THREE

Practice All the Way Down

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What are the scientific practices behind Thomas Kuhn's philosophy of science, and what do they tell us about that philosophy? We have begun to understand how to think about the science behind the philosophical inquiries of Hermann von Helmholtz, Henri Poincaré, Albert Einstein, and the Vienna Circle—looking at the understanding of science not as a transhistorical analytic framework but instead as unfolding within history, directed toward *particular* forms of scientific work. My aim is to understand the formation of Kuhn's *Structure of Scientific Revolutions*, not to pick at this or that aspect of its use of paradigms or relation to social theory. Instead, the point here is to look at Kuhn's work as a long struggle to assemble on one side what he had learned in civilian (solid-state quantum theory) and wartime (radar countermeasure) research. On the other, using his rather detailed reading and documentary notebooks, drafts, and letters, I want to show how he pieced together a fundamentally *psychological* picture of how the physical sciences functioned, from theory down, not observation up.

Thomas Kuhn's guide into physics was the Harvard theorist John Van Vleck. A student of Edwin Kemble (who wrote the first American "old" quantum theory paper), Van Vleck made the application of quanta to molecular systems his—and an American—specialty. Not for the Americans was the "shark-like" high theory of European atomic physics, philosophical inquiry, advanced mathematics of group theory, and novel formulations of axioms, and matrices. Instead, the Americans learned to calculate—soon producing new and important, *pragmatic* applications of quantum theory in the calculation of the properties of solids as well as spectral lines.[1]

Kuhn grew up intellectually in the aftermath of that first generation of American theorists. In 1963, just months after the publication of his *Structure of Scientific Revolutions*, Kuhn interviewed his former teacher in the context of the oral history of quantum mechanics. Van Vleck commented drily, "As you look back on them, they [early American quantum theorists] were a pretty undistinguished lot. I guess maybe it should be in the record that I was so pleased that there was a reference to Kemble in the first edition of Sommerfeld's that any American physicist should be mentioned was really something."[2] Van Vleck was an outstanding physicist (in 1977 he shared the Nobel Prize), but it is clear, and was clear to Kuhn, that he measured his success against an image of the leading lights of European physics. Was writing up his summary of quantum mechanics in his "Quantum Mechanics and Line Spectra" rewarding work? According to Van Vleck:

> I think so. Because I don't think I would have discovered quantum mechanics. I think it [my review article] served as useful a purpose as, say, another paper or two to flounder around in the old quantum theory. Of course, you can never be sure. In retrospect, of course, I wish I'd followed up and thought more about my correspondence principle for absorption. That was the nearest I ever came to being on the path of the discovery of the true quantum mechanics. I just never had the imagination to follow that up. I might have later, I can't tell, if other things hadn't broken.[3]

Van Vleck, an American pioneer of the new quantum physics, never lost his sense that there was another physics, a more powerful, deeper going, more original science of the quantum that was taking place in Europe under the steering of Werner Heisenberg, Max Born, and Erwin Schröedinger. Bohr's comment about the importance of "non-spectroscopic things" clearly meant a lot to Van Vleck: "I remember Niels Bohr saying that one of the great arguments for quantum mechanics was its success in these non-spectroscopic things such as magnetic and electric susceptibilities."[4] Magnetic susceptibility was how magnetized a substance would become in response to a given magnetic field; electric susceptibility was how electrically polarized something became in a given electric field. Kuhn pursued these quantities theoretically, appropriately enough—Van Vleck had literally written the textbook on the subject. Of his own original contributions to the field, Van Vleck said he had been well positioned for the work, since he had already written a thesis on magnetism for Percy Bridgman and even tried his hand on calculating these quantities using the old quantum theory. When Van Vleck finally reckoned the dielectric constants using the full-on quantum mechanics of 1926, he was one of a (small) crowd able to apply the theory in this domain. "I must confess," Van Vleck told Kuhn, "that [facing simultaneous work elsewhere] rather burned me up because I felt it was a quite significant achievement in quantum theory. When I mentioned it to Bohr he said, 'you should have got me to endorse it, it would have gone through quicker.'"[5]

Van Vleck and the post-1926 generation of American quantum mechanicians were perpetually playing catch up to the Europeans, trying to carve out a domain where they could shine. Van Vleck said, "You always had a little of the feeling that you were one lap behind compared to what was going on in Europe because those people had an inside track of things compared to what we had. I presume you're familiar with Born's MIT lectures. This was the first introduction to the U.S. in English, I would say, of the new matrix mechanics, which I studied very avidly."[6]

This physics world that Kuhn entered in the mid-1940s was ambiguous. Harvard clearly had an outstanding department, but one unquestionably distant from the center of physics that had flourished, and then been destroyed, in Central Europe. Kuhn graduated from Harvard College in 1943, and immediately took up work under the direction of Van Vleck in the Radio Research Laboratory, which had been tasked with developing radar countermeasures to foil German defenses. Again, Kuhn found himself a bit off to the side of center stage—in the theater of radar, the limelight was squarely pointed at MIT. There, and at Columbia University, in hastily assembled buildings and laboratories, the deans of American physics were pushing on a critical war technology, and beyond that, on new instruments, new theories, and new tools. I. I. Rabi was working on radar; he thought it much more critical to the war effort than the atomic bomb. So too was a young generation of experimentalists and theorists: Edward Purcell, Robert Pound, Norman Ramsey, Charles Townes, and Julian Schwinger, just to name a few. In rapid-fire succession, they duplicated and profoundly extended the British work on magnetrons that generated the radar waves; they built up groups to study components of radar, from antennae and receivers to transmitters and display tubes, all while coordinating with military forces on one side and industry on the other. From constructing radars for coastal defense to fire control against attacking aircraft to blind bombing through the European cloud cover, demands for more accurate systems never ceased. These teams of physicists and engineers radically shortened the microwave radar wavelengths down to barely over a single centimeter. The devices they produced made a difference in almost every field of battle.

The Harvard countermeasures team was vastly smaller than the assembled microwave army at MIT—and the Harvard task was much more circumscribed. Their job (they were directed not to communicate with the primary radar-building group so as to avoid a destructive cycle of measure and countermeasure) was to make jamming devices that would interfere with German radar. These they did build. But without a doubt the most successful innovation was decidedly low-tech; the use of aluminum strips, cut to lengths that would maximally confuse the fire control operators on the ground, whose antiaircraft guns blasted away at the fleets of Anglo-American bombers.

Kuhn joined the group and was soon assigned to write theoretical reports on a variety of topics: what size echoes could be expected from various ships, how well would the jammers work, and what power levels would be required to foil a radar signal hitting an Allied ship at distance R from the enemy radar. Other people would do actual measurements down at Chesapeake Bay, where the Naval Research Laboratory had a station. Using fairly idealized models of the sea (a flat plane), and a host of empirical and semiempirical parameters, Kuhn listed the reflectivity of a distant target that had to take into account the effect of the sea ("sea zone") and a close target that could be treated as if it was in the air ("air zone"). Calculating effects from semiempirical formulae needed to be done—and Kuhn did it. But it was not, in any sense, a new kind of physics, theoretical or experimental.

Here is an example: The power density delivered to a radar from a jammer was given by

$$S_j = .3808 \left(\frac{P_j G_j}{R^2}\right) \sin^2\left(\frac{2\pi h_j h_r}{3\lambda R}\right),$$

with S_j in watts per square meter and the other lengths in feet; G_j, the jammer gain; h_j (h_r), the height of the jamming (radar) antenna; R, the distance of target from the radar; P_j, the power in watts of the jammer pulse, and λ, the radar wavelength (in feet). As is immediately visible, this was not a complex and analytically-precise electrodynamic calculation. It was instead rough work ("preliminary, tentative and incomplete" in the author's words), a workable combination of measured, guessed, and calculated quantities. Kuhn carefully filled out a table that assembled the measured radar reflection parameters for a series of ships from the battleship USS *New York* and the heavy carrier USS *Franklin* down to the bow and broadside of a surfaced submarine. Using graphical methods to solve some equations, Kuhn extended the work to include a variety of different jamming devices—all the while recognizing that some of his key parameters could be off by a factor of ten.[7]

By the time Kuhn arrived in Europe, the flak directed at Allied bombers by German antiaircraft was falling apart, especially in "blind" conditions.

