Inventing Temperature

Measurement and Scientific Progress

Hasok Chang

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Complementary Science—History and Philosophy of Science as a Continuation of Science by Other Means

Criticism is the lifeblood of all rational thought.

Karl Popper, "Replies to My Critics," 1974

To turn Sir Karl's view on its head, it is precisely the abandonment of critical discourse that marks the transition to a science.

Thomas S. Kuhn, "Logic of Discovery or Psychology of Research?" 1970

This book has been an attempt to open up a new way of improving our knowledge of nature. If I have been successful in my aim, the studies contained in the preceding chapters of this book will defy classification along traditional disciplinary lines: they are at once historical, philosophical, and scientific. In the introduction I gave a very brief characterization of this mode of study as *complementary science*. Having engaged in several concrete studies, I am now ready to attempt a more extensive and in-depth general discussion of the aims and methods of complementary science. The focus here will be to present complementary science as a productive direction in which the field of history and philosophy of science can advance, without denying the importance of other directions. Such a programmatic statement has a threefold aim. First, it will state explicitly some goals that have already been motivating much work in history and philosophy of science, including my own. Second, a strong statement of these goals will hopefully stimulate further work directed toward them. Finally, a clear definition of the mode of study I am

Some of the ideas elaborated here were initially published in Chang 1999.

advocating may encourage other related modes of study to be defined more clearly in opposition or comparison.[1]

The Complementary Function of History and Philosophy of Science

My position can be summarized as follows: history and philosophy of science can seek to generate scientific knowledge in places where science itself fails to do so; I will call this the *complementary* function of history and philosophy of science, as opposed to its *descriptive* and *prescriptive* functions. Lest the reader should reach an immediate verdict of absurdity, I hasten to add: by the time I have finished explaining the sense of the above statement, some peculiar light will have been thrown on the sense of the expressions "generate," "scientific knowledge," "science," "fails," and "history and philosophy of science" itself. (In the following discussion I will use the common informal abbreviation "HPS" for history and philosophy of science, not only for brevity but also in order to emphasize that what I envisage is one integrated mode of study, rather than history of science and philosophy of science simply juxtaposed to each other. HPS practiced with the aim of fulfilling its complementary function will be called *HPS in its complementary mode* or, synonymously, *complementary science* as I have already done in the introduction.)

In tackling the question of purpose, one could do much worse than start by looking at the actual motivations that move people: why does anyone want to study such a thing as HPS, even devote an entire lifetime to it? Here the only obvious starting point I have is myself, with a recognition that different people approach the field with different motivations. What drove me initially into this field and still drives me on is a curious combination of delight and frustration, of enthusiasm and skepticism, about science. What keeps me going is the marvel of learning the logic and beauty of conceptual systems that had initially seemed alien and nonsensical. It is the admiration in looking at routine experimental setups and realizing that they are actually masterpieces in which errors annihilate each other and information is squeezed out of nature like water from rocks. It is also the frustration and anger at the neglect and suppression of alternative conceptual schemes, at the interminable calculations in which the meanings of basic terms are never made clear, and at the necessity of accepting and trusting laboratory instruments whose mechanisms I have neither time nor expertise to learn and understand.

Can there be a common thread running through all of these various emotions? I think there is, and Thomas Kuhn's work gives me a starting point in articulating it. I am one of those who believe that Kuhn's ideas about normal science were at least as important as his ideas about scientific revolutions. And I feel an acute dilemma about normal science. I think Kuhn was right to emphasize that science as we know it can only function if certain fundamentals and conventions are taken for granted

[1]The expository models I wish to emulate for these purposes are the "Vienna Circle Manifesto" of the logical positivists (Neurath et al. [1929] 1973), and David Bloor's statement of the strong program in the sociology of scientific knowledge (1991, ch. 1).

and shielded from criticism, and that even revolutionary innovations arise most effectively out of such tradition-bound research (see Kuhn 1970a, Kuhn 1970b, etc.). But I also think Karl Popper was right to maintain that the encouragement of such closed-mindedness in science was "a danger to science and, indeed, to our civilization," a civilization that often looks to science as the ideal form of knowledge and even a guide for managing social affairs (Popper 1970, 53). The practice of HPS as a complement to specialist normal science offers a way out of this dilemma between destroying science and fostering dogmatism. I believe that this is one of the main functions that HPS could serve, at once intellectual and political.

In other words, a need for HPS arises from the fact that specialist science[2] cannot afford to be completely open. There are two aspects to this necessary lack of openness. First, in specialist science many elements of knowledge must be taken for granted, since they are used as foundations or tools for studying other things. This also means that certain ideas and questions must be suppressed if they are heterodox enough to contradict or destabilize those items of knowledge that need to be taken for granted. Such are the necessities of specialist science, quite different from a gratuitous suppression of dissent. Second, not all worthwhile questions can be addressed in specialist science, simply because there are limits to the number of questions that a given community can afford to deal with at a given time. Each specialist scientific community will have some degree of consensus about which problems are most urgent, and also which problems can most plausibly be solved. Those problems that are considered either unimportant or unsolvable will be neglected. This is not malicious or misguided neglect, but a reasonable act of prioritization necessitated by limitations of material and intellectual resources.

