Feyerabend contribution to modern computational microphysics

Discuss 1967 Paul Feyerabend (PKF) lecturing to Stanford and UC Berkeley physicists based on correspondence between Feyerabend and Imre Lakatos from the Lakatos Archive at London School of Economics. Argues that PKF's mid 1960's anti-formalist methodology was believed. Starting in the 1970s the formalisms became the theories leading to current difficulties and illusionary results in micro-physics. Correspondence shows contribution to QM beyond PKF's 1968-1969 *Phil. Sci.* **'Niels Bohr's World View'.**

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July 30, 1967 to Lakatos on Stanford and Berkeley lectures (IL 13/272 120)

The last two weeks I spent ... going back and forth between Stanford and Berkeley, lecturing in both places. I met Wigner, he attended my first lecture ... On the whole I try to convince my audience that Hilbert and Von Neuman *ruined physics* **which apparently no one is prepared to swallow in spite of my splendid argument**

July 30, 1967 letter continued ...

(main point: the formalisms are very <gross?>, we can now *prove* in the formalism what before we accepted on the basis of a guess, but as a result the correlation to experience becomes so loose that it is sometimes almost becoming metaphysics). Interspersed in my lectures on quantum theory are wicked remarks and arguments against philosophy of science in the Suppes fashion.

September 8, 1967 - more criticism of Von Neumann formalism (13/272 unmarked)

I shall have to say a few very critical things about Von Neumann, NOT about his subjectivism, but about the disservice he did to physics by trying to make the theory precise (Bohr never liked this - by the way Bohr is not at all as muddled as Karl represents him) ...

PKF continues since he may be delayed at Mayo Clinic missing his first scheduled lecture suggesting a topic for Lakatos.

Maybe a talk about formalization etc. would be excellent.

Earlier March 3, 1967 to Lakatos on physics accepting informal complementarity

PKF responding to paper by Lakatos criticizing complementarity -

Bohr expresses it as impossibility of any sharp separation between behaviour of atomic object and the interaction with the measuring instrument.

I see no axioms here, PKF continues

You are unfair to ***complementarity*****. ... I try to show that Bohr's approach while certainly not fully satisfactory (what theory ever is) is the best thing we have.**

Continuing explaining complementarity wide acceptance ...

Even Einstein **when commenting on Bohr's solution of EPR call it** *the best available solution.* **He does not like it, but sees its merits. ... Bohm (who frankly admits that Bohr has got a point) ... Schrodinger (who said to me "I hate to admit it, but Bohr has got something), etc.**

Also PKF argues Bohr already was using complementarity pre 1925 ..

I shall send you a MS where I try to show that the philosophy of complementarity guided Bohr already in his pre-1925 research, only it was not then made explicit.

Background situation in mid 1960s;

1. **Idea that waves needed a medium abandoned - Einsteins relativity.**
2. **Idea that inside the atom behavior was deterministic in Newton's sense - given up.**
3. **Some kind of complementarity needed was accepted.**
4. **Some kind of phenomonolgy was needed - cloud chamber tracks.**
5. **SLAC Linear Collider just started in 1966 to explore inside the nucleus.**
6. **Schlipp Volume 1949 with Einstein biography published and Einstein archive becoming accessible.**
7. **Thomas Kuhn's interviews for the Archive for the History of Quantum Physics (AHQP) published.**

PKF as not contributing to physical theory

1. **Szovil in his *Ein Philisoph aus Wien* essay states PKF's contributions to physics were minor (p. 77) and quotes Kurt Fischer's that UC Berkeley physicists saw PKF as "merely two decades behind current research".**
2. **Szovil writes incorrectly that PKF did not argue "against the quantum logic introduced by Birkoff and Von Neumann" (p. 76).**
3. **Why did Eugene Wigner attend non contributor PKF lecture.**
4. **Szovil quotes PKF as saying "there was no essential difference between a physicist and a good philosopher." (p. 77)**
5. **I interpret this as PKF having studied the development of quantum theory saw the 20 year lag in adopting formalism as a positive.**

Mathematicians also criticized theoretical physicists for anti-formalism

From Arne Schirrmacher's essay '"Theoretiker" zwischen mathematischer und experimenteller Physik' in *Max Planck und die Moderne Physik* **(ed. D. Hoffman), Harvard mathematician Jaffe is quoted as writing "It is mathematically unethical not to maintain the distinction between casual reasoning and proof." (p. 35) Schirrmacher interprets this as claiming "physicists omit correct rigorous proof, rather they engage in casual reasoning and loose thinking."**

Mathematician’s criticism continued ..

