

# Forman Thesis and the Reception of Quantum Mechanics: towards a pluralistic methodology in the history of science?

Forman's thesis<sup>1</sup> creates a direct link between the early history of quantum mechanics and the context of Weimar republic. More precisely, Forman claimed that the Weimar physicists and mathematicians accommodated themselves to a Spenglerian point of view and, as a consequence, they rejected causality. Forman also claims that the unanimous attitude towards a rejection of causality is not due to any *internal* developments of physics (and, particularly, not the specificity of quantum theory). He wants to correlate the acausal features of quantum mechanics with *external* factors.

I choose to separate Forman's claims into a stronger and weaker thesis. The stronger claim asserts that the acausal character of internal developments of physics (specifically, the particular form and content of the theories involved) is motivated by broader cultural factors at play during the Weimar republic. The weaker claim would find the historical and cultural context responsible for an explanation of the different features of the *reception* of the early quantum mechanics.

Roughly, the difference between the two interpretations of Forman's thesis would subsume to saying that following the stronger reading, extrinsic factors (such as the prevalent anti-rationalistic context) which led physicists to actively search for *specifically acausal theory* were the principal factors in the development and rise of the early quantum mechanics in Germany, while taking a weaker a reading, one is only interested in whether the German physicists were more prone to adopt *an acausal interpretation* of the same body of equations.

In my paper I review strong points of criticism against the stronger claim. In view of this critique, the second claim will also need be reformulated: what sort of historical contingencies prevailed in the societal shaping of the development of early quantum mechanics? Based on recent scholarship, there seems to be good grounds for methodological pluralism in approaching this question, and I try to suggest possible answers following Beller (1999) and Cushing (1994).

## 1. Forman thesis.

At the center of Forman 1971 paper is the empirical evidence that physics - traditionally considered a discipline whose content and activity are rigorously constrained by experiment and highly

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<sup>1</sup> Forman 1971, 1984.

mathematical standards- “is led into paths which run perfectly parallel to the paths of the intellectual movements in the other areas of life”. Forman’s starting point is Jammer’s appreciation for the philosophical background for modern quantum mechanics: some ideas of the late nineteenth century which were united in rejecting causality though on different grounds<sup>2</sup>. Forman is set to prove some stronger and far-reaching theses for the German intellectual climate of the Weimar republic: “extrinsic influences led physicists to ardently hope for, actively search for, and willingly embrace an acausal quantum mechanics”. One of his boldest claims is that the cultural climate<sup>3</sup> (the postwar national crisis – social, economical, political) affected the generation of physicists: they abandoned classical notions of causality and fully embraced determinism and more irrationalist lines of thought<sup>4</sup>.

## 2. Direct replies to Forman’s approach.

The stronger thesis in Forman’s articles stirred a considerable echo and there were considerable objections to his conclusions. John Hendry’s “Weimar Culture and Quantum Causality” points out that the *adaptation* of the mathematicians and physicist to the milieu one “must look to their work and to their private correspondence”<sup>5</sup>. Sommerfeld’s number mysticism, for example, does represent an accommodation to the milieu, but nothing in its content relates to the same context<sup>6</sup>. Another objection raised by Hendry is the qualification that causality was rejected only by a handful of physicists and mainly determined by internal developments. Most importantly, he mentions that a discussion on the rejection or adoption of causality as a result of the 'influence of the milieu' is keeping discussion at a naive level. For Weyl and Reichenbach, to give but an example, the matter of causality was complex and the classical deterministic (or lawfulness) seemed "naive". On this line of the argument, it is hard to decide on pure external consideration what is the relation between a theory and causality.

Radder (1983) argues for a shift in the problem raised by Forman (1971) by analyzing "the only really important and influential acausal theory in the period under consideration", i.e. the Bohr-Kramers-Slater theory. He also extends the causal sociological explanation sought after by Forman beyond the level of the (true or false) beliefs of individual scientists, to the (mostly intended) structural

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<sup>2</sup> Jammer, Max. 1966. *The Conceptual Development of Quantum Mechanics*. New York: McGraw-Hill. Chapter 4.

<sup>3</sup> Stern (1961) and Harrington (1996)

<sup>4</sup> “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.”(Forman 1971, p 7)

<sup>5</sup> Forman 1971

<sup>6</sup> Set 2008 and Seth 2010 contain a comprehensive interpretation of Sommerfeld’s “number mysticism”.

effects of these beliefs, which in this case were adaptations to the Weimar intellectual milieu. This milieu does not resume to ideas, but also to social practice (exemplified by the reform of the secondary school curricula).

Kraft and Kroes (1984) succeed in identifying a series of problematic points in Forman's article (1971). For example, they note that Forman assumes a direct connection between quanta and causality, while, in fact, the coupling of quanta with acausality has become standard only *after* 1927 (and so Forman "illicitly projects it back in time")<sup>7</sup>. Kraft and Kroes also criticize the quasi-behaviouristic view with regard to the way physicists and mathematicians ("and via them the content of their science") were influenced by external factors.

### **3. Departing from Forman.**

According to the previous section, the stronger thesis encounters insurmountable problems. But the question remain: how does one hope to account for the historical contingencies in the successful *reception* of the Copenhagen interpretation? From the point of view of the methodology in approaching the answer to this question, I focus on two variants. Both Cushing (1994) and Beller (1999) suggest the subject of reception is better to be referred to as single "Copenhagen spirit" instead of uncontroversial and single "Copenhagen interpretation". Cushing (1994) argued for a sophisticated historical contingency of the Copenhagen interpretation in which acausal character is definitely divorced from the mathematical formalism.

Beller (1999) examines the quantum revolution from the perspective of an ongoing conversation between the major participants. By illuminating the polyphony in the Copenhagen interpretation, she dissolves one such standing dogma: the Copenhagen paradigm was supposedly built on two fundamental epistemological theses (indeterminism and a revision of the classical notion of reality). However, Beller shows that under closer scrutiny the paradigm "has neither coherence, nor stability, despite the mass of rhetoric by Bohr and his followers asserting its "inevitability".

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<sup>7</sup> The fundamental problem raised by quantum mechanics as a mechanics of quanta was the opposition between "discreteness" and "continuity". It was only the acausal Bohr-Kramers-Slater theory that might help Forman's argument. However, the reception of this particular one split physicists into camps, and even in the case of those who were enthusiastic about it, such as Schrödinger and Born, it was not as an external pressure to adopt an acausal theory. (Beller 1999 also presents other reasons and motivations)

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