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Von Neumann's Methodology of Science.

From Incompleteness Theorems to Later Foundational Reflections

Abstract. In spite of the many efforts made to clarify von Neumann's methodology of science, one crucial point seems to have been disregarded in recent literature: his closeness to Hilbert's spirit. In this paper I shall claim that the scientific methodology adopted by von Neumann in his later foundational reflections originates in the attempt to revaluate Hilbert's axiomatics in the light of Gödel's incompleteness theorems. Indeed, axiomatics continues to be pursued by the Hungarian mathematician in the spirit of Hilbert's school. I shall argue this point by examining four basic ideas embraced by von Neumann in his foundational considerations: a) the conservative attitude to assume in mathematics; b) the role that mathematics and the axiomatic approach have to play in all that is science; c) the notion of success as an alternative methodological criterion to follow in scientific research; d) the empirical and, at the same time, abstract nature of mathematical thought. Once these four basic ideas have been accepted, Hilbert's spirit in von Neumann's methodology of science will become clear[*].

Describing the methodology of a prominent mathematician can be an over-ambitious task, especially if the mathematician in question has made crucial contributions to almost the whole of mathematical science. John von Neumann's case study falls within this category. Nonetheless, we can still provide a clear picture of von Neumann's methodology of science. Recent literature has clarified its key feature – i.e. the opportunistic approach to axiomatics – and has laid out its main principles. To be honest, this work can hardly be superseded. What I would like to do is to complete the picture by adding one more step and emphasizing a point so far neglected, namely the role of Hilbert's ideal in von Neumann's epistemology. Von Neumann's methodological opportunism – sketched in his later foundational reflections (von Neumann 1947; 1954; 1955) – originates in his attempt to revaluate Hilbert's

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axiomatics in the light of Gödel's incompleteness theorems, yet, *at the same time,* it is pursued in the spirit of Hilbert's school. While the first term of the conjunction has been stressed, the second seems to have been disregarded by the literature (Stöltzner 2001; Rédei 2005; Rédei and Stöltzner 2006).

To carry out my task I shall describe the historical evolution of von Neumann's foundational standpoint towards mathematics and mathematical sciences. I shall start with some remarks about the reasons why von Neumann joined Hilbert's school. Then I shall move on to consider the fascinating story of incompleteness and von Neumann's revaluation of the concept of mathematical rigour. Finally, I shall describe the opportunistic methodology of science to show that it was grounded on some basic ontological assumptions about the nature of mathematics.

1. Joining Hilbert's School

Officially, von Neumann enters the foundational debate with the article *An Axiomatization of Set Theory* (1925) and until the early 1930s he makes central contributions to Hilbert's finitist program. This set-theoretical article is crucial because it shows von Neumann's position with regard to Hilbert's foundational standpoint and provides the key to understanding the general attitude towards scientific research which the Hungarian mathematician states in his later foundational reflections. The aim of the work is «to give a logically unobjectionable axiomatic presentation of set theory» in response to the foundational crisis opened up by the appearance of contradictions in the naïve theory at the turn of the century (von Neumann 1925, p. 395). In his analysis of the precarious debate on the foundations of mathematics, von Neumann clearly distinguishes two different theoretical tendencies in mathematics. On the one hand, the *radical attitude* of those who, in the attempt to rebuild mathematics upon new and universally evident bases, submit «the entire logical foundation of the exact sciences to a critique» and, on the other, the *conservative attitude* of those who, through the application of the axiomatic method, eschew «so radical a revision», believing that the accepted methods of mathematical reasoning need to be preserved. In the first group, for instance, von Neumann counts both Bertrand Russell and Luitzen Brouwer, while he assigns to the second group only those mathematicians close to Hilbert's school: i.e. Ernst Zermelo and Abraham Fraenkel. That von Neumann's own work falls within this second theoretical tendency – the conservative attitude – is made clear by the very precise description he gives of the procedure to be applied in order to treat axiomatically the notion of set: we must first eliminate its naïve and tricky definition and then introduce a different one by formulating a certain number of axioms which accurately carry out the task of replacing the old concept with the new axiomatic concept of set. Of course, the axioms must provide a complete presentation of the naïve set theory, in the sense that they must enable the formal derivation of all known theorems without the well known contradictions, e.g. Russell's Paradox.

