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Transcending disciplines: Scientific styles in studies of the brain in mid-twentieth century America

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ABSTRACT

Much scholarship in the history of cybernetics has focused on the far-reaching cultural dimensions of the movement. What has garnered less attention are efforts by cyberneticians such as Warren McCulloch and Norbert Wiener to transform scientific practice in an array of disciplines in the biomedical sciences, and the complex ways these efforts were received by members of traditional disciplines. In a quest for scientific unity that had a decidedly imperialistic flavour, cyberneticians sought to apply practices common in the exact sciences—mainly theoretical modeling—to problems in disciplines that were traditionally defined by highly empirical practices, such as neurophysiology and neuroanatomy. Their efforts were met with mixed, often critical responses. This paper attempts to make sense of such dynamics by exploring the notion of a scientific style and its usefulness in accounting for the contrasts in scientific practice in brain research and in cybernetics during the 1940s. Focusing on two key institutional contexts of brain research and the role of the Rockefeller and Macy Foundations in directing brain research and cybernetics, the paper argues that the conflicts between these fields were not simply about experiment *vs.* theory but turned more closely on the questions that defined each area and the language used to elaborate answers.

1. Introduction

At the Seventh Macy Conference on Cybernetics, held at the Beekman Hotel in Manhattan's Upper West Side in March 1950, the University of Chicago physiologist Ralph W. Gerard (1900–1974), a regular, enthusiastic participant at the Macy meetings, presented a critique of what he referred to as "digital notions" in the nervous system. He introduced his remarks with an observation on the mode of communication that for him seemed to dominate discussions at the meetings:

> It seems to me, in looking back over the history of this group, that we started our discussions and sessions in the "as if" spirit. Everyone was delighted to express any idea that came into his mind, whether it seemed silly or certain or merely a stimulating guess that would affect someone else. We explored possibilities for all sorts of "ifs". Then, rather sharply it seemed to me, we began to talk in an "is" idiom. We were saying much the same things, but now saying them as if they were so.[1]

Gerard saw dire consequences for the recklessness he perceived in the transition from this tentative, creative "as-if" spirit to the certainty he now saw attached to such speculations. The ramifications applied both to members of the cybernetics group and to their wider audience, which Gerard envisioned as including both the "lay intelligentsia" and "that precious company of young physical scientists now finding the happy hunting ground in biology."[2] Members of the cybernetics group, in Gerard's view, had legitimate internal and external responsibilities as scientists. Internally, each member of this interdisciplinary group was obliged to communicate as clearly and precisely as possible to each other. With the group being comprised of members from very diverse fields, Gerard noted,

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[1] Gerard (1951, p. 11).
[2] Ibid.

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"no one can be sure another's statements are facts or guesses unless the speaker is meticulous in labeling suggestions as such." As for the group's external responsibility, which Gerard felt was even greater, the group also must be sure to be clear communicators, and not give a "spurious certainty to a credulous audience."

Two aspects of Gerard's remarks are of note. First, in his allusion to an "as-if" spirit, Gerard was pinpointing a particular mode of discussion that for him had come to characterize the exploratory and often heated exchanges that had defined the Macy Foundation-sponsored meetings on cybernetics, which took place between 1946 and 1953. Part of a trend towards interdisciplinary collaboration had begun to define the postwar period in American science,[3] these gatherings involved exchanges between investigators from diverse fields such as psychology, mathematics, sociology, neurology, psychiatry, biology, anthropology, physics, and engineering.[4] Second, Gerard's reference to physicists finding "happy hunting ground in biology" was a reference to a disciplinary dynamic that had come to characterize American life sciences in the mid-twentieth century—the rise of molecular biology being the most oft-cited result of this intellectual migration.[5]

Focusing on disciplinary exchange, tension, and conflict is a useful perspective from which to examine this period in American science, particularly the work of the cybernetics group. Just as transformative as the migrations that led to the rise of molecular biology, I would argue, were the migrations of other figures from the exact sciences into other areas of biomedicine. Indeed, the core members of the cybernetics group itself were physicists, mathematicians, and engineers tackling problems of living systems—in many cases, problems that had traditionally fallen within the purview of the brain sciences. While much scholarship in the history of cybernetics has focused on the far-reaching conceptual, cultural, and technological dimensions of the movement,[6] what has garnered less attention are efforts by cyberneticians such as Norbert Wiener (1894–1964) and Warren McCulloch (1898–1969) to transform scientific practice in an array of disciplines in the life and human sciences, and the complex ways these efforts were received and responded to by members of traditional disciplines. In a quest for scientific unity that had a decidedly imperialistic flavour, cyberneticians sought to apply practices that were common in the exact sciences—mainly mathematical and theoretical modeling—to problems in disciplines that were traditionally defined by highly empirical and experimental practices, for example, neurophysiology and neuroanatomy. Their efforts were met with mixed, often critical responses by members of these disciplines—Gerard's reference to their "as-if" spirit being a typical yet polite response—and in contrast to the efforts of physicists in molecular biology, were only partially and indirectly successful.[7]

