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THE ROAD SINCE

STRUCTURE

Philosophical Essays, 1970–1993, with an Autobiographical Interview

Thomas S. Kuhn

EDITED BY JAMES CONANT AND JOHN HAUGELAND

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Foreword

JEHANE R. KUHN

Tom's preface to an earlier selection of his published papers, *The Essential Tension,* published in 1977, was cast as the narrative of a journey of inquiry—toward, and then on from, *The Structure of Scientific Revolutions,* published fifteen years earlier. Some autobiographical framing was called for, he explained, since his published papers did not tell the story of a journey that had found its way from physics to historiography and philosophy. The preface to that volume closed by focusing on the philosophical/metahistorical issues that "currently . . . concern me the most, and I hope before long to have more to say about them." In the introduction to this new volume, the editors place each paper in relation to those continuing issues, again pointing forward: this time to the work-in-progress which they are preparing for publication. It will represent not the goal of Tom's journey, but the stage at which he left it.

The title of this book again invokes the metaphor of a journey, and its closing section, which records an extended interview at the University of Athens, amounts to another, longer, more personal narrative. I am delighted that the interviewers, and the editorial board of the journal *Neusis,* in which it first appeared, have agreed to its republication here. I was present during the interview and admired the knowledge, perceptiveness, and sympathetic candor of the three colleagues, who were also our hosts in Athens. Tom was exceptionally at ease with these three friends and talked freely on the assumption that he would review the transcript; but time ran out, and that task fell to me, in consultation with the other participants. I know that Tom would have intervened in the transcript substantially—not so much from discretion, which was

not among his virtues, as from courtesy. In his talk as it appears here, there are expressions of feeling and of judgment, some of which I'm fairly sure he would have moderated or perhaps omitted; I did not think it my place—or anyone else's—to moderate or omit them on his behalf. Many of the grammatical inconsistencies and unfinished phrases of informal talk have for that reason been left unsmoothed, as a reminder of the interview's unauthorized status. I am grateful to colleagues and friends, in particular Karl Hufbauer, who have caught local errors of chronology or helped to decipher names.

The circumstances in which Jim Conant and John Haugeland accepted the task of editing this volume are told in their introduction; I have only to add that Tom's wholehearted confidence is the best testimonial they could have. I am warmly grateful to them, and no less warmly to Susan Abrams, for her friendship and, inseparably, her professional judgment, during this project as in the past. Sarah, Elizabeth, and Nathaniel Kuhn are sustaining participants in my role as their father's literary executor.

Introduction

JAMES CONANT AND JOHN HAUGELAND

Shifts happen.

In *The Structure of Scientific Revolutions,* as nearly everyone knows, Thomas Kuhn argued that the history of science is not gradual and cumulative, but rather punctuated by a series of more or less radical "paradigm shifts." What is less well known is that Kuhn's own understanding of how best to characterize these episodes itself underwent a number of significant shifts. The essays collected in this volume represent several of his later attempts to rethink and extend his own "revolutionary" hypotheses.

We discussed the contents of this volume with Kuhn at some length shortly before he died. Although he declined to specify them in full detail, he had a quite definite idea of what he wanted the volume to be. In making this clear to us, he made several explicit stipulations, reviewed with us the pros and cons in several other cases, and then provided four general guidelines for us to follow. For those readers interested in how the final choices were made, we will begin by briefly summarizing these guidelines.

The first three guidelines that we were given flow from Kuhn's vision of this volume as a sequel to, and as modeled upon, his earlier collection, *The Essential Tension,* which appeared in 1977. In that collection, Kuhn restricted himself to substantial essays that he regarded as developing philosophically significant themes (albeit generally in the context of

historical or historiographical considerations), as opposed to those that mainly explore particular historical case studies. Accordingly, our first three guidelines were: include only essays that are expressly philosophical in their concerns; include only those philosophical essays written in Kuhn's last two decades;[1] include only substantial essays, as opposed to brief reviews or addresses.

The fourth guideline concerns material that Kuhn regarded as essentially preparatory to—in effect, early drafts of—the book he had been working on for some years. Since it is also part of our charge to edit and publish that work, making use, where appropriate, of this material, we were instructed not to include any of it here. Covered under this restriction are three important lecture series: "The Natures of Conceptual Change" (Perspectives in the Philosophy of Science, University of Notre Dame, 1980), "Scientific Development and Lexical Change" (The Thalheimer lectures, Johns Hopkins University, 1984), and "The Presence of Past Science" (the Shearman lectures, University College, London, 1987). Although typescripts of these lectures have circulated here and there in samizdat form, and have occasionally been cited and discussed in publications by others,[2] Kuhn did not want any of them published in their present form.

Speaking very broadly, the essays reprinted here can be seen to address four main topics. First, Kuhn reiterates and defends his view, going all the way back to *The Structure of Scientific Revolutions* (hereafter cited as *Structure*), that science is a cognitive empirical investigation of nature that exhibits a unique sort of progress, despite the fact that this progress cannot be further explicated as "approximating closer and closer to reality." Rather, progress takes the form of ever-improving technical puzzle-solving ability, operating under strict—though always tradition-bound—standards of success or failure. This pattern of progress, in its

1. Kuhn made it clear that those essays with expressly philosophical concerns that he chose to omit from *The Essential Tension* were omitted because he had become dissatisfied with them, and that he did not want them collected in this volume, either. In particular, he was adamant that his 1963 essay "The Function of Dogma in Scientific Research" should not be included here, even though it has been widely read and cited.

2. Perhaps the most notable of these is Ian Hacking's essay "Working in a New World: The Taxonomic Solution" (in *World Changes: Thomas Kuhn and the Nature of Science*, ed. Paul Horwich [Cambridge, MA: Bradford/MIT Press, 1993]), in which he expounds and attempts to refine the central argument of the Shearman lectures.

fullest realization exclusive to science, is prerequisite to the extraordinarily esoteric (and often expensive) investigations that are characteristic of scientific research, and thus to the astonishingly precise and detailed knowledge that it makes possible.

Second, Kuhn develops further the theme, which again goes back to *Structure*, that science is fundamentally a social undertaking. This shows up especially in times of trouble, with the potential for more or less radical change. It is only because individuals working in a common research tradition are able to arrive at differing judgments concerning the degree of seriousness of the various difficulties that they collectively face that some of them will be moved individually to explore alternative (often—as Kuhn likes to emphasize—seemingly nonsensical) possibilities, while others will attempt doggedly to resolve the problems within the current framework.

The fact that the latter are in the majority when such difficulties first arise is essential to the fertility of scientific practices. For, *usually*, the problems can be resolved, and eventually are. In the absence of the requisite persistence to find those solutions, scientists would not be able to home in, as they do, on those rarer but crucial cases in which efforts to introduce radical conceptual revision are fully repaid. On the other hand, of course, if no one were ever to develop possible alternatives, major reconceptions could never emerge, even in those cases in which they genuinely became necessary. Thus, a *social* scientific tradition is able to "distribute the conceptual risks" in a way that would be impossible for any single individual, and yet is prerequisite to the long-term viability of science.

Third, Kuhn spells out and emphasizes the analogy, barely hinted at in the closing pages of *Structure*, between scientific progress and evolutionary biological development. In elaborating this theme, he plays down his original picture, which had periods of normal science within a single area of research punctuated by occasional cataclysmic revolutions, and introduces in its place a new picture, which has periods of development within a coherent tradition divided occasionally by periods of "speciation" into two distinct traditions with somewhat different areas of research. To be sure, the possibility remains that one of the resulting traditions may eventually stagnate and die out, in which case we have, in effect, the older structure of revolution and replacement. But at least as often in the history of science, both successors, neither quite like their common ancestor, flourish as new scientific "specialties." In science, speciation is specialization.

Finally, and most important, Kuhn spent his last decades defending, clarifying, and substantially developing the idea of incommensurability. This theme too was already conspicuous in *Structure*, but not very well articulated. It is the feature of the book that was most widely criticized in the philosophical literature, and Kuhn came to be dissatisfied with his original presentation. Commensurability and incommensurability, as presented in Kuhn's later work, are terms that denote a relation obtaining between *linguistic* structures. There are basically two new points underlying this linguistic reformulation of the notion of incommensurability.

First, Kuhn carefully explicates the difference between distinct but commensurable languages (or portions of languages) and incommensurable ones. Between pairs of the former, translation is perfectly possible: whatever can be said in the one can be said in the other (though it may be considerable work to figure out how). Between *incommensurable* languages, however, strict translation is not possible (even though, on a case-by-case basis, various paraphrases *may* suffice for adequate communication).

The idea of incommensurability, as it was elaborated in *Structure*, was widely criticized on the grounds that it made it unintelligible how scientists working under different paradigms were able to communicate with one another (let alone adjudicate and resolve their disagreements) across a revolutionary divide. A related criticism concerned the putative explanations of past scientific paradigms furnished within the pages of *Structure* itself: didn't the work undermine its own doctrine of incommensurability by offering illuminating explanations (in contemporary English) of how alien scientific terms were used?

Kuhn here responds to these objections by pointing out the difference between language translation and language learning. Just because a foreign language is not translatable into whatever language one already speaks does not mean that one cannot learn it. That is, there is no reason that a single person cannot speak and understand two languages that he or she cannot translate between. Kuhn calls the process of figuring out such an alien language (say, from historical texts) *interpretation*, and also—to emphasize its distinctness from so-called "radical" interpretation (à la Davidson)—*hermeneutics*. His own explanations of the terminology from, say, Aristotelian "physics" or phlogiston "chemistry" are exercises in hermeneutic interpretation and, at the same time, aids to the reader in learning a language incommensurable with his or her own.

Kuhn's second main point about incommensurability is a new and fairly detailed account of how and why it occurs in two sorts of scientific

context. Technical scientific terminology, he explains, always occurs in families of essentially interrelated terms; and he discusses two varieties of such families. In the first variety, the terms are kind terms—roughly, sortals—which Kuhn calls "taxonomic categories." These are always arrayed in a strict hierarchy, which is to say that they are subject to what he calls "the no-overlap principle": no two such categories or kinds can have any instances in common unless one of them entirely and necessarily subsumes the other.

Any taxonomy adequate to the purposes of scientific description and explanation is constructed on the basis of an implicit no-overlap principle. The meanings of the relevant kind terms specifying such taxonomic categories, Kuhn argues, are partially constituted by this implicit presupposition: the meanings of the terms depend on their respective subsumption and mutual exclusion relations (plus, of course, the learnable skills of recognizing members). Such a structure—which Kuhn calls a "lexicon"—has, in itself, considerable empirical content, because there are always multiple ways of recognizing (multiple "criteria" for) membership in any given category. Distinct taxonomic structures (ones with different subsumption and exclusion relations) are inevitably incommensurable, because those very differences result in terms with fundamentally disparate meanings.

The other variety of terminological family (also called a lexicon) involves those terms whose meanings are determined in part—but crucially—by scientific laws relating them. The clearest examples are the quantitative variables that occur in laws expressed as equations—for instance, weight, force, and mass in Newtonian dynamics. Though this sort of case is not as well worked out in the extant Kuhnian texts, Kuhn believed that here, as well, the meanings of the relevant fundamental terms are partially constituted through their occurrence in claims—in this case, scientific laws—that categorically exclude certain possibilities; hence any changes in the understandings or formulations of the relevant laws must result, according to Kuhn, in fundamental differences in the understandings (hence, meanings) of the corresponding terms, and thus incommensurability.

This volume is divided into three parts: two groups of essays, each arranged chronologically, and an interview. Part 1 includes five free-standing essays presenting various of Kuhn's views as they developed

from the early 1980s through the early 1990s. Two of these essays include brief replies to comments that were made when the essays were first presented. Although, of course, such replies can be fully appreciated only in the context of those comments themselves, Kuhn is careful in each case to summarize the specific points he is replying to, and the resulting remarks add useful clarity to the main paper. Part 2 includes six essays, varying widely in length, each of which consists mainly of Kuhn's response to the work of one or more other philosophers—often, though not always, itself developments or criticisms of Kuhn's own prior work. Finally, in part 3, we have included a lengthy and candid interview with Kuhn, conducted in Athens in 1995 by Aristides Baltas, Kostas Gavroglu, and Vassiliki Kindi.

Part 1: Reconceiving Scientific Revolutions

Essay 1, "What are Scientific Revolutions?" (~1981), consists primarily of a philosophical analysis of three historical scientific sea-changes (concerning the theories of motion, the voltaic cell, and black-body radiation) as illustrations of Kuhn's then nascent account of taxonomic structures.

Essay 2, "Commensurability, Comparability, Communicability" (1982), is an elaboration and defense of the importance of incommensurability with regard to the two principal charges that (1) it is impossible, because intelligibility at all entails translatability, hence commensurability; and (2), if it were possible, it would imply that major scientific changes cannot be responsive to evidence, and therefore must be fundamentally irrational. Versions of these charges made by Donald Davidson, Philip Kitcher, and Hilary Putnam receive particular attention.

Essay 3, "Possible Worlds in History of Science" (1989), develops the idea—dramatically propounded but not well explained in *Structure*—that incommensurable scientific languages (now called lexicons) give access to different sets of possible worlds. In his discussion, Kuhn clearly distances himself from possible-worlds semantics and from the causal theory of reference (along with the associated forms of "realism").

Essay 4, "The Road since Structure" (1990), is announced as a brief sketch of the book Kuhn had (in 1990) been working on for just over a decade (the book he never finished). Though at the highest level the topic of the book is realism and truth, what it mostly will discuss is

incommensurability—with particular emphasis on why it is not a threat to scientific rationality and its basis in evidence. Thus, in part, the book is conceived as a repudiation of what Kuhn regarded as certain excesses in the so-called "strong program" in the philosophy (or sociology) of science. At the conclusion of the essay (and, in more detail, in the Shearman lectures), he describes his position as "post-Darwinian Kantianism," because it presupposes something like an ineffable but permanent and fixed "*Ding an sich*." Kuhn had earlier rejected the notion of a *Ding an sich* (see essay 8), and he again later repudiated (in conversations with us) both that notion and the reasons he had put forward for it.

Essay 5, "The Trouble with the Historical Philosophy of Science" (1992), considers both traditional philosophy of science and the now fashionable "strong program" in the sociology of science, and what is wrong with each. Kuhn suggests that "the trouble" with the latter may be that it *retains* a traditional conception of knowledge, while noticing that science does not live up to that conception. The reconceptualization that is required—and which gets rationality and evidence back into the picture—is to focus not on rational evaluation of beliefs, but rather on rational evaluation of *changes* in beliefs.

Part 2: Comments and Replies

Essay 6, "Reflections on My Critics" (1970), is the oldest essay in the collection, and the only one that antedates the compilation of *The Essential Tension*. We discussed its inclusion explicitly with Kuhn, who was pulled both ways. And, in trying to decide whether to include it, so were we. On the one hand, it violates the third "guideline" mentioned above, and, moreover, it consists primarily of corrections of various misreadings of *Structure*—corrections that, in a perfect world, ought not to be necessary. On the other hand, many of those misunderstandings persist, and so their correction *is* still needed—something this essay achieves with unique clarity, thoroughness, and vigor. In the end, Kuhn left the decision up to us. We have decided to reprint it because of its still relevant special merits, and because the volume in which it originally appeared—*Criticism and the Growth of Knowledge*—has for some time been out of print.

Essay 7, "Theory Change as Structure Change: Comments on the Sneed Formalism" (1976), is a tentative but mostly very favorable dis-

cussion of Joseph Sneed's model-theoretic formalism for the semantics of scientific theories, along with Wolfgang Stegmüller's uses and elaborations of it. Although the essay will be of particular interest to readers already familiar with the Sneed-Stagmüller approach, Kuhn's remarks are nontechnical and of more general interest as well. He is especially gratified by the way in which, according to this approach, the central terms of a theory acquire a significant part of their determinate content from multiple exemplary *applications*. It is important that there be *several* such applications, because they mutually constrain one another (via the theory), thereby avoiding a kind of circularity. It is important that the applications be *exemplary* because this emphasizes the role of learnable skills, which can then be extended to new cases. Kuhn's only expressed reservation about the approach—albeit a serious one—is that it leaves no obvious place for the essential phenomenon of theoretical incommensurability.

Essay 8, "Metaphor in Science" (1979), is a response to a presentation by Richard Boyd about the analogies he sees between scientific terminology and ordinary-language metaphors. Though they agree on several important points, Kuhn demurs from the specific *way* in which Boyd extends the view to include the causal theory of reference, especially with regard to natural-kind terms. In his conclusion, Kuhn describes himself as, like Boyd, an "unregenerate realist," but he thinks this doesn't mean the same thing in their two cases. In particular, he rejects Boyd's own metaphor of scientific theories (coming closer and closer to) "carving nature at the joints." He likens this idea of nature's "joints" to Kant's *Ding an sich,* an aspect of Kantianism he here rejects.

Essay 9, "Rationality and Theory Choice" (1983), is Kuhn's contribution to a symposium on the philosophy of Carl G. Hempel. In it, he responds to a question that Hempel had put to him on several occasions: does he (Kuhn) recognize the difference between *explaining* theory-choice behavior and *justifying* it? Granted that choices of theories are *in fact* based on their puzzle-solving ability (including accuracy, scope, and so on), this does not get any philosophical bite as a *justification* unless and until those criteria themselves are justified as somehow nonarbitrary. Kuhn replies that they are nonarbitrary ("necessary") in the relevant way because they belong together in an empirically contentful taxonomy of disciplines; reliance on *just such* criteria (plural) is what distinguishes *scientific* investigation from other professional pursuits (fine arts, law, engineering, and so on)—hence is, in effect, definitive of 'science' as a genuine kind term.

Essay 10, "The Natural and the Human Sciences" (1989), mainly discusses Charles Taylor's influential essay "Interpretation and the Sciences of Man," which Kuhn much admires. While he is inclined to agree with Taylor that the natural and human sciences are different, he probably doesn't agree about what that difference is. After arguing that the natural sciences, too, have a "hermeneutic base," he acknowledges that, unlike the present human sciences, they are not, in themselves, hermeneutic. But he questions whether this reflects an essential difference or, rather, simply indicates that most of the human sciences have not yet reached the developmental stage that he used to associate with acquisition of a paradigm.

Essay 11, "Afterwords" (1993), like essay 6, is the final chapter in a volume of essays largely devoted to discussions of Kuhn's own work (*World Changes: Thomas Kuhn and the Nature of Science,* edited by Paul Horwich). Unlike its somewhat feisty predecessor, however, this essay is primarily an appreciative and constructive engagement with essays that are themselves primarily constructive. The main themes are taxonomic structures, incommensurability, the social character of scientific research, and truth cum rationality cum realism. The discussion of these themes is presented here in the form of a brief sketch of some of the central ideas of Kuhn's long promised but never finished new book—on which he continued to work until he no longer could.

Part 3: A Discussion with Thomas S. Kuhn

"A Discussion with Thomas S. Kuhn" (1997) is a candid intellectual autobiography in the form of an interview, conducted by Aristides Baltas, Kostas Gavroglu, and Vassiliki Kindi in Athens in the fall of 1995. It is reprinted, lightly edited, in its entirety.

The volume ends with a complete bibliography of Kahn's published work.

A Discussion with Thomas S. Kuhn

A Physicist who Became a Historian for Philosophical Purposes

A DISCUSSION BETWEEN THOMAS S. KUHN, AND ARISTIDES BALTAS, KOSTAS GAVROGLU, AND VASSILIKI KINDI

"A Discussion with Thomas S. Kuhn" is an edited transcript of a tape-recorded three-day discussion—essentially an extended interview—between Kuhn and Aristides Baltas, Kostas Gavroglu, and Vassiliki Kindi. The discussion took place in Athens on October 19–21, 1995. The occasion was the awarding of an honorary doctorate to Kuhn by the Department of Philosophy and History of Science of the University of Athens, and a symposium in his honor at the University. The symposiasts included, in addition to the above discussants, Costas B. Krimbas and Pantelis Nicolacopoulos. The proceedings of the symposium, as well as this discussion, were published in a special issue of Neusis: Journal for the History and Philosophy of Science and Technology *(1997). The interview has been lightly edited again for this volume.*

K. GAVROGLU: Well, let's start from your school days, especially the kinds of courses that intrigued you, the kinds of courses you hated, the kinds of teachers you met . . .

T. KUHN: I started my education—or started going to school, which is another matter—in New York, in Manhattan. And I was there for

some years in progressive school, from kindergarten and then on through the fifth grade. Progressive school encouraged a sort of independent thinking. On the other hand, it didn't do much to teach subject matter. I remember at a time when I was probably already in second grade, my parents were getting very discouraged because I didn't seem to be able to read; my father held letters up for me and then I got on to it pretty fast. Then, as I went on to sixth grade the family moved out to the country, about forty-fifty miles out of New York to Croton-on-Hudson, and I went to a small progressive school there called the Hessian Hills School. It no longer exists. But it was particularly good in terms of teaching me to think for myself. It was a very left-oriented school; the woman who was its principal founder was called Elizabeth Moos. She was the mother-in-law of a man called William Remington—you may remember him, he was somebody who was ultimately put away for having been a Communist courier; this was something that came out during the McCarthy period. So, there were various radical left teachers all over, except that we were all encouraged to be pacifists. There was no Marxist training or anything of the sort; we were told by our parents that this was a radical school but we didn't quite see it that way ourselves. There I had one teacher who influenced me. I had several teachers who influenced me, but I had a math teacher called Leon Sciaky.[1] Everybody loved him dearly, he was very good at teaching mathematics. What I had with him was mostly elementary algebra but I had always been . . . not very bad but only mediocre in arithmetic, I made too many mistakes, I'd add things up and they wouldn't come out the same way two days in a row. I could do my multiplication tables but I've never really gotten past nine times nine. But suddenly turning to more abstract, with variables, I came alive for mathematics and that was in his hands. And I loved it, I was quite good at it, and that was a rather special experience. I was also rather good I guess in others. . . . There were no grades at the school; when it turned out that I was doing particularly well I was quite surprised—I didn't know that. I was there through sixth, seventh, eighth, and ninth grades—four years. I had a good social studies teacher. Here the radicalness of the school came out a little bit —we read as a group substantial parts of Beard's *Economic Interpretation of the Constitution of the United States,* and talked about it.

1. Leon Sciaky was born in Salonica; he wrote a memoir, *Farewell to Salonica:* Portrait of an Era (New York: Current Books 1946).

In my class there were six or seven people; it was that sort of very much hands-on education; hands-on in order to have hands-off education. And I thought that made a major contribution to my independence of mind.

A. BALTAS: Could you tell us some more things about the notion of progressive school: is it a particular kind of school?

T. KUHN: No. Progressive education was a movement which—to the best of my knowledge—really sprung from some proposals of John Dewey's. It emphasized subject matter less than it emphasized independence of mind, confidence in ability to use one's mind. So it was standard to say it doesn't teach spelling—there was very little drill. We started having French lessons; after three years I still couldn't remember any French—that sort of thing. But I was getting to be bright. I'm not sure that the Hessian Hills School—which I really think was an important formative influence—a small-scale education with very little in the way of set subject matter, a good deal of work by oneself, or what one thought one was doing by oneself. One thing I would say is that when I got to MIT, I found that by the time they were ready to graduate, students, many of them, had never written a ten- or twelve-page paper, the sort of paper I would try to assign. I wrote at least one twenty-five-page paper while I was in sixth grade, or seventh grade. So, there was more of that. And that flavor of work, that flavor of encouragement was I think very important to things that happened later. Now, that school went only through ninth grade. In fact, it often went only through eighth grade; but for our group it went on through ninth grade and after that I went away to boarding school. My parents were worried that I would find the transition difficult, so they sent me to a small school.

K. GAVROGLU: The fact that there were quite a few left people in the school, or the overall climate was left, was that something at that period which was looked down upon, or was it, among a certain group of people, something good?

T. KUHN: I'm sure there were circles in which it was looked down upon; I was more radical than my parents but they did not look down on it. On the other hand, it's worth getting me to say just a little bit about that. This was an age and an age group when people were beginning to join something that was called the American Student Union. A prerequisite for being a member of the American Student Union was willingness to sign the Oxford Oath. It's an oath that you would not fight, even for your country. And I remember talking to my father

about this, because I didn't really feel that I was happy about saying that I would not fight for my country. I wanted to be a member of the Student Union but I wasn't sure I could do that. And I remember his saying to me, "I have signed a lot of oaths and then later violated them. But I don't think I've ever signed an oath thinking I was going to violate it." And I took that very seriously and I did not join the Student Union. On the other hand, there was a meeting of students from numerous Progressive schools at my school, and I don't remember what we were talking about, what the official subject was, but I wound up being reported in the Peekskill, New York paper. I don't know that it was by name, but it was I who had stood up and said, "Who profits from our national possessions? Not you, not I, just the capitalists. Let the Philippines go." So, that gives you some flavor.

A. BALTAS: Approximately when did these incidents occur?

T. KUHN: Well, I left Hessian Hills in ninth grade in 1937 and this would have been one of the two years preceding that, and that gives you the general time scale. Then, I went to spend one year at a school in Pennsylvania called Solebury. That was all right. I didn't have special difficulties there, but the idea was that I should try that for a year and I could stay if I really liked it, but otherwise I would be moved to a still more advanced sort of preparatory school. I liked it all right, but I wasn't enthusiastic, I was missing the sort of interactions I'd had in Hessian Hills. So the next year I was moved to a school which I liked rather less, which really was a preparatory school—mostly a Yale preparatory school. It was a school called Taft, in Watertown, Connecticut. There I think there's nothing special to be said, about either of those. I mean I had good teachers and less good teachers, but nothing special except a very good English teacher in eleventh grade at Taft School. I remember we read Robert Browning a lot. That was important to me. Otherwise, the science teaching there was lousy. I remember a physics course which was taught by somebody who knew some chemistry but not much, and didn't know any physics, or not much. And I suddenly found myself suggesting—I can't have suggested that heat was the mean kinetic energy of the molecules but I sort of made up some kinetic theory and I brought it to the teacher, twice I think, and the second time he said, "Look, just wait till you're ready!" It was clear that was a sort of encouragement that I was not going to get because I don't think he knew the answer. And it was relatively elementary physics.

So, those schools gave me more formal training, more language—

although I was never any good, I've never been any good really at foreign languages. I can read French, I can read German, if I'm dropped into one of those countries I can stammer along for a while, but my command of foreign languages is not good, and never has been, which makes it somewhat ironic that much of my thought these days goes to language.

After those two schools in which I did well, I mean, let that be a matter of record, my grades were good, I went on to Harvard.

K. GAVROGLU: What was your father's profession?

T. KUHN: My father had been trained, and this is of some importance for me, as a hydraulic engineer. He'd gone to Harvard. Briefly, there was a joint program in which he took a five-year bachelor's and master's from Harvard and MIT, and he did that program. It was set up under a will and then the will was thrown out by the courts or something of the sort, so the program was divided again. He finished this five-year program, I think in 1916, and then the *Lusitania* went down and he was off to the wars. And he wound up in the Army Corps of Engineers and I think had the happiest, most productive period of his life. After the war—his father had died while he was away—he came back, sort of hung around Cincinnati to help his mother, and did some civil engineering which was not very interesting. Then, he got married. What he used to say, and I don't trust everything he said, was that he felt he couldn't compete with the younger people right out in hydraulic engineering, so he used his talents, which were very considerable, elsewhere. I was born in Cincinnati. That was his home—he brought my mother there, she was a New York girl. When I was six months old I was moved to New York with my parents. He went into what later became industrial engineering. He worked for a while for a bank, as somebody who investigated investment possibilities and gave advice both to the bank and for their clients. He was very active during the days of the national reconstruction administration, did stuff on the tobacco industry, he gave testimony before Congress and other things of this sort. But he was never, I think, the sort of success he had expected to be and under other circumstances might have been. And I think those around him thought of him—I think, but I've had only one person say this to me—as somebody who had not fulfilled his promise, who was very bright, who could have done much more, and that there was a real waste of talent involved. I think that's right. He would never have said that, but I think he felt it. I admired him greatly. I thought for many years that except for James Conant, he

was the brightest person I had ever known. He wasn't much of an intellectual, but he had a very very sharp mind. He used to catch me out all the time and that didn't do me a lot of good either.

K. GAVROGLU: Did he take an active interest in your education apart from trying to send you to good schools and keeping an eye on how you are doing? But did he actually get into the details of your education?

T. KUHN: No. He was the person who said, "Well, what can you say in French?" and I said "l'éléphant"! He was the one who was afraid I wasn't learning to read and held up letters or words—I don't remember—for a bit. But he was not actively involved except for points of that sort.

K. GAVROGLU: What about your mother in that matter?

T. KUHN: My mother wasn't actively involved either. But in a funny way, although she was not nearly as bright as my father, she was more intellectual. And sometimes in a somewhat flighty way—but she read more books. She did some professional editing. I was brought up to believe—everybody said—I took after my father; and this was fine, I admired him greatly, I was afraid of him and all the rest of it. I was also told that my younger brother was just like my mother. Later I realized this was exactly backwards. My brother was much more like my father, and I was much more like my mother. Now, part of realizing this had to do with the following. I, as you know, and I'll say more how I got there, I was a physics major. I was insistent upon being a *theoretical* physicist. But I loved working with my hands and I later built ham radios, I was never a ham-radio operator but I used to be in the old tube and battery days; I also did shopwork. And I could never finally figure out why it was that I had decided I had to be a theoretical physicist rather than an experimental physicist. And I finally realized that it was because theoretical physics was more nearly an intellectual activity and I was following my mother at this point, not my father. It came to me as a considerable shock to realize this and in the long run of course I have no regrets about how this worked out. But it was a puzzle to me, and it was quite explicitly a puzzle, as to why is it that I had this determination to do this.

A. BALTAS: Do you have only one younger brother?

T. KUHN: I've only the one younger brother. Well, after I finished at the Taft School, I went on from there to Harvard, which had been my father's university. At this point my life changed quite markedly, because what I have not said up to this point but should say now is

that throughout school I had really had almost no friends. I was isolated. I've been isolated since; but I was quite unhappy about it. I somehow wasn't a member of the group and I wanted terribly to be a member of the group. Harvard was big enough, and sufficiently intellectually oriented, and had a large variety of groups. You didn't have to be a member in order to be a part of one, and you could be a part of several. And I began to feel as though I was much more nearly part of something than I had been before, and I began to have happier social relations. It didn't go as far as it might have gone, but it went far enough to give me a very different sense of myself. Now, at Harvard—I had a lovely conversation with my father that you will be interested in, in the summer before I went to Harvard. I had really been good at high school mathematics. And I was a year ahead of myself, I'd done a year of calculus, with one other person at my preparatory school in my last year, I had had a year of physics that had been badly taught. I didn't hold this against physics but it had prevented my getting . . . so I talked with my father. There was no question but that I was going to major in science or math, or physics or math. Should I be a physicist or should I be a mathematician? And this will also tell you something that you know perfectly well, about how the situation has changed in the years since. This would have been in the summer of 1940. He said to me, "Look, if you prefer one of these, significantly, by all means do that. But if you are really torn between the two, I think you should probably do physics. Because, if you do mathematics, unless you wind up in one of the very good mathematics departments, all that's left is teaching high school or being an actuary for an insurance company. Whereas if you do physics, it's not very different but there are a few places like the General Electric Lab and the Naval Research Laboratory where you can still do research even though you are not in one of the research-oriented universities." So, I did physics.

K. GAVROGLU: And at that time you actually applied for a particular department. It was not that you were applying to the Science Faculty.

T. KUHN: No, at the end of your freshman year you announced your major. And that was a major in a department. So, [then] I enrolled in physics. Look, I had a strange experience in this respect in my first year and it's one that I think probably also had a formative influence. I think part of my emphasis on solving problems, puzzles, may come from, or have been affected or prepared by this. I'd always been very good. I'd been a straight-A student in school, that sort of thing. Some-

how, I was having trouble in my physics course. It was a rapid physics course, it was a two-year physics course for the future majors. And I think this was in my freshman year, I didn't do very well on the tests; in the middle of my first semester I had a C average. And I went to talk to the professor and said "Can anybody ever be a physicist with this?" and he encouraged me to give it a try, he didn't say "no" or anything like that, he didn't say "of course" either. But he told me what I should do is prepare myself better for the exams, and I started really learning how to do problems. We called them problems, I call them puzzles. And I got an A-minus in midyear and As from there on. But boy, it was perplexing not to be getting my A. I mean that had also a considerable influence somewhere along the line. So, here I was, in my first year I declared myself as a physics major. Of course the next year . . .

K. GAVROGLU: Before going to the next year, did you contemplate applying to another place, or was Harvard more or less given because of your father being already there?

T. KUHN: I think the answer is that Harvard was the place I wanted to go, various uncles and so forth had been there, what I knew of it I liked, I'd visited and liked it. I'm sure I applied to at least one or two other places to be sure that I would get in somewhere, and I remember I was away visiting when acceptance came in from Harvard and I was terribly pleased.

Years later, a study was published in which my class was a baseline. I had been something like one of 1,016 people admitted out of a total number of eligible applicants of 1,024. I mean, it got to be terribly competitive later. I thought it was terribly competitive then but it really wasn't. That was a myth. Now, my freshman year was '40–'41. In the fall of my sophomore year was Pearl Harbor, and we all sort of prepared ourselves to go to war or whatever. And in my case, since I was a physics major, the Physics Department turned itself largely into training people in electronics, and I had much more concentration in physics, but somewhat oddly distributed physics. There were some courses that every normal physicist would have taken that I didn't get till graduate school and not very fully then. And I turned myself to doing that as rapidly as possible. I got out in three years instead of four by going to summer school two summers. The result was that on the whole I got less of other subjects beyond science than I might have and that showed up in two ways in particular. In my freshman year I took another course that deeply influenced me, which

I should say something about. I decided I wanted to take a philosophy course. I didn't know what philosophy was, but I had a strange uncle who was a Spinozist. He was a private Spinozist. He collected, he wrote about Spinoza. The family did not for the most part like him, but I sort of liked him. And I wanted to find out about philosophy. There were some philosophy courses regularly open to freshmen and there was another course called History of Philosophy, which of course wasn't history, but it was detailed studies of, in the fall, Aristotle and Plato and in the spring, I think, Descartes, Spinoza, Hume, and Kant. And they gave me a hard time about getting into it, but I sort of insisted and I again had a hard time with this course, at the beginning; and I think that was partly not my fault. I had a very odd, although very well known section man, not the person who lectured. The person who lectured was a Greek, Raphael Demos, who's written about the scientists of the Greek Enlightenment. In any case, I had a section man, Isenberg; and he kept knocking me down, I mean, I didn't do very well on the quizzes, but I kept asking questions. One of them in particular that got to me terribly—he was teaching Plato and Plato's idea of the good. And now I'm not even sure that I will remember exactly which Platonic doctrine I am quarreling about, but it was the notion which got illustrated by the person who was a candidate from medical school and wanted to be a doctor. But the night before the exam he went out and stayed up too late and didn't do well on his exams and if he had thought about that and evaluated, he would not have done that and would have gotten into medical school. And I said, "Look, suppose he would have improved his *chances* of getting into medical school surely, but suppose they still weren't going to be very good statistically and suppose he really wanted to go out to that movie that night, why would it be irrational (although that was not the relevant word) for him to do that?" And the instructor treated this as a terribly strange question. He didn't understand it. I asked the same thing next week and he sort of led the class in laughing at me. It was an absurd episode. I mean, I know you are physicists, you see what the question was, but it was not a stupid question. And I guess in the middle of the year I got only a B− in that course. Then he left to go somewhere else and the course was taken over for the spring by another man. I said [just now that] I hadn't been doing very well: that was the course in which I really learned something more about how to study. I went back and made detailed notes and really pinned myself to it and I started doing better in the quizzes. And [then] I

asked to be in the honors section [of that course] and I was allowed to. I was quite fascinated by this stuff although I didn't understand it terribly well. But that was Descartes, Spinoza, Hume, and Kant. Spinoza didn't hit me very hard, Descartes and Hume were both in the current, I could understand them easily; Kant was a revelation. And I remember everybody gave a presentation in the section meeting, and I gave a presentation on Kant and the notion of preconditions for knowledge. Things that had to be the case because you wouldn't be able to know things otherwise. It [my presentation] was thought very well of, but it just knocked me over, that notion, and you can see why that's an important story.