There were, Kuhn reckoned, five possible factors: personnel, ammunition, and early warning deficiencies on the German side. The Allies' "window" (aluminum, radar-reflecting strips) could be blocking the Nazi radar, or finally the Germans' best radar had not been deployed near front line targets. In the end, Kuhn judged that the German deficiencies were real (according to intelligence). To be safer, nonetheless, he recommended that the U.S. Air Force should put jammers in their B-26s.[8]

From August 25 and 26, 1944: "The candle is too short to attempt a complete account, but I'll try to hit the high spots We landed near the base of the Cherbourg peninsula, just East of the peninsula itself. . . . We passed through St. Lo, an amazing sight the entire town is a shambles. There are scarcely five buildings with roofs or walls and none are unscarred. There seems no reason for people to return to it." Kuhn noted the equipment he came across—like this one on August 27: "Four antenna towers about 150″ high. Two unrigged." The retreating Germans had left power supplies, receivers, transmitters, walkie-talkies, telephone lines—Kuhn inspected the sites room by room, recording the abandoned apparatuses by make and frequencies, along with bits of intelligence. "57 Rue Cricourt Alphonse Herzberger—Director of the [Uniprix] Co. He's dead. His wife goes with the Boches."[9] Or two days later: "Interrogation Capt. Kemper (sp.?). . . . There is infrared apparatus. There are 50–60 such machines. . . . They were made in Augsburg by Messerschmidt. Thinks production transferred to Egei. They were airborne." Then the technical questions began, culminating in a summary of procedures: "The approximate position [of the Allied planes] is worked out at the primary center. This is sent to Chateau Beaumont by wire. From their [*sic*] to Chateau Dutreux from which orders are sent to Luftwaffe in the Hotel Luxemburg by wire." Kuhn learned of the attempts to push the power of the antennae, to change transmission frequencies, to penetrate the radar-blocking fog created by the Allies' "window"—he recorded the Nazi frustration with the lack of bearings needed for the apparatuses and the bugs associated with their most up-to-date receivers.[10]

This was Kuhn's war: calculation of jamming capabilities, liaison between units, working at Harvard, and then hitching rides back and forth just behind the advancing Allied front in France as he and others struggled to piece together the technical and organizational structure of German air defense. It was work in a small unit, often by himself, sending information back up the chain of command. For physicists at the end of the war who had been highly placed in the nuclear weapons laboratories or even the main radar development laboratories, their peacetime work was shaped by a host of radically new forms of work. They had learned how to collaborate with industry and in massive groups, how to garner large-scale government contracts, and how to join federal and university funding. From cyclotrons to newly interdisciplinary teamwork laboratory design, from new forms of computation to a daily collaboration with engineers, the war had taught American physicists a new way of working. So too it was with radar, which had to invent a field (microwave physics) that demanded novel forms of calculation, experimentation, and manufacture. Radio astronomy (in part) emerged from microwave expertise; so did astronomical work (not least Purcell's 21-centimeter work); Nicolaas Bloembergen, Edward Purcell, and Robert Pound's theory of nuclear magnetic resonance; and Charles Townes's maser.

Young Kuhn's work on the counter-radar project was *not* at the microwave frontier, not even close. By the end of the war he was worlds away from the transformed physics that marked short wavelength work at MIT, Columbia, or Bell Laboratories. Kuhn, in a certain sense, had remained within an older kind of work, cocooned, as it were, in small-scale work at the Radio Research Laboratory, working on countermeasures to generate interference in the far-from-cutting-edge German radar. He had a close and good working relationship with Van Vleck, but little knowledge of the advanced mathematical physics developed in the theory group run by Julian Schwinger and his colleagues or by the Harvard experimental physicists. Much later, historian Costas Gavroglu asked Kuhn about his return to civilian physics at the end of the war, "You studied solid-state physics with Van Vleck , . . . Were you interested in the subject itself or in working with Van Vleck?" Kuhn responded, "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. . . . Otherwise I would have shot for a chance to work with Julian Schwinger."[11]

Maybe. It would have been a huge jump from the semiempirical radar countermeasure work or the undergraduate physics courses Kuhn had had to Schwinger's hard-driving, formal, mathematically dense, and unvisualizable quantum field theory. Even Schwinger's wartime Green's function calculations of equivalent circuits had no correspondingly difficult work in the Radar Countermeasures program. (For calibration: Schwinger often insisted, not without a bit of disdain, that Richard Feynman had, with his diagrams, "brought quantum field theory to the masses.") In any event, after the war, Kuhn undertook a thesis problem under Van Vleck's supervision, to which Kuhn contributed a significant new approximation method to calculate certain parameters in what was then called solid state physics: the cohesive energy, the lattice constant, and the compressibility of monovalent metallic solids. This was the kind of problem Van Vleck liked, and in fact, it was, essentially, the theoretical solid state physics (applied quantum mechanics) of America before the war.

You can see Kuhn's prewar physics in his citations as he fought his way to a characterization of metallic properties. Central to his efforts was Eugene Wigner and Fred Seitz's work from 1933–34, where they developed a quantum mechanical method for calculating the properties of metallic lattices. For help with the mathematics, the young physicist (like so many others) referred to Whittaker and Watson's classic textbook *A Course of Modern Analysis* (1927)[12]—again, a long way from the novel methods introduced by Schwinger and others to calculate the properties of radar components. Throughout, Kuhn made good use of Van Vleck's by-then classic text, *The Theory of Electric and Magnetic Susceptibilities* (1932), alongside pre-quantum mechanics work like Bohr's own 1923 work, presented in Harvard physicist Edwin C. Kemble's *Principles of Quantum Mechanics* (1937). You can see graphically the contribution of the Kuhn-Van Vleck correction to what was known from Herbert Fröhlich and Frederick Seitz in the figure 3.1. Note the difference between the dashed and solid line for sodium (Na), potassium (K), and rubidium (Rb), as well as the numerical differences (given in figure 3.2) between Seitz's calculation for lithium, for example. For ε measured in Rydbergs, Seitz had 0.700, and Kuhn, using the standard quantum mechanical (W.K.B.) approximation, had 0.706. For the

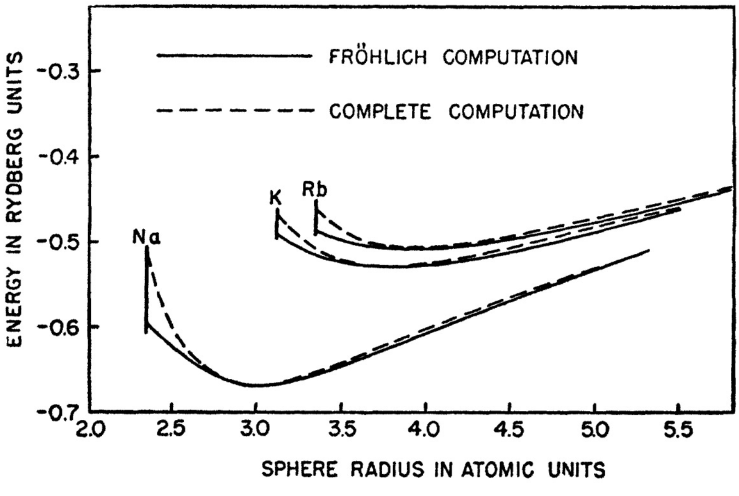

Figure 3.1. Ground-state energy as a function of sphere radius for Na, K, and Rb: comparison of Fröhlich's formula with the complete computations of (A).

TABLE I. Theoretical values of ϵ_{om} and r_{sm}.

Element	ϵ_{om}(Ry.)		r_{sm}(a.u.)	
	W.K.B.	Other[a]	W.K.B.	Other[a]
Li	0.706	0.700	2.84	2.87
Na	0.667	0.668	3.00	2.97
K	0.530	0.530	3.78	3.75
Rb	0.508	0.506	3.92	3.87
Cs	0.468		4.28	

[a] The values for Li are taken from F. Seitz, Phys. Rev. **47**, 400 (1935). The values for the other elements are taken from (**A**).

Figure 3.2. Theoretical values of ϵ_{om} and r_{sm}

radius measured in atomic units, Seitz had 2.87, and Kuhn, with the W.K.B. approximation, got 2.84. A field-changing result this was not.

