INTUITION AND THE AXIOMATIC METHOD

INTUITION AND THE AXIOMATIC METHOD

Edited by
RENATE HUBER
Universität Dortmund, Germany
EMILY CARSON
McGill University, Canada

Kluwer Academic Publishers
Boston/Dordrecht/London

Contents

Preface

Thanks for having a critical look, October 30, 2003

RENATE AND EMILY

SOFT AXIOMATISATION: JOHN VON NEUMANN ON METHOD AND VON NEUMANN'S METHOD IN THE PHYSICAL SCIENCES

Miklós Rédei
Loránd Eötvös University, Budapest, Hungary

Michael Stöltzner
Universität Bielefeld, Germany

One can discern two typical attitudes towards von Neumann's achievements in the physical sciences: the *appreciative* and the *ambivalent.*

Members of the appreciative camp view and evaluate von Neumann's work as the typical representative of the successful application of the axiomatic method in the physical sciences. Members of the ambivalent group acknowledge von Neumann's results as great intellectual achievements but view his work in physics as an example of useless and pointless striving for mathematical exactness for exactness' own sake.

To quote a representative of the appreciative camp, the mathematician Paul Halmos relates:

> The 'axiomatic method' is sometimes mentioned as the secret of von Neumann's success. In his hands it was not pedantry but perception; he got to the root of the matter by concentrating on the basic properties (axioms) from which all else follows. The method, at the same time, revealed to him the steps to follow to get from the foundations to the application. (Halmos (1973), p. 394)

In a similar vein, Arthur Wightman, one of the most prominent representatives of modern mathematical physics, writes:

> I do not know whether Hilbert regarded von Neumann's book as the fulfillment of the axiomatic method applied to quantum mechanics, but, viewed from afar, that is the way it looks to me. In fact, in my opinion, it is the most important axiomatization of a physical theory up to this time. (Wightman (1976), p. 157)

It is more difficult to document the presence of ambivalence towards von Neumann's work by quoting explicit statements; evidence in favor of the case is more circumstantial (absence in physics curricula of von Neumann's achievements, paying lip service to von Neumann's results without actually analysing precisely the content and significance of his results, historians of physics not paying attention to his role in the development of 20th century physics, etc.). Yet we think this attitude is even more widespread than the appreciative one. This ambivalent attitude is an expression of physicist's general understanding of what sort of a science physics is and how a physicist is supposed to proceed within his profession. Richard P. Feynman represents an influential case in point; his views were typical of a whole generation of quantum field theorists who found themselves amidst a set of spectacularly successful rules of computation which, however, involved blatantly inconsistent mathematical objects.

> The mathematical rigor of great precision is not very useful in physics. But one should not criticize the mathematicians on this score . . . They are doing their own job. If you want something else, then you work it out for yourself.
>
> Some day, when physics is complete and we know all the laws, we may be able to start with some axioms . . . so that everything can be deduced. But while we do not know all the laws, we can use some to make guesses at theorems which extend beyond the proof. (Feynman (1965), p. 56, 49)

A similar attitude also prevails in some statements of John S. Bell whose famous inequalities supplanted Von Neumann's No-hidden-variable theorem. Rather than just criticizing Von Neumann's concept of hidden variable as inadequate, Bell considers the axiomatic introduction of this concept as suggesting a kind of a priori reasoning or a "law of thought" (Bell (1987), p. 32, cf. Stöltzner (2002b)).

Surprisingly, even a mathematician such as Hermann Weyl, who was equally interested in foundations and applications of mathematics endorsed Feynman's evaluation of the role of axiomatisation in physics. About Hilbert's program of axiomatisation of physics, Weyl writes:

> The maze of experimental facts which the physicist has to take into account is too manifold, their expansion too fast, and their aspect and relative weight too changeable for the axiomatic method to find a firm enough foothold, except in the thoroughly consolidated parts of our physical knowledge. Men like Einstein or Niels Bohr grope their way in the dark toward their conceptions of general relativity or atomic structure by another type of experience and imagination than those of the mathematician, although mathematics is an essential ingredient. Thus Hilbert's vast plans in physics never matured. (Weyl (1944), p. 653)

Even Wolfgang Pauli, generally known for his insistence on precision in physics, is reported to have doubts about von Neumann's work: Walter Thirring, another prominent active mathematical physicist recalls a conversation he had with Pauli about von Neumann's significance.

> For a long time his [von Neumann's] importance for physics was underrated, Pauli once told me that he had said to v. Neumann: "If a mathematical proof is what matters in physics you would be a great physicist". I disagree with this statement, I think he had the right vision of what will become important in physics. (Thirring (2001), p. 5)

Thirring lists four overlapping areas in physics about which he thinks von Neumann both had the right intuition and to which von Neumann contributed in a decisive way: operators in $\mathcal{B}(H)$, infinite tensor products of von Neumann algebras, quantum statistical mechanics and quantum logic. Thirring's remark also hints at an aspect of von Neumann's work that seems to have escaped even the attention of the appreciative camp, to wit, that von Neumann never saw his achievements in mathematical physics as purely mathematical ones because he did not assume a neat separation between mathematics and the sciences.

