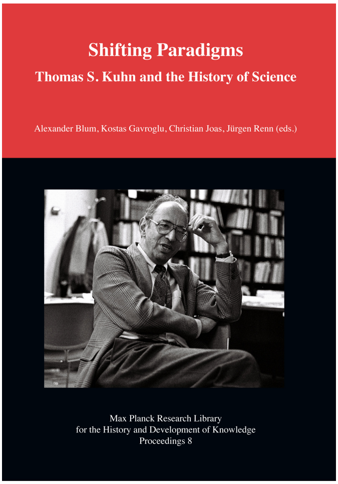
Max Planck Research Library for the History and Development of Knowledge
Proceedings 8

Skúli Sigurdsson:

The Nature of Scientific Knowledge: An Interview with Thomas S. Kuhn



In: Alexander Blum, Kostas Gavroglu, Christian Joas and Jürgen Renn (eds.): *Shifting Paradigms : Thomas S. Kuhn and the History of Science*
Online version at http://edition-open-access.de/proceedings/8/

ISBN 978-3-945561-11-9
First published 2016 by Edition Open Access, Max Planck Institute for the History of Science under

Printed and distributed by:
Neopubli GmbH, Berlin
http://www.epubli.de/shop/buch/50013

The Deutsche Nationalbibliothek lists this publication in the Deutsche Nationalbibliografie; detailed bibliographic data are available in the Internet at http://dnb.d-nb.de

Chapter 1
The Nature of Scientific Knowledge: An Interview with Thomas S. Kuhn

Skúli Sigurdsson

The following interview was published in *Harvard Science Review* (winter 1990) [pp. 18–25] and conducted by Skúli Sigurdsson who at the time was a graduate student in history of science at Harvard University.[1] We publish the original copy by courtesy of the *HSR*. The interview tape was transcribed by Katrin Chua (then editor of *HSR*). The photographs of Kuhn accompanying the text were made by Skúli Sigurdsson and chosen by Kuhn himself (from a whole 36-exposures film). An abridged version of the interview appeared in Persian translation by Elaheh Kheirandish in *Science Policy Quarterly* (Teheran), no. 3 (winter 1993).[2]

Interview

[**p. 18**] *Thomas S. Kuhn, professor of philosophy at MIT, is among the most influential figures in the study of the history of science. He is perhaps best known for his theories on the historical growth of scientific knowledge, which proceeds in what he calls conceptual 'revolutions' or 'gestalt switches.' In this interview, Kuhn discusses the origins of those theories, prominent reactions to them, and their implications for scientific truth.*

HSR: When you were an undergraduate at Harvard, what was it in the sciences that fascinated you and other students of your generation? What made you choose physics in particular? And do you think these motivations have changed over the years?

Kuhn: I came to Harvard in the fall of 1940, terribly proud of having gotten in, only to discover later that I had been one of, say, 1000 students admitted, out of something like 1095 eligible applicants. Yes, situations have changed since those times! But there's a story that will speak to your question about changes in attitude that have arisen since the summer of 1940. I wanted to major in mathematics or

[1] Ph.D. 1991 / dissertation: "Hermann Weyl, Mathematics and Physics, 1900–1927."

[2] Skúli Sigurdsson thanks Nina Ruge for invaluable editorial help in the summer of 2014.

physics, simply because I had enjoyed them and been good at them. I came from a mathematical background, a theoretical outlook. I had taken both chemistry and physics in high school from a man who taught both. But while he knew the chemistry much better, I caught onto the physics. I remember suggesting consequences of what he taught us about the theory of heat, and he told me I was trying to fly before I could walk. But that theoretical turn of mind—theoretical, ontological, cosmological, what you will, but an interest in fundamental problems; that was what drew me to mathematics and physics initially.

Figure 1.1: Thomas S. Kuhn being interviewed November 1989 in his office at MIT; photographer: Skúli Sigurdsson; picture: 11.