All the same, we must face up to the fact that suppressed and neglected questions represent a loss of knowledge, actual and potential. The complementary function of HPS is to recover and even create such questions anew and, hopefully, some answers to them as well. Therefore the desired result of research in HPS in this mode is an enhancement of our knowledge and understanding of nature. HPS can recover useful ideas and facts lost in the record of past science, address foundational questions concerning present science, and explore alternative conceptual systems and lines of experimental inquiry for future science. If these investigations are successful, they will complement and enrich current specialist science. HPS can enlarge and deepen the pool of our knowledge about nature; in other words, HPS can generate scientific knowledge.

The following analogy may be helpful in illustrating my ideas about this complementary function of HPS, though it is rather far-fetched and should not be pushed beyond where it ceases to be useful. The most cogent argument for maintaining capitalism is that it is the best known economic system for ensuring high productivity and efficiency which, in the end, translate into the satisfaction of human needs and desires. At the same time, hardly anyone would deny the need for

[2]From here on I will speak of "specialist science" rather than "normal science," so that my discussion would be acceptable even to those who reject Kuhn's particular ideas about normal science or paradigms.

philanthropy or a social welfare system that ameliorates the inevitable neglect of certain human needs and the unreasonable concentration of wealth in a capitalist economy. Likewise, we cannot do without specialist science because we do not know any other method of producing knowledge so effectively. At the same time, we also cannot deny the need to offset some of the noxious consequences of producing knowledge in that manner, including the neglect and suppression of certain questions and the unreasonable concentration of knowledge to a small intellectual elite. Forcing specialist science to be completely open would destroy it, and that would be analogous to anarchy. A better option would be to leave specialist science alone within reasonable limits, but to offset its undesirable effects by practicing complementary science alongside it. In that way HPS can maintain the spirit of open inquiry for general society while the specialist scientific disciplines pursue esoteric research undisturbed.

Philosophy, History, and Their Interaction in Complementary Science

Having explained my basic ideas about the complementary function of HPS, I would like to take a step back and consider more carefully what it means to do historical and philosophical studies of science. Consider philosophy first. It is often claimed that good science should be philosophical as well as technical, and indeed we are still less than two centuries away from the time when scientists routinely referred to themselves as "philosophers." On the other hand, it is also true that most scientists today would regard most discussions currently taking place in professional philosophy as utterly irrelevant to science. The relation between science and philosophy is certainly complex, and this complexity adds to the confusion in trying to see clearly what it is that we are trying to do in the philosophy of science.

I propose taking the philosophy of science as a field in which we investigate scientific questions that are not addressed in current specialist science—questions that could be addressed by scientists, but are excluded due to the necessities of specialization. In Kuhnian terms, science does not emerge from pre-science until the field of legitimate questions gets narrowed down with clearly recognized boundaries. For a long time it was common for one and the same treatise to contain tangled discussions of metaphysics, methodology, and what we would now identify as the proper "content" of science. Some may yearn for those good old days of natural philosophy, but it is not plausible to turn back the clock. Philosophy once aspired to encompass all knowledge, including what we now recognize as science. However, after various scientific disciplines (and other practices such as law and medicine) gradually carved themselves out, what is left under the rubric of philosophy is not the all-encompassing scholarship it once was. Our current academic discipline called "philosophy" became restricted and defined, as it were, against its own will. Philosophy as practiced now does not and cannot include science. But in my view that is just where its most important function now lies: to address what science and other specialisms neglect.

The last thought throws some interesting light on the general nature of philosophy. We tend to call something a question "philosophical" if it is something that we do not normally deal with in the course of routine action although, on reflection, it is relevant to the practice. Similarly, when we say "the philosophy of *X*," we often mean a discipline which deals with questions that are relevant to another discipline *X* but normally not addressed in *X* itself. There are various reasons why relevant questions may be excluded from a system of thought or practices. The questions may be too general; they may threaten some basic beliefs within the system; asking them may be pointless because every specialist knows and agrees on the correct answers; the answers may not make any significant practical difference; and so on. And in the end, questioning has to be selective because it is simply impossible to ask the infinity of all possible questions. But philosophy can function as the embodiment of the ideal of openness, or at least a reluctance to place restrictions on the range of valid questions.