Our main claim in this paper is that *both* the appreciative and the ambivalent camps misinterpret and misread von Neumann's views and intention in important respects. A closer look at what von Neumann actually said about the scientific method and at how he actually acted as a working scientist, especially in quantum physics, does not entirely confirm the picture painted by either camps of the role of axiomatisation and of the place of mathematical rigor in von Neumann's thinking and work. By painting a more detailed and, we hope, more faithful picture of von Neumann's views and practice we also hope to show a remarkable continuity in his attitude and method, a continuity in the period from 1926, when he first encountered the idea of the axiomatic method in physics in the Hilbert school in Göttingen, through his remarks in the fifties about the method in the sciences.

1. Von Neumann's Opportunistic Soft Axiomatisation

One can distinguish two different notions of "axiomatising" and "axiomatic theory" in von Neumann's works:

(i) axiomatising and axiomatic theory in the strict sense of formal systems or languages
(call this "formal axiomatics");

(ii) axiomatising and axiomatic theory in the less formal sense in which it occurs in physics
(call this "soft axiomatics").

Formal axiomatics is what von Neumann does in his work on axiomatic set theory (which was the topic of his PhD dissertation in 1926). This formal type of axiomatisation is a standard one, more or less as it is understood today. However, even in connection with formal axiomatics von Neumann takes a very sensible, only moderately formalist position, making clear that there is some intuitively given content or meaning behind the primitive concepts and the axioms in terms of which axiomatic set theory is formulated:

> We begin with describing the system to be axiomatized and with giving the axioms. This will be followed by a brief clarification of the meaning of the symbols and axioms. . . . It goes without saying that in axiomatic investigations as ours, expressions such as 'meaning of a symbol' or 'meaning of an axiom' should not be taken literally: these symbols and axioms do not have a meaning at all (in principle at least), they only represent (in more or less complete manner) certain concepts of the untenable 'naive set theory'. Speaking of 'meaning' we always intend the meaning of the concepts taken from 'naive set theory'. (von Neumann (1962b), p. 344, our translation)[1]

As opposed to formal axiomatics, soft axiomatics is a less well-defined, more intuitive and a structured conception. Its explicit formulation can be found already in the 1926 joint paper by Hilbert, Nordheim and von Neumann on the foundations of quantum mechanics. This paper contains a relatively lengthy passage on the axiomatic method in physics. The main idea is that a physical theory consists of three, sharply distinguishable parts:

(i) physical axioms,

(ii) analytic machinery (also called "formalism"),

(iii) physical interpretation.

The physical axioms are supposed to be semi-formal requirements (postulates) formulated for certain physical quantities and relations among them. The basis of these postulates is our experience and observations; thus the basis of the axioms in physics is empirical. (This is not necessarily the case in formal axiomatics: von Neumann points out that the fifth postulate in Euclid's geometry is non-empirical.)

The analytic machinery is a mathematical structure containing quantities that have the same relation among themselves as the relation between the physical quantities. *Ideally*, the physical axioms should be strong and rich enough to *determine* the analytic machinery *completely*. The physical interpretation connects then the elements of the analytic machinery and the physical axioms.

Here is the idea in the author's words and specified for the case of quantum mechanics, where probability density for the distribution of values of physical quantities is taken as the basic concept:

> The way leading to this theory is the following: one formulates certain physical requirements concerning these probabilities, requirements that are plausible on the basis of our experiences and developments and which entail certain relations between these probabilities. Then one searches for a simple analytic machinery in which quantities appear that satisfy exactly these relations. This analytic machinery and the quantities occurring in it receive a physical interpretation on the basis of the physical requirements. The aim is to formulate the physical requirements in a way that is complete enough to determine the analytic machinery unambiguously. This way is then the way of axiomatising, as this had been carried out in geometry, for instance. The relations between geometric shapes such as point, line, plane are described by axioms, and then it is shown that these relations are satisfied by an analytic machinery, namely, linear equations. Thereby one can

> deduce geometric theorems from properties of the linear equations. (Hilbert, Nordheim, von Neumann (1926), p. 105, our translation)[2]

Hilbert, Nordheim and von Neumann see clearly, however, that not even soft axiomatics is practiced in actual science. They point out that what happens is that one typically conjectures the analytic machinery *first* and *without* having formulated the physical axioms. It is only after the analytic, mathematical part is fixed that one gets an insight into what the physical axioms should be. In their words:

> In physics the axiomatic procedure alluded to above is not followed closely, however; here and as a rule the way to set up a new theory is the following.
>
> One typically conjectures the analytic machinery before one has set up a complete system of axioms, and then one gets to setting up the basic physical relations only through the interpretation of the formalism. It is difficult to understand such a theory if these two things, the formalism and its physical interpretation, are not kept sharply apart. This separation should be performed here as clearly as possible although, corresponding to the current status of the theory, we do not want yet to establish a complete axiomatics. What however is uniquely determined, is the analytic machinery which — as a purely mathematical entity — cannot be altered. What can be modified — and is likely to be modified in the future — is the physical interpretation, which contains a certain freedom and arbitrariness. (Hilbert, Nordheim, von Neumann (1926), p. 106, our translation)[3]

