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# Introduction

## A Phenomenon and a Multiplicity of Concepts

Scientific knowledge was once perceived to be universal and hence unified. The practice of science was similarly regarded as unitary (at least ideally), which is another way of stating a second thesis, that of the univocality of the scientific method. Yet, even a superficial observation of different groups of scientists at work shows diversity among local and collectively shared ways of doing science. Decades of work in science studies have produced incontrovertible evidence establishing the need to acknowledge this diversity and to explore its significance. The question is, how?

Many efforts have been made to respond to this need, with Thomas S. Kuhn's (1962, 1970) concept of paradigm perhaps the best known. A key feature of the concept of paradigm is that it allows us to distinguish among approaches to what might appear to be the same range of problems by different groups of practitioners, often working in radically disparate time periods. Ludwik Fleck had developed kindred arguments in his publications on *Denkkollektiv* (thought collectives) (see Fleck 1935, 1979; see also Cohen and Schnelle 1986). And more recently, scholars in science studies focusing on a variety of sociological, historical, and philosophical projects have developed several other concepts to address such concerns. Alistair Crombie introduced the concept of styles of thought to distinguish among six modes of "scientific inquiry, argument and explanation," all within what he regarded as a "specific style of rationality created within European culture."[1] Ian Hacking embraces this anthropological project but speaks instead of styles of reasoning, or of styles of scientific thinking and doing. His concern is to emphasize the dimension of doing so crucial to ways of finding out but missing from Crombie's concept. The shift in language is also important to Hacking's philosophical project of tracking different ways of introducing new objects, new sentences about these objects, and new criteria both for determining what sentences are subject to the judgment of true or false and for adjudicating their truth or falsity (see Hacking 1982, 1992, 2012).

Both Crombie's and Hacking's concepts seek to describe ways of doing science as *longue durée* phenomena. Other concepts have emerged in the context of projects aimed at grasping similar but shorter-term phenomena. With the introduction of "epistemic cultures," sociologist Karin Knorr Cetina seeks to understand knowledge societies in terms that are not only economic (seeing knowledge as a productive force) but also cultural. In this regard, epistemic cultures (alternatively, cultures of knowledge settings or different machineries of knowing) are for her constitutive units of knowledge societies. Her approach to epistemic cultures is mainly through ethnographic studies (carried out, e.g., in laboratories) to identify the entities an epistemic culture brings into play and the relations between these entities such a culture establishes (Knorr Cetina 2005, 65, 67–68; see also Knorr Cetina 1999). By contrast, Evelyn Fox Keller reviews the history of twentieth-century approaches to the problem of embryonic development, focusing on the failure of communication among different collectives working on the same phenomena. She introduces the notion of epistemological culture to account for the reluctance, voiced by one group, to accept the types of explanation, or even the questions, put forward by another as meaningful (see Keller 2002). For her, epistemological factors (e.g., types of explanations sought, or modes of reasoning employed) appear as features of a scientific culture that are essential to take into account in characterizing collectively shared ways of doing science and in explaining failures of communication between groups.

Although the set of concepts mentioned above is far from exhaustive, it suffices to illustrate the variety of approaches that historians, philosophers, sociologists, and anthropologists of science have been advocating, all in the effort to further identify the characteristics that lend particular ways of making scientific knowledge their specificity and that distinguish among the diverse cultures of scientific practice.

How do these concepts relate to one another? To what extent are they redundant? To what extent incompatible? In what ways might they inform and enrich more concrete, empirically based, historical and philosophical studies? These are obvious theoretical questions to be addressed in attempting to sharpen the methodological tools needed to account for diversity in scientific practice. This book is partly devoted to such an inquiry. However, during this effort we have encountered other questions that also need to be addressed.

## Part I: Some Problems Attached to Concepts of Culture, and the Theses of the Book

Several concepts put forward in the effort to attend to the diversity in ways of doing science invoke the notion of culture—a notion that brings with it problems long debated in the humanities, although less so in science studies. We suggest that the time has come to fill this gap.

An overriding problem that has been much emphasized, especially in the anthropological literature, derives from the enormously difficult task of defining what one means by "culture" (see Geertz 2000, 11–13; Knorr Cetina 2005, 71). One of the tasks this book addresses is clarifying what appears important to the meaning of this term in the particular context of scientific practice.

Closely associated with this problem of definition are two risks that the recent interest in local cultures of scientific practice brings with it—risks to which the notion of culture is itself prone and to which our own intellectual histories surely make us especially sensitive: the temptation posed by concepts of culture to slight the dynamic and interactive character of the formation of any kind of cultural identity, and the pitfall of cultural essentialism (what we call culturalism), the view of cultures as essentially homogeneous, static, and fixed by prior constraints or race, gender, or nationality.[2]

In this introduction, we first query the necessity of a notion of culture for our project and then go on to examine these two risks in greater detail. This enables us to then present our own approach to the disunities of scientific practice, which we claim can avoid such risks.

In chapter 1, Donald MacKenzie begins with a reflection on the general difficulties of the concept of culture in the social sciences, and in science studies in particular. Would it be better, he asks, to restrict ourselves to such notions as clusters of practices? In relation to his own case study of securities and ratings in the last financial crisis, and more specifically of the practice of evaluation, he decides not, for doing so would deprive us of a crucial explanatory resource; it would also lose some key features of the situations under study.

More specifically, in MacKenzie's own case study, the concept of culture brings into focus certain key features that prove of general validity. First, it draws attention to the different ontologies associated with different practices (in this case, different assumptions about "what economic value consists of and [about] the nature of the economic processes that create it"). Second, culture captures the different processes of socialization associated with these ontologies, as well as the mechanisms for interaction among participants. Finally, the notion of culture focuses attention on how patterns of change in current practice depend on local histories of past practice. Indeed, these suggestions, along with others to which we return later, signal features of scientific cultures that recur throughout this book.