Something very similar can also be said about the history of science. The similarity has two sources: in past science, there are some things that modern science regards as incorrect, and some other things that modern science regards as unnecessary. As scientific research moves on, much of science's past gets lost in a curious mix of neglect and suppression. Instrumental and mathematical techniques are often handed down to younger generations that happily disregard the arguments that had to be settled before those tools could be accepted. Awkward questions tend to be withdrawn after a period in which no clear answers are found, and defeated theories and worldviews are suppressed. Even when old facts and conclusions are retained, the assumptions, arguments, and methods that originally led to them may be rejected. The official "histories" that appear as mere garnishes in many science textbooks are more than happy to leave out all of these tedious or embarrassing elements of the past. They are left to the professional historians of science. Therefore, when the history of science asserts its independence from science itself, its domain is apt to be defined negatively, to encompass whatever elements of past science that current science cares not to retain in its institutional memory.

Given these considerations, it should not come as a surprise that philosophical questions about science and historical questions about science are co-extensive to a considerable degree. This area of overlap provides a strong rationale for practicing HPS as an integrated discipline, not as a mere juxtaposition of the history of science and the philosophy of science. What are regarded as philosophical questions nowadays are quite likely to have been asked in the past as scientific questions; if so, the philosophical questions are simultaneously topics for historical inquiry as well. Whether an investigation in HPS is initially stimulated by philosophical or historical considerations, the result may well be the same.

There are two obvious methods of initiating inquiry in the complementary mode of HPS, or, complementary science. They are obvious because they are rooted in very standard customs in philosophy and history of science. The first method, which has been my primary mode of questioning in this book, is to reconsider things that are taken for granted in current science. As anyone who has been exasperated by philosophers knows, skeptical scrutiny can raise doubts on just about anything. Some of these philosophical doubts can be fruitful starting points for historical inquiry, as it is quite possible that past scientists in fact addressed the same doubts in the process of the initial establishment of those taken-for-granted elements of modern science. This method is quite likely to focus attention on aspects of past science that may easily escape the notice of a historian who is not driven by the same problematic. After the historical record is established, philosophy can take its turn again to reassess the past arguments that have been unearthed. In that way philosophical analysis can initiate and guide interesting historical studies in the category of what I call "problem-centered narratives." This use of philosophy in history of science is very different from the use of historical episodes as empirical evidence in support of general philosophical theses about how science works.

The second method of initiating inquiry in complementary science is to look out for apparently unusual and puzzling elements in past science. This is something that historians of science have become very accustomed to doing in recent decades. History is probably one of the sharpest tools available to the philosopher wishing to explore the presuppositions and limitations of the forms of scientific knowledge that are almost universally accepted now. The historical record often shows us fresh facts, questions, and ways of thinking that may not occur to us even in the course of an open critical scrutiny of current science. In order to facilitate this possibility, we can actively seek elements of past science that have not survived into modern science. After those elements are identified, it is important to investigate the historical reasons for their rejection and assess the philosophical cogency of those reasons.

These processes of historical-philosophical inquiry are intertwined and self-perpetuating, since they will reveal further philosophical concerns and previously unknown bits of history that can stimulate other lines of inquiry. After some thinking about research in complementary science, and certainly while one is immersed in it, it becomes difficult to see where philosophy ends and history begins or vice versa. Philosophy and history work together in identifying and answering questions about the world that are excluded from current specialist science. Philosophy contributes its useful habits of organized skepticism and criticism, and history serves as the supplier of forgotten questions and answers. History of science and philosophy of science are inseparable partners in the extension and enrichment of scientific knowledge. I propose to call the discipline they form together *complementary science* because it should exist as a vital complement to specialist science.

The Character of Knowledge Generated by Complementary Science

Having explained the basic motivations for complementary science and the nature of the historical and philosophical studies that constitute it, I must now give a more detailed defense of the most controversial aspect of my vision. I have claimed that complementary science can *generate* scientific knowledge where science itself fails to do so. On the face of it, this sounds absurd. How could any knowledge about nature be generated by historical or philosophical studies? And if complementary science does generate scientific knowledge, shouldn't it just be counted as part of science, and isn't it foolhardy to suggest that such scientific activity could be undertaken by anyone but properly trained specialists? Such a sense of absurdity is understandable, but I believe it can be dispelled through a more careful consideration of what it means to generate knowledge. I will make such a consideration in this section, with illustrations from the material covered in previous chapters and occasional references to other works. There are three main ways in which complementary science can add to scientific knowledge, which I will address in turn.

Recovery

First of all, history can teach us about nature through the recovery of forgotten scientific knowledge. The potential for such recovery is shown amply in the material uncovered in chapter 1. Many investigators starting from De Luc in the late eighteenth century knew that pure water did not always boil at the "boiling point" even under standard pressure. They built up a growing and sophisticated body of knowledge about the "superheating" of water and other liquids that took place under various circumstances, and at least in one case observed that boiling could also take place slightly under the boiling point as well. But by the end of the nineteenth century we witness Aitken's complaint that authoritative texts were neglecting this body of knowledge, either through ignorance or through oversimplification. Personally, I can say that I have received a fair amount of higher education in physics at reputable institutions, but I do not recall ever learning about the superheating of water and the threat it might pose to the fixity of the boiling point. All I know about it has been learned from reading papers and textbooks from the eighteenth and nineteenth centuries. I predict that most readers of this book will have learned about it from here for the first time.

