CHAPTER 36

QUANTUM HISTORIOGRAPHY AND CULTURAL HISTORY

Revisiting the Forman Thesis

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THE historiography of science and technology knows several bifurcation points, when the introduction of a radically novel type of argument occurred at just the right moment to touch a sensitive nerve, spark a fundamental, often prolonged controversy, and irreversibly change the direction of the field at large. In retrospect, the list of such landmark intellectual breakthroughs would have to include Hessen's 'social and economic roots' of 1931, the Merton thesis of 1937, Koyré's *Études Galiléennes* of 1939, Kuhn's *Structure* of 1962, the Forman Thesis of 1971, Shapin and Schaffer's *Leviathan and the Air-Pump* of 1986, Haraway's *Primate Visions* of 1989, and possibly a couple of other programmatic texts. As with the rest of the list, the Forman Thesis's methodological influences stretch far beyond its original focus, but it did emerge out of the history of quantum physics and needs to be understood from its actual roots.

This chapter draws in parts from the earlier published, co-authored introduction (Carson, Kojevnikov, and Trischler, 2011). Its first section describes the founding period of quantum historiography during the 1960s. The second summarizes the main ideas of the Forman Thesis and the third examines the controversy it inspired and its subsequent influence. The fourth section discusses several key examples—some well-known, others from the more recent literature—that help test and establish the boundaries of Forman's approach, and that leads to the conclusions about its current status.

36.1 Early Quantum Historiography: Intellectual and Disciplinary History

While still in its formative stage during the 1960s, the professional historiography of the quantum revolution stood then at the forefront of methodological innovations in the history of science writ large. As he reviewed the discipline's major trends, Thomas Kuhn identified a shift of attention away from the ancient and old classics and towards the history of recent, i.e., 20th-century science (Kuhn, 1967). An encouragement for this move came from scientists themselves, especially physicists, who were then at the height of their Cold War power, prestige, and financial largesse. No less importantly, they could also boast a very recent fundamental revolution—relativity and quantum—conceptually as profound and awe-inspiring as any of the greatest scientific achievements of the past. Quantum mechanics by 1960 was considered complete, accomplished, and interpreted with the reigning Copenhagen philosophy. Some of the main heroes of this revolution were still alive and able to share their stories, but the deaths of Albert Einstein (1955), John von Neumann (1957), Wolfgang Pauli (1958), and Erwin Schrödinger (1961) raised the alarm among the physics community and prompted calls to record the history of the passing giants.

Responding to a physicists' initiative, with support from the American Institute of Physics and the American Philosophical Society, a team of first-generation professional historians of physics led by Kuhn used this unprecedented opportunity to create a treasure-trove of primary sources for the history of quantum physics. The US National Science Foundation granted funding for a three-year project, which enabled its almost 'big science' dimensions, by history's disciplinary standards. While empowering historians—most of whom at the time had an educational background and sometimes also research experience as scientists—this support also exposed them to the challenge of whether they could emancipate themselves, professionally and intellectually, from the authority of the scientists they studied. Physicists developed and cherished their own historical mythology, telling and retelling it on numerous celebratory and educational occasions. Quantum mechanics, too, had produced a rich quasihistorical narrative and a canon of autobiographical memoirs, which historians inevitably came to contradict and correct in multiple ways as they started careful investigations with primary documents. In his 1967 review, Kuhn took critical aim at one such immensely influential text, George Gamow's *Thirty Years that Shook Physics: The Story of Quantum Theory* (1966), as an example of unreliable popular history dominated by participants' own accounts, anecdotal memories, and disciplinary myths.

The ambitious project of the *Archive for the History of Quantum Physics* (AHQP) was not an archive in the usual sense, but a major pioneering undertaking in oral history within the field of history of science.[1] Over the course of three years, 1961–1964, Kuhn, together with graduate students John L. Heilbron and Paul Forman, and with bibliographical assistance from Lini Allen, recorded detailed and lengthy interviews with over a hundred surviving scientists who made important contributions to quantum physics and chemistry between approximately 1900 and 1935, among them Niels Bohr, Max Born, P. A. M. Dirac, and Werner Heisenberg. In conjunction with those interviews, the team sought to locate, organize, catalogue, and microfilm relevant primary documents and correspondence held by the interviewees, as well as libraries and archives. The interview transcripts and the microfilms with 'about 100,000 frames of material' of letters and manuscripts were deposited in the library of the American Philosophical Society and several other (currently more than twenty) convenient locations, thus bringing the sources much closer to potential researchers (Kuhn *et al.*, 1967; Heilbron, 1968, p. 98). The resulting primary source collection established the empirical foundation for practically every historian working in the field since, including AHQP team members' own research into the history of quantum ideas.

The quantum revolution in physics was a large collective enterprise, but not exactly a coordinated effort. No other great scientific innovation of the period, including relativity theory, had so many crucial and chronologically overlapping contributions from dozens of prominent authors with often conflicting agendas, preferences, and aspirations. The fast reproduction rate of journal publications made it possible for a submitted paper to be published sometimes within two to three months, and about one month later already be cited in another paper submitted for publication by a different author. This explosive pattern of knowledge production also profited from close personal contacts, global geographical spread, and an unusual mobility of converts, rich correspondence networks, and informal exchanges of proof sheets of as-yet unpublished articles (Kojevnikov, 2020). Unlike a typical 19th-century model of discipline formation, no single major centre or institution of graduate training could accommodate this large community of researchers. Its members often pushed the work in diverging, sometimes contradictory directions, so that no individual leader could stay effectively in charge or claim ultimate credit for the enterprise. In the 1920s, they also produced scientific and philosophical infighting of such intensity and inconsistency of competing views that was almost unprecedented in the history of science.