K. GAVROGLU: Could you elaborate on it a little?

T. KUHN: Oh, it's an important story because I go around explaining my own position saying I am a Kantian with moveable categories. It's got what is no longer quite a Kantian a priori, but that experience surely prepared me for the Kantian synthetic a priori. And I do talk about the synthetic a priori. Well, I had thought I would take more philosophy while I was an undergraduate. I thought also that I'd take more literature as an undergraduate. I had not much liked this English literature survey that I took in my freshman year. I thought the professor was not treating us with very much respect—he was making jokes, not at us but at the things we were supposed to be studying. But I had a very good American literature course in my second year, and I would have gone on with that; I mean, I liked the study of literature and I wanted to do more philosophy. But here I was; we were at war, I was already well into my sophomore year, I was going to be there only for one year after that, so I didn't go on with either of those things. Then I graduated a year ahead of time and that was not uncommon at the time. One other thing that I did then that I should mention. I went out for the paper, the Harvard *Crimson*. In the beginning of my sophomore year I had a roommate who had already gone out in his freshman year and made the news staff; I went out for the editorial board, and I got to be the head of the editorial board in my last year. And that was the year we went into the war, so there I was writing editorials about our presence in the war and what Harvard should do and so forth. I even then had the problem that I have had ever since, of finding it very hard to write. So that it would always take me forever to write an editorial, and I never got that journalist's ability to sit down and turn something out. It was more than a little of a disadvantage.

K. GAVROGLU: Was it an elected position?

T. KUHN: Yes, it was an elected position and there was a competition for people who were interested. The senior board picked me as the candidate. I was part of the slate. But there was then a good deal of opposition to the slate from some of the members; and I remember sitting downstairs with the other person who had been nominated by some of the members, while the discussion went on upstairs, wondering who was going to get it—this was not unheard of, but it was not the usual thing either. And it tells you something about the extent to which I had only been partially socialized by my transition to Harvard and yes, I did get the job. But it was a strange experience.

A. BALTAS: You mentioned writing and you are considered one of the very few people in the business who have a sense of writing. And because you mentioned a difficulty in writing, this fits well with the picture I have of you because writing is something difficult. It's a good thing you did not succumb to the journalistic style. The question is your relation to writing.

T. KUHN: This is one my mother had a lot to do with. She did do some editing and she did read. As a kid, and still, I don't write letters to people, I write the business letters I have to write. I am a terrible correspondent, and I used to get in some difficulty about it. My mother once said to me, "You can say anything you like, but be very careful what you write down." My mother said a lot of things to me, not all of them wise, but all of them stuck. My mother was an extraordinarily tactless woman. She couldn't *not* say what was on her mind, and what was on her mind was not always very well thought out. I remember when I first started going with a woman; I had not had the normal number of dates by the time I was out of graduate school, and there was a woman I saw more than occasionally. My mother, who had not met the woman, saw the two of us on a New York street and just said to me a few days later, "I saw you and G . . . , and she's not the right person for you."Agh!

K. GAVROGLU: Before we get to the graduation. You are going through the undergraduate years when there was a war in Europe. And American society, at certain levels, is a torn society, there are very high emotions as to how America will deal with the situation. How do you relate to this situation, and how is the situation among the people at the university? Is this an issue among the people at the university? Obviously as time nears, it's an issue, Pearl Harbor is an issue, but what about before, what about your own position?

T. KUHN: Look, I'm surprised to find out how little I remember. I will tell you something about it. I indicated that I was quite radical through ninth grade at the school. We used to march in May Day parades, solidarity with labor. After I went away to school, that rather dropped away. I maintained my liberal convictions, but I was no longer an activist at all and I've never been an activist since. It embarrasses me sometimes [that I haven't]. So far as my own attitudes and my family's were concerned, we were very glad when Roosevelt made the arrangements to help the British; we rather thought that America should join the war; but that wasn't a happy feeling. Remember this is a Jewish family—not very Jewish, but I mean we were all certified Jews. Non-practicing Jews. My mother's parents had been practitioners, not Orthodox practitioners. My father's parents had not, the Cincinnati branch of the family. So, it was not a big issue but it undoubtedly put us more ready to go after Hitler than we might otherwise have been. And the fact of the matter is, I'm sure I was around people who felt very differently, but I don't remember that at all. I feel as though all the people I was around felt more or less the same way. Then of course it became irrelevant after Pearl Harbor.

K. GAVROGLU: Were there no voices even among the students not to get into the war after Pearl Harbor?

T. KUHN: If there were I sure don't remember them, and I don't think there really were.

K. GAVROGLU: Could you tell us some things about courses at Harvard? Which courses did you like more, and some of the teachers at Harvard that had at the time one of the best physics departments, not the best I guess. Columbia maybe was better.

T. KUHN: Or Chicago. Harvard was not known for a particularly good physics department.

K. GAVROGLU: Not even at that time?

T. KUHN: No. I think Harvard physics began to get good only after the war. John van Vleck was there then, but I didn't study with van Vleck, I didn't know van Vleck until later. Wendell Furry was the person who I had for that freshman course I spoke to you about. I liked him, he was a good teacher. Street took the second year of that course. I never got to know him well, but he was a known physicist for spectroscopy.

K. GAVROGLU: You had nothing to do with Slater, he was down at MIT.

T. KUHN: Yes, I had nothing to do with him. He became a name to me and I met him during the Quantum Physics Project, but no, I had

nothing to do with him. I had more to do, before long, with the people who were teaching electronics. [Leon] Chaffee, and a man called King [Ronald W. P. King] who was very good on antenna theory. None of these made a deep dent. Chaffee was an incredibly bad teacher, King was a very good teacher. In mathematics I was a year ahead, but I didn't quite dare go into the second-year calculus course, so I took the first-year calculus course and found it so easy that I didn't go to class, I used to do my problems and send them in with somebody else in the course. I don't mean that I never went to class, but I very rarely went to class after the first few weeks, and I did fine. The second year I moved a little bit ahead of my level in the second-year course. I mean, I sort of skipped directly into the second half of the second-year course. It was when I went into the third-year course that suddenly things were terribly hard for me. It was taught by George Birkhoff, a famous mathematician and one of the worst teachers you can imagine. And we were doing multiple integrals and partial differentiation, and I couldn't quite see what was going on. I did all right, but never really felt I was in control of the material. I had a friend who was very good, and when we were nearly in the usual level the next year I said to him, "How are you doing?" and he said good. I told him I had a lot of trouble understanding the course, and he said, "How could you have, it was just what we've all done before, only with more variables." And I said, "Oh," and something clicked and it all fell into place. And although I still don't always get multiple integrals right, yes, he was right. That's what it was and Birkhoff kept me from seeing it.

K. GAVROGLU: You didn't say much about theoretical courses.

T. KUHN: You've got to remember, I was there only three years. The first and second year my main physics courses were this two-year sequence, which is a tough sequence, and a good sequence. I can't remember what else I may have taken, I don't remember exactly what I took in physics in my third year. What I did do was to take a lot of electronics and get physics credit for it. I took some electromagnetic theory, I'm sure; I took a course in electricity and magnetism, which was all right, I wasn't terribly excited by it. It was not yet really Maxwell: Page and Adams, is that a book, do you remember that?[2] It was that level. I can't remember—I took a course in thermodynamics with

2. L. Page and N. I. Adams, Jr., *Principles of Electricity: An Intermediate Text in Electricity and Magnetism* (New York: Van Nostrand, 1931).

[Percy W.] Bridgman when I was in graduate school; I may have taken a previous undergraduate course in thermodynamics, in which case that would have been with Philipp Frank, but I'm not sure whether I did or not. I've always rather liked thermodynamics; that sense of a subject which is largely mathematical but which gives you important physical consequences is a strange, tasteful experience.

K. GAVROGLU: What about relativity courses?

T. KUHN: I took a relativity course in graduate school. You have to remember I had very little physics as an undergraduate. I didn't have an optics course which I would ordinarily have had. I'm not sure I had a thermodynamics course; I probably had an intermediate mechanics course.

A. BALTAS: What about history courses, except the one you mentioned of philosophy, humanities in general?

T. KUHN: I took one history course. History was not a thing I thought of myself as being very fond of, it was a course in summer school in British nineteenth-century history. Why I took that I don't know, the teacher was well liked, and I liked him myself, but the subject matter wasn't going to do anything for me. In summer school also I took a course in political science with Max Lerner, who was also, in some sense, an older friend of the family's. But I took very little, as you see, that wasn't either electricity or electronics and I can't now tell you just what it was. On the other hand, I was very busy with the paper [the *Crimson*] and my friends were people who were on the whole in literature or one thing or another. Mostly they were not physicists, mathematicians, engineers, although there were some of those. So, to my considerable surprise I was in my sophomore year elected a member of something called the Signet Society, which was not really one of the Harvard clubs, but it was sort of an intellectual discussion society, which had lunches and so forth. And then in my senior year I was president of it. And I was probably the only physicist who was ever president of the Signet. Although I was terribly over-concentrated, though I didn't take very many literature courses (I guess I had only two years, one of English literature which I didn't like much, and one of American literature with Matthiessen and Murdock, two very famous Harvard professors whom I admired greatly) I was known for this combination. And that is something to hold onto, because it's going to play a role later.

And then I graduated and I went to work for something called the Radio Research Laboratory. Radio Research Laboratory was physi-

cally at Harvard, in the north wing of the biology building including two extra wooden-structured floors built on top of the north wing. I was in the theoretical group and my boss was van Vleck. We were doing radar countermeasures. King was now there designing special antennas including one rotating antenna that flew on a plane and was supposed to be able to triangulate radar sites. I was largely cooking standard formulas (whose derivations I couldn't have begun to understand, or at least I didn't think I could, and I wasn't given time to find out) on doing radar profiles as a function of distance—there is a standard formula about the square root of the height of the two antennas and so forth with various allowances for propagation conditions and so on. I would produce graphs showing when you would pick up such and such a plane, [and] do maps. I think I did one set, or did some that had to do with radar coverage of Kamchatka. How close could the Japanese get or we get to the Japanese. I'm not even sure now which it was.

A. BALTAS: Was this a job, I mean were you hired there?

T. KUHN: Yes, I was hired there because of my degree, my training and because anybody with that sort of training was needed for the war. And it got me a deferment: I was not drafted under these circumstances. That's not why I was doing it, but I never had any regrets. I mean, it isn't that I wanted to be drafted and that they wouldn't let me be. I started that in the summer or fall of '43 and I was there about a year. They had an advanced base laboratory at Great Malvern, in England, and after about a year, I think, I asked to be sent over there; I had never been abroad, and I went there, and I must say it was my first plane flight. I got on a plane at LaGuardia, we landed once somewhere in Nova Scotia or Iceland, I think, and then came down in Scotland, in Glasgow. I had never been in a plane, and there we were! I kept reciting from Saint-Exupery's "Night Flight"—it was thrilling! And so then I was at Malvern for a while, and then sort of farmed out to a technical intelligence unit at the United States Strategic Air Force Headquarters which was in Bushy Park outside of London—I lived in London. And that was hard. I was having trouble, sort of fitting in and getting interested in what I was being asked to do. But it was all right and there was some fun involved.

Then, I went from there into uniform as a civilian, and went to France, or I went to the Continent—again for the first time I'd ever been there. I was in uniform so that if I were captured I would not be held as a spy. I went to look at radar sites and to bring back further

information about them. That was one of the most fantastic experiences of my life. Because I got on this plane and landed at the base of the Cherbourg Peninsula. I was supposed to be going to the submarine pens at Rennes, where there was supposed to be a major German radar installation. Now, this was just during the breakthrough, with [General] Patton rushing across France, and nobody knew quite where the army was. But I was supposed to join a group that was already there. And I was in a command car with a captain who'd been in this group, he knew them, he was going to come there with me and then he was going on to—I forget where. We got there, the group wasn't there any more. Nobody knew where they were, but the betting was pretty good that they had headed out for Paris and that we could find them there. Nobody was quite sure what was going on in Paris. But we started out and it then turned out with the driver and this captain and I, I was the only one who had ever studied any French. And I hadn't done that for a long time and I kept trying to remember my French. And I would say, "soldier-soldat," no that can't be right, that's German. So, we drove and we drove and I guess we stayed overnight somewhere on the way and then the next morning early we were up and driving again. And another one of those sights that I will absolutely never forget, we were driving across a plain and suddenly coming up over the horizon there was something like this—We were watching it, it was getting higher . . . Chartres! It was those two odd towers of Chartres Cathedral. We drove right in the town and never got out [of the car], kept on around, but wow, was that exciting! And as we came out of Chartres, we began to pass a convoy that was on the road and we got around the convoy, got out ahead of it, started into Paris, somehow we got to the Petit Palais which turned out to be the place where this group I was supposed to join was set up. They were found. And there we were. We'd been there for about an hour, when this convoy started to come down the Champs Elysées. It was de Gaulle entering Paris! And suddenly there was rifle fire from somebody on the roof of the building across the street. Somebody fired, some member of the milice who was shot down. And there was still fighting out at Le Bourget, the other side of Paris. So, it was an exciting time.

Oh, I'm telling you stories of my life, this is quite irrelevant to anything that happened to me later, except that it may tell you something further about me which will not surprise you.

I was working with, teamed up with, an RAF radar man; he was

called Chris Palmer. He and I were told to go up the Eiffel Tower and see what sort of installations there were up there. So, we started out, we got to the Eiffel Tower and we talked to somebody there who told us, "L' ascenseur ne marche pas." We said, "Gee, we'll have to climb it" and they had wire wrapped around the stairs at the bottom level and we went up over the ladder—over the wire, and on to the stairs, and we climbed up to the third level—and here came the elevator. It stopped and invited us aboard. And God damn it, we got in. I've always kicked myself I've never climbed the Eiffel Tower!

K. GAVROGLU: Then you go back to England?

T. KUHN: I was there [in Paris] for a few weeks, and they were exciting weeks. I went back to England for a while and then I came back again [to France]. The transformation of the French, in the interim! I was stumbling over my French that first time, and people kept telling me how well I spoke French; and there was dancing in the streets, and so on and so forth . . . I got back there, not more than about six weeks later, I think, they wouldn't talk to you! Just a total change. And at that point I was assigned to the Ninth Bomb Division in Rheims as an advisor on radar countermeasures. There was a . . . what did you call it . . . industrial engineering group?—that's not quite what it was called. These people who applied mathematics and science to strategic and other such problems in a rather unsystematic way. At that point it was often called the main advantage of having such things that they could talk back to the general, as nobody within the army structure really could. So, I did that and then later still, as we went on into Germany, I was sent on again looking at radar installations, trying to talk to people in Germany and find out what had been going on there. Of course I didn't find out very much; but I saw the flattened city of Hamburg, I'll never forget it. I also saw Saint Lô the day we arrived in France and I'll never forget it. None of this has a lot to do with what happened later, except that as all of this was going on I increasingly realized that I was not all that interested in radar work. This gave me a somewhat bad taste of what it was going to be like to be a physicist. It was of course totally misleading. A number of my classmates, in somewhat similar positions, instead of going on to radar and radar countermeasures, wound up at Los Alamos. And I don't think it's out of the question that if I had gone to Los Alamos I might still be in physics. I doubt it, I mean I think there were too many other factors involved, but certainly an increasing distaste—which is already too strong a word—but an increasing number of

doubts as to whether this was for me began to pile up. I will find it very hard to weigh the various factors that went into this, that went into my decisions, but that was certainly a part of it.

A. BALTAS: Let's talk about your starting doubts about doing physics. Were the doubts related to the war?

T. KUHN: I had been a "physicist." I put that now in quotation marks, because in some sense I wasn't trained to be a physicist in view of what had happened, but it was leading into this, and I was finding it fairly dull, the work was not interesting. I still believed in science and I remember there was somebody with whom I used to talk about the need for the redesign of science instruction, and one thing or another. But I was by no means sure, I was beginning to get doubts as to whether a career in physics was what I really wanted—particularly in theoretical physics. And I guess it may well be at that time, though it may be later, that this question, why did I insist on being a theoretician? began to arise. But those doubts weren't all that big, or anything of the sort, but they were there. I mean, I had been frustrated by not having gotten back to do some philosophy. So, in 1945, shortly after VE Day, I returned to the States, and there I was back at Harvard again, because the war wasn't over; there was some question at that time, whether we were going to be shipped on out to Japan. But it wasn't so long before that was over too. Meanwhile, in that intervening time (I mean, I guess I came back in late spring/early summer of '45, I think) I went back into the lab, and when the fall came, the fighting was still going on in Japan, though it looked as though it would not be going on forever, and I got permission—because work was slowing down at the lab—to sign up for a physics course, or for two physics courses, while I was still employed by the laboratory. One of the courses that I then took was group theory with van Vleck. And I found that somewhat confusing. I had a first course in quantum theory while I was an undergraduate—I have to have had. Group theory is interesting stuff, and although I never felt that I was controlling it, that sense of mathematics giving physical results was very appealing. Van Vleck was not a terribly good teacher.

Then the war was over in Europe [and I signed up to do graduate physics at Harvard]. I can't remember quite when I made this deal. Because I had taken so much physics as an undergraduate, because I was back at Harvard where I hadn't planned to go to graduate school—but it would have been silly not to, given the continuity—it would have cost me at least a year to pick myself up and go some-

where else. I petitioned the department to let me use half of my first year outside of physics, to explore other possibilities. And it was philosophy in particular that I had in mind, and I took a couple of philosophy courses. Well, so here I was first year in graduate school, I had permission to take half of my courses in philosophy. Well, I did that for a semester, probably the fall semester of '45, and unfortunately it was the case that most of the good people who would have interested me, in philosophy, were somewhere else. They hadn't gotten back yet. I took two courses, and I realized that there was just a lot of philosophy I hadn't been taught, and didn't understand, and was not finding it very palatable to pick up this way. I didn't know quite why people were doing the things that they were doing. And I fairly rapidly decided, yes, I was interested in philosophy, but my God, I was a graduate, I had been through war in some sense or other, I couldn't go back and sit still for that undergraduate chicken-shit and go on from there. So, I decided I'm going to take my degree in physics. But it also was clear, and became increasingly clear, that I was not being very much fulfilled by my graduate physics teaching. And that was not as distinct from the undergraduate. Partly, I think, although I continued to do well enough, I was no whiz kid any longer, and it wasn't clear that I was good enough to do it . . . I mean, to really shine. I surely could have been a professional physicist . . . In retrospect I think I was wrong. In retrospect, in view of what I've learned putting myself through stuff as a historian of science and learning more about what the career was like, I think I would have been a damn good physicist; I do not think I would have been Julian Schwinger or you know that first level, but I think I could have done quite respectable work. Whether I'd have liked it much I don't know. But certainly questions about my ability had something to do with a growing disenchantment. A sense of not being focused—there was this whole thing about having been president of the Signet, and liking literature and philosophy—and now, if I was going to go through graduate school successfully and on, I really had to focus my attention in one spot and give my full energies to it; and I found that hard to do. So, my grades remained entirely respectable but I think I began to get some Bs, that sort of thing, and I was very much of two minds; partly it was simply I didn't know what I would do if I didn't do physics. I was looking and thinking about other things, none of which turned me on all that much. I used to talk to my father about this—science journalism or something else of the sort. And then, of course, I had

this extraordinary experience which I've talked about, of being asked by Conant to assist in his course. Who the hell wouldn't have taken the chance to work with Conant for a semester?

K. GAVROGLU: Before we get into Conant, did you have any people from physics encouraging you for your searches, as it were, in philosophy, or any other place outside physics?

T. KUHN: No. They gave me permission and they understood, so they knew I had something less than the ultimate in dedication; but I came back after one semester and so forth.

A. BALTAS: I'd like to ask you a rather strange question. Did you have when you started, you decided to do physics, in graduate school or afterwards, was there any kind of utopian dream, in the sense of "I discover the secrets of nature," "I do this for something big independently if I manage to do it or not," Or was it just related to conditions, to work?

T. KUHN: No, I think initially—you know I would have been very glad to get the Nobel Prize—I certainly wanted fame in some sense. I don't remember it that way, but it has to be the case.

K. GAVROGLU: You studied solid-state physics with van Vleck, which obviously was not one of the trendiest things to do. So, were you interested in the subject itself or in working with van Vleck?

T. KUHN: It was neither. By the time I decided on a thesis topic, I was quite certain that I was not going to take a career in physics, and I didn't want to prolong my time in graduate school. Otherwise I would have shot for a chance to work with Julian Schwinger, but there were a lot of things I didn't know, and would have had to study, if I were to do it that way. I wanted the degree—it would have been stupid to have gone that far and not to get the walking papers that would result. But I didn't want to do the amount of extra training. Now, in practice that degree with van Vleck took a long time, I spent a lot of time punching buttons on a calculating machine. But it was that that motivated the decision.

K. GAVROGLU: And that decision also led you to van Vleck . . .

T. KUHN: Yes, I mean, I liked van Vleck, but he's not the person I would have worked with if I had not wanted to finish this off. I think it would have been Schwinger or I would have tried to be with Schwinger.

K. GAVROGLU: Schwinger at the time was what? A young, bright professor doing things you considered fundamental?

T. KUHN: I first saw Schwinger while I was still in the Radio Research Laboratory. We used to go down to MIT to the Radar Laboratory

for lectures sometimes. He gave a lecture on integral calculus of variations and wave guide computations. I didn't really get it all and I wasn't much interested in doing wave guide calculations, but there was a degree of elegance in the presentation and a degree of control over deeply technical material that was just fascinating to watch. And then I think I had electromagnetic theory with him and maybe audited quantum theory courses or something like that afterwards. And he was a phenomenon, no question.

K. GAVROGLU: Okay, Conant.

T. KUHN: I was asked by Conant.

K. GAVROGLU: How did Conant find you?

T. KUHN: Remember, I had been an undergraduate editorial chairman of the *Crimson* paper at a time when we were entering the war. So, I had gotten to know not Conant, who was away all the time, but the Dean of the Faculty. And when the general education report came out, which Conant had set up to have done for him, I was asked by the Dean of the Faculty to write a précis of the report for the alumni bulletin; and also I was one of several people writing comments on the report; I was the student among those writing comments on the report. So, I was known for this range of interests. Who fed my name to Conant I'm not sure—there are various people that it could have been. But I had a reputation as the physicist who was president of the Signet Society, there were various things of that sort in my record. I was one of the two people Conant then asked to assist him. First time he gave this course out of that little book called *On Understanding Science,* which had been the Terry Lectures at Yale. I accepted with alacrity; and I've never quite forgotten that first time I met him. Here I was, not finished with my physics thesis and being immune to this sort of material—I have by then read the page proofs of *Understanding Science*—being asked to go out and do a case study on history of mechanics for this course? Wow! And that was also Conant; he would do that sort of thing. So, that was the first time—I had sat in on some lectures of Sarton's as an undergraduate and found them turgid and dull. And I was not in my bones a historian; and I *was* interested in philosophy. But I had no real interest in history, and this Aristotle experience[3] was terribly important. Conant, in case histories of his own and in his teaching, never I think saw to the extent that I did the need to say what people had believed *before*. He would always

3. See essay 1, "What Are Scientific Revolutions?" in this volume.

start in more or less with the beginning of the work. There would be something about it, but there was very little preparation for getting to the person. I always felt you had to do more; and that meant you had to do a stage set, within another conceptual framework, in order to get at these things. And that was what this did for me. But the main thing is, it didn't really get me *interested* in history of science; and there are those who feel, and feel with some justice, that I never really did get to be a historian. I think in the end I did get to be a historian, but of a rather special narrow sort. I used to think—forgive me—that with the possible exception of Koyré, and maybe not with the exception of Koyré, I could read texts, get inside the heads of the people who wrote them, better than anybody else in the world. I loved doing that. I took real pride and satisfaction in doing it. So, being a historian of *that* sort was something I was quite willing to be and got a lot of kicks out of being, and did my best to teach other people to do. I'll come back to that. But my objectives in this, throughout, were to make philosophy out of it. I mean, I was perfectly willing to do the history, I needed to prepare myself more. I wasn't going to go back and try to be a philosopher, learn to do philosophy; and if I had, I'd have never been able to write that book! But my ambitions were always philosophical. And I thought of *Structure,* when I got to it finally, as being a book for philosophers. And, boy, did I get fooled for quite a long time!

K. GAVROGLU: So you started preparing for the course.

T. KUHN: So I started preparing for the course, that got me to the reading of Aristotle, and I taught with Conant in the course for one semester. At the end of that time I knew what I wanted to do. I wanted to teach myself enough history of science to establish myself there in order to do the philosophy. I went to Conant—I had developed a relatively warm relationship with him, to the extent that people had warm relationships with him. He was a quite reserved person, rather cold—not so much cold, he was very reserved. I asked him whether he was willing to sponsor me for the Society of Fellows. Officially that's a question you didn't ask; but I felt able to, and he did, and I got in. I had to postpone my entry a little while I finished my thesis. And in some sense or other, after that I never looked back.

K. GAVROGLU: Before that . . . Of course the atomic bomb had already been dropped in Japan. What were your feelings and of the people

that you were directly connected with, people on the verge of Los Alamos? Did you have any relation to Los Alamos?

T. KUHN: I basically had no relation to Los Alamos although I did have some relations to a few people who had some relation. These were high-level, but not necessarily very old, government consultants who flew around and touched base on a lot of these things. And one of them told me about the Los Alamos project. And indeed the context in which he did that was when the V2s began to come down in England, the fear had been that they were carrying atomic warheads. Of course, they weren't, the Germans weren't ready, they were not going to be ready in anything like that time, but that was the fear I didn't know about—that people had. And a man—I think his name was David Griggs, he was a knowledgeable figure—told me about the atomic bomb. So I remember I was in a train going to Washington, to go I guess to the Naval Research Laboratory for some tests, or something of the sort, and on the platform of the Pennsylvania Station in New York I looked out and saw this headline on the paper. I knew what it had to be, and it was the atomic bomb. I guess I would say, yes, I knew that there were people who felt that we should not simply have dropped it, that we should have demonstrated it, but the general feeling was: Look, we had to get out of this. And I was sympathetic with those who felt that maybe we should have taken another technique. But I didn't know enough about it to really feel any great convictions on that score, or any great sense that it would have worked; and probably it wouldn't have worked. So I'm not one of those who has been terribly upset by the behavior of the government. I don't know that I knew anybody who was deeply, deeply upset, although there were plenty of people who admired that group and agreed with them and wished they had been able to do more than they had. And I guess I would have associated myself with them, but it wasn't a big issue for me, I mean I could suppose that the time had come to get this over with.

A. BALTAS: I'd like to come back to what you said before. What did philosophy mean at that time for you?

T. KUHN: I will tell you a story. I had a classmate, also on the *Crimson* at a time when I guess I was in graduate school, I think he was also taking a graduate degree. He got married and to my surprise he asked me to be one of the ushers at his wedding—I'd never been an usher at a wedding, but I was. I met his bride, of whom I got very fond,

but I met this woman—G . . . , the one my mother told me wasn't for me—she was a bridesmaid and it was that that established the relation between us. After a while she gave a cocktail party in New York for me, to meet some of her friends. And I went, and I got to talking to a very beautiful—not so much beautiful but very striking, buxom, well-turned-out woman. I don't know what the conversation was, but suddenly as happens occasionally all the voices in the room dropped and I was heard saying (including by me), "I just want to know what Truth is!" So, that's what it meant to me. And this may well be before I was associated with Conant. I can't date it quite that accurately, it certainly can't be long after. I may already have been in the Society of Fellows, but I think perhaps not.

A. BALTAS: It is very well connected to the Aristotle incident. They connect very well together.

T. KUHN: Yes, and it could have happened either before or after. My Aristotle experience certainly made it problematic, and I'm not sure quite what the problem had been earlier, if this was before that. So I really can't give it to you in terms that will be developmental. But from an early stage, that tells you something; I don't mean that that was the single goal, but that's what it meant to me to be studying philosophy or to have philosophical ambitions, that was the type of thing it meant to me.

K. GAVROGLU: It is not uncommon for a lot of people to start to search for truth through physics first and then going to philosophy as a next stage.

T. KUHN: But remember, when I said that, I wasn't saying that I want to know what is true; I was saying I want to know what it is to *be* true. And that's not something that one gets to through physics.

K. GAVROGLU: No, No. You are right.

T. KUHN: We stopped [the previous tape] just as I had decided that I was now going to move into history of science with an eye to doing something philosophical with it and I asked President Conant to recommend me for the Society of Fellows. He did recommend me, I was elected to the Society of Fellows, that is a three-year term, of which I didn't really, as it turned out, get all of the three years. I had to take some time at the beginning to finish my thesis and to publish some articles out of it, at least to finish the thesis. But in November of 1948, I guess, I began to work in the Society of Fellows. It was terribly important to be there, because it relieved me of other responsibilities and what I was trying to do was to train myself to be a historian

of science. And it was partly just reading, and I didn't mostly read history of science. It was in those years—I mean, I don't remember what got me to it, I guess in reading Merton's thesis[4]—somehow or other, I think it was there that I discovered Piaget. And I read a good deal, beginning with his *Mouvement et vitesse*.[5] And I kept thinking, my, these children develop ideas just the way scientists do, except—and this was something I felt Piaget did not himself sufficiently understand, and I'm not sure that I realized it early—they are being taught, they are being socialized, this is not spontaneous learning, but learning what it is that is already in place. And that was important.

A. BALTAS: Can you tell us a few words about the Society itself?

T. KUHN: The Society itself in those days, and it's still more or less the case, I'm not sure how much it has changed, was a group of twenty-four fellows, usually, eight elected each year. And a group of senior fellows who did the election. The whole group dined together every Monday evening. And the dinners were quite good dinners, so that there was a certain element of ceremony as well as of sociability. The fellows also lunched together I think twice a week and that was less ceremonial but it brought them together and the amount of interaction varied quite a lot. I don't even quite remember the people who were part of my group, but I don't think there was anybody that I talked to in the Society who was terribly important to my development, although the talk was good and I got some sense of support and so forth. A senior fellow at that time was Van Quine. And this was just—I don't remember the dates—this was just about the time that his analytic-synthetic paper was coming out.[6] And that as I said the other day had a considerable impact on me because I was wrestling already with the problem of meaning, and at least to discover that I didn't have to be looking for necessary and sufficient conditions was extremely important. Quine has been important to me for that piece, and for the problems that *Word and Object*[7] presented me in trying to figure out why I was so sure it was wrong (outside the fact that

4. R. K. Merton, "Science, Technology, and Society in Seventeenth Century England," *Osiris* 4 (1938): 360–362; reprinted with a new introduction by the author (New York: Harper & Row, 1970).

5. J. Piaget, *Les Notions de mouvement et de vitesse chez l'enfant* (Paris: Presses Universitaires de France, 1946).

6. W. V. O. Quine, "Two Dogmas of Empiricism" (1951). Reprinted in *From a Logical Point of View: 9 Logico-Philosophical Essays* (Cambridge, MA: Harvard University Press, 1953).

7. W. V. O. Quine, *Word and Object* (Cambridge, MA: Technology Press of the Massachusetts Institute of Technology, 1960).

there isn't much of an argument), where he was going off the rails. We can come back to that later. I've only really been able quite recently to formulate it, in a way that I find satisfactory. But, for those three years in the Society I was beginning to read my way into the field and establish myself; and also doing something else, which I think I *should* put on the record. I've said something yesterday about . . . until I got to Harvard not having had many friends; I was clearly a neurotic, insecure young man. It was also the case that somehow or other my parents, my mother I think in particular, worried about this: I was not having dates and that sort of thing. My relations with women were almost nonexistent. But that was in some part because my environment was a male environment. The result was that I was persuaded, without a lot of difficulty, to go into psychoanalysis. I'd had some experience as a child with child psychiatry which I did not think very much of and don't carry fond memories of. The analysis in the Harvard years was with a man I, in retrospect, hate, because I think he behaved extremely irresponsibly with me. He used to fall asleep and then when I would catch him snoring he would act as though I had no business being at all angry or upset about it. On the other hand, I'd previously read Freud's *Psychopathology of Everyday Life*. I do not for a moment like the theoretical categories that he introduces, or feel that for me, at least, they have any force. The *technique* of understanding people and enabling them to understand themselves better—I'm not sure that it produces real therapy of any sort—but it sure as hell is interesting. And I think myself, I'd have great trouble documenting this, but I think myself that a lot of what I started doing as a historian, or the level of my ability to do it—"to climb into other people's heads," is a phrase I used then and now—came out of my experience in psychoanalysis. So in that sense I think I owe it a tremendous debt. I think it's too bad that it is getting the very bad reputation that it's getting these days, although I think it richly earned it; but I think what gets forgotten is that there is a craft, hands-on aspect to it, that I know no other route to, and that is intellectually of vast interest.

The psychoanalysis must have been mostly before I got into the Society of Fellows, because it terminated when two things happened: I got married and my psychoanalyst moved out of town. At that point I finished my thesis which was typed by my then wife. That was a marriage that went on for just about thirty years, which produced three lovely children, whom I find immensely rewarding.

I think I produced nothing while I was in the Society; I did a good

deal of reading. And also of course, as I said, in the first year, I missed the beginning because of the need to finish my thesis. The second year I had unencumbered. And then, in the third year, Conant decided to stop giving the course and he invited Leonard Nash, a chemist and famous teacher, and me to take it over. Leonard Nash I had not known before. It was good for me, I could not have refused and at that point, knowing that I was going to have very little time the next year, my wife and I went off to Europe. It was not uncommon for members of the Society to spend a last year in Europe to do their research. We went on a two-month trip to meet colleagues abroad; I wasn't even ready for them, because I hadn't got far enough into the history of science. But we were in England for a bit and in France for a bit. I don't think we went further in Europe than that.

K. GAVROGLU: Let me ask you something. You told us that what you really felt was challenging was philosophy and then you get into the Society of Fellows and you are up to your neck with the history of science. Obviously that had something to do with the course that you were teaching, of course, but was it only that?