Still, one should not diminish this work: Kuhn's work with Van Vleck led to an approximation method that was and still is used. But with a few exceptions (Kuhn referred, for example, to the Japanese theoretician, Isao Imai, whose relevant paper appeared in 1948), it was work that, in a very deep sense, was of another, earlier moment in (prewar) history. No fundamentally new instruments were involved in producing the work, Kuhn exploited no radically innovative mathematical or automatic calculation techniques—indeed Kuhn used no novel theoretical concepts. No big collaborations here, no centralized laboratory, no adaptation of war-gleaned knowledge at all. Through both his military and civilian work, Kuhn had seen a physics that was in every way a conglomerate of applications, a working-through of science that had been fundamentally transformed elsewhere. If Van Vleck, despite his remarkable contributions of the 1930s (for which he was awarded the 1977 Nobel Prize), always felt (and said) that American theoretical physics was "a lap behind" the Europeans, Kuhn's was a working-out of an approximation method to Van Vleck's application, a lap behind the lap behind the quantum upheaval of 1926 that had shaken the pillars of physical thought, from causality and visualizability to determinism and locality.

Twice removed from Kuhn's thesis work, shining like distant stars (Kuhn to Van Vleck, Van Vleck to the quantum founders), stood the towering figures of Niels Bohr, Werner Heisenberg, Erwin Schrödinger, and Albert Einstein. By the time Kuhn finished his countermeasure work, he had a strong sense of the productive and necessary function of shared models and textbooks. His own three physics articles squarely built on the published prewar accomplishments of Van Vleck, Seitz, and Wigner; Kuhn cited, and followed, the mathematical physics textbook of Whittaker and Watson, the quantum mechanics text by Kemble, and the defining text on susceptibilities by Van Vleck. Through his war work and the peacetime study of monovalent metals he had gained a real appreciation for the marginal limits of theories with articulated models and better approximations.

Kuhn's whole surround was, to grab his own later term, "normal science": his topic, his working environment, and his techniques—a war, a continent, and twenty years away from one of the extraordinary scientific upheavals in the history of science. In 1949–50, Kuhn was writing up his calculations for publication in the *Physical Review* and the *Quarterly of Applied Mathematics*.[13] Meanwhile, he had been elected to the Harvard Society of Fellows, begun his tenure there, and continued teaching for the university's president, James Conant.

"Notes and Ideas," Kuhn wrote at the top of the first page of a brown "Handy Note Book," and listed for himself his reading through March 1949. Alfred Tarksi's *Introduction to Logic* (check—according to Kuhn's note, that meant "read *in toto*"); Joseph Henry Woodger's *Technique of Theory Construction* (check); Leonard Bloomfield's "Linguistic Aspects of Science" (1939; check); John Dewey's, *Reconstruction in Philosophy* (1920; check); Alfred Jules Ayer, *Language Truth and Logic* (1936; check); and Wiener's *Cybernetics* (1948; check). Somehow, though listed, Immanuel Kant's *Critique of Pure Reason*, John Stuart Mill's *Logic*, and Bertrand Russell's *Scientific Outlook* did not quite get the check of a complete reading.[14]

Right out of the box, Kuhn's position was clear:

> Weaknesses of the positivist or operational position: As Ayer indicates, the doctrine that only questions whose answers can be effected by an activity are meaningful must be taken to be a doctrine of "verification in the weaker sense." Strict verification rules out virtually everything as meaningless. But the notion of "verification in the weaker sense" requires considerable examination."[15]

After ruminating on Ayer's qualifications on "verification in the weaker sense," Kuhn opened a new page, on March 29, 1949, under the header "Language." One should, Kuhn wrote, approach the topic through the written language, since it was simpler had fewer signs, and would be more easily formalized than its spoken counterpart. "Some signs (simple or complex) stand for things i.e. are correlated regularly." Then Kuhn broke off his reasoning and entered in square brackets: "[This is silly: I might as well start this with a physical vocabulary, say [Rudolf] Carnap's. So let's postpone it.]" He scratched an enormous "X" through the page. Nothing then until mid-April 1949, when he came back to Susanne K. Langer (*Philosophy in a New Key: A Study in the Symbolism of Reason, Rite, and Art*) as well as Robert Merton's 1939 article "Science, Technology and Society in Seventeenth Century England" (which he actually read, both got a check) and to Carnap, whose work Kuhn lists as something he should read,(but didn't (or in any case not sufficiently *in toto* to earn a check). But Piaget he did read, check included, focusing on the psychologist's 1946 "*Judgment and Reasoning in the Child* as well as *Notions de vitesse et de movement chez l'enfant*."[16]

Kuhn's reading zeroed in on Piaget from May 21–25, 1949, *Judgment and Reasoning in the Child* and a few days later and more intensively, *Les Notions de mouvement et de vitesse chez l'enfant.* On June 14, 1949, he wrote, "The Piaget reading is useful primarily in shaping my own general view. It can't be transplanted too literally for the kids haven't got the logical criteria of the adults I deal with. However . . ." and then Kuhn went on that same day to list six points that he derived from his reading of Piaget, points I paraphrase (and excerpt) from Kuhn's notes as follows:

"Egocentrism" (Piaget's term for the earliest of his child development stages). In the beginning of each science, the emphasis is on the sensually (observationally) obvious. This is "clearly visible in dealing with the vacuum notion."

Childish conceptualizations are like those of adults insofar as both begin by seeing (or potentially seeing) "all," meaning that they can "reconstruct the experiment correctly." Moreover, the child, like the adult, is capable of "using conceptualizations (or verbalizations) that ignore or contradict something he 'knows.'" In particular, one can discover the intention of a word used by the child by noting its conditions of the word's application—and this way discover the "concept" (Kuhn's term) that the word connotes. But the child's concepts, though *psychologically* coherent or satisfying "generally contain logically contradictory elements, that is, elements such that the word and its opposite can be applied to the same situation." Only through a "gradual evolution" of experience and logic are the contradictions "weeded out."[17]Kuhn continued, "Crudely

& metaphorically we may generalize as follows. There is the 'physically visible world' consisting of what we can see with our available sensory & technical equipment. This phys[ical] v[isible] w[orld] is the pure raw flux, unorganized." We structure this flux by creating gestalts or conceptualizations to which we assign symbols and phrases that constitute our "psychologically visible world that 'may contain all or part'" of the physical visible world. Kuhn's layered account starts with the raw input of experience, but only when this input is structured psychologically does it begin to count as objects and their relations: "To 'see' a tree or a velocity difference is to 'see' something in the psych[ological] v[isible] w[world] which is in turn a creation from the phys[ical] real world." Though we may be conscious of the physical world outside psychological one, this is neither necessary nor even predominant, according to Kuhn (reading to the general case through Piaget). Back to the notebook: "Usually when our attention is drawn to these we'll expand the psychological real world to include them." "We are thus conscious of the physic[ical] v[isible] world, but can only 'see' and can only *talk* about the pysch[ological] v[isible] w[orld]."[18]

Children use "élan" for "impetus." Here, Kuhn drew on Piaget's chapter in *Mouvement et vitesse* (which he refers to as M & V) on acceleration.

"Note that in M & V the children are, presumably, capable of recognizing that accelerated motion is not "at uniform vel[ocity]" and yet apply all analytic tools of the uniform velocity. The clue here is that they don't know uniform velocity either tho[ugh] their tools are nearer to fitting it."

"The theory enunciated in (2) is entirely compatible with the importance of 'mental experiments" (as in Galileo, Einstein, etc.) in fact it demands them. Normal scientific method doesn't do this. Also note (M & V, p. 254) the importance of simplifying mental constructions (the extended incline) in the isolation of minimal conceptualization."