The most interesting fact is that in von Neumann's article the conservative attitude is combined with a more extensive pragmatic approach towards the task of axiomatic presentation. Strangely, the latter is shared by other earlier presentations in Hilbert's school, above all the one published by Zermelo (1908a, pp. 200-1). For instance, both von Neumann and Zermelo give a logically unobjectionable axiomatic presentation of the theory «as it is historically given» but defer the problem of the consistency of their axiom systems:

> The second group, then, while painstakingly avoiding the naïve notion of set (Cantor's), wants to specify an axiom system from which set theory (without its antinomies) follows. Their investigations can, to be sure, never settle the real problem so completely as those by the first group, but their goal is much clearer and closer at hand. Of course, it can never be shown in this way that the antinomies have really been eliminated, and much arbitrariness always attaches to the axioms. But one thing can be attained here with certainty, namely, a precise determination of what the rehabilitated part of set theory is and what is at issue when henceforth we speak of the complete "formalistic set theory". (von Neumann 1925, p. 395-6)

In short, von Neumann shares with Hilbert's foundational standpoint a conservative attitude towards mathematical investigation. Moreover, this conservative attitude is combined with the use of the axiomatic method. Let me just recall what Hilbert once said about this issue: «The goal of finding a secure foundation for mathematics is also my own […]; but I believe that this can be done while fully preserving its accomplishments. The method that I follow is none other than the axiomatic» (Hilbert 1922, p. 1119). Therefore, given a basic agreement about this conservative attitude and the axiomatic method, the aims of Hilbert's program, which now begins to assume a precise finitist formulation, appeared to von Neumann «much clearer and closer at hand».

However, von Neumann's agreement with these essential methodological guidelines never becomes dogmatic. He is indeed very critical of a few more specific foundational issues. Without entering into historical and systematic details, I shall mention that: a) in contrast with Hilbert's optimism, he had many doubts about the solvability of the decision problem, as clearly shown by his paper on proof theory in 1927 (von Neumann 1927, pp. 265-6); b) he believed that consistency was not a sufficient criterion of empirical adequacy for formal theories, as explicitly stated during the round-table at the Königsberg Conference in 1930 (Dawson 1984, p. 123); c) in a letter written to Rudolf Carnap on 6 June 1931, he manifested his criticism of Hilbert's foundational propaganda: «There are some programmatic publications by Hilbert in which he claims that certain things have been proven or almost proven while this is in fact not even approximately the case (continuum problem and so on)» (von Neumann 1931a, p. 40). There is convincing evidence to infer that von Neumann's support of Hilbert's foundational ideal was never blind.

This ambivalence provides a good explanation of von Neumann's reception of Gödel's incompleteness theorems. He declared that Hilbert's program was over and, at the same time, continued to follow Hilbert's guidelines in a coherent opportunistic fashion.

2. The Impact of Incompleteness Theorems

During the 1920s Hilbert's figure dominated the debate on the foundations of mathematics. On numerous occasions he said that he would soon be able «to eliminate once and for all the questions regarding the foundations for mathematics» (Hilbert 1928, p. 464), while *preserving* the entire heritage of mathematical research. Furthermore, he created a specific methodology in order to transfer foundational problems from a philosophical and epistemological domain to a domain which was mathematical in the strict sense (Bernays 1922, pp. 221-2). This methodology was articulated in two different moments: a) finding a suitable axiomatic presentation of any informal mathematical field – i.e. a formal theory, or a formalism, which was as far as possible complete, simple and perspicuous – in order to represent the content of its concepts, propositions and methods of reasoning; b) trying to justify the formal theory thus obtained through its finitist consistency proof. The proof should be developed, according to Hilbert's main idea, in meta-mathematics by means of contentual inference (Hilbert 1922, pp. 1131-2).

