Chapter 1

A Brief Historical Introduction to the Philosophy of Science*

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Philosophy of science is an old and practiced discipline. Both Plato and Aristotle wrote on the subject, and, arguably, some of the pre-Socratics did also. The Middle Ages, both in its Arabic and high Latin periods, made many commentaries and disputations touching on topics in philosophy of science. Of course, the new science of the seventeenth century brought along widespread ruminations and manifold treatises on the nature of science, scientific knowledge and method. The Enlightenment pushed this project further trying to make science and its hallmark method definitive of the rational life. With the industrial revolution, "science" became a synonym for progress. In many places in the Western world, science was venerated as being the peculiarly modern way of thinking. The nineteenth century saw another resurgence of interest when ideas of evolution melded with those of industrial progress and physics achieved a maturity that led some to believe that science was complete. By the end of the century, mathematics had found alternatives to Euclidean geometry and logic had become a newly re-admired discipline.

But just before the turn to the twentieth century, and in those decades that followed, it was physics that led the intellectual way. Freud was there too, he and Breuer having published *Studies in Hysteria* in 1895, but it was physics that garnered the attention of the philosophers. Mechanics became more and more unified in form with the work of Maxwell, Hertz and discussions by Poincaré. Plank derived the black body law in 1899, in 1902 Lorenz proved Maxwell's equations were invariant under transformation, and in 1905 Einstein published his paper on special relativity and the basis of the quantum. Concomitantly, Hilbert in 1899 published his foundations of geometry, and Bertrand Russell in 1903 gave forth his principles of mathematics. The development of unified classical mechanics and alternative geometries, now augmented and challenged by the new relativity and quantum theories made for period of unprecedented excitement in science.

What follows provides a brief historical overview of the problems and concepts that have characterized philosophy of science from the turn of the twentieth century until the present day. This is presented in the form of conceptual and problem-oriented history because I believe that the real interest in philosophy of science and the lessons to be learned from its history are found in the topics it addressed and the methods it used to address them. Further, the cast of characters, and the specific articles and books can be easily researched by anyone who is interested. There is, appended a selective chronological bibliography of "classical" sources.

A few caveats need to be stated from the start. First, I deal almost exclusively with certain aspects of one Austro-Germanic-Anglo-American tradition. This is not because there was not interesting and important work in philosophy of science going on in France and elsewhere. I do this, first, because this tradition is the one that is formative for and dominant in contemporary American philosophy (for good or ill), and, second, because it is the tradition in which I was raised and about which I know the most. Another caveat is that space limitations and ignorance often require the omission of many interesting nuances, qualifications and even outright important facets of the history of philosophy of science. What I try to do is run a semi-coherent thread through the twentieth century, in such ways that a developmental narrative can be followed by those who have not lived within the confines of the discipline. Many scholars would have done things differently. *C'est la vie!*

To provide some structure for the exposition, I shall break this text into three important periods:

- 1918–50s: Logical Positivism to Logical Empiricism
- 1950s through 1970s: New Paradigms and Scientific Change
- Contemporary Foci: What's "hot" today

Logical Positivism to Logical Empiricism: 1918–55

As was noted above, the forming spirit of twentieth century philosophy of science were the grand syntheses and breakthroughs (or revolutions) in physics. Relativity and, later, quantum theory caused scientists and philosophers alike to reflect on the nature of the physical world, and especially on the nature of human knowledge of the physical world. In many ways, the project of this new philosophy of science was an epistemological one. *If* one took physics as the paradigmatic science, and *if* science was the paradigmatic method by which one came to obtain reliable knowledge of the world, then the project for philosophy of science was to describe the structure of science such that its epistemological underpinnings were clear. The two antecedents, that physics was the paradigmatic science and that science was the best method for knowing the world, were taken to be obvious. Once the structure of science was made precise, one could then see how far these lessons from scientific epistemology could be applied to others areas of human endeavor.

Another important background tradition needs to be described. Propositional and predicate logic became the model for clear reasoning and explicit statement. First in the work of Frege (in the 1880s–90s), and later with Russell and Whitehead (in the 19-teens), logic came to be regarded as the way to understand and clarify the foundations of mathematics. It became the ideal language for modeling any cognitive enterprise. Simultaneously, Hilbert re-introduced to the world the ideal of axiomatization. Again this was a clarifying move to ensure that there were no hidden assumptions, and everything in a system was made explicit. This logico-mathematical language became the preferred form, because of its precision, into which philosophy of science had to be cast.