MacKenzie concludes that, in this case at least, culture remains a useful and even indispensable analytical concept, despite the problems it raises.[3] Accepting this conclusion, however, still leaves us with the task of addressing concerns about the risks associated with the notion of culture. But first, do these risks actually manifest in the history and philosophy of science? This question is addressed from a variety of perspectives in the remaining chapters of part I.

Kenji Ito's contribution in chapter 2 of this volume, focusing on histories of science in Japan, bears witness to the fact that such problems are quite real. He identifies (and deplores) essentialist tendencies in a number of prominent publications on the history of physics in Japan. These historical writings, he notes, give differences between physics in Japan and elsewhere pride of place and often account for them in culturalist terms (i.e., by reference to the actors' Japaneseness). Ito's primary concern with the latter view is its tacit view of scientific activities in Asia as exotic and as shaped by stereotypic national identities of "the East." In other words, his focus is on the particular form of essentialism described by Edward Said as orientalism (see Said 1978). Note, however, that in this case orientalism is as much a tacit assumption as it is a product of the history of science. Differences in scientific practices are interpreted as manifesting a Japanese culture and identity, while the interpretation in turn helps shape or reinforce the stereotype on which it is based. This orientalizing stance, Ito stresses, is not just common in historical discussions of science in Japan but is even encouraged by the expectations of the profession. Of particular importance here is his analysis of the forces at play in contemporary history of science that contribute to the promotion of essentialist historiography.[4]

Ito's main criticisms of such orientalist tendencies in the history of science are fourfold. First, they misrepresent the de facto diversity of science in Japan. Second, they overlook the transnational dimensions of current practices.[5] Third, in their preoccupation with difference, these historiographies obscure the extent to which the appearance of sameness (or uniformity) between twentieth-century physics in Japan and elsewhere is in fact a puzzle and not a natural or spontaneous outcome of the universality of science. Sameness in scientific practice or knowledge actually appears as an achievement deriving from actors' intentional effort to overcome differences. Indeed, the issue of shaping sameness, and the work through which actors achieve it, recurs throughout this book and is clearly a phenomenon in critical need of historical analysis.

Finally, Ito criticizes the empirical support provided by these historiographies. This point requires clarification. Indeed, if one can expose problems in the empirical basis that should support observers' essentialist conclusions with respect to scientific practice and knowledge in Japan, the tricky issue is that such essentialist tendencies at the same time echo many accounts by Japanese actors themselves. How are we to deal with this issue? Ito dubs this phenomenon self-orientalism and challenges us to deal with it, at the very least reminding us to exercise caution in distinguishing between actors' and observers' categories. The introduction of culture as an actors' category and actors' use of it are phenomena that observers must examine in a critical way and not absorb automatically into their analytic toolbox. Whether or not actors' conceptions and uses of culture are those relevant and useful for an observer's project must be treated as an open question.

Chapter 3 provides a good example of the need for such an approach. Guillaume Lachenal analyzes the circumstances and contexts in which actors might find it useful to claim authority for resources they perceive as indigenous. His case study illustrates the use of cultural essentialist categories by scientific actors in contexts extending far beyond scientific communities. His particular focus is on the controversy following the announcement in 2001 of the discovery of a vaccine against AIDS by a well-known Cameroon professor of medicine and former minister of health. Of relevance to us here is the fact that the vaccine was defined by local actors as Cameroonian and hence as opposed to the culture of transnational biomedical research. Lachenal also relates the case to other episodes in which actors claim an inherent "Africanity" for their cures in the expectation that such claims might enhance their value.

The risks attached to strategies of enhancing value of scientific research through appeals to native culture are manifest and would only be exacerbated were they to be embraced by anthropologists, sociologists, historians, and philosophers of science. In turn, the use of culturalist categories by observers can also provide support for their deployment by actors themselves, escalating the risks yet further. As Robert Kowalenko (2011) warns us, the story of AIDS in South Africa might well illustrate how disastrous the consequences can be.[6]

Like Ito, Lachenal insists that observers need to take actors' culturalist statements as objects of research and not embrace them uncritically, and he clearly illustrates the value of such research with his own analysis of how the combination of the organization of worldwide biomedical research and local politics influenced the development of these episodes of self-orientalism. Noteworthy for our argument is the fact that, in Ito's case, actors' culturalism relates to the resources available for the practice of science—the Japanese would have access to specific resources helpful in the advancement of a theory—while in Lachenal's case study, Africanity is also claimed for the results of the inquiry, that is, for the knowledge produced. The distinction between these two levels will prove of interest in what follows.[7]

### *Worries with a History*

Problems associated with cultural essentialism with respect to science have worried historians of science at least since World War II. Sinologist Joseph Needham, for example, repeatedly voiced concern that perceiving science in such ways ran the risk of denying what he called the "continuity" of both mankind and science (Chemla 2014). Relatedly, he also described the dangers he attached to the conception that mankind could be decomposed into distinct cultures, separate from one another, each developing its own science, valid only for the originating culture, specific to it, and incommensurate with the other sciences (see Chemla 2014 for references to specific quotations). By contrast, the historiographies of science and culture that Needham and historian Lucien Febvre developed from 1947 onward, in the context of the newly established UNESCO, all emphasized that mankind could not be meaningfully cut into pieces (see Petitjean and Domingues 2007). Interestingly, when in the 1970s Needham became explicit about risks specific to the history of science, he had in mind not only cultures of the type discussed by Ito and Lachenal in this book, which actors of that time were loudly reclaiming as specific, but also the ways of pinpointing diversity in collectively shared ways of doing science, which observers, like Kuhn with his notion of paradigms, were already beginning to advocate.