Such enormous density of recorded details, thoughts, and arguments inspired a hope for a much more invasive history of ideas that would reconstruct and uncover the ways of scientific creativity—a historical strategy commensurable with Kuhn's *Structure of Scientific Revolutions* (conveniently published in 1962). Oral histories, in the long run,

[1] During its execution, the formal name of the project, at least the part of it funded by the NSF and its published report, was *Sources for the History of Quantum Physics*. The resulting collection of oral histories and microfilms is called officially the *Archive for the History of Quantum Physics*, the name under which the entire undertaking has become more commonly known during the subsequent decades. Heilbron (1968) provides a critical review of the early period of quantum historiography before the field's major expansion in the late 1960s and a detailed overview of the AHQP project. For a recent historiographic analysis, especially with regard to oral history, see (te Heesen, 2020).

proved a mixed blessing: while rich in personal and otherwise unavailable details, they were also partially unreliable and self-serving, because long-term memories of participants adapted to post hoc rationalizations and often contradicted many of the often-confused thoughts recorded in primary archival sources. But thousands of extant letters and manuscripts formed the basis of many detailed historical investigations, in particular in the flagship journal *Historical Studies in the Physical Sciences* (*HSPS*, 1969–1979), and allowed historians to question and challenge many historical myths and physicists' disciplinary folklore.[2] For example, Kuhn's *Black-Body Theory and the Quantum Discontinuity, 1894–1912* (1978) contradicted the cult of the founding father in quantum theory that traditionally attributed the introduction of fundamental discontinuity to Max Planck in 1900. According to Kuhn's unceremonious analysis, Planck accepted the conclusion that elementary quanta were discontinuous only reluctantly and noticeably later than other physicists, in particular Einstein and Paul Ehrenfest. The riches of available sources and the emerging power of historians to correct the scientists' disciplinary beliefs also supported the programme of 'rational reconstruction of scientific creativity', attempts to 'follow the thinking' as painstakingly as possible and recapitulate discoveries *in statu nascendi* in scientists' heads. This exaggerated hubris of the history of modern physics in its puberty, during the process of professional and intellectual emancipation from the parental authority of scientists, subsequently came under a harsh critical reassessment in (Forman, 1991).

As explained by Heilbron (2011) in his recollections about that *Sturm und Drang* era, the American present—the politically and socially turbulent 1960s—interfered with thinking about the past and prompted historians of physics to ask questions beyond the traditional repertoire of the history of ideas. The Vietnam War and the Cold War made scientists not only objects of veneration, as before, but also of criticism and suspicions, as the younger generation of Americans focused their attention on physicists' roles as weapon-makers and lobbyists. No longer seen merely as champions of ideal truth, science and its spokesmen increasingly appeared as servants and agents of the ruling political establishment and militarism. Meanwhile, the accelerated world-wide growth of R&D and higher education in the wake of the shocking launch of Sputnik by the Soviet Union, and the resulting transformation of science into a mass profession with supersized infrastructure and budgets, shifted scholarly interests towards investigating the larger scientific community, its structures, institutions, jobs, social relations, patrons, and the sources of funding, with the 'follow the money' method. These notions formed the conceptual vocabulary of the disciplinary-institutional approach to history of science, promoted in *HSPS* under the editorship of Russell McCormmach.

While the AHQP project focused primarily on individuals, the subsequent team effort by Forman, Heilbron, and Spencer Weart produced a survey of the international physics community *circa* 1900 that used a wealth of statistical data to evaluate the

[2] (Heilbron and Kuhn, 1969; Forman, 1969), and many other examples.

entire discipline, its demographics, institutions, positions, and social practices (Forman *et al.*, 1975). Forman's own Ph.D. thesis (1967) analysed the finances, structure, and modes of operation of the German-speaking physics community during the difficult political, economic, and social situation following the Central Powers' defeat in World War I. Forman's dissertation remained unpublished (unjustifiably so), but it influenced the approaches in many a later investigation by other historians in this new area of research that it opened up. Its historical analysis described the inner workings of the academic community and the social background in which the ideas of quantum mechanics brewed. The dissertation and two later articles–one on the post-war international boycott of German science and the other on its attempts to overcome international isolation and secure funding and political alliances (Forman, 1973, 1974)—together, served as the foundation for the seminal Forman Thesis.

36.2 The Forman Thesis: Social and Cultural History

What is usually referred to as the Forman Thesis is, more precisely, an argument, a logical sequence of theses that combine the approaches from social and cultural history to the developments in quantum physics following World War I.[3] It started with Forman's doctoral dissertation of 1967 and continued with a half-dozen articles, both preceding and following the most famous one of 1971—'Weimar Culture, Causality, and Quantum Theory, 1918–1927: Adaptation by German Physicists and Mathematicians to a Hostile Intellectual Environment'—so long, thorough, rich, and fundamental, that it effectively works as a monograph, even if published by a journal.

Forman described the intellectual climate in the economically and psychologically traumatized German cultural space after the defeat in World War I, along with the loss of the Empire, its colonies and territories, its military might and industrial prosperity. The general perception of overwhelming crisis—political, economic, and social—affected all aspects of life, including science. Subjected to harsh treatment by the victorious allies following the Versailles Treaty, the German Empire lost its sense of superiority, self-confidence, and global importance. The internationally isolated and frequently humiliated government of the Weimar Republic was also internally unstable and weak, threatened by revolutions and putsches, from both the radical right and the radical left. The German economy suffered several major blows, especially the 1922/23 hyperinflation and the 1929 stock collapse. The situation in former Austria-Hungary

[3] In addition to the Forman Thesis discussed here, Silvan S. Schweber also defined the 'second Forman thesis' on the symbiotic relationship between military funding agencies and the character and directions of physics research in Cold War America, analysed in Forman's papers of the 1980s, and the 'third Forman thesis', on the post-modern relationship between science and technology, studied by Forman in the 1990s (Schweber, 2014, p. 180).

was even worse, by a significant margin, as the once mighty empire split into a half-dozen small nations, each much more vulnerable than Germany to post-war political and economic insecurity. In many of these territories, the war did not end in 1918, but devolved into the continuing violence of ethnic and civil wars between various nationalistic, proto-fascist, and revolutionary forces (Gerwarth, 2016).