Von Neumann presented Hilbert's foundational standpoint at the Second Conference for Epistemology of the Exact Sciences held in Königsberg from 5 to 7 September 1930. On the first day he ended his talk by recalling the *real* results obtained by Hilbert's finitist program: «Although the consistency of classical mathematics has not yet been proved, such a proof has been found for a somewhat narrower mathematical system. […] Thus Hilbert's system has passed the first test of strength: the validity of a non-finitary, not purely constructive mathematical system has been established through finitary constructive means. Whether someone will succeed in extending this validation to the more difficult and more important system of classical mathematics, only the future will tell» (von Neumann 1931, p. 54). During the round-table that took place on the last day of the Conference, Kurt Gödel announced an early version of the first incompleteness theorem: the existence of undecidable propositions within the formal system of classical mathematics (Dawson 1984, p. 126). «Von Neumann», as reported by Hao Wang, «was very enthusiastic about the result and had a private discussion with Gödel» (Wang 1981, p. 654). After a few weeks – on 20 November 1930 – he wrote to Gödel: «I have recently concerned myself again with logic, using the methods you have employed so successfully in order to exhibit undecidable propositions. In doing so I achieved a result that seems to me to be remarkable. Namely, I was able to show that the consistency of mathematics is unprovable» (von Neumann 1930, p. 337). It is a fact that von Neumann discovered the second incompleteness theorem, too, but he left to Gödel the paternity of a great discovery (Sieg 2003). I shall go into the fascinating details of von Neumann's discovery on another occasion. Here I would like to recall the words with which a few days later – on 29 November 1930 – he declared the end of Hilbert's program: «Thus I think that your result has solved negatively the foundational question: there is no rigorous justification for classical mathematics» (von Neumann 1930a, p. 339).

It might be useful to go one step back to place this piece in the mosaic. According to Paul Bernays, Hilbert converted a philosophical and epistemological problem – the justification of a formal theory – into a problem which is mathematical in the strict sense: providing its consistency proof within finitist mathematics. Thus, Gödel's negative solution to the mathematical problem still left unsolved the more crucial epistemological problem. In 1951 von Neumann recalled Gödel's result: «It must be emphasized that the important point is, that this is not a philosophical principle or a plausible intellectual attitude, but the result of a rigorous mathematical proof of an extremely sophisticated kind» (von Neumann 1951, p. IX). However, it is interesting to note that, in his second letter to Gödel, von Neumann speaks about the epistemological problem left unsolved in terms of the *rigorous* justification – and not in terms of the *simple* justification – of formal theories, since it is precisely through a revaluation of the concept of mathematical rigour that he began to think of a methodology of science to be developed in opportunistic fashion (Stöltzner 2001; Rédei 2005; Rédei and Stöltzner 2006). This methodology is grounded on the crucial notion of *success* as the criterion of justification for formal theories.

Before focusing on von Neumann's revaluation, it is important to clarify that the notion of success is not alien to Hilbert's foundational ideal but, like an underground stream, it flows beneath the canonical form of the finitist program. While describing it in the article *On the Infinite*, Hilbert introduced the notion of success in the following terms:

> If we pay close attention, we find that the literature of mathematics is replete with absurdities and inanities, which can usually be blamed on the infinite. […].
> Even old objections that have long been regarded as settled reappear in a new guise. So in recent time we come upon statements like this: even if we could introduce a notion safely (that is, without generating contradictions) and if this were demonstrated, we would still not have established that we are justified in introducing the notion. […]. No, if justifying a procedure means anything more than proving its consistency, it can only mean determining whether the procedure is successful in fulfilling its purpose. Indeed, success is necessary; here, too, it is the highest tribunal, to which everyone submits. (Hilbert 1926, p. 370)

