1 The Causality Debates of the Interwar Years and their Preconditions: Revisiting the Forman Thesis from a Broader Perspective

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In this paper I have to be sketchy and up-front. Sketchy because I will try to convey the punch line of my 400 page Ph.D.-dissertation (Stöltzner 2005) and the papers ensuing therefrom (Stöltzner 1999, 2000, 2003). I showed there, I guess, that the oft-debated causality debates in Weimar Germany and interwar Austria were an integral part of a much longer causality debate that emerged from two different readings of Boltzmann's legacy, statistical mechanics, at the end of the 19^{th} century and ended only in the late 1930s when the philosophical debates surrounding quantum mechanics abated. Viewed within the context of this longer debate, the years 1918–1927 were, no doubt, a time of turmoil ranging from culture and politics to philosophical terminology. But in contrast to Forman (1971), I think, that most scientists seriously pondering about and writing on causality had taken their general philosophical stand already long before. Rather than an adaptation to a hostile intellectual milieu that, in 1927, proved to be of selective advantage, we find a complex but continuous debate about the philosophical consequences of modern physics that, after 1913, mainly appears on the pages of the leading scientific journal of the German-speaking world, Arnold Berliner's *Die Naturwissenschaften*.

Let me be up-front, as was Forman himself when he spoke of his protagonists as "converts to acausality" who made "quasi-religious confessions to the [anti-scientific] milieu", advocated "existentialism disguised as logical empiricism" (on Reichenbach) and published a book that contained "largely blather" (on Frank 1932). To my mind, Forman's thesis misses core aspects of the causality debate from 1918 to 1927, and it does so because its author followed a methodology that, albeit fruitful in many other domains, is unable to assess the interactions between philosophical and scientific commitments in a historical context of the kind of Weimar Germany and interwar Austria. More generally, in order to appraise the historical dynamics of a philosophical concept, such as causality, any broader historical and sociological approach must be accorded with the recently sharpened methods of the history of philosophy of science.

My own picture of the causality debate is a complex one and it involves philosophical as well as historical, ideological, and sociological motives. Philosophically, the debate about causality and determinism was hardly separable from the quest for a proper interpretation of physical probability, the conception of microphysical reality, and the more general debates about the effects of modern physics on the conception of nature. Some of these lines reached back into the 1890s, when the old mechanical world-view broke into pieces, and they concerned statistical mechanics, relativity theory, and quantum physics alike. Many critics, among them John Hendry (1980), have noted that Forman's main philosophical misconception was to adopt, as did Spengler, a conception of causality that was intimately linked to the strict determinism that predated statistical mechanics. Already in 1872, Emil du Bois-Reymond had touted the idea that firmly sticking to this explanatory ideal forced scientist to forgo forever a full understanding of the essence of matter and force. Interestingly, in the Weimar days hardly any scientist rehearsed the old *Ignorabimus*, but many considered it the basic misconception responsible for the diagnosis of crisis in modern physics. It is true, Ludwig Boltzmann, in his battles with energeticism, conceived himself as the last exponent of the mechanical world-view. But on the other hand, in developing step-by-step the statistical interpretation of the second law of thermodynamics he introduced a notion that was entirely alien to the mechanical picture which DuBois-Reymond had built upon, to wit, an objective physical probability that could no longer be understood as degree of ignorance.

The debate as to whether physical science at all required a causal foundation started with Franz Serafin Exner's rectorial address of 1908, by which he launched a local tradition of empiricist indeterminism that I call Vienna Indeterminism. Among its main advocates, I count, besides Exner, his former assistant Erwin Schrödinger and the Logical Empiricists Philipp Frank and Richard von Mises. Exner (1909) argued that chance is the basis of all natural laws and that the apparent determinism in the macroscopic domain emerged only as the thermodynamic limit of many many random events. Max Planck, in another rectorial address of 1914 that equally sung the praise of Boltzmann, fiercely objected to Exner's indeterminism and insisted on a deterministic foundation of all natural laws including the probabilistic ones. The debate, as I conceive it, ended on the 1936 Copenhagen Congress for the Unity of Science when Frank and Moritz Schlick, Planck's former student and a resolute critic of any indeterminacy in principle, joined arms to combat the increasing number of metaphysical misinterpretations of quantum mechanics.