The German *Bürger* who lost their bank savings and prosperity—the middling classes including their intellectual subsection, the academics—felt the contrast between pre-war confidence and post-war troubles especially painfully. Their sense of a general crisis translated into, on the one hand, nostalgic feelings about the imperial past and political alienation from the left-leaning Weimar government, and on the other, disillusionment and disenchantment with many of the key ideological values of the pre-war era, such as belief in progress, positivism, modernity, and rationality. The new postwar *Zeitgeist* promoted instead a much more conservative, romantic, and pessimistic outlook and blamed the disaster from the war and the country's misfortunes on the earlier infatuation with shallow materialism and technological optimism. The fashionable philosophical and ideological treatises, including *Lebensphilosophie*, Nietzsche, and Oswald Spengler's *Decline of the West*, promoted irrational, organicist, intuitive, and anti-materialist lines of thought. In a cultural environment hostile towards the values associated with the exact sciences, physics came under severe critiques as too rational, abstract, mechanistic, and causal: 'I show that in the aftermath of Germany's defeat the dominant intellectual tendency in the Weimar academic world was a neo-romantic, existentialist "philosophy of life", revelling in crises and characterized by antagonism toward analytic rationality generally and toward the exact sciences and their technical applications particularly. Implicitly or explicitly, the scientist was the whipping boy of the incessant exhortations to spiritual renewal, while the concept—or the mere word—'causality' symbolized all that was odious in the scientific enterprise', explained Forman.

He then immediately drew attention to a 'remarkable [historical] paradox: this place and period of deep hostility to physics and mathematics was also one of the most creative in the entire history of these enterprises . . . I had myself previously assumed that in the face of antiscientific currents the predominant response in these highly professional sciences would be retrenchment . . . and reaffirmation of the discipline's traditional ideology . . . Yet the historian who takes even the most casual notice of the valuations of physical sciences in contemporary American society . . . [is] witnessing . . . a widespread and far-reaching accommodation of scientific ideology to a hostile intellectual environment' (Forman, 1971, pp. 4–5). In the similarly inhospitable intellectual climate of Weimar Germany, Forman observed, many academics started wavering in their attachment to rationalist values that had heretofore been central to the business of science as such.

Economically, as a profession, science was hit earlier and particularly hard in the former Habsburg lands, to the point that in the 1920s even in the capital Vienna, the famous Institute for Radium Research resorted to hiring women as regular research staff, since it could not afford to pay liveable salaries to male scientists (Rentetzi, 2004). Many Austrian and Hungarian scholars, including Erwin Schrödinger and Wolfgang

Pauli, moved to Germany, where the inflation and general economic troubles did not damage research nearly as severely. In fact, German science retained a remarkable vitality: its spokesmen and the Weimar government often saw and used it as *Macht-Ersatz*, the country's one remaining strength, a substitute for power that had been lost in most other domains: political, military, diplomatic, and economic. Under the attempts by the victorious powers to boycott and isolate it, German science was banned from many international meetings, conferences, and exchanges, and its global institutional dominance decreased. It also lost many foreign students, but redirected its main international connections and cultural imperialism to countries that had remained neutral during the war and to Soviet Russia and Japan (Kevles, 1971; Forman, 1973). Meanwhile, in its home base, research and publishing continued as actively as before, even if a prohibitive exchange rate prevented subscriptions to foreign publications and undermined opportunities for international travel. Scientific infrastructure—institutes and laboratories, built and equipped during the imperial period before the war—were still far better and richer than anywhere else in Europe. The government bureaucracies, at least in Prussia, continued to value and support science materially throughout the difficult times: professors and other salaried academics maintained liveable incomes adjusted for inflation, while grants from the newly created emergency fund *Notgemeinschaft der Deutschen Wissenschaft* partially compensated for losses in research support (Forman, 1974). Yet the erosion of prestige, security, and of their previously high social status made the majority of German academics, with a few notable exceptions such as Einstein, significantly more right-wing than their post-war governments and nostalgic about the pre-war *Kaiserreich*.

In a period when revolutions, military coups, and crises threatened German society, science and its individual disciplines were also often declared to be in a state of deep crisis and ripe for radical conceptual changes. The widespread discourse about the 'crisis of science' not merely acknowledged the economic difficulties of the profession or a disciplinary 'crisis' as in Kuhn's model of scientific revolutions, but was understood in a much more general and profound sense. Post-war doubts not only encouraged and made it easier for scientists to question the conceptual foundations of the existing knowledge; they also undermined general values that heretofore had been associated with the very essence of the exact sciences. In particular, mechanical determinism, or the principle of causality, came under severe criticism as too rationalistic and, indeed, mechanical. 'In the vocabulary of *Lebensphilosophie* there were two characteristic words: one—*Anschaulichkeit*, [visual] intuitiveness—had strongly positive connotations; the other—*Kausalität*, causality—was emphatically pejorative. And the epitome of the abstract, unintuitive, and causal mode of apprehending reality was that of the theoretical physicist', observed Forman (1979, p. 13).[4]

[4] Some participants in the debates proposed to draw a distinction between determinism, a more rigid, mechanical, 18th-century concept, and causality as a general, philosophical, and potentially more inclusive and amendable principle. For the purposes of this essay, these two notions will be considered roughly synonymous.

University administrators and ministry officials wanted 'to do new things', and even some older professors felt the need for new agendas and to hire representatives of the new physics (Born, 1978, p. 200). By the latter they meant first and foremost research in the atomic and quantum domains, both theoretical and experimental. Physicists who represented these new lines of research became much more willing, in comparison with more stable times, to revise or entirely abandon the fundamental principles and foundational concepts of classical physics. They also often made more than just rhetorical concessions to the fashionable philosophical critiques of the time and to the hostile intellectual environment, which they faced, most directly, as academics in the public eye at their own universities. Typically, this occurred when an exact scientist delivered a public address, a common genre in the German academic world. In these *Reden*, scholars explained and commented on the developments in their particular discipline to a gathering of university colleagues from all fields or to academically educated general audiences, in language and terms (mostly philosophical) that would be comprehensible to non-specialists. By using these sources, Forman discovered that several prominent physicists and mathematicians declared their readiness to abandon or restrict the principle of causality in physics even before the invention of quantum mechanics itself, i.e., several years prior to 1925. Once that revolutionary theory appeared, acausality was quickly ascribed to it and proclaimed the fundamental scientific principle of the new quantum mechanics of atoms and electrons.