Kuhn's final observation is that the role of numbers comes last and incompletely—"note the tendency of [Piaget's] Stage IV kids [so far along developmentally] to use numbers for accelerated motions." The quantification brought to bear on "simple laws" but not to "physical views." The view that quantitative treatment comes late in the scientific process stays with Kuhn through the whole of his work, from these first jottings through his 1962 *Structure of Scientific Revolutions*.[19]

The next day, June 15, 1949, Kuhn came back in his notebook to the analogy between Piaget's children and the history of science. "The outline in 2 above makes phys[ical] science close to s[ocial] s[cience] and to psych[ology] in its method. The difficulties of s[ocial] s[cience] then inhere in the difficulty of recognizing clearly the sources of formal contradictions with the intensions of its vocabulary." Kuhn is saying that all three domains—physical science, social science, and Piagetian child psychology—exhibit moments of "formal contradiction" that must be worked out to enter a new developmental stage. This is straight from Piaget's *Mouvement et Vitesse*, page 263, where, as Kuhn immediately pointed out in his notes, Piaget highlights the mismatch between the psychological and physical world. The child in such a stage registers this point of contradictory beliefs as "groping" (Kuhn's term). On the one side, Piaget notes that the child tries actions that do not correspond to reality; on the other side, he points out that reality corrects the child's expectations. Slowly, with difficulty, expectations and reality begin to align.[20]

Bit by bit, Kuhn attached his account of the history of science to Piaget's childhood-staged sequence of development for speed and movement. At first, the links were hesitant, with cautionary notes—but as he worked through the psychological texts the identification became ever stronger. By the time he concluded his second point in the Piaget comments above, he had left childhood altogether and spoke directly to the adult world of laboratories and blackboards: "The scientific process consists of the attempt to minimize verbal equipment implied by (or inherent in) the psychological r[eal] w[orld]." That scientific process is a continuing one, forged by the encounter of the psychological with the logical and scientific apparatus that brings us the *physical* visible world.[21] Throughout, Kuhn followed Piaget in presenting a bilayer analysis: on the one side there was the physical world and, on the other, its not-always matched representation in the verbal-psychological. Indicating the relative autonomy of the psychological world, Kuhn pointed out, tentatively, there could be other such "worlds" including "the aesthetic & ethical."[22]

This psychologically inflected Kantianism (sharp division between world and representation) then came to an introspective argument: "Everyone has occasionally had the experience of vehemently defending (with complete assurance) an idea or theory which he suddenly, in mid-course, finds totally contradictory. He is then shaken emotionally. Subsequently he can't understand how he could have thought this. Thus we get an illustration of the ease with which one embraces logical contradiction, and the nature of the recanting of ideas upon its discovery."[23] Logic, and for that matter physical reasoning, is decidedly less powerful than the demands of the psychological. "The growth of a sci[entific] conceptualization," Kuhn wrote, "represents a struggle between

a) Pscyh[ological] reasonableness,
b) Logical consistency,
c) Adequacy and applicability to phys[ical] v[isible] w[orld]."

It is, Kuhn continued, not b) or c) but instead psychological reasonableness that is "most important" though "commonly ignored."[24]

With these reflections on the primordial role of the *psychological* view of the world—and the reasoning behind it—Kuhn completed the arc of his first pass toward an account of scientific change. Having begun with the self-admonition that the passage from child to field (ontogeny to phylogeny) shouldn't be taken "too literally," by June 1949, Kuhn had landed that traverse solidly:

> I am supposing that the process Piaget sketches for children takes place at all ages when the range of conceptual thought is extended. This process is of course then redirected to a particular area of thought. . . . New conceptualizations should come from men who were (1) raised in an atmosphere in which these conflicts were implicitly recognized and (2) not set in an old conceptualization. . . . "Scientific contributions must fit the times . . . ," where "fit the times" means [by Kuhn's lights], "be advanced at a time when there's significant intellectual and emotional dissatisfaction with existing 'intensions' to produce the flexibility necessary for acceptance of a new 'intension.'"[25]

This Piagetian psychological "view of the world" dominates for Kuhn—and structures the way he reads Weber. On June 17, 1949, Kuhn, reading the Edward Shils's translation of *Methodology in the Social Sciences*, was ecstatic: "The Weber book is continually brilliant." Kuhn's reading pulled Weber's attention to the *social* sciences over into the *physical* sciences, sometimes explicitly, sometimes not. Take Kuhn's reading of Weber's "'Objectivity' in Social Science." On page 80 of the English translation Kuhn was using, Weber says quite explicitly that he is *not* talking about scientific laws: "not with the 'laws' in the narrower exact natural science sense, but with *adequate* causal relationships expressed in rules. . . . The establishment of such regularities is not the *end* but rather the *means* of knowledge."[26] Now, when Kuhn glossed this section he read the discussion into his notebook not as about *social* laws but as about *scientific laws*. Quoting the above passage, he wrote, "The establishment of such regularities [Kuhn then interpreted "such regularities" as: "i.e. scientific laws"] is not the *end* but instead the *means* of knowledge."[27]

Even more directly yanking Weber into his own psychological idiom, Kuhn, reading Weber's analysis of objectivity, said that Weber's dissection of social scientific objectivity showed a "marked resemblance to my own of physics if the role of [Weber's] 'value' is taken by the 'pscyh[ological] coherence' etc." Here is a final example of Kuhn's reading of Weber. The "ideal type," which, as Weber stresses, does *not* correspond too closely to reality in the social sciences, becomes, for Kuhn, something else: the ideal type is likened to physical idealization—an ideal type is "an aspect of the 'fact.'"[28]

In short, Kuhn's picture was this: a psychological ordering of the world dominates, subordinating both logical and physical orderings of the world around us. When he read Weber, he did so by assimilating it into a fundamentally *psychological*, rather than *social scientific* or more specifically *sociological* frame, and he ventriloquized a Weber who would speak to the physical sciences. With these thoughts about "re-conceptualizations" and a developmental set of steps from observation through articulation to quantification, Kuhn began, on July 5, 1949, to sketch a systematic treatment. "Consider the following outline for a book," Kuhn wrote, calling it "*The Process of Physical Science*":

Part I. Language and Logic: The Tools of Thought
- Language
- Logic & Math
- New (Linear) Linguistic Modes

Part II. The Scientific Function
- The Physical Real World & the Psychological Real World
- The Problem for Science
- The Emergence of Explicit Tools

Part III. Science at Work. Examples from the History of Science
- Appendix: Relation to other Sciences including Social

The next day, July 6, Kuhn reorganized the book into a simpler, two-part enterprise, putting the psychological up front, pruning the formal linguistic, foregrounding Galileo, and integrating the generalization to other sciences: "The book would be probably easier to write and create less difficulty due to scholarly shortcomings if written as follows":

Part I [Language and Logic: The Tools of Thought]
- The Physical and Psychological World

Language, Logic, and Math
The Adjustment to Science

Part II [The Scientific Function]
1. The Emergence of Explicit Tools and Organized Efforts
2. Cases of Scientific Development & the Impact of G[alileo] G[alilei]
3. The Relation to other Sciences.

Piaget (reinforced by Weber) stands through and through Kuhn's early argument as decisive in his antipositivism. Psychology—not philosophy, not logic, not physical reasoning—was what propelled Kuhn away from the formal positivist philosophy so privileged in textbook versions of scientific theory. Kuhn then explained how a concept (in his first but crucial formulation) could shift its meaning utterly before and after a psychological "re-orientation."

> The development of a word like "velocity" is partly the removal of contradiction in its intension and a shift in the intension itself (Piaget's kids) and partly an increase in the intension itself. This last is accompanied by a switch from *v* as a transitive quantity to *v* as a completely intr[ansitive] measure. This doesn't mean we've learned what vel[ocity] *is* but that we've changed the meaning of the word. The shift in meaning shows up as a shift in formal properties.[29]

With these words, Kuhn closed his 1949 notebook.

