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JOHN VON NEUMANN'S ‘IMPOSSIBILITY PROOF’ IN A HISTORICAL PERSPECTIVE

ABSTRACT

John von Neumann's proof that quantum mechanics is logically incompatible with hidden varibales has been the object of extensive study both by physicists and by historians. The latter have concentrated mainly on the way the proof was interpreted, accepted and rejected between 1932, when it was published, and 1966, when J.S. Bell published the first explicit identification of the mistake it involved. What is proposed in this paper is an investigation into the origins of the proof rather than the aftermath. In the first section, a brief overview of the his personal life and his proof is given to set the scene. There follows a discussion on the merits of using here the historical method employed elsewhere by Andrew Warwick. It will be argued that a study of the origins of von Neumann's proof shows how there is an interaction between the following factors: the broad issues within a specific culture, the learning process of the theoretical physicist concerned, and the conceptual techniques available. In our case, the ‘conceptual technology’ employed by von Neumann is identified as the method of axiomatisation.

1. INTRODUCTION

A full biography of John von Neumann is not yet available. Moreover, it seems that there is a lack of extended historical work on the origin of his contributions to quantum mechanics. This comes as a surprise when we realise that what was at stake, at least on the conceptual level, in his proof that hidden variables are impossible in quantum mechanics were major issues in both philosophy and physics: for example, the question of the necessity of causality and the question whether quantum mechanics is so novel in nature that it is fundamentally different from classical physics. In Reichenbach's terminology, a historical study like the one proposed here may focus either on the context of discovery or on the context of justification.[1] When it comes to von Neumann's proof, the latter context, which includes the historical analysis of the repercussions of the ‘impossibility proof’ and the way the physics community reacted to it, has been successfully accomplished by Max Jammer.[2] It is the origin of the proof which will be our focus of attention. In other words, what is proposed here is an answer to the following question: given that this ‘impossibility proof’, in a sense, turned out to be false about 32 years after its publication, what significant factors in its context of discovery would throw light on the way the physics community responded to it?

The first section will offer a brief historical overview of both the man and his ‘impossibility proof’. Since this poof was presented by von Neumann in the context of the intimate relationship between mathematics and physics, it is in our interest to employ a historiographic method appropriate for discussing theoretical physics. It will be recalled how such a method, especially the one employed by Andrew Warwick, can be considered a development of the methods used in discussing experimental physics. To arrive at some understanding of what lies at the origin of von Neumann's proof, we will therefore be investigating in the first place the question of causality as something inherent in the culture at the time. This is classified as a significant broad issue influencing the way von Neumann approached the subject. In the second place, some attention will be directed towards von Neumann's background: his learning process. In the third place, we will identify the theoretical techniques avaibable given the previously mentioned factors. An argument will be put forward to show that, in working out and publishing his proof, von Neumann was doing physics in a particular way dependent upon the mathematical ‘style’ available at the time, namely axiomatisation. This technique of axiomatisation can be traced back to the style of doing physics in Germany in the 1920's.

2. A BRIEF OVERVIEW OF VON NEUMANN'S PROOF

John von Neumann was the eldest of three boys. He was born on December 28, 1903, in Budapest Hungary, at that time part of the Austro-Hungarian empire. His family was well-to-do, and John was educated privately as a small child. In 1927, after spending some time at the University of Göttingen, he became Privatdozent at the University of Berlin. He held this position for three years. During this time he became well known for his publications in set theory, algebra and Quantum Mechanics. This is when he started his two year work on the book which contained the 'impossibility proof'. In 1929, he transferred to the University of Hamburg, and the next year, 1930, he accepted a visiting professorship at Princeton University, lecturing for part of the academic year and returning to Europe in the summers. This is when he married Marietta Kovesi.

In 1931, he became a permanent professor at Princeton, and the next year he published his important book *Mathematische Grundlagen der Quantenmechanik*.[3] This contains the republishing of previous work in a greatly expanded form, but it was here that he devoted some attention to the 'impossibility proof', which had not been discussed in the previous papers and which was later the subject of much controversy. His brilliant contributions to mathematics continued to appear. In 1933, he was invited to join the Institute for Advanced Studies as a professor. His personal life at this time was marked by the birth of his daughter Marina in 1935, by his divorce in 1937, and by his remarriage to Klara Dan in 1938. In 1954, he was named by presidential appointment as a member of the US Atomic Energy commission, holding the chairmanship of the ICBM Committee, but shortly afterwards, his health declined. He died in Washington in 1957.

