Introduction: Kuhn's insight

In *The Structure of Scientific Revolutions* Kuhn developed a novel and interesting account of the dynamics of scientific change, one that was deeply at odds with the assumptions that had previously informed the outlook of philosophers of science. To many of his readers it seemed that whenever Kuhn denied a widely accepted philosophical assumption about science, he offered a paradox in its place.

To begin with, Kuhn alleged that scientific knowledge was not cumulative. He is famous for drawing our attention to what has come to be called "Kuhn-loss," the "knowledge" allegedly lost when one theory replaces another. Yet he adamantly insisted that there is scientific progress.

He also claimed that observational data could not provide a foundation for scientific knowledge. Instead, he insisted that data are pliable and thus scientists could not unequivocally settle disputes by appealing to data. Yet he emphasized the importance of scientists' work on relatively small, manageable, esoteric problems, which seemed to treat data as capable of disclosing unequivocal answers to the questions driving research. These problems he called the puzzles of normal science. Indeed, in the context of normal science, as Kuhn describes things, the data seem to have an almost veto power. Rather than posing a threat to the theory assumed in research, discrepancies between expectations and results show the incompetence of the scientist.

He also claimed that scientists were not especially open-minded or critical, as Karl Popper claimed. In fact, Kuhn claimed that scientists are remarkably uncritical with respect to the accepted theories. Further, he suggested that the education of scientists was dogmatic, never inviting the student to question the accepted theory. And scientific inquiry, he claimed, was tradition-bound.

No wonder *Structure* was met with fierce criticism. Kuhn was giving us an account of science very different from the positivists' account. It seemed that he was denying every assumption that the positivists made about science.

Kuhn was not alone in challenging the received view. He was part of a new wave in philosophy of science, the historical school. Members of this school believed that philosophers could benefit greatly from examining the history of science. A study of the history of science, they thought, would disclose the way in which scientific inquiry *really* worked. The historical school did not question the epistemic authority of science and scientists. Rather, those who subscribed to this view sought to understand science as it was really practiced. They were not interested in a rational reconstruction or idealization of science.

Initially, the work of the historical school was greeted with enthusiasm, as Kuhn and others working in a similar vein drew attention to the discrepancies between the idealized picture of science that philosophers had been working with and the real world of science, as gleaned from an examination of the history of science. He and the others, drawing on the history of science, promised to enrich our understanding of science.

In developing his own view of science, Kuhn invoked a variety of engaging metaphors that seemed to underscore the inadequacy of traditional philosophical accounts of science. Changes of theory were described as scientific *revolutions*, comparable, in some respects, to political revolutions. They were very unsettling events that required radical breaks with the past. Scientists involved in such events were alleged to undergo something like a conversion experience, much like a religious conversion, an experience that seemed to admit of no rational defense. And, science moved from one paradigm to another. Kuhn likened this move to a gestalt shift, thus raising questions about the relationship between the world and our theories. Kuhn even compared the change scientists underwent when they accepted a new theory to a world change. That is, after a scientific revolution, scientists not only work with a new theory, they seem to work in a *new world*. These metaphors and comparisons were deeply unsettling to many philosophers, even if at the same time they were liberating and promised to offer new insights into science.

The publication of *Structure* quite quickly altered both the philosophy of science and the sociology of science profoundly. Both fields were set in new directions.

Sociology of science, and its successor project, science studies, became more involved in investigating the cognitive dimensions of science than ever before. Prior to the publication of *Structure*, sociologists of science studied the institutional structure of science and the impact of external factors on science, like developments in commerce and trade. After *Structure*, though, sociologists started to examine how social factors

affected the outcome of scientific disputes, determining the way in which disputes were resolved. These sorts of investigations were perceived by most philosophers as unwelcome and threatening intrusions into the traditional domain of philosophy. And they were met with fierce resistance. Because Kuhn was regarded by many sociologists as a source of inspiration, many philosophers held him responsible for encouraging these new developments in the sociology of science.