T. KUHN: I had made that attempt to investigate going into philosophy immediately after the war when I first came back and got into graduate school and I decided I wasn't going to go back to fulfill undergraduate philosophy. And in certain respects I'm extremely glad I didn't, because I would have been taught things that would have given me a cast of mind which would have, in many ways, helped me as a philosopher, but they'd have made me into a different sort of philosopher. So I had decided, when I applied to the Society, to do history of science. My notion was, and my application indicated, that there was important philosophy to come out of it; but I needed first to learn more history, to do more history, and to establish myself professionally as a historian before I let the cat out of the bag.

K. GAVROGLU: And what were your relations with the Department of History of Science at Harvard, which was a well-established department?

T. KUHN: No, it wasn't. Look, at this time there was no Department of History of Science at Harvard.

K. GAVROGLU: What about Sarton and his group?

T. KUHN: There really wasn't a group; I mean Sarton drove people away who wanted to study with him. He would tell them, "Sure, but you'll have to learn Arabic, and Latin, and Greek" and so forth, and very few people were going to do that.

K. GAVROGLU: Why didn't you associate yourself directly with Sarton then, since you wanted to do history of science?

T. KUHN: Look. My notion was that there was a sort of history of science to do that Sarton wasn't doing. I mean, I would not have said then the sorts of things I would say now about him, and I recognize that in some very important sense he was a great man, but he certainly was a Whig historian and he certainly saw science as the greatest human achievement and the model for everything else. And it wasn't that I thought that it was *not* a great human achievement, but I saw it as one among several. I could have learnt a lot of data from Sarton but I wouldn't have learnt any of the sorts of things I wanted to explore. Anybody who took a degree in history of science at that time, he went to talk to Sarton and got it that way, there was no program; and that was not a way for me to do it. Look, when I fairly shortly joined the History of Science Society, there were perhaps then fewer than half a dozen people in the United States—I've written about this somewhere—who were employed in order to teach history of science. There were a number of other people who taught it within one or another of the science departments. But what they taught often was not quite history—in my terms, at least, not quite history; it was textbook history. I have sometimes said that some of the greatest problems that I've had in my career are with scientists who think they are interested in history.

A. BALTAS: There's a phrase in the *Structure*, "If history is viewed as a repository for more than anecdote or chronology . . ." Could you comment on it?

T. KUHN: Yes—of course anecdote and chronology were done by people who were not scientists as well as by scientists. But the stuff I did was, potentially at least, subversive of things that were for very good reasons part of the ideology of scientists. I mean, I'm saying things at this point that I've only gradually learnt as I've tried to understand my situation over the years. And by and large my relations with scientists (until the Planck book came out[8]) have with a few exceptions been very cordial; and from a number of them, including a number of physicists, *Structure* got a very good hearing. It was of course not widely read by scientists. I used to say that if you go through college in science and mathematics you may very well get your bachelor's

8. T. S. Kuhn, *Black-Body Theory and the Quantum Discontinuity 1894–1912* (1978; reprint, Chicago: University of Chicago Press, 1987).

degree without having been exposed to the *Structure of Scientific Revolutions*. If you go through college in *any* other field you will read it at least once. That was not altogether what I had wanted.

K. GAVROGLU: You said reading Merton's thesis was a relatively important experience.

T. KUHN: I got the reference to Piaget there, and that was important. There are just a few things like this . . . It was I think in Reichenbach's *Experience and Prediction*[9] that I found a reference to a book called *Entstehung und Entwicklung einer wissenschaftlichen Tatsache*.[10] I said, my God, if somebody wrote a book with that title—I have to read it! These are not things that are supposed to have . . . they may have an *Entstehung* but they are not supposed to have an *Entwicklung*. I don't think I *learned* much from reading that book, I might have learned more if the Polish German hadn't been so very difficult. But I certainly got a lot of important reinforcement. There was somebody who was, in a number of respects, thinking about things the way I was, thinking about the historical material the way I was. I never felt at all comfortable and I still don't with [Fleck's] "thought collective." It was clear it was a group, since it was collective, but [Fleck's] model [for it] was the mind and the individual. I just was bothered by it, I could not make use of it. I could not put myself into it and found it somewhat repugnant. That helped me keep it somewhat at arm's length, but it was very important that I read that book because it made me feel, all right, I'm not the only one who's seeing things this way.

K. GAVROGLU: Did you have any rapport with any of the other historians of science either by correspondence or at least intellectually? I include Europeans as well as Americans.

T. KUHN: While I was still in the Society of Fellows I didn't know any others. I'd met Sarton, I knew Bernard Cohen; Bernard has done a lot of good for the history of science but he is not someone who thinks about development at all in the way I do. We've not seen eye to eye. In any case, in the third year I started out with Nash offering a general education course: *Science for the Nonscientist*. It was a strange experience and certain things that happened to me that year I think have made a lot of difference [to me] since. Lots of people had come to it when Conant was giving it; they wanted to hear the president of the

9. H. Reichenbach, *Experience and Prediction* (Chicago: University of Chicago Press, 1938).
10. L. Fleck, *Entstehung und Entwicklung einer wissenschaftlichen Tatsache* (1939), reprinted as *Genesis and Development of a Scientific Fact*, ed. T. J. Trenn and R. K. Merton; trans. F. Bradley and T. J. Trenn; foreword by T. S. Kuhn (Chicago: University of Chicago Press, 1979).

university in action. I do not myself think, but it would be hard to be sure, that they got an awful lot out of it. I mean they had an experience which was of some importance, because they listened to things that the president—a very very bright man—wanted to say to them. But I don't think they had deep intellectual involvement with it. Nash and I wanted to increase the intellectual involvement. But what happened was the enrollment at the course of course immediately plummeted, though not to a terribly low level, and we realized suddenly that we were not getting across to the students—they were not really seeing or understanding what we were trying to do—or rather to most of them. There was a sort of top cream of students in that course, that got excited, in ways I love to get students excited, and they remember it and they talk about it still. But most of the people were sitting there like a lump. That was when teaching began to be difficult for me. I had taught in Conant's course when I first gave it, I had given this case history. I had given lectures on some other things also, it had always been easy: I wrote a few notes and went in the class and taught. And I didn't do that badly. Now I started spending much too much time preparing, getting very nervous in advance, and I've never altogether gotten over that. I mean I've never regained the original freedom to just go in with rough notes—knowing I knew the stuff—and start talking. Which has cost me some things; I think probably including some facility and sitting down and writing easily, although writing was always different from talking for me, as I said.

Just before that last year as I said, my wife and I had gone to England, and there I met people, the group particularly at University College, which was then one of the two places in the world where there was a history of science program, the University of Wisconsin really being the other. And we went to France. At some point I had already met Koyré—I guess he'd been some in the United States. My French wasn't good, the French were not all that hospitable, but [Koyré] gave me a letter to Bachelard, and said I should definitely see Bachelard. I delivered the note, was invited to come over, climbed the stairs. The only thing of his I'd read was that *Esquisse d'une Probleme Physique,*[11] I think it's called. But I'd heard he did brilliant work on American literature, and on Blake and other things of the sort; I as-

11. *La Philosophie du non: Essai d'une philosophie du nouvel esprit scientifique* (Paris: Presses Universitaires de France, 1940).

sumed he would greet me and be willing to talk in English. A large burly man in his undershirt came to the door, invited me in; I said, "My French is bad, may we talk English?" No, he made me talk French. Well, this all didn't last very long. It is perhaps a pity, because although I think I have read a bit more of the relevant material since, and have real reservations about it, nevertheless he was a figure who was seeing at least some of the thing. He was trying to put it in too much of a constraining . . . He had categories, and methodological categories, and moved the thing up an escalator too systematically for me. But there were things to be discovered there that I did not discover, or not discover in that way. The English connections established then really were somewhat with the University College group. Mary Hesse, and Alistair Crombie, I met Mackie, I met Heathcote, I met Armytage. But I had more to do with Mary Hesse obviously, somewhat more with Alistair Crombie, and in France really with nobody, except Koyré of course who was not in France during this trip. I came back to the U.S. then, late in the summer. That was the year that America went to war in Korea. And all of the planes were called up for military purposes. We had a hell of a time getting back. But I had to get back and start teaching. So that was that summer.

K. GAVROGLU: Concerning Koyré and Mary Hesse in particular, do you remember some of the things you discussed?

T. KUHN: Look: I realize I've left out something extremely important. When Conant asked me to do this case, which is my first work in history of science in a sense, I started by reading Aristotle to find out what the beliefs had been *before*. And very shortly after that, and it was at Bernard Cohen's suggestion, I picked up Koyré's *Etudes Galiléennes*,[12] I loved them. I mean, this was showing me a way to do things, but I just hadn't imagined it was there. In a sense it wasn't quite so strange as it might have been, because I had read and admired a good deal Lovejoy's *Great Chain of Being*.[13] But that you could do that with *science* had not quite occurred to me, and this is what Koyré was in some sense showing me. And that was important. I liked Mary Hesse and we talked some. The thing I remember best in my interactions with Mary Hesse—and this is of course much too early in this story to tell this—after *Structure* came out she wrote a very nice review

12. A. Koyré, *Etudes Galiléennes* (Paris: Hermann, 1939–1940).

13. A. O. Lovejoy, *The Great Chain of Being* (Cambridge, MA: Harvard University Press, 1936).

of it in *Isis,* a very favorable review. When I next saw her we were in England, and I remember walking with her and going into the Whipple Museum—it's another one of those imprinted images. She turned to me and she said, "Tom, the one problem is now you've got to say in what sense science is empirical" or what difference observation makes." And I practically fell over—of course she was right but I wasn't seeing it that way. Another story out of sequence I don't want to forget: shortly before Alexandre Koyré died—which is now a good many years later, he died shortly after *Structure* came out—I had a last letter from him. We had not really corresponded much, but he wrote me—he was sick and known to be probably dying at that time. He said, "I've been reading your book," and I don't know what adjective he used, but it was a thoroughly agreeable one. He said, and again I had not seen this coming—when I thought about it, I thought he was right—he said, "you have brought the internal and external histories of science, which in the past have been very far apart, together." Now, I hadn't thought of that at all as what I was doing. I saw what he meant, and coming from him it was particularly agreeable because he had been so anti-external history; his gifts were as an analyst of ideas. And that made an impression, or at least it pleased me tremendously.

A. BALTAS: Could you tell us how you met him?

T. KUHN: I didn't see him much in person. I met him through the *Etudes Galiléennes:* then he was in the United States and I think he was visiting Harvard and I think Bernard probably introduced me to him. I saw him from time to time, but never at all closely, never on a continuing basis of interaction. So it was not the personal interactions that made a difference.

After this trip to Europe, I came back, I started to teach the Conant course, which I've already talked about, and one of the things that happened, although it was not the first thing that happened, was that Karl Popper gave the James Lectures, I think they were, at Harvard. I had had reason to think I was going to like these and I was clearly interested in it. I was introduced to Popper at a fairly early stage and we saw a little bit of each other. Popper was constantly talking about how the later theories *embrace* the earlier theories, and I thought that was not just going to work out quite that way. It was too positivist for me. But Popper did me a tremendous favor. This is another example of getting the books that meant something to me from people I would not have expected. He sent me to Emile Meyerson's *Identity*

and Reality.[14] I didn't like the philosophy at all. But, boy, did I like the sorts of things he saw in historical material. He went into those briefly and I mean he didn't do it as a historian but he was getting it right in ways that were different from the ways that history of science was being written. Somebody else I discovered during the trip to France—I had not been aware of her before, and [by then] she was no longer around—whose work I thought extremely well of and it was of some importance to me was Hélène Metzger. Another person whose work was of some importance to me—although I did not read that much, and I never met her—was a medievalist working in Rome out of the Vatican, Anneliese Maier. It's hard to say which of these were important, but these were works in history that I admired. A sort of history, and an approach to history that I admired and which I encountered fairly early.

K. GAVROGLU: My mind is hooked on something that you said before. It's not the time now, but it might as well be put . . . You said that Koyré sent you a letter saying in one form or another that *Structure* merges, as it were, the internalist and externalist approach. Did you say that you were not aware of that? I find it difficult to accept that you had not realized it.

T. KUHN: I hadn't thought of it as doing that. I mean, I saw what he meant . . . I thought of it as pretty straight internalist. It constantly surprises people in England that I'm an internalist. They cannot get their heads around it. Now, there is something I've left out which should go in here: In that Conant course that I worked in the first time—and I think we did the same thing later—Conant put in a significant social dimension. And it came from him, and I liked it, although I never got myself at all involved with it. He had published a little article on Cambridge versus Oxford during the Restoration, and why science in England developed the way it did. In any case, we read a significant selection from Hessen, we read Merton, that was where I first was introduced to the Merton thesis, we read G. N. Clark's *Science and Society in Seventeenth-Century England*, and that we probably read all of, and maybe one other thing, I'm not sure. I've read some Zilsel, and by and large thought well of it. And I may have met him at that time, but I'm not sure. But there were things of this sort that were also going on with me. If you look at the introduc-

14. E. Meyerson, *Identity and Reality* (1908) trans. Kate Loenwenberg (London: Allen and Unwin, 1930).

tion to the Copernican Revolution book, you'll see that I sort of apologize for the lack of much of any external things and point out that if I were going to do that I'd say more about the importance of the calendar, and other things of this sort.

Just to clear that up: although I've never really done external work, and although I am deeply conscious of and have talked a bit about the differences of the research techniques, the sources and so forth, it's a very different state of mind. I have done a bit of methodological writing about relations of internal and external, particularly in that article, "History of Science," in the *Encyclopedia of Social Science*, and somewhere else. I've always been conscious of it, I've always wanted to see the two things put together, and I think they still almost never have been. And I think there are important difficulties . . . The thing that seems to me to be, among the things I've read, the best example of putting them together is also a very special one. And that's the *Great [Devonian] Controversy* book,[15] which I think is splendid. But there it just was begging to be done in both ways at once, and the science was such that you could do it that way; and I don't know how to manage that problem.

Okay, look. Let me jump back or ahead. I had talked about teaching the course with Leonard Nash when we took it over from Conant. After that I became an instructor for one year and then an assistant professor for several more years at Harvard. My primary assignment continued to be the general education course, but I began to give a little history of science elsewhere. I developed a course of my own, a sort of advanced undergraduate course which really was formative [for me], and which is still one of my favorite courses, though I haven't given it for years. I forget whether it was called . . . *The Development of Mechanics from Aristotle to Newton*. But I started out by getting people to read Aristotelian texts and talk about what motion was like and what the so-called laws of motion were and why that was not the thing to call them, and did a certain amount of medieval material and then wound up with Galileo and a little bit of Newton. That was a course I liked. I started that—initially I think I gave it a couple of times at Harvard—I gave it at Berkeley, and so on. I had an undergraduate tutorial group, which is a way of teaching students in small groups, majors in small groups—there hadn't been

15. M. J. S. Rudwick, *The Great Devonian Controversy:* The Shaping of Scientific Knowledge among Gentlemanly Specialists (Chicago: University of Chicago Press, 1985).

any of that in history of science previously. I can't think just what else I did.

A. BALTAS: How long did you stay in Harvard after your thesis till you left?

T. KUHN: My thesis was in my first year in the Society of Fellows, which was 1949. I think '47 was the year in which I'd met Conant. I was there from then until '57—'56 or '57. I think '57 was the year I went to Berkeley.[16]

A. BALTAS: The reason for the change?

T. KUHN: Oh, the reason for the change is Harvard didn't want me. And it's in many ways a very good thing they did not. I didn't like that and I was one of those people who was at least in real danger of breaking up because Harvard didn't want them there. That was something that happened to people who'd spent too much time around Harvard.

Something else that happened while I was still in the Society—it was not uncommon for people in the Society of Fellows to be asked to give the Lowell Lectures. There have been some famous ones including, I guess, *Science in the Modern World,* by Whitehead. And there were other very well known ones. The series is still given, but by then it was pretty pro forma and the audience was no longer the intellectual elite of Boston and so forth. And I undertook to give those and I think I gave them in the year after I'd gotten back from Europe. I guess under the title of "The Quest for Physical Theory." I had a dreadful time preparing it and I nearly cracked up. But I got through them. What I was trying to do was to write the *Structure of Scientific Revolutions* in three lectures, and there were various other attempts as time went on. And there are copies of those in the archives now. They are not very good, but they sure give indications of what I was trying to do. One thing that happened in the course of doing those: I gave a lecture which was intended to trace the role of atomism in the development of science. I was persuaded in one respect or another that it had been a transforming influence in the seventeenth century. I still think that. I think in many ways the nature of the transformation has not yet been fully appreciated, although there are things I've learned since. For the record, the part of it that I think has not been fully appreciated is the extent to which atomism helped say, like other sources as well, that you can learn things about nature not simply by

16. In fact, it was 1956.

looking at things as they happen, but by what Bacon called "twisting the lion's tail." And that is terribly important to the development of an experimental tradition and it attaches very comfortably to atomism and not a bit comfortably to any sort of essentialism. It's something that . . . I've taught it, I've said just a word about it in print in one other piece, but I think it's critically important. And I think it's one of those things that's been missed. I always was going to go back sometime and write an article which was really going to be on Bacon and Descartes, about the emergence of epistemology for the first time as a subject in the seventeenth century, which has also got to do with the fact that atoms did not speak for themselves.

V. KINDI: And was that an influence of philosophy on science or perhaps of science on philosophy?

T. KUHN: Look, one of the things I now insist on, that is not at all well handled in *Structure of Scientific Revolutions,* is that you must not use later titles for fields. And it's not only the ideas that change, it's the structure of the disciplines that are working on them. So, you can't separate this sort of philosophy from science yet in the seventeenth century. That separation begins to come after Descartes, but it isn't there in the early Descartes, it's only partly there in Leibniz . . . it's not there in Bacon. The British empiricists begin to force it . . . Locke in particular. That was something I wanted to write a book about. Well, other people have written a book since, and I got too busy with other things—that book is never going to appear from my hand. But that was one of the things that emerged. Now, in the course of this, thinking about atomism . . . if you believe that the atomism of the seventeenth century was like Epicurean and Democritean atomism in some important ways, but what it was *not* like was those ancient and medieval atomisms which took the atoms to be indivisible but which had built into them Aristotelian qualities, or something like Aristotelian qualities, so that atoms are fire, air, earth, and water—[instead], this was a matter and motion atomism. It suddenly occured to me that if you believed *that,* you would believe that you can make anything out of anything—it's a natural basis for transmutation. I told this idea to Leonard Nash and he said, "I don't know, it's very plausible, but the way you should find it out is of course look at Boyle." So, bright and early on a Monday morning, I was standing outside Widener waiting to get in. And I dashed into Widener and I went to the shelves that had editions of Boyle, and I pulled out one of the *Collected Works,* and found the *Skeptical Chymist* and started to read. Very early there

is a remark in which one of the interlocutors says to the major figure who represents Boyle, "It sounds very much to me as though you don't believe in the elements," or something like this. And Boyle says, "That's a very good question. I'm glad you asked me." And then he proceeds to say, "I mean by element those things out of which all things are made, and into which they can be divided." Now, that is taken to be, and it isn't quite, the definition of an element. And Boyle is given credit for the first definition of an element, but what he's doing at this point . . . He says, "I mean by an element, *as I take it all chemists do*"—[but that phrase is] replaced by dot dot dot, when that definition is quoted! And he says, "I'm going to give you reasons for believing that there are no such things." And that was almost my first article.[17] It is, I think, a very good article—it's totally unreadable because I thought I had to persuade a very learned group of historians of chemistry out there. And what I gradually discovered was that nobody knew nearly as much about this problem as I did. And I shouldn't have encumbered it to that extent with supporting evidence and with lots of quotations. In the course of that I also discovered or saw a strangeness in Newton's thirty-first query, which has to do with *aqua regia,* something that dissolves silver and not gold, and something else that dissolves gold and not silver. I thought there was a misprint here, and I still think there was. And this is anomaly showing up. The anomaly about Boyle's is the first. That actually was published first,[18] it's a short piece—those were my first two articles. And during this time at Harvard, when we—Nash and I—took over the course, I started it with lectures on the Copernican revolution. The book really, though it's got more detail, was modeled very precisely [on those lectures]; it's an extended case history. And it illustrates this thing that I'm deeply convinced about. Sometimes you have to go way back in order to find the starting point, to write something that indicates how powerful these prior beliefs were and why they ran into trouble. And I couldn't have started earlier than prehistory; I had to go practically back that far. So, I put in—it's still extraordinary—it was during those years that I was approached by Charles Morris. He was an author of the *Encyclopedia of Unified Science,* and he wrote a very influential book whose name I can't now remember, that grew out of his mono-

17. T. S. Kuhn, "Robert Boyle and Structural Chemistry in the Seventeenth Century," *Isis* 43 (1952): 12–36.

18. T. S. Kuhn, "Newton's 31st Query and the Degradation of Gold," *Isis* 42 (1951): 296–98.

graph and the encyclopedia; and he asked me whether I would take over a volume in the encyclopedia. That volume had originally been assigned to, I think, he's an Italian who wound up in Argentina—Aldo Mieli probably. If you go back and look at the list, history of science was not one of the projected volumes at the very beginning, but it was listed for a long time before anything appeared, under various different authors. They had gone to Bernard [Cohen] who'd suggested that I do it. And I, thinking that I would use it to produce the first version, a short version of *The Structure of Scientific Revolutions,* said yes, and I put in a proposal for a Guggenheim Fellowship, toward the end of my time at Harvard. My project—I was already writing *The Copernican Revolution*—was to finish that, and to write the monograph for the encyclopedia. Well, I didn't finish *The Copernican Revolution* and the monograph for the encyclopedia appeared only fifteen years later. No, not fifteen years later—it was fifteen years between the time these ideas *started* and the time I was finally able to write *Structure*. So that's sort of where I was in those years; I had published my first book just at the end of this time.

V. KINDI: *The Copernican Revolution* was published in . . .

T. KUHN: '57, I think.

A. BALTAS: Why did you choose the Copernican revolution?

T. KUHN: Oh, I was already writing it—I had been giving it as lectures. I needed a book, I had this material, I could do a book, and I didn't think it was a stupid book to do. I mean, it was not what I mainly wanted to be doing, but it was something worth getting done. But that's why I chose it, because I had been giving lectures on the subject.

V. KINDI: And the application to the Guggenheim, was it earlier?

T. KUHN: I got a Guggenheim Fellowship probably in '55–'56 [actually '54–'55]. But something about my unreality with respect to my project and what I could do . . . I mean, I've never been any good in saying how long it will take me to do things, for ten years I've been saying, I think it will take me about another two years to finish this book I'm still working on, and I still think it will take me another two years. But that's what I was under way with.

V. KINDI: And you had already some ideas about writing *Structure? The Structure of Scientific Revolutions?*

T. KUHN: Oh, look, I had wanted to write *The Structure of Scientific Revolutions* ever since the Aristotle experience. That's why I had gotten into history of science—I didn't know quite what it was going to look like, but I knew the noncumulativeness; and I knew something

about what I took revolutions to be. I mean, I think in retrospect I was wrong, in the ways I talked about the other night; but that was what I really wanted to be doing. And thank God, it took me a long time, because I managed to get myself established in other ways meanwhile, and the ideas—I didn't let go of them *too* prematurely. I did let go of them somewhat prematurely, but . . . thank God!

V. KINDI: Some of your ideas are similar to the ones developed by Hanson in his book *Patterns of Scientific Discovery,* especially chapter 1 on "Observation."

T. KUHN: Yes. It's the logic of discovery I still don't myself believe in, although I think you can talk about not the *logic,* but the *circumstances,* in ways that illuminate discovery.

V. KINDI: What about *seeing?*

T. KUHN: It was the gestalt switch aspect of it, it was the conceptual framework aspect of it.

V. KINDI: Which you had gotten from where?

T. KUHN: I had gotten that from the Aristotle experience. I had it on other occasions, too. When I taught Galileo, I used to teach it in a way in which key things in it were the relatively anomalous things. I thought I understood why . . . You know, there is an argument in Galileo that the freely falling body which starts at the top of the tower moves in a semicircle at a constant rate, and winds up at the center of the earth. That was for me very important. I thought I'd figured out why he was saying that. There is also an argument which people take to be better, which is about why bodies would never be rejected from the earth regardless of how fast it was spinning. Now, that is a mistake and I think I know what the mistake is; if you know how medieval "latitude of forms" so-called analyzed these problems of motion, you can spot it. It's a standard mistake in the early history because people didn't used to have both the notion of accelerated motion and . . . this depends upon medieval . . . So, it was those questions of frameworks which illuminated anomalies, which were right at the center of what I was doing.

yes, and add commas around it.

A. BALTAS: We are at the point where you are leaving Harvard and you are going to Berkeley. Let's take a lead there, I mean what did you do there, whom did you meet, whom did you interact with . . .

T. KUHN: An important story. I had a friend, who was a tutor as I was at that time in Kirkland House, at Harvard, who was a friend of a man called Steven Pepper, who was chairman of the philosophy department, at Berkeley. He knew I was leaving and looking for a job,

and told Steven Pepper about me; Steven Pepper came to call. The philosophers at Berkeley wanted to hire a historian of science. They didn't know that they didn't want one, they didn't know that this was not a philosophical discipline—I jumped at the change, because I wanted to do philosophy. And I got offered that job, and they asked me at the last minute, would I like to be in history too, and I said, of course. I hadn't proposed that, I didn't know that it was possible, but it was clearly a better, in certain ways a better locus. I went out there jointly in the history and in the philosophy departments. It then turned out that Berkeley could not list a course in both departments at once; I had to divide my courses. It could have "see also the philosophy courses" or vice versa, but there was no way to give a history course a number in philosophy. I thought I knew computers could do that and I kept being assured they couldn't, and it wouldn't work, and I was sore as hell. In any case. I did do things that way. And I taught, I guess, two courses in history and two in philosophy. Two of the courses were survey courses. I'd never given a survey course before in history of science, I'd never *had* a survey course in history of science. So that every lecture I gave was a research project and it was very good for me. After a while I couldn't get that much [more] out of the survey course, but I learned a lot of history of science, I learned how to look at books that didn't feel the way I did, and nevertheless I figured out what must have gone on. I mean, that's the way I learned to do history of biology for the survey course. And I learned some of the problems of trying to organize the development of science. And one of the things that really I finally did, that has shown up also in some of my written work, and that I think is very important: the standard division of history of science—ancient-medieval as one, and then modern science starting in the seventeenth century—it just doesn't work. There is a group of sciences which starts in antiquity and comes to a first major culmination in the sixteenth-seventeenth century, and that's mechanics, parts of optics, and astronomy. And then there are a whole lot of fields which scarcely exist in antiquity and have not yet got much identity, and they are the experimental fields. So I used to go through Newton in the fall, and then drop back in the spring to the beginning of the seventeenth century and pick up Bacon and Boyle and the experimental movements. That's a hell of a lot better way to organize a one-year survey than the standard way—and what it really is, is the origin of the article of mine on *Mathematical versus Experimental Traditions in the Development of*

Physical Science.[19] That's a very schematic article, but it's there that I pushed this business about "don't name fields by their subject matters, [but] look to see what the fields were," which I had not done in *Structure*—it's a bad aspect of *Structure*. Then, in the philosophy department I gave this course that I have mentioned before, on Aristotle to Newton. Also it was there that I gave a graduate seminar each year. You couldn't really give graduate seminars in this field at Berkeley. I mean you got enough students, but very few of them had the preparation. So, one had to pick an area and then let people work at all sorts of levels and report it. There were some useful things that came out of this, but it really wasn't until I got to Princeton that there was a group you could count on, you could name a subject that you were going to work on and get people who would work on it . . .

Early at the time at Berkeley I was invited—in fact, the year I went out there I'd been invited to come to the Behavioral Sciences Center [at Stanford]. And I couldn't, because I'd just accepted a new job at Berkeley, that would have been the inaugural year. But after I'd been at Berkeley for a year or two. I was invited again. I took leave and I went there [to the Center] and that's the year I devoted to preparing *Structure*. And I had an impossibly difficult time. I had one very formative paper that I should have mentioned, that I had done earlier. Earlier at Berkeley I was asked to do a command performance appearance in something the social sciences were doing on "the role of measurement in xyz." That was where I met your prime minister, Andy [Andreas Papandreou]. He gave one on economics, I gave one on physical science. The paper that ultimately emerges is *The Function of Measurement in Physical Science*,[20] and that really was extremely important. Just that little phrase very early on about an extended mopping-up operation—I don't even remember quite how it gets introduced, but that's where the notion of normal science enters my thinking. It isn't that I had though everything was revolutionary—revolution in perpetuity is a contradiction in terms. But somehow or other I saw normal science as puzzle solving, although it wasn't already all there; it was something that came out at that point, and which helped me be ready, I thought, to write *Structure*, which was my enterprise for the follow-

19. T. S. Kuhn, "Mathematical versus Experimental Traditions in the Development of Physical Science," *Journal of Interdisciplinary History* 7 (1976): 1–31; reprinted in *The Essential Tension*, 31–65.

20. T. S. Kuhn, "The Function of Measurement in Modern Physical Science," *Isis* 52 (1961): 161–93; reprinted in *The Essential Tension*, 178–224.

ing year. Well, what happened was, I wrote a chapter on revolutions, slowly but not with excessive difficulties, and talking about gestalt [switches] . . . Then I tried to write a chapter on normal science. And I kept finding that I had to—since I was taking a relatively classical, received view approach to what a scientific theory was—I had to attribute all sorts of agreement about this, that, and the other thing, which would have appeared in the axiomatization either as axioms or as definitions. And I was enough of a historian to know that that agreement did not exist among the people who were [concerned]. And that was the crucial point at which the idea of the paradigm as model entered. Once that was in place, and that was quite late in the year, the book sort of wrote itself. I wrestled for the whole year and I got sort of two chapters and one article, or something, done that year. But I went back and I wrote the whole of that monograph very quickly while also teaching in the next twelve-sixteen months at Berkeley. That was the key to it. Now, a question I don't know the answer to—this is a point at which my work is often linked to Polanyi's. Polanyi came to the Center that year and gave a lecture on *tacit knowledge*. I liked the lecture all right, and it's possible that it helped get me to the idea of paradigm, although I'm not sure. There is no great reason why it should have, because tacit knowledge was also propositional knowledge in some sense or other. It's there you will recognize the remark I made about your paper, Aristides, that we need to find something . . .

A. BALTAS: . . . something that's not propositional . . .

T. KUHN: Yes. But I couldn't have said that. So I just don't know. It's perfectly possible; we did read some Polanyi in the Conant course. Conant introduced him to the course, and I liked it quite a lot—I don't remember just what it was, except that I kept feeling terrible at those points where he sort of spoke as though extrasensory perception was the source of what the scientists did. I didn't believe that. That [flavor] gets into the tacit knowledge thing also. I don't know. But Polanyi was certainly an influence. I don't think a great big one, but it was very helpful to me to have him out there. In that connection, another story—two books that came out while I was trying to write *Structure*. One of them was Polanyi's *Personal Knowledge*[21] and another one was Toulmin's *Foresight and Understanding*. Particularly with *Personal Knowledge,* I looked at it and said, I *must* not read this book now. I would have to go back to first principles and start over again,

21. M. Polanyi, *Personal Knowledge* (London: Routledge & Kegan Paul, 1958).

and I wasn't going to do that. I also said that with *Foresight and Understanding,* which I think I could have dealt with more. Later, when I did try to read *Personal Knowledge,* I discovered I didn't like it. I never got through that early bit about statistics, which seems to me just way off, quite wrong. I did later read Toulmin's *Foresight and Understanding,*[22] and I understand why Toulmin might have been sore at me for stealing his ideas, but I don't think I did. Let me be perfectly clear: I'm not at all sure he felt that, he has never said that. Toulmin was one of the people I had met during this trip to England at the end of my time in the Society of Fellows—I got along with him fine, he showed me around Oxford one day, but we hadn't gotten at all close. But since the time he came to the States, he and I have not gotten along very well.

A. BALTAS: What about colleagues at Berkeley . . .

T. KUHN: Very good. I would say only one—I mean there were sympathetic people, on the whole not in the philosophy department. The person who was *extraordinarily* important was Stanley Cavell. My interactions with him taught me a lot, encouraged me a lot, gave me certain ways of thinking about my problems, that were of a lot of importance.

V. KINDI: Did you meet him there?

T. KUHN: He had also been in the Society of Fellows. I met him just before he and we left for Berkeley. The Society of Fellows had a softball game every spring, and he was just back from Europe. We were just going off, and I met him there. But I didn't get to know him at all, until we got to Berkeley. And that was a very close and meaningful relationship at that time. We are both in Cambridge and I don't see him any more, and I regret that.

V. KINDI: Was Feyerabend there?

T. KUHN: Feyerabend was there. He came late in my time. My recollections are not as precise as I would like them to be. I think I remember a talk with Feyerabend. He was sitting behind his desk and I was standing at the door of his office, which was very close to mine. Now, I'm not sure this is right, I mean this is the sort of thing I could easily have constructed. I said something to him about my views, including the word *incommensurability,* and he said, "Oh, you are using that word too." And he showed me some of the things he was doing, and *Structure* came out in the same year as his big article in Minne-

22. S. Toulmin, *Foresight and Understanding* (Bloomington: Indiana University Press, 1961).

sota Studies. We were talking about something which was in some sense the same thing. I messed it up more than he did; I think it's now *all* language and I associate it with change of values. Look, values are acquired together with language, so it isn't that bad a mistake, but it surely made it harder for people to see—or me to see . . . I didn't know enough about meaning, so I was leaning hard on gestalt switches; I think I talked about meaning change in *Structure*, but I looked to find the passages recently, and I was surprised at how few of them there are.

V. KINDI: How did you come up with the terms *paradigm* and *incommensurability*?

T. KUHN: Look, *incommensurability* is easy.

V. KINDI: You mean mathematics?

T. KUHN: I don't remember to whom I told this story recently, but I think it's since I've been here. When I was being a bright high school mathematician and beginning to learn calculus, somebody gave me—or maybe I asked for it because I'd heard about it—there was a sort of big two-volume calculus book by, I can't remember whom. And then I never really read it, but I read the early parts of it. And early on it gives the proof of the irrationality of the square root of 2. And I thought it was beautiful. That was terribly exciting and I learned what *incommensurability* was then and there. So, it was all ready for me, I mean, it was a metaphor but it got very nicely at what I was after. So, that's where I got it. *Paradigm* was a perfectly good word, until I messed it up. I mean, it was the right word at the point where I said, you don't have to have agreement about the axioms. If people agree that this is the right application of the axioms whatever they are, that this is a model application, then they can disagree about the axioms; just as with logic, without its making any difference, they can disagree about the axioms, they can switch axioms and definitions quite freely back and forth, and sometimes do. Here in physics, if you switch axioms and definitions you change to some extent the nature of the field. But the notion that you could have a scientific tradition in which people agreed that this problem had been solved, although they could still disagree vehemently about whether there were atoms or not, or something of that sort. Paradigms had been traditionally models, particularly grammatical models of the right way to do things.