The epistemological project of the positivists was to explicate how science was grounded in our observations and experiments. Simultaneously, the goal was to provide an alternative to the neo-Kantianism that was the contemporaneously concurrent form of philosophy. Taking from the tradition of British empiricism, empirical grounding, or being based on the facts, was seen as the major difference between science and the other theoretical and philosophical pretenders to knowledge. This insight led the positivists to attempt to formulate and solve the problem of the nature of meaning, or more specifically, empirical meaning. What was it, they asked, that made statements about the world meaningful? This attempt to explicate the theory of meaning had two important parts: First, claims about the world would have to be made clear, avoiding ambiguity and the other confusions inherent in natural language. To this end, the positivists tried to restrict themselves to talking about the language of science as expressed in the sentences of scientific theories, and attempted to reformulate these sentences into the clear and unequivocal language of first-order predicate logic. Second, they tried to develop a criterion that would show how these sentences in a scientific theory related to the world, i.e. in their linguistic mode this became the problem of how theoretical sentences related to observation sentences. For this one needed to develop a procedure for determining which sentences were true. This method came to be codified in the verification principle, which held that the meaning of an empirical sentence was given by the procedures that one would use to show whether the sentence was true or false. If there were no such procedures then the sentence was said to be empirically meaningless.

The class of empirically meaningless sentences were said to be non-cognitive, and they included the sentences comprising systems of metaphysics, ethical claims and, most importantly, those sentences that made up theories of the pseudosciences. This latter problem, distinguishing scientific sentences from those only purporting to be scientific, came to known (following Karl Popper's work) as the demarcation problem.

The verification principle was thought to be a way of making precise the empirical observational, or experimental component of science. Obviously, the positivists, following in the empiricist tradition, thought, the basis of science lay in observation and in experiment. These were the tests that made science reliable, the foundation that differentiated science from other types of knowledge claims.

So, formally, what was needed was a set of sentences that bridged the gap from scientific theory to scientific experiment and observation. These sentences that tied theory to the world were called bridge sentences or reduction sentences. The set of sentences that described the world to which theoretical sentences were reduced or related was called the observation language. Sentences in the observation language were taken to be easily verifiable or decidable as to their truth or falsity.

So that these bridge sentences might be made very explicit, theories were themselves idealized as sets of sentences that could be put into an axiomatic structure, in which all their logical relations and deductions from them could be made explicit. The most important sentences in a scientific theory were the laws of science. Laws came in two types: universal and statistical. Universal Laws were sentences of the theory that had unrestricted application in space and time (sometimes they were explicitly said to be causal, and, later, they were held to be able to support counterfactual claims.) Idealized universal laws had the logical form:

$$(x)(Fx \supset Gx)$$

Since such a form could be used to clearly establish their logical implications. Obviously, this was an idealized form, since most of the laws of interest were from physics and had a much more complex mathematical form. Statistical laws only made their conclusions more or less probable.

Scientific explanation was conceived as deducing a particular sentence (usually an observation or basic sentence) from a universal law (given some particular initial conditions about the state of the world at a time). The particular fact, expressed by the sentence, was said to be explained if it could be so deduced. This was called the deductive-nomological model of explanation. "Nomos" is the Greek word for law. If, a particular sentence was deduced before the fact was observed, it was a prediction, and then later if it was verified, the theory from which it was deduced was said to be confirmed. This was the hypothetico-deductive model because the law was considered an hypothesis to be tested by its deductive consequences.

The names of some of the major players in this period of philosophy of science were Moritz Schlick, Rudolf Carnap, Otto Neurath, Hans Reichenbach, and Carl Hempel. There were two main groups, one centered in Vienna (Schlick, Carnap and Neurath), called the Vienna Circle that was established late in the 1920s, and the other, coming a bit later, in Berlin (Reichenbach and Hempel). There was a important third group in Warsaw, doing mostly logic and consisting of Alfred Tarski, Stanislau Lesnewski and Tadeusz Kotarbinski.

This view of science, as an idealized logically precise language which could have all its major facets codified, never worked. Throughout the history of logical positivism there were debates and re-formulations among its practitioners about the idealized language of science, the relations of explanation and confirmation, the adequate formulation of the verification principle, the independent nature of observations, and the adequacy of the semantic truth predicate. The static, universalist nature of science that was idealized by positivism proved to be wrong. The attempt to fix procedures and claims in a logically simplified language proved to be impossible. The neat, clear attempts at explicating explanation, confirmation, theory and testability, all proved to have both internal difficulties with their logical structures and external problems in that they did not seem to fit science as it was actually practiced.

The positivists themselves were the first to see the problems with their program, and, as they attempted to work out the philosophical difficulties, the positions changed shifted into what became called logical empiricism. This happened in the mid-to late 1930s, the same time that many of the group left Germany and Austria because of World War II and the rise of Adolph Hitler. Reichenbach left Germany immediately after Hitler took power in 1933 and went first to Istanbul, Turkey, Richard von Mises went also. Reichenbach then in 1938 went to UCLA in the USA. Neurath and Popper both ended up in England. Carnap, from Prague, and Hempel, from Berlin, came to the USA.