Even before these developments in the sociology of science, however, philosophers of science were critical of Kuhn's work. The tone was set early by Popper and his fellow Popperians, when Popper and Kuhn engaged each other at the 1965 London International Colloquium in the Philosophy of Science. The Popperians were most disturbed by Kuhn's account of normal science. Popper had emphasized the critical attitude of science, the readiness to subject any belief to empirical testing. Kuhn, on the other hand, described the normal research activities of scientists as dogmatic. Scientists, according to Kuhn, looked at the world uncritically, unreflectively employing the concepts of accepted theories. Moreover, science education was described as a process that made scientists myopic, often even unable to see evidence contrary to their theoretical expectations. Indeed, one might wonder how on Kuhn's account a change of theory was even possible.

This dimension of Kuhn's view, in combination with his unsettling remarks about the apparently non-rational process that leads to a change in theory, led to the development of a very negative reading of Kuhn. According to this reading, Kuhn's account of science and scientific change threaten the rationality of science. If scientists really are in the grip of the accepted theory to the extent that Kuhn implies, and it takes something like a religious conversion to set them free, it is hard to see how theory change could be a rational process. Many found it very difficult to reconcile Kuhn's picture of science with the accepted view of science as critical inquiry, an enemy of dogmatism, and driven by a healthy, skeptical attitude. Consequently, many thought that Kuhn's account of science was deeply mistaken.

It was not only Kuhn who was mistaken. A generation of sociologists was under his spell, extending his ideas in ways that even Kuhn found distressing. Indeed, by the mid 1980s, Larry Laudan felt the need to write a book aimed at saving us from the Kuhnians, showing why their view of science is deeply mistaken (see Laudan 1984). By the time Laudan published his book, though, it was not Kuhn's own view that was the real object of concern. Rather, it was a particular reading of Kuhn, one influenced substantially by developments in the sociology of science.

In much of his later work Kuhn tried to correct some of the misunderstandings of his position. In his attempts to defend his views from criticism, Kuhn tried to clarify his account, modifying and developing it along the way. But unfortunately many of these developments went unnoticed by his critics and commentators. To a large extent, philosophers of science seemed content to accept the existence of a standard Kuhnian position, a threatening but ultimately indefensible position against which they would define their own positions. This attitude, I believe, is quite unfortunate as Kuhn's developed position is thoughtful, offering important insights into the nature of scientific change and scientific knowledge. Indeed, Kuhn also offers important insight into how we should study science as philosophers.

My aim in this book is to make a case for taking Kuhn's developed view seriously. Kuhn offers us a framework for developing an epistemology of science. Given the social nature of scientific inquiry, Kuhn believed that an epistemology of science needs to be a *social* epistemology. He also believed that an epistemology of science needs to be an *evolutionary* epistemology. Both "social epistemology" and "evolutionary epistemology" are labels that pick out a wide range of projects. I aim to clarify the nature of Kuhn's approach to epistemology, outlining the respects in which it is an evolutionary epistemology and those in which it is a social epistemology.

None of the existing books about Kuhn's philosophy of science give adequate attention to the social dimensions of scientific inquiry. Nor have they given much attention to research in the sociology of science. Such research, I argue, is extremely relevant to advancing the goals of epistemologists of science. Moreover, none of the existing books take account of Kuhn's attempt to develop an evolutionary epistemology. My aim is to address these shortcomings.

It should be noted that the continuity between Kuhn's later work and the view he developed in *Structure* is quite extensive. Thus, though in developing his view Kuhn revised his views in significant ways, he was motivated, to a large extent, by the desire to clarify what he was trying to say in the early 1960s. Indeed, some of the developments in his later work are best described as extensions of the project that he began with *Structure*.

OVERVIEW

My aims in this book are: (1) to clarify the nature of Kuhn's epistemology of science, (2) to offer a defense of his epistemology, and (3) to clarify the relationship between Kuhn's views and recent work in sociology of science

and science studies. Kuhn's view is too often mistakenly characterized as an unacceptable form of constructionism or relativism. Motivating my study is a concern to show that Kuhn has a positive legacy to offer philosophers of science, a constructive and insightful framework for developing an epistemology of science. Moreover, I aim to show that philosophers cannot afford to be dismissive about sociology of science. Given the social nature of scientific inquiry, sociological studies of science will play a key role in developing an adequate descriptive account of scientific inquiry and change.

