Review Essay

Scientific Styles: Toward Some Common Ground in the History, Philosophy, and Sociology of Science

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Introduction

Several recent studies in the history, philosophy, and sociology of science have argued that there are different ways of doing science, or different scientific styles.[1] The term "style" refers to the peculiar or characteristic ways of an individual or group, to an individual or collective trademark. In a recent monograph devoted to this issue, a scientific style is defined as "a pattern emerging at any level of scientific work (theoretical, experimental, institutional, normative, etc.) that stamps it as characteristic of a person, school, or nationality" (Daston and Otte 1991, p. 227).[2]

The view that one can identify styles in science is not new. Some classic works in the sociology of science by Karl Mannheim (1953) and Ludwik Fleck ([1935] 1979) explored this idea in some depth.[3] In more recent years, the notion of national styles has attracted the attention of

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1. This essay deals with the following works: Jonathan Harwood's *Styles of Scientific Thought: The German Genetics Community, 1900–1933* (Harwood 1993) and Jane Maienschein's *Transforming Traditions in American Biology, 1880–1915* (Maienschein 1991*a*). I also discuss Ian Hacking's views on scientific styles.

2. Daston and Otte (1991), a monograph dedicated to the issue of style in science, contains some important articles. Given that two of the authors discussed here, Maienschein and Harwood, examine the differences between German and American science, of special interest is Harrington (1991), which analyzes German research in psychology in the interwar period.

3. For a brief and clear analysis of Mannheim's and Fleck's notions of style, see Wessely (1991).

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historians. But the idea that one can identify different ways of doing science along national boundaries has also received criticism. Nathan Reingold (1991), for example, claimed that national styles are artifacts of the historian's perspective. According to him, preexisting historiographic assumptions largely determine whether a historian will identify the existence of a specific national style. Lorraine Daston also pointed out the restricted utility of the notion of national style: "The existence and recognition of national styles depends greatly on the ascent of the nation-state as the principal bearer of cultural identity, and this is a rather recent development" (Daston 1991, p. 368). Furthermore, in the development of the various sciences, we will probably find big differences regarding the influence of national settings. Those sciences that concern local practices will undoubtedly be more greatly affected, as happened in the case of sexual science (Robert Nye 1991). I would also suspect it to be characteristic of the medical sciences. Here I am not going to concern myself with the idea of national styles; I will be concerned more fundamentally with whether there are any scientific styles, not where their geographical boundaries lie.

This article explores some views about scientific styles and considers the significance of this concept for our understanding of science. Does the existence of scientific styles indicate something about science beyond the fact that in scientific research, just as in other human activities, people have idiosyncratic preferences, diverse interests, and different ways of doing things? As a springboard, I use two recent contributions to the history of biology that have raised important questions about the analytical categories used to understand science: Jonathan Harwood's *Styles of Scientific Thought: The German Genetics Community, 1900–1933* (Harwood 1993) and Jane Maienschein's *Transforming Traditions in American Biology, 1880–1915* (Maienschein 1991). This article is less a review of these books than an exploration of Harwood's and Maienschein's ideas about styles in science. First, I will present their main theses and summarize Ian Hacking's ideas about styles of reasoning. Hacking, Maienschein, and Harwood have all argued that the concept of style can serve as a basic unit of historical analysis. As we will see, however, their notions of style are fundamentally different from each other. I will then propose that these differences reflect the various goals and interests present in the history, philosophy, or sociology of science. Indeed, the differences are the result of different styles (a sociological style, a philosophical style, and a historical style) of studying science. Has this situation led philosophers, historians, and sociologists to talk at cross-purposes, as it often seems? I will suggest a more uplifting conclusion. Despite substantial differences among the posi-

tions of Hacking, Maienschein, and Harwood, their focus on scientific styles reveals that some common ground is developing among historians, philosophers, and sociologists of science.

Harwood's Styles of Thought

In *Styles of Scientific Thought,* Harwood (1993) analyzes research on heredity carried out in Germany during the interwar period (1914–33). Profound sociocultural differences among the German scientists, Harwood argues, gave rise to two different research styles: "comprehensive" and "pragmatic." Geneticists with a comprehensive style usually pursued problems in heredity that were connected to broad issues in evolution and embryology. They selected holistic approaches and holistic theories over atomistic ones. Pragmatic geneticists focused on narrower problems, such as those of transmission genetics, and they placed more emphasis on practical payoffs. Comprehensive and pragmatic researchers chose different problems, adopted different methodologies, supported different theories, and conceptualized genetic research in distinctive ways. Furthermore, the comprehensives were more interested in literature, music, philosophy, and art than were the pragmatics. They also tended to remain aloof from politics, whereas pragmatic scientists were more likely to engage in party politics. There were institutional differences as well. While comprehensive scientists were attracted to universities, pragmatics were more likely to work in agricultural and research stations. According to Harwood, these patterned differences in values, goals, interests, and scientific approaches comprise two distinct styles of thought (Harwood 1993, chaps. 6 and 7).

He presents Alfred Kuhn's school, which worked on developmental genetics, as a clear example of the comprehensive approach, which was dominant. Erwin Baur's school, with its emphasis on transmission genetics, illustrates the minority pragmatic approach. To see how the different approaches affected theory choice, Harwood analyzes the 1930s debate over a hypothesis of cytoplasmic inheritance, the plasmon theory. The comprehensive thinkers, whose ideological interest in harmonious social order led them to support holistic views, tended to endorse the holistic plasmon theory. The pragmatics, however, tended to reject theories of cytoplasmic inheritance (for a table representing the positions of various researchers, see Harwood [1993], p. 316).