My main sociological point is that in the German-speaking world the causality debates were conducted by 'physicist-philosophers', a role model that was more widespread there than in other countries. It had influential historical prototypes, among them Hermann von Helmholtz and Ernst Mach, and there was a clear conduit how a physicist laid claim to it. Most importantly, these philosophical ambitions were only loosely embedded into the then current philosophical schools, among them neo-Kantianism and *Existenzphilosophie*.

Since the philosophical convictions of physicist-philosophers were not forced into a coherent philosophical system, there was ample leeway to simultaneously participate in different thought collectives. This conception that we owe to Ludwik Fleck (1929) is an important clue to understand the seven years the Forman thesis is about. Yes, there were those who defended scientific modernism and technological progress, but diagnosed a cultural crisis. Richard von Mises, for one, still in the 1950s heeded sympathies for Spenglerian ideas. And there were others, like his fellow physicist-philosophers from the Vienna Circle, who at the 1929 Congress of the German Physical Society went public with the claim that the achievements of modern science demanded an entirely new style of scientific philosophizing and, more broadly, a scientific world conception. No wonder that Richard von Mises disliked the manifesto, but he and Frank, in the opening session of the same congress, appeared almost as intellectual twins in their plea for abandoning the old triad of "school philosophy", the categories of space, time, and causality, and replacing it with more suitable notions. (Frank 1919, von Mises 1930) This message was well understood by those who rejected the neo-positivist assault on metaphysics, among them the third speaker of the Prague session, physicist-philosopher Arnold Sommerfeld (1929).

My paper is organized as follows. First, I provide a sketch of the causality debate. Second, I characterize the role model of physicist-philosopher and to what extent *Die Naturwissenschaften* provided a forum for this discourse. Third, I historically contextualize the Forman thesis itself and indicate in what way new methodological insights in the history of philosophy of science may prove helpful in understanding the causality debate.

Vienna Indeterminsim and the Causality Debate

Rather than representing merely a "subterranean anticausality current" (Forman 1971, 67), as Forman put it, Exner's inaugural speech of 1908 made a great stir and triggered a polemic with Planck that continued the earlier Mach-Planck controversy. Frank's recollections in the interview with T. S. Kuhn show how influential this speech was for the then younger generation of Vienna physicists. Not that Exner in 1908 would have continued Mach's skepticism about atoms and the energeticist interpretation of thermodynamics. To the contrary, he closely followed the brand of empiricism that Boltzmann had developed from the late 1880s on in order to employ Mach's anti-metaphysics against his primary opponent, Wilhelm Ostwald's energeticism. Boltzmann's consistent empiricism had important consequences for the basic principles of physical science. In the last years of his life and especially in his 1903–1906 lectures on natural philosophy, he contemplated that even the law of energy conservation was only statistically valid—an idea that would surge much later in the 1924 Bohr-Kramers-Slater quantum theory—and that the entropy of a system might be described by a nowhere differentiable function. (Boltzmann 1898)

Exner amended Boltzmann's late indeterminism in an important dimension. While Boltzmann had devoted surprisingly little attention to the interpretation of probability, Exner brought the relative frequency interpretation, or the *Kollektivmaßlehre*, developed by Gustav Theodor Fechner (1897) to bear on the kinetic theory of gases. On this basis, he could simultaneously claim that (*i*) in physics "we observe regularities which are brought out exclusively by chance" (Exner 1909, 13) but whose probability is so high "that it equals certainty for human conceptions" (Ibid., 16); while (*ii*) in the domain of the humanities and the descriptive sciences "the random single events succeed one another too slowly [such that] there can be no talk about a law." (Ibid., 14) Still, or so he would claim in an unpublished manuscript (1923), the evolution of culture was shaped by the second law in virtue of which culture and science necessarily advance and spread despite the death of the individual cultural organism. Such read Exners's own reaction to the Spenglerian challenge. (Cf. Hiebert 2000, Stöltzner 2002)