So, to the extent axiomatics is a method practiced in science (physics) it is only this soft axiomatics, and as Hilbert-Nordheim-Neumann point out, even axiomatisations of this kind are typically practiced in a very opportunistic manner with many concessions to the given science's state of formalisation. It seems fair to say then that, according to the Hilbert-Nordheim-Neumann paper, axiomatisation in physics is of an *opportunistic soft kind*, which seems such a soft notion indeed that one may wonder whether such a method should at all bear the name "axiomatisation" and not be called simply "model building". We think von Neumann would agree with this "model building" terminology; in fact, as we shall quote him shortly, he says explicitly that the aim of the sciences is building models.

One might think that the notion of soft axiomatics as formulated in the Hilbert-Nordheim-Neumann paper is mainly Hilbert's idea; after all the paper was based on Hilbert's 1926 lectures on the foundations of quantum mechanics. To some extent, this is indeed the case, and passages similar to the one above in which Hilbert cites geometry (in particular his own *Foundations of Geometry*, Hilbert (1899)) as the model for the axiomatisation of science abound throughout the years following 1900 when he formulated the Sixth Problem in his famous Paris Lecture. But there are two conflicting tendencies in Hilbert's manifold applications of the axiomatic method. There are cases, such as continuum mechanics, where Hilbert attributes only a preliminary status to the system of axioms — because the conceptual framework provided by physics is still far from being a definitive one (see Majer (2001), p. 26). But when it comes to general relativity, or in Hilbert's earlier axiomatizations of mechanics, a strongly reductionist attitude prevails. In these cases Hilbert believed that the conceptual framework was close to its final stage and that the axiomatic method had already succeeded in finding the deepest structural level. Axiomatising in this second sense came very close to formal axiomatics because what remained to be done was the definition of appropriate number fields to reduce the consistency of the physical theory to the consistency of arithmetics. And only in this last step did Gödel's incompleteness theorems come to bear and put an unsurmountable limit to the import of axiomatisation.

Soft axiomatics is hardly affected by the results that restrict formal axiomatics because the axiomatic method taken in the soft sense deals with still preliminary conceptual frameworks and its main objective is to deepen the foundations — *Tieferlegung* in Hilbert's terms (Stöltzner (2002a)). On the basis of von Neumann's remarks on method in Section 4 and 5 it can be argued (Stöltzner 2001) that von Neumann shifted the balance in Hilbert's axiomatic method between pragmatism and foundationalism strongly towards the first tendency. Soft axiomatics so conceived is not driven any more exclusively by the search for universality and is thus less susceptible to results such as Gödel's, which put a principal limit to formal axiomatics. Deepening the foundations then becomes an effective tool for critical analysis rather than a reductionist project.

2. Soft Axiomatisation in Quantum Mechanics

We claim that von Neumann followed the method of opportunistic soft axiomatics in his work on quantum mechanics. To prove the claim in full would require a detailed and lengthy historical analysis of what von Neumann does in his 1927 papers (von Neumann (1927a, b, c)) and in his book Neumann (1932), which cannot be done here (see Rédei (1996, 98, 99, 2001) for some results in this direction). Let us just briefly recall (without presenting the actual wording) the key elements in von Neumann's argumentation. In doing so we will treat the 1926 papers and von Neumann's book on a par although there are revealing and significant differences between the book and the papers — even in chapters of the book that are largely verbatim identical with the papers.

There are only two, explicitly formulated physical axioms, both concern the nature of the expectation value of physical quantities in a statistical ensemble:

A. Expectation value assignments $a \mapsto E(A)$ are linear:

$$E(\alpha a + \beta B + \ldots) = E(\alpha a) + E(\beta B) \ldots$$

B. Expectation value assignments are positive:

$$E(A) \geq 0 \qquad \text{if } a \text{ can take on only non-negative values}$$

These two postulates are informal and are based on empirical observations exactly in the sense in which the Hilbert-Nordheim-Neumann paper talks about physical axioms: The physical quantities a, b are left completely unspecified, and the two postulates spell out something that is a basic, empirically observable feature of expectation value assignments in a relative frequency interpreted probability theory, which von Neumann takes in a rather intuitive sense, without detailing the concept.

The analytic machinery is the set of all selfadjoint operators on a Hilbert space, the third (C) and fourth (D) "postulates" (von Neumann does not even calls them "postulates") specify the physical interpretation, the bridge between the physical quantities and the operators:

C. If the operators $S, T \ldots$ represent the physical quantities $a, b \ldots$ then the operator $\alpha S + \beta T + \ldots$ represents the physical quantity $\alpha a + \beta b \ldots$.