In 1913, tactful Max Planck wrote to David Hilbert saying Hilbert’s axiomization of Kirchoff’s radiation law were unsuitable. (p. 43).

I think Einstein’s incompleteness of QM can be interepreted as reply to mathematicians (Hilbert?) that QM is incomplete in the formal mathematical sense (missing something - points, sets, classes, or maybe formulas?). This is my interpretation of Arthur Fine’s early essays "The Young and Old Einstein" and "Einsteins critique of quantum theory" in *The Shaky Game: Einstein Realism and the Quantum Theory.*

Intellectual climate around 1967 (my recollection):

1. **Wolfgang Panofsky would say the physics is the math, but William Shockley disagreed in his Freshman seminar discussions.**
2. **Advice to young math students (from Polya, but he was not teaching then) was to not bother learning formal logic.**
3. **The older physicists used Bohr's style of linguistic explaination. Particularly Shockley and Bloch (see later slides).**
4. **Something changed in the early 1970s. PKF moved to "Against Method", Lakatos moved from quasi-empirical philosophy of math to MSRP philosphy of science. Theories because their axiomaticized expression.**

PKF’s view of Bohr’s anti-formalism versus Sommerfeld

The difference between Bohr and Sommerfeld is best described as the difference between a mathematical physicist who is content with formally satisfactory and factually adequate equations and a philosopher who looks beyond success and who realizes the need for a sense of perspective, even in the face of the most surprising confirmations. (’Niels Bohr’s World View, p. 271).

Also quotes Bohr caution on quantum theory in 1923, ’always to remember the [limited] domain of application of the theory, especially at the present state of science.’

Bohr Anti-Formalist View from 1954 Columbia University Lecture "The Unity of Knowledge"

The general lesson of the role that mathematics has played through the ages in natural philosophy is the recognition that no relationship can be defined without a logical frame and that any apparent disharmony in the description of experience can be eliminated only by an appropriate widening of the conceptual framework. This lesson, familiar to mathematicians, and conspicuous in studies in the foundations of their science, has been enforced by the development of physics in a way that a bearing on many other fields of human knowledge.

Felix Bloch’s anti-math:

And I chose Utrect--I think that was partly also Pauli’s influence, maybe also partly Heisenberg’s. Both were somewhat critical of Goettingen spirit; that is, Born’s school and approach were considered highly formal and mathematical (Kuhn’s AHQP interview, p.29 1930 decision).

I had the impression that group theory is something tremendously import. Later on, I didn’t think so much of it any more, but at the time I did. (AHQP, p.34 on 1929-1931 research).

Heisenberg’s complimentarity particle matrix to wave equation transformation

The dualism between the two complimentary pictures - wave and particles is clearly brought out in the flexibility of the mathematical scheme. The formalism is usually written out to resemble Newtonian mechanics. ... But by a simple transformation it can be written out to resemble a wave equation [Schrodinger’s] for a ordinary three-dimensional matter wave (*Physics and Philosophy***, p. 50).**

Dedekind cut real definition versus Cantor’s

Current formal mathematics accepts Cantor’s definition of real numbers as sets as superior to Dedekind’s 1959 cut line definition (See Golderi, D. 1996, *Classic Set Theory for Guided Independent Study,* **p. 8-17). But Cantor’s definition requires an equivalence class calculation to create unique numbers (p. 19-21).**

It seems to me that this path to Zermalo-Frankel existence as able to be formally generated is exactly the formalism the founders of modern physics opposed and the reason for current illusions in micro-physics.

Finsler Viewed Mathematical Foundations Empirically

See *Finsler Set Theory: Platonism and Circularity***, Booth, D. and Ziegler R. (eds.), 1996.**

Finsler called himself a platonist but the term realist would be used in physics.

Finsler contrasts with Hilbert's theory of mathematical proofs (1923, p. 50):

1. **A Proof is an array which must be graphically representable in its entirety.**
2. **A formula is [mechanically] provable if it is an axiom or arises by substitution.**

Finsler (1926, p. 55): ´[a] proof becomes free from objection as soon as it is transferred from the formal symbolism into pure thoughts .. abstracted from its formal expression.' *(Bohr's method?)*

Finsler thought what was to become Tarski style meta-mathematical truth wrong

From 1996 *Finsler set theory: platonism and circularity*, **'Are there contradictions in mathematics', written 1923, p. 44.**

1. **Write 1, 2, 3, and x on the board.**
2. **Define x to be the smallest natural number not written on the blackboard.**
3. **We might expect x to exist but it does not.**
4. **If x equals 4 then x equals 5, but if x equals 5 then x equals 4**

Finsler’s incompleteness proof - logical systems are illusions (chimera)

[in 1926] .. I showed that in formal systems of a general kind one can specify propositions which are not decidable by means of formal proofs within the systems, but which nevertheless can be decided by their conceptual content (Finsler, 1944, p. 63).