According to Forman's analysis, outside pressure thus contributed to both ingenuity and opportunism of Weimar scientists:

> I am convinced . . . that the movement to dispense with causality in physics, which sprang up so suddenly and blossomed so luxuriantly in Germany after 1918, was primarily an effort by German physicists to adapt the content of their science to the values of their intellectual environment. The explanation of the creativity of this place and period must therefore be sought, in part at least, in the very hostility of the Weimar intellectual milieu. The readiness, the anxiousness of the German physicists to reconstruct the foundations of their science is thus to be construed as a reaction to their negative prestige. Moreover the nature of that reconstruction was itself virtually dictated by the general intellectual environment: if the physicist were to improve his public image he had first and foremost to dispense with causality, with rigorous determinism, that most universally abhorred feature of the physical world picture. And this, of course, turned out to be precisely what was required for the solution of those problems in atomic physics which were then at the focus of the physicists' interest. (Forman. 1971, pp. 7–8)

In two subsequent papers, Forman further developed and extended his original thesis. The first one looked at the reception of quantum mechanics outside of the German cultural sphere, in particular in Great Britain, where hostility towards the values of science and, correspondingly, commitment to acausality were much less pronounced at the time, and in America, where the issue of (in)determinism was largely treated with indifference (Forman, 1979). The other paper took the argument beyond the question of causality by analysing two additional and important culturally sensitive notions of the Weimar milieu: *Anschaulichkeit* (the word combining the meanings of 'visualizable' and 'intuitively grasped', depending on the context) and *Individualität* (or individuality). Forman drew the distinguishing line between the actual character of quantum physics and some philosophical features frequently ascribed to it as accommodation to the popular value-laden concepts of the time. Thus, despite the highly abstract and counterintuitive nature of the quantum description, physicists often presented its results with the label *Anschaulich*, or intuitive. Quantum mechanics' formalism abandoned the absolute individuality and distinguishability of elementary particles (Monaldi, 2009), but was publicly proclaimed to represent the opposite, their 'indestructible individuality' in the world of atoms, sometimes with an analogy to individuality in the organic world. The laws of the new theory were probabilistic and statistical, but its authors were more than willing to make much stronger ontological claims. Max Born in 1926 declared himself 'inclined to abandon determinedness in the atomic world', and a few months later Werner Heisenberg proclaimed categorically that 'quantum mechanics established definitively that the law of causality is not valid' (Forman, 1984, p. 336).

The cultural values that appealed to the predominantly conservative, anti-rationalist intellectual milieu within which the German physicists operated thus became written into the prevailing philosophical interpretation of quantum theory. 'My conclusion is that there was little connection between quantum mechanics and the philosophic constructions placed on it, or the world-view implications drawn from it. The physicists allowed themselves ... to make the theory out to be whatever they wanted it to be—better, whatever their cultural milieu obliged them to want it to be. This conclusion is admittedly radical. But it does not touch the question of the social construction of reality so directly as one might at first be inclined to suppose. It is neither a statement about the physicists' practice in their laboratories nor about the physicists' theories as descriptions of reality. It is rather a meta-meta statement, a statement about the physicists' statements about their description of reality', summarized Forman (1984, pp. 342–343).

36.3 Impact and Controversies

The shock and uproar created by the Forman Thesis at the time of its introduction were practically guaranteed, as his landmark study explicitly contradicted then generally accepted and cherished beliefs about science. It put forward and placed in the centre of a broader discussion the argument that culture and cultural values prevalent in a given place and time condition the results of scientific research, i.e., the very content of scientific knowledge. Heilbron, whom Forman had asked to deliver the first public presentation of his 'thesis' at the Christmas 1970 meeting of the History of Science Society in Chicago, described the reaction as a 'maelstrom'. As had been anticipated.

After all, acausality at the time was generally accepted as the very core, fundamental concept of quantum mechanics, and to ascribe this glorious scientific discovery to the influence of reactionary Spenglerian philosophy seemed like a blasphemous offence against the truthfulness of physics and the purity of its spirit. For Forman, of course, the idea that famous physicists could come under the corrupting spell of a hostile public environment and reactionary ideology was not a thoughtcrime, but a lamentable reality of the Cold War (Heilbron, 2011).[5]

For many of the outraged, Forman's study represented another incarnation of the abhorrent 'externalist' approach to scientific content. Indeed, there had probably been only one even more influential and more controversial article ever published in the history of science, the 1931 analysis of classical mechanics in Boris Hessen's 'The Social and Economic Roots of Newton's *Principia*'. The two classic works did have something in common: they both enormously upset, each in its own ways, the essentially Platonic ideology of science as a pure intellectual activity, a noble search for abstract truth, supposedly in control of its intrinsic scientific method and of the criteria of true knowledge. Instead, both approached science as an essentially human, and thus also earthly, social and cultural activity, and accepted the necessary epistemological consequences of such an assumption. (Freudenthal and McLaughlin, 2009).

Yet the differences between these two papers, separated by forty years, were no less important than their similarities. Hessen developed a deliberately Marxist argument that proclaimed the influence of the economic and technological basis of the time period upon its scientific superstructure. In Forman's analysis, culture played a key role, mediating and channelling the impact of economic and social conditions. Hessen, writing at the time of the revolutionary industrialization of the Soviet Union, promoted an unabatedly progressivist view on science, without reservation counting it among the major forces of social and political progress. For Forman, in the era of DDT, napalm, and Agent Orange, the question of science's and scientists' political associations became less optimistic and more ambivalent. His study found some leading proponents of the quantum revolution entering a pact with anti-rationalist conservative ideological currents, whereas those physicists who upheld the values of causality and reason and