Needham's strategy in reaction to the dilemma resembles that advocated by Ito: he focused on "sameness." He did so in a specific way. He systematically read similarity between scientific results found in two distinct parts of the planet as proof of a circulation of knowledge from one to the other, and hence as proof that knowledge could be shared worldwide. However, this strategy has been criticized for methodological flaws, and rightly so. Statements of similarity overshadowed significant differences, and the hypothesis of independent occurrence was almost systematically ruled out. Moreover, Needham's strategy actively denies the manifest diversity in cultures of scientific practice that we want to address. Is there no way of accounting for such diversity without running into the problems associated with cultural essentialism that both Ito's and Lachenal's chapters demonstrate?

The question is not confined to science studies. Evelyn Fox Keller in chapter 4 examines what historians and philosophers of science might learn from decades of debate in feminist theory about similar issues. Recognition of the force of gender categories in organizing our conceptual and social landscapes marked a critical milestone in the emergence of feminist theory, but discussion quickly became enmired in worries about the lure of cultural essentialism. The question for feminist scholars—how can one recognize the force of gender categories while avoiding the pitfalls of gender essentialism?—bears an obvious parallel to the one we are concerned with here. At the same time, the history of debates in feminist studies also reveals another pitfall: an anxiety over essentialism so great as to threaten the very effort to understand the significance of cultural differences. Exactly the same questions arise here. How do we avoid throwing out the baby with the bath water? How can one write about scientific cultures (or gender) as both recognizable and consequential, without inviting the assumption that such cultures (or categories) are fixed and closed to external influence?

To summarize the points made so far, we can readily recognize the importance of highlighting differences between cultures of scientific practice in the history and philosophy of science, both in efforts to combat tacit assumptions of uniformity/spontaneous universality (and hence the hegemony implied by such assumptions) and in the effort to do justice to the actual life of the sciences. But doing so is fraught with dangers. Up to now we have focused on the dangers of culturalism, both in science studies and in feminist theory. These risks relate to the shaping of collectives as separate using either the way they practice science (against the assumption of uniformity) or the kind of knowledge they produce (against the assumption of universality of science). But in this latter respect, Keller emphasizes, highlighting such differences also creates other problems, eliciting other dangers. In particular, it raises the old but still critical problem of relativism—the other problem Needham attempted to avoid by insisting on the universality of science.

Can the conclusions of different scientific cultures be evaluated in relation to each other? Or does the recognition of differences among them imply that they can be judged only from within? Must we assume (as was once common) that these conclusions have equal relevance to the world (or worlds) to which they ostensibly refer? These questions are consubstantial with the very project of considering differences among cultures. Here again, it proves vital for the analysis to distinguish between actors' claims, with respect to the specificity of their knowledge, and observers' assessments. Actors' claims are important objects of research, much in the way Lachenal advocates in his chapter. Keller reminds us, as observers, of pitfalls of ignoring how much of the natural world we inhabit is in fact shared. She also insists on the questions compelled by the recognition of diversity among cultures of scientific practice about how these different cultures connect to one another and how a wider and more critical consensus can be—and often is—reached. She argues that, especially in the light of the global problems that now loom, these questions too are part of the challenge we must face.

### *Our Approach to Cultures of Scientific Practice*

We can now lay out the agenda of this book more clearly. For us, a key outcome of part I is that it sheds light on critical aspects in notions of culture that are at least sometimes invoked by actors themselves. In these conceptions, cultures are all-encompassing. They are general contexts in which scientific activity takes place. They leave their imprint on the practices and bodies of knowledge achieved, granting them their value in the eyes of the collective sharing the culture in question. Furthermore, these cultures are taken to be impervious to change. Most important, the scientific activity of the actors has no impact on their culture—*that* remains unaffected by external influences. These features of a notion of culture, we claim, are precisely those that allow the emergence of culturalist agenda and give rise to the risks identified. But do they accord with what we observe in scientific practice? We don't believe so.

If, following MacKenzie's analysis, we acknowledge the need for science studies to put into play some concept of culture, this book aims at developing another approach. Its core idea is introduced in MacKenzie's chapter and explored from various viewpoints in the remaining parts of the book. Above all, MacKenzie (along with many of the other contributors) insists that a culture of scientific practice is a product of what actors do—it is an outcome rather than a cause of their activity. In other words, the book places emphasis on the fact that actors shape the immediate context in which they carry out their scientific activity. The term "context" can refer to many different notions, but it is especially its use in relation to the specifics of scientific practice on which this book concentrates.

Furthermore, as the outcome of activity, a culture of scientific practice is subject to constant change—in relation to the problems actors address and the goals they pursue and in the ways they draw on the resources available to them to mold and remold their objects of research, their values, and so forth. Finally, we suggest that establishing bridges between cultures of scientific practice is also part of what actors do. Overcoming differences in knowledge and practice, constructing sameness (or even universality), and achieving consensus are not properties of scientific practice and knowledge that are given a priori but are outcomes of actors' knowledge activities. However, this part of scientific work—whether considered synchronically or diachronically—has as yet not been systematically examined as a general phenomenon in the history and philosophy of science (we return to this issue later).[8]

We will proceed in three main steps. Part II of the book examines specific components of these cultures, whereas part III takes a more global view. Finally, part IV addresses the historiographic implications of our suggestions for investigating cultures of scientific practice while avoiding the pitfalls of culturalism.

## Part II: A Toolbox for Investigating Cultures of Scientific Practice

The essays in part II engage the issues of cultural diversity by adopting an analytic approach, discussing constituent elements and features of cultures that appear essential to characterize a given way of carrying out scientific activities.

The thesis of culture as something that actors do comes out forcefully from Nancy J. Nersessian's case studies in chapter 5, where she examines the constitution of new cultures arising at the interface of biology and engineering. By focusing especially on the material models that are deployed in different laboratories, she demonstrates how these devices not only constitute an organizing center for the laboratory's culture but also come to represent its signature. Here, actors' shaping of the laboratory cultures in each of the cases in question is made manifest by the central role played by a material device, collectively designed and transformed throughout the research process.