Here is bit more sociology of the how philosophy of science developed. The first modern program in history and philosophy of science (HPS) was set up at University College, London. A. Wolf first offered a history of science course in collaboration with Sir William Bragg and others in 1919–20. Then a "Board of Studies in Principles, Methods and History of Science" was established in 1922, and an M.Sc. was first offered in 1924. Wolf was the first holder of the chair in "History and Method of Science." In 1946, the Chair became full time with the appointment of Herbert Dingle. The London School of Economics' Department evolved after the appointment of Karl Popper to the Readership in Logic and Scientific Method in 1945. The same Wolf who was associated with U.C., London also held the Chair in Logic and taught courses at LSE, prior to Popper. The University of Melbourne in 1946 began teaching courses in HPS.

Erkenntnis, the journal of the Vienna Circle, or rather the Max Plank Society, was first published in 1930. This followed on the first congress on the Epistemology of the Exact Sciences held in Prague in September of 1929. In 1934 the journal, *Philosophy of Science*, published its first issue. William M. Malisoff, a Russian biochemist, was its first editor. Malisoff died unexpectedly in 1947, and C. West Churchman became editor. The Philosophy of Science Association was in existence in 1934. In 1948 the PSA had 153 members, and Philipp Frank was its President. In the discipline of history of science, the American History of Science Society was founded in 1924. The HSS journal *Isis*, had been started earlier in 1912 by George Sarton when he was still in Belgium.

Logical empiricism never had the coherence as a school that logical positivism had. Various influences began to make themselves felt after the late 1930s. One most important conceptual addition came from American born pragmatism. Its specific influences can be seen clearly in the post-1940 work of Hempel, and even Carnap; also in the work of American born, Ernest Nagel and W. V. O Quine. But, until the late 1950s, philosophers of science, despite significant changes in the programs and allowable methods, philosophers of science were still trying to work out and change things to fit into the goals and aspirations left by the positivists. Moreover, it ought to be noted clearly that virtually all the major moves that were to come later and so change the character of philosophy of science were first initiated by the original positivists themselves. This continuity was not noted by those who became famous during the next decades; they saw themselves as revolutionary and stridently anti-positivistic. By the late 1950s, philosophy of science included ever-increasing complex models, much looser claims, many new philosophical methods and increasingly vague philosophical goals.

New Paradigms and Scientific Change: Late 1950s through the 1970s

While the logical positivists, and later the logical empiricists, were attempting to explicate and clarify the structure of science, another group of scholars had begun to transform an old activity into the modern academic discipline of history of science. The goal of much history of science was to examine historically significant intellectual episodes in science and to articulate these analytically in a way that exhibited the character of science at that particular historical moment and also showed that moment fit into the development and progress of science. Questions for which answers were sought were, e.g. about the nature of Galileo's physics, and what made it both continuous with and yet different from his medieval predecessors. Was Galileo the last of the Medievals or the first of the moderns? What was the nature of Galileo's methodology, and how did he frame explanations? Was Galileo's use of mathematics in physics really revolutionary? Did Galileo really use experiments in some modern sense? Of course, it was not just Galileo who was of interest, historians of science studied all the heroes of modern science, and reached backwards into the Greek, Roman and Medieval periods. The attempt was to describe the actual practice of science of these thinkers and to discern what was peculiar to these historical periods. While history of science courses had been taught in a number of places, by the mid-1960s history of science was an established enterprise with programs and departments in Universities that trained graduate students in the discipline. Actually, the University of Wisconsin started its department in 1942, but World War II kept it from being staffed until 1947. Harvard offered degrees in History of Science, but their department was started only in 1966.

In the late 1950s, philosophers too began to pay more attention to actual episodes in science, and began to use actual historical and contemporary case studies as data for their philosophizing. Often, they used these cases to point to flaws in the idealized positivistic models. These models, they said, did not capture the real nature of science, in its ever-changing complexity. The observation language, they argued, could not be meaningfully independent of the theoretical language since the terms of the observation language were taken from the scientific theory they were used to test. All observation was theory-laden. Yet, again, trying to model all scientific theories as axiomatic systems was not a worthwhile goal. Obviously, scientific theories, even in physics, did their job of explaining long before these axiomatizations existed. In fact, classical mechanics was not axiomatized until 1949, but surely it was a viable theory for centuries before that. Further, it was not clear that explanation relied on deduction, or even on statistical inductive inferences. The various attempts to formulate the deductive-nomological model in terms of necessary and sufficient conditions failed not only because counter-examples were found, but also because explanation seemed to be more complex phenomena when one looked at examples from actual sciences. Even the principle of verification itself failed to find a precise, or even minimally adequate, formulation.

All the major theses of positivism came under critical attack. But the story was always the same – science was much more complex than the sketches drawn by the positivists, and so the concepts of science – explanation, confirmation, discovery – were equally complex and needed to be rethought in ways that did justice to real science, both historical and contemporary. Philosophers of science began to borrow much from, or to practice themselves, the history of science in order to gain an understanding of science and to try to show the different forms of explanation that occurred in different time periods and in different disciplines.