So in the summer before I came to Harvard, I talked at length with my father about which of the two I should choose. And I have never forgotten what he said to me, because nobody would say it now. "If you have a strong preference for mathematics," he said, "then I certainly [**p. 19**] think that is what you should follow. But if you don't, perhaps it would be better to major in physics, because in mathematics, if you don't make one of the good universities, the only things to do are to be an insurance company actuary or a high school teacher; whereas in physics, I think there are a few other opportunities. Bell Laboratory and General Electric are very interesting places, and then there are some government positions, like the Bureau of Standards, or the Naval Research Laboratory." As I didn't have a strong preference, I majored in physics.

I don't think I need to comment on the sense in which the situation has changed since that time. And clearly, it's changed in motivation as well. It isn't that you don't have to like physics, or that most people don't, but you don't think that you're giving up a great deal today in order to pursue it. I didn't think I was giving up a great deal either, but the notion that physics was an area of expanding career opportunities was not one people had. There was a *New Yorker* article that appeared after WWII called "Farewell to String and Sealing Wax," in which Sam Goudsmit talked about the enormous changes arising from the institutionalization of physics. That sense of a string and sealing wax career in physics was not unrepresentative of the sort of thing we had at the time.

HSR: How did these changes affect your own studies?

Kuhn: Freshman year was 1940. There was a war on in Europe. Sophomore year, there was Pearl Harbor, and at that point, anybody in physics at Harvard was urged to concentrate in electronics, so as to prepare to help the war effort. So I took a lot of electronics at the expense of physics, and much less liberal arts than I would have liked. For physics and math were by no means the only subjects I liked, and I also had a considerable interest in literature.

I did my best to pursue those interests with some literature courses, and one very important course in philosophy—important, that is, in my own development. I was an editor of the *Crimson*, a member of an undergraduate literary society, that sort of thing. So there were real conflicts. I had the not uncommon problem of being reasonably good at and interested in things that went off on opposite directions.

Now I'm sure you're going to ask me at some point how I got out of physics, and one of the factors was that my interests had always been somewhat torn. But there's certainly much more to the story. After graduating, I wound up working at the Radio Research Laboratory, doing radar counter measures out of the top of the biology building. After about a year's work there, I went overseas to our advanced European Base in England, and there, worked mostly with the air force. We worked on technical intelligence problems, trying to learn about German radar installations, with an eye, of course, to jamming them; and on installing various sorts of equipment in aircraft. When I returned to Cambridge in the summer of '45, things were over in Europe, but not yet over in Japan, and I was uncertain whether I was going to be sent off to the Pacific to do the same sort of thing.

Those experiences were also part of the reason that my feelings towards physics as a career were gradually changing; I didn't find my war science terribly interesting. It's not out of the question that had I gone to Los Alamos as some of my contemporaries at Harvard did, and been working in that environment, I might never have left the field. I suspect I would have, and certainly have no regrets about having done it, but there was something in the fact that I found the sort of work I was doing something of a drag.

Figure 1.2: Thomas S. Kuhn being interviewed November 1989 in his office at MIT; photographer: Skúli Sigurdsson; picture: 23.

Then came a fortuitous situation—when I finally heard that I wouldn't be going to Japan, the fall semester was just about starting at Harvard, and there I was. So I went on and took my degree in physics. But increasingly as I continued my work, I wondered whether a physics career was what I really wanted. I was very conscious of [**p. 20**] the narrowing, the specialization required, and though I had no conclusion on that score, I was beginning to look for alternatives. No one of those seemed more attractive than the rest, until all of a sudden I was asked to assist President [James B.] Conant in teaching an experimental General Education course on the history of science, through readings of case histories. It sounded like a pretty good idea; it would be a good experience, a chance to work with the President of Harvard, and also my first exposure to history of science. So I grabbed the opportunity and found it fascinating.

