments.

Key words: economics of innovation, knowledge, intangible assets, R&D management

JEL Classification: O32, O34, L10

1. Introduction

At least since Joseph Schumpeter, scholars have struggled to understand the nature and the dynamics of the economics of technical change. Edwin Mansfield was born into that struggle and was for many decades a true pioneer in the study of the economics of technological change. His early books including 'The Economics of Technological Change' (1968), and 'Technological Change: An Introduction to a Vital Area of Modern Economics' (1971) summarize his early insights and display his passionate desire to wake up the field of economics to a critical area of research. He undoubtedly was the leader in the study of the nature of industrial research in America, certainly during the period of his active scholarship, and arguably to this day.

Although both classical economists and 'modern' economists such as Solow, Nelson, David, Rosenberg, and Kuznets had recognized the importance of innovation and understood it's key role in economic growth and wealth creation, it was not until Mansfield that anyone had performed serious empirical studies of industrial research. Mansfield provided leading insights into issues such as the role of academic and basic research in increasing innovation and productivity, the diffusion of technological innovations, the private and social returns to innovation, and the role of patents and the patent system. With great wisdom, Mansfield chose areas of study that have emerged as being critically important to managers and policy makers.

However, Ed Mansfield showed considerable frustration with modern economics and the work of economic theorists. Indeed, by the 1970s Ed openly displayed almost a disdain for modern economic theory because of the field's infatuation with static analysis, and its abject failure to embrace the study of technology and technological change.

As one of Ed Mansfield's students, I must first acknowledge my huge dept to him personally and intellectually. As a graduate student at Penn in the early 1970s, I was fortunate to end up in his Ph.D. class on the economics of technological change. He opened my eyes to a set of issues for which I had no previous exposure. Because I had a background in international trade and finance and economic development, he encouraged me to study technology transfer. No one at that time, including Ed, knew much about the topic. We learned together, with Ed sending me into the field to collect data and absorb what I could from corporate R&D managers, from licensing executives, and from the experiences of the international departments of the Fortune 500. Some of my findings, along

Director, Institute of Management
Innovation and Organization Professor
Haas School of Business
University of California
Berkeley, U.S.A.

with my reflections on those findings, are discussed in Section 3.

Besides developing a substantive understanding of technology transfer, I learned quite a lot methodologically from Ed. He was a well-recognized statistician with a good nose for data. He was comfortable working with small samples. He let the data, not theory, lead him to answers. In fact, much of my work and methodological approaches can be seen as combining Mansfield's insights and approaches with other traditions, in particular transaction cost economics, and evolutionary and behavioral theory.

In the rest of this paper I shall describe in more detail the intellectual influence of Ed Mansfield on my work on the economics of technological change and technology transfer. I shall track some of the recent developments with respect to these early ideas and mention how recent work builds on the early Mansfield studies. In doing so I hope to demonstrate that his influence was substantial, and that his legacy in the field deserves more recognition. If Schumpeter founded the study of the economics of innovation,[1] then Mansfield was the first to give it empirical meaning at the micro level.

2. Mansfield's vision and early work

One of many lessons that I learned from Mansfield—and he in turn was undoubtedly shaped by his early years at Carnegie Mellon University (which in the late 1950s and early 1960s when Mansfield was there had scholars such as Herb Simon, Dick Cyert, Jim March, Franco Modigliani and Bill Cooper, among others)—was the importance of interdisciplinary research. As a young graduate student, I wanted to believe that the hard problems of the world were solvable. I came to realize with Ed's help that this would require a multidisciplinary approach. Mansfield always made the case for interdisciplinary research. In his later years he wrote:

> "[the economics of technological change] remains an area where there is particular need for people who are comfortable working in, and drawing on, a variety of disciplines. Very few problems of any consequence can be solved within the confines of a single discipline. It continues to require persons who have a lively interest in both basic and applied work, and who are able to use each to enrich the other. It is still an area needing people who like to work on ill-defined problems where little is known and nothing is tidy, but where the rewards for even a partial solution are very high. Those with such attributes should be encouraged to enter this field because the opportunities continue to be enormous. While a lot more is known now than 40 years ago, the truth is that economists have only scratched the surface' (Mansfield, 1995, p. xxi)."

This was the mantra Mansfield had been advancing to his students for over 20 years. It was good advice, although risky for a young economist to follow. Ed was keenly aware how little was known about innovation and industrial research. Mansfield, like March and Simon and the Carnegie School, was ahead of his time, substantively and methodologically. Half a century later David Kreps would write: 'I am increasingly convinced that economists should—and will—have to change large pieces of the paradigm that has kept us relatively monolithic for the past 50 years.- We'll increasingly look like and work with our colleagues in the other.. social sciences' (2004). Were David Kreps a Mansfield student, he would have realized this much earlier.

Thinking outside the box of conventional economics was particularly necessary when it came to issues of the economics of technical change. For one thing, neoclassical economics can not address issues of change other than comparative statics (Machlup, 1967) because even adjustments to equilibrium are outside the domain of neoclassical economics. As a result, neoclassical theory can not really deal with issues of innovation. Ed recognized this, but few others did.[2]

Mansfield's methodological response was always to start first with observation (influenced, perhaps, by the 'problem driven' research that was present at Carnegie). He encouraged me—as well as his other students and colleagues—to collect data in the field. This was extremely wise. Late in life he reflected on this method, in the introduction to the two volumes of collected papers of his:

> "In general, my approach has been to try to get a reasonably solid empirical footing before attempting to model complex phenomena about which very little is known; to keep the theoretical apparatus as simple, transparent and robust as possible;