Debates began to spring up about the theory ladeness of observation, about the continuity of scientific change, about shifts in meaning of key scientific concepts, and about the changing nature of scientific method. These were both fed by and fed into philosophically new areas of interest, areas that had existed before but which had been little attended to by philosophers. The social sciences, especially sociology, became of considerable interest, as did evolutionary biology. These fields provided not only new sciences to study and to be contrasted with physics, but also new models and methods which were then borrowed to study science itself.

By the early 1960s, as the result of the work of Thomas Kuhn – and concurrently Norwood Russell Hanson and Paul Feyerabend – the big philosophical question had become: Were there revolutions in science? The problem of scientific change, as it was called, dealt with issues of continuity and change.

Kuhn had argued that science in one period is characterized by a set of ideas and practices that constitute a paradigm, and when problems or anomalies begin to accumulate in a given paradigm, there often was introduced a new paradigm which, in fact and in logic, repudiated the old and supplanted it. (This model was not unlike Gaston Bachelard's view about crises in science leading to *rupture*.) This concept of a revolutionary paradigm shift implied that scientific change was discontinuous, and that the very meaning of the same terms, e.g. "mass", changed from their use in one paradigm (Newtonian) to their use in the new paradigm (Einsteinian). This was called meaning variance. One methodological implication for philosophers of science, clearly, was that to study science, one had to confine oneself to a historically dominant paradigm and one could not look for more general, trans-paradigmatic models that covered all science, except maybe for the process of paradigm change itself.

Many philosophers made a job of criticizing Kuhn's paradigms and his program. They began to search for alternative, general models of scientific change that were more accurate in describing episodes in science, more sensitive in analyzing the parts of science that actually underwent change, and that avoided the ambiguities and unclarities of Kuhn. So, talk of paradigms gave way to research programmes (Lakatos) and then to research traditions (Laudan). Another group of philosophers began to look at explanations in different periods and disciplines to find out if there could be general principles that could be said to apply to all explanations, and thus undercut the meaning variance thesis. Yet, other thinkers, including some philosophers, began to take Kuhn's claims about practices seriously, argued, as had some historians of science earlier, that science could not be explained solely in terms of its concepts and internal structure. One needed, it was held, to understand the social and political settings in which such concepts were developed to understand how they became acceptable and why they were thought to be explanatory.

It should be noted also that many of the more purely philosophical moves (including those of Hanson, Kuhn and Feyerabend) had been influenced by the new dominance of the more central philosophical practices of ordinary language philosophy, inspired to a large extent by the work of the later Wittgenstein. This was still philosophy which dealt with analyzing language, but the language was no longer just the formal a language of logic, but the various language games the comprised the various disciplines of human endeavor. New directions in linguistics, spurred on by Chomsky and his followers, had also changed the way people, including philosophers, looked the problem of syntax, semantics, and meaning. Even basic epistemology itself began to be questioned. W. V. O. Quine (1969) announced to world that philosophy of science was philosophy enough, and epistemology had to be naturalized and was part of natural science.

By the mid 1960s, logical positivism and logical empiricism was quite out of fashion in Anglo-American philosophy. At this time, philosophical analysis was the key mode of operation, and the logicism that had provided the guiding model for the earlier philosophical work, was superseded by the study of real scientific language and by the complexities uncovered in studying the history of science. During this period Indiana University founded its Department of History and Philosophy of Science (1960), which was followed a decade later by the institution of HPS at the University of Pittsburgh (1971). Adolph Grünbaum was president of the Philosophy of Science Association in 1968. (The preceding President was Ernest Nagel.) The PSA seems to have waned somewhat during the post war years, but Grunbaum began the tradition of biennial meetings that continues to this day.

The result for philosophy of science was invigorating, exciting, and devastating. General characterizations of scientific change proved to be just as intractable as earlier general models of scientific explanation. The laudable tendency to explore the nature of sciences other than physics and to examine in detail cases from the history of many sciences left philosophers without a "paradigm." There was little consensus about the nature of explanation, confirmation, theory testing or, even, scientific change. Yet science itself, more than ever, was recognized by the populace at large, as a (if not the) major force in human life, and philosophy of science had become a discipline to stand along side of ethics, epistemology and metaphysics. But there was intellectual disarray over its nature in the philosophical community at large. In fact, some philosophers, following Paul Feyerabend took the intellectual confusion as evidence that science had no identifiable structure, and proffered the view that in science, as in art, "anything goes." All evidence and proof is just rhetorical, and those with the best rhetoric, or the most power (Foucault), become the winners, i.e. their theories became the ones accepted. Luckily, this epistemological relativism was not followed by many philosophers, though, as we shall see below in some contemporary communities this idea still flourishes.