Nersessian investigates the processes by which these models both reflect and help shape the laboratory's material, conceptual, and social practices. First, they allow people in the lab to connect their work to one another, serving as hubs through which the collective remains connected. These devices are thus central to the mechanisms of interaction among participants. Second, the devices provide the means through which cognitive operations are carried out. Cognitive operations in the laboratory are hence intimately tied to the laboratory's culture, as indeed are the results they yield. Nersessian thereby introduces a new general phenomenon, echoed in other chapters as well: the knowledge produced in the context of a scientific culture presents specificities that can be correlated with features of the culture itself.

The aspects so far evoked could have been captured through looking at the laboratory cultures as epistemic cultures. However, a third key dimension is precisely what Keller's epistemological cultures bring into focus: these devices, Nersessian argues, come to embody the epistemological values and hypotheses of the lab. Indeed, the study shows that, far from being static, the devices and hence the cultures they help constitute are shaped and reshaped by the actors in the laboratory according both to past results and to the (evolving) dictates of particular experimental agendas. Also, illustrating MacKenzie's comments about the historicity of epistemological cultures, they embody the cognitive history of the lab—a history that is recorded by the actors both as a potential guide to future developments and as a conscious reminder of the evolution of the laboratory's culture. The correlation between the scientific culture, on the one hand, and the questions addressed and the goals pursued in the laboratory, on the other, is here not only established but also clarified.

The cases Nersessian studies also illustrate how specific cultures are shaped as separate from other cultures in which similar problems are addressed. At the same time, these cases highlight the openness and ongoing interactivity of the cultures with respect to their environments—in this context, specifically the other cultures with which they are in communication. The devices are thus hybrid—borrowing from biomedicine and engineering while at the same time feeding back into the cultures from which they derive; the values they embody are equally hybrid and dynamic, collectively shaped through the research process.

In chapter 6 Mary S. Morgan focuses on the ways in which a scientific culture shapes an object of study while also fashioning new resources for research. With her case study of the emergence of an epistemic object out of a wider cultural transformation, she illustrates yet another form of openness of a local scientific culture. Her concern is with the "glass ceiling," a phenomenon that became an object of study for social scientists—indeed, that became visible to the public at large—only in late twentieth-century America, its visibility arising in direct response to the rise of second-wave feminism. Also, and of particular interest to us here, is the extent to which the scientific study of the glass ceiling made use of qualitative personal experience—a mode of analysis that had previously been absent from conventional social science studies but that was now demanded by feminist critiques. Here too, the dynamic and open nature of the way of working and its shaping by researchers are manifest, in this case affecting the type of the data used as resources. The inclusion of these data may have been a response to external pressure, but it clearly led to a radical shift in the internal epistemological culture of those studying the glass ceiling.

Morgan draws from this historical example some penetrating observations about potential differences between scientific cultures in the social and natural sciences, arising not only from the intimate relation between the collectives of scientist-observers and the larger community in which they live but also from the relations among communities of observers, of observed, and of generators of questions and concerns. As her case study clearly illustrates, cultures of social-scientific practice are de facto both open to and dependent upon the environments in which they are embedded. Indeed, it is in good part the larger communities that "raise questions and prompt what is found problematic and thus what is studied." They also influence how it is to be studied. Thus, in addition to providing the context for scientific research, these wider communities may be far more involved in defining matters of content and ontology than they are in the study of the natural sciences.

Chapters 5 and 6 demonstrate the centrality of communication among various types of contiguous collectives to a culture of scientific practice. We see actors borrowing elements that they then recycle to shape tools, topics of research, and even data. Forms of communication forged and adopted to connect laboratories and their environments thus emerge as essential features of the constitution and transformations of (at least some) scientific cultures, and it is precisely this topic that Claude Rosental addresses in chapter 7.

Rosental's case study deals with a very specific form of such communication, one that has become omnipresent in certain present-day scientific and technological practices: the public demonstration (demo). The simple fact that forms of communication vary illustrates their historicity and, accordingly, the malleability of cultures that adopt and adapt to new modes of communicating. In this specific case, demos depend on new techniques of communication, and they emerged in parallel with new modes of funding and management. By concentrating on them, Rosental in fact also suggests the fruitfulness of a cultural approach to funding schemes and management.

The focal point of Rosental's analysis is the use of demos in facilitating communication among managers, policy makers, and the public, on the one hand, and scientific workers on the other, and the impact of this form of communication on the knowledge culture itself. In the fields studied by Rosental, the need to produce such demonstrations for managers of large-scale programs shaped the organization of scientists' work in rather specific ways (a feature brought into focus by Knorr Cetina's notion of epistemic cultures). Further, Rosental argues that such modes of communication have a recognizable impact on the content of the work produced (issues that Keller's notion of epistemological cultures asks us to think about). Finally, because demos can be addressed to and visualized by a wider audience—that is, specific features and modalities of communication—this wider audience's reactions can also contribute to defining the goals that practitioners set themselves.

Such demos have become an essential part of the scientific practices that use them. By virtue of their role in mediating communication between two different cultures (i.e., between managers of science programs and groups engaged in specific projects), the demos are inevitably shaped by the culture of the managers to whom they are directed. With the example of modes of communication, Rosental demonstrates once again the dynamic character of cultural formation as bodies of knowledge and forms of practice evolving in response to interactions among different communities.