Harwood aims not merely to describe the different styles of thought in the German genetics community but to explain their sociological origins as well. As he sees it, the specific character of early twentieth-century German science can be explained as the cognitive consequence

of the modernization of German society. Comprehensive and pragmatic scientists advocated disparate views of science and favored different roles for scientists in academia because they came from contrasting social backgrounds. The comprehensives came from mandarin or upper-middle-class families, while the pragmatists came from less conservative, less established families. These class differences fostered divergent perceptions of their own social roles. The comprehensives saw themselves as the carriers of the culture (*Kulturträger*), in charge of preserving the spirit of cultivation against the disruptive forces of modernization. Coming from industrial and commercial backgrounds, the pragmatics—whom Harwood defines as outsiders—were less worried about industrialization and defined themselves as scientific experts, not as intellectuals responsible for German civilization. Thus, different social positions led them to embrace distinct values and goals, which, in turn, supported divergent views about the social role of science and the practice of scientific research (Harwood 1993, chap. 8).

The division between pragmatic and comprehensive styles, Harwood further argues, also helps us to understand the contrast between genetics research in Germany and genetics research in the United States during the interwar years. Although the communities of geneticists were not homogeneous, he believes that, overall, they conceptualized genetics in profoundly different ways. He claims that after 1915, geneticists in the United States had no interest in embryology, development, and evolution; instead, they focused mainly on transmission genetics. In Germany, however, transmission genetics was not as clearly separated from studies on evolution and embryology. Part of the explanation lies in the differences between university systems. While rapid growth in the American system allowed geneticists to organize independently of other biological fields, the more structured, less flexible, and much poorer German system rendered such a move practically impossible. But, Harwood adds, there is a more important explanatory factor, namely, the unequal representation in those countries of the two scientific styles that he analyzes. While most German researchers adopted a comprehensive style, in America the situation was the reverse; most American geneticists adopted the pragmatic approach (Harwood 1993, chap. 4).

Given the novelty of his approach and his thorough analysis of the German genetics community, Harwood's historical work is extremely important. Most impressive, he makes a convincing case for considering the peculiarities of that community in relation to the wider social, cultural, and political circumstances. These are hardly minor accom-

plishments. But Harwood wants to go further. He aims to contribute "not only to the history of genetics, but to the historiography of science more generally. I hope to persuade others that 'style of thought' is a useful analytical (rather than merely descriptive) concept in the history of science" (Harwood 1993, p. xvii). So, let us look more carefully at his notion of style.

Harwood presents his conception of scientific styles as a modification of Mannheim's notion of thought style. Styles of scientific thought exist, Harwood claims, "when particular ontological and/or epistemological assumptions recur in a variety of scientific domains *and* those assumptions differ from one group to the next" (Harwood 1993, p. 10). Thus, he would call the particular characteristics of an individual or a group in science a style, no matter how idiosyncratic they might be. To constitute a style, a set of characteristics must be found in a number of scientific fields. Furthermore, as patterned differences in the beliefs, interests, and actions of given groups, Harwood's styles of thought cannot be discerned by looking at science alone. One has to look at different realms, such as art, science, and politics, to see whether the differences in one realm correlate with differences in others. Appropriately, Harwood refers not only to scientific styles but to styles of thought. For him, a scientific style of thought is always part of a more general style of thought characteristic of a specific social group.

But the presence of idiosyncratic characteristics in a group's thoughts and actions is not sufficient to attribute to it a specific style. To "identify a style of thought is not merely to catalog a group's attitudes on various issues; one must also demonstrate some *coherence* among those attitudes. A style of thought is not simply an aggregate; it is structured" (Harwood 1993, p. 269). Harwood is not talking simply about logical coherence. Instead, coherence involves what Mannheim referred to as the "basic intention" of a style of thought. Thus, "one has to analyze those patterns in action, looking at how they are used to advance the carrier group's aims" (Harwood 1993, p. 272). In his own study, Harwood considers why the comprehensive and the pragmatic scientists in Germany advocated different conceptions of science and favored different social roles for academicians. The answer lies, he argues, in their different responses to modernization and their different perceptions of their own roles in the new social order.

Maienschein's Epistemic Styles

In *Transforming Traditions in American Biology, 1880–1915,* Jane Maienschein argues that during the period specified, the morphological tradition in biology was significantly transformed. She provides an excel-

lent analysis of the early careers of four biologists: E. B. Wilson (1856–1939), E. G. Conklin (1863–1952), T. H. Morgan (1866–1945), and R. G. Harrison (1870–1959). All of them were trained at Johns Hopkins under the physiologist H. Newall Martin and the morphologist William Keith Brooks. They also shared similar training and experiences at the Naples Zoological Station. Influenced by the German morphological tradition, these four gradually developed a distinctively new way of doing biology, which by 1915 had given rise to four separate research programs. Wilson focused on the cell nucleus and on cell lineage studies, Conklin concentrated on cell lineage work to address evolutionary and developmental problems, Morgan worked on problems of regeneration and experimental embryology before moving to genetics, and Harrison studied the development of the nervous system. According to Maienschein, their experimental approach led to a new way of conceptualizing biological research. Specifically, there was a shift in focus from development to heredity and from external factors to factors internal to the organism. Most important, this transformation involved epistemological changes regarding the goals, methods, and criteria of evaluation used by scientists. As Maienschein sees it, the changes had to do with the questions scientists asked, the criteria they considered acceptable, and the type of evidence they looked for. In short, she claims that it was a transformation in how science was done, not a change in particular theories or metaphysical assumptions (Maienschein 1991*a*, pp. 3–9).