At our first meeting, Conant turned to me and said "I can't imagine a General Education course in science that doesn't have something about mechanics in it. But I'm a chemist, I can't *imagine* how to do that! You're a physicist, go find out!" So I went out to learn something about the history of mechanics, and it rapidly became clear that if it was going to be a case history, it would have to be built around Galileo, since Newton would have been far too complicated. And to do that, I would have to learn something about what people had believed before Galileo. So I wound up looking at a series of monographs by Alexandre Koyré, called *Etudes galiléennes* [1939], and I started to read Aristotle's *Physics*. And the experience was enlightening.

What Aristotle could be saying baffled me at first, until—and I remember the point vividly—I suddenly broke in and found a way to understand it, a way which made Aristotle's philosophy make sense. It was that case history, and others, that in some sense first got me onto the idea of gestalt switches and changes in conceptual frameworks, which was to show up in the *Structure of Scientific Revolutions* in 1962.

I had this long-standing interest in philosophy. I had been reading a lot of elementary philosophy of science during the war—[Bertrand] Russell, [Philipp] Frank, and [Percy W.] Bridgman, though unfortunately not much [Rudolf] Carnap. And I also was mulling over certain ideas about scientific method that I'd happened upon while being trained in the sciences. There are certain implications about what historical growth of knowledge is that I felt deserved greater consideration. So this project seemed important, worth working on, and something that might be just the thing to take as an alternative to physics. And that's the story of how I got into physics, and how I got out of it.

HSR: In the *Structure of Scientific Revolutions*, you discuss the notion of conceptual changes in the development of scientific knowledge. As you've mentioned, it first arose during your struggle with Aristotle's *Physics*. What, specifically, did this understanding amount to? In what ways, perhaps, is it more than simply making a translation?

Kuhn: What I discovered in studying Aristotle was that a text required interpretation. And by interpretation I mean something similar to what was then quite well known in Europe (although I didn't know it at the time) as *hermeneutics*, but without all the claims of hermeneutics as a way to Truth. It was a way of reading texts, of looking for things that don't quite fit, puzzling over them, and then suddenly finding a way of sorting out the pieces. I had never heard of interpretation in that sense, for I'd never read any continental philosophy. But in reading Aristotle, I began to see what sort of physics this had been, and why it had been taken so seriously, which had not been in the least visible to me before. What I discovered was *not* the fact that you could translate, but rather *that you couldn't*. You can teach Aristotle, but you have to teach some part of his vocabulary in order to do it, and there's no way you can put that vocabulary in its entirety into the vocabulary you had when you came to the text in the first place. So it was untranslatability, rather than translatability that I increasingly saw in studying the history of science.

HSR: Since you published the *Structure of Scientific Revolutions*, there has been widespread reaction to it. In retrospect, what surprises you most at the responses? How do you see some of the misinterpretations of the book as being related to specific problems within philosophy or history of science?

Kuhn: I would first distinguish between philosophy of science and history of science. Mine was a historical approach. But what I thought was important in looking at [**p. 21**] the history was the notion of a revolution, the sort of rupture that the gestalt switch was intended to represent. I was talking about the non-cumulativeness of the development of knowledge, the problem with bringing an older science to the bar of judgment of a later one; about the inappropriateness of speaking of Aristotle as simply having made a mistake when he spoke of heavy bodies as falling faster than light bodies; about the sort of vocabulary I objected to, which took Aristotle as being merely false, the abhorrence of a vacuum as merely a mistake. I found something wrong with the standard way of grinding clearly bright and influential historical figures in the meat grinder of the categories or laws of a later science. These notions were not going to strike people who came to history primarily as historians, and philosophers were certainly going to have a lot to say about the issue.