Let me turn to the opposite side in the first phase of the causality debate. Planck, in his famous Leiden lecture of 1908 that had started the polemics with Mach, vigorously defended what he took to be Boltzmann's legacy against Mach's anti-realism. In

1914, he now felt obliged to save Boltzmann's legacy from Exner's overinterpretation. Planck stressed "the fundamental importance of performing an exact and fundamental separation between ... the *dynamical*, strictly causal, and the merely *statistical* type of lawfulness for understanding the essence of all scientific knowledge." (1914, 57) This distinction finds its expression in the sharp contrast between reversible processes subsumed under a dynamical law and irreversible processes governed by the second law of thermodynamics. "This dualism ... to some may appear unsatisfactory, and one has already attempted to remove it—as it does not work out otherwise—by denying absolute certainty and impossibility at all and admitting only higher or lower degrees of probability. ... But such a view should very soon turn out to be a fatal and shortsighted mistake. (Ibid., 63) This was an obvious allusion to Exner, who responded to Planck's in a separate chapter of his 1919 *Lectures on the Physical Foundations of Natural Science.*

If we look at Planck's line of argument, we come to recognize that the principal difference between what I call Vienna Indeterminism and the Berlin reading of Boltzmann consisted in the relationship between causality and physical ontology. Either one followed, as did the Berliners, Kant by claiming that to stand in a causal relationship was a condition of the possibility for the reality of a physical object (Kant called this empirical realism), or one agreed with Mach that causality consisted in functional dependencies between the determining elements and that physical ontology was about 'facts' (*Tatsachen*) that consisted in stable complexes of such dependencies. To those standing in the Kantian tradition, the latter stance fell short of the aims of scientific inquiry. Those standing in the Hume-Mach tradition, however, had more leeway in searching for an ontology that was suitable for a new scientific theory. Notice that this difference in ontology extends across a larger historical time scale than the debate I am focusing on because it reached back to Mach's works of the 1870s and 1880s and ended only when philosophers of science abandoned the ideal of descriptivism after 1945.

Based upon this basic distinction between two notions of causality, Vienna Indeterminism—as touted by Exner in 1908—can be characterized by the following three commitments: (*i*) The highly *improbable events* admitted by Boltzmann's statistical derivation of the second law of thermodynamics exist. (*ii*) In a radically empiricist perspective, the burden of proof rests with the determinist who must provide a sufficiently specific theory of microphenomena before claiming victory over a merely statistical theory. Even worse, assuming a deterministic micro-theory without cogent reasons would lead to a "duplication of natural law [that] closely resembles the animistic duplication of natural *objects*." (Schrödinger 1929, 11)—as Schrödinger put it in his 1922 Zurich inaugural address. (iii) The only way to arrive at an empirical notion of objective probability is by way of the limit of relative frequencies. It is meaningful to assume the existence of statistical collectives (*Kollektivgegenstände*) and relate them to experience even though the limit is only obtained for infinitely large collectives. In 1912 and 1919, von Mises provided the rigid mathematical framework for the relative frequency interpretation.

Let me add that one has to distinguish two kinds of realism within the within the younger generation of Vienna Indeterminists. While Frank and von Mises came to elaborate the conventionalist picture and take theories as purely symbolic entities co-ordinated to experience, Schrödinger never abandoned Boltzmann's conception of theories as universal pictures.