D. If operator S represents the physical quantity a then the operator $f(S)$ represents the physical quantity $f(a)$.

The opportunistic aspect of this soft axiomatisation manifests itself in the fact that postulates A. and B. do *not* imply that the physical quantities must be represented by the set of *all* linear operators on a Hilbert space. One has to, and von Neumann does so indeed, *stipulate* that the physical quantities are represented by the formal machinery of linear operators on a Hilbert space. Hence one knows what properties the physical quantities possess only by inspecting the structural properties of the Hilbert space operators.

From $A + B + C + D$ von Neumann deduces that every expectation value assignment is of the form

$$E(a) = Tr(US) \tag{1}$$

with some statistical operator U (= positive, linear). This formula is the heart of the whole theory, it contains all probability statements; specifically, according to von Neumann's interpretation, the formula (1) yields the probabilities of quantum events:

$$p(P^S(d)) = Tr(UP^S(d)) \tag{2}$$

where $P^S(d)$ is a spectral projection of some observable S with spectral measure P^S, the projection $P^S(d)$ representing the event that observable S takes its value in the set d of real numbers.

This is, in a nutshell, the skeleton of the core of von Neumann's approach to quantum mechanics in the years 1926–1932. Von Neumann realized however that his interpretation of the trace formula was beset with deep conceptual problems: in order to be able to interpret the probabilities $p(X)$ as relative frequencies (in von Mises' sense) the probability assignment $X \mapsto p(X)$ needs to satisfy the following "subadditivity" property:

$$p(X) + p(Y) = p(X \wedge Y) + p(X \vee Y) \qquad \text{for all projections } X, Y \tag{3}$$

where $\wedge$ and $\vee$ are the standard lattice operations between Hilbert space projections. But the subadditivity property is violated by every p defined by a *every* non-trivial statistical operator $U \neq I$; on the other hand, the "probabilities" given by the identity operator as statistical operator are not finite, hence they *cannot* be interpreted as relative frequencies at all.

Von Neumann was struggling with this problem already in his second 1926 paper on the foundations of quantum mechanics and also in his book, and this conceptual problem was the main reason, we claim, why he lost his belief in the Hilbert space formalism by about 1935 (see Rédei (1996) for more details). This is clear in his 1936 work with G. Birkhoff on quantum logic, where Birkhoff and von Neumann suggest the theory of type $\mathbf{II}_1$ factor von Neumann algebras as the proper framework of quantum mechanics. It is worth pointing out that von Neumann's preference for the theory of $\mathbf{II}_1$ factors as the proper mathematical framework of quantum theory was *not* based on any mathematical imprecision in the Hilbert space formalism, nor was it motivated by any discovery of a new physical fact or phenomenon: it was motivated exclusively by informal, conceptual-philosophical difficulties — this we take as another indication that what drove von Neumann's research in physics was *not* his desire to have mathematically impeccable theories but to create theories that, besides being mathematically precise, are also conceptually sound. What better additional proof of this claim could one imagine than the fact that von Neumann realized that even taking the theory of $\mathbf{II}_1$ factor von Neumann algebras as the proper mathematical framework for quantum theory does not solve the problem of how to interpret quantum probability, and in 1936 he finally abandoned the relative frequency view of quantum probabilities altogether:

> This view, the so-called 'frequency theory of probability' has been very brilliantly upheld and expounded by R. von Mises. This view, however, is not acceptable to us, at least not in the present 'logical' context. (von Neumann (1937), p. 196)

(See Rédei (1999, 2001) for further details of von Neumann's post 1932 views on quantum mechanics and quantum probability.)

Further evidence that von Neumann was not at all dogmatic in his insistence on mathematical rigour in physics is provided in the Preface of his 1932 book:

> The object of this book is to present the new quantum mechanics in a unified representation which, *so far as it is possible and useful,* is mathematically rigorous. ... The method of Dirac ... in no way satisfies the requirements of mathematical rigor — *not even if these are reduced in a natural and proper fashion to the extent common elsewhere in theoretical physics.* (von Neumann (1932), p. vii–ix, our emphasis)

It is true, in the late 1940s, Laurent Schwartz succeeded in giving a rigorous mathematical basis to Dirac's distributions. Von Neumann's unwarranted skepticism that such a theory was at all feasible does not fault his mehtodology, but rather confirms it. For, opportunistic soft axiomatics derives large part of its support from the fact that an axiom system can be improved at a later stage and that the deep mathematical significance of certain concepts could gradually come to light. In 1932, the alternative was simply to adopt a mathematically well-entrenched framework, operators in Hilbert space, or one that had no mathematical basis at all. In the absence of the Hilbert space concept, von Neumann would most probably not have objected to Dirac's pragmatic research strategy.