This is not to say that knowledge of superheating has been lost entirely to modern science. The relevant specialists do know that liquid water can reach temperatures beyond the normal boiling point without boiling, and standard textbooks of physical chemistry often mention that fact in passing.[3] Much less commonly noted is the old observation that water that is actually boiling can have various temperatures deviating from the standard boiling point. There are vast numbers of scientifically educated people today who do not know anything about these very basic and important phenomena. In fact, what they do claim to know is that superheating does not happen, when they unsuspectingly recite from their textbooks that pure water always boils at 100°C under standard atmospheric pressure. Most people are not taught about superheating because they do not need to know about it. As explained in "The Defense of Fixity" in chapter 1, the routine conditions under which thermometers are calibrated easily prevent superheating, so that people who use thermometers or even those who make thermometers need not

[3]See, for example, Oxtoby et al. 1999, 153; Atkins 1987, 154; Silbey and Alberty 2001, 190; Rowlinson 1969, 20. Interestingly, the explanations of superheating they offer are quite diverse, though not necessarily mutually contradictory. Silbey and Alberty attribute it to the collapse of nascent vapor bubbles due to surface tension (cf. De Luc's account of "hissing" before full boil). According to Atkins it occurs "because the vapor pressure inside a cavity is artificially low," which can happen for instance when the water is not stirred. But Oxtoby et al. imply that superheating can only occur when water is heated rapidly.

have any knowledge of superheating. Only those whose business it is to study changes of state under unusual circumstances need to be aware of superheating. This is a case of knowledge that is not widely remembered because knowing it does not help the pursuit of most of current specialist research.

There is another category of experimental knowledge that tends to get lost, namely facts that actively disturb our basic conceptual schemes. The best example of this category that I know is Pictet's experiment discussed in "Temperature, Heat, and Cold" in chapter 4, in which there is an apparent radiation and reflection of rays of cold, as well as rays of heat. This experiment received a good deal of attention at the time and it seems that most people who were knowledgeable about heat in the early nineteenth century knew about it, but gradually it became forgotten (see Chang 2002 and references therein). Nowadays only the most knowledgeable historians of that period of physics seem to know about this experiment at all. Unlike superheating, the radiation of cold is not a phenomenon recognized by most modern specialists on heat and radiation, to the best of my knowledge. It just does not fit into a scheme in which heat is a form of energy and cold can only be a relative deficit of energy, not something positive; remembering the existence of cold radiation will only create cognitive dissonance for the energy-based specialist.

When we make a recovery of forgotten empirical knowledge from the historical record, the claimed observation of the seemingly unlikely phenomenon is likely to arouse curiosity, if not suspicion. Can water really reach 200°C without boiling, as observed by Krebs?[4] Other people's observations can and should be subjected to doubt when there is good reason; otherwise we would have to take all testimony as equally valid, whether they be of N-rays, alien abductions, or spontaneous human combustion. Radical skepticism would lead us to conclude that there is no way to verify past observations, but more pragmatic doubts would lead to an attempt to re-create past experiments where possible.

In conducting the studies included in this book, I have not been in a position to make any laboratory experiments. However, historians of science have begun to re-create various past experiments.[5] Most of those works have not been carried out for complementary-scientific reasons, but the potential is obvious. One case that illustrates the potential amply is the replication of Pictet's experiment on the radiation and reflection of cold, published by James Evans and Brian Popp in the *American Journal of Physics* in 1985, in which they report (p. 738): "Most physicists, on seeing it demonstrated for the first time, find it surprising and even puzzling." Through this work, Evans and Popp brought back the apparent radiation and reflection of cold as a recognized real phenomenon (though they do not regard it as a manifestation of any positive reality of "cold"). However, all indications are that it

[4]Rowlinson (1969, 20) actually notes a 1924 experiment in which a temperature of 270°C was achieved.

[5]Salient examples include the replication of Coulomb's electrostatic torsion-balance experiment by Peter Heering (1992, 1994), and Joule's paddle-wheel experiment by H. Otto Sibum (1995). Currently William Newman is working on repeating Newton's alchemical experiment, and Jed Buchwald has been teaching laboratory courses at MIT and Caltech in which students replicate significant experiments from the history of science.

was quickly forgotten all over again, or not noticed very much. This is not only based on my own patchy impressions of what people do and do not seem to know. A search in the combined Science Citation Index (Expanded), the Social Sciences Citation Index and the Arts and Humanities Citation Index, conducted in March 2003, turned up only two citations. One was a one-paragraph query published in the Letters section of a subsequent number of the *American Journal of Physics* (Penn 1986), and the other was my own article on this subject (Chang 2002)!