Of the things that surprised me tremendously in the reactions to *Structure*, a major one was the talk about irrationality, for that was something that had never occured to me. I didn't know how the word 'rationality' functioned in philosophy of science. And so the notion that I was showing the irrationality of science absolutely blew my mind. I did spend substantial time and rhetoric in *Structure* discussing the quite different notion that when people talk about proof in the sciences, it isn't like proof in mathematics; that the former has none of the latter's force of compulsion. I was *not* saying, however, that there aren't good reasons in scientific proofs, good but never conclusive reasons. In formal mathematics, if two people disagree about this being a correct proof, we can take them through it one step at a time, and one of them can be forced to acknowledge the other side. There's just nothing like that in the sciences. That was what I was trying to say in *Structure*. So I was surprised at the extent of the reaction to it as a charge of science's irrationality.

I found something wrong with the standard way of grinding clearly bright and influential historical figures in the meat grinder of the categories or laws of a later science.

I was also surprised at the relativism charge. Not that I didn't see why it was made, but it seemed to me that if relativism was what my thoughts amounted to, it was not nearly so damaging as the sort of relativism it was being taken to be. And it wasn't clear to me that relativism was the right word to be used at all. Essentially, I drew a Darwinian parallel in the first edition of *Structure*, to remind people that getting a better and better instrument (the hand and the eye were standard examples) does not require a process aimed at a pre-existent goal. Evolution isn't guided towards some preconceived perfect form, and I was arguing that science wasn't either. Now while it's clear why Darwin and the notion of evolution upset people, it wasn't clear at all that relativism was the proper charge to level.

HSR: This talk of irrationality became prevalent as the sixties drew on, against the backdrop of much criticism in American society of the Vietnam War. How do you see some of the responses to your book in light of these larger social movements and the criticism initiated by them?

Kuhn: I'm sure that part of the reason the book attracted the sort of attention that it did, particularly among people who were under thirty in the sixties, was for exactly those reasons. It could be used, and was used as a whip with which to beat the sciences. I am told that [Herbert] Marcuse and Kuhn were the heroes on the campus of San Francisco State. After all, that was my second book with the word "Revolution" in the title! I'm sure that *part* of what went on was due to those trends, and I had a number of relatively radical students who came along hoping that I would inculcate the new revolution or something, which I didn't do.

> *Evolution isn't guided towards some pre-conceived perfect form and I was arguing that science wasn't either. Now while it's clear why Darwin and the notion of evolution upset people, it wasn't clear at all that relativism was the proper charge to level.*

I discovered that students who had been attracted to history of science because of this book didn't have a clue, and on the whole neither did my colleagues, as to where this book had come from. I taught people how to read texts, trying to replicate my experience with Aristotle. The people who *did* discover what I thought *Structure* was about were those who took graduate seminars with me, in which we read Kelvin or Maxwell or Galileo or whoever, [**p. 22**] closely, and tried to figure out how those people could ever have said the sorts of things they said. That's always been for me the central part of that book, and of course it scarcely shows. We asked, "Why would he say that?" We found things that didn't make sense, and tried to find a way of reading that would make it make sense. For it is only at that point that a text you thought you understood takes on a somewhat different significance.

> *I am told that [Herbert] Marcuse and Kuhn were the heroes on the campus of San Francisco State. After all, that was my second book with the word 'Revolution' in the title!*

HSR: How would you say your notion of *revolution* differs from more common connotations of the word, in particular, with respect to whether in studying the history of science, our aim is to deconstruct and undermine the basis of science's validity, or rather to reconstruct those foundations?

Kuhn: I was not trying to deconstruct science. I'm still not trying to deconstruct science. I'm not all that sure I understand what deconstruction is. But there's an important element that persists in me that Dr. Johnson's argument against Berkeley was right—that you can refute the person who doesn't believe in material bodies by kicking the stone. Experiment and observation really do play an absolutely crucial role in the development of the sciences. There are many things to be said about the nature of progress in the sciences; the thing that you *cannot* I think say coherently is that they get closer and closer to the truth. But that doesn't mean they don't have a coherent evolutionary development, that there aren't criteria with respect to which they can improve with time. But those are primarily instrumental criteria.

