Thomas S. Kuhn

The Essential Tension

Selected Studies in Scientific Tradition and Change

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My own enlightenment began in 1947, when I was asked to interrupt my current physics project for a time in order to prepare a set of lectures on the origins of seventeenth-century mechanics. For that purpose, I needed first to discover what the predecessors of Galileo and Newton had known about the subject, and preliminary inquiries soon led me to the discussions of motion in Aristotle's *Physica* and to some later works descended from it. Like most earlier historians of science, I approached these texts knowing what Newtonian physics and mechanics were. Like them, too, I asked of my texts the questions: How much about mechanics was known within the Aristotelian tradition, and how much was left for seventeenth-century scientists to discover? Being posed in a Newtonian vocabulary, those questions demanded answers in the same terms, and the answers then were clear. Even at the apparently descriptive level, the Aristotelians had known little of mechanics; much of what they had had to say about it was simply wrong. No such tradition could have provided a foundation for the work of Galileo and his contemporaries. They necessarily rejected it and began the study of mechanics over again.

Generalizations of that sort were widely current and apparently inescapable. But they were also puzzling. When dealing with subjects other than physics, Aristotle had been an acute and naturalistic observer. In such fields as biology or political behavior, his interpretations of phenomena had often been, in addition, both penetrating and deep. How could his characteristic talents have failed him so when applied to motion? How could he have said about it so many apparently absurd things? And, above all, why had his views been taken so seriously for so long a time by so many of his successors? The more I read, the more puzzled I became. Aristotle could, of course, have been wrong—I had no doubt that he was—but was it conceivable that his errors had been so blatant?

One memorable (and very hot) summer day those perplexities suddenly vanished. I all at once perceived the connected rudiments of an alternate way of reading the texts with which I had been struggling. For the first time I gave due weight to the fact that Aristotle's subject was change-of-quality in general, including both the fall of a stone and the growth of a child to adulthood. In his physics, the subject that was to become mechanics was at best a still-not-quite-isolable special case. More consequential was my

recognition that the permanent ingredients of Aristotle's universe, its ontologically primary and indestructible elements, were not material bodies but rather the qualities which, when imposed on some portion of omnipresent neutral matter, constituted an individual material body or substance. Position itself was, however, a quality in Aristotle's physics, and a body that changed its position therefore remained the same body only in the problematic sense that the child is the individual it becomes. In a universe where qualities were primary, motion was necessarily a change-of-state rather than a state.

Though drastically incomplete and far too baldly stated, those aspects of my new understanding of Aristotle's enterprise should indicate what I mean by the discovery of a new way to read a set of texts. After I achieved this one, strained metaphors often became naturalistic reports, and much apparent absurdity vanished. I did not become an Aristotelian physicist as a result, but I had to some extent learned to think like one. Thereafter I had few problems understanding why Aristotle had said what he did about motion or why his statements had been taken so seriously. I still recognized difficulties in his physics, but they were not blatant and few of them could properly be characterized as mere mistakes.

Since that decisive episode in the summer of 1947, the search for best, or best-accessible, readings has been central to my historical research (and has also been systematically eliminated from the narratives that report its results). Lessons learned while reading Aristotle have also informed my readings of men like Boyle and Newton, Lavoisier and Dalton, or Boltzmann and Planck. Briefly stated, those lessons are two. First, there are many ways to read a text, and the ones most accessible to a modern are often inappropriate when applied to the past. Second, that plasticity of texts does not place all ways of reading on a par, for some of them (ultimately, one hopes, only one) possess a plausibility and coherence absent from others. Trying to transmit such lessons to students, I offer them a maxim: When reading the works of an important thinker, look first for the apparent absurdities in the text and ask yourself how a sensible person could have written them. When you find an answer, I continue, when those passages make sense, then you may find that more central passages, ones you previously thought you understood, have changed their meaning.[2]

If this volume were addressed primarily to historians, that autobiographical fragment would not be worth recording. What I as a physicist had to discover for myself, most historians learn by example in the course of professional training. Consciously or not, they are all practitioners of the hermeneutic method. In my case, however, the discovery of hermeneutics did more than make history seem consequential. Its most immediate and decisive effect was instead on my view of science. That is the aspect of my encounter with Aristotle that has led to my recounting it here.

Men like Galileo and Descartes, who laid the foundation for seventeenth-century mechanics, were raised within the Aristotelian scientific tradition, and it made essential contributions to their achievement. Nevertheless, a key ingredient of that achievement was their creation of the way of reading texts that had initially so misled me, and they often participated in such misreadings themselves. Descartes, for example, early in *Le monde*, ridicules Aristotle by quoting his definition of motion in Latin, declining to translate on the ground that the definition makes equally little sense in French, and then proving his point by producing the missing translation. Aristotle's definition had, however, made sense for centuries before, probably at one time to Descartes himself. What my reading of Aristotle seemed therefore to disclose was a global sort of change in the way men viewed nature and applied language to it, one that could not properly be described as constituted by additions to knowledge or by the mere piecemeal correction of mistakes. That sort of change was shortly to be described by Herbert Butterfield as "putting on a different kind of thinking-cap,"[3] and puzzlement about it quickly led me to books on Gestalt psychology and related fields. While discovering history, I had discovered my first scientific revolution, and my subsequent search for best readings has often been a search for other episodes of the same sort. They are the ones that can be recognized and understood only by recapturing out-of-date ways of reading out-of-date texts.

2. More on this subject will be found in T. S. Kuhn, "Notes on Lakatos," *Boston Studies in Philosophy of Science* 8 (1971): 137–46.

3. Herbert Butterfield, *Origins of Modern Science, 1300–1800* (London, 1949), p. 1. Like my own understanding of the transformation of early modern science, Butterfield's was greatly influenced by the writings of Alexandre Koyré, especially his *Etudes galiléennes* (Paris, 1939).