The recovery of forgotten knowledge is not restricted to facts, but extends to ideas as well (and it is, after all, very difficult to separate facts and ideas cleanly). In fact, historians of science for many decades have made great efforts to remember all sorts of ideas that have been forgotten by modern science. This kind of recovery is the mainstay of the history of science, so much so that there is no point in picking out a few examples out of the great multitude. But in order for the recovered ideas to enter the realm of complementary science, we need to get beyond thinking that they are merely curious notions from the past that are either plainly incorrect or at least irrelevant to our own current knowledge of nature. I will be considering that point in more detail later.

The consideration of recovery raises a basic question about what it means for knowledge to exist. When we say we have knowledge, it must mean that *we* have knowledge; it is no use if the ultimate truth about the universe was known by a clan of people who died off 500 years ago without leaving any records or by some space aliens unknown to us. Conversely, in a very real sense, we create knowledge when we give it to more people. And the acquisition of the "same" piece of knowledge by every new person will have a distinct meaning and import within that individual's system of beliefs. When it comes to knowledge, dissemination is a genuine form of creation, and recovery from the historical record is a form of dissemination—from the past to the present across a gap created by institutional amnesia, bridged by the durability of paper, ink, and libraries.

Critical Awareness

Superficially, it might appear that much of the work in complementary science actually undermines scientific knowledge because it tends to generate various degrees of doubt about the accepted truths of science, as we have seen in each of the first three chapters of this book. Generating doubt may seem like the precise opposite of generating knowledge, but I would argue that constructive skepticism can enhance the quality of knowledge, if not its quantity. If something is actually uncertain, our knowledge is superior if it is accompanied by an appropriate degree of doubt rather than blind faith. If the reasons we have for a certain belief are inconclusive, being aware of the inconclusiveness prepares us better for the possibility that other reasons may emerge to overturn our belief. With a critical awareness of uncertainty and inconclusiveness, our knowledge reaches a higher level of flexibility and sophistication. Strictly speaking, complementary science is not necessary for such a critical awareness in each case; in principle, specialist scientists could take care not to forget the imperfection of existing knowledge. However, in practice it is going to be very difficult for specialists to maintain this kind of critical vigilance on the foundations of their own practice, except in isolated cases. The task is much more easily and naturally undertaken by philosophers and historians of science.

Even philosophers tend not to recognize critical awareness and its productive consequences as contributions to scientific knowledge. But there philosophy is underselling itself. There is a sense in which we do not truly know anything unless we know how we know it, and on reflection few people would doubt that our knowledge is superior when we are also aware of the arguments that support our beliefs, and those that undermine them. That is not incompatible with the fact that such superior knowledge can constitute a hindrance in the achievement of certain aims that require an effective non-questioning application of the knowledge. I am not able to give a full-fledged argument as to why critical awareness makes superior knowledge, but I will at least describe more fully what I believe in this regard, especially in relation to the fruits of complementary science.

For example, there is little that deserves the name of knowledge in being able to recite that the earth revolves around the sun. The belief carries more intellectual value if it is accompanied by the understanding of the evidence and the arguments that convinced Copernicus and his followers to reject the firmly established, highly developed, and eminently sensible system of geocentric astronomy established by Ptolemy, as detailed by Kuhn (1957) for instance. This is exactly the kind of scientific knowledge that is not available in current specialist science but can be given by HPS. There are many other examples in which work in HPS has raised and examined very legitimate questions about the way in which certain scientific controversies were settled. For example, many scholars have shown just how inconclusive Antoine Lavoisier's arguments against the phlogiston theory were.[6] Gerald Holton (1978) revealed that Robert Millikan was guided by an ineffable intuition to reject his own observations that seemed to show the existence of electric charges smaller than what he recognized as the elementary charge belonging to an individual electron. Allan Franklin (1981) has furthered this debate by challenging Holton's analysis (see also Fairbank and Franklin 1982). Klaus Hentschel (2002) has shown that there were sensible reasons for which John William Draper maintained longer than most physicists that there were three distinct types of rays in the sunbeam.[7] I once added a small contribution in this direction, by showing the legitimate reasons that prompted Herbert Dingle to argue that special relativity did not predict the effect known as the "twin paradox" (Chang 1993).

There is no space here to list all the examples of HPS works that have raised the level of critical awareness in our scientific knowledge. However, I cannot abandon the list without mentioning the thriving tradition in the philosophy of modern

[6]In my view, the most convenient and insightful overview of this matter is given by Alan Musgrave (1976). According to Musgrave, the superiority of Lavoisier's research program to the phlogiston program can only be understood in terms of Lakatos's criterion of progress. Morris (1972) gives a detailed presentation of Lavoisier's theory of combustion, including its many problems.

[7]See also Chang and Leonelli (forthcoming) for a further sympathetic discussion of Draper's reasons.

physics, in which a community of philosophers have been questioning and re-examining the orthodox formulations and interpretation of various theories, especially quantum mechanics. Works in this tradition are often criticized as being neither philosophy nor physics. I think that criticism is understandable, but misguided. Much of the work in the philosophy of modern physics should be regarded as valuable works of complementary science, not as poor pieces of philosophy that do not address general and abstract philosophical concerns sufficiently. An exemplary instance of what I have in mind is James Cushing's (1994) scrutiny of the rejection of the Bohmian formulation of quantum mechanics.