3. Von Neumann on Method in the Physical Sciences

Compare following quotation:

> To begin, we must emphasize a statement which I am sure you have heard before, but which must be repeated again and again. It is that the sciences do not try to explain, they hardly ever try to interpret, they mainly make models. By a model is meant a mathematical construct which, with the addition of some verbal interpretations describes observed phenomena. The justification of such a mathematical construct is solely and precisely that it is expected to work — that is correctly to describe phenomena from a reasonably wide area.
>
> I will further limit myself to saying a few things about procedure and method which will illustrate the general character of method in science. Not only for the sake of argument but also because I really believe it, I shall defend the thesis that the method in question is primarily opportunistic – also that outside of the sciences, few people appreciate how utterly opportunistic it is. (von Neumann (1961a), p. 492)

In his 1955 paper on "Method in the Physical Science", von Neumann goes on to discuss various pragmatic criteria of theory preference which are valid both for the empirical sciences and mathematics. "Simplicity is largely a matter of historical background . . . and it is very much a function of what is explained by it" (von Neumann (1961a), p. 492), to wit, how heterogeneous the material covered by the explanation is. Accordingly, simplicity and unificatory power have to be constantly weighed against each other. Von Neumann attributes surprisingly little importance to whether a fact can be predicted in advance or just explained after the fact has been observed. A theory's capability of dealing with heterogeneity ranks higher; in particular "confirmations in areas which were not in the mind of anyone who invented the theory" (von Neumann (1961a), p. 493). He emphasizes that both these criteria are "clearly to a great extent of an aesthetical nature" (von Neumann (1961a), p. 493). which brings them rather close to the mathematical criteria of success.

Mathematics proper possesses a genuine criterion of success. In "The Mathematician" of 1947 von Neumann writes: "One expects a mathematical theorem or a mathematical theory not only to describe and to classify in a simple and elegant way . . . One also expects 'elegance' in its 'architectural', structural makeup" (von Neumann (1962a), p. 9) e.g., a surprising twist in the argument which immediately makes a point very easy, or some general principle which explains why difficulties crop up and which reduces the apparent arbitrariness. "These criteria are clearly those of creative art" (von Neumann (1962a), p. 9), so that

> the subject begins to live a particular life of its own and is better compared to a creative one, governed by almost entirely aesthetical motivations, than to anything else and in particular, to an empirical science. . . . As a mathematical discipline

> travels far from its empirical source . . . it is beset with very grave dangers. It becomes more and more purely aestheticizing, more and more purely *l'art pour l'art*. (von Neumann (1962a), p. 9)

The field is then in danger of developing along the line of least resistance and will "separate into a multitude of insignificant branches" (von Neumann (1962a), p. 9). "[W]henever this stage is reached, the only remedy seems . . . to be a rejuvenating return to the source: the reinjection of more or less directly empirical ideas" (von Neumann (1962a), p. 9).

While the aesthetic criteria of success in mathematics and theoretical physics are quite similar, von Neumann locates major differences regarding their aims and their actual *modus procedendi*. Even without signs of degeneration, mathematics is more finely subdivided into subdisciplines because often the selection of problems itself is aesthetically oriented. Theoretical physics, on the contrary, is typically highly focused to resolve an internal difficulty or to solve a problem that was posed by experimental results. Once a break-through is reached, "the predictive and unifying achievements usually come afterward" (von Neumann (1962a), p. 8). From this diagnosis von Neumann concludes that

> the problems of theoretical physics are objectively given; and, while the criteria which govern the exploitation of a success are . . . mainly aesthetical, yet the portion of the problem, and that which I called above the original 'break-through', are hard, objective facts. (von Neumann (1962a), p. 8).

Thus instead of furnishing an absolute foundation, mathematics plays a central role in the sciences and in society — perhaps still as central as Hilbert had believed — by being aesthetically-oriented and opportunistic.

> I feel that one of the most important contributions of mathematics to our thinking is, that it has demonstrated an enormous flexibility in the formation of concepts, a degree of flexibility to which it is very difficult to arrive in a non-mathematical mode (von Neumann (1961b), p. 482)

In the two examples von Neumann discusses in the 1955 paper, namely classical mechanics and quantum mechanics, further aspects of von Neumann's opportunistic soft axiomatics come to the fore; they show the pragmatic virtues of a mathematization *not* driven by foundationalism.

In connection with classical mechanics von Neumann argues that there are two mathematically equivalent formalisms of the theory: One can either set up a second-order differential equation which locally describes the dynamical evolution, or one can apply the Principle of Least Action over a finite time interval, i.e. globally. Von Neumann identifies the first formalism with a "teleological", and the second one with a "causal" mode of description. Since the two are strictly equivalent mathematically, von Neumann declares the problem of whether processes are causal or teleological, as a pseudo-problem:

> Newton's description is causal and d'Alembert's description is teleological. . . . All the difference between the two is a purely mathematical transformation. . . . Thus whether one chooses to say that classical mechanics is causal or teleological is purely a matter of literary inclination at the moment of talking. This is very

> important, since it proves, that if one has really technically penetrated a subject, things that previously seemed in complete contrast, might be purely mathematical transformations of each other. Things which appear to represent deep differences of principle and of interpretation, in this way may turn out not to affect any significant statements and any predictions. They mean nothing to the content of the theory. (von Neumann (1961a), p. 496).