Focusing on the period from 1880 to 1915, right before the period Harwood studies (1914–30), Maienschein also points to differences between American and German science. In her opinion, American biology started to develop an idiosyncratic character at the turn of the century. Rejecting the speculative theories and descriptive studies of the Germans, Americans aimed to obtain concrete and definitive results. They became interested primarily in facts and in problems that could be solved with empirical evidence. This new orientation had a profound impact on the field of morphology, as it rejected a decidedly descriptive and global approach in favor of a more experimental, interventionist, and focused one. In a sense, Maienschein supports the view that American biologists were more pragmatic than their German counterparts. Americans focused on narrower questions that promised practical payoffs, preferred limited rather than grandiose and abstract theories, and embraced experimental approaches. Maienschein even proposes that American biologists embarked on a "search for proximate causes and moved away from historical, evolutionary explanations" (Maienschein 1991*a*, p. 137). It seems, then, that by 1915 Ameri-

can biologists were already leaving behind the study of evolutionary, embryological, and developmental problems. According to Harwood, those problems were also of no interest to the American genetics community between the years 1915 and 1930.

Maienschein does not use the notion of styles in this book. Instead, she puts forth the idea of scientific traditions as particularly useful for understanding the process of scientific change. Traditions exist when scientists share a set of basic tenets about how to do science. A single tradition may comprise different disciplines, fields, and schools over a long period of time. What holds a tradition together is not a set of common beliefs or theories but shared epistemological and ontological assumptions that specify worthy goals and adequate methods for reaching them (Maienschein 1991*a*, pp. 75–80).

In other writings Maienschein has developed the notion of "epistemic styles," defined as ways of doing research and knowing about the world. Specifically, she says that "a biological style is characterized by a shared set of problems regarded as appropriate, techniques regarded as useful, and approaches regarded as productive" (Maienschein 1988, p. 173). These styles are epistemic because they comprise ways of reaching knowledge, criteria for deciding what counts as knowing, and what should be considered objects of knowledge (Maienschein 1991*b*, pp. 410, 410–13, 423–26). They concern the questions scientists ask, the problems they study, the techniques they employ, the approaches and methodologies they adopt, the organisms they choose, and the criteria they use to evaluate evidence. In Maienschein's view, traditions, unlike epistemic styles, are unique and historically bound. They cannot exist in different times or places. Styles, in contrast, can become independent from the historical context in which they originally arose. Therefore, they can exist in different places and at different times.

Maienschein has used the concept of epistemic style in several contexts. In her analysis of Charles Otis Whitman's work at Chicago, for example, she claims that there existed a "Chicago style" of biology around the turn of the century. That style was characterized by a commitment to studying the organization of whole organisms and populations and engaging in cooperative and comparative inquiries (Maienschein 1988, p. 173).[4] More interesting for us here, in a 1991

4. A special issue of *Perspectives on Science* (Mitman, Maienschein, and Clarke 1993) explored the question of whether there is a Chicago style of science. The issue contains an introduction by Gregg Mitman, Jane Maienschein, and Adele E. Clarke and articles by Adele E. Clarke, Bonnie Ellen Blustein, Sharon E. Kingsland, Ronald Rainger, and Eugene Cittadino.

essay she argues that, at the turn of the century, German and American embryological researchers had distinct epistemic styles. She analyzes the German researchers Wilhem Roux, August Weismann, and Oscar Hertwig and compares them to Wilson and Morgan, their American counterparts. Whereas the Germans "sought causal mechanical explanations of as many phenomena as possible, guided by strong theories which achieved confirmation when they fit with as much of the available data as possible," the Americans "sought definitive facts, as many as possible, which might be quite specific or narrowly based" (Maienschein 1991*b*, p. 407; see also pp. 416, 419, 424). In sum, whereas German scientists sought causal explanations and studied a wide range of related issues, American researchers focused on specific facts and empirical results and thus tended to be more specialized. These two epistemic styles "emphasized different goals, processes of investigation, and standards of evidence" (Maienschein 1991*b*, p. 407).

Both Maienschein and Harwood claim that in the case of genetics and embryology, sufficiently distinctive characteristics in the German and American communities indicate that there were different ways of doing science and, more important, different ways of conceptualizing what scientific research was all about. While suggestive and thought provoking, these generalizations need to be explored further. We need additional studies before we can map out in detail the research from various fields in both countries. Regarding the history of genetics, I am skeptical that further research will confirm that the differences between the German and American communities were distinct enough to qualify as two disparate styles of thought. Harwood argues that American geneticists were not interested in evolutionary problems, yet he also mentions that some researchers—William E. Castle, Edmund Sinnott, Sewall Wright, and L. C. Dunn—were very interested in evolution and development (Harwood 1993, p. 407). He adds, though, that these were exceptions to the rule. My own work on Castle, Edward M. East, and their students at the Bussey Institution of Harvard University clearly indicates that many American biologists working on genetics were concerned with evolutionary problems. In fact, some of them studied genetics precisely because of its relation to evolution. Furthermore, the work of many researchers in agricultural stations focused on genetics and evolution (Vicedo and Kimmelman 1993; Vicedo, n.d.). Perhaps Harwood was misled by the historiography of American genetics, which has focused almost exclusively on the *Drosophila* group directed by Morgan.

Maienschein needs to clarify the differences between German and American embryology. How unique was the American experience?

How deep was the epistemological gap between American and German science? Did Americans really have a way of defining science that was incongruous with the scientific ways of German researchers? Or did the Americans simply have different short-term interests? After all, researchers in neither country claimed that their counterparts in the other were not doing science or had not reached knowledge. They merely seem to have had heterogeneous interests. In sum, how distinct and incommensurable are the styles identified by Harwood and Maienschein?

