

Historical polyphony and strategies of historians of science: Some reflections

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In the collection of essays on Michael Frayn's *Copenhagen*, which appeared in German a few months ago,¹ I argued that Frayn's piece is a masterful example of what I call **historical polyphony**. Assuming that many of you may not have read this volume, let me summarize the basic idea of my essay, and then elaborate on why—in my opinion—writings on the history of science so rarely exhibit this polyphony.

In a nutshell, my point was that even after taking into account *all* available documentation of a given complex historical event, such as the meeting between Bohr and Heisenberg in September 1941, irresolvable ambiguities remain about what really happened. Versions by Bohr, Heisenberg, and still others (Aage Bohr, Stefan Rozental)² partially conflict with one another, without there being any clear-cut way of coming to a definite decision. This does not exclude personal preferences for one of these versions, but assignments of their likelihood will differ from person to person. (The situation is curiously analogous to a state function Ψ in quantum mechanics: In general, prior measurement, we can only assign probabilities a_i^2 to a set of orthonormal eigenstates Ψ_i with $\Psi = \sum_i a_i \Psi_i$ and $\sum_i a_i^2 = 1$.) It is misleading to assume that the quantum mechanical system already **is** in one of these eigenstates, and that it is only us who don't yet know which one, because 'reduction of the wave-packet' only occurs upon actual measurement. As counter-intuitive as this violation of naive realism is in quantum mechanics, as difficult it is to construe the consequences of its analogue in historiography. Michael Frayn has achieved a remarkable '**historical polyphony**', as I would like to call it, by not restricting himself to just *one* version, but playing out *three* versions in succession, each one broadly consistent with the available documentation (insofar as the latter is not contradictory or ambiguous in itself).

Unlike Jan Golinski, I do *not* regard Frayn's play as "history of science narrative"³—in my opinion, this would be a category mistake. But, as I express towards the end of my paper for the anthology on 'Copenhagen' I do hope that historians of science will be inspired by Frayn's play, and take more seriously the implications of such an inherently probabilistic situation, about which we—probably—never will be able to say definitely what 'really happened'. This might sound premature: Towards the end of the symposium on 'Copenhagen and beyond' it was announced that all of Bohr's later written statements about the meeting with Heisenberg (including an unsent draft of a letter to Heisenberg) will be released. Welcome though this news is, I don't

¹Michael Frayn: *Copenhagen: Zwölf wissenschaftshistorische Lesarten zu Copenhagen*, Göttingen: Wallstein Verlag, 2001, appendix, pp. 175-181.

²See the pertinent quotes in Helmut Rechenberg's contribution to the appendix mentioned in footnote 1, pp. 210ff.

³Quote from the title of Golinski's contribution to the symposium on 'Copenhagen and beyond'.

expect these documents to settle the issue as definitely as some scholars such as Gerald Holton and Bohr's family seem to think, because we will only hear Bohr's side of the story. We will have gained one more version, one more kaleidoscopic perspective, to which yet others might be added later in time. As authoritative as each source may be in its own right, it remains a party, burdened with its own interests and its own context, its own temporal and spatial locality. No doubt, there are plenty of historical events with documentation as ambiguous as the Bohr-Heisenberg meeting, or even worse. Nevertheless, so far there are very few (if any) good examples of such historical polyphony implemented in the history of science literature. Why?

Let me go immediately to the heart of the matter: namely the **basic strategies** of historians (not only historians of science, by the way) in researching their subject matter and in composing their narrative. We are trained to investigate our topic as exhaustively as possible: whether an individual for a biographical study or an institution, a discipline or a particular issue. As each finding is made, our view of it is constrained further, more and more possibilities are excluded and less and less variants remain. The driving force behind this endeavor is to produce a "fully documented", a "definitive" historical account: These are revealing adjectives that often appear in the promotional materials for our finished products and indicative of the somewhat 'naive realism' with which a 'correspondence' between historical sujet and historian's narrative is attempted. Please note: I am not speaking about the actual publication, which will inevitably be imperfect, (as we colleagues are always eager to point out,) but rather about the guiding ideal.