Thus far, all of the chapters in part II concentrate on specific scientific cultures, and through a close study of some of their constituent elements and forms of communication they underscore the dynamic and interactive nature of the constitution of these cultures. By focusing on yet another cultural element, David Rabouin in chapter 8 also draws our attention to patterns of cross-fertilization between different cultural formations in the context of a single discipline. Especially noteworthy are the patterns of intercultural circulation prominent in his discussion of style in the context of mathematics. In contrast to other notions of style, notably Hacking's "style of reasoning," Rabouin suggests that a style in mathematics can best be characterized as a way of writing. In the sense that he elaborates, a way of writing is an essential component of the scientific culture to which a mathematician belongs.[9]

For this notion of style, Rabouin draws inspiration from a paper written by the mathematician Claude Chevalley in 1935. In effect, Rabouin, as observer, adopts an actor's category in his analytic toolbox. The historical context in which Chevalley introduced the concept of style, Rabouin argues, might shed light on its usefulness for us today, at the same time as it indicates the variety of views actors formed about this concept at that time. Just shortly before Chevalley's paper, the notion of style had been invoked with another meaning by the German mathematician Ludwig Bieberbach to denigrate Jewish mathematics and praise the German or Aryan style of doing mathematics. The risks attached to these views were unfortunately realized quite dramatically, and historians like Needham never lost sight of them. Rabouin suggests that Chevalley's 1935 article, putting forth a different notion of style that emphasized circulation not just between France and Germany but among mathematicians of all origins, was nothing less than naive, and not unrelated to his historical context.

In any case, Rabouin highlights several interesting features that a notion of style conceived along these lines possesses. First, it helps account for the disunities in a mathematical culture of actors who otherwise share the same practices. In particular, Rabouin suggests that actors can share a way of writing but can differ in their interpretations of this writing. In short, they need not have a completely uniform way of working for their culture to be fruitful and dynamic. This remark points to a general and crucial issue of intracultural variation that awaits systematic research. Second, in contrast to Hacking's style of reasoning, in which all of mathematics falls under a single style (or, at best, under two styles), Rabouin's notion of style enables us to distinguish among mathematical collectives. It also seems to better describe how actors actually work. Particular styles, understood as ways of writing, are cultural elements that readily circulate among local cultures. How actors might specify them, in local contexts, and which changes styles undergo in the process of circulation are likewise issues worth pondering. Perhaps more important, we can infer from Rabouin's argument that styles are thus able both to connect local cultures and to mediate between local and more global features. We return below to this type of phenomenon.

Such circulation not only illustrates cultural interaction but also helps account for the stability that mathematical knowledge so often displays. Indeed, insofar as the phenomenon of the stabilization of mathematical knowledge resembles the process toward, and establishment of, sameness with which Ito is concerned, it would be interesting to see if the circulation of ways of writing physics might also prove of explanatory value.

## Part III: Toward a More Global Approach—Elements of a History of Culture Making

Taken together, the chapters in part II demonstrate how actors shape constituent elements specific to the culture in which they work. Importantly for our purpose, the chapters highlight how the formation of these elements draws on both internal and external resources and, more generally, how they are shaped by their larger political and social contexts. Actors design their working tools in response, for instance, to the directions of research and goals chosen, or to requirements from outside cultural formations with which they need to comply. In short, the making of scientific cultures has a history, and this history is as meaningful as the history of concepts and theories. What is more, these histories are intimately related to one another. Indeed, if these cultural elements provide analytic tools for understanding how collectives work, they also have explanatory power with regard to the types of knowledge produced in these contexts. We learn that knowledge and culture are dynamically correlated, with elements of both constantly circulating, taken up, and reworked by actors through the same type of processes.

Similar conclusions arise from the more global approaches to scientific cultures presented in part III. These studies illustrate the variation (according to the cases and issues dealt with) in scale at which analysis must be conducted. Albeit from quite different perspectives, the first two chapters focus on the resources individual actors bring into play in their shaping of their culture.

Koen Vermeir in chapter 9 explores the immediate context in which the term "culture" (as an actor's category, borrowed from agriculture) was first systematically employed to refer to the cultivation of a group of humans, in discussions of educational reform in Jesuit institutions in early modern Europe. These early uses of the term "culture" bring to the fore an issue of general importance to our concerns: the specific type of activity required for cultivating oneself or one's talent—in other words, what MacKenzie calls the process of socialization required to reach the state of culture valued in a particular social context. Vermeir's study also sheds light on how the actors themselves contribute to designing the process of acculturation.

Vermeir identifies the successive editions of Antonio Possevino's *Cultura Ingeniorum* (starting in 1593) as the main sites attesting to the shaping of this Jesuit epistemic culture. Through his analysis, he is able to show the relevance of both goals (e.g., Possevino's political goals, dictated by the agenda of the Jesuit company) and social context (notably, that of the Counterreformation) in the shaping of that culture. As is often the case with actors' categories of culture, the crafting of an identity that could be opposed to others was at the core of the project. But, for Possevino, culture was an activity through which this identity could be achieved, in contrast to other actors' conceptions of culture as a state deriving either from nature or from belonging to a group. Symptomatically, to define this new epistemic culture, Vermeir shows Possevino nevertheless borrowing elements from various sources, selecting and remolding them for his own uses—indeed, much as Georges Cuvier later did in shaping a new scientific culture in paleontology, as Bruno Belhoste describes in chapter 10.

For Possevino, the emphasis was placed on the making of cultivated men, able to fulfill tasks useful for the Jesuit company. For Cuvier, by contrast, the point was to create an environment, modes of reasoning, and practices that would enable him to draw conclusions about new epistemic objects. To approach the constitution of this culture, Belhoste first focuses on the working spaces developed by Cuvier at the end of the eighteenth century and beginning of nineteenth century in his effort to identify and restore extinct species. However, the task Belhoste sets himself leads him to suggest extending the meaning of the notion of working space, to designate less a physical location than a *dispositif*, the system of social, material, and epistemological resources that Cuvier assembled and mobilized (all of which could be found in the urban setting of Paris) to pursue his goals.