Coming back to the topics discussed in this book, the critical awareness achieved in complementary science is best illustrated in chapter 2. There it was revealed that scientists found it impossible to reach a conclusive positive solution to the problem of choosing the correct thermometric fluid, though Regnault's comparability criterion was effective in ruling out most alternatives except for a few simple gases. Similarly, in chapter 3 we saw that the extension of the thermometric scale to the realms of the very hot and the very cold suffered from similar problems, and that scientists forged ahead without being able to say conclusively which of the competing standards were correct. That is how matters stood at least until Kelvin's concept of absolute temperature was operationalized in the late nineteenth century, as discussed in chapter 4. But the discussion in that chapter showed the futility of the hope that a highly theoretical concept of temperature would eliminate the inconclusiveness in measurement, since the problem of judging the correctness of operationalization was never solved completely, though the iterative solution adopted by the end of the nineteenth century was admirable. And in chapter 1 it was shown that even the most basic task of finding fixed points for thermometric scales was fraught with difficulties that only had serendipitous solutions. I would submit that when we know everything discussed in the first four chapters of this book, our scientific knowledge of what temperature means and how it is measured is immeasurably improved.

New Developments

Recovery and critical awareness are valuable in themselves, but they can also stimulate the production of genuinely novel knowledge. Historians have generally shrunk from further developing the valid systems of knowledge that they uncover from the past record of science. The most emblematic example of such a historian is Kuhn. Having made such strenuous and persuasive arguments that certain discarded systems of knowledge were coherent and could not be pronounced to be simply incorrect, Kuhn gave no explicit indication that these theories deserved to be developed further. Why not? According to his own criterion of judgment, scientific revolutions constitute progress when the newer paradigm acquires a greater problem-solving ability than ever achieved by the older paradigm (Kuhn 1970c, ch. 13). But how do we know that the discrepancy in problem-solving ability is not merely a result of the fact that scientists abandoned the older paradigm and gave up the effort to improve its problem-solving ability? A similar question also arises at the conclusion of some other historians' works on scientific controversy. For example, Steven Shapin and Simon Schaffer (1985) strongly challenged the received wisdom that Thomas Hobbes's ideas about pneumatics were rightly rejected, in favor of the superior knowledge advanced by Robert Boyle. But they gave no indication that it would be worthwhile to try developing Hobbes's ideas further.

The historian of science, of course, has an easy answer here: it is not the job of the historian to develop scientific ideas actively. But whose job is it? It is perfectly understandable that current specialist scientists would not want to be drawn into developing research programs that have been rejected long ago, because from their point of view those old research programs are, quite simply, wrong. This is where complementary science enters. Lacking the obligation to conform to the current orthodoxy, the complementary scientist is free to invest some time and energy in developing things that fall outside the orthodox domain. In this book, or elsewhere, I have not yet engaged very much in such new developments. That is partly because a great deal of confidence is required to warrant this aspect of complementary science, and I have only begun to gain such confidence in the course of writing this book. But some clues have already emerged for potential future work, which I think are worth noting here.

One clear step is to extend the experimental knowledge that has been recovered. We can go beyond simply reproducing curious past experiments. Historians of science have tended to put an emphasis on replicating the conditions of the historical experiments as closely as possible. That serves the purpose of historiography, but does not necessarily serve the purpose of complementary science. In complementary science, if a curious experiment has been recovered from the past, the natural next step is to build on it. This can be done by performing better versions of it using up-to-date technology and the best available materials, and by thinking up variations on the old experiments that would not only confirm but extend the old empirical knowledge. For example, various experiments on boiling, discussed in chapter 1, would be worth developing further. In another case, I have proposed some instructive variations of Count Rumford's ingenious experiments intended to demonstrate the positive reality of what he called "frigorific radiation," following Pictet's experiment on the apparent radiation of cold (Chang 2002, 163). I have not had the resources with which to perform those experiments, but I hope there will be opportunities to carry them out.

Less demanding of resources but mentally more daring would be new theoretical developments. For example, in "Theoretical Temperature without Thermodynamics?" in chapter 4, I made a brief suggestion on how a theoretical concept of temperature might be defined on the basis of the phenomenalistic physics of gases, without relying on thermodynamics or any other highly abstract theories. Less specifically, in my article on the apparent radiation of cold I registered a view that Rumford's theory of calorific-frigorific radiation would be worth developing further, just to see how far we could take it (Chang 2002, 164). Similarly, in a forthcoming article (Chang and Leonelli) on the debates on the nature of radiation, I make an allowance that there may be useful potential in reviving for further development the pluralistic theory postulating different sets of rays responsible for the illuminating, heating, and chemical effects of radiation. These are very tentative suggestions, and not necessarily very plausible lines of inquiry, but I mention them in order to illustrate the kind of developments that may be possible when complementary science reaches its maturity.