The ‘impossibility proof’ employes the notion of hidden varibales. To understand the significance of hidden variables we have to refer to the role of statistics in physics. Now, even in classical mechanics, where causality is not challenged, statistics can play a significant role. We can see this if we consider a system with *k* degrees of freedom. The state of this system is known exactly if we know the 2*k* numbers that are necessary: the *k* space co-ordinates $q_1, .., q_k$ and their *k* time derivatives, or in the place of these, the *k* momenta. We can then give the value of each physical quantity uniquely and with numerical exactness. A statistical method, however, can also be applied to such a case, because averaging over gives at least some information when we do not have all the data, but, as von Neumann puts it, «this is, as it were, a luxury or extra addition.»[4] A completely different situation is met in quantum mechanics. Here, for *k* degrees of freedom, the state is described by the wave function $\Psi(q_1, .., q_k)$, or in other words by a vector in the Hilbert space. Even when Ψ is taken to define the state completely, nevertheless, only statistical statements can be made on the values of the physical quantities involved.[5]

Now, in classical physics, the notion of reduction has been very useful in transforming many statistical relations to the causally connected propositions of mechanics. An example of this is the kinetic theory of gases: pressure and temperature can be ‘reduced’ to average values of the independent variables of the 6×10^{23} molecules in 32 grams of oxygen. This is what, in classical physics, explanation by means of the hidden parameters means. A similar procedure may be envisaged for quantum mechanics. If we want to explain the non-causal character of the connection between Ψ and the values of the physical quantities following the pattern of classical mechanics, then we have to say that in reality Ψ does not determine the state exactly. If we manage to obtain a hidden variable theory, then in the words of von Neumann, this fact would «brand the present form of the theory as provisional, since the description of the states would be essentially incomplete.»[6]

Statistical considerations in quantum mechanics employ the two notions of *ensemble* and *dispersion,* which must now be briefly discussed to enable a minimal understanding of the 'impossibility proof'. An *ensemble* is a group of systems. There is a difference between a system as such, and a system in a certain state. An example of a system is a hydrogen atom: an electron and a proton with the known forces acting between them, for which a Hamiltonian function can be written. A state is then determined by additional data. In classical mechanics, this is done by the assigning of numerical values for position and momentum. In quantum mechanics it is done by specifying the wave function $\Psi(q_1, .., q_6)$. If both system and state are known, then the theory gives unambiguous directions for answering all questions by calculation. The investigation of the physical quantities related to a single object or system, *S*, is not fruitful if doubts exist relative to the simultaneous measurability of several quantities. In such cases, it is desirable to observe great statistical *ensembles* which consist of many systems $S_1, ..., S_N$ (in other words, *N* models of *S*, *N* is large). Such ensembles are in general necessary for establishing probability theory as the theory of frequencies.

For ensembles, it is not surprising that a physical quantity $\underline{R}$ does not have a sharp value. This is where the notion of *dispersion* comes in. The distribution function does not consist of a singe value a_o, but several values or intervals of values are possible: this means that a positive dispersion exists. The question of hidden variables arises because we may try to account for the dispersions of the ensembles characterised by the wave functions Ψ by claiming that the ensembles are only mixtures of several states. This in fact entails that, for the knowledge of the actual state, additional data, besides the data of the wave function Ψ, would be necessary. Everything will then be determined causally and, as in the classical case of, for example, gas pressure, the statistics of the homogeneous ensemble would then have resulted from the averaging over all the actual states of which it was composed.

Having said something about the main concepts employed by von Neumann, we may now turn to the ‘impossibility proof’ itself. Although it will not be possible to give an overview of the entire detailed argument, it is very important to indicate its general form, which is essentially deductive. Everyone agrees that the proof is based on well defined postulates, but since von Neumann did not produce a clear list of these postulates, there still seems to be some minor disagreement as to which of his propositions should be considered the axioms. According to Jammer, the proof is based on the following four:

P1: If a quantity (observable) is represented by the operator *R*, then a function *f* of this quantity is represented by the operator *f(R)*.

P2: If quantities are represented by the operators *R*, *S*, ..., then the sum of these quantities is represented by the operator $R + S + \ldots$, regardless of whether the operators commute or not.

P3: If the quantity $\underline{R}$ is by nature non-negative, then its expectation value $<R>$ is non-negative.

P4: If $\underline{R}$, $\underline{S}$, ..., are arbitrary quantities, and *a*, *b*, ..., real numbers, then $<a\underline{R} + b\underline{S} + \ldots> = a<\underline{R}> + b<\underline{S}> + \ldots$

Wigner gives five postulates: he includes one between P2 and P3 consisting of the claim that the correspondence between Hermitian operators and the observables is one to one.[7] This disagreement concerning the number of postulates does not concern us much because what is important here is to realise that, given this system of axioms, von Neumann *deduced*, in a logically rigorous manner, the conclusion that hidden variables are impossible within quantum mechanics.

