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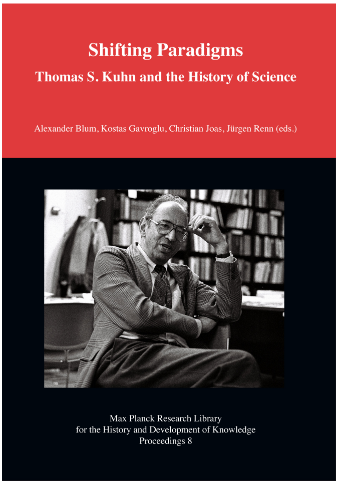
Gerald Holton:

Steve's Question and Tom's Last Lecture: A Personal Perspective



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Chapter 2
Steve's Question and Tom's Last Lecture: A Personal Perspective

Gerald Holton

I deeply regretted not to be joining you at the star-studded conference, but I shall respond here to the invitation to submit some remarks on the topic set out for us. Our discipline does indeed deserve attention to its own history, and your choice to center attention on Tom Kuhn's celebrated book *The Structure of Scientific Revolutions* of 1962 is eminently reasonable, since among the effects it caused was the resurgence of lively and wide-ranging interests, both in and outside our field.

But the announced topic of the conference also invites some reflection on the prehistory of Tom's book itself. I leave aside the well-discussed possibility that the timing of its composition and publication, without intention, was perfect at that historic period of rupture and national trauma in the 1960s. Also, the long reign of logical empiricism was running out of steam. Thus, for different reasons, many were looking for new paradigms.

Yet, there may also have been some important events in Tom's own life and thoughts leading up to and shaping the famous concepts in his *Structure*. On this possibility there have been some preliminary investigations; further study would be well within the project of a history of the history of science.

When your invitation reached me, I wondered on what specific aspect I could contribute. It occurred to me that I might, on this occasion, think about Tom's creative work in a personal way, being now perhaps one of the few who knew and interacted with Tom in those early days, for over a dozen years.

After all, we had some overlapping lives, intellectually, institutionally, culturally and socially. Born in the same year, we received our doctorate degrees in physics at about the same time, under brilliant and demanding scientists, in the same building (while Harvard University was only just abandoning its quota system with respect to admitting Jewish students). Jim Conant and his hugely ambitious General Education Program excited in both of us intense interest in the history of science.

We also publicly acknowledged our intellectual debts to many of the same powerful scholars (among the contemporaries, Koyré, Sarton, Merton, Nagel, etc., among those from whom we had courses or consulted, Quine, P. Frank, P. W. Bridgman, Van Vleck, Richard von Mises, Raphael Demos, etc.). We both took part in an informal workshop on how best to teach in this new field, under E. C. Kemble and including common friends such as the unforgettable Lenn Nash. Tom and I saw each other, and our families, at many gatherings, and we later corresponded, with Tom generously providing his opinions on some of my work. And not least, all of us were then bathed in the powerful local mythology, although with different reactions to it.

Moreover, we both grew up in a philosophical climate much indebted to logical empiricism; yet, each of us, although in different ways, turned to a very different position, yet in both cases centered on the role of predispositions.

So despite the complexities we all know may hide behind even close friendships, I feel that, for long enough segments, our lives moved along strangely parallel paths, especially during the period of our personal and professional maturing. That fact may give me some standing here, specifically in trying to help answer a persistent question about the history behind Tom's historical work.

That question was raised early and indirectly by Tom's friend and mentor, Harvard's President Jim Conant, in Conant's famous letter, in which he begged off to writing a preface to Tom's *Structure*, with Conant dismissing the conception of paradigm as "a magical verbal word to explain everything," and perceptively using the words "you have fallen in love," to suggest what may have prompted Tom's choice of his main concepts.

The inquiry became quite explicit in Steven Weinberg's essay, "The Revolution That Didn't Happen." While lauding many aspects of Tom's writings, Steve called the description of scientific revolutions "seriously misleading," insisting that changes in understanding nature "have been evolutionary, not revolutionary," and then asked: "What in Kuhn's life led him to his radical skepticism, to his strange view of the progress of science?"

In trying to provide an answer to this question, Steve shared a portion of a letter Tom had sent to him, in which Tom had written of having experienced a crucial "epiphany" around 1947, when he suddenly thought he could understand Aristotle's own mindset about the physics of that period, and so to speak slip into Aristotle's own paradigmatic preference. (Tom referred to the same incident also at other times.)

Tom's response to Steve is surely fascinating. But there may be other contributions to be made on this point. The time and place for one such additional insight came when Tom returned in November 1991 to Harvard to give his last lecture there, at his old home, launching in great style the new, distinguished annual Robert and Maurine Rothschild Lecture series, with his talk entitled "The Trouble with the Historical Philosophy of Science." Some analysis of that event may suggest how to reconsider Steve's question.

In this quest, one has to start with a fact, based on observation and readings, that Tom, while of course a world-class scholar, was internally deeply anguished. (This mixture in great figures is of course not unknown to us historians of science.) Part of his anguish was the result of his well-known shifting disciplinary identity. He first saw himself as a physicist, at a time when the Harvard Physics department was astonishingly flowering. The work of professors there such as Ed Purcell, Norman Ramsey, Julian Schwinger, Bob Pound and Van Vleck set the bar for good work to be done in this field very high indeed. For every graduate student who was inspired by this constellation there was likely to be another to feel discouraged. At any rate, right after having gotten his degree in 1949, Tom said later tersely, "I got out of physics."