[5] I am grateful to John L. Heilbron for his letter of 21 January 2021, with a description of the meeting and the following clarifications: 'The reason Paul did not attend the Chicago meeting was that he had had an operation from which he did not feel sufficiently recovered to face what he called the "maelstrom". But he had wanted to attend and regretted the loss of conversations he had anticipated with other quantum historians. The paper as delivered was a condensed version of the third part of Paul's long Weimar article in *HSPS*. In sending it to me, he wrote that he regarded it "as tending to show that the philosophers of science were more right than we have allowed—more right about the necessity for a conventionalized, stylized, 'idealized', picture of science as the basis for a history of science." (Letter of 22 Nov) 'Maelstrom' was his word. I do not think that the reaction to the paper was as negative or noisy as your text may suggest. There were friendly people in attendance: Russ McCormmach, Stan Goldberg (I think), myself, and I do not remember how many others; Erwin Hiebert was the chairman (Paul did not think he would be favorable!) and Gerry Holton the commentator. As usual with him, Holton did not write out his comments; but you might be able to reconstruct his and Hiebert's reactions from their publications of the period.'

often adhered to more progressive politics, were nevertheless rhetorically dismissed at the time as scientifically 'conservative'. Last but not least, Hessen's essay was largely declarative and programmatic. It inspired and required further empirical justification, including Robert Merton's *Science, Technology, and Society in 17th century England* (1938). Forman's 'Weimar Culture' relied on an enormous body of primary sources, many heretofore unused, and came out of a vast empirical—archival and historical—project.

The main ideological and methodological conflict of the Cold War history of science was by that time already old, tired, and entrenched, but still arousing strong passions. The Marxist-inspired approach, imported from the Soviet Union by Hessen in the 1930s and dubbed 'externalism' as disambiguation by its ideological opponents, by the late 1960s had been largely abandoned back in the USSR but still promoted by Western Marxists such as J.D. Bernal (Bernal, 1954). It interpreted the Scientific Revolution as a product of social revolutions in early modern Europe, and science as a progressive force that was intimately linked and responsive to the economic and technological needs of the rising capitalism. To exclude subversive Marxism from major academic programmes in history of science, the Western establishment picked a different approach, also with intellectual roots in revolutionary Russia, developed by an émigré and avowedly anti-Marxist revolutionary, Alexandre Koyré (Mayer, 2004). His Anglo-American epigons mistakenly labelled it 'internalism', but Koyré did not try to isolate and restrict science to its own internal logic. His original interpretation defined the Scientific Revolution as a destruction of the Cosmos and a change in the worldview—cosmological, metaphysical, and religious all at once.[6] Koyré analysed the new European science as intimately entangled within the broadly defined Platonic world of cultural ideas and philosophical introspection of the time period, but emphatically proclaimed it above and irreducible to the profanity of material concerns and technological artisanship. As a methodological model, Forman's study did not satisfy either of the Cold War camps. To the predominantly anti-communist 'internalist' side, it was as much an anathema as the Marxist programme, and similarly subversive. For the 'old left' tradition, however, it sounded too anti-science. To them, a true science worthy of its good name was supposed to be an ally of progressive politics and the major force of social development, whereas reactionary ideological influences could only corrupt scientific knowledge, not contribute to its revolutionary advancement.

Despite the wide outrage provoked by the Forman Thesis, only a few explicitly negative rebuttals appeared in print (Hendry, 1980; Kraft and Kroes, 1984). The main counter argument relied on drawing or assuming a boundary between the outwardly oriented 'ideology' or 'rhetoric' of scientists and their supposedly 'autonomous', 'internal' knowledge. If the former, maintained the opponents, could be influenced or insincerely adjusted to hostile pressures from the outside, at the latter, internal level,

[6] Although teaching the concept of the Scientific Revolution continues to provide bread and butter for historians of science at many universities, debates regarding its meaning and applicability continue to this day, with shifting foci and reinterpretations. See, for example (Osler, 2000).

one should expect from scientists an entrenchment and professional autonomy rather than adaptation. Critics claimed that Forman had underestimated the internal reasons from the mounting problems in physics that around the same time pushed scientists towards accepting acausality as a fundamental feature of the atomic world (although the chronological coincidence still remained an unexplained puzzle as long as one continued to insists that 'internal' arguments did not interact with outside pressures).

Opponents also questioned the interpretations and the limited number of individual cases of physicists and mathematicians whose conversion to acausality was publicly recorded prior to 1925 and pointed out counterexamples of resistance by well-known scientists who refused to adapt even at the level of rhetoric. Hendry still had to concede that '[d]espite the criticisms that may be levelled against his analysis, Forman has succeeded in demonstrating that physicists and mathematicians were generally aware of the values of the milieu, and that this milieu did incorporate a marked hostility toward the causality principle. But when we come to the crucial claims, that there was a widespread rejection of causality in physics, and that there were no internal reasons for this rejection, then the weaknesses in his argument also become crucial. For there were strong internal reasons for the rejection of causality, and when these are taken into account, and Forman's supposed "converts to acausality" critically re-examined, it would appear that the reaction of physicists to the causality challenge was far from being accommodation, and that there may even have been a tendency to isolation.' (Hendry, 1980, p. 160).[7]

Yet, general perspectives on the nature and practice of science were already changing at the time, and the Forman Thesis both reflected and influenced these tectonic shifts. It did so precisely by undermining the boundary, or the very assumption that the internal content of science can be isolated and unequivocally separated from cultural impact and social context. In hindsight, Julia Menzel and David Kaiser observe: 'Since the article's publication, it has become a matter of principle within the history of science to insist always on the embeddedness of science in society—and there can be no doubt that even the abstruse concepts of quantum physics are worldly things produced by particular people in specific cultural contexts, toward interested ends' (Menzel and Kaiser, 2020, p. 34). Despite some temperamental objections to its findings, Forman's work has fundamentally changed directions of research and established itself as a classic in science studies, including history, sociology, and philosophy of science. In subsequent decades it became required reading in practically every graduate programme that trained students in the above fields.