The form of the argument starts with the statement:

S1: If there are hidden variables, then no dispersive ensemble is homogeneous.

To understand this we remind ourselves that an ensemble, which is a group of systems, is dispersive if the values of observables are not definite but conceivable only within a certain probability distribution. In von Neumann's mathematical terminology, an ensemble is dispersion-free if and only if, for all operators R, $\langle R^2\rangle - \langle R\rangle^2 = 0$.

An ensemble is homogeneous if its statistical behaviour is the same as that of any of its sub-ensembles. In mathematical terms, for ensemble E whose sub-ensembles are E_1 and E_2,

if $\quad \langle \underline{R}\rangle_E = a\langle \underline{R}\rangle_{E_1} + b\langle \underline{R}\rangle_{E_2}$ where $a>0$ and $b>0$, $a + b = 1$

then $\quad \langle \underline{R}\rangle_E = \langle \underline{R}\rangle_{E_1} = \langle \underline{R}\rangle_{E_2}$

Now, statement S1 is the case because if we have dispersion and we have also the right hidden variables, then the dispersion is explained by some states, or sub-ensembles of states, having different values for these variables. This entails that the ensemble cannot be such that all of its sub-ensembles are statistically alike. In other words, the ensemble will not be homogeneous.

von Neumann showed that

(a) every ensemble is dispersive; and

(b) homogenous ensembles do exist.

(In fact, von Neumann showed that ordinary quantum mechanics states describe homogeneous states.)

Hence, from S1, there can be no hidden variables.[8]

This proof, only briefly sketched here, engendered widespread admiration for von Neumann's method. Jammer's historical investigations into its aftermath show that: «[he] was hailed by his followers and credited even by his opponents as having succeeded in bringing the foremost methodological and interpretative problem of quantum mechanics down from the realm of speculation into the reach of mathematical analysis and empirical decision.»[9] von Neumann provided what seemed to be a bulwark protecting the Copenhagen Interpretation against the claim that determinism could be recovered, and in doing so, he was satisfying the needs of the physics community. He legitimised mathematically what the great majority of physicists had quite peacefully accepted as the 'constraint' of the new very powerful theory. It should be remembered that this 'impossibility proof' was just one part of a book which puts the whole of quantum mechanics on a 'proper' mathematical and axiomatic base.

In spite of this initial acceptance, there came a time when serious doubts about the validity of the proof started to be publicly expressed. It is interesting to follow up this process historically. According to many writers, public recognition that somehow von Neumann's formulation cannot have the generality and exhaustiveness which he suggests goes back at least to Bohm.[10] Belinfante claims that no physicist challenged von Neumann's proof before 1952, but in fact, the logical consistency of his postulates had been called into question as early as 1935 by George Temple of King's College, London, who showed that P1 could lead to serious contradictions.[11] Before David Bohm, the validity of the proof was also challenged by philosophers: in 1935 by the philosopher Grete Hermann, whose criticism was unfortunately largely ignored; in 1944 by Hans Reichenbach who challenged the proof on logical grounds.

The first to publicly identify the axiom by which von Neumann's formulation violated the elementary principles of any realistic hidden-variables theory was J.S. Bell. In 1964, Bell discovered that the proof of the theorem depended on the assumption that expectation values are additive — that is to say that the average value of a sum of observed quantities is equal to the sum of the averages of the individual quantities. This is true in quantum mechanics, but need not be true in some other theory.[12]

It is still astounding to realise that the proof enjoyed popularity and respect for such a long time. Belinfante makes the following reflection: «I have always been puzzled how people could have been convinced by von Neumann's arguments that hidden variables could not be introduced. The lack of validity of

$$\langle aA + bB \rangle = a\langle A \rangle + b\langle B \rangle$$

in any decent hidden variables theory would have been obvious to anybody by inspection.»[13] The fact is that it wasn't. The great upheaval in Europe during the Second World War could be one of the reasons. The prominent figures who were against the non-causality of quantum mechanics were von Laue, Planck, Schrödinger and Einstein. By 1933, political problems in Germany reached dramatic proportions, and it seems that no proper physics was done anymore. Von Laue and Planck remain in Germany and try to save what they can. Schrödinger goes first to Oxford, then Vienna, then Dublin. Einstein never returns from the U.S.A.