His thesis adviser, Van Vleck, let it be known that this move annoyed him greatly, because Van Vleck thought he had wasted his time on his student. But now Tom could begin to train himself to become a historian of science under the auspices of Harvard's President, Jim Conant, co-teaching in an undergraduate course in General Education that centered on case studies of the seventeenth-century Scientific Revolution and its consequences. The profession was still quite young in the USA—there were few universities with history of science programs, Harvard having no such department for years to come.

Tom took his place as a historian of science with a book, meant for undergraduate-level courses, titled significantly *The Copernican Revolution*, though it was not published (in part because of Tom's meticulousness) until 1957. Meanwhile, in 1955, the possibility of a tenure appointment at Harvard was denied him by its Committee on General Education, reportedly because of Tom's then still thin publication record. Tom was fond of that university, and its refusal was a real blow.

Philosophy of science had been a side interest for Tom since his school days, but began to move to the center by 1952–53, when Tom looked for funds to have time for writing a monograph that eventually became the *Structure* book of 1962. Happily, the University of California in Berkeley offered Tom an Assistant Professorship in History of Science, located in both the Department of History and the Department of Philosophy. This arrangement illustrated his straddling of professional identities at the time.

Yet, this arrangement soon caused a deeply upsetting event. As late as 1995, Tom reported in an interview, "a quite destructive thing happened" and "I was extraordinarily angry, as you can guess, and very deeply hurt. I mean that's a hurt that has never altogether gone away." What happened was that when Tom's appointment to a full professorship came up, the Philosophy Department at Berkeley specifically opposed Tom's membership in that department.

From his perspective, he had left physics early, had become a sound historian of science, but his final, public turn into a professional philosopher of science had been questioned in a manner that was hurtful for the rest of his life.

However, there was a way left for him to clearly establish his credentials in the field, although there too the bar was very high (one thinks of Quine and Putnam back "home," and others elsewhere). This possibility, on which he had been working on and off for years, came into full view at Tom's last lecture at Harvard, at the Rothschild Lecture.

Tom began his talk by confessing that the "transformation" of the "image of science," which he thought he had helped to bring about, troubled him because some of his concepts had been used and developed by people who called themselves "Kuhnians," although he regarded their viewpoints as "damagingly mistaken." He was pained to be associated with their misunderstandings. In this feeling he was not alone. There were others who had reached astonishing popular success but suffered the same sort of pain. For example, Bridgman, in a publication in which he reassessed his own writings in the philosophy of science, confessed that regarding "this thing called 'operationalism' [...] I feel that I have created a Frankenstein, which certainly got away from me."

Next, in his lecture, Tom announced that he was currently at work on a new book, "a far larger project," devoted to "a theory which I once called incommensurability," although he regretted that in this talk he could not give details. But, importantly, in this talk he would speak "as a philosopher." A key point was that "for a philosopher who adopts the historical perspective, the problem is [...] understanding small incremental changes of belief" (rather than preoccupation with evaluation of belief itself).

The use of the word "small" in that sentence prepared one to expect next his revisiting his conception of large changes, such as Revolutions. Instead, to my surprise, Tom went into the opposite direction, saying that "scientific development is like Darwinian evolution." He elaborated this viewpoint with his use of related conceptions such as "evolutionary tree" and "speciation."

Of course Tom had briefly touched on evolutionary models toward the end of his *Structure* book of decades earlier, but in the context of chapters there with headings such as "Progress through Revolutions" and "Revolution and Relativism." No longer. Now his evolving view—he called it "reconceptualization"—had brought him, as he declared at the end of his talk, to the need to reinterpret the main parts of his previous thoughts. That, he announced, would be found in the new, to-be-expected work, where, as he put it, "the answer is incommensurability." Much of Tom's promise of a reconceptualized and reinterpreted version of his previous conceptions—as well as his analogy of scientific development with Darwinian evolution—would have appealed to previous critics like Steven Weinberg (and there had been many others). But the proof of the promise had to wait for the book.

One could feel that once more the stakes were high for Tom. Speaking explicitly as a philosopher, his standing in that profession would now hinge on the new work, of which he could give us in his lecture only hints. Tom talked about this important project also later (for example in a long interview, published in 1991). But in the end he was not able to publish the work. And that, in my view, was a chief source of Tom's internal state of dismay, especially in his last decade, as he was trying to reach the new, high professional identity level he had set for himself.

He had always been hard on himself, and had been through the harsh school of making himself anew—physics, history, philosophy—each time with his characteristic, impeccable honorability. As he told his interviewers in October 1995, less than a year before his death: "I am an anxious, neurotic." Sadly, it was worse. There are good reasons to think that near the end of his career Tom considered himself to have been a failure.

Tom would be the only one who would make such a severe judgment. On the contrary, as illustrated by the persistent, widespread attention being paid to his work, his distinguished place in scholarship is secure.