Establishing generalizations about the research done in different scientific communities during a specific period of time also raises a methodological problem. Different historiographical approaches lead scholars to emphasize different aspects of the period under study. Consider the following analogy: When we photograph a landscape, the patterns visible in the picture will vary depending on the lens we use. A similar situation arises when we compare individuals, groups, or national scientific communities. For example, when I compare Castle with East, I perceive many differences between their approaches to genetic problems. These differences are due mainly to their training, their use of different experimental organisms, and their different views on the value of biological knowledge for social problems. Castle was trained at Harvard under Charles Davenport, worked on small mammals, and became increasingly skeptical of eugenics. East was trained at agricultural stations, worked on plants, and was a staunch supporter of eugenics and population control. But, when I compare the research of Castle and East at the Bussey to that of other groups, such as the *Drosophila* group at Columbia, I detect many idiosyncratic differences as well (Vicedo, n.d.). What, then, is the relevant level of analysis? Should I say that Castle and East had different styles? Or should I discuss the Bussey style and the Columbia style? While we can talk meaningfully about scientific styles, we need to make sure that we have identified genuine patterns in scientific thinking. Otherwise, the perception of distinctive features in a scientific community may be simply an artifact of our historical perspective.

Maienschein and Harwood argue that the concept of style is useful in historical analysis because it helps to illuminate what scientists do and what they take to be scientific knowledge. It also helps us to understand why science took certain paths rather than others. Yet, despite superficial resemblances, Maienschein's and Harwood's notions of style are very different. Maienschein focuses exclusively on epistemological stands while, for Harwood, epistemological differences alone do not qualify as distinctive scientific styles. Maienschein adopts a rel-

atively internalistic approach, focusing on how scientists work; Harwood selects a relatively externalistic approach, considering why scientists adopt a particular direction in science. To better appreciate the differences in their positions, let us now compare them to Hacking's conception of scientific styles.

Hacking's Styles of Reasoning

The philosopher Ian Hacking has written extensively about the concept of style (Hacking 1982, 1992*a*, 1992*b*). He received inspiration from Alistair C. Crombie's analysis of thought styles. In an impressive history of Western science, Crombie has argued that "the scientific movement from its Greek beginnings has been a programme of promoting a unified conception of nature and of science, diversified into its different styles of argument by its interactions with changing general beliefs about what exists and by the diversity of subject-matters" (Crombie 1994, p. 1764). Thus, novelties in the history of science were introduced by different styles that developed through time in different areas. The scientific styles of thinking of any given period were influenced by a variety of intellectual or moral commitments or dispositions. Specifically, each thought style involves three types of commitments: conceptions of nature, conceptions of science and the organization of scientific inquiry, and intellectual and moral positions that foster particular attitudes toward issues such as innovation and change. Shared commitments in these three areas shape the path of science by defining what counts as science: "The commitments of a period or group or individual . . . have regulated the problems seen, the questions put to nature, and the acceptability of both questions and answers" (Crombie 1988, p. 4). Crombie's styles are different approaches for obtaining knowledge: the method of postulation, experimental argument, hypothetical modeling, the taxonomical method, statistical and probabilistic analysis, and the genetic method or historical derivation.

Hacking has developed Crombie's ideas in new directions. In an attempt to "historicize Kant," Hacking aims to extend "Kant's project of explaining how objectivity is possible" (Hacking 1992*a*, p. 4). In particular, he tries to find a middle ground between relativism and abstract metaphysical accounts of truth and objectivity. He develops the notion of "styles of reasoning," rather than styles of thinking, for two reasons: first, because science is not only about thinking but about doing, constructing, and experimenting as well; and second, because scientific practice involves a public process of legitimating a given way of reaching knowledge. Hacking concentrates on some major styles of

reasoning: the laboratory style, the statistical style, the mathematical style, the taxonomic style, and the historico-genetic style (Hacking 1992*b*).

Styles, Hacking says, serve as models for thinking and reasoning about a particular type of subject matter. Furthermore, each style introduces novelties, including new types of objects, evidence, sentences, laws, and possibilities (Hacking 1992*a*, p. 23). The establishment of each style therefore typically provokes an ontological debate about a new type of scientific object. Styles of reasoning, then, do not merely describe different ways of doing science: they literally configure different ways of knowing by defining the standards of objectivity and truth. As Hacking puts it, styles of reasoning determine "what it is to be objective (truths of certain sorts are just what we obtain by conducting certain sorts of investigations, answering to certain standards)" (Hacking 1992*a*, p. 4). These styles are not context dependent and are not tied to a particular group or place. Although they arise as historical entities, they become autonomous. Eventually, each style has become "what we think of as a rather timeless canon of objectivity" (Hacking 1992*a*, p. 10).

Both Maienschein and Hacking go beyond the analysis of the products of science—theories—to study the practices of science, the ways in which scientific knowledge is developed. However, while Maienschein focuses exclusively on epistemological factors and identifies trademarks characteristic of particular research groups or fields of work, Crombie and Hacking are interested in much wider units of analysis (e.g., laboratory style). Their thought styles do not refer to the body of work and methods of any particular group of scientists. They do not identify the trademark of any particular individual, group, or research program. Moreover, a style can be used simultaneously with another style (for example, a research group could use both the laboratory and statistical styles), which is not possible in the case of Maienschein's epistemic styles. Also, Crombie and Hacking might find it interesting at some level if two groups use different reasoning styles—for example, one group does laboratory work and the other statistical work—but it is not the individuating level in their analyses. For Maienschein, though, the use of different epistemic styles by diverse groups calls for historical analysis. Hacking's styles, which become timeless and objective ways of reasoning, are also very different from Harwood's styles, which are always part of a specific culture and rooted in particular sociopolitical conditions.