So it was that by July 1949, Thomas Kuhn had a meaning-centered, developmental, psychologically-driven account of a staged structure of scientific process. Soon Kuhn had a chance to voice his newly configured views, if not to the general public then to a correspondent. Sometime in 1949 (the exact date is unclear), Dr. Sándor Radó, a Hungarian psychoanalyst working at Columbia, wrote to Kuhn, hoping to find support for his view that psychoanalysis could indeed be a "basic science" with experimentation at the center and a sharp transition from "why" to "how" questions marking the triumph of scientificity. The full letters back and forth between Radó and Kuhn have not survived in the archives, but the bulk of Kuhn's tough responses has. Your view of science, Kuhn told the analyst, was a myth, an "immense overestimate of the role of experimentation and of novel observation in the scientific revolution of the sixteenth and seventeenth centuries." This stress on theory over experiment is already a central and often-emphasized feature of Kuhn's thought. He continued, "It is my own opinion that most of the progress in *physical* science before 1750 was achieved by conceptual reorientation toward areas of experience which had been considered by ancient and medieval thinkers, that this reorientation was accomplished without much qualitatively new observation of natural phenomena, that most of the so called crucial experiments of this period were actually designed as demonstrations for sceptics [*sic*] rather than as research tools, and that the *gedanken Experiment* was a more important tool than the physical experiment."[30]

No scientist, Kuhn went on to tell Radó, actually proceeds from the start by producing objective, quantitative work (shades of Piaget). No one. Instead, this process only occurs step-by-step over time, with abstraction playing the truly fundamental role.

> This may be usefully restated in psychological terms by pointing out that observation (and thus experiment) are not capable of the sort of objectivity required for a truly Baconian investigation. At the most elementary level this is shown by psychological experiments on perception, and at a level of greater complexity it is indicated by the experiments associated with the Gestalt school which indicate that we tend to see things first as wholes, that our perceptions of the parts are affected by the manner in which we view the wholes, and that cultural and educational factors may alter our perceptional groupings.

There is, in fact, a failure to see—later generations would go on to wonder how their forebears could be "so dumb."[31]

Gestalts reshaped the parts of an inquiry in sudden and determinative ways. This left Kuhn dubious that "how" and "why" questions could be separated, much less made the basis of scientific status. Any "how" query, argued Kuhn, was only "how" with respect to abstract aspects of the phenomena. Psychology was *always* going to shape what, at a given time, would count as a satisfactory response to "why" questions.[32]

Over the following year or two, Kuhn's views solidified, in part through his reading and research within the open-ended Society of Fellows postdoctoral position, and in part through his experience teaching in the General Education program. On January 6, 1951, Kuhn wrote to historian David Owen, chair of Harvard's General Education Committee, about his field of study and the course he wanted to teach. His emphasis throughout would be on the sources of science, not the end products of research. Such a process rather than product approach could make "an important reorientation in methodological thinking, and I suspect that such reorientation cannot but affect our notions as to the sources of knowledge."[33]

Here in the letter to Owen, Kuhn applied "re-orientation" to the work he was doing—bringing his label of "re-orientation" (applied, most directly to the switch from Aristotle's to Galileo's notion of velocity) to refer to Kuhn's own historical work. (Aristotle to Galileo identified terminologically with the logical positivists to Kuhn.)

> My starting point is that the implicit scientific injunction, "Go ye forth and gather the fruits of objective observation," is a meaningless one which no one could carry out. The complexity of the objects presented by experience permits an infinity of independent observations; so that the process of scientific observation presupposes a choice of those aspects of experience which are to be deemed relevant. But the judgment of relevancy is made on a largely unconscious basis in which commonsense experience and pre-existing scientific theories are intimately intermingled.

Here again, the "psychological view" dominates; as with the early-stage Piagetian child, perception flows in regular, determined channels.

Through insight prized from experimental psychology and linguistic research, Kuhn reported to Owen, he now took it "that objective observation is, in an important sense, a contradiction in terms. Any particular set of observations . . . presupposes a predisposition toward a conceptual scheme of a corresponding sort: the 'facts' of science already contain (in a psychological, not a metaphysical, sense) a portion of the theory from which they will ultimately be deduced." The "conceptual scheme" or "orientation" leads the researcher to attend to some elements and to ignore the rest. In fact, as Kuhn stresses, the conceptual scheme actually *blocks* perception of the (nonconforming) rest.[34]

Still, in the January 1951 letter to Owen, Kuhn brought up again the "re-orientation" of the concept of velocity that marked the last entry of his 1949 notebook. "By the time of Galileo a complete reorientation toward these common experiences had occurred—" Aristotle saw speed as a total distance divided by time, and focuses on the gradual stopping of the pendulum. By contrast, Galileo, saw speed as instantaneous, and it is this fundamental change that is the main step—the quantitative law comes last: "given the reorientation with which Galileo starts, the laws for which he is known could not for long have evaded scientific imagination." In a further development of the outline he had set in 1949, Kuhn now wanted to stress the question: "what reorientation and from what sources," saying he aimed to write a "history of reorientation" in the modern world, for example in the conflict between the mechanical and field theories that led, inter alia, to general relativity and unified field theory.[35]

By the time Kuhn wrote to David Owen, he was deep in preparations to give the Lowell lectures set for March 1951. Above all, Kuhn envisioned his series of talks to begin in history but aim for a philosophical and methodological goal, the "isolation of certain non-logical, perhaps even psychological characteristics of creative research in physical science."[36] His picture of a physical view and psychological view remained—the latter structuring the flux of the former and it is this two-step that Kuhn had in mind when he assigned a preliminary title to his series: "The Creation of Scientific Objects."

The first of the Lowell lectures (ultimately called "The Quest for Physical Theory: Problems in the Methodology of Scientific Research") commenced on Friday, March 2, 1951. Kuhn laid out his target: the widespread empiricist view that science proceeded by dispassionately reasoning from observation to theory. Against this position, he attacked with a very different picture of the work accomplished by Galileo, Dalton, and Lavoisier. Defending his decision to focus entirely on early science, he argued that first, contemporary science was far too "technical" and "abstract" for the occasion. But second and more importantly, Kuhn cautioned his audience that "we" believe in contemporary science, and only older science could offers us the distance needed to study the formation of its conceptual schemes. Nothing would be lost choosing the antique, because "I believe the historical unity of science, or more accurately the historical unity of scientists, permits the picture of science which we will derive in this manner to be applied without significant alteration to contemporary science." "Textbook science" is responsible for the widespread empiricist understanding of how science works. Kuhn said it was a "fable," nothing more, to think that our way of justifying science today has anything to do with the creative science that generated it in the first place. In fact, there were "two distinct meanings of the word science." Note the still-sharp imprint of Hans Reichenbach's contexts of discovery and justification. "In the first," said Kuhn, "science is conceived as an activity, as the thing which the scientist does. In its other meaning science is knowledge, a body of laws and of techniques assembled in texts and transmitted from one scientific generation to another."[37]

For Kuhn, only a fabulous account of science could make the past look like its textbook image. About a third of the way through his first lecture, Kuhn wrote in red capital pencil: "BAD HISTORY" (as opposed, presumably, to the good history Kuhn was reading in the work of, for example, the

French historian of science Alexandre Koyré). Galileo, so Koyré and Kuhn contended, could not have gotten his results from the Leaning Tower of Pisa—he wasn't there when the mythic history had him doing the experiment; he was just at that moment writing about the physics of fall in a thoroughly incompatible way. Had he, against the facts, tossed wood and lead simultaneously off the tower, he would have seen the lead hit first because of air resistance. Even the inclined plane, Kuhn argued, could not have served as advertised. (Kuhn and his physics colleagues at Harvard had built such an inclined plane apparatus that worked the way it was supposed to, but it took the Cambridge physicists the best modern machine tools and set them back the startling sum of over $500.) In opposition to the textbooks, Kuhn emphasized that Galileo had used "vague facts," "qualitative facts," indeed facts "entirely lacking in numerical precision" in the formulation of his law of acceleration.[38] Theory was no slave to experimental induction, and we would have to look elsewhere for the infrastructure of the "re-orientation" that had taken place between Aristotle to Galileo.

