Hagstrom W O. *The scientific community*. New York: Basic Books, 1965. 304 p.
[University of Wisconsin, Madison, WI]

The scientific community is described as one in which relatively autonomous scientists exchange new information for recognition from their peers. This conception informs analyses of collaboration, competition for priority, the organization of specialties, disciplinary differentiation, and the conduct of intellectual disputes. [The *SCI*® and *SSCI*® indicate that this book has been cited in over 440 publications.]

In at the Ground Floor? Or Constructing Foundations?

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September 21, 1988

Works published early in the rapid growth of a specialty are disproportionately likely to receive many citations,[1] and the sociology of science grew rapidly in the years following the publication of *The Scientific Community*.[2] So one might say that I was lucky, just as I was lucky to be born 20 years before the baby boom into an age cohort with unusually good opportunities for upward mobility. On the other hand, I might argue, at the risk of immodesty, that works like *The Scientific Community* helped generate that rapid growth in the sociology of science. (I also did my little bit to produce the baby boom, but ideas can multiply more rapidly than people.)

The book grew out of my doctoral dissertation at Berkeley. It was based largely on unstructured interviews with a sample of physical and biological scientists, supplemented by analysis of available survey data and by a review of a wide variety of publications produced by such scientists and by sociologists and historians of science, the work of Robert K. Merton being especially influential. Despite my prior reading, I felt quite ignorant when I began my interviews. Tom Scheff successfully prodded me to get into the field quickly, before I felt I was "ready," arguing that one does this kind of exploratory research *because* one is ignorant.

My scientist informants were quite helpful, treating me as an "acceptable incompetent," a role easier to play when one is relatively young. I selected a purposive sample of scientists, old and young, eminent and not-so-eminent, solitary workers and organizational leaders. One of the unanticipated benefits of the study was the opportunity to talk to some memorable characters. Although I assured them of anonymity (readers of the book will understand why), it won't hurt to name some of them now. I conversed with such men as Renato Dulbecco, Richard Feynman, Murray Gell-Mann, Edwin McMillan, Jerzy Neyman, and Robert Sinsheimer. (All men: one of the things that dates the book was my almost complete indifference to the role of gender in science.)

The kind of exploratory research I did can generate hypotheses but provides, at best, illustrative evidence for them. Testing the hypotheses requires more systematic research, such as survey research with representative samples of scientists or using sources such as the *Science Citation Index*®, which can provide a quantitative measure of "recognition." My own survey research[3] provided systematic evidence for my hypotheses about competition in science, but most of the research, not all of it lending support to my hypotheses, has been done by others.

One final note: The title of the dissertation was *Social Control in Modern Science*. With that title I'm sure it would never have become a *Citation Classic*. I believe Bernard Barber suggested the more fitting title to the publisher, and I'm grateful to him.

1. **Hargens L L & Felmlee D H.** Structural determinants of stratification in science. *Amer. Sociol. Rev.* 49:685-97, 1984. (Cited 10 times.)
2. **Cole J R & Zuckerman H.** The emergence of a scientific specialty: the self-exemplifying case of the sociology of science. (Coser L. A. ed.) *The idea of social structure: papers in honor of Robert K. Merton*. New York: Harcourt Brace Jovanovich, 1975. p. 139-74. (Cited 55 times.)
3. **Hagstrom** W O. Competition in science. *Amer. Sociol. Rev.* 39:1-18, 1974. (Cited 55 times.)