When I first tried to understand these various concepts of styles, I found them so different that I was tempted to conclude that any com-

parison would be meaningless. But then I began to wonder why these authors had such different views. I soon realized that Maienschein, Hacking, and Harwood have very different goals and interests, very different "styles." In fact, their notions of styles reflect the different concerns in the fields of history, philosophy, and sociology of science. To see this more clearly, the next section looks at what these scholars have to say about two specific issues: what styles do and how styles arise.

Styles: What They Do and Where They Come From

Hacking is mostly interested in the metaphysical, ontological, and epistemological implications of styles, not in their origins. As a philosopher, he is particularly concerned about the construction of objectivity and knowledge. This, in fact, is precisely what his styles of reasoning do. These styles, he argues, "become not the uncoverers of objective truth but rather the standards of objectivity" (Hacking 1992*a*, p. 19). Styles not only constrain thought; they also determine what knowledge is. Thus, truth and objectivity do not exist independently of styles of reasoning. Hacking's project, then, involves unraveling the ways in which each style of reasoning defines objectivity. This is part of the larger philosophical project of rejecting relativism and showing how objective knowledge is possible.

Hacking's definition of styles of reasoning, however, leaves open the issue of how we can compare knowledge claims offered by different styles. Imagine that there is a conflict between what the statistical and the laboratory styles say about a given subject. If each style has its own criteria for determining truth, it would seem impossible to decide which claim is more accurate or more likely to be correct. How should the scientific community decide between the conflicting claims of different styles? Would those claims be assessed only on a pragmatic basis, by choosing the most useful position? We need to know how to evaluate what counts as truth and objectivity not only within each style but also among different styles.

Hacking suggests that styles of reasoning can serve as a common area of inquiry for the history, sociology, and philosophy of science. He clearly recognizes that styles of reasoning have histories in which social factors have played central roles. Consider the following passage about the statistical style: "As this style of thought evolved, every social dimension is on show. If you want interests, we have interests. If you want rhetorical devices, we have those. And institutions, modes of legitimation, takeover battles, constructions, uses of power, networks, intimations of control, and much, much more" (Hacking 1992*b*, p. 133).

Nevertheless, he argues that the study of origins is irrelevant to the philosopher's task. As he puts it, a style of reasoning "starts by being pushed and shaped by social vectors of every sort," but "we end with a self-sustaining mode of knowledge" (Hacking 1992*b*, p. 132). On the role of social factors, he asserts that "as the style becomes increasingly secure, these are decreasingly relevant to its status. The style ends as an *autonomous* way of being *objective* about a wide class of facts, armed with its own authority, and available as a *neutral tool* for any project or ideology that seeks to deploy it" (Hacking 1992*b*, p. 133; emphasis added).

Hacking has introduced through the back door the contested distinction between the context of discovery and the context of justification. I do not mean that he believes the process of reaching knowledge is irrelevant in assessing its status. In fact, his position suggests that they are one and the same thing. By conducting an investigation according to the standards of a given style, one obtains knowledge as defined by that style. What I mean, instead, is that Hacking invokes the division between discovery and justification in the study of the styles themselves. For Hacking, the conditions under which a style develops shape its criteria for assessing knowledge claims, but at some point a style becomes stable and acquires a substantial measure of autonomy. Once it is established, its criteria of assessment are considered neutral and objective. So social and historical factors are important only while a style is developing. Thus, despite Hacking's suggestion that the study of styles should foster a meeting of philosophical, sociological, and historical minds, the old division of labor among historians, sociologists, and philosophers is maintained and legitimated. Historians and sociologists study the processes that give rise to new styles of reasoning, and philosophers focus on the metaphysical, epistemological, and ontological issues raised by each style.

But this scheme is rather problematic. When does a style become neutral? Why do social factors no longer impinge on it? Take, for example, the laboratory style. Was there really a point at which its procedures became neutral and objective? The development of a new style of reasoning provokes controversy about its validity. Perhaps that style is later considered a reliable way of acquiring knowledge and thus has a measure of stability and respect; but this surely does not mean that social factors no longer influence it. Furthermore, scientific styles are not static entities. The laboratory style is continually in flux, as new experimental methods constantly modify it. And social factors shape the process of negotiation through which new modes of the laboratory

style gain legitimacy. For these reasons, the idea that styles can break free from social influences is problematic. It therefore seems arbitrary to divide the tasks among philosophers, historians, and sociologists, as Hacking would.

Maienschein focuses mainly on the historicity of scientific knowledge, namely, the way in which specific conceptions of science as well as the practice of science have changed over time. As a historian, she is interested in how specific epistemological values have influenced particular research programs at certain times. However, she also argues, like Hacking, that styles can transcend their own historicity and national imprint. They can acquire independence from their historical context and can exist in different places and at different times. As I mentioned previously, for Maienschein this characteristic of styles differentiates them from traditions, which are historically bound.

Like Hacking, Maienschein thinks that styles influence scientific thought. But Maienschein's styles differ from Hacking's in important ways, mainly because hers do not require new ontologies. Her epistemic styles refer only to different ways of studying the world. These clusters of beliefs about how to do science influence the practice of scientific research. To identify styles, Maienschein says, one has to look closely at scientific practice, at what scientists do, instead of focusing exclusively on their theories and on what they say about their work. Different groups of researchers do science differently precisely because of their different epistemological styles. Maienschein does not think, though, that researchers are aware of this process: "I have found no evidence, however, that they self-consciously sought to effect a uniquely American style of work" (Maienschein 1991*b*, p. 423).