It is interesting to note that an important issue related to hidden variables, namely the question about the completeness of quantum mechanics, was being discussed during the years just before and just after the publication of von Neumann's *Mathematical Foundations*. This debate on completeness reached its climax with the publication of the famous ‘EPR paper’ of 1935. It is a curious historical fact that there was a nearly total divorce between the EPR argument and the von Neumann proof. The only exception seems to be a paper by W.H. Furry who elaborated the 'impossibility proof' to be able to apply it to the EPR paradox.[14] His conclusion was that a real description according to the criteria in the EPR argument was at variance with quantum mechanics rather than being an indication of its completeness. One may be tempted to say that an explanation of this divorce could be the fact that only two references in English to von Neumann's book can be found between 1933 and 1958.[15] It seems unlikely however that physicists during that period did not read German. A more plausible explanation is the fact that physicists who had fled Nazi Germany were keen on finding a job in a new environment. They were obliged to seek to conform to new ways of doing physics. In fact, it is obvious that Einstein and his collaborators in the EPR argument were doing physics in a different way: a style based on the notion of a *thought-experiment* rather than on the notion of *axiomatisation,* which was the foundation of von Neumann's approach.[16]

3. BROAD CULTURAL FACTORS UNDERPINNING THE FORMULATION OF THE PROOF.

Having given a sketch of the 'impossibility proof' and illustrated some significant aspects of its aftermath, I will now describe some features of the historiographic method employed here. There are a number of important historical studies concerning this 'impossibility proof', but none treat specifically of its origins.[17] Our attention will be focused rather on a recent paper by A. Warwick in spite of the fact that this treats of a different subject matter. Here we have a valuable analysis of how to write the history of theoretical physics.[18]

Andrew Warwick investigates the earliest commentaries of the 1905 Einstein paper on special relativity. He considers these commentaries as *active reinterpretations* of the text rather than as responses to a commonly perceived theory of relativity. In this way he reaches two conclusions: (a) that the original text (in his case the 1905 paper) becomes a much more powerful tool for the comparative study of different traditions in physics; and (b) that the traditions revealed by his analysis do not represent 'national' styles but different networks of collaboration and competition. Now, it is clear that when considering the von Neumann 'impossibility proof', we have something similar to the case studied by Warwick, because this proof, like special relativity, can be traced back to a single author and a single paper, which has been read by many.

The most significant notion in Warwick's paper is that of 'theoretical technology", a notion with which he endeavours to give an account of the history of science in its mathematical and theoretical dimension rather than in its experimental dimension. This latter dimension had been, and still is, well analysed by historians like Shapin and Schaffer who argue that, if experiments are derivative of the instruments and techniques possessed by a particular culture at a particular time, then knowledge claims based upon those experiments can be related to the broad issues within that culture.[19] Warwick rightly claims that if the work of mathematical physicists, largely considered to be a solitary activity, is characterised in terms of abstract theories whose essence can be stated in purely conceptual terms, then an unsatisfactory account results. Such a history captures neither the real-time experience of learning nor the skill needed to apply these theoretical concepts to practical problems. «In short, the history of idealised theories makes no place for the local cultural resources that generate and sustain individual theoretical enterprises.»[20] This explains why Warwick, in formulating a methodology applicable to theoretical practice, attempts the same micro-sociological studies that inspired the historical investigation of experimental practice:

> I shall employ the term theoretical technology to describe pieces of theoretical work that are not constitutive of a general theory, but which are used to solve particular problems and which are taken for granted by members of a local community...Just as the products of experimental work can be seen as cultural artefacts of the instruments and skills from which they are constituted, so the products of

> theoretical work can be viewed as the cultural artefacts of the theoretical practices learned and articulated by theoreticians.[21]

Andrew Warwick's ideas are an extension of the method employed by Shapin and Schaffer. These latter authors presuppose an important interaction occurring between the following factors: firstly, broad issues within the culture where a science develops, secondly the instruments and techniques, thirdly the experiments performed, and fourthly the knowledge claims proposed by the scientists. We may therefore assume that the same kind of interaction is at play between the following corresponding factors: firstly the broad issues within the specific culture, secondly the conceptual techniques available, thirdly the learning process characterised by canonical solutions to set problems, and fourthly the final results of the theoretical practice in the form of theorems or proofs.

To identify some important elements of the broad issues in the case of the von neumann proof, we have to discuss the nature of presupposed ideas within the community of physicists at the time. These ideas are usually never formulated explicitly in a rigorous way. However they determine, at least in part, which type of mathematics or physics is worth working on. That this factor was operating to some extent in the formulation of the 'impossibility proof' is attested to by a close friend of von Neumann's, Eugene Wigner, who wrote:

> Apparently, even mathematicians are convinced occasionally by considerations which they cannot formulate in a rigorous fashion. [...] the point [...] is that all schemes of hidden parameters which either von Neumann himself, or anyone else whom he knew, could think of [...] had some feature which made it unattractive, in fact unreasonable. This was, in my opinion, the true reason for his conviction of the inadequacy of the theories of hidden variables.»[22]