Over the next three Lowell lectures, Kuhn took on some of the great issues of "early" science: subtle fluids, physical fields, atomism, and dynamics. With a relentless and often gleeful Oedipal bashing of the received "empiricist methodology," Kuhn defended his "homicidal attack" on the Galileo fable and the related fables of Lavoisier and Dalton. Positively, Kuhn used the second half of his lecture series to probe the role that "preconceptions" played in shaping "creative" (not textbook) science. "Can any set of preconceptions prove fruitful? Is the creative scientist actually the man who most strongly displays his individuality of judgment by proceeding from preconceptions different from those of the majority of his profession? And if so what are the sources of these new prejudices? How complete is their domination of research; by what can they be altered?"[39]

Kuhn's responses to his set agenda developed many of the keywords for which *Structure* would come to be known, starting with the notion of a "crisis." An "orientation," as Kuhn characterized it, functions as a corral of "prejudices and preconceptions," is learned by training, and remains continuous over many years. Already in these 1951 lectures, the orientation is an amalgam of theory and experiment, an "inchoate" combination that can only be replaced by another. This (said Kuhn) is both an accurate historical description and a psychological and logical necessity. Building on the historical examples, Kuhn argued that we can now see the strongly fixed ideas embedded in an orientation can be obstacles to progress, for example, a hard commitment to atomism created an impediment to the development of heat theory or seventeenth-century chemistry. But "orientations" are more than frictional. Views about cosmology (Does the universe have center? Is it infinite or finite? Does the earth move?) open new possibilities for scientific thought. Orientations (or synonymously, "conceptual frameworks") shape similarity relations and fix acceptable analogies. Through such a determination of categories, the orientation shapes the very form of explanation—making it possible, for example, to see circular and linear motion as part of the same "thing." Orientations (returning to Kuhn's original lecture series title) are central to the creation of scientific objects.[40]

But over time, bit by bit, driven by theoretical or empirical difficulties, the old orientation accrues ever more ad hoc assumptions about instruments and objects. Eventually the field enters into a "crisis" stage in which everyone believes in the theory but the theory is so encumbered it begins to sag under its own weight. Eventually (here it is at last), a *scientific revolution* knocks the old orientation out in favor of a new one.[41] Crises could come from economic forces that change motivations, social forces, political sources, speculative philosophy, or changes in cosmology—but this would mean beginning a full-scale sociology of science—which is not where he wants to go here. Such an enterprise would be tying science to, for example, "extra-scientific climate of opinion." Quite deliberately, Kuhn set aside the "extra-scientific," restricting himself to changes in "professional orientation," to shifts of "points of view." (Indeed, on June 14, 1949, Kuhn wrote in his notebook, "Lewis Feuer, *Dialectical Materialism & Soviet Science*"—but not a word about it then or anywhere else in his notes.)[42]

Ending his fifth lecture, Kuhn promised to come back with a more precise "anatomy" of the orientation. To do that, he assigned a "homework problem" which he addressed, ad lib, from the blackboard. Imagine, Kuhn said (as best I can reconstruct his presentation), that you had a square array of alternating types of squares, missing the top right and lower left squares. Could you completely cover all squares by covering in each step two adjacent squares of different types? Kuhn promised his audience a "paradigm" that would be more effective if the problem were stated in advance.[43]

When Kuhn began his sixth lecture at 8 p.m. on Tuesday, March 20, 1951, he offered a restatement of the puzzle: picture the array as a checkerboard and the covering mechanism as a domino that fit over exactly two squares. Now since the opposite corners of a checkerboard are the same color (say, black) and the problem specified that two opposite corners were missing, there are thirty-two red but only thirty black squares. Since a domino must cover one black and one red square, it is immediately apparent that after laying out thirty dominos there will be no black and two red squares left uncovered. But there is no way to cover just two red squares with any number of dominos, so there is simply no way to cover all the squares with two-square dominos. By transforming the problem from something unfamiliar (the array and a rule for covering) to something familiar (a checkerboard and dominos) we experience a reorientation. Said Kuhn: "This puzzle can serve quite successfully as a paradigm of many of the effects of orientation which we have already observed." In particular, the now-obvious fact that the two-square coverings cannot completely cover the array could be put into a long, logical, ad rigorous "textbook form." But the underlying creative insight moved in its own way. The analogy continues: Kuhn pointed out that we could imagine more elaborate rules, extending the dominos to L-shaped blocks covering more than two colored squares. Indeed, we could create a whole new topic in mathematics out of such covering rules.[44]

Now, once a new orientation is in place (say, Galileo's understanding of instantaneous speed), the quantitative presentation of the law becomes imaginable, soluble. But the board-game instance is more or less as far as Kuhn got with the term "paradigm" in the Lowell lectures. The array-checkerboard problem occupies the role of an exemplar. But it is an exemplar of the general features that Kuhn wanted to point to in the process of "re-orientation." Over the next decade of course, paradigms take over (and develop further) the role that "orientation" or "points of view" played in 1951.

So much here reminds us of *The Structure of Scientific Revolutions* (crises, revolutions, Gestalt switches, allowable analogies, among others) that the differences could be dismissed, wrongly. For Kuhn, there were parallels—deep parallels—that persisted in the Lowell lectures between psychology of perception and child psychology on one side and science on the other. Kuhn noted that when a subject in a psychology experiment with anomalously marked cards (red spades, for example) sees them in passing, it precipitates a "crisis" of classification. When a Piagetian child-subject gets caught between two conflicting uses of the phrase "as far as," he too enters into a "crisis". Here, on the last page of his script for his sixth lecture, Kuhn handwrote in orange pencil "*SLOW*" (double underlined):

> It is because of parallels like this, parallels susceptible of a far more detailed development, that I suggest we equate the notion of scientific orientation with that of a behavioral world. And it is in part the psychological necessity of some behavioral world as a mediator and organize[r] of the totality of perceptual stimuli that I suggest we will never be able to eliminate from the scientific process orientations which originate in experience but which subsequently transcend it and legislate for it.

Kuhn's reformulation of "creative science" had gone far in these lectures—but he was operating entirely within a picture of the world in which a physical world is acted upon and reclassified by a psychological one. The effect may be dramatic on scientists, but it is not, for that, an argument for a multiplicity of worlds, an *ontological* shattering, as is indicated by the last words of the last lecture: "Continuing progress in research can be achieved only with successive linguistic and perceptual re-adaptations which radically and destructively alter the behavior worlds of professional scientists."[45] It is behavior worlds that are destroyed, not worlds full stop.

Applying for a Guggenheim grant for 1954–55, Kuhn reported (probably during the fall of 1953) on his still recent agreement with series editors Charles Morris, Rudolf Carnap, and Philipp Frank that he would write an essay for *The International Encyclopedia of Unified Science*. He would show the vastly more important role that theory plays in scientific development, and the correspondingly limited action of experiment. Theory would direct research, restrain the "creative imagination," restrict the problems deemed by the community "real" or "worthwhile," establish allowable models and metaphors, and dictate the "value judgments" that fix any experimental program.[46]

"Any major shift in the theoretical basis of a science must be *revolutionary* in the destructive as well as in the constructive sense." His future monograph, which Kuhn now titled *The Structure of Scientific Revolutions*, would be the "history of science" contribution to the encyclopedia "devoted to the role of established scientific theories as *ideologies* which direct experimentation and which lend special plausibility to certain sorts of interpretations of experiments. More precisely, I plan to begin by showing that, once established by professional consensus and once embodied in the texts and teaching programs by which a profession is perpetuated, scientific theories play a role far larger than their operationally admissible functions as records of nature's regularity."[47] Experiment demoted, theory promoted.

Over the course of the next six years, there was a slow but systematic swap-out of many of the psychological figures that had figured so large in Kuhn's formulation of his project. Jean Piaget, in my view *the* central figure in Kuhn's early work, the model for stable, coherent, conceptual structures broken by periods of acute disturbance, where meanings become unmoored? Vanished with barely a trace, other than a brief reference in the preface. Heinz Werner? Gone. Max Weber? Not a single reference. In their place appeared an entirely new cast of characters to carry the older tune. For the cognitive and social psychology of perception, there was the work of psychologists Jerome S. Bruner, Leo Postman, John Rodrigues, Harvey Carr, and Albert Hastorf, whose work on gestalts, expectation, memory and observation brought up to date the older work, though it dispensed altogether with the developmental analogy that had been so important for Kuhn.

Famously, Kuhn introduced Ludwig Wittgenstein's notion of family resemblance into his account, though only during the second half of the 1950s. This addition gave him a more philosophically-grounded way of proceeding from exemplary solutions (a signal function of paradigms) to problems solved by students and working scientists. "Conceptual schemes" and "ideologies" went the way of the carrier pigeon. Even "re-orientation," not long before the defining concept of Kuhn's whole project, entered *Structure* exactly once, and even there it deferred to "paradigm change." Kuhn wrote, "One perceptive historian [Herbert Butterfield], viewing a classic case of a science's reorientation by paradigm change, recently described it as picking up the other end of the stick . . . giving [the bundle of data] a different framework."[48] In all these ways, Kuhn moved, incompletely but noticeably, from the structural-developmental psychology of Piaget to a more third-person vision of crises, paradigms, normal science and revolutions.