Epistemic styles constrain and shape the course of science. Different goals lead to the selection of different problems and promote the acceptance of different theories. Maienschein sometimes goes further by arguing that diverse epistemic styles use different criteria in determining what counts as science. We should be careful not to infer from this that different epistemic styles always foster different conceptions of science. A group with one epistemic style may perceive that another style simply leads to uninteresting problems and not necessarily to illegitimate science.

Although Maienschein discusses the origin of scientific styles, she does not develop a general position. Perhaps their appearance and persistence depends on local and proximate sociocultural factors. But she suspects this is not enough: "We can point to many examples of styles in the sciences and elsewhere that are too widespread to appeal only to local explanations. For a full account, then, we must look more

deeply and further into the past, probably at a mixture of social, economic, institutional, practical, and intellectual factors" (Maienschein 1991*b*, p. 425). Her primary interest, however, centers on the influence of clusters of epistemological convictions on scientific practice. This is a historical enterprise, involving the description and analysis of the many ways in which scientists have conceptualized their work and the examination of the methods and criteria used to produce reliable scientific knowledge.

Both Maienschein, who focuses on how scientific styles influence scientific practice, and Hacking, who studies how objectivity is constructed by different styles of reasoning, are primarily interested in what styles do. It is significant that Harwood argues against viewing styles in this way. He warns that we must not reify styles because, according to him, a style is only a heuristic device, a useful category in historical analysis. Styles of thought, he adds, are cognitive patterns that we can discern in history, but they do not have independent status. Moreover, they do not act as cognitive constraints. Thus, we cannot study what they do because they do not *do* anything. Nevertheless, the concept of styles is useful because it indicates that "thought is patterned, that thought is not simply a hodgepodge of unrelated attitudes" (Harwood 1993, p. 15). In Harwood's view, the genesis of a style, not its function, requires explanation. Thus, the historian and the sociologist should describe styles and explain how they emerge rather than analyze their functions.

Harwood especially wants to show that the origins of styles are sociological, that particular styles are fostered by specific sociological settings. He seeks to provide a sociological account of problem choice and theory choice and thinks that the concept of style is useful in this endeavor. In an analysis of Fleck's views, Harwood argues that identifying styles simplifies the sociologist's task: "The concept of style thus enables us to reduce the complexity of the bodies of thought which we analyze, arriving at a handful of lowest-commondenominators. Once these have been inferred, sociological explanation of scientists' commitment is greatly simplified: it can concentrate upon these deeply embedded assumptions" (Harwood 1986, p. 183). Thus, according to Harwood, styles are a heuristic device useful for a very specific task: the construction of a sociological account of the scientific enterprise.

The idea that styles of thought cannot have causal power is worth exploring. Styles are a level above scientific practice: they can only be seen by comparing the work of several scientists or groups. Moreover, they become apparent only when the historian looks at broad patterns. Nevertheless, the reasons offered by Harwood for denying causal

power to styles are not completely convincing. First, it is puzzling that Harwood denies power to styles of thought yet, at the same time, argues that we should look for their "basic intention." How can he separate the basic intentions of styles from their function? He might say that intentions are things that people have. Then, the intention of the style is a shorthand for the intention of the group. Harwood argues that styles cannot constrain thought because "ideas of themselves do not coerce. . . . It is rather human beings who coerce, using ideas to justify that coercion" (Harwood 1993, p. 15). Perhaps Harwood rejects the view that styles prescribe or coerce because he wants to emphasize the plasticity of human thought. Coercion may be a rather strong term to use here, but nothing depends on its use. The important issue is whether adopting a given style can influence scientific practice, and how. It is also worth keeping in mind that constraints do not have an entirely negative role. Elsewhere, I have talked about "enabling constraints" (Vicedo 1992). The adoption of a given framework, assumption, or commitment may eliminate some choices, but it can also help reveal unforeseen possibilities.

The crucial issue is whether ideas have causal power. If a group has certain intentions and goals, and if its members believe that certain methods, assumptions, and commitments will help them to obtain their goals, then those ideas are having an effect in their decisions and actions. In fact, a group may select certain ideas precisely because of the way these ideas influence specific choices and lead in particular directions. In turn, these ideas may stimulate the group to rethink its intentions and goals. The relationships among social conditions, intentions, actions, and ideas is multileveled and has several feedback loops. It is not a one-way process, as Harwood seems to assume. In his book Harwood does not say why he wants to deny ideas any causal power, but elsewhere (Harwood 1986) he has opposed the views of Fleck and others who believe that styles influence the conduct of science.

In *Genesis and Development of a Scientific Fact,* Fleck presented the idea of thought style (*Denkstil*), a set of epistemological, methodological, and ontological assumptions shared by the members of what he called a thought collective (*Denkkollektiv*). These assumptions shape science by influencing the selection of problems and even the definition of what counts as an answer. For Fleck, the existence of thought styles is a function of social forces, the effect of a given social setting. But he also believed that styles of thought are causal entities that prescribe how science should be done. Harwood finds Fleck's reification of styles troublesome: "I suspect that, like some others, [Fleck] simply

did not notice this explanatory tension in his work. Despite the best of intentions, it is quite easy to slip into the habit of regarding thoughtstyle as a kind of intervening variable, *emerging* through the actions of the research community and, in turn, *feeding back* upon its inventors to channel their perception. But to portray cognition in this way is inconsistent: the former process is sociological but the latter remains intellectualist" (Harwood 1986, p. 183). The differences between Fleck and Harwood can be represented as follows: for Fleck, social influences lead to styles, which lead to beliefs, values, and actions; for Harwood, social influences lead to beliefs, values, and actions, and the patterns of those beliefs, values, and actions are styles. According to Harwood, the reification of styles introduces an obstacle to the development of a thoroughly sociological account of science. This, however, is not evident. In both Fleck's and Harwood's theories, the beliefs and actions of a given group depend ultimately on social factors. Even if styles are introduced as a mediating element between the social setting and the specific actions of the group, it is not clear that this is a problem for a sociology of science. Surely a sociological account does not require that ideas play no role, that ideas be merely effects of particular social settings.