If we attempt to uncover the exact nature of the presupposed features which made hidden variables so unattractive, we will be obliged to examine the complicated issue of the role of causality in the mind of scientists in the 1920's and 1930's. The classic paper about this is the one by Paul Forman, who endeavours to describe how a conversion from determinism to indeterminism was a result of cultural factors in the lives of Hermann Weyl and Richard von Mises, both of whom were very well known to von Neumann.[23] The attitude of von Neumann towards the question of causality is not very clear: Trevor Pinch goes to the point of suggesting that his position was self-contradictory. However, what can be said with a high degree of certainty is that he clearly indicated his belief that causality is a matter to be decided upon while considering the micro-world rather than while considering macroscopic objects: «That macroscopically identical objects exhibit identical behaviour has little to do with causality: they are in fact not equal at all, since the co-ordinates which determined the states of their atoms almost never coincide exactly, and the macroscopic method of observation averages over these co-ordinates (here they are the ‘hidden parameters’). [...] The question of causality could only be put to a true test only in the atom, in the elementary processes themselves, and here everything in the present state of our knowledge militates against it.» Such a manner of writing about the notion of causality adds more confirmation to our hypothesis that broad cultural issues, consisting of unformulated presuppositions, did play a significant role in the ‘impossibility proof’.

4. FACTORS INHERENT IN THE LEARNING PROCESS.

A second broad issue which has a substantial role in the life of any scientist is the learning process. David Bloor developed models of theoretical practice that characterise theoreticians as skilled artisans rather than as contemplative philosophers.[24] According to these enculturational models, scientists learn their trade not by being taught the logical application of strict theoretical principles (the algorithmical model of learning), but by being taught to tackle a range of problems by reference to a number of canonical solutions. The skills that are common to all members of a community form a pool of common expertise that lends currency to physical theories.

If we turn to our case of John von Neumann, we discover that a particular way of doing mathematics was developing in Hungary around the turn of the century. Education became compulsory in Hungary in 1868. Mathematical education in particular started to become very popular after the first generation of important mathematicians. von Neumann's teacher, L. Fejér (1880-1959), is described in by M. Mikolás as «the most outstanding of the second generation mathematicians who studied at or shortly after the turn of the century.»[25] Budapest in the period of the two decades around the First World War proved to be an exceptionally fertile breeding ground for scientific talent. According to von Neumann himself, when asked by his student S. Ulam, the explanation of the phenomenon lies in the fact that cultural factors were involved in producing a subconscious feeling of extreme insecurity in individuals. This motivated scientists to produce the unusual as a means of escaping extinction.[26]

Following von Neumann as he left Hungary, we are obliged to focus our attention on the particular way theoretical research was carried out in German universities at the turn of the century. The man most responsible for this was David Hilbert (1862-1943), whose influence could be said to have radiated over all Germany from the University of Göttingen, where he arrived in 1895. His major mathematical accomplishments centred around his desire to axiomatise, a desire which can be traced back to his work on the consistency of geometry. The wish to axiomatise number theory has even been described as an 'obsession' by Hans Freudenthal.[27] Hilbert thus came upon the notion of a formalism, a concept used to designate the task of reducing mathematics to a finite operation with an infinite but finitely defined group of formulae, the only condition being that the operation be consistent. It is interesting to note that he obtained encouragement for such a project from the Academy of Sciences of Berlin University: in 1900 he shared the ‘Steiner Prize’ for outstanding previous work in mathematics. In a letter he addressed to the Academy, he explicitly mentioned the fact that the prize served as an encouragement to continue in his work of treating Mechanics as he had treated Geometry. He described his main problem as «das Problem der logisch-mathematischen Behandlung der Axiome der Physik.»[28]

It seems certain that von Neumann was deeply affected by this project. His close friend Wigner attests to this:

> His [von Neumann's] work in mathematics — which was always closest to his heart and in which his brilliance could manifest itself most decisively — was strongly under the influence of Hilbert's axiomatic school. This applies not only to his work in mathematical logic, but also to his approach to other problems to which he contributed fundamentally.[29]

In fact, the motivation behind von Neumann's work in set theory can be traced back to the fact that Cantor's theory of sets had been greatly affected by the discovery of paradoxes. This led the German mathematician Ernst Zermelo (1871-1953) to axiomatise set theory, and von Neumann added the so-called ‘axiom of foundation’ to eliminate certain extraordinary sets.