Von Neumann also had a rather opportunistic attitude concerning the philosophical issues in quantum theory. He writes:

> [W]hile there appears to be a serious philosophical controversy between the interpretations of Schrödinger and Heisenberg, it is quite likely that the controversy will be settled in quite an unphilosophical way. The decision is likely to be opportunistic in the end. The theory that lends itself better to formalistic extension towards valid new theories will overcome the other, no matter what our preference up to that point might have been. It must be emphasized that this is not a question of accepting the correct theory and rejecting the false one. It is a matter of accepting that theory which shows greater formal adaptability for a correct extension. This is a formalistic, esthetic criterion, with a highly opportunistic flavor. (von Neumann (1961a), p. 498)

4. Von Neumann on Mathematical Rigour

In his writings about pure mathematics and mathematical rigour von Neumann also takes a very down-to-earth, relaxed attitude, taking the position that mathematical rigour is changeable and has changed several times in the course of history of mathematics. Nevertheless, in order to be of pragmatic value, an appropriate standard of rigour had to be maintained.

> Whatever philosophical or epistemological preferences anyone may have in this respect, the mathematical fraternities' actual experiences with its subject give little support to the assumption of the existence of an a priori concept of mathematical rigor.
>
> I have told the story of this controversy [about the foundations of mathematics] in such detail, because I think that it constitutes the best caution against taking the immovable rigor of mathematics too much for granted. This happened in our lifetime, and I know myself how humiliatingly easily my own views regarding the absolute mathematical truth changed during this episode, and how they changed three times in succession! (von Neumann (1962a), p. 6)
>
> [I]t is *not* necessarily true that the mathematical method is something absolute, which was revealed from on high, or which somehow, after we got hold of it, was evidently right and has stayed evidently right ever since. To be more precise, maybe it *was* evidently right after it was revealed, but it certainly didn't stay evidently right ever since. There have been very serious fluctuations in the professional opinion of mathematicians on what mathematical rigor is. To mention one minor thing: In my own experience, which extends over only some thirty years, it has fluctuated so considerably, that my personal and sincere conviction as to what mathematical rigor is, has changed at least twice. And this is in a short time of the life of one individual! (von Neumann (1961b), p. 480)

The last change in the concept of rigour was in a certain respect the most radical one. After Gödel there existed no longer any absolute foundation for mathematics in the purely algorithmic sense which Hilbert had in mind, and Gödel's proof was valid under all current definitions of mathematical rigour including the intuitionist one. Von Neumann was one of the first to admit defeat. Hilbert's program of formal axiomatics was — though not based on wrong intentions — simply unfeasible. But soft axiomatics was not threatened by this wreckage because it had never attempted to create an absolute foundation. And accordingly von Neumann simply went on to provide an axiomatic foundation of quantum mechanics. While such a strategy might have been provisionally acceptable Hilbert (who never really commented in depth on Gödel's results), von Neumann's conclusions about rigour are more radical, and he combines mathematics and the sciences in a manner that was very far from Hilbert's repeated talk about a non-Leibnizian pre-established harmony between mathematics and physics. In von Neumann's hands, the ontological problem became a pragmatic one. In his 1947 "The Mathematician" he writes:

> The main hope for justification of classical mathematics — in the sense of Hilbert or of Brouwer and Weyl — being gone, most mathematicians decided to use that system anyway. After all, classical mathematics . . . stood on at least as sound a foundation as, for example, the existence of the electron. Hence, if one is willing to accept the sciences, one might as well accept the classical system of mathematics. (von Neumann (1962a), p. 6)

It would be wrong to infer from the above quotations that von Neumann subscribed to the view that rigour can be no more than a sociological criterion. To von Neumann's mind, mathematics — although any particular set of basic propositions can be doubted — "establishes certain standards of objectivity, certain standards of truth . . . rather independently of everything else" (von Neumann (1961b), p. 478). The source of this objectivity does not contradict the historical fact that many non-rigorous arguments were accepted — either with a certain sense of guilt or due to *bona fide* disagreements as to whether a particular proof was really a proof. Already fluctuations in the style of proofs can come close to differences in rigour.

This yields surprising consequences for mathematical ontology. "The variability of the concept of rigor shows that something else besides mathematical abstraction must enter into the makeup of mathematics" (von Neumann (1962a), p. 4). The "something else", as indicated already in the quotation comparing the degree of mathematical certainty with that of an electron, is empirical science: "The most vitally characteristic fact about mathematics is . . . its quite peculiar relationship . . . to any science which interprets experience on a higher than purely descriptive level" (von Neumann (1962a), p. 1). This relationship has two sides: On the one side,

> [i]n modern empirical sciences it has become a major criterion of success whether they have become accessible to the mathematical method or to the near-mathematical methods of physics. Indeed, throughout the natural sciences an unbroken chain of pseudomorphoses, all of them pressing toward mathematics, and almost identified with the idea of scientific progress, has become more and more evident. (von Neumann (1962a), p. 2)

On the other side, “[s]ome of the best inspirations of modern mathematics (I believe, the best ones) clearly originated in the natural sciences” (von Neumann (1962a), p. 2). Von Neumann quotes the examples of geometry and analysis as two branches of mathematics that have empirical origins and show that history was richer than a linear increase of abstractness and rigour. In his view an adequate appraisal of former inexact results of these disciplines is not reached by merely stressing that they satisfied the standards of the day and that the gaps were filled in later years. Thus in the end, soft axiomatics might appear as a fourth change in the concept of rigour — at least for the domain of mathematical physics.