A. BALTAS: This is your first relation to the word—I mean that's why you took it over.

T. KUHN: That's right.

V. KINDI: You were not aware of perhaps Lichtenberg's use of *paradigm*, or Wittgenstein's use of the term . . .

T. KUHN: I certainly was not aware of either of them. Lichtenberg has been called to my attention, and I'm a little surprised that I haven't had my nose dragged through Wittgenstein's use of it. But no, I was not. Now, a very bad thing happened immediately after that. The first time it's introduced into a published work of mine, was in a paper called *The Essential Tension*,[23] which I read at a conference. And there I use it right. But I had been seeking to describe what scientists—the way a tradition worked in terms of *consensus*. And what consensus was about. Consensus was about models, but it was about a hell of a lot of other things [as well] that weren't models. And I proceeded to use the term for the whole lot, for all of the things, which made it very easy to miss what I thought of as my point entirely, and to simply make it the whole bloody tradition, which is the main way that has been used since.

V. KINDI: What about Masterman's twenty-one uses?[24]

T. KUHN: All right, I'll tell you a story. This story comes from a little later day. There was an International Colloquium in the Philosophy of Science held at Bedford College, London. The Proceedings appeared in the volume titled *Criticism and the Growth of Knowledge*. At that meeting I read a paper, Popper was in the Chair and Watkins commented on the paper and there was to have been further discussion, on the original plan for what was going to happen. One of the people who had been invited to participate in this further discussion was Margaret Masterman—whom I'd never met, but of whom I'd heard, and what I'd heard about her was not altogether good, and it was largely that she was a madwoman. She got up at the back of the room in the discussion, strode toward the podium, turned to face the audience, put her hands in her pockets and proceeded to say, "In my sciences, in the social sciences" (she was running something called the Cambridge Language Lab), "everybody is talking about paradigms. That's the

23. T. S. Kuhn, "The Essential Tension: Tradition and Innovation in Scientific Research," in *The Third (1959) University of Utah Research Conference on the Identification of Creative Scientific Talent*, ed. Calvin W. Taylor (Salt Lake City: University of Utah Press, 1959), pp. 162–74; reprinted in *The Essential Tension: Selected Studies in Scientific Tradition and Change* (Chicago: University of Chicago Press, 1977), pp. 225–39.

24. M. Masterman, "The Nature of a Paradigm," in *Criticism and the Growth of Knowledge: Proceedings of the International Colloquium in the Philosophy of Science, London 1965*, vol. 4, ed. I. Lakatos and A. Musgrave (Cambridge: Cambridge University Press, 1970), pp. 59–89.

word." And she said, "I was recently in hospital and I went through the book and I think I found twenty-one, twenty-three, whatever, different uses of it." And, you know, they are there. But she went on to say, and this is the thing that people don't know, although it's more or less in her article, "And I think I know what a paradigm is." And she proceeded to list four or five characteristics of a paradigm. And I sat there, I said, my God, if I had talked for an hour and a half I might have gotten these all in, or I might not have. But she's got it right! And the thing I particularly remember, and I can't make it work quite but it's very deeply to the point: a paradigm is what you use when the theory isn't there. And she and I interacted then, during the rest of my stay, quite a lot.

A. BALTAS: This conference is in London, in '65, I think, so your book is out three years already. It's published when you reached London . . . What is the initial reception?

V. KINDI: It was published already in the Encyclopedia, right?

T. KUHN: Yes, that appeared in '62. Look, I'll tell you a story about that also. I told you that I had written very quickly, after I got back from the Stanford Center, the manuscript. I hoped it was important. I had wanted to do this, I was not altogether satisfied, but I was pretty excited about it. I didn't know how it was going to go over. I began to have vast reservations about putting it in the *Encyclopedia of Unified Science,* because the encyclopedia had been an exciting thing fifteen years before, but its reputation had declined considerably, and it was no longer in the forefront. But I had a commitment. I went and talked to a friend of mine at the [University of] California Press as to what was right to do under these circumstances. And he said, look, the assistant director of the [University of] Chicago Press is a lovely man called Curley Bowen. Write to him telling your problems and see what he says. So I wrote a long letter to Curley Bowen and I had by then just got a dittoed copy of the manuscript, which was going to need some revisions but I think not very many. And I described the problem—what I was seeing as the problem—and I also said, "would you by any chance . . . it's twice as long as any of the other monographs and I don't know how to cut it. But if you would bring it out full-length more or less, independent of the encyclopedia, I'll cut it somehow or other for the encyclopedia." This went into the mail I think late Sunday afternoon, or Monday morning. Wednesday, as I was leaving the house in California, the telephone rang and it was Bowen. He said, "Don't worry about a thing, we'll . . ." Wow, what an experi-

ence to have with a publisher! My relations with Chicago, although he left fairly early, have been very good ever since. He said, "We'll publish it, you don't have to cut it." And they brought it out, they also brought out a hardcover version initially, from which they omitted the encyclopedia stuff. After that it's sort of taken care of itself.

I think that with the exception of one story—that I think I do want to put on the record—that we've about gotten to the end of the time at Berkeley. But a strange, and to me quite destructive thing happened at this point. I had been invited to take a job at Johns Hopkins. The job at Johns Hopkins would have promoted me to full professor, given me a significantly higher salary, and the opportunity to appoint three or four more people; it was a major offer. And I went east, I told the chairmen of both my departments [philosophy and history] that I was going to go and look at it. I said, I don't think there is anything for you to worry about, and I will tell you if there is, but I simply want to put it on record that I'm going off to do this. And I did go, and in fact I found it extremely attractive. I got back, and I told the chairman that I didn't know how it was going to come out, and that I proposed to argue with myself about it. But in practice I was finding it extremely attractive. Then I was asked what it would take to make me stay [at Berkeley], and I said, look, if under these circumstances you don't give me a full professorship, I would at least want to know why. The title doesn't mean that much to me, I'm sure you won't match the finances, but I don't need to ask you to, though I could use a raise. But, I said, the thing I *must* have is an expansion; I got permission to appoint one other person—that was all I could get, one junior appointment. As I sat around thinking about this, I said to myself, look, maybe five years from now I'd have gone back to Hopkins, but I've only been here two or three years. It's a very rich institution, I mean rich in terms of very good people. I decided I can't leave this at this point. I told the chairman of the history department, and I went to the chairman of the philosophy department and told him, and he said, "Don't make up your mind too fast. Hold off." And in fact I had already written Hopkins that I wasn't coming. I don't think I told him that. So, I went on with what I was doing, my teaching, and some weeks and weeks later coming out of a class I had a call asking me to come down to the chancellor's office. The acting chancellor, who was my colleague in philosophy, Ed Strong, who had done some historical work himself, wanted to talk with me. I got down there and he said, "The recommendation for your promotion has now gone all

the way through, it's favorable, and I have it on my desk. There is just one thing. The senior philosophers voted unanimously for your promotion—in history." And I said, "Suppose I don't accept that." He said, "You get it anyway, but . . ." And then I said, "You mean, but why would you want to stay where you are not wanted?" He sort of nodded. I was extraordinarily angry, as you can guess, and very deeply hurt, I mean that's a hurt that has never altogether gone away. The fact that I'd been asked by philosophers, and was in a philosophy department . . . I knew it wasn't altogether congenial to them, but I sure as hell wanted to be there, and it was my philosophy students who were working with me, not on philosophy but on history, were nevertheless my more important students. And I said, "I'll have to think about it." And Strong said, "Oh, but I have to get it to the Regents on Friday if it's to go through before the next meeting." I said, "You will have my decision before Friday." And I went up and had a terrible fight with the chairman of the philosophy department. And I finally said, "All right, what else can I do, I'll accept it." I've been very sorry since. I mean, I felt what I should have done is to say, look, I will accept it after sitting down and discussing the situation with the senior members of the philosophy department. If they still want to do it that way, I will accept it. But I won't accept it on these terms. I think if I'd said that, they wouldn't have faced me. In any case, I think I should not morally have simply let myself be treated that way. But that hurt a lot. I stayed for another year or so, and it wasn't because of that that I left Berkeley. But there were certain things that had happened at Berkeley that had diminished my pleasure in being there, though they hadn't taken it all away. I got an offer to go to Princeton and at Princeton I was going to have a senior colleague who'd set up the program. The two of us were going to work together, there were going to be other people. And that was just a much more manageable situation. I got that offer while I was in Denmark. I said, I can't answer until I've gotten back, but I will work on it as hard as I can when I get back, and I'll come visit Princeton. So when we got back, which was probably the fall of '63, my wife and I went to Princeton, visited, and I decided I was going to take this offer—which I did.

K. GAVROGLU: Why were you in Denmark?

T. KUHN: Shortly after I finished the manuscript of *Structure*, I got asked by a committee of members of the American Physical Society to direct an archival project on the history of quantum theory. One of the peo-

ple who asked me was the man with whom I'd done my Ph.D. thesis, van Vleck. And I did. If I hadn't finished *Structure* at that point, I wouldn't have accepted. But the thing I had been dying to get done—my big commitment to myself—I'd known what I wanted to do next, and it was just this book about science and philosophy in the seventeenth century. But I thought, that one I can put aside. So I did. And I accepted the job; and the rest of that, basically you know. There is nothing particular to tell about it, except one thing: that project has probably had some real influence. We brought back a lot of microfilm of archives and we got manuscripts and letters deposited in various places. And we catalogued them. And that was probably the more important part of it. Interviewing was frustrating as hell! Some of the interviews are really very good. But the physicists, including the ones sponsoring the project, really wanted to get the development of ideas, and that's of course what I wanted also. I knew from experience as a historian that scientific autobiographies are invariably inaccurate, they tell the wrong story. But it's usually the case that if you sit down with the published papers and whatever else there may be, and then ask, why did he tell that story instead of this story . . . you get very important clues to a reconstruction. What I hadn't anticipated was the number of times people would say, "I don't know, I can't remember how, why would you expect me to remember that?" In that sense, for that sort of thing, we got much less than I had hoped. The other side of this is that what you can get scientists to talk about quite freely and richly is what it was like to be at Munich, and so forth, and who the important teachers were, and what your first experience was when you went from there to Göttingen or vice versa, or whatever. And that you could get some talk about. If you started back at the places I used to try to start at and say—how did you get into science in the first place, did your parents approve—too often you'd get, "That's not physics." So, that's the quantum physics project; and I came back from it to look at Princeton, and the next year we went to Princeton.

V. KINDI: Would you like to talk about the students who worked with you?

T. KUHN: I'd never had a lot of graduate students. Presumably that's partly because there haven't *been* so many graduate students, and partly because I tend to scare them away. I criticize. My first two graduate students—although one officially didn't take his degree with me—the first one was John Heilbron, and [the second was] Paul Forman, who was the one who took finally his degree with Hunter

Dupree after I left, though he'd got into it through my courses. John was all but finished with a physics thesis, but he'd been ill and while he was ill he had read Thorndike's *History of Magic and Experimental Science* and decided he wanted to be a historian of science.

I may be mixing up two stories when I tell you that, because my first experience at Berkeley sitting in my office, not yet having taught a course, was a graduate student in philosophy—oh, no, I'm not mixing it up—who came down, wanted to find out about the course, and he said to me, "What do you think of Dampier?" Dampier is a one-volume history of all of the sciences,[25] and I said something like, "I've never quite been able to read all the way through it, I think it's supremely dull." And he said, "Oh, I think it's wonderful!" But you see why I thought I might have confused these two stories.

So, that was the beginning of my production of students. It must be said: there had been other students—I think none who emerged with the authority and reputation that John has . . . I am still a devotee of Paul's *Weimar Culture and the Quantum Theory*.[26] One knew it couldn't be right altogether, but I think he's given much too much away as he sort of backed away from the criticisms. And I remember when I first read that. I was at Princeton, I went and put a note on the bulletin board of the department office, saying, "I have just read the most exciting piece that I've read since I discovered Alexander Koyré!" So, that's my first two students. There have been some others at Princeton and I'd like to say something about them when we get there. But as both John and Paul indicate, I have exposed my students to the sort of history of science I do, they are in principle capable of doing it and they've each showed that in early work. But they've both turned entirely away from it. So, in that sense, I haven't produced any children. With one exception, and that exception is in fact Jed Buchwald—who was not a graduate student of mine, but an undergraduate student of mine. I turned him on to history of science, and that was the point at which he decided to do it. But it's always been a source of some chagrin that these things that I like to do, and have taught people to do, don't get pushed along. But there are a lot of

25. W. C. Dampier and W. M. Dampier, Cambridge Readings in the Literature of Science: Being Extracts from the Writings of Men of Science to Illustrate the Development of Scientific Thought (Cambridge: Cambridge University Press, 1924).

26. P. Forman, "Weimar Culture, Causality, and Quantum Theory, 1918–1927: Adaptation by German Physicists and Mathematicians to a Hostile Intellectual Environment," *Historical Studies in the Physical Sciences* 3 (1971): 1–115.

reasons for that. One of them, of course, is that people have to get away from their doctor-father; and another is that the field has been moving way away from the sort of history of science I do . . . did. But I still don't altogether like it!

V. KINDI: Did you have students doing philosophy of science?

K. KUHN: No. I have never had a philosophy graduate student. At Princeton I wouldn't have had, at MIT I might have, but I'm so far away from the main sources of jobs in the philosophical tradition that are being fostered by my colleagues. I had one or two people start to do a degree with me, but I've sort of driven them away. One of them finally looked at me in one discussion and said, and he was very good about it, "You really think this is off the wall, don't you?" and I said, yes, I do. He picked himself up and went to find another thesis director and did it on a different thing. The other one we finally asked to accept a master's, and go do something else. Those are the two people who were in philosophy departments, and that was at MIT. I have given seminars for philosophers, occasionally at Princeton and regularly [at MIT?], and I've had some very good interactions there. That's something we can come back to.

One thing I realize I left out before, that should be filled in, and that is the question as to where I got the picture that I was rebelling against in *The Structure of Scientific Revolutions*. And that's itself a strange and not altogether good story. Not altogether good in the sense that I realize in retrospect that I was reasonably irresponsible. I had been, as I'd said, vastly interested, caught a real interest in philosophy in my freshman year, and then had no opportunity to pursue it—initially, at least. It then turned out that after I graduated and went to the Radio Research Laboratory, and indeed continuing most of the time while I was in Europe—I was no longer having school assignments and papers to write—I had what was basically a nine-to-five job, and I suddenly had time to read. And I started reading what I took to be philosophy of science—it seemed the natural place to be reading. And I read things like Bertrand Russell's *Knowledge of the External World*,[27] and quite a number of others of the quasi-popular, quasi-philosophical works; I read some von Mises; I certainly read Bridgman's *Logic of Modern Physics*;[28] I read some Philipp Frank; I read a little bit of Carnap, but not the Carnap that people later point

27. B. Russell, *Our Knowledge of the External World*, 2d ed. (London: Allen & Unwin, 1926).
28. P. W. Bridgman, *The Logic of Modern Physics* (New York: Macmillan, 1927).

to as the stuff that has real parallels to me. You know this article that recently appeared.[29] It's a very good article. I have confessed to a good deal of embarrassment about the fact that I didn't know it [the Carnap]. On the other hand, it is also the case that if I'd known about it, if I'd been into that literature at that level, I probably would never have written *Structure*. And the view that emerges in *Structure* is not the same as the Carnap view, but it's interesting that coming from what were partially different . . . Carnap staying within the tradition had been driven to this—I had rebelled already and come to it from another direction, and in any case we were still different. But that was the state of affairs in my mind at the time that I had this experience of being asked to work in the Conant course. And it was against that sort of everyday image of logical positivism—I didn't even think of it as logical empiricism for a while—it was that that I was reacting to when I saw my first examples of history. We have gotten me to Princeton . . .

A. BALTAS: Yes, you have *Structure* out, you have started the project about the quantum mechanics sources, you are moving to Princeton . . .

T. KUHN: Yes. Well, I really have finished the Archives, I mean I still had to do with some of the putting together of the catalogue after I got to Princeton, and that took a good deal of my time during the first year at Princeton.

A. BALTAS: Perhaps a good way to continue is to ask you the following question: You have *Structure* out in '62. The way we have perceived things, which may be wrong, is that the big boom, as it were, the big explosion of the reception of *Structure,* arrives after '65, more or less—when you had this London thing. I mean *Criticism and the Growth of Knowledge* gets published in the '70s or thereabout, or there are rumors already circulating about your debate with Popper, or things like that. This is a kind of image we have, which may be completely wrong.

T. KUHN: I can't tell you you are wrong, I'm a little surprised at it; I would not have told the story that way. But it's very possible that the evidence will show me wrong. I would say myself . . . It built up a year at a time more or less under its own momentum, and I would not have thought that there was any particular burst in connection with '65. On the other hand, what may well have happened in '65,

29. G. Irzik and T. Grunberg, "Carnap and Kuhn: Arch Enemies or Close Allies?" *British Journal for the Philosophy of Science* 46 (1995): 285–307.

or as a result, is that the philosophers began to pay more attention. I mean, a lot of the early audience was social scientists. But not just social scientists. I mean, the book was well reviewed by Shapere in the journal that's published from Cornell.[30] Well reviewed, except for some strong reservations which I thought were largely incorrect. People picked on 'paradigm' from the beginning, and I don't think they were wrong to do that. It made it harder for me to recall people to what I was really after; and if I had seen what I had done myself, I could have done that better. I have impressions but I'm not at all sure that they're accurate, and some of them come from certain feelings of disappointment, or whatever. The early reactions—the book got good reviews.

A. BALTAS: In what kind of journals, philosophy mostly or . . .

T. KUHN: I'd almost have to go back and look at the file in which I have reviews. Pretty widely; probably not mostly in philosophy journals. But it wasn't only the Shapere review. Mary Hesse wrote a review in *Isis,* that I do remember . . . I gradually realized that a lot of the response was coming from social scientists, and I was quite unprepared for that; I thought of the book as directed to philosophers. And I think not a lot of them read it, I think it was picked up much more widely than that; it was no particular force for some time in philosophy, although the philosophers surely knew it. But I remember—I guess it was Peter Hempel's telling me that he had been to a meeting, I think it was in Israel, at which groups had said, "That book should be burned!" and "All this talk about irrationality! . . ." Irrationality in particular, irrationality and relativism—the thing that bothered me about the Shapere review is all the talk about relativism. I saw why he said that, but I thought, if he'd thought a little bit more seriously about what relativism was and what I was saying, he would not have said anything like that. If it *was* relativism, it was an interesting sort of relativism that needed to be thought out before the tag was applied. In practice, I would say it's not a relativistic book. And although I would have had trouble initially, I tried in the end of *Structure* to say in what sense I thought there is progress. I largely squeezed out the answer to that, talked about the accumulation of puzzles, and I think I would now argue very strongly that the Darwinian metaphor at the end of the book is right, and should have been taken more seriously than it was; and *nobody* took it seriously. People passed it right by.

30. *Philosophical Review* 73 (1964): 383–94.

delete quotes and comma after i.e.

This question of stopping to see us, i.e., "ceasing to see us," as getting *closer to* something, but see us instead as moving *away from* where we were—that was beyond anything I'd really quite grasped until the point at which I had to really wrestle with that problem. But saying that was important to me and it led to things that have happened since. And I think it might have been picked up and recognized more. And what went with all of this, Vasso, I saw in one of your papers,[31] which talks about how just the things that made me unpopular in the sixties make me popular in the eighties. And that's I think a very revealing and very apt remark, but it's wrong in one respect: the sixties were the years of the student rebellions. And I was told at one point that "Kuhn and Marcuse are the heroes at San Francisco State [University]." Here was the man who had written two books about revolutions . . . Students used to come to me saying things like "thank you for telling us about paradigms—now that we know what they are we can get along without them." All seen as examples of oppression. That wasn't my point at all. I remember being invited to attend and talk to a seminar at Princeton organized by undergraduates during the times of trouble. And I kept saying, "But I didn't say that! But I didn't say that! But I didn't say that!" And finally, a student of mine, or a student in the program who had sort of helped get me into this, and had come along to listen, said to the students, "You have to realize that in terms of what you are thinking of, this is a profoundly conservative book." And it is; I mean, in the sense that I was trying to explain how it could be that the most rigid of all disciplines, and in certain circumstances the most authoritarian, could also be the most creative of novelty. And to cut my way through that aporia, I had to set it up; but of course to set it up as an aporia ran into all sorts of resistance. So, it's hard to say how I felt. I thought I was being—I want to say badly treated—badly misunderstood. And I didn't like what most people were getting from the book. On the other hand, I did not for a moment think that that's all that was going on. There were people who picked it up and really did seem to be moving it ahead and getting on with it, probably initially more in the ways that at the beginning some of the sociologists picked it up. I got very good initial responses from scientists.

A. BALTAS: Physicists, biologists . . .

31. V. Kindi, "Kuhn's *The Structure of Scientific Revolutions* Revisited," *Journal for General Philosophy of Science* 26 (1995): 75–92.

T. KUHN: Yes, both. Various people reported to me that this book was the first thing in philosophy that they'd read, that really felt to be about what they were doing. And I valued that, and there were other things . . . I mean, clearly, I wanted it to be an important book; clearly it was being an important book—I didn't like most of the ways in which it was being an important book, but on the other hand, I recognized that if I had to do it all over again, I could, if I had the opportunity, eliminate some of the misunderstandings. But if I couldn't do that, I'd do it all over again the way it was. I mean, I had disappointments, I didn't have regrets.

A. BALTAS: Are there any incidents in your discussions with philosophers that were significant, in both ways as regards your own perception of what you had been doing, as well as regarding the overall reception of the book. Some incidents in conferences, or people who talked to you about it and then gave you a new light . . .

T. KUHN: Initially not a lot. I got invited to talk at a couple of places, and I was glad to, but I wasn't very well received. I was not really getting through to philosophers, although some of them were very interested. When I got to Princeton, I began to work a good deal with Peter [Hempel]. This was the first philosopher, I guess of any sort, but certainly the first philosopher in the logical empiricist tradition who began to respond, and to respond seriously to what I was doing. And his position along the way has not become mine and there is no Wittgenstein in it. But it shifted markedly in ways that I think are important. And when I used to try to compare the two traditions, I used to point to the time—which I'm not sure is my responsibility, but I think it may well be—at which Hempel, instead of talking about theoretical and observational terms, started to talk about *antecedently available terms*. And that in itself is already putting things in a sort of historical developmental perspective. I don't think he saw it quite that way, but it was a very important step.

V. KINDI: What about the other philosophers of science of the period—Feyerabend or Lakatos—you were all received together, in a sense.

A. BALTAS: The historicist turn, as it was dubbed.

T. KUHN: It's hard to talk about that. Certainly there were some philosophers who took it up and more than a few. And they began to talk about the historical philosophy of science. From my point of view, I was glad to see that, but it struck me very forcefully that all of them entirely dropped the problem of *meaning* when they made that turn, and that they therefore dropped incommensurability, and that they

therefore were back having eliminated [what for me was] the philosophical problematic. With Feyerabend I had strange experiences. He was at Berkeley, I gave him this draft manuscript of the book that I'd sent out to Chicago. I think he liked it in one sense, but he was terribly upset by this whole business of dogma, rigidity, which of course is exactly counter to what he believed himself. And I couldn't get him to talk about anything except that. And I tried, and I tried: we would have lunch together, or something—he'd always come back to it. I got more and more frustrated and I finally just stopped trying. So, he and I never really did have good talk about these problems. The quasi-sociological elements of my approach were overwhelmed by his desires for society in the ideal. And we really never made contact.

V. KINDI: What about the ones that come after, like Laudan or van Fraassen? It seems that the field has stopped dealing with the phenomenon of science as a whole, with the kind of questions you raised, and has now gotten back to the standard problems of philosophy of science, induction, confirmation, Bayesianism . . .

T. KUHN: I'm surprised that you include van Fraassen.

V. KINDI: I don't mean that he belongs to the historicist tradition. I include him because he dealt with issues like the theory/observation dichotomy.

T. KUHN: But that was long before me.

V. KINDI: Didn't you contribute to its being undermined?

T. KUHN: It was being undermined already. He in a sense, I think, was trying to get it back again, to show that it was still a viable notion. And I wasn't the primary underminer of it. The theory/observation distinction was in trouble already before me. Putnam was undoubtedly a more important person than I for the philosophers in undermining the distinction. There are several very important Putnam papers.

A. BALTAS: There is a certain incommensurability going at this moment, because in the following sense I think it's rather different in the States. It's better to clarify it. I think that in Greece for sure, and I think also in places like Italy, or France, I don't know much about the other places, the perception of this period is the following: you have logical positivism with its own problems, etc., some criticism within the tradition, and then *you* come along and change the paradigm, so to speak. And by changing that, people who already had done parallel things, let's say, criticizing the main logical empiricist framework, join forces, as it were, with you, not in the real sense of writing papers together, but you are all perceived . . .

V. KINDI: You opened up the field . . .

T. KUHN: I'm sure that's right, but I nevertheless was quite surprised to have van Fraassen and Laudan put into the same . . . Laudan is somebody who said he was doing historical philosophy of science. He says things about me that are absolutely not the case. People point it out to him and he goes right on saying them. He tries to hold on to the traditional view of scientific progress, closer and closer to the truth, absolutely dropping the problems that [I had] pointed out. From my point of view, that's very bad stuff!

Somebody who has done both philosophy and history, and who has encouraged me and whom I'm very fond of, is Ernan McMullin. He's really been a supporter over the years. He doesn't like some things I would try to persuade him he ought to like, but that's been helpful. What I have found is that now—and this I delight in—as historians of science turn further and further from scientific substance, a number of important *philosophers* of science have gotten more and more involved with doing some history. And they do it more nearly the way I would like to see it done. And that's been a very agreeable route to watch.

V. KINDI: Who are the philosophers you refer to now?

T. KUHN: Well, John Earman has done some of it. Clark Glymour has done some of it, but I don't think that comes from me, though Earman probably does. John was one of the products of the Princeton program in philosophy and history of science from the philosophy end. I had him in a seminar the first year I was at Princeton, and I talked with him some after that, and he went on. So that had a role. Ron Giere is another person who began to do some of this. There was an increasing influence of a different approach to historical examples among the philosophers of science gradually. I first discovered how much the change had been when I was suddenly elected president of the Philosophy of Science Association, about five or six years ago. And I had not even been a member of the Association, except for one year after which I dropped out, or something of the sort. The scene among philosophers of science was just very very different. And I had clearly had something important—though not I alone—to do with that. It becomes important to remember Russ Hanson, and to some or lesser extent Polanyi, Toulmin. I think Russ Hanson was probably more important than either of those. Feyerabend and so forth. There were a whole bunch of people moving in this direction. I don't think that the people who were doing history, by and large, saw everything in it that I was

seeing in it. They were not coming back asking "What does this do to the notion of truth, what does it do to the notion of progress," or if they did, they were finding it too easy to find answers that seemed to me superficial. It isn't that I knew the answers, but I didn't think that theirs were ones that were going to withstand the scrutiny that they needed. I was worrying about it, I mean I was back to writing history for a change. But I wanted nothing more than to get back and straighten out these problems—and I really didn't know how to do it—and I kept saying, it's like going around on a stage set, opening doors, to see which ones have just a painted canvas behind them and which one will lead into another room. Well, I gradually found some that led into another room, or part way into another room: the causal theory of reference. Kripke made a very big difference,[32] because I was absolutely persuaded that it was a breakthrough with respect to proper names—but then it didn't work for the other things, common nouns. Putnam's stuff also helped—but I simply couldn't reconcile myself to saying, "If heat is molecular motion, then it always was molecular motion." That just wasn't where the action was. But I got some very important tools out of that, and one of them was to go back and think about the Copernican revolution, and suddenly realize, look, you can trace the individual planets, Mars, heavenly bodies through the Copernican revolution—what you can't trace through it is 'planets'. Planets are just a different collection before and afterwards. There was a sort of localized break that fitted very closely. And now it turns out that some people, to an extent that surprises me and others, simply say, "in the Ptolemaic system the planets go around the earth and in the Copernican system they go around the sun." But that's an incoherent statement! And it is! It's too easy to get around it, because you then start doing: there is a finite number of planets, and they have proper names, and you do it that way. But it isn't wrong that the statement is incoherent. It's highly suggestive of this sort of thing, and I think one needs to talk about it. It's always been clear to me also, or clear to me for some time, that the two people I was sure were taking the problems I was looking at seriously were me and Hilary. When Hilary started talking about internal realism. I thought, hell, *now* he's talking my language. Well, he sort of stopped talking my language. But at this point these problems were getting to be important in philosophy in ways that they had not been before. Nobody

32. S. Kripke, *Naming and Necessity* (Cambridge, MA: Harvard University Press, 1980).

could reasonably show anything but respect for Putnam; but they could joke about him a bit, because he went so far and then took so much back, and wrote the same thing so many times changing it each time. For the Putnam who had written a paper about incommensurability called *How Not to Talk About Meaning*,[33] in which the whole thing was the rope—you could change one strand or change another strand but it was still the same rope, and therefore there wasn't the sort of problem that Feyerabend and I were talking about—that was a big step . . . internal realism and the things that went with that. And causal theory seems to me—thinking about causal theory had been very important to me. I don't think it works for common nouns. But it's terribly interesting to see why it seems to work. And that becomes clearer in this thing I'm working on now—the senses in which it almost works. It doesn't work across periods of revolution or whatever, but it works very well between those. And by the time you reconstruct what has occurred after a revolution, you get it back to seeming to have worked again. In this paper I mentioned the other day of mine, called *Possible Worlds and History of Science*,[34] I talk about what's wrong with Putnam's "water is and always was H_2O." And this has gradually been sneaking into philosophical discussion. And I have felt rather good about that, and I think I am more read in philosophy courses than I used to be, and more talked about, and I have more influence. But I have to say, as I've already been saying, I have never been in the sort of lovely situation that you people have put me in here. And it's extra agreeable by virtue of this background.

A. BALTAS: What about the *Black-Body* [book]? I mean you have become a big success with the *Scientific Revolutions*, some might expect that you go on from there, explain yourself better than the *Postscript*[35] and things like that; and a book comes out which, apparently at least, does not look like one much expected . . . for example, in application let's say, in quotation marks.

T. KUHN: I have said repeatedly, and I will say again: you cannot do

33. H. Putnam, "How Not to Talk about Meaning," in *Boston Studies in the Philosophy of Science*, vol. 2, *In honor of Philipp Frank*, ed. R. Cohen and M. Wartofsky (New York: Humanities Press, 1965); reprinted in *Mind, Language and Reality*, Philosophical Papers, vol. 2 (Cambridge: Cambridge University Press, 1975).

34. T. S. Kuhn, "Possible Worlds in History of Science," in *Possible Worlds in Humanities, Arts and Sciences: Proceedings of Nobel Symposium 65*, ed. Sture Allén, Research in Text Theory, vol. 14 (Berlin: Walter de Gruyter, 1989), pp. 9–32; reprinted in this volume as essay 6.

35. T. S. Kuhn, "Postscript," in *The Structure of Scientific Revolutions*, 2d ed., rev. (Chicago: University of Chicago Press, 1970), pp. 174–210.

history *trying* to document, or to explore, or to apply a point of view that is as schematic . . . Clearly, I do history differently, because of things that I think I have learned, that lie behind *The Structure of Scientific Revolutions,* and that were perhaps further developed. I like doing history, and I've gone back and forth—you can't do the two things at the same time. The philosophy has always been more important, and if I had seen a way to go back and work on the philosophical problems straightforwardly at the time I wrote the *Black-Body* book, I probably would have. Look, I'll tell you just how far this goes. Before the book came out, I'd agreed to talk with a group of people about this book, and when I got to this small group that was supposed to sit around the table it turned out the room was full, or more or less full. So, I had to give an impromptu lecture, and I did. When it was done, somebody held up their hand, and said, "It's all very interesting, but tell me, did you find incommensurability?" I thought, "Jesus! I don't know, I haven't even thought about that." Now, yes, I mean I *had* found it, and I later recognized what it was, recognized it particularly when I began to get reviews from people, like Martin Klein: it had to do with the energy element *h*ν. I mean, there is an aspect of that that's talked about in the book. I talk about Planck's letter to Lorenz in 1910 or 1911 in which Planck says it's the switch from resonator to oscillator. He says, "you will see I have stopped calling them resonators, they are oscillators"; and my view is that that is a very significant switch. Resonators respond to a stimulation, oscillators just go back and forth. And others, I mean . . . the energy "element" of Planck's was not to be understood the way the energy "quantum" of Plank's is to be understood, and so forth. So, it's in there, but I wasn't looking for it particularly. And the reason for telling you this story about the question, was simply to tell you I hadn't thought about it! It was a perfectly good question; I later realized how to answer it, but it just floored me at the time, and I sort of stammered around.

V. KINDI: Because you don't apply the philosophical theory to do history.

T. KUHN: No. If you have a theory you want to confirm, you *can* go and do history so it confirms it, and so forth; it's just not the thing to do.

A. BALTAS: Because there has been a lot of discussion regarding the relation between history and philosophy of science, what kind of advice would you give, what is your position in the sense of giving some advice to younger people who want to do either or both.

V. KINDI: You said something at the lecture, like getting into their minds . . .

T. KUHN: Yes, and that's what I think the intellectual historian has to do. It's exactly what the philosophers systematically resist doing. But the way they tell the story—history of philosophy telling the story of Descartes, what he got right and what he got wrong, and what could have been done to bring the two together.

I wrote a paper on the relations of history and philosophy of science[36] in which I insist that, although I'm the chairman of a program in "history and philosophy of science," there is no such field. And I tried to talk a little bit about my experience of having philosophers and historians and scientists in the same classroom. The philosophers and the scientists are much closer to one another, because they all come in being concerned about what's right and wrong—not about what happened—and therefore tending to look at a text and simply pick out the true and the false from a modern point of view, from what they already know. The historian, at least doing it my way, insists on saying: that's a respectable human being, [so] how could he ever have thought anything of the sort? A remark of Vasso's in her work that I particularly like is, just this. . . . I mean, yes, people treated me as though I were a fool! I want to say, how the hell could anybody ever have thought that I would believe anything like that! That really was fairly destructive, and I fairly early simply stopped reading the things about me, from philosophers in particular. Because I got too angry. I knew I couldn't answer, but I got too angry trying to read them and I would throw them across the room, and I wouldn't finish them, and I would miss anything that might have been helpful in it, by virtue of this rage. It was too painful.

About history and philosophy . . . So, I said these are very different fields. I speak of it as different ideologies, as different goals, and correspondingly different methods, different senses of what it is obligatory to be responsible for. Both of them will say "yes, but that's trivial, but that doesn't matter." But the historians and the philosophers feel licensed, and able, to say that at very different places. On the other hand, it is my sense that there can be quite a lot of cross-fertilization, if you can get some interaction between the two, which becomes harder rather than easier in one sense, because the historians have

36. T. S. Kuhn, "The Halt and the Blind: Philosophy and History of Science," *British Journal for the Philosophy of Science* 31 (1980): 181–92.

stopped dealing with technical issues. But it is the case, I think, that there is quite a lot to be done in interaction, and at least I produce myself as an example, because I'm never a philosopher and a historian at the same time, but the two do interact. And that's the ideal arrangement, from my point of view.

K. GAVROGLU: After the appearance of *Structure* . . . and not totally independent of it, history of science branched into quite a few well-articulated approaches. What has come to be known as the Strong Program has been the most controversial. Although you have, not very systematically, expressed your thoughts on the Strong Program, I think it may be interesting to tell us your views as regards the scholarship of the Strong Program.