Other indications that Hilbert's axiomatic school had a decisive influence on von Neumann are linked to one of ‘Hilbert's problems’ presented originally in a lecture delivered before the international congress of mathematicians at Paris 1900. The problem of interest to our topic is the sixth one, formulated by Hilbert in the following words: «to treat in the same manner, by means of axioms, those physical sciences in which mathematics plays an important part.»[30] Hilbert attempted to do this himself, and in the winter term of 1926-1927, he gave a series of lectures on Quantum

Mechanics on these lines. These lectures were prepared in collaboration with two assistants: L. Nordheim and J. von Neumann, who was in Göttingen at the time.

5. THE CONCEPTUAL TECHNOLOGY BEHIND THE FORMULATION OF THE PROOF

The broad issues described up to now lead naturally to the conclusion that von Neumann's main contribution in his *Mathematical Foundations* was the application of the conceptual technique of axiomatisation which had proved so useful in mathematics. According to Paul Halmos,

> 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 applications.[31]

One may wonder what David Hilbert himself thought of von Neumann's approach. It seems plausible that he considered his former colleague's work as the fruit of his own previous methodological insights. Wightman writes: «I do not know whether Hilbert regarded von Neumann's book as the fulfilment of the axiomatic method applied to quantum mechanics, but, viewed from afar, that is the way it looks to me. In fact, in may opinion, it is the most important axiomatisation of a physical theory up to this time.»[32]

This method was not however without its problems; and von Neumann seems to have been aware of them. Hilbert himself never felt the same sort of security within physics as he felt within mathematics. And this seems to have been known by his collaborators. His obituary notice, written by Hermann Weyl in 1944, explicitly refers to this point in the following words:

> The maze of experimental facts which the physicist has to take into account is too manifold ... for the axiomatic method to find a firm enough foothold. Men like Einstein and Niels Bohr grope their way in the dark towards their conceptions of general relativity or atomic structure by another type of experience and imagination then those of the mathematician, although no doubt mathematics is an essential ingredient. Thus Hilbert's vast plans in physics never matured.[33]

Even during Hilbert's lifetime, his axiomatic school was not without its enemies. The most intransigent adversary to his method was L.E.J. Brouwer, who from 1907 onwards held that it is truth rather than consistency that matters in mathematics. In the 1920's, Hermann Weyl, one of Hilbert's famous students and friend of von Neumann, took Brouwer's side. The calamity came in 1931 when Kurt Gödel proved that Hilbert's program was not feasible. How did this affect von Neumann? It seems probable that Gödel's 'other method' of doing mathematics deeply impressed and intrigued von Neumann, who was so attached to being absolutely clear in thought or in expression. Halmos testifies that von Neumann «knew his own strengths and he admired, perhaps envied, people who had the complementary qualities, the flashes of irrational intuition that sometimes change the direction of scientific progress.» He continues a little later that von Neumann, «admired Gödel and praised him in strong terms.»[34]

The 'impossibility proof' contains in fact the following words of caution. «The only formal theory existing at the present time which orders and summarised our experiences in this area in a half-way satisfactory manner, i.e., quantum mechanics, is in compelling logical contradiction with causality. Of course it would be an exaggeration to maintain that causality has thereby been done away with: quantum mechanics has, in its present form, several lacunae, and it may even be that it is false, although this latter possibility is highly unlikely, in the face of its startling capacity in the qualitative explanation of general problems.»[35] One of von Neumann's biographers, who knew him personally, Leon van Hove, testifies that «he was conscious of the fact that axiomatisation is a limited process which keeps us within a specific theory.»[36] This did not prevent him from later criticism like that of Belinfante.[37] Whatever the outcome of his proof, we may conclude that von Neumann, having endorsed the method of axiomatisation, was a typical example of German physicists who were interested mainly in deriving physics from first principles, who considered phenomenological theories as not to be regarded highly, and who were, in effect, mathematicians rather than experimental physicists.

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[1] H. REICHENBACH, *Experience and Prediction*, Chicago, University of Chicago Press, 1938, pp. 6-7.

[2] M. JAMMER, *The Philosophy of Quantum Mechanics*, New York, Wiley-Interscience Publication,1974.

[3] J. VON NEUMANN, *Mathematische Grundlagen der Quantenmechanik*, Berlin, Springer, 1932; *Mathematical Foundations of Quantum Mechanics*, translated by R.T. Beyer, Princeton, Princeton University Press, 1955.

[4] *Mathematical Foundations of Quantum Mechanics*, Princeton, Princeton University Press, 1955, p. 206.

[5] This concept of quantum mechanics, which accepts its statistical expression as the actual form of the laws of nature, and which abandons the principle of causality is the so-called Statistical Interpretation. It is due to Max Born, *Zur Quantenmechanik der Stossvorgänge*, «Zeitschrift für Physik», XXXVII, 1926. Notice that this statistical character is limited to statements on the values of physical quantities, while the preceding and subsequent states Ψ_t can be calculated causally from the time dependent Schrödinger equation.