A consensus did emerge among philosophers of science. It was not a consensus that dealt with the concepts of science, but rather a consensus about the "new" way in which philosophy of science must be done. Philosophers of science could no longer get along without knowing science and/or its history in considerable depth. They, hereafter, would have to work within science as actually practiced, and be able to discourse with practicing scientists about what was going on. This was a major shift in the nature of philosophy. It is true that most of the early positivists were trained in science, usually physics. But this scientific training had led them to try to make philosophy scientific after the image of their own philosophical–logical model of science. In contrast, from the 1950s on, more and more philosophers had been trained by the Oxbridge inspired analytic philosophers, who adhered to Wittgenstein's dictum that philosophy was a *sui generis* enterprise and so had nothing to do with, and nothing to learn from, science. It is no wonder that students of philosophy so trained found it hard to figure out what philosophers of science should be doing, and as a result turned either to science itself or to various forms of sociology of science, which was taken to be legitimate because it was a sub-discipline of an actual science (sociology). Ironically, despite this confusion about goals, there were more philosophers of science than ever before.

Contemporary Foci and Future Directions

The turn to science itself meant that philosophers not only had to learn science at a fairly high level, but actually had to be capable of thinking about (at least some) science in all its intricate detail. In some cases philosophers actually practiced science, usually theoretical or mathematical. This emphasis on the details of science led various practitioners into doing the philosophy of the special sciences. Currently, there are philosophers of space-time, who variously specialize in special or general relativity theory, and philosophers of quantum theory and quantum electro-dynamics. There do not seem to be any philosophers of plasma physics. Fairly recently, philosophy of chemistry has become somewhat of a "hot" research area. Philosophers of biology continue to work on problems in evolutionary theory, and finally some study molecular biology, which is the area in which almost all biologists work. Work on genetics has been around for some time, but usually connected to evolutionary biology. Work on biological development is just starting and is seen to be increasingly important.

With the explosion of health care, philosophy of medicine also became a newly emergent and important field of research. Philosophy of the social sciences still continues to be worked upon, but sociology as the paradigmatic social science has been replaced by anthropology, except for those people who work in science studies which still treats sociology with some respect. Philosophy of economics, especially game theoretic modeling, is a somewhat popular field today. This is interesting since the game theory model had been started in the 1940s (von Neumann and Morgenstern), and then mostly dropped in 1960s, only to be revived by biologists using game theory to model evolution and by experimental economists trying to find an empirical model for studying economic behavior; these then influenced philosophers of economics who revived game theory as tool for economic analysis.

One of the most innovative and biggest changes has come in the area that used to be known as philosophy of psychology. Philosophy of psychology used to be tied to philosophical psychology, to philosophy of mind, and to behaviorism and cognitive psychology, especially to questions about the nature of the mental. In a way it still is, but the "cognitive revolution" hit philosophy quite hard. Cognitive studies now includes many of those working in experimental psychology, neuroscience, linguistics, artificial intelligence, and philosophers. There are many aspects to this re-defined field, including work on problems of representation, explanatory reduction (usually to neuroscience), and even confirmation. Confirmation theory has used techniques from artificial intelligence to re-establish a modern form of older confirmation functions as developed originally by Carl Hempel. Cognitive problem solving has even been used by some to model the nature of science itself. A new direction to be explored are the relations of neuroscience to traditional philosophical problems, such a representation and knowledge.

Historically, it is of note that cognitive science began to emerge in the mid-1950s, close to the time that the shift away from logical positivism began. Many of the intellectual forces that caused the philosophical change were also the causes of the emerging new cognitive paradigm, but, even more importantly, one needs to note the impact of the computer and its related ways of acting and thinking. The computer was not only a tool for calculation, reasoning and processing, but also became also a model for thinking about human beings, and, even, for thinking about science.

One interesting implication of this work in the specialized sciences is that many philosophers have clearly rejected any form of a science/philosophy dichotomy, and find it quite congenial to conceive of themselves as, at least in part of their work, "theoretical" scientists. Their goal is to actually make clarifying and, sometimes, substantive changes in the theories and practices of the sciences they study.

A very different current trend is exhibited by those philosophers of science who have become part of the science studies movement, which is dominated by historians and sociologists. This movement focuses on the social dimensions of science (as opposed to the "outmoded" intellectual aspects.) In one sense the social study of science grew out of the dispute between internalist and externalist historians of science, which was resolved in favor of the externalists when the discipline of history itself shifted to quantitative social history and away from intellectual history. From another direction the work of the epistemological relativists, whom I referred to earlier, fits nicely with the relativism thought to characterize historical periods and with cultural (and ethical) relativism that is rampant in much of cultural anthropology. Essentially the view here is that science is a human social activity not unlike any other and so is subject to historical and cultural contingencies. In order to study such activities we must look at the socio-cultural milieu in which scientists are raised, trained, and in which their work occurs. So, for example, we should study the laboratories in which scientists work and describe how these function to self-validate knowledge claims issued from the laboratory. Moreover, we should study the conventions of discourse that comprise the "rules" by which scientists' influence and exert power over one another. For example, in the seventeenth century there were codes of conduct that English gentleman "had" to adhere to, and these provided (somehow) the structure of the debates and experimental practices for the members of the Royal Society. A concomitant belief held by most of the science studies group, though it is not necessarily implied by their position, is the relativism of different or competing claims. That is, it is a historical, cultural and/or epistemic peculiarity that a given group of scientists holds the views that they do. From this, it is presumed to follow that no one view is any better than any other. You are what your time and culture have made you, and that's an end to it.