Kuhn continued to develop his epistemology of science until the end of his life. Many of the later developments in his view, however, have been neglected by philosophers, who have tended to focus on the view articulated in *Structure*. This is unfortunate, as his developed view clarifies the nature of revolutionary changes in theory, one of the most contentious parts of his position as presented in *Structure*. Most significantly, he replaces the highly criticized notion of a paradigm change with the notion of a taxonomic or lexical change. I aim to show how such changes are both radical and yet rationally defensible.

Further, Kuhn develops an account of the process that leads to the creation of new scientific specialties, a topic that has been largely neglected by philosophers, though discussed extensively by sociologists and historians of science. Central to Kuhn's account of specialization is a radical understanding of the end or goal of scientific inquiry. Traditionally, philosophers have uncritically assumed that truth is the end of inquiry, and the success of science is best explained in terms of the pursuit of this goal. Kuhn, on the other hand, suggests that science is better conceived as developing through a process of increasing specialization. This dimension of Kuhn's project has been largely overlooked, in large part because he never presented his views on specialization systematically. Moreover, specialization has typically not been a topic of concern to philosophers of science. I aim to provide a clear and systematic presentation of Kuhn's account of specialization. Further, I aim to articulate the philosophical relevance of Kuhn's account of scientific specialization, showing how the process of specialty formation is driven by cognitive or epistemic considerations. In this respect, Kuhn's account of specialization differs significantly from sociological accounts, which tend to emphasize the social dimensions of the change, and downplay the epistemic dimensions. In fact, Kuhn came to believe that specialization is one of the means by which scientists are able to develop an increasingly accurate and comprehensive understanding of the world.

Unlike some other philosophers writing on Kuhn, I aim to critically analyze the relation between Kuhn's view and sociological studies of science. *Structure* had a profound impact on the sociology of science. But the directions in which sociology of science developed has created a rift between philosophy of science and sociology of science, and Kuhn is often thought to be partly responsible for this state of affairs. On the one hand, I aim to show how Kuhn's view differs from many of the sociological studies of science that were inspired by his work. Consequently, I argue, Kuhn has been unfairly criticized as a social constructionist. I believe that Kuhn is nonetheless a constructionist of sorts, though we need to take some care in distinguishing the form of constructionism he endorses from other untenable forms. On the other hand, I aim to show that given Kuhn's conception of the epistemology of science, and especially his view that the loci of theory changes are research communities, philosophers will have to either work with sociologists of science or draw on research in the sociology of science. This will enable philosophers to develop a richer descriptive account of scientific change. It is unfortunate that Kuhn never systematically articulated the relationship between his view and the views of contemporary relativist sociologists of science.

This book is in three parts.

The first part is titled "Revolutions, paradigms, and incommensurability." In it I re-examine some of the most important and contentious concepts that Kuhn employed in *Structure* with the aim of clarifying how his view developed with respect to these concepts. Though now widely used in philosophy of science, these concepts are often used in ways very different from the ways in which Kuhn used them or intended them to be used.

I begin with Kuhn's modified account of scientific revolutions, developed in the later part of his career. Originally, in *Structure*, Kuhn characterized scientific revolutions as paradigm changes. But because of the variety of meanings "paradigm" had in *Structure*, the notion of a paradigm change led to many misunderstandings and much criticism. Later, in an effort to correct misunderstandings and address his critics, Kuhn came to characterize scientific revolutions as involving taxonomic or lexical changes, a reordering of the relationships between concepts in a theory. I defend Kuhn's revised account of scientific revolutions against a series of common criticisms.

I also examine in detail the Copernican revolution in early modern astronomy to illustrate the explanatory power of Kuhn's account. Kuhn's own book-length treatment of this episode in the history of science was

published before he published *Structure*, and thus before he had worked out the details of his account of scientific change. This episode in the history of science has also been the subject of much debate, and the historical scholarship on the topic has developed extensively since Kuhn published *The Copernican Revolution*. Consequently, it is worth re-examining this episode in the history of science with the aid of Kuhn's developed account of scientific change.