The realm of theoretical development is where the complementary scientist is likely to face the greatest degree of objection or incomprehension. If an idea proposed in complementary science does not conform to the currently orthodox view of the directions in which productive new developments are likely to come, specialists will dismiss it out of hand as wrong, implausible, or worthless in some unspecified way. But complementary science is inherently a pluralistic enterprise. Although there may be some past systems of knowledge that are quite beyond the horizon of meaningful revival because they have become so disconnected from even everyday beliefs of the modern world, there is no unthinking dismissal of theoretical possibilities in complementary science. If we look back at a decision made by past scientists and there seems to be room for reasonable doubt, that is a plausible indication that what was rejected in that decision may be worth reviving. When the complementary scientist picks up a rejected research program to explore its further potential, or suggests a novel research program, that is also not done with the crank's conviction that his particular heresy represents the only truth. And if specialists should ever choose to adopt an idea originating from complementary science, they may want to adopt it as the undisputed truth; however, that would still not change the fact that complementary science itself is not in the truth business.

Relations to Other Modes of Historical and Philosophical Study of Science

There are many modes of study that take place under the rubric of the history of science or the philosophy of science. My goal has been to articulate the complementary mode of HPS, not to deny the importance of other modes by any means. Conversely, the complementary mode must not be rejected simply because its aims are different from those adopted in other modes.

In this connection I have one immediate worry. To many historians of science, what I am proposing here will seem terribly retrograde. In recent decades many exciting works in the fields of history and sociology of science have given us valuable accounts of the sciences as social, economic, political, and cultural phenomena. HPS as I am proposing here may seem too internalistic, to the exclusion of the insights that can be gained from looking at the contexts in which science has developed and functioned. The important distinction to be stressed, however, is that HPS in its complementary mode is not *about* science. Its aims are continuous with the aims of science itself, although the specific questions that it addresses are precisely those not addressed by current science; that is why I call it complementary science. HPS in its complementary mode is not meant to be an incomplete sort of history that ignores the social dimension; it is ultimately a different kind of enterprise altogether from the social history of science. One might even say it is not history at all, because history does not in the first instance seek to further our understanding of nature, while complementary science does. I cannot emphasize too strongly that I do not intend to deny the essential importance of understanding science as a social phenomenon, but I also believe that the complementary function of HPS is a distinct and meaningful one.

If we grant that the complementary mode of HPS is legitimate and useful, it will be helpful to clarify its character further by comparing and contrasting it with some other modes of HPS that bear some similarity to it.

Sociology of scientific knowledge. Perhaps curiously, complementary science has one important aspect in common with the sociology of scientific knowledge (SSK): the questioning of accepted beliefs in science. The reinvestigation of familiar facts can be seen as a process of opening Bruno Latour's (1987) "black box" and revealing the character of "science in action." But there is a clear difference between the intended outcomes of such questioning in SSK and in complementary science. SSK deflates the special authority of science as a whole by reducing the justification of scientific beliefs to social causes. In contrast, the aim of skepticism and anti-dogmatism in complementary science is the further enhancement of particular aspects of scientific knowledge. In some cases work in complementary science may show some past scientific judgments to have been epistemically unfounded, but that is different from SSK's methodological refusal to recognize a distinction between epistemically well founded and unfounded beliefs.[8]

Internal history. From the concrete studies I have offered, it will be obvious that much of what I regard as the past achievement of HPS in its complementary mode comes from the tradition of the internal history of science. Is complementary science simply a continuation of that tradition, in which one tries to uncover and understand scientific knowledge faithfully as it existed in the past? There is one important reason why it is not. If we pursue internal history for its own sake, our ultimate aim must be the discovery of some objective historical truth, about what past scientists believed and how they thought. This is not the final aim of complementary science, which only makes use of the internal history of science in order to increase and refine our current knowledge. One significant difference stemming from this divergence of aims is that complementary science does not shrink from making normative epistemic evaluations of the quality of past science, which would be anathema to the "new" internal history of science.[9] Still, complementary science is by no means committed to Whiggism, since the judgments made by the historian-philosopher can very easily diverge from the judgments made by the current specialist scientists.

Methodology. Complementary science is also distinct from the search for "the scientific method," namely the most effective, reliable, or rational method of gaining

[8]David Bloor (1991, 7) states this point unequivocally, as one of the main tenets of the strong program in the sociology of scientific knowledge: "It would be impartial with respect to truth and falsity, rationality or irrationality, success or failure. Both sides of these dichotomies will require explanation." Moreover, "the same types of cause would explain, say, true and false beliefs."