[6] *Mathematical Foundations of Quantum Mechanics*, Princeton, Princeton University Press, 1955, p. 209.

[7] E.P. WIGNER, *John von Neumann*, «Yearbook of the American Philosophical Society», 1957, pp.149-153.

[8] A very comprehensive and condensed exposition of von Neumann's proof containing the mathematical details omitted here may be found in L.E. BALLENTINE, *The statistical interpretation of quantum mechanics*, «Review of Modern Physics», XLII, 1970, pp. 374-375.

[9] M. JAMMER, *The Philosophy of Quantum Mechanics*, New York, Wiley-Interscience Publication, 1974, p. 270.

[10] D. BOHM, *A suggested interpretation of the quantum theory in terms of hidden variables*, «Physics Review», LXXXV, 1952, pp. 166-193.

[11] G. TEMPLE, *The fundamental paradox of quantum theory*, «Nature», CXXXV, 1935, p. 957. See also H. FRÖHLICH and E. GUTH, *The fundamental paradox of quantum theory*, «Nature», CXXXVI, 1935, p. 179.

[12] J.S. BELL, *On the problem of hidden-variables in Quantum Mechanics*, «Review of Modern Physics», XXXVIII, 1966, pp. 447-475.

[13] F.J. BELINFANTE, *A Survey of Hidden Variables Theories*, Oxford, Pergamon Press, 1973, p. 34.

[14] W.H. FURRY, *Note on the quantum-mechanical theory of measurement*, «Physical Review», XLIX, 1936, pp. 393-399; *Remarks on measurement in quantum theory*, «Physical Review», XLIX, 1936, p. 476.

[15] The first one was a short note by H. MARGENAU (in «Bulletin of the Mathematical Society», XXXIX, 1933, pp. 493-494) and the second was the review of the English translation of the German 1932 original (the review by P.K. FEYERABEND appeared in «The British Journal for the Philosophy of Science», VIII, 1957-58, pp. 343-347).

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[16] A comment by Hermann Weyl concerning this point will be mentioned later on in this paper. It seems probable that there was an important difference between the German style of doing physics and the style of the American physicists. The style of Van Vleck, Slater, Kemble and Mulliken may be called "pragmatic" since it concerned more the experimental work in quantum mechanics.

[17] See for example S. BRUSH, *The Chimerical Cat: Philosophy of Quantum Mechanics in a Historical Perspective,* «Social Studies of Science», X, 1980, pp. 393-447; B. HARVEY, *Plausibility and the Evaluation of Knowledge: a case study of experimental Quantum Mechanics,* «Social Studies of Science», XI, 1981, pp. 95-130; and T. PINCH, *What does a proof do if it does not prove? A study of the social conditions and metaphysical divisions leading to David Bohm and John von Neumann failing to communicate in Quantum Physics*, in The Social Production of Scientific Knowledge, ed. by E. Mendelsohn, P. Weingart and R. Whitley, Boston, Reidel Publishing Co., 1977, pp. 171-215. Brush's work is too general for our purposes, and Harvey's paper is concerned essentially with the experimental justification of some features of quantum mechanics. Although useful, these articles will not therefore be considered in detail here. Pinch's approach contains two issues which make it inappropriate for our purposes. First of all, he concentrates on the effects of the proof rather than on the possible techniques that were at its source. Secondly, he centres his discussion on the alleged communication problem between David Bohm and von Neumann. This way of approaching the issue seems questionable because in reality there never was any attempt on von Neumann's part to enter into dialogue concerning hidden variables with Bohm, nor with anyone else, for that matter. Four years after Bohm's paper, von Neumann was in hospital, incurably ill. The issue is very different from, say, the communication-problems in the Bohr-Einstein debate or the Leibniz-Clarke debate.

[18] A. WARWICK, *Cambridge Mathematics and Cavendish Physics: Cunningham, Campbell, and Einstein's Relativity, 1905-1911. Part I: The Uses of Theory*, «Studies in the History and Philosophy of Science», XXIII, 1992, pp. 625-657.

[19] S. SHAPIN and S. SCHAFFER, *Leviathan and the Air-Pump, Hobbes, Boyle, and the Experimental Life,* Princeton, Princeton University Press, 1985.

[20] A. WARWICK, *Cambridge Mathematics and Cavendish Physics: Cunningham, Campbell, and Einstein's Relativity, 1905-1911. Part I: The Uses of Theory*, «Studies in the History and Philosophy of Science», XXIII, 1992, p. 631.

[21] *Ibid*., p. 633.