Finsler’s result preceded, but Godel’s incompleteness results were accepted because they were specific to formal systems (Russell’s originally).

PKF implicitly expressed Finsler view in a Dec. 27, 1964 letter to Lakatos using quasi-empiricsm of proofs. ’The fate of Russells program ought to have overcome Hilbert’s even without Godel.’

My expericens late 1970s RAM alpha particles really pattern sensitivities

In semiconductor manufacturing in late 1970s periodic out breaks of computer RAM failures.

Explanation was atomic - alpha particles were cause. Circuits were redesigned to be alpha particle resistent.

But background alpha particles were orders of magnitude too small.

No data was released but I thought problem wes pattern sensivity either from combinatorics or electro-magnetic interference.

Problem of bad science versus incomplete theory determination.

Description of LHC Methodology Problems from Edinburgh Festival Session

1. **Discovery based on fewer than 10 particles observed per year. What if only 3 are discovered next year. Or what if hundreds some year?**
2. **Boson discovered is light (126 gev) so too small to be Higgs by 6 times.**
3. **Phenomenology requires electronic circuit to filter tracks. Computer program is based on plasma calorimetry (outside QM).**
4. **Best experimental opportunity for real super-symmetric extra dimensions to exist.**
5. **Best opportunity for bringing back Bohr anti-formal complementarity.**

Formalism dominance political - 1972 UK Lighthill Report (p. 19):

Research on AI in some other countries may be funded by military agencies (ARPA in the USA) or by other mission-oriented public bodies. With this type of funding it is common for Scientists to 'close their ranks' and avoid public disagreement among themselves, in the hope that the total funds available for science may thus be enhanced to an extend that may outweigh any harmful results of a distribution of those funds determined on the basis of insufficient scientific discussion.

Werner Heisenberg description (*Physics and Philosophy*, p. 167)

[...] the scientific problems have finally become connected with political issues, and some scientists have taken recourse to political methods to carry their views through.

Need for bringing back PKF microphysics

Some phenomenology obviously good MRIs, some ridiculous such as NP completeness as physical law.

There really should be at least 4 types of complementarity: wave mechanics calculating electron orbits, wave mechanics Bohrian conceptual theory, Bohrian conceptual theory of the nucleus, matrix theory particle property calculations in the nucleus.

Finsler's empiricism suggests there should be informal but mathematical structures that can provide more complete explanations.

More future microphysics ...

QM has been mostly relegated to explanatory mode after atoms were understood in the 1930s. But recently measurements purely based on formal mathematics are occurring: entanglement from the Von Neumann matrices or from "information" theory. Those measurements need philosophical analysis that does not assume the formal axioms.

The physics of plasmas that is neither the physics of electron orbits nor of nucleus states may offer an opportunity for natural philosophy that leads to new discoveries.

Lakatos Criticism of Feyerabend:

[...] On studying your *Against Method* **I am increasingly worried. To my mind you have a basic weakness in your position which is at least as bad as mine. If you were consistent you would have the courage to be a sceptic. For the first time to my knowledge, you now say that epistemological anarchism cannot be equated with scepticism. If so, I shall prove that epistemological anarchism is double faced. One face is the face of a sceptic, the other is the face of a Kuhnian authoritarian. I am terribly sorry about this, but you either return to complete scepticism or I shall show that you are inconsistent.**

Feyerabend criticism of Lakatos:

So--forget about rationality and find out what it was that made everyone accept Einstein's research programme and abandon Lorentz's. 'Everyone,' this means a few big shots in England, Germany, France, for the rest are content with the Lorentz transformations and $E=mc^2$ tacked onto it; that is, they are content with some purely formal tricks and would not even know the difference between Einstein and Lorentz. That Lorentz turns out to be not ad hoc at all, but progressive, pleases me very much and cheers me up on an otherwise rainy day.

Recent Nick Maxwell Hopos post giving PKF's 1960s method

I am of the view that the splitting of natural philosophy into science and philosophy was a bit of an intellectual disaster - especially for philosophy. Philosophy of science ought to be an integral part of science. This becomes clear the moment it is appreciated that, inherent in the aims of science, there are problematic assumptions concerning metaphysics, values and politics. The task of articulating and improving aims and methods - something philosophy of science should surely seek to do - needs to be undertaken as an integral, influential part of the scientific enterprise.