Imagine a **biographer**, targeting his or her 'hero'. As each new document surfaces, the contours of this subject becomes more and more distinct; expectations are confirmed or refuted. The mental image gradually takes shape and there are fewer and fewer surprises as the author's intuition about the protagonist's character and actions improves. Late 19th-century historiography has provided further backing for this approach by demanding deep empathy and even virtual re-living of historical events.⁴

Of course, historiography has not remained where Wilhelm Dilthey had left it, but I dare to say that in practice this goal of stepping into the shoes of past thinkers (*sich Hineinversetzen*) still governs the overwhelming majority of biographical works. Unlike in literature, where Sartre's biography of Flaubert has found many admirers, in the history of science one rare exception to this rule is David Nye's 'anti-biography' of Thomas A. Edison.⁵ Nye tries to show how not only Edison himself, but also his contemporaries and later biographers, "invented his self", in various rather conflicting ways: Edison as the Victorian husband, eccentric scientist, amateur tinkerer, elegant

⁴See, e.g., Wilhelm Dilthey: *Der Aufbau der geschichtlichen Welt in den Geisteswissenschaften*, posthumously publ. 1927 as Dilthey's *Gesammelte Schriften*, vol. 7; see especially sec. II: 'Das Verstehen anderer Personen und ihrer Lebensäußerungen' about the demands of *Sich Hineinversetzen* und *Nacherleben*.

⁵David E. Nye: *The invented self: an anti-biography, from documents of Thomas A. Edison*, Odense: Odense Univ. Press, 1982.

magician, or secretive alchemist. Here we have historical polyphony, albeit on a modest scale. However, Nye's study also exhibits some significant weaknesses, one being that it is very difficult to read and far too theoretical in its approach. It appeared in a small press (actually here in Denmark at Odense Universitet), and is unlikely to ever reach a broader public—such as you—which tells us something about public expectations with regard to history writing in general, and biographies in particular!

Perhaps **psychobiography** would be an alternative? Aren't we all familiar with that curious state of mind before a clear direction has been formed in our thoughts and actions? To quote Frayn's Bohr facing his visitor:

Bohr: He stands on the doorstep blinking in the sudden flood of light from the house. Until this instant his thoughts have been everywhere and nowhere, like unobserved particles, through all the slits in the diffraction grating simultaneously. Now they have to be observed and specified.

Heisenberg: And at once the clear purposes inside my head lose all definite shape. The light falls on them and they scatter.

As promising as this avenue sounds in principle, most psychobiographies of scientists that I know of have more or less given away the chance to explore along these lines inherent ambiguities and polyvalencies of concepts or actions in *statu nascendi*. Frank Manuel's biography of Isaac Newton, for instance, shares the shortcoming of Freudians in being too monocausal, too much focused on Newton's fatherless early childhood, trying to mold his character from these traumatic influences. Developmental psychology at least offers a quasi-evolutionary sequence of developmental stages, but within each of these personality sketches there is no space left for historical polyphony either. No psychobiography of a scientist that I know of fully explores the wide range of potentials that a personality might have developed, given slightly different circumstances or constraints, or the various ways in which a certain episode might have developed.

This is more than mere lack of documentation and abhorrence of counterfactual history: It is as if we historians rather follow the plot of Jean-Paul Sartre's play 'Les jeux sont faits', in which a dead revolutionary gets another chance at life only to find that he and his lover are trying in vain to avoid the tracks of the former plot. So he is killed again under similar circumstances. Historians of science, "hugging the shores of fact, paddling in the safe shallows of honesty" (to quote from Michael Frayn's novel *Headlong*),⁶ likewise aim for maximal determination, eschewing the very soil on which historical polyphony might grow: which would be a representation space in which neither of the three classical axioms: uniqueness, separability and inevitability, hold. One of my favorite books in the history of science literature is Russell McCormach's 'Night thoughts of a classical physicist'. McCormach

⁶London, Faber and Faber, 1999, p. 99, a novel in which Frayn pokes fun at the obsessions of our colleagues, the art historians. Historians of science might well be the subject of another novel by Frayn in the near future, as our symposium will have provided him with an ample set of oddities.

even artificially construes such uniqueness in his fictitious physicist Victor Jakob, based on findings among the papers of a dozen turn-of-the-century scientists. The documentation would not have been sufficient for any one of these real historical figures to be able to draw such a vivid portrait of a character so ridden with nagging doubts about himself and his discipline's future, tragically ending in suicide. But McCormmach's narrative strategy, as admirable as it is in itself, is the obverse of what historical polyphony would amount to, namely an unraveling of a *multitude* of permissible versions from the limited documentation about just *one* historical actor or episode.