[9]I am referring to the anti-Whiggish type of internal history that Kuhn once designated as the "new internal historiography" of science ([1968] 1977, 110).

knowledge about nature. This may sound puzzling, considering that a good deal of the discussion in my concrete studies was very much about scientific methodology, and all of chapter 5 was devoted to it. Studies in complementary science can and do involve questions about the methods of acquiring knowledge, but there is a significant difference of focus to be noted. The attitude toward methodology taken in complementary science is much like most practicing scientists' attitude toward it: methodology is not the primary or final goal of inquiry. What we call good methods are those methods that have produced useful or correct results; this judgment of goodness comes retrospectively, not prospectively. In other words, methodological insights are to be gained as by-products of answering substantive scientific questions; when we ask a question about nature, how we find an answer is part of the answer. In complementary science we do not set down general methodological rules for science to follow. We only recognize good rules by seeing them in action, as successful strategies perhaps worth trying elsewhere, too.

Naturalistic epistemology. Finally, complementary science must be distinguished from a strong trend in current philosophy of science, which is to give a characterization of science as a particular kind of epistemic activity, without a commitment to normative implications (see Kornblith 1985). This trend probably arises at least partly in reaction to the apparent futility of trying to dictate methodology to scientists. The "naturalistic" impulse is to an extent congenial to complementary science because it provides a strong motivation for an integrated HPS. But what naturalistic epistemology fosters is HPS in the *descriptive* mode, which takes science primarily as a naturally existing object of description. In contrast, for HPS in the complementary mode, the ultimate object of study is nature, not science.

A Continuation of Science by Other Means

In closing, I would like to return briefly to the relation between specialist science and complementary science. One big question that I have not discussed sufficiently so far is whether complementary science is an enterprise that is critical of orthodox specialist science, and more broadly, what normative dimensions there are to the complementary function of HPS. This is a difficult question to answer unequivocally, and I think the subtlety of the issue can be captured as follows: complementary science is *critical* but not *prescriptive* in relation to specialist science.

There are two different dimensions to the critical stance that complementary science can take toward specialist science. First, when complementary science identifies scientific questions that are excluded by specialist science, it is difficult to avoid the implication that we would like to have those questions answered. That is already a value judgment on science, namely that it does not address certain questions we consider important or interesting. However, at least in a large number of cases, this judgment also comes with the mitigating recognition that there are good reasons for specialist science to neglect those questions. That recognition prevents the step from judgment to prescription. The primary aim of complementary science is not to tell specialist science what to do, but to do what specialist science is presently unable to do. It is a shadow discipline, whose boundaries change exactly so as to encompass whatever gets excluded in specialist science.[10]

The second dimension of the critical stance is more controversial, as I have discussed in "The Character of Knowledge Generated by Complementary Science." On examining certain discarded elements of past science, we may reach a judgment that their rejection was either for *imperfect reasons* or for *reasons that are no longer valid*. Such a judgment would activate the most creative aspect of complementary science. If we decide that there are avenues of knowledge that were closed off for poor reasons, then we can try exploring them again. At that point complementary science would start creating parallel traditions of scientific research that diverge from the dominant traditions that have developed in specialist science. It is important to note that even such a step falls short of a repudiation of current specialist science. Since we do not know in advance whether and to what degree the complementary traditions might be successful, the act of creating them does not imply any presumption that it will lead to superior results to what the specialists have achieved since closing off the avenues that we seek to reopen. (All of this is not to deny that there are possible situations that would call for a prescriptive mode of HPS, in which we question whether science is being conducted properly, and propose external intervention if the answer is negative.)

Complementary science could trigger a decisive transformation in the nature of our scientific knowledge. Alongside the expanding and diversifying store of current specialist knowledge, we can create a growing complementary body of knowledge that combines a reclamation of past science, a renewed judgment on past and present science, and an exploration of alternatives. This knowledge would by its nature tend to be accessible to non-specialists. It would also be helpful or at least interesting to the current specialists, as it would show them the reasons behind the acceptance of fundamental items of scientific knowledge. It may interfere with their work insofar as it erodes blind faith in the fundamentals, but I believe that would actually be a beneficial effect overall. The most curious and exciting effect of all may be on education. Complementary science could become a mainstay of science education, serving the needs of general education as well as preparation for specialist training.[11] That would be a most far-reaching step, enabling the educated public to participate once again in building the knowledge of our universe.

[10]That is not to say that those boundaries are completely sharp. The boundaries of complementary science will be fuzzy, to the extent that the boundaries of science are fuzzy. But the existence of gray areas does not invalidate the distinction altogether. Also, someone who is primarily a specialist scientist may well engage in some complementary scientific work and vice versa; that is no stranger than a scientist exploring the artistic dimensions of scientific work.

[11]The importance of the history and philosophy of science to "liberal" science education has been argued by many authors, as documented thoroughly by Michael Matthews (1994). For me the chief inspiration comes from the vision behind James Bryant Conant's general education program at Harvard, and its extension by Gerald Holton and his associates (see the introduction to Conant 1957, and Holton 1952).