Cuvier's achievements depended on the particular way of carrying out research in paleontology that he forged out of this dispositif. He drew crucially from three distinct cultures, from which he created a unique synthesis: the traditions of museum culture, with the knowledge machineries and epistemological values attached to them; the culture of late Newtonianism then dominating the physical sciences in France, and especially the value it placed on specific styles of reasoning and types of explanation; and, finally, the epistemological resources of contemporary historical and antiquarian scholarship. Cuvier also put into play the many social and administrative resources provided and/or facilitated by his position at the National Museum of Natural History, as well as, of course, the wealth of analytic, observational, and graphical skills he personally had honed from years of experience. Belhoste's study shows us how Cuvier was able to fashion from these disparate resources a new and phenomenally productive way of carrying out research in paleontology, even while recycling features from other cultural formations. Cuvier's example (like Possevino's) illustrates the ability of a single individual, while clearly drawing from the social, institutional, and professional world around him, to forge a practice that could evolve from one man's way of doing science into a disciplinary culture. In fact, the forging of this culture represents one of the most important outcomes of Cuvier's work.

So far, we have mainly focused on the local shaping of a culture at a time, examining how this process involved taking up elements from other cultures. However, such a perspective, attentive to a single culture, or at most the cultures from which the latter drew, does not allow us to consider what happens through borrowing and reshaping at higher levels. The question that presents itself now is, does every culture of scientific practice follow a specific path, or are there phenomena that connect and relate these processes with one another? In brief, what does a more global perspective reveal about how these local cultures exchange with one another? Hans-Jörg Rheinberger in chapter 11 offers a crucial contribution to precisely these questions.

In his own effort to account for the generation of meaning in the context of scientific activity, Rheinberger finds it necessary to shift focus to what he calls a meso-level of analysis, intermediate between the micro-level of most laboratory studies and macro-level histories of disciplines. To this end, he invokes the concept of cultures of experiment—networks of experimental systems that are not only held together by material forms of sharing (i.e., by the sharing of technologies, of human agents with scientific know-how, and of biological substrates, objects, and/or experimental environments) but that actually unfold from these material interactions (see Rheinberger 1997, esp. chap. 9, for a development of these ideas).

Sharing, a form of communication that establishes bridges between a local culture and other cultural formations, appears here as a crucial operation, which incidentally evokes Rabouin's suggestion with respect to how styles as ways of writing work in mathematics. However, it would be a mistake to take sharing as a transparent operation. What is shared across a network, how it is shared, and what the effects of sharing are—these are crucial issues that remain in need of further analysis. Indeed, they are the elements by which another form of culture is shaped. What Rheinberger in fact suggests is that here, too, it is part of actors' activity to develop and shape these modes of sharing (indeed, in shaping the entire meso-level of cultural formation), and it behooves us to attend to the work thereby involved.

Rheinberger's particular case study focuses on the culture of in vivo experimentation, and he traces the historical evolution of this culture through the interactions between experimental systems developed in physiology and biochemistry. Despite operating on a different scale, experimental systems play much the same role for Rheinberger in the generation of scientific cultures as do Nancy Nersessian's devices: both serve simultaneously in the formation of a scientific community and as generators and embodiments of its current state of knowledge. In a sense, this view offers a suggestion with respect to both how phenomena like Kuhn's paradigms can take shape, and how the collective in the context of which they are adopted is dynamically formed.

Finally, as to the larger question of what, if anything, is special about scientific/experimental cultures, Rheinberger calls our attention to the inadequacies of the conventional opposition between culture and nature—an opposition that typically echoes the equally canonical division between mind and matter. In opposition to such dichotomies, Rheinberger insists on grounding culture itself in material interactions, while at the same time emphasizing that these interactions constitute the nodes through which meaning/culture is collectively engendered. In so doing, he points to the particular modes of circulation between mind and matter, nature and culture, as a way of addressing the question of what is special about experimental cultures. Rheinberger's approach thus further contributes to elucidating the dynamic and collective construction of sameness.

Through the clear-cut contrast it presents with Rheinberger's work in chapter 11, Fa-Ti Fan's study of an experimental culture in chapter 12 highlights the role actors play in shaping such cultures. It shows different modes of data shaping and data sharing, other types of actors involved, other processes of socialization, and other kinds of networks and forms of communication. Finally, it also brings into focus the impact of other cultural formations in the process of shaping these cultures.

By comparison with Rheinberger's case study, where the scale was smaller than that of a discipline, Fan's analysis of earthquake prediction as practiced in communist China during the Cultural Revolution in the 1960s and 1970s requires more of a macro perspective. It deals not with an academic culture but with a "people's science" of earthquake monitoring. The broader political and cultural context in which this scientific culture took shape can scarcely be ignored. Fan's main point is to stress the interaction of scientific ideas and political beliefs in shaping earthquake monitoring practices. From this political environment developed a culture of seismological practice with an agenda of its own. In particular, Fan emphasizes the variety of means (publication, training, material devices, and even a rhetoric of cultural essentialism) deployed to facilitate the inclusion of all citizens. This new knowledge culture further employed specific knowledge machinery: particular instruments, modes of observation, criteria for the mobilization of personnel and for the collection of data, and modes of communication. Each of these features reflected a value more generally placed on mass participation in the production and consumption of scientific knowledge, and together they account for the marked differences between the culture of seismology that developed in the China of that time and other, more exclusively academic cultures.

Fan's culture of earthquake prediction is further distinguished by methodological assumptions (e.g., an emphasis on the role of macroscopic phenomena in short-term prediction, the relatively greater significance attached to correlations over mathematical modeling, and the importance of a phenomenological approach) that embody prized epistemological values (e.g., precision). Noteworthy for us is the fact that all these cultural features can be correlated with the type of knowledge produced in this context.

Fan's study thus echoes some of the points made by Morgan in her chapter concerning the influence of the wider community on both the knowledge produced and the practices developed by a particular scientific culture. In like manner, Fan's observations of Mao extolling "Chinese science" as a complement to Western science echo Lachenal's and Ito's concerns about the opportunistic uses of self-orientalism. Like these other contributors, Fan too warns against the uncritical adoption of this actors' category in the observers' analytic toolbox.