Forman's analysis of Weimar physics furnished a paradigmatic example for the sociology of scientific knowledge that developed by the 1980s and made the idea of

[7] The known cases of resistance by Einstein, Planck, Schrödinger, and others did not contradict Forman's argument and thus were not really counterexamples. They were discussed in his paper and were necessary for the very formulation of the argument: 'My sympathies have consequently been with the conservatives in their defence of reason rather than with the "progressives" in their denigration of it.' (Forman, 1971, p. 113).

social construction widely acceptable (Hacking, 1999). The special importance of this example derived from the fact that it focused on physics and mathematics, the so-called 'hard sciences' that were and still are considered a much more challenging target for social constructivism than the life sciences and social sciences. It also dealt with a very recent breakthrough and with concepts considered true and fundamentally valid by living scientists, rather than with some outdated knowledge or antiquated theory from the era of Newton, for which it is psychologically easier to invent a historical deconstruction. Thus, some pioneers of the new sociological approaches to science could cite the case of quantum acausality as one of the most powerful demonstrations of the far-reaching influence of social factors all the way down to the hard theoretical core of scientific knowledge (Barnes, 1974; Bloor, 1981; Shapin, 1982). Eventually, another classic breakthrough by Shapin and Schaffer inquired into the very process of how the boundary between the scientific and the social, and their respective definitions, could be constructed and contested historically (Shapin and Schaffer, 1986; Shapin, 1992).

Forman's work also became one of those rare historical studies which, by relativizing the existing scientific dogmas, helped contemporary physicists such as John Bell to critically reassess them, something that Ernst Mach had also achieved heuristically for Einstein at the end of the 19th century with his critical historical exposé of absolute space and time in Newtonian mechanics. Physicists' attitudes towards the philosophical interpretation of quantum theory have changed dramatically since the 1960s. When the Forman Thesis was published, acausality of the quantum laws was still generally seen as part of the core scientific formalism, according to the then-dominant Copenhagen interpretation (Howard, 2004, 2021). Due largely to the work of Dmitry Blokhintsev, David Bohm, and John Bell, physicists' views shifted in the direction of philosophical pluralism within which different interpretations, including causal ones, are possible. Bell was aware of Forman's historical critique and used it as additional encouragement in his efforts to challenge the Copenhagen orthodoxy from within physics (Bell, 1982). In subsequent decades, ever more historians and philosophers of science also turned in their analyses to those 'conservative' physicists who had disagreed with the prevailing opinions of their colleagues and defended rationality and the causality principle in quantum mechanics, to whose previously neglected views Forman had called sympathetic attention.[8]

Since the 1960s, the history of quantum physics has matured as a field, with detailed studies of the technical formalism, philosophical questions, institutional settings, biographies, and collected editions of major contributors (Staley, 2013; Badino, 2016). Forman's work exerted profound influence on subsequent generations of researchers: its methodology, problematic, conceptual vocabulary, and questions continue to inspire further inquiries and generate controversies. Forman defined his approach as 'sociological' rather than 'psychological'. His description of scientists' ideology, or self-serving, idealized, and public representation of their activities, did

[8] See, in particular, (Cushing, 1994; Beller, 1999; Freire Jr, 2014).

not need to inquire whether these beliefs were individually sincere—or not. Most often, the critics questioned Forman's explanatory model as too rigid, for its assumption that the adaptation to social pressures was itself a causal, practically deterministic one-way street. 'With the information available, Forman has succeeded in demonstrating an influence of the milieu upon physicists' attitudes to causality, and were he to adopt a suitable concept of historical causation he could even assert quite reasonably that the attitudes were in some (weak) sense "caused" by the milieu . . . Physicists *were* influenced by the crisis-consciousness of post-war Europe and by the attitudes characteristic of the Weimar milieu. On the other hand, Forman's work has also demonstrated the dangers of a purely external treatment and the poverty of any naive social reductionism,' insisted Hendry (1980, pp. 170–171).

The above quote shows that even those who disagreed profoundly still could not deny the main discovery of a meaningful connection between Weimar cultural ideology and the quantum mechanical revolution. Many others who worked in the field could agree with Forman or disagree on the details, and yet continued to grapple with the problem of how exactly to characterize and describe the social causation's *modus operandi*. The problem presents itself as theoretical, possibly unsolvable, not just for this case but for social studies of science in general, with scholars taking different stances, from more straightforward and deterministic to indirect and variable, in a stronger or weaker sense. In the broader social and cultural history of science, into which the sociology of scientific knowledge has partly been folded, a vast range of new methodologies has been advanced since 'Weimar Culture'. In particular, the strongly causal models of interest characteristic of the early years of the sociological programme have been supplemented by (or watered down to) more modest accounts of resonances. Thus Norton Wise, in his critique of Forman, has proposed a 'model in which resources and participation replace influences and capitulation' (Wise, 2011, p. 430).

In the meantime, historians working in the genre of cultural history of science applied and extended Forman's argument further, adapting its conceptual approach to other cases and situations, and checking its applicability to different cultural milieus. To mention only a few examples of important investigations, and only those belonging to quantum historiography, Heilbron (1985) described the post-1927 spread of the Copenhagen mystical philosophy with its characteristic 'combination of imperialism and resignation', whereas Stephen G. Brush developed a longue-durée model of cultural affinities in physics, in which periods of realism and positivism alternated with more conservative and irrational (neo)romanticism (Brush, 1976, 1980). Silvan S. Schweber applied Forman-inspired analysis to the cases of Arnold Sommerfeld and Hans Bethe (Schweber, 2009, 2014) and Richard Beyler to the case of Pascual Jordan (Beyler, 2011). Dealing with historical contexts outside of Weimar Germany, Richard Staley analysed the early 20th century cultural debates about mechanics that contributed to Spengler's views on causality, Alexei Kojevnikov revealed the impact of Soviet collectivism on the development of conceptual language in solid-state and condensed-matter physics, and David Kaiser described the application of democratic ideals to models of subnuclear particles in post-war America (Staley, 2011; Kojevnikov, 1999; Kaiser, 2002).