Such claims for relativism often lead people to worry about values and their status, for cultural relativism is closely tied with ethical relativism. But questions about the relations between values and science also arose from even more pressing sources. Perhaps the most important and influential questions about values arose from medicine. The practical problems of medical ethics began to make themselves felt due to changes in the practice of medicine and in medical technology. All of a sudden, there were urgent questions concerning life and death, physician-patient relations, and informed consent that had to be answered in pragmatically expeditious ways. This coincided with, and was in part responsible for, a shift in philosophical ethics away from the theoretical, from meta-ethics, towards the practical. Philosophers, of ethics and of science, became involved in consulting about the day to day decisions in hospitals and about the re-writing of health care policies. Philosophers of science are especially useful here because they actually know some of the science that is involved in making informed decisions, and they have often studied various aspects of decision making and the use of evidence.

This practical side of ethics in the sciences has other dimensions too. Codes of ethics for the various professions, e.g. engineers, have become "hot" topics for philosophical research. One of the more interesting and important new fields that philosophers of science dealing with values are involved in have to do with issues concerning how science is used to base regulatory decisions, e.g. concerning lead or dioxins or global warming. Also, there is work being done of the values that are implicitly or explicitly involved in the actual doing of scientific research. For example, what values are assumed in choosing a certain type of experimental paradigm, or, more generally, what values are assumed in giving more money to AIDS research rather than malaria (which is back with us in a big way.) The feminist movement of the late 1960s, also brought many value questions to the fore, and some excellent work has been done on how gender assumptions have influenced scientific practice.

This practical side of the "new" philosophy of science, I believe, derives from the same need for relevance that pushed other thinkers into dealing with the special sciences. There is an, often unacknowledged, awareness that philosophy must become important in ways that go beyond the hallowed halls of academe. The logical positivists, though some of them had studied physics, had little influence on the practice of physics, though their criteria for an ideal science and their models for explanations did have substantial influence on the social sciences as they tried to model themselves on physics, i.e. on "hard" science. The analytic philosophers of the mid-1950s onwards had little influence outside of the Universities in which they taught. They were content to defend their professional turf as being a thing unto itself and in some ways were quite proud to be "irrelevant" to the concerns of ordinary life, despite the ironic emphasis on ordinary language. By the 1980s, this intellectual isolationism had begun to break down, philosophers, and especially philosophers of science, had to get involved in the real world, the world of science.

I end this little essay by noting that the old questions and topics that had been raised by the logical positivists, and even in previous 2000 years, have not disappeared. Philosophers of science still puzzle over what makes a good explanation, what kind of evidence provides what kind of confirmation for theory, and what is the difference between science and pseudo-science. These are the perennial questions of philosophy of science. Today, we still try to answer them in specific ways that will have effects on science and the larger world. Philosophers of science have been instrumental in showing the non-scientific status of creationism and some versions of sociobiology and, now, evolutionary psychology. They have discussed fruitfully the role of scientific evidence in making decisions about nuclear energy plants or about levels of toxicity in our environment. They have asked hard questions about how to discover mechanisms such that finding them allows us to understand how systems of molecular biology or neuroscience work. And they have continued to elucidate and elaborate the unclarities and confusions in the special sciences.

Of course, there is much left to do. There are always more puzzles than people, more problems than solutions. The twentieth century saw many changes in what are taken to be the important puzzles and problems, but even more importantly, these same years have seen changes in how people need to be trained to approach problems and in what solutions to problems must look like. Maybe this past century has only taught us that there are no simple answers to truly complex questions. Yet, with this realization comes the awareness that there must be pragmatic answers provided in a timely and efficacious manner. Decisions must be made, and, hopefully, philosophy of science can help us to see how they may be made in better ways.

Note

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Appendix: Selected Relevant Philosophical and Scientific Publications (1895–1969), their dates, and a few events

1895	Josef Breuer and Sigmund Freud, *Studies in Hysteria*
1897	Leon Brunschvig, *La Modalité du Judgment*
1899	David Hilbert, *Die Grundlagen der Geometrie*
	Max Plank derives black body law
	Sigmund Freud, *The Interpretation of Dreams*
1901	Ernst Mach, *Die Mechanik in ihrer Entwicklung*, 4th edn.
1902	Lorentz proves Maxwell's equations were invariant under transformations
	Henri Poincaré, *La Science et l'Hypothèse*
1903	Bertrand Russell, *Principles of Mathematics*
1905	Ernst Mach, *Erkenntnis und Irrtum*,
	Bertrand Russell, "On Denoting" *Mind*
	Albert Einstein, "Zur Elektrodynamik bewegter Koeper" *Annalen der Physik*