Despite these shifts, much remained of Kuhn's original formulation—the supremacy of theory, subordination of experiment, and holistic transformations chief among the elements of continuity. This stress on what I have elsewhere called "block periodization" was already apparent to Paul Feyerabend in 1961, while the manuscript was still in rexographic form. "If I understand you correctly," Feyerabend wrote Kuhn, "the ideal is 'normal science' or pattern guided science (science guided by a *single* pattern which everybody accepts with the sole exception of some people you would perhaps be inclined to call cranks). But you never state clearly that this is your ideal . . . you insinuate that this is what *historical research* teaches you. . . . You *falsify* history just as Hegel falsified it in order to finally arrive at the Prussian State."[49] Elsewhere in the letter, Feyerabend hit the theme again: "Your hidden predilection for monism (for *one paradigm*) leads you to a false report of historical event." False, Feyerabend contended, because it ignored the multiplicity of forms of physical reasoning hidden within one of Kuhn's paradigms. "You regard as *one* paradigm (classical physics, for example) which is in fact a bundle of alternatives (contact action: Maxwell vs. action at a distance . . . reversibility . . . vs. irreversibility . . .)." These disputes and conflicts *within* the paradigm of "classical physics" undermined its homogeneity.[50]

Though very obviously not Feyerabend's picture of the ideal form of a more anarchic science of many forms, Kuhn's attachment to the single block went deep, his belief ever solid that outside a revolution the individual researcher was captive to the dominant paradigm—and revolution occurred like a tiny crack propagating, zig by zag, through a solid. Feyerabend wanted more than that, he wanted a willful, driving battle among contenders. "You," he wrote Kuhn, "allow for deviations which are brought about *unintentionally* (deviations, that is, from the original paradigm) whereas you frown up on the *explicit* development of alternatives. What is your *reason* for this position . . . [?] i.e. that alternative to the paradigm which are unintentional side effects . . . are to be welcomed whereas alternatives which are the result of an explicit effort to look for something different are not so good."[51] Feyerabend wanted a scientific street brawl, Kuhn a single Gestalt switch. "All existing philosophies of science," Feyerabend insisted, "(yours included!) are monistic in that they deal with what happens when one paradigm reigns supreme,*" with the asterisk leading to the rebuke: "*You only say that if there are more paradigms, then there will be a mess."[52]

Even Kuhn's invocation of a politically-laden notion of revolution came under fire. "Remember my reservations concerning your comparing *political* revolutions with scientific revolutions. The most fundamental revolution, to me, in the domain of knowledge, would be the transition from a stage of *dogmatism* to a stage where replacement of *any* paradigm is possible . . . Seems to me that political revolutions are more closely related to this fundamental revolution than to changes of paradigms about nature . . ." On this point, Kuhn and Feyerabend would never agree.[53]

Kuhn's achievement in those years from the brown notebook of mid-1949 to *The Structure of Scientific Revolutions* was remarkable. He used his experience as Van Vleck's student and in the Radar Countermeasures group to put scientific practice where logical reconstruction had been. He gave theory its due, not as an auxiliary codification of observations but as a directive, forceful part of what it meant to do physics, or science more generally. He showed the importance of the articulation of an established orientation-ideology-conceptual scheme-paradigm. And he allowed for the startling shock of "revolutionary" work with all the disruption and destruction—as well as production—that attended it.

But we can learn from the real physical, psychological, and philosophical practices that Kuhn had to work within the formative years of his picture of science. His physics—the physics of radar countermeasures and quantum magnetic susceptibilities—was in many ways a vestige of the 1930s. Here was small-scale, mostly individual work. With the quantum revolution now twenty years in the past, the techniques Kuhn needed were mostly available from textbooks like those of Whittaker and Watson, Kemble, Seitz, and Van Vleck. There was no real problem of calculation, no need for the new electronic computers, and no role for simulations carried out by hand or computer. Even Kuhn's war work was mostly individual—whether in Cambridge calculating required jamming power or in France sorting out the lines of communication and authority in the German radar defense.

Just as a generation of work has pried open physics practices, philosophical practices have their own history. Instead of seeing Kuhn's account as a successful or failed gloss of science or physics *in general*, we might do better to see it as a valiant and productive analysis of the physics of the 1930s done in the 1940s about the science of the seventeenth, eighteenth, and nineteenth centuries. Not here do we find the tools to analyze the 2,500 physicists who collaborated to produce the Higgs, nor of wartime Los Alamos, Lawrence Berkeley Laboratory, or CERN. Not in monolithic paradigms and not in the study of textbooks does one find the resources to grapple with the hybrid of algebraic geometry and quantum field theory that forms string theory so present in the last decades of the twentieth century or the first several of the twenty-first. Not in the physics of the late 1930s are analytic or historical tools needed to understand the cross-breed research that is part start-up, part bioprospecting, part global pharma and part biochemistry. Nor is the world of solid state quantum approximations the right place to look at the massively parallel computing power set to work on simulations of galaxy collisions, thermonuclear weapons, or quark plasmas.

Other tools are needed for these other jobs—and we need other means to analyze a world where textbooks and preprints have utterly vanished, giving way to the ArXiv; where the boundaries between disciplines make the biological physical sciences harder to distinguish; where mathematics and physics, astrophysics and particle physics are in constant, morphing changes. Physics results circulate in new ways, learning is increasingly decentralized, asynchronous, hacked, monetized, and distributed.

And yet: If Kuhn teaches us something about a kind of attentiveness to a theoretically-inflected analysis of the conduct of science; if his works show us the virtue of focusing hard on scientific practices; if it inspires a practice-based analysis of the philosophy of science itself—well, that would be a great and good thing and a legacy worth guarding.

Notes

1. The American physicist Raymond Birge called the European quantum physicists "atomic structure sharks." See the excellent article, Alexi Assmus, "The Americanization of Molecular Physics," *Historical Studies in the Physical and Biological Sciences* 23, no. 1 (1992): 1–34 (quote, 8).

2. John H. Van Vleck, interview by Thomas Kuhn, October 2, 1963, Harvard University, Cambridge, MA. American Institute of Physics website, accessed February 8, 2014, http://www.aip.org/history/ohilist/4930_1.html; John Van Vleck, "Quantum Principles and Line Spectra," *Bulletin of the National Research Council* 10, no. 54 (1926).
3. Ibid. There were, Van Vleck recalled, only a few, mostly younger physicists who were even following the new work:

> Right after the discovery of quantum mechanics, I can remember one prominent mathematical physicist who claimed all the matrix elements were zero. He had a little of an informal public listening to him in the hall. But he had not taken into account the fact that the square of minus 1 need not have the same sign in two different equations, which I pointed out to him. I think you can say that, by and large, only the younger physicists in this country were ever able to get quantum mechanics in their bones. There were exceptions, but, by and large, I think, this was true.