The sort of thing I now say, and was not very far from saying in the last chapter of *Structure*, is that truth, at least in the form of a law of noncontradiction, is absolutely essential. You can't have reasonable negotiation or discourse about what to say about a particular knowledge claim if you believe that it could be both true and false. One has to notice, however, how different all this is from a notion of truth which is a correspondence to something external to the logic, the theoretical system, the conceptual scheme. You have to split those two conceptions of truth quite wide apart, stop working back and forth as though this prerequisite for the sort of discourse which can sustain agreement on different points, which requires a law of noncontradiction and a corresponding notion of truth and falsity, were the same as a notion of Absolute Truth. The first thing is something one cannot get on without. But there are all sorts of ways one can go from talking about the relationships of older and newer theories without having to say the new one makes the old one false. I take theories to be whole systems, and as such they don't need to be true or false. All we need to do is by some criteria or other decide which one we would rather have. In general, this is roughly specifiable, but that doesn't get me into the true-false game. Of course, it doesn't eliminate true-false as very *important*. That's what you do *within* a system,—judge the truth or falsity of statements. *Across* a system you can't apply that sort of calculation.

HSR: Many people have argued that scientific theories are underdetermined by the evidence. That is, more than one theory could adequately account for any given body of evidence. Would you distinguish between that idea and your own, and to what extent do you think that notion can be taken? What about the argument that although many theories may be *adequate*, the nature of scientific gathering of data renders those theories far more determined than we might originally think? What do you think of the notion that science might after all be converging upon a sort of Truth?

Kuhn: I've never worried a lot about the underdetermination thesis, but I've no quarrel with it, at least in the weak form that a theory is underdetermined by any finite body of evidence. The stronger forms, however, seem to me vastly more difficult to prove or to make out than I think people usually take them to be.

One way of making the underdetermination point is to use something like Nelson Goodman's argument that it's always possible to generate an incompatible theory by redefining the terms of the theory from which you started, so that both account for the evidence you actually have. You can use his paradox that all emeralds are "grue" or "bleen," and ask how that's any worse a theory than the one that says emeralds are green or blue. I take those techniques to be available for argument, but also to be not quite to the point, though I would hate to have to say in exactly what respect they aren't! They are brilliant arguments and they're about something important, but they don't cut quite the ice that some people think they do with respect to underdetermination. Nevertheless, I think there's real plausibility about the underdetermination thesis.

However, what I don't find plausible are the arguments that say even with *all possible evidence*, the theories would still be underdetermined. The argument becomes [**p. 23**] problematic as soon as you start assuming such ideal situations, and at that point, I'm *unhappy* with the claim. Furthermore, if that's a reasonable unhappiness, then I simply want to say that I am uncertain what would happen to the argument, even with a *limited* amount of data, if I were allowed to have *total accuracy*; if I didn't have to take into account that data is always approximate and that it leaves a certain penumbra around itself.

I think it's at least possible that with full precision on the observations that I have, which is of course just as unavailable as an infinite body of potential data, then maybe I would not be able to find two equally valid theories either.

Considering all possible data, do you really get Kepler's laws from Newton's? Well, you don't quite. Do you get Galileo's law of fall from Newton's, well not exactly, just near the surface of the earth. So I'm not sure what happens to even this more limited version of the thesis, if one doesn't acknowledge that theories are only *approximately* the same, that the data is the best you can hope for within the limits of error.

That's the way I feel about the question of underdetermination, and although I don't quite want to say it's an entirely different ballpark, I don't think it has direct relevance to the sorts of things I was saying in *Structure*. There are two main sorts of people who talk about the underdetermination thesis. In Emerson Hall, [W.V.O.] Quine and [Hilary] Putnam both talk about it, and both of them would, I think, see me as being somewhat of an idealist. But then the strong program people also talk about underdetermination, in order to show that science has *no* content, and from that point of view I'm on the Quine-Putnam side. So the underdetermination thesis constantly gets talked about, but I can be heard as being on either side of it. I certainly don't think it's a *mistaken* thesis, though I think there are some things one would like to know about just how strong a thesis it is.