In developing his view on revolutions, Kuhn did not completely discard the notion of a paradigm. As a result, I will be clarifying the role that paradigms play in Kuhn's developed philosophy of science. Because theory change is no longer characterized as paradigm change, one might be led to think that paradigms have little significance in his developed view. This is not so. Paradigms still function as the widely recognized concrete scientific achievements that are used as models for solving hitherto unsolved problems in a field. They are also the means by which young aspiring scientists learn the norms, standards, practices, concepts, and theories in their field. Hence, paradigms play an essential role in the socialization of young scientists. Further, I argue that Kuhn's discovery of the concept "paradigm" exemplifies the complex process of discovery in science. Hence, as odd as it may sound, by the time he wrote *Structure*, Kuhn had not yet discovered what a paradigm was.

Part I ends with an examination of the role of incommensurability in science. I distinguish the various ways in which Kuhn used the term "incommensurable" and identify the epistemic significance of each type of incommensurability. Incommensurability is often thought to pose a significant threat to the rationality of theory change. If two theories are not even comparable, it is difficult to understand how scientists are able to reach a rational judgment about which of the theories is superior from an epistemic point of view. Initially, in *Structure*, Kuhn appealed to the concept of incommensurability in order to capture the fact that scientists lack a common measure by which to evaluate competing theories. This is why revolutionary changes can be such protracted affairs. But in his efforts to address his critics, Kuhn talked more and more about "meaning incommensurability," the fact that a single term, like "mass" for example, has a different meaning in competing theories. Kuhn also came to describe the lexicons of neighboring fields as incommensurable. In fact, he came to believe that the incommensurability of the lexicons of neighboring scientific specialties plays an important role in isolating scientists, and thus allowing them to develop concepts appropriate to the phenomena they study. Although meaning-incommensurability has attracted the most

attention from philosophers writing about incommensurability, I believe that it has less epistemic significance than the two other forms of incommensurability described here.

The second part of this book is titled "Kuhn's evolutionary epistemology." In it I examine the aspects of Kuhn's epistemology of science that make it an evolutionary epistemology.

The popularity of and enthusiasm for evolutionary epistemologies has waxed and waned over the last five decades. And there is hardly a uniform understanding about what makes an epistemology an evolutionary epistemology. I aim to clarify the senses in which Kuhn's epistemology of science is aptly described as an evolutionary epistemology. In addition, I aim to show that his evolutionary perspective on science is an important resource for developing an adequate epistemology of science. His evolutionary perspective, though, profoundly alters the way we see science. Indeed, I believe it is the magnitude of the changes caused by this change in perspective that has led to so many misunderstandings of Kuhn's view.

Kuhn is widely recognized as one of the pioneers of the historical school in philosophy of science, a group that aimed to look to the history of science as a source of data for developing a philosophy of science. Such an approach to the study of science was meant to lead to a more accurate account of science, in contrast to the idealizations that emerged from the rational reconstructions of his predecessors. But Kuhn changed his mind about the relevance of the history of science to the philosophy of science. He came to believe that the key lesson philosophers must learn from history is a particular perspective, a developmental or historical or evolutionary perspective.

Traditionally, philosophers have assumed that science aims for the truth, that is, to mirror a reality that is indifferent and essentially unchanging. Moreover, traditionally, it is assumed that the history of science is marked by a steady accumulation of knowledge, often aided by the development of unifying theories, theories that bring together disparate phenomena under a set of laws. Kuhn challenged this traditional picture of science in a variety of ways. First, in *Structure* Kuhn suggested that science is best seen as moved from behind, rather than aiming at some goal set by nature in advance. He compares scientific change to evolutionary change by natural selection. According to Darwin, the process of biological change is not teleological. This was Darwin's most radical innovation. Similarly, Kuhn maintains that science is not aptly described as moving toward a fixed goal, set by nature in advance. Instead, scientists

are moved by research agendas set by their predecessors, and they work with instruments and theories developed by their predecessors. I defend Kuhn's view. I argue, in addition, that scientists must even determine what phenomena their theories aim to account for. In this respect, the target at which scientists aim in their efforts to develop theories is not predetermined.