In 1914, Planck rejected all three tenets of Vienna Indeterminism, but in the 1920s he reconciled himself with the highly improbable events (*i*). But even after the advent of quantum mechanics, Planck still cherished the hope for a deterministic reformulation of atomic physics. His former student Moritz Schlick gradually approached Vienna Indeterminism as far as the burden of proof (*ii*) was concerned, but he never accepted the relative frequency interpretation (*iii*). Still in 1925, when already chairing the Vienna Circle's discussions, he held that "only in the utmost case of emergency will the scientist or philosopher decide to postulate purely statistical micro-laws, since the scope of such an assumption would be enormous: The principle of causality would be abandoned, ... and hence the possibility of exhaustive knowledge would have to be renounced." (Schlick 1925, 461/61) After the case of emergency had occurred in the form of quantum mechanics, Schlick (1931) presented an entirely new theory of causality in which the verificationist criterion of meaning blocked the assumption of a micro-world that was deterministic but unobservable in point of principle. But Schlick still demanded to separate all statistical regularity (*Gesetzmäßigkeit*) into strict law and pure randomness, such that there were strictly speaking no "statistical laws"—a thesis that surprised physicists as diverse as Einstein and Heisenberg. The reason was that Schlick till the end remained committed to Johannes von Kries's (1886) *Spielraumtheorie* of probability in which objective randomness was integrated into a deterministic Kantian universe. While Schlick had to openly revoke his 1920 theory of causality in the face of quantum mechanics, the Vienna Indeterminists could feel themselves confirmed. It is important for the sociological coherence of the latter tradition that they typically combined such a declaration with an explicit reference to Exner's priority, while Schlick held that Exner's works contained nothing beyond the traditional philosophical criticism against determinism.

The confrontation just sketched provides a framework, in which also other alleged 'converts to acausality' can be integrated. Here are just two examples. Walter Nernst, for one, had not forgotten the days he had worked with Boltzmann in Graz. "Among all laws [of physics] the thermodynamical ones occupy a distinctive position because unlike all others they are not just of a special kind, but applicable to any process one can imagine." (1922, 492) In the same vein, Exner had, in 1908, argued that the second law is the basic principle of nature. If one related all physical laws to the second law of thermodynamics, so Nernst continued, this would not reduce their significance or rank; "it would however put an end to the logical overuse of the laws of nature." (Ibid., 493)

In a review of Exner's (1919) *Lectures*, Hans Reichenbach endorsed "that Exner unequivocally advocates the objective meaning of the probabilistic laws in which he rightly conceives a very general regularity of nature." (Reichenbach 1921, 415) As did Exner, Reichenbach held that the basic laws of nature were of a statistical kind. But he did so for reasons that contradicted the radical empiricism of the Vienna Indeterminists. In his Ph.D. dissertation of 1915 and in a series of papers ensuing from it in the early 1920s, he argued that the principle of causality must be supplemented with a principle of lawful distributions (later called principle of the probability function) that guarantees that future empirical findings do not constantly change the form of the law. Due to the unavoidability of measurement errors the connection between our experiences and probability theory was of a more basic kind than the one between any other theory and our experiences. This thesis of Reichenbach became a source of conflict with von Mises.

I am afraid that I have to leave it at these sketchy remarks about the continuity of the causality debate across the breakdown of the two empires in 1918 and the quantum revolution in 1926. Let me just add that the problem remained under philosophical dispute even at a time when most physicist had accepted quantum mechanics and Born's statistical interpretation of the wave function, and it did so even within the narrower circle of Logical Empiricists.

The Physicist-Philosophers and Their Main Forum

Let me now turn to the sociological context of the causality debates. I have already emphasized the importance of the role model of physicist-philosopher for my case. Let me now describe how a physicist laid claim to this status, even though he might have remarked to his colleagues, cunningly or wittingly, that this represented only his Sunday's activity.