4. Van Vleck, interview by Thomas Kuhn.
5. "As it was, I think Mensing and Pauli beat me to it on being the first to publish that factor one-third. It was essentially a triple tie, though Kronig had it too, all three of us" (ibid.).
6. Ibid.
7. T. S. Kuhn, War Reports, [1943], box 1, folder 8, Thomas Kuhn Papers, MC240, MIT Archives [hereafter, TSK Papers], Cambridge, MA.
8. Kuhn to Commanding General, 9th Bombardment Division, "Desirability of a Carpet Program for Daylight Operations of Medium and Light Bombers," February 7, 1945, box 1, folder 19, TSK Papers.
9. Kuhn, War Notebook, August 27, 1944, box 1, folder 10, TSK Papers.
10. Kuhn, War Notebook, August 29, 1944, box 1, folder 10, TSK Papers. At the end of January 1945, the American British Laboratory (ABL-15) requested that Kuhn, now versed in intelligence aspects of radar countermeasures, be assigned to a liaison position, mediating between the ABL and the 9th U.S. Tactical Air Force.
11. Aristides Baltas, Kostas Gavroglu, and Vassiliki Kindi, "A Discussion with Thomas S. Kuhn," in Kuhn, *The Road Since Structure: Philosophical Essays, 1970–1993*, ed. James Conant and John Haugeland (Chicago: University of Chicago Press, 2000), 274.
12. E. T. Whittaker and G. N. Watson, *A Course of Modern Analysis*, 4th ed. (Cambridge: Cambridge University Press, 1927).
13. T. S. Kuhn, "An Application of the W.K.B. Method to the Cohesive Energy of Monovalent Metals," *Physical Review* 79 (August 1950): 515–19; T. S. Kuhn and J. H. Van Vleck, "A Simplified Method of Computing the Cohesive Energies of Monovalent Metals," *Physical Review* 79 (July 1950): 382–88; and T. S. Kuhn, "A Convenient General Solution of the Confluent Hypergeometric Equation, Analytic and Numerical Development," *Quarterly of Applied Mathematics* 9 (1951): 1–16.
14. Kuhn, Notebook, "Reading to 3–31–49," box 1, folder 7, p. 1, TSK Papers.
15. Kuhn, Notebook, March 29, 1949, box 1, folder 7, p. 3, TSK Papers.
16. On the discussion of language, see ibid., 8. Jean Piaget, *Les Notions de Movement et de Vitesse Chez l'Enfant* (Paris: Presses Universitaires de France, 1946).
17. Kuhn, Notebook, June 14, 1949, box 1, folder 7, pp. 10–12, TSK Papers.
18. Ibid., 14–15.
19. Ibid.
20. Ibid., 18–19.

21. Ibid., 15–16.
22. Ibid., 16.
23. Kuhn, Notebook, June 15, 1949, box 1, folder 7, p. 21, TSK Papers.
24. Ibid., 22.
25. Ibid., 20–21. Kuhn used Heinz Werner's *The Comparative Psychology of Mental Development* (Chicago: Follett Pub. Co., 1948), to bolster his Piagetian conclusions—"synaesthesia shows a general form which psychology can influence perception, from which one can derive primitive notions of "psych[ological] compatibility.' So does 'physiognomic perception'" (Kuhn, Notebook, June 15, 1949, box 1, folder 7, pp. 22–23). Kuhn then notes that Werner's first point, on page 128 of *Comparative Psychology*, "is reminiscent of Aristotle's incommensurability of various types of motion" (ibid., 24).
26. Max Weber, *The Methodology of the Social Sciences*, ed. and trans. Edward Shils and Henry A. Finch (New York: The Free Press, 1949), 80 (original emphasis).
27. Kuhn, Notebook, June 15, 1949, box 1, folder 7, p. 24, TSK Papers.
28. Kuhn, Notebook, June 21, 1949, box 1, folder 7, p. 25, TSK Papers.
29. Kuhn, Notebook, July 6, 1949, box 1, folder 7, p. 35, TSK Papers.
30. Kuhn, incomplete response to Sángór Radó, "Incomplete Memos & Ideas," 1949, box 1, folder 6, p. 2, TSK Papers (original emphasis).
31. Ibid., 3–4.
32. Ibid., 6. Kuhn went on to write a fragment called "How Questions, Why Questions, and the Role of Experiment in Physical Science," saying, to be sure, the separation is in textbooks. But: "the prospective worker in a field pretending to the status of science is advised to examine his subject without preconception of embryonic theory, to classify or describe, and only then to generalize. Such a prescription has all the affective values of any appeal for objectivity, but its validity as description and utility as norm may be few or perhaps questioned, since no creative scientific generalizations have been achieved in this manner" (Kuhn, "How Questions, Why Questions, and the Role of Experiment in Physical Sciences," 1949, box 1, folder 6, p. 3, TSK Papers).
33. Kuhn to David Owen, chair, General Education Committee, Harvard University, January 6, 1951, box 5, folder 84, TSK Papers.
34. Ibid.
35. Ibid.
36. The initial invitation to give the lectures is Ralph Lowell to Kuhn, March 8, 1950, box 3, folder 10, TSK Papers. Kuhn responds, accepting, to Ralph Lowell, March 19, 1950, box 3, folder 10, TSK Papers, which includes the quotation given here and the preliminary title. His final title: "The Quest for Physical Theory. Problems in the Methodology of Scientific Research," is contained in Kuhn to William H. Lawrence, April 13, 1950, box 3, folder 10, TSK Papers.
37. The eight unpublished lectures (in hand-corrected typescript form) are located in box 3, folder 11, TSK Papers. "Two reasons to study early science," lecture 1, pp. 7–8; "Two meanings of science," lecture 1, p. 5.
38. Lecture 1, box 3, folder 11, p. 13, TSK Papers; discussion of Galileo, lecture 1, box 3, folder 11, pp. 13–19, TSK Papers. Kuhn cites Koyré explicitly in his manuscript, for example, at lecture 1, p. 20 (Koyré III-23, specifically on Galileo's account that his theoretical considerations should push opponents into confessing the truth of Galileo's view more than experiment). See generally, Alexander Koyré, *Études Galiléennes* (Paris: Hermann, 1939).

39. "Homicidal," lecture 1, box 3, folder 11, p. 21, TSK Papers; Kuhn on "preconceptions," lecture 1, box 3, folder 11, pp. 26ff, TSK Papers. In a later version Kuhn softened this attack and removed the phrase "homicidal attack."
40. For Kuhn's discussion of "Orientations" and "conceptual frameworks" in the Lowell lectures, see lecture 5, box 3, folder 11, , p. 13, TSK Papers; On facts and theories enter together in inchoate form given by orientation, lecture 5, box 3, folder 11, p. 16, TSK Papers [hereafter referring to Kuhn's red pencil paginations]. On orientations being both descriptive and prescriptive: "science has in fact progressed by a series of circular attempts to apply differing orientations or points of view to the natural world. I state this here as a matter of fact, but I think it is also a matter of [psychological and logical] necessity," lecture 5, box 3, folder 11, p. 17, TSK Papers. Kuhn includes a bracketed reference to Emil Myerson as "precursor" to these views about taxonomy and classification, lecture 5, box 3, folder 11, p. 6, TSK Papers; Kuhn includes more on similarity relations, e.g. in lecture 5, box 3, folder 11, p. 13, TSK Papers.
41. On the "crisis stage" of science, see lecture 5, box 3, folder 11, p. 26, TSK Papers; Kuhn comments that only in retrospect are revolutions additive, lecture 5, box 3, folder 11, p. 32, TSK Papers.
42. Kuhn, lecture 5, box 3, folder 11, pp. 43–44, TSK Papers. Kuhn, Notebook, notation on Feuer, June 14, 1949, box 1, folder 7, p. 18, TSK Papers. For a reading of Kuhn and depoliticization, see S. Fuller, *Thomas Kuhn: A Philosophical History for Our Times* (Chicago: University of Chicago Press, 2000); George Reisch, "Anticommunism, the Unity of Science Movement and Kuhn's Structure of Scientific Revolutions," *Social Epistemology* 17 (2003): 271–75.
43. Kuhn, lecture 5, box 3, folder 11, p. 45, TSK Papers.
44. Kuhn, lecture 6, box 3, folder 11, pp. 1–3, TSK Papers.
45. Kuhn, lecture 6, box 3, folder 11pp. 43–45; lecture 8, box 3, folder 11p. 38, TSK Papers.
46. Kuhn, application for Guggenheim for academic year 1954–55, box 5, folder 84, TSK Papers.
47. Ibid., 2 (*revolutionary*, emphasis added; *ideologies*; original emphasis).
48. Citation is to Herbert Butterfield, *The Origins of Modern Science, 1300–1800* (London: G. Bell, 1949), 1–7, iquoted n Kuhn, *Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1970), 85.
49. The letters from Paul Feyerabend to Kuhn are reproduced in Paul Hoyningen-Heune, "*Two Letters of Paul Feyerabend to Thomas S. Kuhn on a Draft of* The Structure of Scientific Revolutions," *Studies in History and Philosophy of Science* 26 (September 1995): 353–87 (quote, 360; original emphasis).
50. Ibid., 367.
51. Ibid., 372 (original emphasis).
52. Ibid., 375.
53. Ibid., 373 (original emphasis).