Harwood presents other objections to the view that styles influence scientific thought. These are problematic as well. He claims that styles cannot shape scientific research because scientists are not aware of them. Scientists are only aware of their use of theories, concepts, and methods (Harwood 1986, p. 183). To be sure, if styles are sets of deeply embedded assumptions, scientists may not be aware of them. However, this does not prevent assumptions from having causal power. Our thoughts and actions are constrained by many things of which we are not aware. One of Voltaire's heroes was surprised to find out that all his life he had been speaking in prose. But his use of the language had been constrained by a particular literary form, even while he was unaware of it. Similarly, the thoughts and actions of scientists are influenced by their beliefs about the world and their goals in scientific research. Whether or not scientists recognize that their clusters of beliefs, goals, and characteristics configure a distinctive style is irrelevant. This point is not very different from Harwood's claim that even though scientists are often not aware of their social circumstances, their work is nevertheless influenced by them.

Some other issues in Harwood's framework need clarification. It is not clear whether he is talking about a causal or a parallel relationship between the sociocultural context and scientific practice. His work considers whether the peculiarities of scientific approaches are present

in—and may even be derived from—the wider culture (Harwood 1993, p. 14). The difference, however, is very important. The first approach gives us a correlation between social and scientific factors, the second a causal relation between them. In the latter, the geneticists' work is determined by their culture, and the differences between comprehensives and pragmatics are reduced to a dispute between different sociopolitical positions. But it could also be that as members of a specific social framework, scientists' thought shared common elements with other cultural expressions. Does the social structure determine the path of scientific development? Or is science a part of that social structure, which is in turn modified by science? Do social interests drive cognitive aims? Are patterns in the social order correlated with cognitive patterns? Does culture constrain science from the inside or from the outside, as an external force or as an internal component of science? Do we have external forces impinging on internal scientific processes? Or do we have parallel processes inside and outside science?

If Harwood is defending a causal relationship, we need to know how and to what extent social factors influence science. Consider his argument that styles illuminate theory choice. Regarding the problem of specifying the relationship between the nucleus and the cytoplasm, he argues that "theory-choice was channeled by geneticists' self-understanding as mandarins or as outsiders" (Harwood 1993, p. 316). But the idea of channeling is rather ambiguous. Harwood seems to believe there is a causal relationship between a sociocultural setting and the specific characteristics of scientific research. He seems to think that styles are produced by certain social conditions and that, under certain social conditions, particular styles will be dominant. Still, the question of whether styles are influenced, determined, or fostered by the social setting remains. What are the causal relationships among cognitive patterns, social order, and institutional settings? Do social positions influence, determine, or constrain? How? Note that even constraints do not determine, since usually there would be more than one constraint, and multiple constraints may pull in different directions.

Furthermore, if social factors play a role, however important, in the shaping of scientific knowledge, this does not imply that social factors are the only ones operating. This is why I find it puzzling that Harwood denies ideas any causal role. For social groups to have any aims or intentions at all, they have to endow their world with some intellectual coherence. As historian David Hollinger has said, "Social action necessarily takes place within a framework of meanings that serve to enable and to restrict what people do" (Hollinger 1982, p. 309). It is

clear that for some issues to be intellectually meaningful in a given framework, they also have to be socially meaningful. But for those actions to be socially meaningful, there has to be an intellectually coherent framework in which they can be interpreted. Thus, the patterns of interaction between social, cultural, and scientific factors show different directions and levels of influence. Our aim then should not be to connect forms of knowledge with specific social structures but to analyze how they interact in various ways.

Let us review the discussion in this section. As a historian, Maienschein is interested in the historical development of standards of discovery and demonstration. As a sociologist, Harwood aims to offer a sociological account of science. As a philosopher, Hacking focuses on the ways in which certain methods become accepted means of acquiring knowledge and determine frameworks of meaning. We have a historian talking about change, a sociologist discussing the social determination of scientific beliefs, and a philosopher analyzing objectivity. It is tempting to conclude that with such different interests, they are, in fact, talking at cross-purposes. Are philosophers, historians, and sociologists looking through such different glasses that they literally see different landscapes? There is, I propose, a more optimistic interpretation. In the next section I argue that the study of scientific styles leads philosophers, sociologists, and historians to emphasize particular features of the scientific enterprise. The analysis of styles reflects the common interests of those disciplines and points toward new venues of rapprochement.

In Search of Common Ground

Harwood, Maienschein, and Hacking have developed very different notions of scientific styles. As we have seen, their positions reflect the interests of their respective fields. Despite their differences, though, their focus on styles points to a common arena of discussion. After all, it seems intuitively plausible that different approaches to studying science would complement each other. Even though this is not always the case, the study of styles emphasizes certain ideas about the study of science that should be acceptable to all parties. In this section I want to briefly point to some of these.