Forman was right to assume "that institutions of German academic life provided frequent occasions for addresses before university convocations", and that this indicates "the extraordinary heavy social pressure which the academic environment could and did exert upon the individual scholar and scientist placed within it." (1971, 6f.) Both in the Wilhelmin Empire and in the Weimar Republic, the main duty of a physicist having been elected rector, dean of the philosophical faculty, or secretary of an academy was to build a bridge to his colleagues from the humanities. They demanded something more profound than just popularizing one's scientific achievements and emphasizing their importance for technology and state.

Academic customs thus set the stage for the physicist-philosophers. The most influential role models were Hermann von Helmholtz and Ernst Mach, because it was mainly them who led the way out of the older *Naturphilosophie* by developing a new style of discussing the foundations of science as a philosophical problem. For this reason, many physicist-philosophers following in their footsteps remained critical towards excessive speculation, or published them anonymously and with a grain of salt, and they often eschewed entering into popular discourse. Quite a few of them, accordingly, regarded Wilhelm Ostwald's monistic sermons and Ernst Haeckel's writings with suspicion.

The publication of the academic addresses followed a typical scheme. Initially, they came out as separate booklets and were republished in one or two journals of the learned societies. At a certain point, a physicist would then assemble a collection of those academic speeches into a book that bore a title such as "Popular Writings" (Boltzmann 1905) or "Physical Panoramas" (Planck 1922). The publication of such a book testified the author's new status as a physicist-philosopher. There were also a few journals that combined publications of scientists having a philosophical thrust with papers penned by guilded philosophers who were positively disposed towards the sciences, most prominently among them the *Vierteljahrsschrift für wissenschaftliche Philosophie und Soziologie* and Ostwald's *Annalen der Naturphilosophie.* They ceased publication in 1916 and 1921 respectively.

In 1913, the media landscape for the physicist-philosopher underwent a significant change. From now on, the mentioned academic addresses to a large percentage were published by the newly founded *Die Naturwissenschaften.* Modeled after the British *Nature*, the "scientific weekly for the progresses of science, medicine, and technology"—thus read its subtitle—strove to "follow the major developments within the whole of natural science and present them in a generally comprehensible and captivating form"

(1913, 1). The journal not only emphasized the unity of the sciences in a time of rapidly progressing specialization, but also—and this was among its most distinctive features as compared to *Nature*—the philosophical and cultural context of the sciences. To a large extent, this orientation was the product of the singular nature of Arnold Berliner, who ran the journal from 1913 until he was forced out in 1935 under the Nuremberg laws. Berliner was both one of the early technical physicist, who had worked as a factory director, and a "*Kulturmensch*", who venerated Goethe and was one of Gustav Mahler's closest friends. From the recollections of Wilhelm Westphal (1952) we learn that for the younger Berlin physicists Berliner was an intellectual father figure not unlike what Exner had become for his circle.

The impressive number of philosophical articles solicited by Berliner can be divided into two groups. On the one hand, he ran a kind of education program by publishing survey papers on Goethe, Kant, Schopenhauer, etc. On the other hand, he published papers in which scientists reflected on the conceptual and philosophical foundations of their most recent achievements, among them most papers of the causality debate and many papers and reviews penned by Logical Empiricists. Conversely, until 1930 those Logical Empiricists who had a science background published roughly a third of their papers in *Die Naturwissenschaften.* (For further figures, see Stöltzner (2000).) This proves that among the readers of this journal neo-positivism was by far less fringy than Forman assumed.

Berliner's journal unequivocally took sides in two debates that were of great importance for science as a whole in the early years of the Weimar republic. First, in the struggles about relativity theory that had intensified after 1918, *Die Naturwissenschaften* became an important stronghold in the "defense belt" (Hentschel 1990) around Einstein. This debate shaped the philosophical understanding of modern physics, be it relativistic or quantum, and prompted physicist-philosophers to take a stand against public accusations.