T. KUHN: Let me tell you two stories. One is when I read that lecture on the relations between the history and philosophy of science, a philosopher came up to me afterwards and said, "But we have such good *scholarship!* We have such good *scholars* in the history of philosophy!" Yes, but they're not doing history. I mean, I didn't say that then. But I say this because you used the term, what do I think of the scholarship. The scholarship is often damn good! You and I have talked about *Leviathan and the Air Pump*,[37] in which I think the scholarship *is* very good, and I think it's quite a fascinating book. It upsets all hell out of me that they [the authors] can't understand what everybody now learns in high school, or even elementary school about the theory of the barometer . . . They talk basically about the "emptiness" of the dialogues between Hobbes and Boyle, and they get it all very badly wrong. I said to you when we talked about this before, they talk about Boyle's switching back and forth between talking about "pressure" and talking about "the spring of the air." It's not a consistent way of talking, but there's a very important reason why he hasn't yet quite gotten out the way to talk about it; it's not things that don't matter. He's using a hydrostatic model. Hydrostatic models deal with an incompressible fluid. Air is not an incompressible fluid. So, what you get in one case from straight pressing down, you can get in the other also by compression; and you can get these two together. But the fact that he is going back and forth between the two . . . they are not incompatible, they are different ways of talking about the same thing,

37. S. Shapin and S. Schaffer, *Leviathan and the Air Pump: Hobbes, Boyle, and the Experimental Life* (Princeton: Princeton University Press, 1985).

but they had better get more deeply integrated than they are at the point that Boyle's talking about it; there is an incompleteness there. And again, they talk about the impossibility of proving that the barometer explanations which talk about subtle fluid coming in and filling up the top—you can't indeed show those to be wrong—these are the antiperistalsis explanations. Of course you can't show them to be wrong, but what [Shapin and Schaffer] totally miss is the vastly greater explanatory power that comes, including that straightforwardly with the Puy-de-Dôme experiment and many others. So that there is every rational reason to switch from one of these ways of doing it to the other, whether you think you've shown that there is a void in nature or not. And that sort of thing bugs me. And as I also said to you, Kostas, what really bothers me most about it is that history of science students themselves now don't care. I talked to Norton [Wise] about this, and he had sort of gotten me to read *Leviathan and the Air Pump*. And I think it's in many ways an extraordinarily interesting and good book. So it's not the scholarship that's bothering me. Norton, who is a physicist himself, thought about it, he felt I was right, so he told his class about them. He told me that nobody in the class could see that it was of any importance. At that point I'm bothered. I mean, that to me is a bother. Now, the whole thing is turning around in some ways again, and I don't know where it comes out. It isn't that I think it's all wrong. I said to you that the term "negotiation" seems to me just right, except that when I say "letting nature in," it's clear that that's an aspect of it to which the term "negotiation" applies only metaphorically, whereas it's fairly literal in the other cases. But you are not talking about anything worth calling science if you leave out the role of [nature]. Some of these people simply claim that it doesn't have any, that nobody has shown that it makes any difference. Now, I don't think they are saying that any more, but I don't think it's coming back to the point where they've really got room for it . . . I haven't read Pickering's latest book, *The Mangle of Practice*.[38]

V. KINDI: What about the Stegmüller group?

T. KUHN: Look. I don't know, it's moot how much I had to do with Sneed. I met their work through Stegmüller. Stegmüller sent me a copy of the journal—I guess I talked to Aristides about this, and I

38. A. Pickering, *The Mangle of Practice: Time, Agency, and Science* (Chicago University of Chicago Press, 1995).

think I'd be glad to have it on tape. He sent me a copy of *Theorienstrukturen und Theoriendynamik*,[39] with a very nice inscription, and a card in which he described himself as a Carnapian who was perhaps becoming a proto-Kuhnian, or something of the sort. I started looking at the book and I realized, I've got to learn to read this, but it was all in German, and it was all in set theory, and I didn't know set theory, and I didn't know what a function was, represented as in set theory. I still don't really know model theory and I didn't have the German vocabulary for them either. But I realized I had to read the book; it took me two or three years—a year and a half maybe. I used to carry it on planes, and so forth, and nibble at a little more of it. I thought it was immensely exciting! The thing that I could not support about it was the reduction thesis, which was basically a one-language thesis again. I thought what he said about paradigms came closer to what I had had in mind than anything I had seen by a philosopher! Or anybody else for that matter—it was paradigms as examples, and this was a point of view that said, you don't have a structure unless you include in it at least a few examples. Now, those were terribly exciting things, and furthermore some of the things I have been doing since have fed off of their discussion in certain ways. I mean, the stuff about learning force, mass, and so forth in that paper really I probably would never have written without having been exposed some years before to the Sneed-Stegmüller stuff. And I think it's absolutely first class. Certainly it's had an impact on me, and that will show again, and I will talk about it a little bit in this book that I'm doing. I said to Sneed, I don't think you got to this through me, and he said, "don't be too sure, I had read you!"

I tried to get philosophers more interested in that stuff. And on the whole, for a long time, I couldn't succeed at all. Now everybody is talking about the semantic view of theories—but on the whole leaving Sneed and Stegmüller out. And I think now I see the reason; I'd been looking at Fred Suppes's book, and I think I see what that's about. They don't want to get back to anything that looks like . . .

A. BALTAS: Models?

T. KUHN: Well, it's not that. I mean, structures are formal. They see the Ramseyfication, the use of Ramsey sentences, as reintroducing—

39. W. Stegmüller, *Probleme und Resultate der Wissenschaftstheorie und analytischen Philosophie*, vol. 2, *Theorie und Erfahrung*, part 2, *Theorienstrukturen und Theoriendynamik* (Berlin: Springer-Verlag, 1973); reprinted as *The Structure and Dynamics of Theories*, trans. W. Wohlhueter (New York: Springer-Verlag, 1976).

and this is why Sneed does it—something like the theory/observation distinction. And they think that's got no place any more. But unless you have something like that—it's not just theoretical/observational, I don't believe in that either—but what it sure is, is antecedent vocabulary, or shared vocabulary . . . If you take a dynamic view, you've got to have something that talks about revision of terminology and introduction of new terminology as part of the introduction of a new theory, of a new structure. And I don't think you can do it without that, and that's why I would still point back to the Sneed-Stegmüller version as the one that best fits with what goes on. It adapts itself to a historical developmental approach.

K. GAVROGLU: Even though you had at least two students in the history of science, you had none in the philosophy of science.

T. KUHN: I've never directed a philosophy graduate student.

K. GAVROGLU: We can go to the last question.

T. KUHN: Yes. Look, there is only one of my students—he didn't take a degree with me—and that's Jed Buchwald, who does the sort of history of analytic ideas that I do, and I love doing. I was the person who turned him on to history of science as an undergraduate; he took his degree at Harvard. And things have gone on since. But all of my other students have, in one way or another, gone more toward . . . no, this isn't quite true . . . but mostly they have turned to things that are much more science and society oriented, social environment of science, institutions, and so forth. Which is a natural development given where the field has gone, and given the need of any graduate student to get his independence from Papa. But I'd have been glad if Jed weren't the only one who was carrying this on.

A. BALTAS: I have two questions—the one is not really a question, but you said you have not finished with Princeton yet, you may want to add something about your colleagues, the atmosphere of the students . . .

T. KUHN: Look. There is only one thing I would really like to add. I liked Princeton a lot. I had good colleagues, I had good students. I didn't get very far trying to talk to the philosophers, and one of the advantages of being at MIT is that the philosophers are not quite so sure they're as good as the ones in Princeton. So, it's easier to get through to them—it's not very easy anyway, because they *are* very good. Things worked for me there very well, and the reason I left and went to MIT was because I was divorced. It wasn't anything about MIT versus Princeton as such. Look, there is one thing that should

be mentioned. After I had been there . . . I'm not sure what the date would have been, fairly late, but not the end of my time in Princeton, Princeton announced that it would be willing to let people negotiate reduced workload for reduced salary. My mother had died in the interim, I could afford to do that, and I wanted more time to do my own work. And I did do that; and then I got invited to have a long-term membership, which was not on the faculty, at the Institute for Advanced Studies. So I had an office over there. That led me to interact with people some of whom perhaps I would not have known at all, and some of those interactions had anything from a little to a lot of importance. The one that had a lot of importance, I think, was with Clifford Geertz, the anthropologist. A couple of other people whom I knew and liked and got encouragement from, but I don't think there was much in the way of ideas feedback—one is Quentin Skinner, who is a philosopher, political scientist at Cambridge, England, and the other is a young historian now in the political science department at Chicago called William Sewell. Both of whom do things in ways that are deeply empathetic.

K. GAVROGLU: Why was it MIT and not Harvard?

T. KUHN: Harvard didn't want me. Furthermore, if [Harvard had asked me and] I could have resisted, I would have been very well advised to. When it was known that I was looking, Harvard did not ask me, MIT did. But you know enough about the Harvard department to see why that might have been the case.

A. BALTAS: The other question is a kind of political statement vis-à-vis traditions in philosophy. Because one of the ways you've been perceived—and I think in a private discussion we've more or less agreed upon that—is that your work has not explained perhaps partly why it had this influence. It's a kind of work that crosses boundaries among philosophical traditions. You cannot be branded as a continental metaphysician, but on the other hand, you cannot be branded as somebody who doesn't know his logic and theories of explanation, and things like that. So, given that this split is somehow getting bridged, how do you see yourself in this?

T. KUHN: Oh, I thought you were going to ask me about traditions in philosophy. Look, there's got to be something right about that, I mean you start out by saying, here's a man who was never trained as a philosopher, who's been an amateur increasingly learning things about it by himself, from interactions and so forth—but not a philosopher. A

physicist turned historian for philosophical purposes. The philosophy I knew and had been exposed to, and the people in my environment to talk to, were all of them out of the English logical empiricist tradition, in one way or another. This was a tradition which by and large had no use for the continental and particularly the German philosophical tradition. I think, in some sense or other, I can be described as in some part having reinvented that tradition for myself. And clearly it's not the same, and there are all sorts of ways in which it goes in other directions and past and so forth—there's a whole body of work there that I don't even know very well. But when people say, didn't Heidegger say that, or something of the sort, yes, he probably did, and I haven't read it, and if I had I'd like to think this was going to help bridge the gap. And I think that's part of what it's doing. It's a sort of view I've had, but this is not a statement about philosophical traditions in general; you wouldn't have philosophy without traditions.

K. GAVROGLU: Who was the public Kuhn?

T. KUHN: The answer to the question, Kostas, is complicated. I knew someone at Princeton, who congratulated me on avoiding being a guru. It would be too much to say quite that I didn't want it, although in all sorts of ways I didn't want it, it scared the shit out of me. I mean, I am an anxious, neurotic—I don't bite my nails but I don't know why I don't bite my nails . . . So, I tend quite hard to avoid invitations to be on TV; I've had a few, I haven't had very many, but that's partly because the word gets around that I turn them down. I feel somewhat the same way about interviews, although I have given a few interviews, but I try to set up conditions (a) that I'm not being interviewed by anybody who doesn't know my work, including my more recent work, and (b) that I get a look at the script before it's published and I retain some control. And those are not agreeable conditions in the great world in which interviewing gets done. So there haven't been many interviews, which is a break. That's not a comment on this one . . . Look, I'm saying some things that I'm glad to think will be around somewhere.

K. GAVROGLU: But I remember once you had mentioned to me, and then somehow we couldn't continue the discussion, this trial about creationism to which you were requested to go, in Arizona.

T. KUHN: Look, that one I declined for I think an excellent reason. [The people who approached me were resisting the creationists. I was sympathetic, but] I didn't think there was a chance in the world . . .

I mean I was being used by the creationists, for God's sake![40] At least to some extent. And I didn't think there was any way in the world in which somebody who didn't quite believe in Truth, and getting closer and closer to it, and who thought that the essence of the demarcation of science was puzzle solving, was going to be able to make the point. And I thought I would do more harm than good, and that's what I told them.

K. GAVROGLU: What about when you were at the National Science Foundation and the guidelines in research in the history and philosophy of science?

T. KUHN: I was certainly on various committees way back in the old days. On committees, particularly fellowship committees, to go over fellowship applications, but I was on various other committees also. I thought of that as a professional obligation and I did it. But I never tried to play a leadership role. And the few times I did, in some sense or other, get very angry about something that was going on, I was totally ineffective—in part because I was angry.

A. BALTAS: You have not discussed at all your recent work, what you are working on now, but perhaps you can give us an idea of what is the state of the field today.

T. KUHN: Which field?

A. BALTAS: Both. I mean, both history and philosophy of science.

T. KUHN: I'm not close enough to history of science. I mean I really, as I've gone on now in the last ten, fifteen years, really trying now to develop this philosophical position, I have just stopped reading history of science. I have read practically nothing in history of science. Look, the fact of the matter is that the time I stopped reading history of science, not totally but largely, was when I was writing *Structure*. I had to stop reading for that. By the time I got back out of that, the literature had expanded so large. Now, that doesn't mean that I stopped reading it; I went on reading stuff in the fields that I was working on right on through the time at Princeton, and I kept up with the literature some. Nobody could keep up with it in its entirety any more, and I didn't even try. But now I'm not doing it at all. And I see references to one thing or another, and I think, that sounds very interesting and I didn't know it was there. So, I don't want to comment on the state of the field, except to the sense that I already have com-

40. They quoted Kuhn in support of their anti-science position.

mented by saying, I wish there were more attention to the internalities of science. Beyond that I don't want to go.

A. BALTAS: And philosophy of science?

T. KUHN: [conspiratorial whisper:] I think everybody's waiting for my book!

V. KINDI: We sure need it. Do you have any other interests . . . like listening to music, an interest in painting . . .

K. GAVROGLU: Obsessions. Obsessions except philosophy of science.

T. KUHN: You don't really want to know! I read detective stories.

V. KINDI: Oh, that sounds like Wittgenstein.

T. KUHN: And I like music all right—it took me a long time to discover that I did, in part because I had a musical father and a very musical younger brother, and that was not good for my relation to music. People used to have symphonies on the record player, or I was taken to symphonies; I didn't like that, I mean they bored the hell out of me. When I discovered chamber music, my feelings changed. I don't listen a lot, it's hard for me to sit still, but I do like it. And that we still do, we don't go to concerts much, which is partly for one reason and partly for another. I like the theater, though we don't see much theater. I like, or used to like, to read. But most of what I read now is detective stories. I remember, my kids used to sort of ridicule me, or whatever—not ridicule me, but wish—how could I go on with this sort of thing. And I remember when my daughter who has gone into the academic, there she was reading detective stories, and she said to me, "It's the only thing I can read that doesn't feel like work!" That's it! And Jehane was scornful of detective stories when she married me, and she now reads almost as many of them as I do! I'm a corruptor of the mind!

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An earlier version of this bibliography of the publications of Thomas S. Kuhn was prepared by Paul Hoyningen-Huene and published in his book, Reconstructing Scientific Revolutions: Thomas S. Kuhn's Philosophy of Science *(Chicago: University of Chicago Press, 1993). Stefano Gattei updated and expanded that bibliography for Thomas S. Kuhn: Dogma contro critica (Milano: Rafaello Cortina Editore, 2000), which he edited. The editors and the Press thank them both for permitting us to include the bibliography in this volume.*

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Videorecording

The crisis of the old quantum theory, 1922–25. Science Center, Harvard University, Cambridge, MA, 5 November 1980. 120 minutes.

PN / PT

Part ~~II:~~ 2 Comments and Replies

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CT ~~Editors'~~ Introduction

CA James Conant and John Haugeland

In *The Structure of Scientific Revolutions*, as nearly everyone knows, Thomas Kuhn argued that the history of science is not gradual and cumulative, but rather punctuated by a series of more or less radical "paradigm shifts". What is less well known is that Kuhn's own understanding of how best to characterize these episodes itself underwent a number of significant shifts. The essays collected in this volume represent several of his later attempts to rethink and extend his own "revolutionary" hypotheses.

We discussed the contents of the is volume with Kuhn at some length shortly before he died. Although he declined to specify them in full detail, he had a quite definite idea of what he wanted the volume to be. In making this clear to us, he made several explicit stipulations, reviewed with us the pros and cons in several other cases, and then provided four general guidelines for us to follow ~~in filling out the rest.~~ For those readers interested in how the final choices were made, we will begin by briefly summarizing these guidelines.

The first three guidelines that we were given flow from Kuhn's vision of this volume as a sequel to, and as modeled upon, his earlier collection, *The*

Essential Tension, which appeared in 1977. In that ~~selection~~ collection, Kuhn restricted himself to substantial essays that he regarded as developing philosophically significant themes (albeit generally in the context of historical or historiographical considerations), as opposed to those that mainly explore particular historical case studies. Accordingly, ~~these~~ our first three guidelines ~~are~~ were: ~~to~~ include only essays that are expressly philosophical in their concerns; ~~to~~ include only those philosophical essays written in ~~his~~ Kuhn's last two decades;~~[1]~~

1.

~~[1]~~ Kuhn made it clear that those essays with expressly philosophical concerns that he chose to omit from *The Essential Tension* were omitted because he had become dissatisfied with them, and that he did not want them collected in this volume either. In particular, he was adamant that his 1963 essay "The Function of Dogma in Scientific Research" should not be included here, even though it has been widely read and cited.

~~and to~~ include only substantial essays, as opposed to brief reviews or addresses.~~[2]~~

~~[2]~~ A complete bibliography of Kuhn's published work, ~~based on the research of Paul Hoyningen-Huene and~~ compiled by Stefano Gattei, appears at the end of this volume.

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The fourth guideline concerns material that Kuhn regarded as essentially preparatory to ~~series:~~ in effect, early drafts of the book he had been working on for some years. Since it is also part of our charge to edit and publish that work, making use, where appropriate, of this material, we were instructed not to include any of it here. Covered under this restriction are three important lectures: "The Natures of Conceptual Change" (Perspectives in the Philosophy of Science, University of Notre Dame, 1980); "Scientific Development and Lexical Change" (The Thalheimer Lectures, Johns Hopkins University, 1984); and "The Presence of Past Science" (The Shearman Lectures, University College, London, 1987). Although typescripts of these lectures have circulated here and there in samizdat form, and have occasionally been cited and discussed in publications by others,[2]

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[3] Perhaps the most notable of these is Ian Hacking's essay "Working in a New World: The Taxonomic Solution" (in *World Changes: Thomas Kuhn and the Nature of Science*, ed Paul Horwich, ~~editor,~~ [Cambridge, MA: Bradford/MIT Press, 1993]), in which he expounds and attempts to refine the central argument of the Shearman Lectures.

Kuhn did not want any of them published in their present form.

* * * * *

¶ Speaking very broadly, the essays reprinted here can be seen to address four main topics. ~~(The specific topics of the individual essays will be outlined below.)~~ First, Kuhn reiterates and defends his view, going all the way back to *The Structure of Scientific Revolutions* (hereafter # cited as *Structure*), that science is a cognitive empirical investigation of nature that exhibits a unique sort of progress, despite the fact that this progress cannot be further explicated as "approximating closer and closer to reality". Rather, progress takes the form of ever improving technical puzzle-solving ability, operating under strict though always tradition-bound standards of success or failure. This pattern of progress, in its fullest realization exclusive ~~to~~ of science, is prerequisite to the extraordinarily esoteric (and often expensive) investigations that are characteristic of scientific research, and thus to the astonishingly precise and detailed knowledge that it makes possible. ok?

Second, Kuhn develops further the theme, which again goes back to *Structure*, that science is fundamentally a social undertaking. This characteristic shows up especially in times of trouble, with the potential for more or less radical change. It is only because individuals working in a common research tradition are able to arrive at differing judgements concerning the degree of seriousness of the various difficulties ~~which~~ that they collectively face that some of them will be moved individually to explore alternative (often as Kuhn likes to emphasize seemingly nonsensical) possibilities, while others will attempt doggedly to ok?

resolve the problems within the current framework.

The fact that the latter ~~group~~ are in the majority, when such difficulties first arise, is essential to the fertility of scientific practices. For, *usually*, the problems can be resolved and eventually are. In the absence of the requisite persistence to find those solutions, scientists would not be able to home in, as they do, on those rarer but crucial cases in which efforts to introduce radical conceptual revision are fully repaid. On the other hand, of course, ~~were~~ if no one were ever to developing possible alternatives, major reconceptions could never emerge, even in those cases ~~where~~ in which they genuinely become a necessary. Thus, a *social* scientific tradition is able to "distribute the conceptual risks" in a way that would be impossible for any single individual, and yet is prerequisite to the long-term viability of science.

Third, Kuhn spells out and emphasizes the analogy, barely hinted at in the closing pages of *Structure*, between scientific progress and evolutionary biological development. In elaborating this theme, he plays down his original picture, ~~which had~~ of periods of normal science within a single area of research punctuated by occasional cataclysmic revolutions, and introduces in its place a new picture, ~~which has~~ one of periods of development within a coherent tradition divided occasionally by periods of "speciation" into two distinct traditions with somewhat different areas of research. To be sure, ~~in such cases~~ the possibility

remains that one of the resulting traditions may eventually stagnate and die out, in which case ~~you~~ we have, in effect, the older structure of revolution and replacement. But at least as often in the history of science both successors, neither quite like their common ancestor, flourish as new scientific "specialties". In science, speciation is specialization.

Finally, and most importantly, Kuhn spent his last decades defending, clarifying, and substantially developing the idea of incommensurability. This theme too was already conspicuous in *Structure*, but not very well articulated. It is the feature of the book that was most widely criticized in the philosophical literature, and Kuhn came to be dissatisfied with his original presentation. Commensurability and incommensurability, as presented in Kuhn's later work, are terms ~~which~~ that denote a relation obtaining between *linguistic* structures. There are basically two new points underlying this linguistic reformulation of the notion of incommensurability.

First, Kuhn carefully explicates the difference between distinct but commensurable languages (or portions of languages) and incommensurable ones. Between pairs of the former, translation is perfectly possible: whatever can be said in the one can be said in the other (though it may be considerable work to figure out how). Between *incommensurable* languages, however, strict translation is not possible (even though, on a case by case basis, various

paraphrases *may* suffice for adequate communication).

The idea of incommensurability, as it was elaborated in *Structure*, was widely criticized on the grounds that it ~~made it unintelligible~~ failed to explain how scientists working under different paradigms were able to communicate with one another (let alone adjudicate and resolve their disagreements) across a revolutionary divide. A related criticism ~~was made of~~ concerned the putative explanations of past scientific paradigms furnished within the pages of *Structure* itself: didn't the work undermine its own doctrine of incommensurability by offering illuminating explanations (in contemporary English) of how alien scientific terms were used?

Kuhn here responds to these objections by pointing out the difference between language translation and language learning. Just because a foreign language is not translatable into whatever language one already speaks does not mean that one cannot learn it. That is, there is no reason that a single person cannot speak and understand two languages that he or she cannot translate between. Kuhn calls the process of figuring out such an alien language (say, from historical texts) *interpretation*, and also $\frac{I}{M}$ to emphasize its distinctness from so-called "radical" interpretation (à la Davidson) $\frac{I}{M}$ *hermeneutics*. His own explanations of the terminology from, say, Aristotelian "physics" or phlogiston "chemistry" are exercises in hermeneutic interpretation, and, at the same time, aids to the

reader in ~~coming to~~ learning a language incommensurable with his or her own.

Kuhn's
~~The~~ second main point about incommensurability comprises ~~is~~ a new and fairly detailed account of how and why it occurs in two ~~sorts of~~ scientific contexts. Technical scientific terminology, he explains, always occurs in families of essentially interrelated terms, and he discusses two varieties of such families. In the first variety, the terms are kind-terms -- roughly, sortals -- which Kuhn calls "taxonomic categories". These are always arrayed in a strict hierarchy, which is to say that they are subject to what he calls "the no-overlap principle": no two such categories or kinds can have any instances in common unless one of them entirely and necessarily subsumes the other.

OK? or "is made by"?

Any taxonomy adequate to the purposes of scientific description and explanation is constructed on the basis of an implicit no-overlap principle. The meanings of the relevant kind-terms specifying such taxonomic categories, Kuhn argues, are partially constituted by this implicit presupposition: the meanings of the terms depend on their respective subsumption and mutual exclusion relations (plus, of course, the learnable skills of recognizing members). Such a structure -- which Kuhn calls a "lexicon" -- has, in itself, considerable empirical content, because there are always multiple ways of recognizing (multiple "criteria" for) membership in any given category. Distinct taxonomic structures (ones with different subsumption and exclusion relations)

are inevitably incommensurable, because those very differences result in terms with fundamentally disparate meanings.

The other variety of terminological family (also called a lexicon) involves those terms whose meanings are determined in part -- but crucially -- by scientific laws relating them. The clearest examples are the quantitative variables that occur in laws expressed as equations -- for instance, weight, force, and mass in Newtonian dynamics. Though this ~~sort of~~ case is not as well worked out in the extant Kuhnian texts, Kuhn believed that here, as well, the meanings of the relevant fundamental terms are partially constituted through their occurence in claims -- in this case scientific laws -- ~~which~~ that categorically exclude certain possibilities; hence any changes in the understandings or formulations of the relevant laws must result, according to Kuhn, in fundamental differences in the understandings (hence, meanings) of the corresponding terms, and thus incommensurability.

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This volume

~~The present collection~~ is divided into three parts: two groups of essays, each arranged chronologically, and an interview. ~~The first~~ part 1 includes five ~~self~~ free standing essays presenting various of Kuhn's views as they developed from the early 1980's through the early 1990's. Two of these essays include brief replies

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to comments that were made when the essays were first presented. Although, of course, such replies can be fully appreciated only in the context of those comments themselves, Kuhn is careful in each case to summarize the specific points he is replying to, and the resulting remarks add useful clarity to the main paper. Part ~~two~~ 2 includes six essays, widely varying in length, each of which consists mainly of Kuhn's responses to the work of one or more other philosophers often, though not always, itself developments or criticisms of Kuhn's own prior work. Finally, in part ~~three~~ 3, we have included a lengthy and candid interview ~~of~~ with Kuhn, conducted in Athens in 1995 by Aristides Baltas, Kostas Gavroglu, and Vassiliki Kindi.

Part 1: Reconceiving Scientific Revolutions

Essay #1, "What are Scientific Revolutions?" ~~(~1981), though slightly revised for later presentation and publication on its own, is drawn largely from the first of the Notre Dame lectures (1980), mentioned above. It~~ consists primarily of a philosophical analysis of three historical scientific ~~sea-changes~~ transformations (concerning the theories of motion, the voltaic cell, and black-body radiation), as illustrations of Kuhn's then nascent account of taxonomic structures.

Essay #2, "Commensurability, Comparability, Communicability," ~~(1982),~~ is an elaboration and defense of the importance of incommensurability with regard to

the two principal charges that (1) it is impossible, because intelligibility at all entails translatability, hence commensurability; and (2), if it were possible, it would imply that major scientific changes cannot be responsive to evidence, and therefore must be fundamentally irrational. Versions of these charges made by Donald Davidson, Philip Kitcher, and Hilary Putnam receive particular attention. ~~This essay was written for a symposium (at the annual meetings of the Philosophy of Science Association) in which Philip Kitcher and Mary Hesse served as commentators. Kuhn's replies to their comments are included as a postscript.~~

Essay #3, "Possible Worlds in History of Science" ~~(1989),~~ develops the idea dramatically propounded but not well explained in *Structure* that incommensurable scientific languages (now called lexicons) give access to different sets of possible worlds. In his discussion, Kuhn clearly distances himself from possible-worlds semantics and from the causal theory of reference (along with the associated forms of "realism"). ~~This essay was written for a Nobel Symposium on "Possible Worlds in Science" in which Arthur I. Miller and Tore Frängsmyr served as commentators. Kuhn's replies to their comments are included as a postscript.~~

Essay #4, "The Road Since Structure" ~~(1990),~~ is announced as a brief sketch of the book Kuhn had (in 1990) been working on for just over a decade (the book

he never finished). Though at the highest level the topic of the book is realism and truth, what it mostly will discuss is incommensurability -- with particular emphasis on why ~~that~~ it is not a threat to scientific rationality and its basis in evidence. Thus, in part, the book is conceived as a repudiation of what Kuhn regarded as certain excesses in the so-called "strong program" in the philosophy (or sociology) of science. At the conclusion of the essay (and, in more detail, in the Shearman Lectures), he describes his position as "post-Darwinian Kantianism", because it presupposes something like an ineffable but permanent and fixed "*Ding an sich*". Kuhn had earlier rejected the notion of a *Ding an sich* (see essay #8) and he again later repudiated (in conversations with us) both that notion and the reasons he had put forward for it.

Essay #5, "The Trouble with the Historical Philosophy of Science" ~~(1992),~~ considers both traditional philosophy of science and the now fashionable "strong program" in the sociology of science, and what is wrong with each. Kuhn suggests that "the trouble" with the latter may be that it *retains* a traditional conception of knowledge, while noticing that science does not live up to that conception. The reconceptualization that is required -- and which gets rationality and evidence back into the picture -- is to focus not on rational evaluation of beliefs, but rather on rational evaluation of *changes* in beliefs.

2 Comments and Replies

H1

Part ~~H~~: ~~Discussions and Responses~~

Essay #6, "Reflections on my Critics," ~~(1970),~~ is the oldest essay in the collection, and the only one that antedates the compilation of *The Essential Tension*. We discussed its inclusion explicitly with Kuhn, ~~and he~~ who was pulled both ways. And, in trying to decide whether to include it, so were we. On the one hand, it violates the third "guideline" mentioned above, and, moreover, it consists primarily of corrections of various misreadings of *Structure* -- M corrections ~~which~~ that, in a perfect world, ought not to be necessary. On the other hand, many of those misunderstandings persist, and so their correction *is* still needed -- M something ~~which~~ this essay achieves with unique clarity, thoroughness, and vigor. In the end, Kuhn left the decision up to us. We have decided to reprint it because of its still relevant special merits, and because the volume in which it originally appeared -- M *Criticism and the Growth of Knowledge* -- M has for some time been out of print.

Essay #7, "Theory Change as Structure Change: Comments on the Sneed Formalism," ~~(1976),~~ is a tentative but mostly very favorable discussion of Joseph Sneed's model-theoretical formalism for the semantics of scientific theories, along with Wolfgang Stegmüller's uses and elaborations of it. Although the essay will be of particular interest to readers already familiar with the Sneed-Stegmüller approach, Kuhn's remarks are non-technical and of more general interest as well. He is especially gratified by the way in which, according to this approach, the central terms of a theory

acquire a significant part of their determinate content from multiple exemplary *applications.* It is important that there be *several* such applications, because they mutually constrain one another (via the theory), thereby avoiding a kind of circularity. It is important that the applications be *exemplary* because this emphasizes the role of learnable skills, which can then be extended to new cases. Kuhn's only expressed reservation about the approach -- albeit a serious one -- is that it leaves no obvious place for the essential phenomenon of theoretical incommensurability.

Essay #8, "Metaphor in Science" ~~(1979),~~ is a ~~1977 symposium discussion paper~~ ~~responding~~ response to a presentation by Richard Boyd about the analogies he sees between scientific terminology and ordinary-language metaphors. Though they agree on several important points, Kuhn demurs from the specific *way* in which Boyd extends the view to include the causal theory of reference, especially with regard to natural-kind terms. In his conclusion, Kuhn describes himself as, like Boyd, an "unregenerate realist", but he thinks this doesn't mean the same thing in their two cases. In particular, he rejects Boyd's own metaphor of scientific theories (coming closer and closer to) "carving nature at the joints". He likens this idea of nature's "joints" to Kant's *Ding an sich*, an aspect of Kantianism he here rejects.

Essay #9, "Rationality and Theory Choice" ~~(1983),~~ is Kuhn's contribution to an

~~American Philosophical Association~~ symposium on the Philosophy of Carl G. Hempel. In it, he responds to a question that Hempel had put to him on several occasions: does he (Kuhn) recognize the difference between *explaining* theory-choice behavior and *justifying* it? Granted that ~~such~~ choices of theories are *in fact* based on their puzzle-solving ability (including accuracy, scope, and so on), this does not get any philosophical bite as a *justification* unless and until those criteria themselves are justified as somehow nonarbitrary. Kuhn replies that they are nonarbitrary ("necessary") in the relevant way because they belong together in an empirically contentful taxonomy of disciplines; reliance on *just such* criteria (plural) is what distinguishes *scientific* investigation from other professional pursuits (fine arts, law, engineering, and so on) -- hence is, in effect, definitive of 'science' as a genuine kind term.

Essay #10, "The Natural and the Human Sciences," ~~(1989) is a contribution to a panel discussion that was to have included Charles Taylor (who, however, had to withdraw at the last minute). In the paper, Kuhn~~ mainly discusses Charles Taylor's influential essay "Interpretation and the Sciences of Man", which ~~he~~ Kuhn much admires. While he is inclined to agree with Taylor that the natural and human sciences are different, he probably doesn't agree about what that difference is. After arguing that the natural sciences, too, have a "hermeneutic base", he acknowledges that, unlike the present human sciences, they are not, in themselves, hermeneutic. But he questions whether this reflects an essential

difference, or rather, simply indicates that most of the human sciences have not yet reached the developmental stage that he used to associate with acquisition of a paradigm.

Essay #11, "Afterwords" ~~(1993),~~ like essay #6, is the final chapter in a volume of essays largely devoted to discussions of Kuhn's own work (*World Changes: Thomas Kuhn and the Nature of Science*, edited by Paul Horwich). Unlike its somewhat feisty predecessor, however, this essay is primarily an appreciative and constructive engagement with essays that are themselves primarily constructive. The main themes are taxonomic structures, incommensurability, the social character of scientific research, and truth cum rationality cum realism. The discussion of these themes is presented here in the form ~~terms~~ of a brief sketch of some of the central ideas of Kuhn's long promised but never finished new book on which ~~Kuhn~~ he continued to work until he no longer could. ok?

3 A Discussion with Thomas S. Kuhn

Part ~~III~~: ~~Autobiographical interview~~

"A Discussion with Thomas S. Kuhn" ~~(1997)~~ is a candid intellectual autobiography in the form of an interview, conducted by Aristides Baltas, Kostas Gavroglu, and Vassiliki Kindi in Athens in the fall of 1995, ~~and published two years later in *Neusis*~~. It is reprinted, lightly edited, in its entirety.

4 The volume ends with a complete bibliography of Kuhn's published work. ok?

(PN) / (PT)

Part 1 Reconceiving Scientific Revolutions

CN 1

CT What Are Scientific Revolutions?

EDN ~~[1] [This should REPLACE Kuhn's footnote in the original]~~ "What Are Scientific Revolutions?" was first published in *The Probabilistic Revolution, Volume I: Ideas in History*, edited by Lorenz Kruger, Lorraine J. Daston, and Michael Heidelberger (Cambridge, MA: ~~The~~ MIT Press, 1987). The three examples that constitute its bulk were developed in this form for the first of three lectures delivered under the title "The Natures of Conceptual Change" at the University of Notre Dame in late November 1980, as part of ~~in~~ the series "Perspectives in the Philosophy of Science". In very nearly its present form, but under the title "From Revolutions to Salient Features", the paper was read to the ~~Third Annual~~ Conference of the Cognitive Science Society in August 1981.