	General strike and revolution in Russia
	Sigmund Freud, "Three essays on the Theory of Sexuality"
1906	Pierre Duhem, *La Theorie Physique. Son Objet. Sa Structure*
	Albert Einstein and Paul Ehrenfest, *hv* indivisible unit of energy
1907	Hans Hahn, Otto Neurath and Philipp Frank in Vienna
1908	Ernst Zermelo, "Untersuchungen uber die Grundlagen der Mengenlehre I" *Mathematische Annalen*
	Emile Meyerson, *Identite et Realite*
1910–13	Russell and A. N. Whitehead, *Principia Mathematica*
1911	Arthur Sommerfeld introduces phase-integral form of quantum law
	Einstein, "Uber den Einfluss der Schwerkraft auf die Ausbreitung des Lichtes" *Annalen der Physik*
	Solvay Congress, Brussels
1913	Edmund Husserl, *Ideen zu Einer reinen Phanomenologie und Phanomenologischen Philosophie, vol. 1*
	J. B. Watson, "Psychology as the Behaviorist sees it" *Psych. Rev.*
	Niels Bohr, publishes on the atom (*Phil. Mag.*)
1914	Russell, *Our Knowledge of the External World as a Field for Scientific Method in Philosophy*
	WWI (till 1918): Franz Ferdinand assassinated
	Easter Rising in Ireland
	Russian Revolution
1915	Sommerfeld explains fine structure of spectral lines
	Max Plank estimates value for *h* (*Phys. Rev.*)
1916	Einstein "Die Grundlage der allgemeinen Relativitatstheorie" *Annalen der Physik*
1917	Robert Millikan, *The Electron*
1918–19	Bertrand Russell, "Philosophy of Logical Atomism", *Monist* Moritz Schlick, *Allgemeine Erkenntnislehre*
	Arthur Eddington observes eclipse confirming general relativity
	Niels Bohr's "Principle of Correspondence"
1920	N. R. Campbell, *Physics, the Elements*
1921	Ludwig Wittgenstein, *Tractatus Logico-Philosophicus* [*Logische-Philosophische Abhandlung*] English version 1922
	J. M. Keynes, *A Treatise on Probability*
1922	Moritz Schlick to Vienna as professor of inductive sciences
	Leon Brunschvig, *L'Expérience Humaine e la Causalité Physique*
1923	David Hilbert, "Die Logische Grundlagen der Mathematik" *Math ematische Annalen*
	Helene Metzger. Les Doctrines Chimiques Début du XVIIème à la Fin du XVIIIème Siècle
1925	Erwin Schrödinger develops wave mechanics
1926	Rudolf Carnap to Vienna as instructor in philosophy
	Niels Bohr shows equivalence of matrix and wave mechanics

1927	Werner Heisenberg formulates indeterminacy principle
1928	Verein Ernst Mach (Ernst Mach society) founded
	Rudolf Carnap, *Der Logische Aufbau der Welt*
	David Hilbert, *Grundzuge der Theoretische Logik* (3rd edn. 1949 by Hilbert and Ackermann)
1927	P. W. Bridgman, *The Logic of Modern Physics*
	Charles Lindberg makes first solo transatlantic flight
1929	Carnap, Hahn and Neurath, *Wissenschaftliche Weltauffassung, Der Wiener Kris*
	Ernst Mach Society Congress held in Prague
	Wall Street Crash
1930	*Erkenntnis* founded (till 1940)
	Gödel's Completeness Theorem
1931	Carnap to Prague, Feigl to Iowa
	Gödel's Incompleteness Theorem
1932	E. A. Burtt, *The Metaphysical Foundations of Modern Science* (revised edn.)
1933	Hitler appointed Chancellor
1934	Carnap, *Logische Syntax der Sprache*
	M. R. Cohen and E. Nagel, *Introduction to Logic and Scientific Method*
	Gaston Bachelard, *Le Nouvel Esprit Scientifique*
	Philosophy of Science first published
	Hitler becomes *Führer* of Germany (till 1945)
1935	Karl Popper, *Logik der Forschung* (English, 1959)
	Kurt Koffka, *Principles of Gestalt Psychology*
1936	Carnap appointed at Chicago
	Alfred Tarski "Der Wahrheitsbegriff in den Formalisierten Sprachen" *Studia Philosophica*
	Carnap, "Testability and Meaning" *Philosophy of Science* (and 1937)
	A. J. Ayer, *Language, Truth and Logic*
	Spanish Civil War (to 1939)
1938	Ernst Mach Society formally dissolved (publications of the society forbidden in Germany)
	Waismann and Neurath to England
	Zilsel and Kaufmann to USA (Menger and Gödel already there too)
	Erkenntnis moved to The Hague, and renamed *Journal of Unified Science*
	Claude Shannon, "A Symbolic Analysis of Relay and Switching Circuits" *Trans. of Am. Inst. of Electrical Engineers*
	Alexandre Koyre, *Etudes Galileennes*
	B. F. Skinner, *The Behavior of Organisms*
	Hans Reichenbach, *Experience and Prediction*
	WWII (to 1945)