With this chapter, we conclude our exploration of the approach to culture that is at the core of this book. We began with the theses that cultures are an outcome of actors' knowledge activity, in parallel and, more important, in correlation with other outcomes such as concepts, results, and theories. Exploring these theses led us to notice other equally important phenomena (e.g., the ways in which cultures of scientific practice borrow both from one another and from other cultural formations). Moreover, we can observe the work that actors perform in establishing the networks of sharing that shape higher-level cultures.

## Part IV: Historiographic Implications

Yet a final question still remains: what might be the historiographic implications of identifying multiple coexisting scientific cultures? This is the question that part IV addresses, first from a synchronic and then from a diachronic viewpoint. Both of the case studies in part IV are devoted to mathematics, a discipline for which the historiographic implications of the notions of scientific culture, or of a multiplicity of cultures, have only recently begun to be systematically examined. Furthermore, previous discussions of such issues have either taken "culture" in the singular, or have taken an approach cast in conventional culturalist terms. We aim for a discussion more faithful to the actual practices of mathematics.

Caroline Ehrhardt takes a synchronic perspective in chapter 13, illustrating what can be gained by considering the distinct professional mathematical cultures of nineteenth-century Europe. She makes the strategic choice of concentrating on a single work, one that was widely read at the time: Galois's memoir about the resolution of equations. To shed light on the multiplicity of mathematical cultures, she focuses on the variety of ways in which Galois's ideas, as outlined in this memoir, were appropriated and developed by different groups of mathematicians working in different contexts. Her key point is to suggest that the notion of local culture helps us account for the various readings that were made of Galois's writings and the various mathematical theories that were built on the basis of these readings. Of particular relevance in this case is the fact that these local cultures differ in their practices of both proof and definition, in their ways of writing, in the mathematical contexts in which they interpret and develop Galois's writings, in their goals, and in their epistemological choices. At the same time, however, sufficient commonality needed to underlie all of these local cultures—this points to the existence of the meso-level introduced by Rheinberger—for Galois's writings to be meaningful to all of them in spite of their differences. What Ehrhardt shows is that these separate developments were later reworked and integrated into what came to be considered a unified Galois theory (despite interesting variations from one context to another). She thereby highlights the mathematical fruitfulness of having different mathematical cultures reworking, each in its own way, the same ideas. More generally, at several points, Ehrhardt also emphasizes how mathematicians regularly combine results and practices developed in different mathematical cultures. In this respect, textbooks appear as a specifically important site for the construction and circulation of such hybrid theories—in Rheinberger's terms, one of the sites for the fashioning of a knowledge that can be shared at a meso-level.

Recognizing the coexistence of different mathematical cultures thus enables the historian to account for the various ways in which a single work can come to be appropriated and enriched. In addition, it sheds light on the problem of the highly nonlinear processes by means of which knowledge comes to appear universal. At the same time, like Rabouin, Ehrhardt stresses the limits of uniformity, showing that, even within a given mathematical culture, mathematicians can and do follow their own individual trajectories.

Karine Chemla in chapter 14 also focuses on the historiographic implications of identifying distinct mathematical cultures but from a diachronic perspective. Also, because it deals with long-term history, and more specifically with ancient history, her chapter raises a new set of issues. Sociologists and anthropologists have rightly emphasized fieldwork as providing privileged access to the description of cultures of scientific practice. However, were this the only access, historians of science would be seriously limited in their inquiry. Several chapters in the book have argued that sources from the past also shed light on the character of scientific cultures. Ancient history, with the scarcity of documents that usually haunts it, represents a particular challenge.

The first issue Chemla addresses in her description of mathematical cultures of the past is precisely one of method. Sources that were written in relation to specific cultures of mathematical practice, she argues, often contain clues that historians can use to describe these cultures. Employing a set of Chinese mathematical sources selected from writings composed between the first and the thirteenth centuries, she demonstrates the existence of distinct mathematical cultures in ancient China, appearing at different times and displaying both considerable overlap with and significant differences from one other.

She argues that the main historiographic benefit provided by an interest in ancient cultures is that the identification and description of such cultures give us crucial information for interpreting the available sources. She illustrates this claim with the example of quadratic equations, demonstrating how characterizing the cultures to which the various Chinese sources bear witness enables the historian not only to identify distinct kinds of knowledge about these equations, attested to by these sources, but also to capture in a new way the continuities and differences among them. In brief, conceptual history requires a history of culture making.

Chemla's main thesis—the second historiographic implication she points to—is that the description of cultures thereby yields important tools to carry out conceptual history and brings out new phenomena. Indeed, she argues that the development of new concepts of quadratic equations to which her corpus attests can be directly correlated with aspects of the mathematical culture in which they take shape. However, there is no determinism in this correlation. Rather, Chemla suggests, it indicates "that cultures also change partly in relation to the conceptual work done . . . as much as the concepts change in relation to how actors worked." Especially noteworthy for the general project of the book are the interconnections and correlations between these cultural and conceptual changes, similar to those Nersessian identified in her anthropological study of laboratories. The history of concepts thus appears as deeply intertwined with the history of culture making.

## Conclusion: Suggestions for Future Research

We began this introduction by inquiring about the relation between and among the various concepts that have been recently introduced to characterize the specificity of different ways of making scientific knowledge. The essays in this book suggest a clear answer to that question: differences in cultures of scientific practice are multidimensional. Far from offering alternative descriptions/conceptions of differences among ways of practicing science, each of the various labels (e.g., styles of thought, styles of reasoning, epistemic cultures, or epistemological cultures) captures certain dimensions of these differences. One can argue over the suitability of these labels for particular contexts and suggest alternatives (as, e.g., Rabouin does in response to Hacking), but it is useful to recognize that which dimensions are focused on likely reflect the particular aims of the author.