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Notes are at the end of the essay; please set them as footnotes.

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1 What Are Scientific Revolutions?

Thomas S. Kuhn

"What Are Scientific Revolutions?" attempts to refine and clarify the distinction between normal and revolutionary scientific development. After an introductory presentation of the issue, most of the chapter is devoted to the presention of three examples of revolutionary change: the transition from an Aristotelian to a Newtonian understanding of motion, from the contact to the chemical theory of the Voltaic cell, and from Planck's to the now familiar derivation of the law of black-body radiation. A concluding section epitomizes three features common to the examples. All are locally holistic in that they require a number of interrelated changes of theory to be made at once; only at the price of incoherence could these changes have occurred one step at a time. All require changes in the way some set of interdefined scientific terms attached to nature, in the taxonomy provided by scientific language itself. And all also involved changes in something very like metaphor, in the scientist's acquired sense of what objects or events are like each other and of which differ.*

It is now almost twenty years since I first distinguished what I took to be two types of scientific development, normal and revolutionary.[1] Most successful scientific research results in change of the first sort, and its nature is well captured by a standard image: normal science is what produces the bricks that scientific research is forever adding to the growing stockpile of scientific knowledge. That cumulative conception of scientific development is familiar, and it has guided the elaboration of a considerable methodological literature. Both it and its methodological by-products apply to a great deal of significant scientific work. But scientific development also displays a noncumulative mode, and the episodes that exhibit it provide unique clues to a central aspect of scientific knowledge. Returning to a long-standing concern, I shall therefore here attempt to isolate several such clues, first by describing three examples of revolutionary change and then by briefly discussing three characteristics which they all share. Doubtless revolutionary changes share other characteristics as well, but these three provide a sufficient basis for the more theoretical analyses on which I am currently engaged, and on which I shall be drawing somewhat cryptically when concluding this paper.

Before turning to a first extended example, let me try—for those not previously familiar with my vocabulary—to suggest what it is an example of. Revolutionary change is defined in part by its difference from normal change, and normal change is, as already indicated, the sort that results in growth, accretion, cumulative addition to what was known before. Scientific laws, for example, are usually products of this normal process: Boyle's law will illustrate what is involved. Its

* The three examples that constitute the bulk of this chapter were developed in this form for the first of three lectures delivered at the University of Notre Dame during November 1981 in the series Perspectives in Philosophy. In very nearly their present frame, but under the title "From Revolutions to Salient Features," they were read to the Third Annual Conference of the Cognitive Science Society in August 1981.

discoverers had previously possessed the concepts of gas pressure and volume as well as the instruments required to determine their magnitudes. The discovery that, for a given gas sample, the product of pressure and volume was a constant at constant temperature simply added to the knowledge of the way these antecedently understood[2] variables behave. The overwhelming majority of scientific advance is of this normal cumulative sort, but I shall not multiply examples.

Revolutionary changes are different and far more problematic. They involve discoveries that cannot be accommodated within the concepts in use before they were made. In order to make or to assimilate such a discovery one must alter the way one thinks about and describes some range of natural phenomena. The discovery (in cases like these "invention" may be a better word) of Newton's second law of motion is of this sort. The concepts of force and mass deployed in that law differed from those in use before the law was introduced, and the law itself was essential to their definition. A second, fuller, but more simplistic example is provided by the transition from Ptolemaic to Copernican astronomy. Before it occurred, the sun and moon were planets, the earth was not. After it, the earth was a planet, like Mars and Jupiter; the sun was a star; and the moon was a new sort of body, a satellite. Changes of that sort were not simply corrections of individual mistakes embedded in the Ptolemaic system. Like the transition to Newton's laws of motion, they involved not only changes in laws of nature but also changes in the criteria by which some terms in those laws attached to nature. These criteria, furthermore, were in part dependent upon the theory with which they were introduced.

When referential changes of this sort accompany change of law or theory, scientific development cannot be quite cumulative. One cannot get from the old to the new simply by an addition to what was already known. Nor can one quite describe the new in the vocabulary of the old or vice versa. Consider the compound sentence, "In the Ptolemaic system planets revolve about the earth; in the Copernican they revolve about the sun." Strictly construed, that sentence is incoherent. The first occurrence of the term "planet" is Ptolemaic, the second Copernican, and the two attach to nature differently. For no univocal reading of the term "planet" is the compound sentence true.

No example so schematic can more than hint at what is involved in revolutionary change. I therefore turn at once to some fuller examples, beginning with the one that, a generation ago, introduced me to revolutionary change, the transition from Aristotelian to Newtonian physics. Only a small part of it, centering on problems of motion and mechanics, can be considered here, and even about it I shall be schematic. In addition, my account will invert historical order and describe, not what Aristotelian natural philosophers required to reach Newtonian concepts, but what I, raised a Newtonian, required to reach those of Aristotelian natural philosophy. The route I traveled backward with the aid of written texts was, I shall simply assert, nearly enough the same one that earlier scientists had traveled forward with no text but nature to guide them.

I first read some of Aristotle's physical writings in the summer of 1947, at which time I was a graduate student of physics trying to prepare a case study on the development of mechanics for a course in science for nonscientists. Not surprisingly, I approached Aristotle's texts with the Newtonian mechanics I had previously read clearly in mind. The question I hoped to answer was how much mechanics Aristotle had known, how much he had left for people like Galileo and Newton to discover. Given that formulation, I rapidly discovered that Aristotle had known almost no mechanics at all. Everything was left for his successors, most those of the sixteenth and seventeenth centuries. That conclusion was standard, and it might in principle have been right. But I found it bothersome because, as I was reading him, Aristotle appeared not only ignorant of mechanics, but a dreadful bad physical scientist as well. About motion, in particular, his writings seemed to me full of egregious errors, both of logic and of observation.

These conclusions were unlikely. Aristotle, after all, had been the much admired codifier of ancient logic. For almost two millennia after his death, his work played the same role in logic that Euclid's played in geometry. In addition, Aristotle had often proved an extraordinarily acute naturalistic observer. In biology, especially his descriptive writings provided models that were central in the sixteenth and seventeenth centuries to the emergence of the modern biological tradition. How could his characteristic talents have deserted him so systematically when he turned to the study of motion and mechanics? Equally, if his talents had so deserted him why had his writings in physics been taken so seriously for so many centuries after his death? Those questions troubled me. I could easily believe that Aristotle had stumbled, but not that, on entering physics, he had totally collapsed. Might not the fault be mine rather than Aristotle's, I asked myself. Perhaps his words had not always meant to him and his contemporaries quite what they meant to me and mine.

Feeling that way, I continued to puzzle over the text, and my suspicions ultimately proved well-founded. I was sitting at my desk with the text of Aristotle's *Physics* open in front of me and with a four-colored pencil in my hand. Looking up I gazed abstractedly out the window of my room—the visual image is one I still retain. Suddenly the fragments in my head sorted themselves out in a new way, and fell into place together. My jaw dropped, for all at once Aristotle seemed a very good physicist indeed, but of a sort I'd never dreamed possible. Now I could understand why he had said what he'd said, and what his authority had been. Statements that had previously seemed egregious mistakes, now seemed at worst near misses within a powerful and generally successful tradition. That sort of experience—the pieces suddenly sorting themselves out and coming together in a new way—is the first general characteristic of revolutionary change that I shall be singling out after further consideration of examples. Though scientific revolutions leave much piecemeal mopping up to do, the central change cannot be experienced piecemeal, one step at a time. Instead, it involves some relatively sudden and unstructured transformation in which some part of the flux of experience sorts itself out differently and displays patterns that were not visible before.

To make all this more concrete let me now illustrate some of what was involved in my discovery of a way of reading Aristotelian physics, one that made the texts make sense. A first illustration will be familiar to many. When the term "motion" occurs

in Aristotelian physics, it refers to change in general, not just to the change of position of a physical body. Change of position, the exclusive subject of mechanics for Galileo and Newton, is one of a number of subcategories of motion for Aristotle. Others include growth (the transformation of an acorn to an oak), alterations of intensity (the heating of an iron bar), and a number of more general qualitative changes (the transition from sickness to health). As a result, though Aristotle recognizes that the various subcategories are not alike in *all* respects, the basic characteristics relevant to the recognition and analysis of motion must apply to changes of all sorts. In some sense that is not merely metaphorical; all varieties of change are seen as like each other, as constituting a single natural family.[3]

A second aspect of Aristotle's physics—harder to recognize and even more important—is the centrality of qualities to its conceptual structure. By that I do not mean simply that it aims to explain quality and change of quality, for other sorts of physics have done that. Rather I have in mind that Aristotelian physics inverts the ontological hierarchy of matter and quality that has been standard since the middle of the seventeenth century. In Newtonian physics a body is constituted of particles of matter, and its qualities are a consequence of the way those particles are arranged, move, and interact. In Aristotle's physics, on the other hand, matter is very nearly dispensable. It is a neutral substrate, present wherever a body could be—which means wherever there's space or place. A particular body, a substance, exists in whatever place this neutral substrate, a sort of sponge, is sufficiently impregnated with qualities like heat, wetness, color, and so on to give it individual identity. Change occurs by changing qualities, not matter, by removing some qualities from some given matter and replacing them with others. There are even some implicit conservation laws that the qualities must apparently obey.[4]

Aristotle's physics displays other similarly general aspects, some of great importance. But I shall work toward the points that concern me from these two, picking up one other well-known one in passing. What I want now to begin to suggest is that, as one recognizes these and other aspects of Aristotle's viewpoint, they begin to fit together, to lend each other mutual support, and thus to make a sort of sense collectively that they individually lack. In my original experience of breaking into Aristotle's text, the new pieces I have been describing and the sense of their coherent fit actually emerged together.

Begin from the notion of a qualitative physics that has just been sketched. When one analyzes a particular object by specifying the qualities that have been imposed on omnipresent neutral matter, one of the qualities that must be specified is the object's position, or, in Aristotle's terminology, its place. Position is thus, like wetness or hotness, a quality of the object, one that changes as the object moves or is moved. Local motion (motion *tout court* in Newton's sense) is therefore change-of-quality or change-of-state for Aristotle, rather than being itself a state as it is for Newton. But it is precisely seeing motion as change-of-quality that permits its assimilation to all other sorts of change—acorn to oak or sickness to health, for examples. That assimilation is the aspect of Aristotle's physics from which I began, and I could equally well have traveled the route in the other direction. The conception of motion-as-change and the conception of a qualitative physics prove

deeply interdependent, almost equivalent notions, and that is a first example of the fitting or the locking together of parts.

If that much is clear, however, then another aspect of Aristotle's physics—one that regularly seems ridiculous in isolation—begins to make sense as well. Most changes of quality, especially in the organic realm, are asymmetric, at least when left to themselves. An acorn naturally develops into an oak, not vice versa. A sick man often grows healthy by himself, but an external agent is needed, or believed to be needed, to make him sick. One set of qualities, one end point of change, represents a body's natural state, the one that it realizes voluntarily and thereafter rests. The same asymmetry should be characteristic of local motion, change of position, and indeed it is. The quality that a stone or other heavy body strives to realize is position at the center of the universe; the natural position of fire is at the periphery. That is why stones fall toward the center until blocked by an obstacle and why fire flies to the heavens. They are realizing their natural properties just as the acorn does through its growth. Another initially strange part of Aristotelian doctrine begins to fall into place.

One could continue for some time in this manner, locking individual bits of Aristotelian physics into place in the whole. But I shall instead conclude this first example with a last illustration, Aristotle's doctrine about the vacuum or void. It displays with particular clarity the way in which a number of theses that appear arbitrary in isolation lend each other mutual authority and support. Aristotle states that a void is impossible: his underlying position is that the notion itself is incoherent. By now it should be apparent how that might be so. If position is a quality, and if qualities cannot exist separate from matter, then there must be matter wherever there's position, wherever body might be. But that is to say that there must be matter everywhere in space: the void, space without matter, acquires the status of, say, a square circle.[5]

That argument has force, but its premise seems arbitrary. Aristotle need not, one supposes, have conceived position as a quality. Perhaps, but we have already noted that that conception underlies his view of motion as change-of-state, and other aspects of his physics depend on it as well. If there could be a void, then the Aristotelian universe or cosmos could not be finite. It is just because matter and space are coextensive that space can end where matter ends, at the outermost sphere beyond which there is nothing at all, neither space nor matter. That doctrine, too, may seem dispensable. But expanding the stellar sphere to infinity would make problems for astronomy, since that sphere's rotations carry the stars about the earth. Another, more central, difficulty arises earlier. In an infinite universe there is no center—any point is as much the center as any other—and there is thus no natural position at which stones and other heavy bodies realize their natural quality. Or, to put the point in another way, one that Aristotle actually uses, in a void a body could not be aware of the location of its natural place. It is just by being in contact with all positions in the universe through a chain of intervening matter that a body is able to find its way to the place where its natural qualities are fully realized. The presence of matter is what provides space with structure.[6] Thus, both Aristotle's theory of natural local motion and ancient geocentric astronomy are

threatened by an attack on Aristotle's doctrine of the void. There is no way to "correct" Aristotle's views about the void without reconstructing much of the rest of his physics.

Those remarks, though both simplified and incomplete, should sufficiently illustrate the way in which Aristotelian physics cuts up and describes the phenomenal world. Also, and more important, they should indicate how the pieces of that description lock together to form an integral whole, one that had to be broken and reformed on the road to Newtonian mechanics. Rather than extend them further, I shall therefore proceed at once to a second example, returning to the beginning of the nineteenth century for the purpose. The year 1800 is notable, among other things, for Volta's discovery of the electric battery. That discovery was announced in a letter to Sir Joseph Banks, President of the Royal Society.[7] It was intended for publication and was accompanied by the illustration reproduced here as figure 1. For a modern audience there is something odd about it, though the oddity is seldom noticed, even by historians. Looking at any one of the so-called "piles" (of coins) in the lower two-thirds of the diagram, one sees, reading upward from the bottom right, a piece of zinc, *Z*, then a piece of silver, *A*, then a piece of wet blotting paper, then a second piece of zinc, and so on. The cycle zinc, silver, wet blotting paper is repeated an integral number of times, eight in Volta's original illustration. Now suppose that, instead of having all this spelled out, you had been asked simply to look at the diagram, then to put it aside and reproduce it from memory. Almost certainly, those of you who know even the most elementary physics would have drawn zinc (or silver), followed by wet blotting paper, followed by silver (or zinc). In a battery, as we all know, the liquid belongs between the two different metals.

If one recognizes this difficulty and puzzles over it with the aid of Volta's texts, one is likely to realize suddenly that for Volta and his followers, the unit cell consists of the two pieces of metal in contact. The source of power is the metallic interface, the bimetallic junction that Volta had previously found to be the source of an electrical tension, what we would call a voltage. The role of the liquid then is simply to connect one unit cell to the next without generating a contact potential, which would neutralize the initial effect. Pursuing Volta's text still further, one realizes that he is assimilating his new discovery to electrostatics. The bimetallic junction is a condenser or Leyden jar, but one that charges itself. The pile of coins is, then, a linked assemblage or "battery" of charged Leyden jars, and that is where, by specialization from the group to its members, the term "battery" comes from in its application to electricity. For confirmation, look at the top part of Volta's diagram, which illustrates an arrangement he called "the crown of cups." This time the resemblance to diagrams in elementary modern textbooks is striking, but there is again an oddity. Why do the cups at the two ends of the diagram contain only one piece of metal? Why does Volta include two half-cells? The answer is the same as before. For Volta the cups are not cells but simply containers for the liquids that connect cells. The cells themselves are the bimetallic horseshoe strips. The apparently unoccupied positions in the outermost cups are what we would think of as binding posts. In Volta's diagram there are no half-cells.

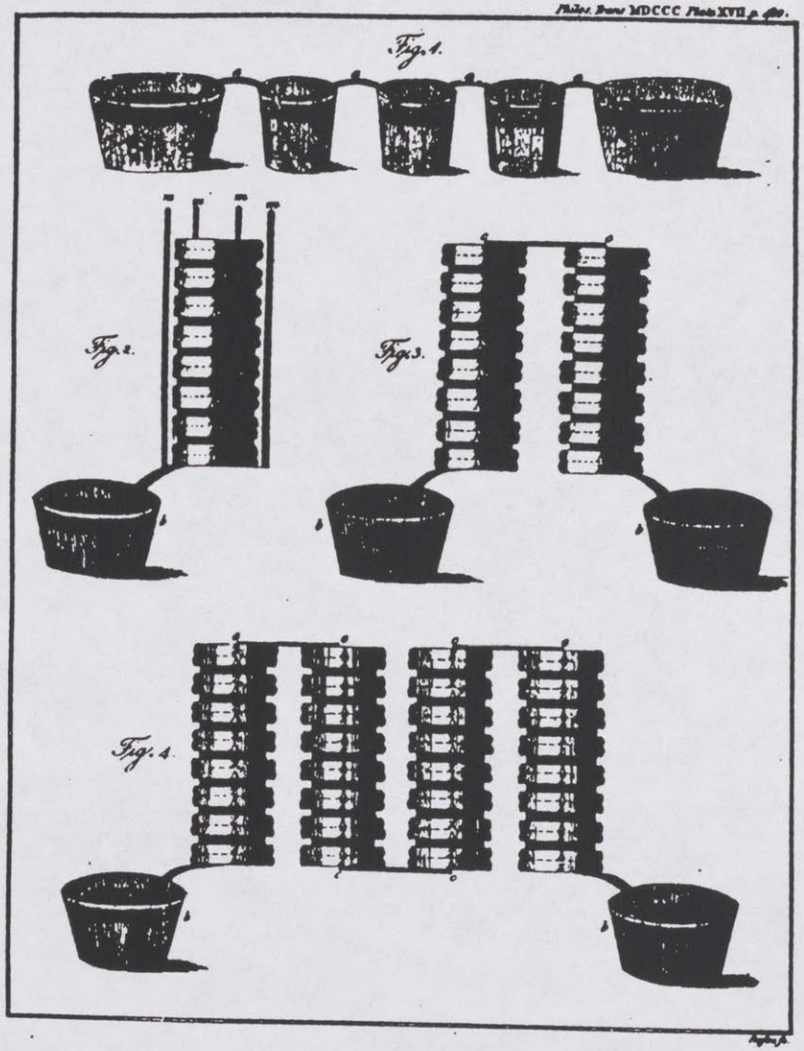

Figure 1

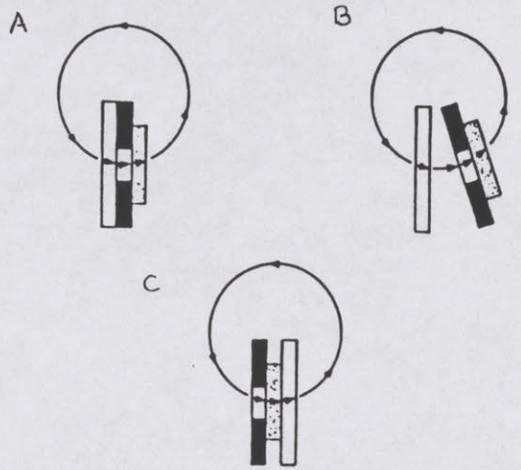

Figure 2

As in the previous example, the consequences of this way of looking at the battery are widespread. For example, as shown in figure 2, the transition from Volta's viewpoint to the modern one reverses the direction of current flow. A modern cell diagram (figure 2~~, bottom~~) can be derived from Volta's (~~top left~~) by a process like turning the latter inside out (~~top right~~). In that process what was previously current flow internal to the cell becomes the external current and vice versa. In the Voltaic diagram the external current flow is from black metal to white, so that the black is positive. In the modern diagram both the direction of flow and the polarity are reversed. Far more important conceptually is the change in the current source effected by the transition. For Volta the metallic interface was the essential element of the cell and necessarily the source of the current the cell produced. When the cell was turned inside out, the liquid and its two interfaces with the metals provided its essentials, and the source of the current became the chemical effects at these interfaces. When both viewpoints were briefly in the field at once, the first was known as the contact theory, the second as the chemical theory of the battery.

Those are only the most obvious consequences of the electrostatic view of the battery, and some of the others were even more immediately important. For example, Volta's viewpoint suppressed the conceptual role of the external circuit. What we would think of as an external circuit is simply a discharge path like the short circuit to ground that discharges a Leyden jar. As a result, early battery diagrams do not show an external circuit unless some special effect, like electrolysis or heating a wire, is occurring there, and then, very often the battery is not shown. Not until the 1840s do modern cell diagrams begin to appear regularly in books on electricity. When they do, either the external circuit or explicit points for its attachment appears with them.[8] Examples are shown in figures 3 and 4.

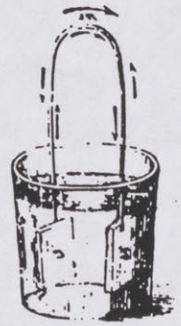

Figure 3

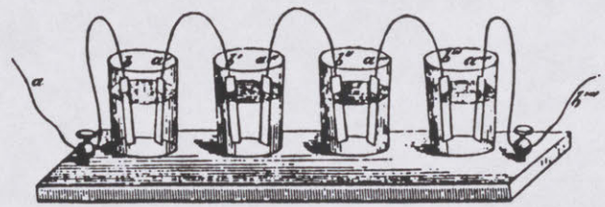

Figure 4

Finally, the electrostatic view of the battery leads to a concept of electrical resistance very different from the one now standard. There is an electrostatic concept of resistance, or there was in this period. For an insulating material of given cross section, resistance was measured by the shortest length the material could have without breaking down or leaking—ceasing to insulate—when subjected to a given voltage. For a conducting material of given cross section, it was measured by the shortest length the material could have without melting when connected across a given voltage. It is possible to measure resistance conceived in this way, but the results are not compatible with Ohm's law. To get those results one must conceive the battery and circuit on a more hydrostatic model. Resistance must become like the frictional resistance to the flow of water in pipes. The assimilation of Ohm's law required a noncumulative change of that sort, and that is part of what made his law so difficult for many people to accept. It has for some time provided a standard example of an important discovery that was initially rejected or ignored.

At this point I end my second example and proceed at once to a third, this one both more modern and more technical then its predecessors. Substantively, it is controversial, involving a new version, not yet everywhere accepted, of the origins of the quantum theory.[9] Its subject is Max Planck's work on the so-called black-body problem, and its structure may usefully be anticipated as follows. Planck first solved the black-body problem in 1900 using a classical method developed by the

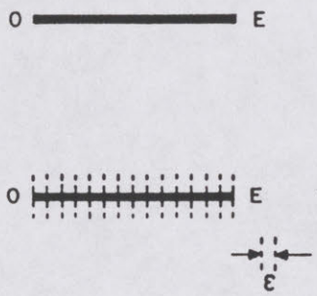

Figure 5

Typesetter: Set fractions as case fractions throughout unless otherwise noted

Austrian physicist Ludwig Boltzmann. Six years later a small but crucial error was found in his derivation, and one of its central elements had to be reconceived. When that was done Planck's solution did work, but it then also broke radically with tradition. Ultimately that break spread through and caused the reconstruction of a good deal of physics.

Begin with Boltzmann, who had considered the behavior of a gas, conceived as a collection of many tiny molecules, moving rapidly about within a container, and colliding frequently both with each other and with the container's walls. From previous works of others, Boltzmann knew the average velocity of the molecules (more precisely, the average of the square of their velocity). But, many of the molecules were, of course, moving much more slowly than the average, others much faster. Boltzmann wanted to know what proportion of them were moving at, say, 1/2 the average velocity, what proportion at 4/3 the average, and so on. Neither that question nor the answer he found to it was new. But Boltzmann reached the answer by a new route, from probability theory, and that route was fundamental for Planck, since whose work it has been standard.

Only one aspect of Boltzmann's method is of present concern. He considered the total kinetic energy E of the molecules. Then, to permit the introduction of probability theory, he mentally subdivided that energy into little cells or elements of size ε, as in figure 5. Next, he imagined distributing the molecules at random among those cells, drawing numbered slips from an urn to specify the assignment of each molecule and then excluding all distributions with total energy different from E. For example, if the first molecule were assigned to the last cell (energy E), then the only acceptable distribution would be the one that assigned all other molecules to the first cell (energy 0). Clearly, that particular distribution is a most improbable one. It is far more likely that most molecules will have appreciable energy, and by probability theory one can discover the most probable distribution of all. Boltzmann showed how to do so, and his result was the same as the one he and others had previously gotten by more problematic means.

That way of solving the problem was invented in 1877, and twenty-three years later, at the end of 1900, Max Planck applied it to an apparently rather different problem, black-body radiation. Physically the problem is to explain the way in which the color of a heated body changes with temperature. Think, for example, of the radiation from an iron bar, which, as the temperature increases, first gives off heat (infrared radiation), then glows dull red, and then gradually becomes a brilliant white. To analyze that situation Planck imagined a container or cavity filled with radiation, that is, with light, heat, radio waves, and so on. In addition, he supposed that the cavity contained a lot of what he called "resonators" (think of them as tiny electrical tuning forks, each sensitive to radiation at one frequency, not at others). These resonators absorb energy from the radiation, and Planck's question was: How does the energy picked up by each resonator depend on its frequency? What is the frequency distribution of the energy over the resonators?

Conceived in that way, Planck's problem was very close to Boltzmann's, and Planck applied Boltzmann's probabilistic techniques to it. Roughly speaking, he used probability theory to find the proportion of resonators that fell in each of the various cells, just as Boltzmann had found the proportion of molecules. His answer fit experimental results better than any other then or since known, but there turned out to be one unexpected difference between his problem and Boltzmann's. For Boltzmann's, the cell size ε could have many different values without changing the result. Though permissible values were bounded, could not be too large or too small, an infinity of satisfactory values was available in between. Planck's problem proved different: other aspects of physics determined ε, the cell size. It could have only a single value given by the famous formula $\varepsilon = h\nu$, where ν is the resonator frequency and h is the universal constant subsequently known by Planck's name. Planck was, of course, puzzled about the reason for the restriction on cell size, though he had a strong hunch about it, one he attempted to develop. But, excepting that residual puzzle, he had solved his problem, and his approach remained very close to Boltzmann's. In particular, the presently crucial point in both solutions the division of the total energy E into cells of size ε was a mental division made for statistical purposes. The molecules and resonators could lie anywhere along the line and were governed by all the standard laws of classical physics.

The rest of this story is very quickly told. The work just described was done at the end of 1900. Six years later, in the middle of 1906, two other physicists argued that Planck's result could not be gained in Planck's way. One small but absolutely crucial alteration of the argument was required. The resonators could not be permitted to lie anywhere on the continuous energy line but only at the divisions between cells. A resonator might, that is, have energy $0, \varepsilon, 2\varepsilon, 3\varepsilon, \ldots$, and so on, but not $(1/3)\varepsilon$, $(4/5)\varepsilon$, etc. When a resonator changed energy it did not do so continuously but by discontinuous jumps of size ε or a multiple of ε.

After those alterations, Planck's argument was both radically different and very much the same. Mathematically it was virtually unchanged, with the result that it has been standard for years to read Planck's 1900 paper as presenting the subsequent modern argument. But physically, the entities to which the derivation refers are very different. In particular, the element ε has gone from a mental division of the total energy to a separable physical energy atom, of which each resonator may have 0, 1, 2, 3, or some other number. Figure 6 tries to capture that change in a way that

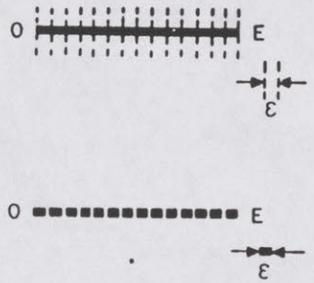

Figure 6

suggests its resemblance to the inside-out battery of my last example. Once again the transformation is subtle, difficult to see. But also once again, the change is consequential. Already the resonator has been transformed from a familiar sort of entity governed by standard classical laws to a strange creature the very existence of which is incompatible with traditional ways of doing physics. As most of you know, changes of the same sort continued for another twenty years as similar nonclassical phenomena were found in other parts of the field.

Those later changes, I shall not attempt to follow, but instead conclude this example, my last, by pointing to one other sort of change that occurred near its start. In discussing the earlier examples, I pointed out that revolutions were accompanied by changes in the way in which terms like "motion" or "cell" attached to nature. In this example there was actually a change in the words themselves, one that highlights those features of the physical situation that the revolution had made prominent. When Planck around 1909 was at last persuaded that discontinuity had come to stay he switched to a vocabulary that has been standard since. Previously he had ordinarily referred to the cell-size ε as the energy "element." Now, in 1909, he began regularly to speak instead of the energy "quantum," for "quantum," as used in German physics, was a separable element, an atomlike entity that could exist by itself. While ε had been merely the size of a mental subdivision, it had not been a quantum but an element. Also in 1909, Planck abandoned the acoustic analogy. The entities he had introduced as "resonators" now became "oscillators," the latter a neutral term that refers to any entity that simply vibrates regularly back and forth. By contrast, "resonator" refers in the first instance to an acoustic entity or, by extension, to a vibrator that responds gradually to stimulation, swelling and diminishing with the applied stimulus. For one who believed that energy changes discontinuously, "resonator" was not an appropriate term, and Planck gave it up in and after 1909.

That vocabulary change concludes my third example. Rather than give others, I shall conclude this discussion by asking what characteristics of revolutionary change are displayed by the examples at hand. Answers will fall under three headings, and I shall be relatively brief about each. The extended discussion they require, I am not quite ready to provide.

A first set of shared characteristics was mentioned near the start of this paper. Revolutionary changes are somehow holistic. They cannot, that is, be made piecemeal, one step at a time, and they thus contrast with normal or cumulative changes like, for example, the discovery of Boyle's law. In normal change, one simply revises or adds a single generalization, all others remaining the same. In revolutionary change one must either live with incoherence or else revise a number of interrelated generalizations together. If these same changes were introduced one at a time, there would be no intermediate resting place. Only the initial and final sets of generalizations provide a coherent account of nature. Even in my last example, the most nearly cumulative of the three, one cannot simply change the description of the energy element ε. One must also change one's notion of what it is to be a resonator, for resonators, in any normal sense of the term, cannot behave as these do. Simultaneously, to permit the new behavior, one must change, or try to, laws of mechanics and of electromagnetic theory. Again, in the second example, one cannot simply change one's mind about the order of elements in a battery cell. The direction of the current, the role of the external circuit, the concept of electrical resistance, and so on, must also be changed. Or still again, in the case of Aristotelian physics, one cannot simply discover that a vacuum is possible or that motion is a state, not a change-of-state. An integrated picture of several aspects of nature has to be changed at the same time.

A second characteristic of these examples is closely related. It is the one I have in the past described as meaning change and which I have here been describing, somewhat more specifically, as change in the way words and phrases attach to nature, change in the way their referents are determined. Even that version is, however, somewhat too general. As recent studies of reference have emphasized, anything one knows about the referents of a term may be of use in attaching that term to nature. A newly discovered property of electricity, of radiation, or of the effects of force on motion may thereafter be called upon (usually with others) to determine the presence of electricity, radiation, or force and thus to pick out the referents of the corresponding term. Such discoveries need not be and usually are not revolutionary. Normal science, too, alters the way in which terms attach to nature. What characterizes revolutions is not, therefore, simply change in the way referents are determined, but change of a still more restricted sort.

How best to characterize that restricted sort of change is among the problems that currently occupy me, and I have no full solution. But roughly speaking, the distinctive character of revolutionary change in language is that it alters not only the criteria by which terms attach to nature but also, massively, the set of objects or situations to which those terms attach. What had been paradigmatic examples of motion for Aristotle—acorn to oak or sickness to health—were not motions at all for Newton. In the transition, a natural family ceased to be natural; its members were redistributed among preexisting sets; and only one of them continued to bear the old name. Or again, what had been the unit cell of Volta's battery was no longer the referent of any term forty years after his invention was made. Though Volta's successors still dealt with metals, liquids, and the flow of charge, the units of their analyses were different and differently interrelated.

What characterizes revolutions is, thus, change in several of the taxonomic categories prerequisite to scientific descriptions and generalizations. That change, furthermore, is an adjustment not only of criteria relevant to categorization, but also of the way in which given objects and situations are distributed among preexisting categories. Since such redistribution always involves more than one category and since those categories are interdefined, this sort of alteration is necessarily holistic. That holism, furthermore, is rooted in the nature of language, for the criteria relevant to categorization are *ipso facto* the criteria that attach the names of those categories to the world. Language is a coinage with two faces, one looking outward to the world, the other inward to the world's reflection in the referential structure of the language.

Look now at the last of the three characteristics shared by my three examples. It has been the most difficult of the three for me to see, but now seems the most obvious and probably the most consequential. Even more than the others, it should repay further exploration. All of my examples have involved a central change of model, metaphor, or analogy—a change in one's sense of what is similar to what, and of what is different. Sometimes, as in the Aristotle example, the similarity is internal to the subject matter. Thus, for Aristotelians, motion was a special case of change, so that the falling stone was *like* the growing oak, or *like* the person recovering from illness. That is the pattern of similarities that constitutes these phenomena a natural family, that places them in the same taxonomic category, and that had to be replaced in the development of Newtonian physics. Elsewhere the similarity is external. Thus, Planck's resonators were *like* Boltzmann's molecules, or Volta's battery cells were *like* Leyden jars, and resistance was *like* electrostatic leakage. In these cases, too, the old pattern of similarities had to be discarded and replaced before or during the process of change.

All these cases display interrelated features familiar to students of metaphor. In each case two objects or situations are juxtaposed and said to be the same or similar. (An even slightly more extended discussion would have also to consider examples of dissimilarity, for they, too, are often important in establishing a taxonomy.) Furthermore, whatever their origin—a separate issue with which I am not presently concerned—the primary function of all these juxtapositions is to transmit and maintain a taxonomy. The juxtaposed items are exhibited to a previously uninitiated audience by someone who can already recognize their similarity, and who urges that audience to learn to do the same. If the exhibit succeeds, the new initiates emerge with an acquired list of features salient to the required similarity relation—with a feature-space, that is, within which the previously juxtaposed items are durably clustered together as examples of the same thing and are simultaneously separated from objects or situations with which they might otherwise have been confused. Thus, the education of an Aristotelian associates the flight of an arrow with a falling stone and both with the growth of an oak and the return to health. All are thereafter changes of state; their end points and the elapsed time of transition are their salient features. Seen in that way, motion cannot be relative and must be in a category distinct from rest, which is a state. Similarly, on that view, an infinite motion, because it lacks an end point, becomes a contradiction in terms.

The metaphorlike juxtapositions that change at times of scientific revolution are thus central to the process by which scientific and other language is acquired. Only after that acquisition or learning process has passed a certain point can the practice of science even begin. Scientific practice always involves the production and the explanation of generalizations about nature; those activities presuppose a language with some minimal richness; and the acquisition of such a language brings knowledge of nature with it. When the exhibit of examples is part of the process of learning terms like "motion," "cell," or "energy element," what is acquired is knowledge of language and of the world together. On the one hand, the student learns what these terms mean, what features are relevant to attaching them to nature, what things cannot be said of them on pain of self-contradiction, and so on. On the other hand, the student learns what categories of things populate the world, what their salient features are, and something about the behavior that is and is not permitted to them. In much of language learning these two sorts of knowledge—knowledge of words and knowledge of nature—are acquired together, not really two sorts of knowledge at all, but two faces of the single coinage that a language provides.