1940 *Journal of Unified Science* discontinued
Carl G. Hempel "Studies in the Logic of Confirmation I & II", *Mind*
Clark L. Hull, *The Principles of Behavior*
1947 Carnap, *Meaning and Necessity*
J. von Neumann and O. Morgenstern, *Theory of Games and Economic Behavior*
1948 C. G. Hempel and Paul Oppenheim, "Studies in the Logic of Explanation", *Philosophy of Science*
J. H. Woodger, *Biological Principles*
Norbert Wiener, *Cybernetics*
1949 H. Feigl and W. Sellars (eds.), *Readings in Philosophical Analysis*
Herbert Butterfield, *The Origins of Modern Science, 1300–1800*
Anneliese Maier, *Die Vorlaufer Galileis im 14 Jahrhundert*
Hans Reichenbach, *The Theory of Probability*
1951 Reichenbach, *The Rise of Scientific Philosophy*
1952 Carnap, *Logical Foundations of Probability*
Georges Canguilhem, *La Connaissance de la Vie*
1953 Wittgenstein, *Philosophical Investigations (Philosophische Untersuchungen)*
H. Feigl and M. Brodbeck (eds.), *Readings in Philosophy of Science*
W. V. O. Quine, *From a Logical Point of View*
Stephen Toulmin, *Philosophy of Science*
R. B. Braithwaite, *Scientific Explanation*
1954 Gustav Bergmann, *The Metaphysics of Logical Positivism*
A. R. Hall, *The Scientific Revolution, 1500–1800*
Nelson Goodman, *Fact, Fiction, and Forecast*
Leonard J. Savage, *The Foundations of Statistics*
1955 Canguilhem succeeds Gaston Bachelard as Professor of Philosophy at the Sorbonne and Directeur of Institut d'Histoire des Sciences et des Techniques
1956 Ernest Nagel, *Logic without Metaphysics*
J. O. Urmson, *Philosophical Analysis*
Herbert Feigl and Michael Scriven, *Minnesota Studies in the Philosophy of Science, Vol. 1*
1958 Norwood Russell Hanson, *Patterns of Discovery*
Marshall Clagett, *The Science of Mechanics in the Middle Ages*
E. H. Gombrich, *Art and Illusion: A Study in the Psychology of Pictorial Representation*
M. Clagett (ed.), *Critical Problems in the History of Science*
Paul Feyerabend, "An Attempt at a Realistic Interpretation of Experience" *Proc. Aristotelian Society*
1959 Morton Beckner, *The Biological Way of Thought*

1960	W. V. O. Quine, *Word and Object*
1961	Ernest Nagel, *The Structure of Science*
1962	Thomas Kuhn, *The Structure of Scientific Revolutions*
	Mary Hesse, *Models and Analogies in Science*
	Israel Scheffler, *The Anatomy of Scientific Inquiry*
	Robert G. Colodny, *Frontiers of Science and Philosophy* (first volume of the Pittsburgh series)
1965	Hempel, *Aspects of Scientific Explanation*
	Paul Feyerabend, "Problems of Empiricism" in R. G. Colodny (ed.), *Beyond the Edge of Certainty*
	Michel Foucault, *Les Mots et les Choses*
1968	Imre Lakatos, "Criticism and the Methodology of Scientific Research Programmes"
	W. V. O. Quine, "Epistemology Naturalized" lecture delivered (published 1969)
1969	Foucault, *L'Archeolgie du Savoir*

Further reading

Contemporary presentations of the basic issues in philosophy of science

Merrilee Salmon, et al., *Philosophy of Science*, (by the Department of History & Philosophy of Science, University of Pittsburgh), Prentice-Hall, 1991

A collection of readings which cover the field of philosophy of science

Baruch Brody and Richard Grandy (eds.), *Readings in the Philosophy of Science*, 2nd edn, Prentice Hall, 1989

Historical overviews of the history of positivism

J. Alberto Coffa, *The Semantic Tradition from Kant to Carnap*, Cambridge: CUP, 1991

Michael Friedman, *Reconsidering Logical Positivism*, Cambridge CUP, 1999

Frederick Suppe, Critical Introduction, to *The Structure of Scientific Theories, 2nd edn*, Urbane, Ill.: University of Illinois Press, 1977

A systematic treatment of the main parts of the logical positivist/empiricist program: Quite difficult in parts

Israel Scheffler, *The Anatomy of Inquiry*, New York: Borzoi Books, 1964

A review of the critics of positivist/empricist program.

Israel Scheffler, *Science and Subjectivity*, Indianapolis. Bobbs Merrill, 1967