There is another limit to standard historiography: we normally channel the development of our historical topic of study into quasi-evolutionary strands; we might perhaps even study the interaction of two or three such strands, but in narrative modes that are still linear in the sense explained in Mark Walker's announced contribution to the symposium. The same limitation also applies to studies on scientific disciplines: they describe stages of formation, birth, acceptance, dissemination and possibly decay, or death. How to get away from the linear sequencing which ultimately underlies all these efforts at 'chronicling'?

One option might be Mara Beller's '**dialogical historiography**'. I would like to emphasize, though, that her central goal (as I understand it) is different from mine. She wants to avoid the rhetorics of inevitability and finality that not only governs the winner's rendition of the development of quantum mechanics, but (as she shows compellingly) also features in many writings by historians of science.⁷ The polyphony resuscitated by Beller is the "polyphony of the creative act", in other words, the open-endedness and ambiguity of emergent science in flux. It is *not* a polyphony of competing, and in a way complementary readings and representations of past events by historians. Her quite aggressive, combative book leaves the impression that she wants to *refute* Jammer's, Mehra-Rechenberg's or Pais's versions of the emergence of quantum mechanics instead of having *supplanted* them with yet another reading.

Another strategy—occasionally—pursued by historians is the **comparative approach**: By comparing two or even more cases they turn away from the idiosyncracies of each of them and either identify recurrent patterns or differences that might single one of them out.⁸ In our instance, Dieter Hoffmann has unearthed documents about a trip by Walter Grotrian to Norway in 1940: here we have another German scientist in another territory occupied by German troops during World War II. Michael Eckert has compared the Heisenberg–Bohr dialogue to a fictitious one between two musicians in Lion Feuchtwanger's novel 'Exile'. Just like Heisenberg, Feuchtwanger's fictional character Leonhard Riemann, with obvious parallels to Wilhelm Furtwängler (1886–1954), is no Nazi, but a high-profile intellectual. Despite an 'unpolitical' self-image,

⁷See Mara Beller: *Quantum Dialogue*, Chicago Univ. Press, 1999 for her account of the 'dialogical emergence' of quantum mechanics, and chap. 15 for her 'historiography of dialogism'.

⁸For a good literature survey on historical comparisons in general, and for examples of generalizing and individuating comparisons, see, e.g., Hartmut Kaelble: *Der historische Vergleich: eine Einführung zum 19. und 20. Jahrhundert*, Frankfurt am Main: Campus, 1999.

both are liable for compromising themselves morally in a context aptly described by Herbert Mehrrens as inadvertently collaborationist (“Kollaborationsverhältnisse”).

However, a strong asymmetry between the compared units weakens both of these historical comparisons, interesting though they are. So they widen the horizon, but: Is the visit by the astrophysicist Grotrian really on a par with Heisenberg’s visit to Bohr? Grotrian apparently had received the order to contact Norwegian geophysicists about the Tromsø Northern Lights Observatory. He went there wearing a German air force uniform, and with a relatively clear goal, namely to obtain locally gathered data on the ionosphere, which were of some relevance for military wireless communications. Heisenberg’s visit, on the other hand, was far more complex and potentially wide-ranging in its implications. What can be usefully compared are Grotrian’s and Heisenberg’s strategies of justification after the war, but other levels remain absolutely disparate, because of the overall asymmetry. Eckert’s comparison with the fictitious conversation between two musicians tends to reduce the Bohr-Heisenberg-meeting to such mundane issues as Heisenberg’s career and worries about the fate of his field, for which there are parallels in the otherwise quite different sphere of music. Feuchtwanger’s dialogue is situated in the exile environment of a shabby hotel, while the dialogue between the two physicists took place in an occupied country. Both comparisons thus only selectively illuminate a few of the many levels on which the meeting between Bohr and Heisenberg can and must be seen. Frayn’s dialogues, by contrast, manage to keep the intricate balance between so many different levels, ranging from natural philosophy to personality traits, and from nuclear physics to social constellations.