Once we adopt the developmental perspective that Kuhn recommends, we realize that scientists are always working within research traditions, working from sets of beliefs inherited from their predecessors. Moreover, their evaluations of theories are comparative, for they are unable to compare their theories directly with a mind-independent reality. Further, the increasing predictive accuracy achieved in mature fields is not to be explained by citing the (alleged) fact that we are getting increasingly closer to the truth. Rather, our success in science is better explained as a consequence of the increasing specialization in science. As new specialties are formed, scientists can develop instruments, practices, and concepts suited to a narrower range of phenomena. The result is an increase in predictive power.

I also compare Kuhn's account of specialty formation with sociological and historical accounts of the process. The accounts of specialization developed by historians and sociologists of science tend to privilege the social dimension of the change that occurs, and treat the conceptual changes as derivative. Kuhn, on the other hand, gives a privileged place to the conceptual dimension of the developments of a new specialty. Given Kuhn's account of specialization, the process that leads to the creation of new specialties is of great importance to philosophers of science interested in the epistemic dimensions of science. I examine two case studies to illustrate Kuhn's account: the formation of endocrinology as a field and the formation of virology as a field of research.

Kuhn's account of specialization is important for three reasons: (1) it provides insight into the often overlooked cognitive or epistemic dimensions of the process; (2) it makes clear why specialization is relevant to philosophers of science, and not just sociologists and historians of science; and (3) it is an aspect of Kuhn's developed account of scientific change that is still either largely neglected or misunderstood. Specialization, I argue, will prove to be an important topic in developing a richer understanding of both scientific inquiry and scientific knowledge.

The third and final part of the book is titled "Kuhn's social epistemology." Here I examine the respects in which Kuhn's epistemology of science is aptly described as a social epistemology. I also provide some

direction as to what we need to do next as we seek to develop a Kuhnian social epistemology of science.

I begin by examining the charge that Kuhn is a social constructionist. Though sociologists of science often enthusiastically accept such a label, indeed, even self-consciously describe themselves as constructionists, philosophers of science are averse to being called constructionists. In philosophical circles the term still carries connotations of relativism and irrationalism. In addition, for philosophers of science constructionism also connotes a commitment to externalism and nominalism. Further, because the label "constructionist" is used in a variety of ways, it is far from clear what is meant when someone is labeled a constructionist.

I aim to clarify the relationship between Kuhn's epistemology and constructionism. Contrary to what some of Kuhn's critics claim, I argue that Kuhn is an internalist, believing that changes in theory are ultimately caused by a consideration of epistemic factors, not external factors. Kuhn does in fact attribute a significant role to subjective factors in theory change, arguing that such factors are responsible for ensuring that there is an efficient division of labor and competing theories are developed. It is only when competing theories are developed that the epistemic merits and shortcomings of the theories emerge. And only when the epistemic merits and shortcomings of competing theories are revealed can a rational choice be made between competing theories. I also argue that Kuhn is not a radical nominalist. Kuhn does not believe that there are *no* constraints imposed by the world on how a successful theory groups things in the world. In fact, he is quite insistent that the mind-independent world imposes constraints that are irreconcilable with some hypotheses. Still, contrary to what many philosophers and scientists claim, Kuhn does not believe that there is a single ultimate way our theories need to group things. My analysis of Kuhn's constructionism is meant to clarify the relationship between his view and popular contemporary sociological views of science, with special attention to his relationship to the views of the Strong Programme.

I then examine the ways in which Kuhn's epistemology of science is aptly described as a social epistemology. Most importantly, Kuhn regards the research community or specialty as the locus of change in science. A change of theory is not effected merely by a change in the view accepted by a scientist. Rather, a change of theory is a change in the research community. Thus for Kuhn theory change is a form of social change. This is evident from the way Kuhn characterizes the development of a mature field, from a stage of normal science, to a crisis, to a revolution resulting