One may object that the word «success» here does not express anything significant about foundational issues. But I do not think so. I believe that success represents, in Hilbert's ideal, a real methodological principle, absolutely consistent with the idea, widespread in his school, according to which «principles must be judged from the point of view of science, and not science from the point of view of principle fixed once and for all» (Zermelo 1908, p. 189). In this view, success concerns the introduction of several mathematical objects: i.e. a particular formal device, a concept, an axiom or, in principle, even a set of axioms. Notice that the use of formal devices and the meaning of concepts fall under the rule of certain axioms. Thus, the introduction of any of these mathematical objects, if it is empirically «safe», can only be judged by its theoretical benefit, namely by the purpose successfully fulfilled within the formal system; while its absolute «safety» would be guaranteed by the meta-theoretical proof of its consistency, which is both required and expected. One can indeed find very famous examples from Hilbert's school for the use of success as a criterion to justify the introduction of particular mathematical objects. I shall mention only two, which are, moreover, well known. The first concerns the axiom of choice introduced by Zermelo within his set-theoretical system in 1908, in order to allow the unquestionable derivation from axioms of the well-ordering theorem, which met Hilbert's approval. He justified this introduction by saying that the principle of choice is an unprovable but indispensable axiom for science, applied without hesitation everywhere in mathematical deduction (Zermelo 1904, p. 141; 1908, pp. 186-90, 198). The second example concerns Hilbert directly. During the 1920s he introduced the τ-operator – and later the more direct ε-operator – to represent in his formal systems the informal transfinite reasoning of classical mathematics. Clearly Hilbert thought that these operators – and the axioms governing their use – should be guaranteed by an appropriate consistency proof (Hilbert 1923). But it is well known that this was not the case and that the same can be said for the axiom of choice.

I shall claim that, once Hilbert's program had been superseded, von Neumann ascribed a key foundational task to the criterion of success. But first I shall illustrate von Neumann's later reflections on incompleteness theorems, which express his essential revaluation of the concept of mathematical rigour.

3. Conceptual Revaluation of Mathematical Rigour

It is very surprising that von Neumann's later diagnosis of the foundational debate follows closely the one in his set-theoretical article. In *The Mathematician* (1947) he distinguishes the same main periods and he supports exactly the same conservative attitude towards mathematical research. According to the distinction he proposes, there are three periods which mark the evolution of the whole debate up to his time (von Neumann 1947, pp. 5-6). First, the appearance of antinomies in naïve set theory that initiated the crisis in the foundations of mathematics. Next cautiousness, when people began to rethink the old methodological assumptions. These were split into two groups. On the one hand, those who banished the philosophically objectionable concepts and developed (actually very restricted) systems which were free from the difficulties and contradictions generated by naïve set theory. However, their results were disappointing because, together with the doubtful methods used in earlier mathematical practice, they ended up eliminating even the most vital and unquestioned parts of modern mathematics. This position was shared above all by Russell and Brouwer. On the other hand, there were those who sought to justify, without any deformation, the whole of classical mathematics through absolute and unquestionable methods. It must be noted here that such a conservative attitude is now associated only with the name of Hilbert. What he tried to do, according to von Neumann, was to prove, by means of incontestable methods, that classical procedures would not generate contradictions. Finally Gödel's results came out and showed that Hilbert's program was hopeless for conceptual reasons. Von Neumann thus describes the foundational situation created by the incompleteness theorems:

> The main hope of a justification of classical mathematics […] being gone, most mathematicians decided to use that system anyway. After all, classical mathematics was producing results which were both elegant and useful, and, even though one could never again be absolutely certain of its reliability, it stood on at least as sound a foundation as, for example, the existence of the electron. Hence, if one was willing to accept the sciences, one might as well accept the classical system of mathematics. […]. At present,