Another historiographic strategy is to turn to **metahistory**: instead of producing yet another version of the same episode, we limit ourselves to mere (metahistorical) comparisons of various versions of it found in the literature. Frequently studied subjects such as, say, Niels Bohr, have been portrayed quite variously by different historians, physicists, philosophers, and journalists. Bohr the philosopher-scientist (in the older literature), Bohr the father figure (in writings by his pupils and friends), Bohr the subjectivist (Popper) versus Bohr the realist (Murdoch) versus Bohr the pragmatist (Folse), Bohr the Höfding pupil (Jan Faye: pro, versus David Favrholt: contra), Bohr the science politician (Finn Aaserud), Bohr the master guru with brilliant, aggressive disciples (John Heilbron’s paper on the “earliest missionaries of the Copenhagen spirit”), and most recently, Bohr the skillful rhetorician, orchestrating the Potemkin façade of a coherent Copenhagen interpretation (Mara Beller).⁹

This strategy of moving onto the meta-level without committing yourself to any of these readings may help dislodge fixations on the currently dominant interpretational

⁹For details and references see my historiographic survey of writings on Bohr in Arne Hessenbruch (ed.) *Reader’s Guide to the History of Science*, London: Fitzroy Dearborn Publ., 2001, pp. 92-94. Mara Beller’s book: *Quantum Dialogue*, Cambridge Univ. Press, 1999 had not yet appeared when I wrote this survey. Heilbron’s paper first appeared in the *Revue d’Histoire des sciences* **38** [1985] pp. 1895–230, and was reprinted in E. Ullman-Margalit (ed.) *Science in Reflection*, Dordrecht: Kluwer, 1987, pp. 201–233.

strand, but to me it remains unsatisfactory to relegate all first-order history to ‘the others’ and to limit your own work to critical juxtapositioning of their various versions of historical episodes or characters. Sure, we can endlessly compile and contextualize different visions of, say, Humboldt, Bohr or Einstein (David Cassidy once wrote such a comparative analysis of Einstein biographies), but such a second-order historiographic survey is not the historical polyphony I am searching for: on the ground floor, not on the elevated second level of historiography, so to speak.

Perhaps the last resort is not to demand that any *single* historian reach historical polyphony, but that several ‘monophonic’ tunes be carefully orchestrated in a setting such as Finn Aaserud’s symposium. But a lot then hinges upon setting the right pitch to all players to prevent a cacophony of individual voices. Neither simple alphabetical ordering, as in the Dörries volume nor mere chronology in the case of collections of primary documents will suffice. What I tried to do in my anthology on *Physics and National Socialism* (Basel: Birkhäuser, 1996) was to combine strident and mild voices, famous and unknown physicists, students and professors, staunch patriots and disillusioned emigrés, etc. And yet, any such selection of a few hundred documents from among the millions of possible ones is an enormous funneling of the material, a limitation of the tonal range that some of you might find unsatisfactory or even questionable. At least there remains a deeply engrained distrust in ready-made choices—like some of the scholars here still seem to question Frayn’s choice and sequencing of just these three versions among so many other possible scenarios of the Bohr–Heisenberg meeting.

To conclude: it seems we have still not found adequate ways to arrive at what Mark Walker called “truly non-linear accounts” in historical writing. Finding suitable narrative modes to express “**historical polyphony**” (as I prefer to call it) still lies ahead of us historians— and correspondingly, accepting these attempts at polyphonic accounts, instead of clinging to the popular streamlined versions, lies ahead of you, the reading public.

Dr.habil. Klaus Hentschel
Institute for History of Science
Univ. of Göttingen
e-mail: khentsc@gwdg.de
<http://www.gwdg.de/~khentsc>

Mailing Address:

Dr.habil. K. Hentschel,
Postfach 2216
D-37012 Göttingen, Germany