Second, Berliner's journal devoted two papers to a severe criticism of Oswald Spengler's views on biology and physics. One of the critics, the applied mathematician Paul Riebesell, had already participated in the relativity debates. (Riebesell 1916) As did the Vienna Indeterminists, Riebesell accepted statistical laws as genuine laws. This even permitted him to turn the tables against Spengler. "Science—not the philosophy of nature—will now as before stick to the principle of causality and will approach precisely Spengler's problem of the predetermination of history with its new methods. For, by means of statistical laws—which *Spengler* incidentally does not recognize as mathematical laws—one has already successfully analyzed those mass phenomena, which historical questions are all about." (Riebesell 1920, 508) Thus Riebesell drew terminological consequences from the present state of physics and the social sciences, but not in an act of adaptation. In a certain sense, the break between Spengler's cultural morphology and the quantitative social science mentioned by Riebesell was even more radical than the one between Spengler's concept of causality and statistical mechanics.

In evaluating the sociological impact of this confutation of Spengler for the German scientific community, we have to consider that *Die Naturwissenschaften* was closely connected to two leading research organizations of the German-speaking world, the *Deutsche Gesellschaft der Naturforscher und Ärzte* and the *Kaiser-Wilhelm-Gesellschaft.* In virtue of this authoritative character, I have considered (Stöltzner 2000, 2005) *Die Naturwis-*

senschaften as a scientifically modernist submilieu that provided scientists with a cultural identity more specific than just being *Bildungsbürger*, such that they did not face the general milieu directly without a stabilizing group identity. Today, however, I believe that the concept of milieu—even in the operationalist sense of Fleck (1929)—is too unspecific and ultimately forces us to accept the alternative that Forman had posed at the beginning of his study, to wit, retrenchment versus adaptation. To my taste, all this sounds too passive for physicist-philosophers. Moreover, after 1900 there existed no longer a homogeneous intellectual milieu that could, as a whole, change under the influence of the lost war. As I will outline in the final section, one has to take a multi-layered approach instead. Thus I would now say that *Die Naturwissenschaften* simply provided a forum for those who endorsed scientific modernism, which could mean different things in different disciplines, and considered science as an integral part of the general culture, towards which scientists heeded different attitudes. Much of this orientation of *Die Naturwissenschaften*, especially in the fields of physics and philosophy, was due to the singular nature of Arnold Berliner and his embedding into the Berlin scientific elite.

A characteristic element of the conduit of physicist-philosophers was, a few notable exceptions notwithstanding, their independence from ruling philosophical schools. This permitted them to form strategic alliances. Let me first provide an abstract characterization this notion. A strategic alliance was formed if there was a set of basic philosophical convictions that a group of physicists considered as central in order to further their philosophical agenda within a particular intellectual, social or disciplinary context. In this case, they confined their disagreements to internal discussions, even though in retrospect these may appear substantial. Strategic alliances dissolved and their members regrouped, as the convictions considered pivotal within the respective context underwent changes. It is important to stress that the philosophical ambitions of a physicist-philosopher were not exhausted by the intersection constituting this strategic alliance. Or to cast it into Fleckian terms, the members of a strategic alliance were typically members of different thought collectives.

It seems to me that both Logical Empiricism, at least initially, and the Göttingen-Copenhagen interpretation of quantum mechanics represented strategic alliances of this kind. When Logical Empiricists joined up with the latter, the tradition of Vienna Indeterminism ended in the mid 1930s because this move alienated Schrödinger from Frank and von Mises. The reasons were at least twofold. For one, Logical Empiricists decided to combat the metaphysical misinterpretations of quantum theory by developing an empiricist reading of Bohr's complementarity. To do so they invoked a verificationist criterion of meaning that, in its language-oriented version, was unacceptable to Schrödinger who sought for universally valid pictures rather than concepts with a limited domain of applicability. More generally, after the EPR-paper of 1935 and Schrödinger's (1935) cat paper, the discussions about the interpretation of quantum mechanics shifted from causality and indeterminism to questions of reality in the atomic domain.