36.4 Revealing Examples: Ehrenfest, Pauli, Schrödinger, Joffe

A June 1919 letter to Bohr from Paul Ehrenfest, professor of theoretical physics at Leiden University, almost literally confirms the core claim of the Forman Thesis: '[I]t is remarkable that precisely here, in the circles of men having much to do with technology, production, industry, patents etc., opinions develop so uniformly about perspectives of culture. Overall there is building up an uncannily intensive reaction *against rationalism* . . . If I am not entirely mistaken, in the next 5–10 years we will see the following happening at the institutes of higher learning (including technical!). Professors raised as relatively *rational* and disciplined individuals will despairingly and uncomprehendingly face the complaints and demands of a relatively "*mystical*" student body. At the same time, scientifically less clear but personally warmer teachers will gain the main influence over students . . . As I write this, it suddenly became so much clearer to me why, in the opinion of the young, I am so much more strongly associated with the older.'[9]

The document illustrates that, in the immediate wake of World War I, a tidal reaction against rationalism and favouring more mystical lines of thought swept through not just the intellectual public in general, but also such professionals as engineers and exact scientists previously expected to strongly resist such trends. The communication from a theoretical physicist in the Netherlands to his colleague in Denmark also signified that the mood did not remain confined to Germany and Austria, but also affected at least the neighbouring neutral countries. The letter revealed a striking admission and expectation that professors would adapt self-consciously, rather than unreflectively, to the direction of the prevailing intellectual wind. Although not abandoning his personal rationalistic convictions, Ehrenfest appeared to defer to the opinions of the younger students, pointing at an additional effective milieu capable of extracting concession from physicists. Indeed, many professors probably cared more about pleasing students in their auditoriums than academics from other disciplines. In his own field of theoretical physics, Ehrenfest regarded Bohr as precisely the kind of professor whose thoughts were too profound to be understood or even expressed clearly, which only helped him to be tremendously inspiring and resonate with the younger generation of students. Whether or not Ehrenfest's letter contained implicit advice to Bohr, and whether or not Bohr accepted the hint or arrived at similar ideas on his own, around the same time he was already inclined 'to take the most radical *or rather mystical* views imaginable' regarding the daunting problem of the quantum

[9] Paul Ehrenfest to Niels Bohr, 4 June 1919, Niels Bohr Scientific Correspondence, Niels Bohr Archive, Copenhagen (emphasis in the original).

interaction of matter and radiation and did not consider a modicum of such mysticism inconsistent with the practice of natural science.[10]

Five years later, in a desperate attempt to ward off the concept of light quantum, Bohr resorted to one such idea in the famous, or infamous, Bohr–Kramers–Slater theory of 1924. He proposed that mysteries of the quantum could be resolved if one assumed that the conservation laws for energy and momentum are valid only statistically, if averaged over a great many atomic interactions, but violated in individual processes at the microscopic level. The quantum community split in reaction to this radical proposal. Schrödinger, who had already two years earlier publicly declared himself opposed to causality in physics, welcomed it enthusiastically (Forman, 1971, pp. 87–88; Hendry, 1980, pp. 164–167). Others were much more reserved or sceptical: even if willing to abandon causality in principle, they were reluctant to sacrifice for this purpose the revered law of energy conservation. Pauli (in letters) vehemently opposed the 'reactionary Copenhagen Putsch'. After Bohr's proposal had been refuted in experiment, he went even further in proclaiming: 'I definitely believe that *the probability concept should not be allowed in the fundamental laws of a satisfying physical theory*. I am prepared to pay any price for the fulfilment of this desire, but unfortunately I still do not know the price for which it is to be had'.[11]

Interestingly, both Schrödinger and Pauli would reverse themselves during the subsequent process of creating quantum mechanics. The former, once he had authored wave mechanics early in 1926, traded his philosophical stance from acausality to *Anschaulichkeit*. Both concepts appealed to the ideological milieu, but *Anschaulichkeit* corresponded better to Schrödinger's latest fundamental breakthrough and his ambitious hopes for wave mechanics. Pauli changed his attitude towards fundamental probabilities once, in the wake of Max Born's probabilistic treatment of scattering, he made his own important contribution to wave mechanics in the fall of 1926. Having shown that Schrödinger's psi-function could be interpreted as the probability of the electron's position, Pauli would forever remain a staunch proponent of the statistical interpretation of quantum mechanics.[12] Schrödinger formulated his philosophical dilemma in August 1926: 'Today I no longer like to assume with Born that an individual process of this kind is "absolutely random", i.e., completely undetermined. I no longer believe today that this conception (which I championed so enthusiastically four years ago) accomplishes much. From an offprint of Born's last work in the *Zeitschr. f. Phys.* I know more or less how he thinks of things: the *waves* must be strictly causally determined through field laws; the wave functions, on the other hand, have only the meaning of probabilities for the *actual* motions of light or material particles. I believe that Born overlooks that—provided one could have this view worked

[10] 'or rather mystical' is inserted into the sentence above the line. Bohr to Charles Galton Darwin, July 1919, Niels Bohr Scientific Correspondence, Niels Bohr Archive, Copenhagen, draft of a presumably unsent letter. On Ehrenfest's philosophical struggles, see (Lunteren and Hollestelle, 2013).

[11] Pauli to Bohr, 17 November 1925, emphasis added (Pauli, 1979, p. 260).

[12] (Born, 1926); Pauli to Heisenberg, 19 October 1926 (Pauli, 1979, pp. 340–49).

out completely—it would depend on the taste of the observer *which* he now wishes to regard as *real*, the particle or the guiding field.'[13] Schrödinger's and Pauli's dramatic and opportunistic flip-flops on fundamental philosophical principles reveal that while the chief authors of quantum mechanics did feel compelled to relate their work to the *Zeitgeist* of the time, the latter was still rich enough to allow quantum physicists some flexibility and choices, to be better able to advance various personal agendas and interests (Kojevnikov, 2011).

By the end of 1927, the winning parties of the philosophical battle over quantum mechanics defined their choices if not completely identically, at least with sufficient overlap. Born and his Göttingen student Jordan proclaimed the abandonment of classical causality to be the main fundamental lesson of quantum mechanics. Bohr in his Como address (supported by Heisenberg, who held somewhat deviating views on the matter) stressed *Individualität*, limitation on causality, and also restricted, but still possible *Anschaulichkeit*, or spatio-temporal description of microscopic phenomena (Kojevnikov, 2020). All of these notions resonated in the Central European, German-focused academic milieu, but, as Forman has shown, they did not arouse equally strong feelings in the Anglophone culture of the time (Forman, 1979). We can find an even more striking contrast by looking at how these philosophical problems played out in a milieu with very different cultural values—in the ideological atmosphere of Soviet Russia.