> *I take theories to be whole systems, and as such they don't need to be true or false. All we need to do is by some criteria or other decide which one we would rather have.*

HSR: In the early sixties you directed a project, the Archive for the History of Quantum Physics, where you and your co-workers conducted interviews with the scientists who had played key roles in the development of quantum physics. Why do you suppose you were chosen to direct the quantum project, what intrigued you most about it, and did the experience affect your view of the ideas set forth in the *Structure of Scientific Revolutions*?

Kuhn: I was asked to help direct the project because I had a PhD in physics, and was a known historian of science. I was not unique in that respect, but I was one of very few people who had both those qualifications.

I knew as a historian that scientists' recollections of their own work is quite bad historically; that they see themselves as having worked towards the thing they eventually discovered, although when you look back you find that in fact they were looking for something entirely different. So I did not expect that the interviews would produce the sort of information about sources of discovery that the physicists on the committee expected.

But I also knew that if you study the papers against the recollections of the scientists, you often find terribly important clues about the processes the scientists had gone through. Here's what the man says, here's what the paper says, and they're obviously incompatible. Now what could it be that leads him to *this* memory construct as opposed to some other? You often get clues that way. So that's what I thought would occur, and what surprised me, then, was the number of times I got simply "I don't remember [...] How would you expect me to remember something like that."

Part of it, as a couple of scientists said fairly explicitly, was that trying to remember is uncomfortable, under these circumstances. The people who write autobiographies have made themselves go through the process, were motivated to go through it. But have somebody come in for five days with a tape-recorder, and they merely don't remember.

I would say that the project had substantially no effect on my views in *Structure*. I never thought that *Structure* was more than a highly schematic sketch. I did not expect any direct lessons. I've always said, assimilate this point of view and this way of doing it, and then see what it does for you when you try to write a history, but don't go out looking at history to see whether this is true or false, to test the ideas. The only test of the ideas, at least at this level of development, is going to be whether having assimilated those ideas, you see the material usefully different. But it's not going to be "Can you always locate the paradigm, can you always tell the difference between a revolution and a normal development?" It's not meant to be applied that way.

> *It is also the case that my concerns are ultimately much more with epistemology than with philosophy of science. I want to know what the nature of knowledge is.*

HSR: When the quantum project was undertaken, historians of science were generally not looking at contemporary science. Nowadays, the emphasis seems to have shifted from the eighteenth century to the late [**p. 24**] nineteenth and far into the twentieth centuries, where science itself has become a much larger, more complex and institutionalized enterprise, with many more texts and much more science to consider. How do you see the changes in history of science in terms of both the *Structure of Scientific Revolutions* and the quantum project?

Kuhn: I think the Quantum Physics project probably did play a role in the development of history of science, in that it labeled the existence of an archive publicly enough so that nobody could write on something without going to look at that material. It wouldn't have been respectable. It almost didn't matter whether the material was good or not. You establish a base-line which sets a level for scholarship, and it helps. I think work is being done not necessarily always very well, but probably with a higher level of responsibility to evidence than it would have been if that material hadn't existed. That's not meant to be a tremendously big claim, and it's not the reason I got into the project. But as I watched what happened later, yes, some people were attracted to twentieth-century stuff because that material was there, and I think it meant that anybody doing twentieth-century stuff had to look at archives, whether the material was in that archive or elsewhere. In that sense the project made history of science a more scholarly discipline.