The reappearance of the double-faced character of scientific language provides an appropriate terminus for this paper. If I am right, the central characteristic of scientific revolutions is that they alter the knowledge of nature that is intrinsic to the language itself and that is thus prior to anything quite describable as description or generalization, scientific or everyday. To make the void or an infinite linear motion part of science required observation reports that could only be formulated by altering the language with which nature was described. Until those changes had occurred, language itself resisted the invention and introduction of the sought after new theories. The same resistance by language is, I take it, the reason for Planck's switch from "element" and "resonator" to "quantum" and "oscillator." Violation or distortion of a previously unproblematic scientific language is the touchstone for revolutionary change.

Notes

1. Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 2nd ed., rev. (Chicago: University of Chicago Press, 1969). The book was first published in 1962.

2. The phrase "antecedently understood" was introduced by C. G. Hempel, who shows that it will serve many of the same purposes as "observational" in discussions involving the distinction between observational and theoretical terms (cf., particularly, his *Aspects of Scientific Explanation* [New York: Free Press, 1965], pp. 208ff.). I borrow the phrase because the notion of an antecedently understood term is intrinsically developmental or historical, and its use within logical empiricism points to important areas of overlap between that traditional approach to philosophy of science and the more recent historical approach. In particular, the often elegant apparatus developed by logical empiricists for discussions of concept formation and of the definition of theoretical terms can be transferred as a whole to the historical approach and used to analyze the formation of new concepts and the definition of new terms, both of which usually take place in intimate association with the introduction of a new theory. A more systematic way of preserving an important part of the observation/theoretical distinction by embedding it in a developmental approach has been developed by Joseph D. Sneed (*The Logical Structure of Mathematical Physics* [Dordrecht: Reidel, 1971], pp. 1–64,

249–307). Wolfgang Stegmüller has clarified and extended Sneed's approach by positing a hierarchy of theoretical terms, each level introduced within a particular historical theory (*The Structure and Dynamics of Theories* [New York: Springer, 1976], pp. 40–67, 196–231). The resulting picture of linguistic strata shows intriguing parallels to the one discussed by Michel Foucault in *The Archeology of Knowledge*, trans. A. M. Sheridan Smith (New York: Pantheon, 1972).

3. For all of this see Aristotle's *Physics*, Book V, Chapters 1–2 (224a21–226b16). Note that Aristotle does have a concept of change that is broader than that of motion. Motion is change of substance, change from something to something (225a1). But change also includes coming to be and passing away, i.e., change from nothing to something and from something to nothing (225a34–225b9), and these are not motions.

4. Compare Aristotle's *Physics*, Book I, and especially his *On Generation and Corruption*, Book II, Chapters 1–4.

5. There is an ingredient missing from my sketch of this argument: Aristotle's doctrine of place, developed in the *Physics*, Book IV, just before his discussion of the vacuum. Place, for Aristotle, is always the place of body or, more precisely, the interior surface of the containing or surrounding body (212a2–7). Turning to his next topic, Aristotle says, "Since the void (if there is any) must be conceived as place in which there might be body but is not, it is clear that, so conceived, the void cannot exist at all, either as inseparable or separable" (214a16–20). (I quote from the Loeb Classical Library translation by Philip H. Wickstead and Francis M. Cornford, a version that, on this difficult aspect of the *Physics*, seems to me clearer than most, both in text and commentary.) That it is not merely a mistake to substitute "position" for "place" in a sketch of the argument is indicated by the last part of the next paragraph of my text.

6. For this and closely related arguments see Aristotle, *Physics*, Book IV, Chapter 8 (especially 214b27–215a24).

7. Alessandro Volta, "On the Electricity Excited by the mere Contact of Conducting Substances of Different Kinds," *Philosophical Transactions*, 90 (1800), 403–431. On this subject see T. M. Brown, "The Electric Current in Early Nineteenth-Century French Physics," *Historical Studies in the Physical Sciences*, 1 (1969), 61–103.

8. The illustrations are from A. de la Rive, *Traité d'électricité théorique et appliquée*, Vol. 2 (Paris: J. B. Bailière, 1856), pp. 600, 656. Structurally similar but schematic diagrams appear in Faraday's experimental researches from the early 1830s. My choice of the 1840s as the period when such diagrams became standard results from a casual survey of electricity texts lying ready to hand. A more systematic study would, in any case, have had to distinguish between British, French, and German responses to the chemical theory of the battery.

9. For the full version with supporting evidence see my *Black-Body Theory and the Quantum Discontinuity, 1894–1912* (Oxford and New York: Clarendon and Oxford University Presses, 1978).

xtent to which they cut themselves off from their past, substituting for it a ystematic reconstruction. Few scientists read past scientific works; science braries ordinarily displace the books and journals in which such work is ecorded; scientific life knows no institutional equivalent for the art museum. nother symptom is presently more central. When reconceptualization occurs a scientific field, displaced concepts rapidly vanish from professional view. ater practitioners reconstruct their predecessors' work in the conceptual vobulary they use themselves, a vocabulary incapable of representing what those redecessors actually did. Such reconstruction is a precondition for the cumutive image of scientific development familiar from science textbooks, but it idly misrepresents the past.[34] No wonder that the historian, breaking through that past, experiences the breakthrough as a gestalt switch. And, since what e historian breaks through to is not simply the concepts deployed by a single ientist but those of a once active community, it is natural to speak of the mmunity itself as having undergone a gestalt switch when it displaced its evious conceptual vocabulary for a new one. The temptation to use 'gestalt vitch' and related phrases in that way is particularly strong both because the erval in which the conceptual vocabulary shifted is usually short and because, ring that interval, a number of individual scientists did experience gestalt vitches.

The transfer of terms like 'gestalt switch' from individuals to groups is, wever, clearly metaphorical, and in this case the metaphor proves damaging. sofar as the historian's gestalt switch provides the model, the magnitude of the nceptual transpositions characteristic of scientific development is exaggerated. storians, working backward, regularly experience as a single conceptual shift ransposition for which the developmental process required a series of stages. ore important, treating groups or communities as though they were individls-writ-large misrepresents the process of conceptual change. Communities not have experiences, much less gestalt switches. As the conceptual vocabuy of a community changes, its members may undergo gestalt switches, but ly some of them do and not all at the same time. Of those who do not, some se to be members of the community; others acquire the new vocabulary in dramatic ways. Meanwhile, communication goes on, however imperfectly, taphor serving as a partial bridge across the divide between an old literal usage l a new one. To speak, as I repeatedly have, of a community's undergoing a talt switch is to compress an extended process of change into an instant, ving no room for the microprocesses by which the change is achieved.

Recognition of these difficulties opens two directions for further development. The first is the one for which Professor Miller calls and which his

[34] On this subject see "The Invisibility of Revolutions," in my *Structure of Scientific Revolutions* (2nd ed.; Chicago, 1970), pp. 136–143; "Comment on the Relations of Science and Art," in my *Essential Tension* (Chicago, 1977), pp. 340–351; and my "Revisiting Planck," *Historical Studies in the Physical Sciences*, 14 (1984), 231–252, esp. Part 4.

comments illustrate: study of the microprocesses which occur within a community during periods of conceptual change. Excepting in its repeated references to metaphor, my paper has nothing to say about them, but its formulation, unlike that of my older work, is designed to leave room for their exploration.[35] The second, which may prove even more important, is a systematic attempt to separate the concepts appropriate to the description of groups from those appropriate to the description of individuals. That attempt is currently among my central concerns, and one of its products plays a central, though mostly implicit, role in my paper.[36] People may, I there insist, "use the same lexicon, refer to the same items with it, and yet pick out those items in different ways. Reference is a function of the shared structure of the lexicon but not of the varied feature spaces within which individuals represent that structure" (n. 25). A number of the classical problems of meaning, I am suggesting, may be seen as a product of the failure to distinguish between the lexicon as a shared property constitutive of community, on the one hand, and the lexicon as something carried by each individual member of the community, on the other.

Notes 34-36: retyped, see 3-28A

[35] The contrast is, of course, only with my older metahistorical work. As historian I have often dealt with the detail of the transition process. See, especially, *Black-Body Theory and the Quantum Discontinuity, 1894–1912* (Oxford and New York, 1978), paperback edition (Chicago, 1987).

[36] Others are indicated in my "Scientific Knowledge as Historical Product," to appear in *Synthèse*.

(FN1) 1. For Newton, see my "Newton's '31st Query' and the Degradation of Gold, *Isis* 42 (1951): 296-98. For Bohr, see J. L. Heilbron and T. S. Kuhn, "The Genesis of the Bohr Atom," *Historical Studies in the Physical Sciences* 1 (1969): 211-90, where the nonsense passages that gave rise to the project are quoted on p. 271. For an introduction to the other examples mentioned, see my "What Are Scientific Revolutions?" Occasional Paper 18, Center for Cognitive Science (Cambridge, MA: Massachusetts Institute of Technology, 1981); reprinted in *The Probabilistic Revolution,* vol. 1, *Ideas in History,* ed. L. Krüger, L. J. Daston, and M. Heidelberger (Cambridge, MA: MIT Press, 1987), pp. 7-22; also reprinted in this volume as essay 1.

(FN2) 2. Throughout this paper I shall continue to speak of the lexicon, of terms, and of statements. My concern, however, is actually with conceptual or intensional categories more generally, e.g., with those which may be reasonably be attributed to animals or to the perceptual system. For the support this extension receives from possible-world semantics see B. Partee, **"<!Eds.: Please supply article title, here and for notes 8, 9, 10, and 21!>"**, in *Possible Worlds in Humanities, Arts and Sciences: Proceedings of Nobel Symposium 65,* ed. Sture Allén, Research in Text Theory, vol. 14 (Berlin: Walter de Gruyter, 1989), pp. **<!Eds.: Please supply page numbers!>.**

(FN3) 3. For a fuller and more nuanced discussion of this point and those that follow, see my "Commensurability, Comparability, Communicability," in *PSA 1982: Proceedings of the 1982 Biennial Meeting of the Philosophy of Science Association,* vol. 2, ed. P. D. Asquith and T. Nickles (East Lansing, MI: Philosophy of Science Association, 1983), pp. 669-88; reprinted in this volume as essay 2.

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THOMAS S. KUHN

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Possible Worlds in History of Science*

The invitation to open this symposium's discussion of possible worlds in history of science has been particularly welcome, for several issues raised by the topic are central to my current research. Their centrality is, however, also a source of problems. In the book on which I am at work, these issues arise only after much prior discussion has led to conclusions I must here present as premises. Limited illustration and evidence for those premises will follow, but only in the later portions of this paper, where they are put to work.

What I am presupposing will be suggested by the following claim: to understand some body of past scientific belief, the historian must acquire a lexicon that here and there differs systematically from the one current in his own day. Only by using that older lexicon can he or she accurately render certain of the statements that are basic to the science under scrutiny. Those statements are not accessible by means of a translation that uses the current lexicon, not even if the list of words it contains is expanded by the addition of selected terms from its predecessor.

That claim is elaborated in the first of the three sections of this paper, and its relevance to an ongoing debate in possible-world semantics is briefly suggested in the second. The third section, an extended analysis of some interrelated terms from Newtonian mechanics, illustrates the entanglements of the lexicon with the substantive claims of a scientific theory, entanglements which may make it impossible to change the theory without changing the lexicon as well. Finally, the closing section of the paper examines the way such entanglements restrict the applicability to scientific development of the conception of possible worlds.

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A historian reading an out-of-date scientific text characteristically encounters passages that make no sense. That is an experience I have had repeatedly whether my subject was an Aristotle, a Newton, a Volta, a Bohr, or a Planck.[1] It

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* This paper has been considerably revised since the Symposium. For much relevant criticism and advice during that process, I am grateful to Barbara Partee and to my M.I.T. colleagues Ned Block, Sylvain Bromberger, Dick Cartwright, Jim Higginbotham, Judy Thomson, and Paul Horwich.

[1] For Newton, see my "Newton's '31st Query' and the Degradation of Gold," *Isis*, 42 (1951), 296–298. For Bohr, see ~~John~~ L. Heilbron and ~~Thomas~~ S. Kuhn, "The Genesis of the Bohr Atom,"

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has been standard to ignore such passages or to dismiss them as the products of error, ignorance, or superstition, and that response is occasionally appropriate. More often, however, sympathetic contemplation of the troublesome passages suggests a different diagnosis. The apparent textual anomalies are artifacts, products of misreading.

For lack of an alternative, the historian has been understanding words and phrases in the text as he or she would if they had occurred in contemporary discourse. Through much of the text that way of reading proceeds without difficulty; most terms in the historian's vocabulary are still used as they were by the author of the text. But some sets of interrelated terms are not, and it is failure to isolate those terms and to discover how they were used that has permitted the passages in question to seem anomalous. Apparent anomaly is thus ordinarily evidence of the need for local adjustment of the lexicon, and it often provides clues to the nature of that adjustment as well.[2] An important clue to problems in reading Aristotle's physics is provided by the discovery that the term translated 'motion' in his text refers not simply to change of position but to all changes characterized by two end points. Similar difficulties in reading Planck's early papers begin to dissolve with the discovery that, for Planck before 1907, 'the energy element *hν*' referred, not to a physically indivisible atom of energy (later to be called 'the energy quantum') but to a mental subdivision of the energy continuum, any point on which could be physically occupied.

These examples all turn out to involve more than mere changes in the use of terms, thus illustrating what I had in mind years ago when speaking of the "incommensurability" of successive scientific theories.[3] In its original mathematical use 'incommensurability' meant "no common measure," for example of the hypotenuse and side of an isosceles right triangle. Applied to a pair of theories in the same historical line, the term meant that there was no common language into which both could be fully translated.[4] Some statements constitutive of the older theory could not be stated in any language adequate to express its successor and vice versa.

Incommensurability thus equals untranslatability, but what incommensurability bars is not quite the activity of professional translators. Rather, it is a quasi-mechanical activity governed in full by a manual which specifies, as a function of context, which string in one language may, *salva veritate*, be substituted for a given string in the other. Translation of that sort is Quinean, and the point at which I aim will be suggested by the remark that most or all of Quine's arguments for the indeterminacy of translation can, with equal force, be directed to an opposite conclusion: instead of there being an infinite number of translations compatible with all normal dispositions to speech behavior, there are often none at all.

With that much, Quine might very nearly agree. His arguments require that a choice be made, but they do not dictate its outcome. In his view, one must either entirely abandon traditional notions of meaning, of intension, or else one must give up the assumption that language is, or could be, universal, that anything expressible in one language, or by using one lexicon, can be expressed also in any other. His own conclusion – that meaning must be abandoned – follows only because he takes universality for granted, and this paper will suggest that there is no sufficient basis for doing so. To possess a lexicon, a structured vocabulary, is to have access to the varied set of worlds which that lexicon can be used to describe. Different lexicons – those of different cultures or different historical periods, for example – give access to different sets of possible worlds, largely but never entirely overlapping. Though a lexicon may be enriched to yield access to worlds previously accessible only with another, the result is peculiar, a point to be elaborated below. In order that the "enriched" lexicon continue to serve some essential functions, the terms added during enrichment must be rigidly segregated and reserved for a special purpose.

What has made the assumption of universal translatability so nearly inescapable is, I believe, its deceptive similarity to a quite different one, in this case an assumption that I share: anything which can be said in one language can, with imagination and effort, be *understood* by a speaker of another. What is prerequisite to such understanding, however, is not translation but language learning. Quine's radical translator is, in fact, a language learner. If he succeeds, which I think no principle bars, he will become bilingual. But that does not ensure that he or anyone else will be able to translate from his newly acquired language to the one with which he was raised. Though learnability could in principle imply translatability, the thesis that it does so needs to be argued. Much philosophical discussion instead takes it for granted. Quine's *Word and Object* provides a notably explicit case in point.[5]

I am suggesting, in short, that the problems of translating a scientific text, whether into a foreign tongue or into a later version of the language in which it

Historical Studies in the Physical Sciences, 1 (1969), pp. 211–290, where the nonsense passages that gave rise to the project are quoted on p. 271. For an introduction to the other examples mentioned see my "What are Scientific Revolutions?" in L. J. Daston, M. Heidelberger, and L. Krüger (eds.), *The Probabilistic Revolution, vol. 1: Ideas in History* (Cambridge, Mass., 1987), pp. 7–22.

[2] Throughout this paper I shall continue to speak of the lexicon, of terms, and of statements. My concern, however, is actually with conceptual or intensional categories more generally, e.g. with those which may reasonably be attributed to animals or to the perceptual system. For the support this extension receives from possible-world semantics see the paper by Barbara Partee in this volume.

[3] For a fuller and more nuanced discussion of this point and those that follow see my "Commensurability, Comparability, Communicability," in P. D. Asquith and T. Nickles (eds.), *PSA 1982*, vol. II (East Lansing, 1983), pp. 669–688.

[4] My original discussion described non-linguistic as well as linguistic forms of incommensurability. That I now take to have been an overextension resulting from my failure to recognize how large a part of the apparently non-linguistic component was acquired with language during the learning process. The acquisition during language learning of what I once took to be incommensurability with respect to instrumentation is, for example, illustrated by the discussion of the spring balance in the next section of this paper.

[5] W. V. O. Quine, *Word and Object* (New York, 1960), pp. 47, 70f.

was written, are far more like those of translating literature than has generally been supposed. In both cases the translator repeatedly encounters sentences that can be rendered in several alternative ways, none of which captures them completely. Difficult decisions must then be made about which aspects of the original it is most important to preserve. Different translators may differ, and the same translator may make different choices in different places even though the terms involved are in neither language ambiguous. Such choices are governed by standards of responsibility, but they are not determined by them. In these matters there is no such thing as being merely right or wrong. The preservation of truth values when translating scientific prose is very nearly as delicate a task as the preservation of resonance and emotional tone in the translation of literature. Neither can be fully achieved; even responsible approximation requires the greatest tact and taste. In the scientific case, these generalizations apply, not only to passages that make explicit use of theory, but also and more significantly to those their authors took to be merely descriptive.

Unlike many people who share my generally structuralist leanings, I am not attempting to erase or even to reduce the gap generally thought to separate literal from figurative use of language. On the contrary, I cannot imagine a theory of figurative use a theory, for example, of metaphor and other tropes that did not presuppose a theory of literal meanings. Nor, to turn from theory to practice, can I imagine how words could be employed effectively in tropes like metaphor except within a community whose members had previously assimilated their literal use.[6] My point is simply that the literal and the figurative use of terms are alike in their dependence on preestablished associations between words.

That remark provides entree to a theory of meaning, but only two aspects of that theory are centrally relevant to the arguments which follow, and I must here restrict myself to them. First, knowing what a word means is knowing how to use it for communication with other members of the language community within which it is current. But that ability does not imply that one knows something that attaches to the word by itself, its meaning, say, or its semantic markers. Words do not, with occasional exceptions, have meanings individually but only through their associations with other words within a semantic field. If the use of an individual term changes, then the use of the terms associated with it normally changes as well.

The second aspect of my developing view of meaning is both less standard and more consequential. Two people may use a set of interrelated terms in the same way but employ different sets (in principle totally disjunct sets) of field coordinates in doing so. Examples will be found in the next section of this paper; meanwhile the following metaphor may prove suggestive. The United States

[6] See my "Metaphor in Science," in Andrew Ortony (ed.), *Metaphor and Thought* (Cambridge University Press, 1979), pp. 409–419.

can be mapped in many different coordinate systems. Individuals with different maps will specifiy the location of, say, Chicago by means of a different pair of coordinates. But all will nevertheless locate the same city provided that the maps are scaled to preserve the relative distances between the items mapped. The metric that accompanies each of the various sets of coordinates must, that is, be chosen to preserve the structural geometrical relations within the mapped area.[7]

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The premises just sketched have implications for a continuing debate within possible-world semantics, a subject I shall briefly epitomize before relating it to what has already been said. A possible world is often spoken of as a way our world might have been, and that informal description will very nearly serve present purposes.[8] Thus, in our world the earth has only a single natural satellite (the moon), but there are other possible worlds, almost the same as ours except that the earth has two or more satellites or has none at all. (The "almost" allows the adjustment of phenomena like the tides which, the laws of nature remaining the same, would vary with number of satellites.) There are also possible worlds less like ours: some in which there is no earth, others in which there are no planets, and still others in which not even the laws of nature are the same.

What has recently excited a number of philosophers and linguists about the concept of possible worlds is that it offers a route both to a logic of modal statements and to an intensional semantics for logic and for natural languages. Necessarily true statements, for example, are true in all possible worlds; possibly true statements are true in some; and a true counterfactual is a statement true in some worlds but not in that of the person who made it. Given a set of possible worlds over which to quantify, a formal logic of modal statements appears within reach. Quantification over possible worlds can also lead to an intensional semantics, though by a more complex route. Since the meaning or intension of a statement is what picks out the possible worlds in which that statement is true, each statement corresponds to and may be conceived as a function from possible worlds to truth values. Similarly, a property may be conceived as a function from possible worlds to the sets whose members display that property in each world. Other sorts of referring terms may be conceptually reconstructed in related ways.

Even so brief a sketch of possible-world semantics suggests the likely significance of the range of possible worlds over which quantification occurs, and on this issue opinions vary. David Lewis, for example, would quantify over the entire range of worlds that have been or might be conceived; Saul Kripke, at the

[7] Some preliminary indications of what these cryptic remarks intend are supplied in my "Commensurability, Comparability, Communicability," *op. cit.*

[8] Barbara Partee's essay in this volume provides an elegant summary of the objectives and techniques of possible-world semantics as seen by both linguists and philosophers. Readers unfamiliar with the topic are advised to read it first.

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6. See my "Metaphor in Science," in *Metaphor and Thought*, ed. Andrew Ortony (Cambridge: Cambridge University Press, 1979), pp. 409-19; reprinted in this volume as essay 8.

other extreme, restricts attention to possible worlds that can be stipulated; intermediate positions are available and some have been filled.[9] Partisans of these positions debate a variety of issues, most of which have no present relevance. But the debate's participants all appear to assume, with Quine, that anything can be said in any language. If, as I have premised, that assumption fails, additional considerations become relevant.

Questions about the semantics of modal statements, or about the intension of words and strings constructed from them, are *(ipso facto)* questions about statements and words in a specified language. Only the possible worlds stipulatable in that language can be relevant to them. Extending quantification to include worlds accessible only by resort to other languages seems at best functionless, and in some applications it may be a source of error and confusion. One relevant sort of confusion has already been mentioned – that of the historian who tries to present an older science in his or her own language – and the next two sections will explore some others. At least in their application to historical development, the power and utility of possible-world arguments appears to require their restriction to the worlds accessible with a given lexicon, the worlds that can be stipulated by participants in a given language-community or culture.[10]

* * *

I have so far dealt in general assertions, omitting both illustration and defense. Let me now begin to supply them, acknowledging again that I shall not complete the task in this place. My argument will proceed in two stages. This section examines part of the lexicon of Newtonian mechanics, especially the interrelated terms 'force', 'mass', and 'weight'. It asks, first, what one need and need not know to be a member of the community that uses these terms, and, then, how possession of that knowledge constrains the worlds which members

[9] Partee provides a fuller account of these divisions as well as a useful bibliography. A more analytic account is included in Robert C. Stalnaker, *Inquiry* (Cambridge, Mass., 1984). The debate focusses on the ontological status of possible worlds, i.e., on their reality: differences about the range of quantification appropriate for possible-world theories follow directly.

[10] Partee emphasizes that possible worlds are not conceivable worlds, points out "that we can conceive of there being possibilities we can't conceive of", and suggests that restricting possible worlds to conceivable worlds may make it impossible to deal with such cases. Talk with her since the symposium has made me realize the need for still another distinction. Not all the worlds accessible or stipulatable with a given lexicon are conceivable: a world containing square circles can be stipulated but not conceived; other examples will recur below. It is only worlds access to which requires restructuring of the lexicon which I mean to exclude when quantifying over possible worlds. Note also that to speak of different lexicons as giving access to different sets of possible worlds is not simply to add one more to the standard kind of accessibility relations discussed at the start of Partee's paper. There is no type of necessity corresponding to lexical accessibility. Excepting statements that stipulate an inconceivable world, no statement framable in a given lexicon is necessarily true or false simply because it can be accessed in that lexicon. More generally, the issue of lexical accessibility seems to arise for all applications of possible-world arguments, thus cutting across the standard set of accessiblity relations.

of that community can describe without doing violence to the language. Some of the worlds they cannot describe are, of course, described at a later time, but only after a change of lexicon which bars coherent description of some worlds describable before. That sort of change is the subject of this paper's last section. It focusses upon the so-called causal theory of reference, an application of possible-world concepts said to eliminate the significance of such changes.

The vocabulary in which the phenomena of a field like mechanics are described and explained is an historical product, developed over time, and repeatedly transmitted, in its then current state, from one generation to its successor. In the case of Newtonian mechanics, the required cluster of terms has been stable for some time, and transmission techniques are relatively standard. Examining them will suggest characteristics of what the student acquires in the course of becoming a licensed practitioner of the field.[11]

Before the exposure to Newtonian terminology can usefully begin, other significant portions of the lexicon must be in place. Students must, for example, already have a vocabulary adequate to refer to physical objects and to their locations in space and time. Onto this they must have grafted a mathematical vocabulary rich enough to permit the quantitative description of trajectories and the analyses of velocities and accelerations of bodies moving along them.[12] Also, at least implicitly, they must command a notion of extensive magnitude, a quantity whose value for the whole of a body is the sum of its values for the body's parts. Quantity of matter provides a standard example. These terms can all be acquired without resort to Newtonian theory, and the student must control them before that theory can be learned. The other lexical items required by that theory – most notably 'force', 'mass', and 'weight' in their Newtonian senses – can only be acquired together with the theory itself.

Five aspects of the way in which these Newtonian terms are learned require particular illustration and emphasis. First, as already indicated, learning cannot begin until a considerable antecedent vocabulary is in place. Second, in the process through which the new terms are acquired, definition plays a negligible role. Rather than being defined, these terms are introduced by exposure to examples of their use, examples provided by someone who already belongs to the speech community in which they are current. That exposure often includes actual exhibits, for example in the student laboratory, of one or more exemplary

[11] I discuss the acquisition of a lexicon because it is a source of clues to what the individual's possession of a lexicon entails. Nothing about the end product depends, however, upon the lexicon's being acquired by generation-to-generation transmission. The consequences would be the same if, for example, the lexicon were a genetic endowment or had been implanted by a skilled neurosurgeon. I shall, for example, shortly emphasize that transmitting a lexicon requires repeated recourse to concrete examples. Implanting the same lexicon surgically would, I am suggesting, have involved implanting the memory traces left by exposure to such examples.

[12] In practice, the techniques for describing velocities and accelerations along trajectories are usually learned in the same courses that introduce the terms to which I turn next. But the first set can be acquired without the second, whereas the second cannot be acquired without the first.

situations to which the terms in question are applied by someone who already knows how to use them. The exhibits need not be actual, however. The exemplary situations may instead be introduced by a description conducted primarily in terms drawn from the antecedently available vocabulary but in which the terms to be learned also appear here and there. The two processes are for the most part interchangeable, and most students encounter them both, in some mix or other. Both include an indispensable ostensive or stipulative element: terms are taught through the exhibit, direct or by description, of situations to which they apply.[13] The learning that results from such a process is not, however, about words alone but equally about the world in which they function. When I use the phrase 'stipulative descriptions' in what follows, the stipulations I have in mind will be simultaneously and inseparably about both the substance and the vocabulary of science, about both the world and the language.

A third significant aspect of the learning process is that exposure to a single exemplary situation seldom or never supplies enough information to permit the student to use a new term. Several examples of varied sorts are required, often accompanied by examples of apparently similar situations to which the term in question does not apply. The terms to be learned, furthermore, are seldom applied to these situations in isolation but are instead embedded in whole sentences or statements, among which are some usually referred to as laws of nature.

Fourth, among the statements involved in learning one previously unknown term are some that include other new terms as well, terms that must be acquired together with the first. The learning process thus interrelates a set of new terms, giving structure to the lexicon that contains them. Finally, though there is usually considerable overlap between the situations to which individual language learners are exposed (and even more between the accompanying statements), individuals can in principle communicate fully even though they acquired the terms with which they do so along very different routes. To the extent that the process I am describing supplies individuals with anything resembling a definition, it is not a definition that need be shared by other members of the speech community.

For illustrations, consider first the term 'force'. The situations which exemplify a force's presence are of varied sorts. They include, for example, muscular

[13] The terms 'ostension' and 'ostensive' seem to have two different uses, which for present purposes need to be distinguished. In one, these terms imply that *nothing but* the exhibit of a word's referent is needed to learn or to define it. In the other, they imply only that *some* exhibit is required during the acquisition process. I shall, of course, be using the second sense of the terms. The propriety of extending them to cases in which description in an antecedent vocabulary replaces an actual exhibit depends on recognizing that description does not supply a string of words equivalent to the statements containing the words to be learned. Rather it enables students to visualize the situation and apply to the visualization the same mental processes (whatever they may be) that would otherwise have been applied to the situation as perceived.

exertion, a stretched string or spring, a body possessed of weight (note the occurrence of another of the terms to be learned), or, finally, certain sorts of motion. The last is particularly important and presents particular difficulties to the student. As Newtonians use 'force', not all motions signify the presence of its referent, and examples which display the distinction between forced and force-free motions are therefore required. Their assimilation, furthermore, demands the suppression of a highly developed pre-Newtonian intuition. For children and Aristotelians the standard example of a forced motion is the hurled projectile. Force-free motion is for them exemplified by the falling stone, the spinning top, or the rotating flywheel. For the Newtonian all of these are cases of forced motion. The only example of a Newtonian force-free motion is motion in a straight line at constant speed, and that can be exhibited directly only in interplanetary space. Teachers nevertheless try. (I still remember the contrived lecture demonstration $\frac{f}{m}$ a block of ice sliding on a sheet of glass $\frac{1}{m}$ that helped me undo prior intuitions and acquire the Newtonian concept of 'force'.) But for most students the main path to this key aspect of the use of the term is provided by the string of words known as Newton's First Law of motion: 'in the absence of an external force applied to it, a body moves continuously at constant speed in a straight line'. It exhibits, by description, the motions which require no force.[14]

More will need to be said about 'force', but let me first look briefly at its two Newtonian companions, 'weight' and 'mass'. The first refers to a particular sort of force, the one which causes a physical body to press on its supports while at rest or to fall when unsupported. In this still-qualitative form the term 'weight' is available prior to Newtonian 'force' and is used during the latter's acquisition. 'Mass' is usually introduced as equivalent to 'quantity of matter', where matter is the substrate underlying physical bodies, the stuff of which quantity is conserved as the qualities of material bodies change. Any feature which, like weight, picks out a physical body is an index also of the presence of matter and of mass. As in the case of 'weight' and unlike the case of 'force', the qualitative features by which one picks out the referents of 'mass' are identical with those of pre-Newtonian usage.

But the Newtonian use of all three terms is quantitative, and the Newtonian form of quantification alters both their individual uses and the interrelationships between them.[15] Only the unit measures may be established by convention; the

[14] Newton's First Law is a logical consequence of his Second, and Newton's reason for stating them separately has long been a puzzle. The answer may well lie in pedagogic strategy. If Newton had permitted the Second Law to subsume the First, his readers would have had to sort out his use of 'force' and of 'mass' together, an intrinsically difficult task further complicated by the fact that the terms had previously been different not only in their individual use but in their interrelation. Separating them to the extent possible displayed the nature of the required changes more clearly.

[15] Though my analysis diverges from theirs, many of the considerations that follow (as well as a few of those introduced above) were suggested by contemplation of the techniques developed by J. D. Sneed and Wolfgang Stegmüller for formalizing physical theories, especially by their

scales must be chosen so that weight and mass are extensive quantities and so that forces can be added vectorially. (Contrast the case of temperature in which both unit and scale can be chosen by convention.) Once again, the learning process requires the juxtaposition of statements involving the terms to be learned with situations drawn directly or indirectly from nature.

Begin with the quantification of 'force'. Students acquire the full quantitative concept by learning to measure forces with a spring balance or some other elastic device. Such instruments had appeared nowhere in scientific theory or practice before Newton's time, when they took over the conceptual role previously played by the pan balance. But they have since been central, for reasons that are conceptual rather than pragmatic. The use of a spring balance to exhibit the proper measure of force requires, however, recourse to two statements ordinarily described as laws of nature. One of these is Newton's Third Law, which states, for example, that the force exerted by a weight on a spring is equal and opposite to the force exerted by the spring on the weight. The other is Hooke's Law, which states that the force exerted by a stretched spring is proportional to the spring's displacement. Like Newton's First Law, these are first encountered during language learning where they are juxtaposed with examples of situations to which they apply. Such juxtapositions play a double role, simultaneously stipulating how the word 'force' is to be used and how the world populated by forces behaves.

Turn now to the quantification of the terms 'mass' and 'weight'. It illustrates with special clarity a key aspect of the lexical acquisition process, one that has not yet been considered. To this point, my discussion of Newtonian terminology has probably suggested that, once the required antecedent vocabulary is in place, students learn the terms that remain by exposure to some single specifiable set of examples of their use. Those particular examples may well have seemed to provide necessary conditions for the acquisition of those terms. In practice, however, cases of that sort are very rare. Usually there are alternate sets of examples that will serve for the acquisition of the same term or terms. And, though it usually makes no difference to which set of these examples an individual has, in fact, been exposed, there are special circumstances in which the differences between sets prove very important.

In the case of 'mass' and 'weight', one of these alternate sets is standard. It is able to supply the missing elements of both vocabulary and theory together, and it probably therefore enters the lexical acquisition process for all students. But logically other examples would have done as well, and for most students some of them also play a role. Begin with the standard route, which first quantifies 'mass' in the guise of what today is called 'inertial mass'. Students are presented

manner of introducing theoretical terms. Note also that these remarks suggest a route to the solution of a central problem of their approach, how to distinguish the core of a theory from its expansions. For this problem see my paper, "Theory Change as Structure Change: Comments on the Sneed Formalism," *Erkenntnis* 10 (1976), 179–199. ; reprinted in this volume as essay 7.

with Newton's Second Law $\frac{1}{m}$ force equals mass times acceleration $\frac{1}{m}$ as a description of the way moving bodies actually behave, but the description makes essential use of the still incompletely established term 'mass'. That term and the Second Law are thus acquired together, and the Law can thereafter be used to supply the missing measure: the mass of a body is proportional to its acceleration under the influence of a known force. For purposes of concept acquisition, centripetal force apparatus provides a particularly effective way to make the measurement.

Once mass and the Second Law have been added to the Newtonian lexicon in this way, the law of gravity can be introduced as an empirical regularity. Newtonian theory is applied to observation of the heavens and the attractions manifest there are compared to those between the earth and bodies resting on it. The mutual attraction between bodies is thus shown to be proportional to the product of their masses, an empirical regularity that can be used to introduce the still missing aspects of the Newtonian term 'weight'. 'Weight' is now seen to denote a relational property, one that depends on the presence of two or more bodies. It can therefore, unlike mass, differ from one location to another, at the surface of the earth and of the moon, for example. That difference is captured only by the spring balance, not by the previously standard pan balance which yields the same reading at all locations. What the pan balance measures is mass, a quantity that depends only on the body and on the choice of a unit measure.