The study of styles leads us to focus on the processes and practices of science. For a long time the philosophy and history of science focused mainly on the development and nature of scientific theories. More recently, a consensus has emerged regarding the value of analyzing not only the products of science but its practices as well. Many of us no longer focus only on what scientists say; we also try to understand

what they do and how they do it. Taken to an extreme, this approach has led some scholars to disregard scientific beliefs and theories, an approach that seems just as misguided as the older one. There is no clear way of separating what scientists do from how they do it. A more balanced focus requires us to pay attention to the dynamics of science, especially the relationship between its practices and its products as embedded in specific sociocultural settings. The study of styles can contribute to that task because it leads us to examine the linkages among goals, methods, products, and social visions and to analyze the relationships among the construction, demonstration, and acceptance of theories.

Justification in science is not between science and the world. The process of justification in science involves the relationship of objects, methods, and results within a given framework that is itself embedded in a social context. Among the different perspectives in the sociology, history, and philosophy of science, there would not be much consensus on questions about truth, objectivity, and justification in science. The existence of styles emphasizes the need to readdress these issues from an integrative perspective. Hacking has called for a radical reinterpretation of these issues. He argues that "the truth is what we find out in such and such a way. We recognize it as truth because of how we find it out" (Hacking 1992, p. 135). He goes even further by proposing that styles "have settled what it is to be objective (truths of certain sorts are just what we obtain by conducting certain sorts of investigations, answering to certain standards)" (Hacking 1992, p. 4). For Hacking, then, scientific styles are self-authenticating forms of knowledge, and a sentence is a candidate for being true or false only in the context of a given style. Whether or not we accept this redefinition of truth, the study of styles suggests that different scientific styles may configure different frameworks of meaning. If this is so, perhaps we should consider the extent to which the criteria of justification are tailored to each style. We can then talk only about contextual objectivity, or objectivity relative to a given framework of meaning. This would force us to rethink our views about reliable knowledge. In sum, both the question of realism and the problem of objectivity would undergo considerable reinterpretation if we accepted the notion of scientific styles.

Rationality is publicly constructed. Science is not made inside individual minds. Instead, reaching scientific consensus involves negotiation in and among groups. Thus, the study of science should not focus mainly on the contributions of isolated individuals. It must look at the bigger picture by exploring what is done in and among different scientific communities. We need to go beyond analyses of truth and

rationality as defined inside the scientific discourse to analyze the public construction of meaning, acceptance, and value. The concept of style orients us toward groups, institutions, and the scientific community with the individual being a less significant level of analysis.

The existence of different scientific styles implies that there is no unified scientific method. In science there are alternative ways of constructing rationality. The existence of many scientific styles suggests that we should abandon the idea of a single, unified scientific method. We should also recognize that there is no unique way of reasoning that is compelling. The authors discussed in this article all stress this point. As Maienschein expresses it, the existence of multiple styles means that "there is no one scientific method for all of science for all times and places, but neither is there any one way of doing science for any one given time" (Maienschein 1991, p. 425; see also Harwood 1993, p. xiv). There have been and continue to be alternative ways of searching for and acquiring knowledge. We need to study how they relate to each other and how distinct social and institutional settings foster their development.

Science does not proceed by a linear replacement of theories and methods. The history of science has not been a continuous, linear process of replacing discarded beliefs and methods. Alternative and even complementary ways of doing science (different styles) have coexisted. The idea of scientific styles leads us away from a unidirectional picture and toward more complex and correct theories. If there are several scientific styles, it follows that the image of scientific progress as a steady march of more successful methods and theories replacing older ones is flawed. Of course, many questions remain about how the practices of science change and how those changes shape scientific knowledge.

We need to construct a systematics of scientific methods. In biology, systematics analyzes the genealogical relationship between the species. Elsewhere, I have argued that to appreciate the temporal dimension of science and to obtain a better understanding of science as a process, we should develop a systematics of science. This project would focus on the different methods developed by the sciences and would analyze the genealogical relationships among different belief systems, methodologies, and evaluative strategies. We could eventually map the patterns—if there are any to be found—in the evolution of scientific methods (Vicedo 1992, p. 492).

The study of styles takes us in this direction. In analyzing styles we must look for patterns and make comparative analyses of the characteristics that define different groups. Crombie has called for a "com-

parative historical anthropology of thought" in order to establish a taxonomy of styles (Crombie 1988, p. 2). Fleck had called for a comparative epistemology whose task would be "to find out how conceptions and hazy ideas pass from one thought style to another" (Fleck 1979, p. 28). While there are some differences in these definitions, the common thread is the idea that we need to make comparative analyses and study the relationships among different scientific methods and approaches. We need to understand the relationships among the methods, ideas, goals, cultural values, and social visions of different groups and scientific styles in order to fully capture the complexity of the dynamics of science.

I have only sketched the main ideas that the study of styles forces us to consider. None of them are new. Although it is beyond the scope of this article to review the literature on them, it is clear that philosophical, sociological, and historical inquires have all contributed to their development. Work on scientific styles suggests a means of integrating the various approaches to studying science. Analyzing styles pushes us beyond isolated studies of institutions, individuals, theories, social factors, methodological factors, social visions, or philosophical issues; the interrelationships of those factors must be considered seriously. The study of styles demands an integration of different angles, allowing us to see different patterns, ones that were not visible when our scope was more limited. To understand scientific styles and, more generally, to understand the different ways in which scientists do science, we need to analyze their selection of experimental organisms, sources of funding, institutional settings, interests, epistemological beliefs, training, ontological assumptions, practical and metaphysical goals, political concerns, social views, and so on. We must look simultaneously at how, where, and why scientists do science. This is not a simple task, but it will certainly be fruitful. Whether styles can be a useful category for historical explanation is still an open question, but I believe it to be a fruitful one for raising the kind of historiographical and interpretative issues that we need to confront.

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