5. Summary

Mathematical precision and rigour without conceptual clarity was for von Neumann neither possible nor desirable either in physical sciences or in mathematics. It seems justified to say that what drove von Neumann in his research, especially in physics, was the desire to achieve conceptual clarity and formulate conceptually consistent theories. Von Neumann’s work on quantum mechanics and especially his abandoning the Hilbert space formalism corroborates this interpretation to a large extent. In arriving at acceptable theories von Neumann was relying on the method of an opportunistically interpreted soft axiomatics, a method of axiomatisation which was not affected by Gödel’s results. Von Neumann himself, when speaking of the method in physics, emphasized that the aim of theoretical physics is to create mathematical models. His success in creating powerful mathematical models in physics was due to his unparalleled skill and talent in combining algebraic-combinatorial techniques with analysis.

Acknowledgement: Miklós Rédei’s work was supported by OTKA (contract numbers: T 035234, T 032771 and TS 040899). Michael Stöltzner’s work was supported by the Volkswagen Foundation.

Notes

1. “Wir beginnen mit der Beschreibung des zu axiomatisierendes Systems und mit der Angabe der Axiome. Eine kurze Erläuterung des Sinnes der einzelnen Symbole und Axiome lassen wir nachfolgen. . . . Es ist übrigens selbstverständlich, daß man bei axiomatischen Untersuchungen, wie die unsere, die Ausdrucksweise ‘Sinn eines Symbols’ oder ‘Sinn eines Axioms’ nicht wörtlich nehmen darf: diese Symbole und Axiome haben (in Prinzip wenigstens) keinerlei Sinn, sie vertreten nur (in mehr oder minder vollständiger Weise) gewisse Begriffsbildungen der unhaltbar gewordenen ‘naiven Mengenlehre’. Wenn wir von ‘Sinn’ sprechen, so ist damit also stets der Sinn der ersetzten Begriffe der ‘naiven Mengenlehre’ gemeint.”

2. “Der Weg, der nun zu dieser Theorie führt, ist folgender: Man stellt gewisse physikalische Forderungen an diese Wahrscheinlichkeiten, die durch unsere bisherigen Erfahrungen und Entwicklungen nahe gelegt sind, und deren Erfüllung gewisse Relationen zwischen den Wahrscheinlichkeiten erfordern. Dann sucht man zweitens einen einfachen analytischen Apparat, in dem Größen auftreten, die genau dieselben Relationen erfüllen. Dieser analytische Apparat, und damit die in ihm auftretenden Rechengrößen, erfahren nun auf Grund der physikalischen Forderungen eine physikalische Interpretation. Das Ziel ist dabei, die physikalischen Forderungen so vollständig zu formulieren, dass der analytische Apparat gerade eindeutig festgelegt wird. Dieser Weg ist also der einer Axiomatisierung, wie sie z. B. in der Geometrie durchgeführt worden ist. Durch die Axiome werden die Relationen zwischen den geometrischen Gebilden, wie Punkt, Gerade, Ebene, beschrieben, und dann gezeigt, dass diese Relationen gerade ebenso bei einem analytischen Apparat, nämlich den linearen Gleichungen erfüllt sind. Dadurch kann man wieder umgekehrt aus den Eigenschaften der linearen Gleichungen geometrische Sätze gewinnen.”

3. “Das oben angedeutete Verfahren der Axiomatisierung wird nun in der Physik gewöhnlich nicht genau so befolgt, sondern der Weg zur Aufstellung einer neuen Theorie ist, wie in der Regel, so auch hier, folgender.
Man mutmaßt meistens den analytischen Apparat, bevor man noch das vollständige Axiomensystem aufgestellt hat, und kommt dann erst durch die Interpretation des Formalismus zur Aufstellung der physikalischen Grundrelationen. Es ist schwer, eine solche Theorie zu verstehen, wenn man diese beiden Dinge, der Formalismus und seine physikalische Interpretation, nicht scharf genug auseinanderhält. Diese Scheidung soll hier möglichst deutlich durchgeführt werden, wenn wir auch, dem jetzigen Zustand der Theorie entsprechend, noch nicht eine vollständige Axiomatik begründen wollen. Das, was jedenfalls eindeutig festliegt, ist der analytische Apparat, der — rein mathematisch — auch keiner Abänderung fähig ist. Was dagegen modifiziert werden kann, und voraussichtlich auch noch werden wird, ist die physikalische Interpretation, bei der eine gewisse Freiheit und Willkür besteht.”

References

Bell, J.S. (1987), *Speakable and Unspeakable in Quantum Mechanics*, Cambridge University Press.