[22] E.P. WIGNER, *On hidden variables and quantum mechanical probabilities*, «American Journal of Physics», XXXVIII, 1970, p 1009; and *Rejoinder*, «American Journal of Physics», XXXIX, p.1097; quoted by T. PINCH, *What does a proof do if it does not prove? A study of the social conditions and metaphysical divisions leading to David Bohm and John von Neumann failing to communicate in Quantum Physics,* in The Social Production of Scientific Knowledge, ed. by E. Mendelsohn, P. Weingart and R. Whitley, Boston, Reidel Publishing Co., 1977, p. 192.

[23] P. FORMAN, *Weimar Culture, Causality and Quantum Theory, 1918 - 1927: adaptation by German physicists and mathematicians to a hostile intellectual environment*, «Historical Studies in the Physical Sciences», III, 1971, pp. 1-115. This study has continued to be considered a classic paper in spite of the criticism of J. HENDREY, *Weimar Culture and Quantum Causality,* «History of Science», XVIII,

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1980, pp. 155-180. It constitutes very interesting background to our case here, but the period treated by Forman is 1918 - 1927, just before von Neumann is reputed to have started his work on quantum mechanics.

[24] D. BLOOR, *Wittgenstein: A Social Theory of Knowledge,* London, Macmillan Press, 1983.

[25] M. MIKOLÁS, *Some historical aspects of the development of mathematical analysis in Hungary*, «Historia Mathematica», II, 1975, pp. 304-308.

[26] S. ULAM, *John von Neumann, 1903-1957*, «Bulletin of the American Mathematical Society», LXIV, 1958, pp. 1-49.

[27] H. FREUDENTHAL, *David Hilbert*, in Dictionary of Scientific Biography, ed. by C.C. Gillispie, VI, p. 392.

[28] K-R. BIERMANN, *Aus der Geschichte Berliner mathematischer Preisaufgaben*, «Wissenschaftlische Zeitschrift der Humboldt-Universität zu Berlin (Mathematisch-Naturwissenschaftliche Reihe)», XIII, 1964, p. 188.

[29] E.P. WIGNER, *John von Neumann*, «Yearbook of the American Philosophical Society», 1957, pp.149-153.

[30] The full text may be found in Mathematical Developments arising from Hilbert's Problems, ed. by F.E. Browder, Providence R.I., American Mathematical Society, 1976.

[31] P.R. HALMOS, *The Legend of John von Neumann (1903 - 1957),* «American Mathematical Monthly», LXXX, 1973, p. 394.

[32] A.S. WIGHTMAN, *Hilbert's 6th Problem: Mathematical Treatment of the Axioms of Physics*, in Mathematical Developments arising from Hilbert's Problems, ed. by F.E. Browder, Providence R.I., American Mathematical Society, 1976, pp. 147-240.

[33] H. WEYL, *David Hilbert and his Mathematical Work*, «Bulletin of the American Mathematical Society», L, 1944, pp. 612-654.

[34] P.R. HALMOS, *The Legend of John von Neumann (1903 - 1957)*, «American Mathematical Monthly», LXXX, 1973, p. 383.

[35] J. VON NEUMANN, *Mathematical Foundations of Quantum Mechanics*, Princeton, Princeton University Press, 1955, p. 327.

[36] L. van HOVE, *Von Neumann's contributions to quantum theory*, «Bulletin of the American Mathematical Society», LXIV, 1958, pp. 95-99.

[37] Consider for example Belinfante's statement: «He should have concluded that the kind of hidden-variables theory *defined by him* was an impossible theory [...] Unfortunately, he did not express himself in this kind of cautious way. Instead, he claimed, "we need not go any further into the mechanism of the hidden parameters, since we now know that the established results of quantum mechanics can never be re-derived with their help" (cf. von Neumann, *Mathematical Foundations*, p. 324). Though at present we know very well that this claim is contrary to the results of explicit hidden-variables theories of the first kind [...], the authority of von Neumann's overgeneralised claim for nearly two decades stifled any progress in the search for hidden-variables theories. This is especially surprising because of the obviousness of *inapplicability* of one of von Neumann's axioms to any realistic hidden-variables theory of the kind that attempts to explain quantum theory as a special case.» F.J. BELINFANTE, *A Survey of Hidden Variables Theories*, Oxford, Pergamon Press, 1973, p. 24. Consider also: «So, what we have been discussing should have been obvious. The truth, however, happens to be that for decades nobody spoke up against von

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Neumann's arguments, and that his conclusions were quoted by some as the gospel. There must be some magic in his arguments that could fool people into believing that *his* definition of hidden-variables theory would be the only correct one rather than the obviously inappropriate one.» *Ibid.,* p. 34.