But there was also a sociological element. By organizing specific meetings, through the foundation of their own journal *Erkenntnis*, with their search for international allies, and through a debate whether their distinctive method consisted in the logic of science (Carnap), scientific philosophy (Reichenbach), or encyclopedism (Neurath), Logical Empiricists after 1930 accomplished the basic steps in establishing a new scientific discipline. Through this process of discipline formation, the role model of physicist-philosopher, albeit still existing, lost its pervasiveness in the German-speaking world. The further course of the new discipline "philosophy of science" was not to be without implications for how the Forman thesis was cast.

Contextualizing the Forman Thesis

In this section, I first want to show that the Forman thesis is a child of its days in more than one respect; not only by revealing a strong, or even causal, influence of social factors on the conceptual structure of empirical science—a perspective that would prove most influential during the 1970s and beyond. Forman's treatment of the relationship between physical theory and cultural milieu was also deeply informed by what was common to both Rudolf Carnap's philosophy of science and Thomas S. Kuhn's revolutionist perspective on the historiography of science. As did most members of their respective disciplines, both focused on theory and in doing so they treated a scientific theory as a single and largely homogeneous entity. This, so I claim, prevented Forman and others from conceiving the causality debate in its larger historical context and its proper discursive mode, and made him disregard the philosophical continuities beyond how the protagonists of his study, after 1918, used philosophical keywords, such as 'causality', 'crisis' and 'Spengler'.

In the famous debates with W.V. Quine, Carnap (1950) had argued that questions about the existence of scientific objects were only meaningful once a linguistic framework had been specified. While Carnap continued to hold that the choice of a framework was guided by pragmatic concerns, Kuhn's (1962) *Structure of Scientific Revolutions* pointed out that at certain moments in history, scientific revolutions overthrew an old conceptual framework and instated a new one. The main point of Kuhn's argument, at least in its original form, was that the old and new frameworks were incommensurable, such that there was no rational bridge from one paradigm to the other. Kuhn's book has often been understood as the final blow to a philosophy of science in the Vienna Circle style even though it had received Carnap's endorsement. For, both agreed that there existed not meta-framework that could justify the transitions between two frameworks as rational. Their main difference was that Carnap denied strong versions of the theory-ladenness of observation, such that the brute facts always provided a bridge between two paradigms even if there was no theoretical bridge between the old and the new paradigm. Both their consensus and disagreement hence concerned the relationship between one or two theories and unstructured empirical data.

The subsequent debate among philosophers centered around whether the history of science could be rationally reconstructed—as Lakatos and Popper held—or whether it was essentially contingent—as Feyerabend came to radicalize Kuhn's analysis. This debate was still in its early phase when Forman's paper came out. Seen from perspective of the philosophical frontline between Lakatos and Feyerabend in its mature form, we find an interesting ambiguity in Forman's thesis. On the one hand, by claiming a causal influence of the post-war milieu he argued in favor of historical contingency in the style of Feyerabend. On the other hand, Forman held that after 1927 there was sufficient reason to abandon causality and thus assumed, in contrast, the rationality of scientific development. Forman, it becomes clear, was not a social constructivist.

In her *Quantum Dialogue*, Mara Beller (1999) has made the case that Forman's thesis parallels the Kuhnian picture and that he accordingly has written the history of the winners. On her account, it was mainly Bohr's power politics that changed a fruitful continuous dialogue into a revolutionary narrative. In the form of deBroglie's pilot wave theory, she stressed, the possibility of a causal quantum mechanics had always existed. While I agree with Beller's paralleling Forman and Kuhn, I doubt that the radical counterposition between dialogical emergence and rhetorical consolidation—so the two parts of her book—does justice to the rather stable philosophical convictions of the protagonists of her narrative because it downgrades them to justificatory rhetoric.