Whatever the case may be, it thus appears that these labels are complementary. This is why we have mainly spoken of "culture" in this introduction, emphasizing the features brought into focus by this or that more specific concept. Interestingly enough, these labels each reflect the disciplinary culture of their author and the kinds of source material on which he or she has been working. It is, for instance, not by chance that it was through an ethnographical study devoted to, on the one hand, present-day high-energy physics and, on the other, molecular biology that sociologist Knorr Cetina was struck by the relevance of various types of machineries of knowing in the production of knowledge. Such a dimension would not have appeared so prominently through a research work devoted to ancient Greek mathematics and yet might nonetheless prove fruitful for it. Similarly, it is not by chance that Keller's historical work on different disciplinary approaches to the study of embryonic development drew her attention to the philosophical and even logical dimensions of scientific culture.

The variety of case studies explored from different disciplinary perspectives and that make use of different kinds of sources thus appears to be a clear asset. Not surprisingly, the essays in this book, written by scholars with different backgrounds working on different time periods, different disciplines, and different regions of the planet, not only show how different concepts of cultural difference can inform and enrich concrete studies but also invite us to consider other dimensions that might be relevant in characterizing scientific cultures. In science studies, too, the diversity of scientific cultures is an asset, provided that we strive to achieve the meso-level, where our different practices and bodies of conceptual knowledge can be at least partially integrated.

Another question we raised in the beginning of this introduction is, how can one write about scientific cultures as recognizable and consequential categories, without inviting the assumption that they are fixed and closed to external influence? And here, too, we suggest the essays conjointly support and inform the theses we have put forward. The concept of culture that emerges from these studies is something fluid, dynamic, and porous, all properties that result from the fact that, as we have repeatedly stressed, scientific cultures are de facto

- forged by actors in relation to the questions they address, the goals they set themselves, and the resources they have available and recycle;
- in constant interaction with both other cultures and the external environment; and, for that very reason,
- open rather than closed.

These remarks thus call for the development of a history of culture making, in parallel with the histories that attend to concepts or theories.

One important consequence emerges from the features listed above, and it is duly noted and examined in some of the essays. The bodies of knowledge produced in these cultures present correlations with features of the cultures in which they took shape. However, more than determinism, what we see is a phenomenon of coconstruction, whereby each of the two terms, knowledge and culture, is shaped in intimate relation to the other. Moreover, the elements of knowledge thus produced are de facto taken as resources in other cultural contexts. This phenomenon reveals another dimension of actors' activity: establishing bridges between cultures. One facet of the phenomenon is the establishment of meso-level entities introduced by Rheinberger. Another facet is the creation of syntheses of the kind studied by Ehrhardt. These are some of the processes through which the making of sameness is carried out.

Finally, just a few words about the problem of relativism with respect to the sciences. In our view, this problem is sorely in need of clarification. Indeed, the problem is closely related to issues, addressed by several of the contributors here, concerning the stabilization, unification, and even universalization of scientific knowledge, all of which require further investigation. But even on the basis of these brief forays, it seems evident that the emergence of the very question of relativism depends, at least to some extent, on notions of culture as fixed, closed, and impervious to outside influence. If, by contrast, we recognize the fostering of interaction among cultures of scientific practice (both with one another and with the worlds around them) as a key dimension of actors' work, we can begin to recognize the construction of consensus as an ongoing process—one that persists in the face of differences of interpretation, interests, and the purposes for which knowledge is sought. Consensus, sameness, and universality may be ideals to work toward, but they can never be fully realized. And fortunately so, for variation is essential for the fertilization of new cultures.

## *Notes*

1. See Crombie 1994. See the summary of the project in Crombie 1995, which he presents as a "comparative historical anthropology of thinking" (232), comparing between "different civilizations and societies" (227) as well as between different modes of inquiry actors within Western Europe identified and distinguished. The quotations in the main text are on pp. 225, 232, and 225, respectively.

2. In other words, we use the term "culturalism" as defined by Jens-Martin Eriksen and Frederik Stjernfelt (2009): "Culturalism is the idea that individuals are determined by their culture, that these cultures form closed, organic wholes."

3. Knorr Cetina (2005, 68–71), addressing the same issue, also answers positively, emphasizing what in her view the concept of epistemic culture adds to the consideration of practices.

4. We further suggest that any discourse in terms of "Western science" or "Chinese science" is likely to derive from, and further reinforce, assumptions of this type.

5. Noteworthy is the fact that these two criticisms highlight more general ways in which the historical record can be distorted in historiographies of this kind.

6. Kowalenko (2011, 9) notes that Hacking's concept of style of reasoning is not immune to such risks, since "by Hacking's very unrestrictive characterisation [of] styles of scientific thinking, . . . African magical thinking amounts to a distinct African style of scientific thought." Nothing prevents Hacking's concept from being enrolled as support of the validity of cures for AIDS developed in the context of self-proclaimed African exceptionalism. Hacking (2012, 608) shows his awareness of similar objections and promises to reply to them in a forthcoming publication. He is also aware of the fact that some concepts of style have a bleak history. We return to this below.

7. Incidentally, these remarks can help us further clarify the purpose of this book. It is clear that the problems we have in mind are not with a notion of "culture" in the singular. We are not dealing with culture as a range of phenomena separate from science and whose relation to science should be investigated. The problems we address relate to uses of "cultures" in the plural, as illustrated by both Ito's and Lachenal's chapters. The culturalism we focus on emerges in such contexts, and we are specifically interested in understanding the role science and the history of science play at large in these culturalist developments.

8. In one sense, our project bears a resemblance to that of Peter Galison and David J. Stump (1996). The groups of scholars whose contributions are gathered in that book and this one share a similar profile. The main difference lies in the key theses just outlined, and the concerns about ways of conceptualizing "disunity" from which we proceed (see esp. Galison 1996, 2).

9. Note that, compared with material devices (Nersessian), data (Morgan), and forms of communication (Rosental), "styles" now bring into focus ways of working specific to cultural formations, along with the processes of socialization they require.

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