From the similarly tragic experiences of World War I, the Russian revolutionaries and the German conservative intellectuals drew lessons that went in almost exactly opposite ideological directions. The ideals of progress, rationality, modernity, and scientism rose to unprecedented cultural authority in Soviet Russia following the War and the Revolution, not only among the Marxists, but among the educated public in general, especially scientists. Their emphatically pro-science general stance, however, did not prevent Soviet Marxists from feeling suspicious of certain irrational tendencies in 'bourgeois science', including quantum mechanics, or rather, its philosophy (Kojevnikov, 2012). The 'dean' and top manager of Soviet physics, Abram Joffe, attempted to assuage these concerns in 1934 when speaking to a political gathering on the occasion of the 25th anniversary of Lenin's *Materialism and Empiriocriticism*. Addressing this, 'hostile' in its own way, audience on 'The Development of Atomistic Views in the 20th Century', Joffe emphasized those features of quantum mechanics that could provide grounds for cooperation between Soviet physicists and Marxist philosophers. His choices, not surprisingly, were almost polar opposites to those preferred by his German colleagues. According to Joffe, quantum mechanics was *unanschaulich* (the corresponding Russian term is *nenagliadnyi*, or non-visual, non-pictorial), statistical but causal, and most importantly, it signified a fundamental 'loss of individuality' for quantum particles (Joffe, 1934, p. 60).

In Joffe's interpretation, *Unanschaulichkeit* stood for the truly revolutionary character of quantum mechanics: the theory appeared counterintuitive and non-visual,

[13] Schrödinger to Wilhelm Wien, 25 August 1926, archive of the Deutsches Museum, Munich.

because scientists' existing pictorial intuitions had been formed by the traditional, classical theories. The abandonment of the old ways of visual representation, including the notion of the electron's trajectory, meant that the physical laws in the microscopic world were radically new and qualitatively different from the familiar laws operating at the macroscopic level. Dispensing with *Anschaulichkeit* could thus be easily explained and even turned into an advantage in the Soviet context, in conformity with the anti-reductionism of the official Marxist philosophy of dialectical materialism (Martinez, 2021). But the principle of causality, or *Kausalität*, was ideologically sacrosanct for the Soviet Marxists and could not be questioned. Joffe and other Soviet physicists therefore carefully avoided mentioning 'acausality' or attaching this label to quantum mechanics. Instead, they proclaimed that the old crude version of mechanical determinism, dating from the 18th century, had been superseded by a more refined and sophisticated causality in the quantum world. To Soviet authors, the validity of statistical laws and probabilistic formulae did not necessitate the abandonment of causality as the fundamental principle of science: the former could be used in quantum mechanics without sacrificing the latter (Kojevnikov, 2012).

The Soviet political ideology promoted collectivism instead of individualism, and its representatives were certainly happy to hear from Joffe that quantum statistics proved that atomic particles no longer possessed absolute individualities, and that the laws of quantum mechanics described collective behaviour and processes. Joffe reassured his Marxist audience that the existing quantum theory was still quite young and not necessarily complete, given continuing disagreements among its main contributors, but both quantum mechanics and relativity, the other profound revolutionary development in physics, were definitely confirming the philosophy of dialectical materialism. Had it been just this talk alone, Joffe's philosophical interpretation could have been dismissed as merely rhetoric, necessary to please the authorities and protect quantum mechanics from ideological criticism in the Soviet Union. But around the same time, Soviet theoretical physicists were already designing new physical models—quasiparticles and collective excitations—that would transform the socialist philosophy of collectivism into the conceptual language and mathematical apparatus of the quantum theory of condensed matter (Kojevnikov, 1999).

36.5 Conclusions

The Forman Thesis *was* controversial fifty years ago, when first introduced, but it is anachronistic to continue branding it this way now. What made it scandalous back then has since become generally accepted. Most of the initial outrage came from the modernist rejection of the possibility that local and idiosyncratic culture could influence the supposedly universal scientific knowledge. As the paradigmatic example demonstrating such interaction, the Forman Thesis was instrumental in the rise of new scholarly understandings of science during the 1970s and 1980s. As the number of other cases involving various cultures and scientific disciplines grew, the scholars who described them met with significantly less opposition than Forman had initially. Today the understanding that science is produced locally, in social settings, and conditioned by culture is fully accepted in cultural studies and histories of science, almost to the point of carrying no burden of proof. Current assumptions about science, however, make it harder to explain how such locally produced knowledge manages to travel across cultures and establish itself internationally; hence the importance of comparative studies related to the case described by Forman.

While its central methodological lesson became de facto commonly accepted and went to the masses, stereotypes of perception continued, by inertia, to ascribe the label 'controversial' to the original Forman Thesis. Although for somewhat different reasons, Forman's case can be compared to that of one of the physicists he studied, Erwin Schrödinger. Schrödinger's equation and the psi-function he introduced became universally known and enormously successful tools of the discipline, not requiring a reference or any explicit allocation of credit. At the same time, their author's figure and standing continued to be seen as somewhat marginal, by reputation, in large part because he did not control the field institutionally, or the interpretation of quantum mechanics. Also, as in Schrödinger's case, some aspects of the initial version of Forman's study, which is now fifty years old, are open for update, revisions, modifications, and debates about the historically evolving relationship between science and culture.

Despite its by now classic status, the Forman Thesis generates both inspiration for new studies and criticism in the field, providing a reference point for the shifting approaches and methodological changes underway in science studies. His sociological and scientistic explanatory model of causation, in particular, continues to cause disagreements and discomfort for contemporary post-modernist sensibilities. His description of physicists succumbing to ideological currents of the time flies in the face of today's currents insisting that individuals are free, even when they shop as prescribed by the latest advertisement in social media. Forman's moralism can be unsettling. He simultaneously rebuked scientists for their betrayal of disciplinary values and demonstrated that the idea of the autonomous disciplinary community is an ideological fiction, in direct opposition to Kuhn's then popular model of scientific revolutions within the self-contained scientific community. Kuhn's model also used to be somewhat controversial back in the day when it was influential, but has since become simply outdated, so that now it can be safely praised and glorified as a dead classic. This is not the case with the Forman Thesis.

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