Because it establishes both the Second Law and the use of 'mass', the sequence just sketched provides the most direct route to many applications of Newtonian theory.[16] That is why it plays so central a role in introducing the theory's vocabulary. But it is not, as previously indicated, required for that purpose, and, in any case, it rarely functions alone. Let me now consider a second route along which the use of 'mass' and 'weight' can be established. It starts from the same point as the first, by quantifying the notion of force with the aid of a spring balance. Next, 'mass' is introduced in the guise of what is today labelled 'gravitational mass'. A stipulative description of the way the world is provides students with the notion of gravity as a universal force of attraction between pairs of material bodies, its magnitude proportional to the mass of each. With the missing aspects of 'mass' thus supplied, weight can be explained as a relational property, the force resulting from gravitational attraction.

That is a second way to establish the use of the Newtonian terms 'mass' and 'weight'. With them in hand Newton's Second Law, the still missing component of Newtonian theory, can be introduced as empirical, a consequence simply of observation. For that purpose, centripetal force apparatus is again appropriate, but no longer to measure mass, as it did on the first route, but now rather to determine the relation between applied force and the acceleration of a mass

[16] All applications of Newtonian theory depend on understanding 'mass', but for many of them 'weight' is dispensable.

previously measured by gravitational means. The two routes thus differ in what must be stipulated about nature in order to learn Newtonian terms, what can be left instead for empirical discovery. On the first route the Second Law enters stipulatively, the law of gravitation empirically. On the second, their epistemic status is reversed. In each case one, but only one, of the laws is, so to speak, built into the lexicon. I do not quite want to call such laws analytic, for experience with nature was essential to their initial formulation. Yet they do have something of the necessity that the label 'analytic' implies. Perhaps 'synthetic a priori' comes closer.

There are, of course, still other ways in which the quantitative elements of 'mass' and 'weight' can be acquired. For example, Hooke's Law having been introduced together with 'force', the spring balance can be stipulated as the measure of weight, and mass can be measured, again by stipulation, in terms of the vibration period of a weight at the end of a spring. In practice, several of these applications of Newtonian theory usually enter into the process of acquiring Newtonian language, information about the lexicon and information about the world being distributed in an indivisible mix among them. Under those circumstances, one or another of the examples introduced during lexical acquisition can, when occasion requires, be adjusted or replaced in the light of new observations. Other examples will maintain the lexicon stable, keeping in place a set of quasi-necessities equivalent to those initially induced by language learning.

Clearly, however, only a certain number of examples may be altered piecemeal in this way. If too many require adjustment, then it is no longer individual laws or generalizations that are at stake but the very vocabulary in which they are stated. A threat to that vocabulary is, however, a threat also to the theory or laws essential to its acquisition and use. Could Newtonian mechanics withstand revision of the Second Law, of the Third Law, of Hooke's Law, or the law of gravity? Could it withstand the revision of any two of these, of three, or of all four? These are not questions that individually have yes or no answers. Rather, like Wittgenstein's "Could one play chess without the queen?" they suggest the strains placed on a lexicon by questions that its designer, whether God or cognitive evolution, did not anticipate its being required to answer.[17] What should one have said when confronted by an egg-laying creature that suckles its young? Is it a mammal or is it not? These are the circumstances in which, as Austin put it, "*we don't know what to say*. Words literally fail us."[18] Such circumstances, if they endure for long, call forth a locally different lexicon, one

[17] Twenty-five years ago the quotation was a standard part of what I now discover was a merely oral tradition. Though clearly "Wittgensteinian", it is not to be found in any of Wittgenstein's published writings. I preserve it here, because of its recurrent role in my own philosophical development and because I've found no published substitute that so clearly bars responding that additional information might permit the question to be answered.

[18] J. L. Austin, "Other Minds", in ~~Collected~~ *Philosophical Papers* (Oxford, 1961), pp. 44–84. The quoted passage occurs on p. 56, and the italics are Austin's. For examples from literature of situations in which words fail us, see ~~James~~ ~~Boyd~~ White, *When Words Lose their Meaning*

that permits an answer but to a slightly altered question: "Yes, the creature is a mammal" (but to be a mammal is not what it was before). The new lexicon opens new possibilities, ones that could not have been stipulated by the use of the old.

To clarify what I have in mind, let me suppose that there are only two ways in which use of the terms 'mass' and 'weight' can be acquired: one which stipulates the Second Law and finds the law of gravity empirically; another which stipulates the law of gravity and discovers the Second Law empirically. Suppose further that the two routes are exclusive; students traverse one or the other so that on each the necessities of the lexicon and the contingencies of experiment are kept separate. Clearly, these two routes are very different, but the differences will not ordinarily interfere with full communication among those who use the terms. All will pick out the same objects and situations as the referents of the terms they share, and all will agree about the laws and other generalizations governing these objects and situations. All are thus fully participants in a single speech community. What individual speakers may differ about is the epistemic status of generalizations that community members share, and such differences are not usually important. Indeed, in *ordinary* scientific discourse, they do not emerge at all. While the world behaves in anticipated ways – the ones for which the lexicon evolved – these differences between individual speakers make little or no difference.

But change of circumstance may make them consequential. Imagine that a discrepancy is discovered between Newtonian theory and observation, for example celestial observations of the motion of the lunar perigee. Scientists who had learned Newtonian 'mass' and 'weight' along the first of my two lexical-acquisition routes would be free to consider altering the law of gravity as a way to remove the anomaly. On the other hand, they would be bound by language to preserve the Second Law. But scientists who had acquired 'mass' and 'weight' along my second route would be free to suggest altering the Second Law but would be bound by language to preserve the law of gravity. A difference in the language learning route, one which had had no effect while the world behaved as anticipated, would lead to differences of opinion when anomalies were found.

Now suppose that neither the revisions that preserved the Second Law nor those that preserved the law of gravity proved effective in eliminating anomaly. The next step would be an attempt at revisions which altered both laws together, and those revisions the lexicon will not, in its present form, permit.[19] Such

(Chicago, 1984). I have compared an example from the sciences with one from developmental psychology in "A Function for Thought Experiments", reprinted in *The Essential Tension* (Chicago, 1977), pp. 240–265.

[19] At this point I will seem to be reintroducing the previously banished notion of analyticity, and perhaps I am. Using the Newtonian lexicon, the statement "Newton's Second Law and the law of gravity are both false" is itself false. Furthermore, it is false by virtue of the meaning of the Newtonian terms 'force' and 'mass'. But it is not – unlike the statement "Some bachelors are

attempts are often successful nonetheless, but they require recourse to such devices as metaphorical extension, devices that alter the meanings of lexical items themselves. After such revision, say, the transition to an Einsteinian vocabulary, one can write down strings of symbols that *look like* revised versions of the Second Law and the law of gravity. But the resemblance is deceptive because some symbols in the new strings attach to nature differently from the corresponding symbols in the old, thus distinguishing between situations which, in the antecedently available vocabulary, were the same.[20] They are the symbols for terms whose acquisition involved laws that have changed form with the change of theory: the differences between the old laws and the new are reflected by the terms acquired with them. Each of the resulting lexicons then gives access to its own set of possible worlds, and the two sets are disjoint. Translations involving terms introduced with the altered laws are impossible.

The impossibility of translation does not, of course, bar users of one lexicon from learning the other. And having done so, they can join the two together, enriching their initial lexicon by adding to it sets of terms from the one they just have acquired. For some purposes such enrichment is essential. At the beginning of this paper, for example, I suggested that historians often required an enriched lexicon to understand the past, and I have argued elsewhere that they must transmit that lexicon to their readers.[21] But the sense of enrichment involved is peculiar. Each of the lexicons combined for the historian's purposes embodies knowledge of nature, and the two sorts of knowledge are incompatible, cannot coherently describe the same world. Except under very special circumstances, like those of the historian at work, the price of combining them is incoherence in the description of phenomena to which either one might alone have been applied.[22] Even the historian avoids incoherence only by being sure at all times which lexicon he is using and why. Under these circumstances, one may reasonably ask whether the term 'enriched' quite applies to the enlarged lexicon formed by combinations of this sort.

A closely related problem – that of the grue emeralds – has recently been much discussed in philosophy. An object is grue if it has been observed to be green before time *t* or if, alternatively, it is blue. The puzzle is that the same set of observations, if made prior to *t*, support two incompatible generalizations: "All emeralds are green" and "All emeralds are grue". (Note that a grue emerald, if

married" – false by virtue of the *definitions* of those terms. The meaning of 'force' and 'mass' is not embodiable in definitions but rather in their relation to the world. The necessity to which I here appeal is not so much analytic as synthetic a priori.

20 In fact, for the Newton to Einstein transition, the most significant lexical change is in the antecedent kinematic vocabulary for space and time, and it moves from there upward into the vocabulary of mechanics.

21 See ~~the reference cited in note 2.~~ Postee,

22 In describing the expanded lexicon it is essential to use terms like 'incompatible' and 'incoherent' rather that 'contradictory' and 'false'. The two latter terms would apply only if translation were possible.

not examined before time *t*, can only be blue.) Here, too, the solution depends upon segregating the lexicon containing the normal descriptive color vocabulary, 'blue', 'green', and the like, from the lexicon that contains 'grue', 'bleen', and the names of other occupants of the corresponding spectrum. One set of terms is projectible, supports induction, the other not. One set of terms is available for descriptions of the world, the other is reserved for the special purposes of the philosopher. Difficulties emerge only when the two, embodying incompatible bodies of knowledge of nature, are used in combination, for there is no world to which the enlarged lexicon could apply.[23]

Students of literature have long taken for granted that metaphor and its companion devices (those which alter the interrelations among words) provide entree to new worlds and make translation impossible by doing so. Similar characteristics have been widely attributed to the language of political life and, by some, to the entire range of the human sciences. But the natural sciences, dealing objectively with the real world (as they do), are generally held to be immune. Their truths (and falsities) are thought to transcend the ravages of temporal, cultural, and linguistic change. I am suggesting, of course, that they cannot do so. Neither the descriptive nor the theoretical language of a natural science provides the bedrock such transcendence would require. I shall not in this place even attempt to deal with the philosophical problems consequent upon that point of view. Let me, instead, attempt to increase their urgency.

* * * *

The threat to realism is the foremost of the problems I have in mind, and it can here stand for the entire set.[24] A lexicon acquired by techniques like those discussed in the preceding section gives members of the community that employs it conceptual access to an infinite set of lexically stipulatable worlds, worlds describable with the community's lexicon. Of these worlds, only a small fraction are compatible with what they know of their own, the actual world: the others are barred by requirements of internal consistency or of conformity with experiment and observation. As time passes, continuing research excludes more and more possible worlds from the subset that could be actual. If all scientific

23 For the original paradox see ~~Nelson~~ Goodman, *Fact, Fiction, and Forecast* ~~(2nd ed.,~~ New York, 1977), Chapt. 3 & 4. Note that the similarity just emphasized is in one very important respect incomplete. Both the Newtonian terms discussed above and the terms in any color vocabulary form an interrelated set. But in the latter case the difference between vocabularies does not affect vocabulary structure, and it is therefore possible to translate between the projectible 'blue'/'green' vocabulary and the unprojectible vocabulary containing 'bleen' and 'grue'.

24 Contrary to a widespread impression, the sort of position here sketched does not raise problems of relativism, at least not if 'relativism' is used in any standard sense. There are shared and justifiable, though not necessarily permanent, standards that scientific communities use when choosing between theories. On this subject see my papers, "Objectivity, Value Judgement, and Theory Choice" in *The Essential Tension*, *op. cit.*, pp. 320–339, and "Rationality and Theory Choice," *The Journal of Philosophy*, 80, (1983), pp. 563–570.

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development proceeded in this way, the progress of science would consist in ever closer specification of a single world, the actual or real one.

A reiterated theme of this paper has been, however, that a lexicon which gives access to one set of possible worlds also bars access to others. (Remember the Newtonian lexicon's inability to describe a world in which the Second Law and the law of gravity were not simultaneously satisfied.) And scientific development turns out to depend not only on weeding out candidates for reality from the current set of possible worlds but also upon occasional transitions to another set, one made accessible by a lexicon with different structure. Once such a transition has occurred, some statements previously descriptive of possible worlds prove untranslatable in the terminology developed for the subsequent science. These are the statements the historian first encounters as anomalous word strings; one cannot imagine what those who uttered or wrote them were trying to say. Only when a new lexicon has been mastered can they be understood, and that understanding does not provide them with later equivalents. Individually, they are neither compatible nor incompatible with statements embodying the beliefs of a later age, and they are therefore immune to an evaluation conducted with its conceptual categories.

The immunity of such statements is, of course, only to being judged one at a time, labeled individually with truth values or some other index of epistemic status. Another sort of judgement is possible and in scientific development something very like it repeatedly occurs. Faced with untranslatable statements, the historian becomes bilingual, first learning the lexicon required to frame the problematic statements and then, if it seems relevant, comparing the whole older system (a lexicon plus the science developed with it) to the system in current use. Most of the terms used within either system are shared by both, and most of these shared terms occupy the same positions in both lexicons. Comparisons made using those terms alone ordinarily provide a sufficient basis for judgement. But what is then being judged is the relative success of two whole systems in pursuing an almost stable set of scientific goals, a very different matter from the evaluation of individual statements within a given system.

Evaluation of a statement's truth values is, in short, an activity that can be conducted only with a lexicon already in place, and its outcome depends upon that lexicon. If, as standard forms of realism suppose, a statement's being true or false depends simply on whether or not it corresponds to the real world independent of time, language, and culture then the world itself must be somehow lexicon-dependent. Whatever form that dependence takes, it poses problems for a realist perspective, problems that I take to be both genuine and urgent. Rather than explore them further here a task for another paper I shall close by examining a standard attempt to dismiss them.

What I have been describing as the problem of lexical dependence is often called the problem of meaning variance. To avoid it and related problems from other sources, many philosophers have in recent years emphasized that truth values depend only on reference, and that an adequate theory of reference need

not call upon the way in which the referents of individual terms are in fact picked out.[25] The most influential version of such theories is the so-called causal theory of reference developed primarily by Kripke and Putnam. It is rooted firmly in possible-world semantics, and its expositors resort repeatedly to examples drawn from scientific development. A look at it should both reinforce and extend the viewpoint sketched above. For the purpose, I restrict myself primarily to the version developed by Hilary Putnam, for Putnam deals more explicitly than others with problems of scientific development.[26]

According to causal theory the referents of natural kind terms like 'gold', 'tiger', 'electricity', 'gene', or 'force' are determined by some original act of baptizing or dubbing samples of the kind in question with the name they will thereafter bear. That act, to which later speakers are linked by history, is the "cause" of the term's referring as it does. Thus, some samples of a naturally occurring yellow, malleable metal were once baptized 'gold' (or some equivalent in another language), and the term has since referred to all samples of the same stuff as the original whether they displayed the same superficial qualities or not. What establishes the reference of a term is, thus, the original sample together with the primitive relationship, sameness-of-kind. If the original samples were not all or mostly of the same kind, then the term in question, for example 'phlogiston', fails to refer. Theories about what makes the samples the same are, on this view, irrelevant to reference, as are the techniques used in identifying further samples. Both may vary over time as well as from individual to individual at a given time. But the original samples and the relation sameness-of-kind are stable. If meanings are the sorts of things that individuals can carry around in their heads, then meaning does not determine reference.

Excluding proper names, I doubt that there is any set of terms for which this theory works precisely, but it comes very close to doing so for terms like 'gold',

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[25] Views which, like mine, depend on talking about the way words are actually used, the situations in which they apply, are regularly charged with invoking a "verification theory of meaning," not currently a respectable thing to do. But in my case at least that charge does not hold. Verification theories attribute meanings to individual sentences and through them to the individual terms those sentences contain. Each term has a meaning determined by the way in which sentences containing it are verified. I have been suggesting, however, that with occasional exceptions terms do not individually have meanings at all. More important, the view sketched above insists that people may use the same lexicon, refer to the same items with it, and yet pick out those items in different ways. Reference is a function of the shared structure of the lexicon but not of the varied feature spaces within which individuals represent that structure. There is, however, a second charge, closely related to verificationism, of which I am guilty. Those who maintain the independence of reference and meaning also maintain that metaphysics is independent of epistemology. No view like mine (in the respects presently at issue there are a number) is compatible with that separation. The separation of metaphysics from epistemology can come only after a position that involves both has been elaborated.

[26] *Op. cit.* Putnam has, I believe, now abandoned significant components of the theory, moving from it to a view ("internal realism") with significant parallels to my own. But few philosophers have followed him. The views discussed below are still very much alive.

and the plausibility of the application of causal theory to natural kind terms depends on the existence of such cases. Terms that behave like 'gold' ordinarily refer to naturally occurring, widely distributed, functionally significant, and easily recognized substances. They occur in the languages of most or all cultures, retain their original use over time, and refer throughout to the same sorts of samples. There is little problem about translating them, for they occupy closely equivalent positions in all lexicons. 'Gold' is among the closest approximations we have to an item in a neutral, mind-independent observation vocabulary.

When a term is of this sort, modern science can often be used not only to specify the common essence of its referents but actually to single them out. Modern theory, for example, identifies gold as the substance with atomic number 79 and licenses specialists to identify it by the application of such techniques as x-ray spectroscopy. Neither the theory nor the instrument was available seventy-five years ago, but it is nevertheless reasonable to suggest that "The referents of 'gold' are and have always been the same as the referents of 'substance with atomic number 79'." Exceptions to that equation are few, and they result primarily from our increased ability to detect impurities and forgeries. For the causal theorist, therefore, 'having atomic number 79' is *the* essential property of gold, the single property such that, if gold in fact does have it, then it has it necessarily. Other properties, yellowness and ductility, for example, are superficial and correspondingly contingent. Kripke suggests that gold might even be blue, its apparent yellowness resulting from an optical illusion.[27] Though individuals may, in fact, use color and other superficial characteristics when picking out samples of gold, that practice tells nothing essential about the referents of the term.

'Gold' presents a relatively special case, however, and what is special about it obscures essential limitations on the conclusions it will support. More representative is Putnam's most developed example, 'water', and the problems which arise with it are still more severe in the case of such other widely discussed terms as 'heat' and 'electricity'.[28] For water the discussion divides in two parts. In the first, which is the more familiar, Putnam imagines a possible world containing Twin Earth, a planet just like our own except that the stuff called 'water' by Twin Earthians is not H_2O but a different liquid with a very long and complicated chemical formula abbreviated XYZ. "Indistinguishable from water at normal temperatures and pressures," XYZ is the stuff that on Twin Earth quenches thirst, rains from the skies, and fills oceans and lakes, much as water does here. If a spaceship from Earth ever visits Twin Earth, Putnam writes:

[27] *Op. cit.*, p. 118.

[28] The force of Putnam's discussion depends in part upon an equivocation that needs to be eliminated. As used in everyday life or by the laity, 'water' has through history behaved much like 'gold'. But that is not the case within the community of scientists and philosophers to which Putnam's argument needs to be applied.

> then, the supposition at first will be that 'water' has the same meaning on Earth and Twin Earth. This supposition will be corrected when it is discovered that 'water' on Twin Earth is XYZ, and the Earthian spaceship will report somewhat as follows:
>
> "On Twin Earth the word 'water' means XYZ."

As in the case of gold, superficial qualities like quenching thirst or raining from the skies have no role in determining to what substance the term 'water' properly refers.

Two aspects of Putnam's fable require special notice. First, the fact that Twin Earthians call XYZ by the name 'water' (the same symbol that Earthians use for the stuff that lies in lakes, quenches thirst, etc.) is an irrelevancy. The difficulties presented by this story will emerge more clearly if the visitors from Earth use their own language throughout. Second, and presently central, whatever the visitors call the stuff that lies in Twin Earthian lakes, the report they send home must take some form like:

> "Back to the drawing board! Something is badly wrong with chemical theory."

The terms 'XYZ' and 'H_2O' are drawn from modern chemical theory, and that theory is incompatible with the existence of a substance with properties very nearly the same as water but described by an elaborate chemical formula. Such a substance would, among other things, be too heavy to evaporate at normal terrestrial temperatures. Its discovery would present the same problems as the simultaneous violation of Newton's second law and the law of gravity described in the last section. It would, that is, demonstrate the presence of fundamental errors in the chemical theory which gives meanings to compound names like 'H_2O' and the unabbreviated form of 'XYZ'. Within the lexicon of modern chemistry, a world containing both our earth and Putnam's Twin Earth is lexically possible, but the composite statement that describes it is necessarily false. Only with a differently structured lexicon, one shaped to describe a very different sort of world, could one, without contradiction, describe the behavior of XYZ at all, and in that lexicon 'H_2O' might no longer refer to what we call 'water'.

So much for the first part of Putnam's argument. In the second he applies it more concretely to the referential history of 'water', suggesting that "we roll the time back to 1750" and continuing:

> At that time chemistry was not developed on either Earth or Twin Earth. The typical Earthian speaker of English did not know water consisted of hydrogen and oxygen, and the typical Twin Earthian speaker of English did not know 'water' consisted of XYZ. . . . Yet the extension of the term 'water' was just as much H_2O on Earth in 1750 as in 1950; and the extension of the term 'water' was just as much XYZ on Twin Earth in 1750 as in 1950.

In journeys through time as in those through space, Putnam suggests, it is chemical formula, not superficial qualities, that determines whether a given substance is water.

For present purposes attention can be restricted to Earthian history, and on earth Putnam's argument for 'water' is the same as it was for 'gold'. The extension of 'water' is determined by the original sample together with the relation sameness-of-kind. That sample dates from before 1750 and the nature of its members has been stable. So has the relation sameness-of-kind, though *explanations* of what it is for two bodies to be of the same kind have varied widely. What matters, however, is not explanations but what gets picked out, and identifying samples of H_2O is, according to causal theory, the best means yet found to pick out samples of the same kind as the original set. Give or take a few discrepancies at the margins, discrepancies due to refinement of technique or perhaps to change of interest, 'H_2O' refers to the same samples that 'water' referred to in either 1750 or 1950. Apparently causal theory has rendered the referents of 'water' immune to changes in the concept of water, the theory of water, and the way samples of water are picked out. The parallel between causal theory's treatment of 'gold' and of 'water' seems complete.

But in the case of water difficulties arise. 'H_2O' picks out samples not only of water but also of ice and steam. H_2O can exist in all three states of aggregation solid, liquid, and gaseous and it is therefore not the same as water, at least not as picked out by the term 'water' in 1750. The difference in items referred to is, furthermore, by no means marginal, like that due to impurities for example. Whole categories of substance are involved, and their involvement is by no means accidental. In 1750 the primary differences between chemical species were the states of aggregation or modelled upon them. Water, in particular, was an elementary body of which liquidity was an essential property. For some chemists the term 'water' referred to the generic liquid, and it had done so for many more only a few generations before. Not until the 1780s, in an episode long known as "The Chemical Revolution", was the taxonomy of chemistry transformed so that a chemical species might exist in all three states of aggregation. Thereafter, the distinction between solids, liquids, and gases became physical, not chemical. The discovery that *liquid* water was a compound of two *gaseous* substances, hydrogen and oxygen, was an integral part of that larger transformation and could not have been made without it.

This is not to suggest that modern science is incapable of picking out the stuff that people in 1750 (and most people still) label 'water'. That term refers to *liquid* H_2O. It should be described not simply as H_2O but as close-packed H_2O particles in rapid relative motion. Marginal differences again aside, samples answering that compound description are the ones picked out in 1750 and before by the term 'water'. But this modern description leads to a new network of difficulties, difficulties that may ultimately threaten the concept of natural kinds and that meanwhile must bar the automatic application of causal theory to them.

Causal theory was initially developed with notable success for application to proper names. Its transfer from them to natural kind terms was facilitated perhaps made possible by the fact that natural kinds, like single individual creatures, are denoted by short and apparently arbitrary names, names coextensive with those of the corresponding kind's single essential property. Our examples have been 'gold' paired with 'having atomic number 79' and 'water' paired with 'being H_2O'. The latter member of each pair names a property, of course, as the name coupled with it does not. But so long as only a single essential property is required by each natural kind that difference is inconsequential. When two non-coextensive names are required, however, 'H_2O' and 'liquidity' in the case of water then each name, if used alone, picks out a larger class than the pair does when conjoined, and the fact that they name properties becomes central. For if two properties are required, why not three or four? Are we not back to the standard set of problems that causal theory was intended to resolve: which properties are essential, which accidental; which properties belong to a kind by definition, which are only contingent? Has the transition to a developed scientific vocabulary really helped at all?

I think it has not. The lexicon required to label attributes like being-H_2O or being-close-packed-particles-in-rapid-relative-motion is rich and systematic. No one can use any of the terms that it contains without being able to use a great many. And given that vocabulary, the problems of choosing essential properties arise again, except that the properties involved can no longer be dismissed as superficial. Is deuterium hydrogen, for example, and is heavy water really water? And what may one say about a sample of close-packed particles of H_2O in rapid relative motion at the critical point, under the conditions of temperature and pressure, that is, at which the liquid, solid, and gaseous states are indistinguishable? Is it really water? The use of theoretical rather than superficial properties offers great advantages, of course. There are fewer of the former, the relations between them are more systematic, and they permit both richer and more precise discriminations. But they come no closer to being essential or necessary properties than the superficial ones they appear to supplant. The problems of meaning and meaning variance are still in place.

The inverse argument proves even more significant. The so-called superficial properties are no less necessary than their apparently essential successors. To say that water is liquid H_2O is to locate it within an elaborate lexical and theoretical system. Given that system, as one must be in order to use the label, one can in principle predict the superficial properties of water (just as one could those of XYZ), compute its boiling and freezing points, the optical wavelengths which it will transmit, and so on.[29] If water is liquid H_2O, then these properties are necessary to it. If they were not realized in practice, that would be a reason to doubt that water really was H_2O.

[29] Laypeople can, of course, say that water is H_2O without controlling the fuller lexicon or the theory which it supports. But their ability to communicate by doing so depends upon the presence of experts in their society. The laity must be able to identify the experts and say something of the nature of the relevant expertise. And the experts must, in turn, command the lexicon, the theory, and the computations.

This last argument applies also to the case of gold, in which causal theory apparently succeeded. 'Atomic number' is a term from the lexicon of atomic-molecular theory. Like 'force' and 'mass', it must be learned together with other terms deployed in that theory, and the theory itself must play a role in the acquisition process. When the process is complete, one can replace the label 'gold' with 'atomic number 79', but one can then also replace the label 'hydrogen' with 'atomic number 1', 'oxygen' with 'atomic number 8', and so on to a total well over a hundred. And one can do something more important as well. Invoking such other theoretical properties as electronic charge and mass, one can in principle, and to a considerable extent in fact, predict the superficial qualities $\frac{l}{M}$ density, color, ductility, conductivity, and so on $\frac{l}{M}$ that samples of the corresponding substance will possess at normal temperatures. Those properties are no more accidental than having-atomic-number-79. That color is a superficial property does not make it a contingent one. Furthermore, in a comparison of superficial and theoretical qualities, the former have a double priority. If the theory that posits the relevant theoretical properties could not predict these superficial qualities or some of them, there would be no reason to take it seriously. If gold were blue for a normal observer under normal conditions of illumination, its atomic number would not be 79. In addition, superficial properties are the ones called upon in those difficult cases of discrimination characteristically raised by new theories. Is deuterium really hydrogen, for example? Are viruses alive?[30]

What remains special about 'gold' is simply that, unlike 'water', only one of the underlying properties recognized by modern science $\frac{l}{M}$ having atomic number 79 $\frac{l}{M}$ need be called upon to pick out members of the sample to which the term has continued through history to refer.[31] 'Gold' is not the only term that possesses or closely approximates this characteristic. So do many of the

[30] At issue, of course, is where to draw the boundary lines that delimit the referents of 'water', 'living thing', and so on, a problem which arises from and seems to threaten the notion of natural kinds. That notion is closely modelled on the concept of a biological species, and discussions of causal theory repeatedly invoke the relation between a particular gene-type and a corresponding species (often tigers) to illustrate the relation said to hold between a natural kind and its essence, between H_2O and water, for example, or between atomic-number-79 and gold. But even individuals who are unproblematically members of the same species have differently constituted sets of genes. Which sets are compatible with membership in that species is a subject of continuing debate, both in principle and practice, and the subject of the argument is always which superficial *properties* (e.g., the ability to interbreed) the members of the species must share.

[31] Even for gold this generalization is not *altogether* correct. As mentioned above, scientific progress does result in marginal adjustments of the original samples of gold by virtue of "our increased ability to detect impurities". But what it is for gold to be pure is determined in part by theory. If gold is the substance with atomic number 79, then even a single atom with a different atomic number constitutes an impurity. But if gold is, as it was in antiquity, a metal that ripens naturally in the earth, changing gradually from lead through iron and silver to gold in the process, then there is no single form of matter that is gold *tout court*. When the ancients applied the term 'gold' to samples from which we might withhold it, they were not always simply mistaken.

basic-level referring terms used in everyday speech, including the everyday use of the term 'water'. But not all everyday terms are of this sort. 'Planet' and 'star' now categorize the world of celestial objects differently from the way they did before Copernicus, and the differences are not well described by phrases like "marginal adjustment" or "zeroing in". Similar transitions have characterized the historical development of virtually all the referring terms of the sciences, including the most elementary: 'force', 'species', 'heat', 'element', 'temperature', and so on.

Over the course of history, these and other scientific terms have participated, sometimes repeatedly, in the sorts of changes epitomized incompletely above by the change in the chemist's use of 'water' between 1750 and 1950. Such lexical transformations systematically split up and then regroup in new ways the members of sets to which terms in the lexicon refer. Usually the terms themselves remain the same through such transitions, though sometimes with strategic additions and deletions. So do many of the items referred to by those terms, which is why the terms endure. But the changes in membership of the sets of items to which these enduring terms refer are often massive, and they affect not just the referents of an individual term but of an interrelated set of terms between which the preexisting population is redistributed. Items previously regarded as quite unlike are grouped together after the transformation, while previously exemplary members of some single category are later divided between systematically different ones.

It is lexical change of this sort that results in the apparent textual anomalies with which this paper began. Encountered by an historian in a text of the past, they strenuously resist elimination by any translation or paraphrase that uses the historian's own lexicon, the one he or she initially brought to the text. The phenomena described in those anomalous passages are stipulated neither as present nor as absent in any of the possible worlds to which that lexicon gives access, and the historian cannot therefore quite understand what the author of the text can be trying to say. Those phenomena belong to another set of possible worlds, one in which many of the same phenomena occur that occur in the historian's own, but in which things also occur that the historian, until reeducated, cannot imagine. Under such circumstances the only recourse is reeducation: the recovery of the older lexicon, its assimilation, and the exploration of the set of worlds to which it gives access. Causal theory provides no bridge across the divide, for the transworld voyages it envisages are limited to worlds in a single lexically possible set. And in the absence of the bridge that causal theory has sought to provide, there is no basis for talk of science's gradual elimination of all worlds excepting the single real one. That way of talking, neatly illustrated by the discussion of gold but not by that of water, has provided causal theory's version of what the tradition described as successively closer approximations to the truth, cutting the world closer to its joints, or just zeroing in.

Such descriptions of scientific development can no longer be sustained. I know of only one other strategy available for their defense, and it seems to me

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18. J. L. Austin, "Other Minds," in *Philosophical Papers* (Oxford: Clarendon Press, 1961), pp. 44-84. The quoted passage occurs on p. 56, and the italics are Austin's. For examples from literature of situations in which words fail us, see J. B. White, *When Words Lose Their Meaning: Constitutions and Reconstitutions of Language, Character, and Community* (Chicago: University of Chicago Press, 1984). I have compared an example from the sciences with one from developmental psychology in "A Function for Thought Experiments," in *Mélanges Alexandre Koyré*, vol. 2, *L'aventure de la science*, ed. I. B. Cohen and R. Taton (Paris: Hermann, 1964), pp. 307-34; reprinted in *The Essential Tension: Selected Studies in Scientific Tradition and Change* (Chicago: University of Chicago Press, 1977), pp. 240-65.

self-defeating, an artifice born of desperation. In the case of 'water' that strategy would be implemented as follows: until sometime after 1750 chemists were misled by superficial properties into believing that water is a natural kind, but it is not; what they called 'water' did not exist, any more than did phlogiston; both were chimerical, and the terms used to refer to them did not, in fact, refer at all.[32] But that cannot be right. Putatively non-referring terms like 'water' can neither be isolated nor replaced by more primitive terms of indubitable referential status. If 'water' failed to refer, then so did other chemical terms like 'element', 'principle', 'earth', 'compound', and many others. Nor was referential failure restricted to chemistry. Terms like 'heat', 'motion', 'weight', and 'force' were equally empty; statements in which they appeared were about nothing. On this showing, the history of science is the history of developing vacuity, and from vacuity one cannot zero in. Some other explanation of the achievements of science is needed.

[32] I take this to be the sort of response Putnam would have provided when the paper I have been discussing was written.

ARTHUR I. MILLER

Discussion of Thomas S. Kuhn's paper "Possible Worlds in History of Science"

The theme of Professor Thomas S. Kuhn's paper is that the history of science is comprised of a succession of theories that can be viewed as a series of separate possible worlds whose lexicons are incommensurable. The thrust of his paper is that the causal theory of reference "provides no bridge across the divide, for the transworld voyages it envisages are limited to worlds in a single lexically possible set" (p. 31). For illustration Kuhn examines how the lexicon of Newtonian mechanics is learned by a scientific community. He focuses on two ways to teach the terms mass and weight. The lexicons of each method are open to the same community. One method stipulates Newton's second law with the gravitational law emerging as an empirical regularity, and vice versa. As long as no anomalies are found there are no lexical difficulties between the segments of the scientific community who have taken the two different paths to learning these terms. Suppose now, continues Kuhn, anomalies are found that require revisions of both laws at once. Revisions may be attempted that include metaphorical extension of certain terms. Since the lexicon of neither route can withstand such changes, eventually a transition (discontinuous) is made to another theory (world) with a lexicon disjoint (incommensurable) with the former. Certain of the new theory's syntax, however, could look the same as the old theory.[1]

Kuhn rightly points out the threat to realism incurred by transitions to new and incommensurable theories. He then turns to Hilary Putnam's causal theory of reference. Putnam tries to avoid the problem of meaning-incommensurability by lifting natural kind terms out of theories and describing their ever-evolving reality status independently of any theory. So, for example, according to Putnam the term electron is the natural kind term that was in the theories of Lorentz, Einstein, Bohr, quantum mechanics and modern quantum electrodynamics despite the incommensurability of certain of these theories. The reason is that the quantity electron in these theories can be referred back through a historical causal chain to a description of electricity responsible for certain

[1] For an analysis of concepts that have persisted almost unchanged through the history of science see G. Holton, *Thematic Origins of Scientific Thought: Kepler to Einstein*, Harvard University Press, 1973.