Now the other question about how, when science gets as big as it has, can we know the texts—I don't know the answer. I see it as a question about practice, not a question about principles. I think the best study in conceptual change I've done is my Planck book [1978], although it's not always been viewed that way. And that doesn't *begin* to tell you about gigantic science. But it sure as hell presents problems of scale not found when working on Galileo, and it was still feasible to write conceptual history. I've never tried anything that gets to post WWII science, and I absolutely see that it's difficult.

But if you feel as I do that there are many more traces left of the stories than their authors and editors think there are, there are going to be clues. The problems are gigantic, but I'm not persuaded that there's nothing to be done about reconstructing conceptual change. How much of it can be done and in what ways, I'm not sure. But I think that whole business of looking for the things that don't make sense still applies.

John [L.] Heilbron and I wrote a paper about the genesis of the Bohr atom, which we started during the Quantum Physics project when we read Bohr's 1913 paper, in preparation for interviewing him. There were 2 or 3 passages in there that made absolutely no sense. Taken as a whole, the paper gives the Bohr model of the hydrogen atom on the one hand, and on the other, an atom with only a ground state, but in which the electron strums all the strings as it falls into the ground state from outside the atom. I don't think traces of that sort are going to have vanished. And they lead back through footnotes and other things into earlier papers, as the Bohr paper led back to a [C.G.] Darwin paper, which proved a very useful piece of background for understanding it.

It's also the case that my concerns are ultimately much more with epistemology than with philosophy of science. I want to know what the nature of knowledge is. I think science is an excellent thing to look at, if you're concerned with epistemology, and that's no novelty on my part—that has been going on since the seventeenth century when science provided epistemological examples. And with that interest, it doesn't make a whole lot of difference to me if things are now different. I see no reason to suppose that the things I think I have learned about the nature of *knowledge* are going to be disturbed by the need to change the theory of *science*. I could be all wrong with respect both to science and to the nature of knowledge, but I would make this separation to explain why I'm less concerned about the question "Is science changing?" than I might be if studying the nature of science weren't in the first instance simply a way of looking at the picture of knowledge.

I see no reason to suppose that the things I think I have learned about the nature of knowledge are going to be disturbed by the need to change the theory of science.

HSR: How do the developments in the last thirty years which we have been discussing, bear upon your interests today? What are your current thoughts and projects?

Kuhn: What I've been working on for the last eight years [**p. 25**] and may be working on for the next five, is a book about the philosophical problems, especially incommensurability, left over from the *Structure of Scientific Revolutions*. I've been going back to the book and looking at whether in fact those thoughts I had are going to work with what's been going on in philosophy of science recently, to see how I can deal with those other ideas. As I've said, when I wrote *Structure*, I hadn't read much philosophy of science, and had no idea how much was going on in that field. I had seen what I thought was something important about the way science and conceptual frameworks worked, and that's what I was writing about. But today I would look at what Quine has to say, what Putnam has to say, and they both have a lot to say about science.

I *think* I see a way in which what I was doing in *Structure* might be made to take account of all that. But I'm not sure. It might be that in these contexts, the ideas in *Structure* will have to be revised, it might not. So I've been reading a good bit of that, and now I think I've got it all ready enough to begin writing. But of course when I write, well there's no guarantee that it will turn out the way I envisioned things when I started. I've learned that the greatest changes come about when the actual writing begins. But this is certainly another one of those books that will be a decade at least in the making, a decade or more, I can't say at this point. But that's what I'm working on now.

Figure 1.3: Thomas S. Kuhn being interviewed November 1989 in his office at MIT; photographer: Skúli Sigurdsson; picture: 10.

Thomas S. Kuhn is the author of:

The Copernican Revolution: Planetary Astronomy in the Development of Western Thought (1957)

The Structure of Scientific Revolutions (1962)

Sources for History of Quantum Physics: An Inventory and Report (1967) [co-authored with John L. Heilbron, Paul Forman and Lini Allen]

The Essential Tension: Selected Studies in Scientific Tradition and Change (1977)

Black-body Theory and the Quantum Discontinuity, 1894–1912 (1978)