Again motivated by the deBroglie-Bohm theory, Jim Cushing (1994) has rightly interpreted quantum mechanics as a case of Duhemian underdetermination. But he additionally construed a counterfactual history showing how the causal picture could have prevailed, filing thus an equal rights claim for alternative interpretations of quantum mechanics. This move was justly criticized among others by Beller (1999) and Forman (1995). Once again, we find the above-mentioned confrontation between one or two theories and empirical data. No wonder that Cushing stressed that philosophical motives were of little importance, apart perhaps from positivism's role in justifying the Copenhagen dogma. Both Beller and Cushing identify philosophy with guilded or academic philosophy and thus miss the peculiar role model of physicist-philosopher that lies at the heart of my reconstruction of the causality debate.

It seems to me, in contrast to Forman, Beller and Cushing, that the causality debates among German physicist-philosophers can only be assessed by departing from a multi-layered structure of beliefs and attitudes encompassing general philosophical principles, mathematical formalisms, specific theories, personal research agendas and cultural self-identities that evolved and changed on different time scales. Let me integrate my story into this picture.

I have claimed that the causality debate extended across roughly three decades, from Exner's 1908 inaugural address until the discussions ensuing from the EPR-paper. There was one thought collective, Vienna Indeterminism, whose members were not forced to change their philosophical principles, neither in 1918, when a deep political crisis began, nor in the face of the growing problems in atomic physics that had started around 1920, nor after the recognition of the strange features of the new quantum theory that Born's interpretation of the wave function brought to light. In Schlick's case we have seen that for those who, unlike Schrödinger, fully endorsed the new quantum mechanics, the philosophical reorientation was eased by the fact that indeterminism had already been an option widely discussed. If we look at physicist-philosophers, such as Frank, von Mises and Schlick, we see that the orientation at a Machian or Kantian conception of causality, that lives on an even longer time scale, proved to be an enormously stable philosophical disposition. Schlick, in particular, needed much longer to abandon the Kantian category of causality than he did for those of space and time in the context of relativity theory.

I have remained largely silent about the mathematical levels involved in my story. But I have to mention at least one of them, not least because it demonstrates that I am not writing a winners' history myself. In spite of the important role of probability for the causality debate, the most important breakthrough in the field came only in 1932 with Kol'mogorov's axiomatization that in virtue of its abstract nature avoided the problems that plagued Fechner's and von Mises's statistical collectives. (See Hochkirchen 1999) This achievement only made clear that the problem of quantum probabilities was not fully resolved and that a full-blown frequentism as advocated by the Vienna Indeterminists does not do the job.

It remains to be seen at further historical examples whether the higher-level principles in the above structure, the philosophical and the mathematical ones, may exert such a strong force on concept formation that they can be considered historically relativized a prioris, as Michael Friedman (2001) has suggested in order to save the Kuhnian insight from social constructivism, or whether they only mediate across fractures in the conceptual development that occur on another level. The latter claim, it seems to me, can be reconciled with Fleck's (1935) insight that a single scientist may simultaneously belong to different thought collectives. Such was the tack taken in the present analysis because it permitted me to incorporate also other thinkers, such as Reichenbach and Nernst, who neither were part of Vienna Indeterminism nor shared Planck's insistence of determinism or Schlick's separation between lawfulness and randomness.

Finally, it is important to note that the existence of local traditions, such as Vienna Indeterminism, does not contradict the integrity of the causality debate and the debates occurring in *Die Naturwissenschaften* in so far as they took place within a single but multifarious German-speaking scientific culture. For this reason, I am happy to observe that Forman considers the Vienna tradition no longer just as a "subterranean anticausality current" (Forman 1971, 67) but as part of a broader Austro-Hungarian tradition in which positivist tendencies were pivotal and which had its roots long before 1918. (Forman 2007, 40) I hope to have shown that also the stance of the Weimar participants in the debate—both thematically and socially—was not primarily a product of the fall of the Wilhelminian Empire but shaped by earlier philosophical commitments that had been defined in a struggle about Boltzmann's legacy statistical mechanics.

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