Feynman, R. (1965), *The character of physical law*, MIT Press, Cambridge, Mass.

Halmos, P. (1973), “The legend of John von Neumann” in: *American Mathematical Monthly* **50**, 382–394.

Hilbert, D. (1899), *Grundlagen der Geometrie*, Teubner, Leipzig (English translation: Foundations of Geometry, Open Court, La Salle, IL, 1971).

Hilbert, D., Nordheim, L. and J. von Neumann (1926), “Über die Grundlagen der Quantenmechanik” in: von Neumann (1962/I), 104–133.

Majer, U. (2001), “The Axiomatic Method and the Foundations of Science: Historical Roots of Mathematical Physics in Göttingen (1900–1930)” in: Rédei, Stöltzner (2001), 11–33.

Rédei, M. (1996), “Why John von Neumann did not like the Hilbert space formalism of quantum mechanics (and what he liked instead)” in: *Studies in History and Philosophy of Modern Physics* **27**, 493–510.

Rédei, M. (1998), *Quantum Logic in Algebraic Approach*, Kluwer Academic Publishers, Dordrecht.

Rédei, M. (1999), “Unsolved problems in mathematics’ J. von Neumann’s address to the International Congress of Mathematicians, Amsterdam, September 2-9, 1954” in: *The Mathematical Intelligencer* **21**, 7–12.

Rédei, M. (2001), “John von Neumann’s concept of quantum logic and quantum probability” in: Rédei, Stöltzner (2001), 153–172.

Rédei, M. and M. Stöltzner (eds.) (2001), *John von Neumann and the Foundations of Quantum Physics* (Vienna Circle Institute Yearbook 8), Kluwer, Dordrecht.

Stöltzner, M. (2001), “Opportunistic axiomatics: John von Neumann on the methodology of mathematical physics” in: Rédei, Stöltzner (2001), 35–62.

Stöltzner, M. (2002a), “How Metaphysical Is ‘Deepening the Foundations’ — Hahn and Frank on Hilbert’s Axiomatic Method” in: M. Heidelberger and F. Stadler (eds.) (2002), *History of Philosophy of Science. New Trends and Perspectives*, Kluwer, Dordrecht, 245–262.

Stöltzner, M. (2002b), “Bell, Bohm, and von Neumann: Some Philosophical Inequalities Concerning No-go Theorems and the Axiomatic Method” in: T. Placek and J. Butterfield (eds.) (2002), *Modality, Probability, and Bell’s Theorem*, Kluwer, Dordrecht, 37–58.

Thirring, W. (2001), “J. v. Neumann’s influence in mathematical physics” in: Rédei, Stöltzner (2001), 5–10.

von Neumann, J. (1927a), “Mathematische Begründung der Quantenmechanik”, *Göttinger Nachrichten*, 1–57; in: von Neumann (1962/I), 151–207.

von Neumann, J. (1927b), “Wahrscheinlichkeitstheoretischer Aufbau der Quantenmechanik”, *Göttinger Nachrichten*, 245–272; in: von Neumann (1962/I), 208–235.

von Neumann, J. (1927c), “Thermodynamik quantenmechanischer Gesamtheiten”, *Göttinger Nachrichten*, 245–272; in: von Neumann (1962/I), 236–254.

von Neumann, J. (1932), *Mathematische Grundlagen der Quantenmechanik*, Springer Verlag, Heidelberg (English translation by R. T. Beyer, Princeton Universtity Press, 1955).

von Neumann, J. (1937), “Quantum logics (strict- and probability logics)”, unfinished manuscript, John von Neumann Archive, Libarary of Congress, Washington, D.C., reviewed by A. H. Taub in: von Neumann (1962/IV), 195–197.

von Neumann, J. (1961/VI), *Collected Works Vol. VI. Theory of Games, Astrophysics, Hydrodynamics and Meteorology*, A.H. Taub (ed.), Pergamon Press, Oxford.

von Neumann, J. (1961a), “Method in the physical sciences” in: von Neumann (1961/VI), 491–498.

von Neumann, J. (1961b), “The Role of Mathematics in the Sciences and in Society” in: von Neumann (1961/VI), 477–490.

von Neumann, J. (1962/I), *Collected Works Vol. I. Logic, Theory of Sets and Quantum Mechanics*, A.H. Taub (ed.), Pergamon Press, Oxford.

von Neumann, J. (1962a), “The Mathematician” in: von Neumann (1962/I), 1–9.

von Neumann, J. (1962b), “Die Axiomatisierung der Mengenlehre” in: von Neumann (1962/I), 339–422.

von Neumann, J. (1962/IV), *Collected Works Vol. IV. Continuous Geometry and Other Topics*, A.H. Taub (ed.), Pergamon Press, Oxford.

Weyl, H. (1944), “David Hilbert and his mathematical work” in: *Bulletin of the American Mathematical Society* **50**, 612–654.

Wightman, A.S. (1976), “Hilbert’s sixth problem” in: *Proceedings of Symposia in Pure Mathematics* **28**, AMS, 147–240.