In an attempt to make sense of such dynamics, this paper will explore the notion of a scientific style and its usefulness for bringing into focus the contrasts in scientific practice that emerged between traditional brain sciences and cybernetics during the mid-twentieth century. The notion of style has had various uses as an analytical tool in the history, philosophy, and social studies of the sciences.[8] Meanings of style have ranged from a highly idealistic concept of style in the philosophical sense—for example Ian Hacking's notion of "styles of scientific reasoning"—to more historically specific and sociologically mediated notions, such as Jonathan Harwood's concept of style in his study of the German genetics community during early 20th century.[9] My own use of style will fall somewhere in between these two extremes. While the examples I discuss invoke the classical philosophical distinction between empirical and theoretical approaches in the sciences, my ultimate aim is to understand the specific historical conditions that led to the emergence or flourishing of particular styles. In light of this, Harwood's approach will be most relevant here. In his analysis of the idea of "national scientific styles" in genetics during the early twentieth century, Harwood argues persuasively that the contrasts in scientific practice between American and German genetics during this period can be fruitfully understood using the concept of style. Institutional developments and dynamics in these respective milieux help Harwood make sense of such contrasts: the organization of the German university system hindered disciplinary genesis and thus prevented specialization, while American research institutions rapidly expanded during this period and encouraged specialization. American practitioners favoured pursuit of problems that could be pursued through experiment and measurement, with their European counterparts stressing "theorizing on a grand scale".[10] Harwood places his story in the general context of German and American scholarship between the wars. American genetics benefited from institutional strength and expansion, as opposed to German stagnation—or at least the weak institutionalization of genetics in Germany during the interwar period.[11]

While Harwood emphasizes that his model is historically specific and would not apply to different periods, what is applicable to other contexts is his demonstration that differences in style in the sciences, or the contrasts in patterns of scientific work that become characteristic of a particular community, can be explained by focusing on the ways that methodological commitments can be explained institutionally. A straightforward institutional approach can present challenges for accounting for the contrasts in cybernetics and brain sciences, since cybernetics was not a scientific discipline in the traditional sense and thus did not have a clear-cut institutional home. Furthermore, a clear picture of the genesis and development of scientific styles in brain research would require a detailed, comprehensive look at the important institutional contexts of the mid-twentieth century, for example the Chicago "triumvirate" of Northwestern University, the University of Chicago, and the University of Illinois; as well as Washington University at St Louis, Yale University, and Harvard University.[12] My approach in this paper will be only a first step towards illuminating such a picture, and will focus on specific case studies that can be said to exemplify work in both brain research and cybernetics during the 1940s—a transformative decade for both fields.[13]

3 World War II has been touted as a turning point for American science by many commentators as the period emerged as one of close collaboration between the US government and natural and social scientists: e.g. Dupree (1992), Forman (1985), Heims (1993), Leslie (1993), Pickering (1995) and Rossiter (1985).

4 The participants were officially brought together with common interests in understanding phenomena in their diverse fields within the framework of negative feedback mechanisms. Heims (1993) is the groundbreaking historical account of these meetings.

5 Abir-Am (1987), Dev (1990), Kay (1992, 1993), Keller (1990) and Rasmussen (1997).

6 E.g. Bowker (1993), Dupuy (2000), Edwards (1996), Galison (1994), Hayles (1999), Heims (1993), Kay (2000), Mindell (2002) and Pickering (1995).

7 While a strong case could be made for aligning the late-20th and early-21st century fields of connectionism and artificial intelligence with cybernetics, a detailed account of the cybernetic legacy is beyond the scope of this paper.

8 For a useful overview, see Vicedo (1995).

9 Hacking (1992), Harwood (1987, 1993), Kusch (2010) offers a comprehensive historiographical, philosophical, and sociological critique of Hacking's concept of styles of scientific reasoning.

10 Harwood (1987).

11 Harwood's analysis moves beyond institutions to show that the *genesis* of particular styles in the German context can be explained by differences in values and attitudes not confined to genetics itself (Harwood, 1993, Chap. 6–9).

12 For more on the University of Chicago context, see Blustein (1992, 1993).

13 A more comprehensive account of these dynamics can be found in my book manuscript in preparation, Warren S. McCulloch, *Cybernetics, and the Sciences of Brain and Mind, 1930–1970*.