> the controversy about the "foundations" is certainly not closed, but it seems most unlikely that the classical system should be abandoned by any but a small minority.
>
> I have told the story of this controversy in such detail, because I think that it constitutes the best caution against taking the immovable rigour of mathematics too much for granted. (von Neumann 1947, p. 6)

In this passage von Neumann's thesis becomes explicit, once the historical impact of Gödel's incompleteness theorems has been taken for granted: an absolute and immutable concept of mathematical rigour dissociated from human experience does not exist. At the same time, however, the key to interpreting the thesis correctly is hidden, namely the fact that it has to be seen against the background of a conservative attitude to mathematical research. There is indeed a close analogy between the first foundational crisis, initiated by the appearance of set-theoretical antinomies, and the situation created by the discovery of incompleteness theorems. A few years later, in *The Role of Mathematics in the Sciences and in Society* (1954), von Neumann discussed again the issue of mathematical rigour, reiterating once more his distinction between radical and conservative mathematicians. But this time he clearly stated his agreement with the second group:

> This group was quite ready to accept something like this: Those portions of mathematics which had been questioned and which had been clearly useful, specifically for the internal use of the fraternity – in other words, when very beautiful theories could be obtained in those areas – that those were after all at least as sound as, and probably somewhat sounder than, the constructions of theoretical physics. And, after all, theoretical physics was all right; so why shouldn't such an area, which had possibly even served theoretical physics, even though it did not live up to 100 per cent of the mathematical idea of rigour, why shouldn't this be a legitimate area in mathematics; and why shouldn't it be pursued? This may sound odd, as well as a bad debasement of standards, but it was believed in by a large group of people for whom I have some sympathy, for I'm one of them. (von Neumann 1954, pp. 480-1)

What I would like to claim is that only on the basis of a general conservative attitude, in von Neumann's revaluation, does the absolute concept of mathematical rigour turn into a more flexible concept based on the crucial methodological criterion of success. This change also affects the attempt to find an appropriate solution to the epistemological problem left open by the second incompleteness theorem: i.e. the problem of an adequate justification for formal theories presented in axiomatic fashion. In fact, the idea of justifying formal theories through consistency proofs had revealed itself to be conceptually untenable; now «[i]t 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» (von Neumann 1955, p. 498). To paraphrase: success plays a crucial foundational role in solving the open problem of justification for formal theories. This is the core of von Neumann's methodological opportunism – stressed in *Method in the Physical Sciences* (1955) – which I would now like to discuss more fully.

4. The Opportunistic Methodology of Science

First, let me make clear something crucial. According to von Neumann, opportunism is not *one way*, which can differ from another, but it is *the way* to do science. The procedures of science are opportunistic: «Not only for the sake of argument but also because I really believe it, I shall defend the thesis that the method in question [the method of science] is primarily opportunistic – also that outside the sciences, few people appreciate how utterly opportunistic it is» (von Neumann 1955, p. 492).

Moreover, science neither explains nor interprets, but creates mathematical models which, «with the addition of certain verbal interpretations», describe phenomena belonging to some particular scientific field. The only justification for such a model, indeed, is that «it is expected to work – that is, correctly to describe phenomena from a reasonably wide area». And, according to von Neumann, describing means first of all predicting; and this is what gives meaning to the words «correctly to describe». Of course, a model should satisfy «certain aesthetic criteria» – first of all simplicity – but they too are a function of what is described by the model. To sum up: the *descriptive and predictive power* is the essential feature to evaluate in judging the effectiveness of a mathematical model – namely, the first criterion to use in choosing one model over another. But this power, at the same time, has to be judged on the grounds of three specifications: a) the extensiveness of the material described and predicted; b) its heterogeneity; c) the unexpected areas which provide further confirmation of the model (von Neumann 1955, p. 493). Taking these into consideration, one can say that the model also has to manifest its *formal adaptability* for further correct extensions. Both features guarantee the effectiveness of the model and define its criterion of success:

> This means that the criterion of success for such a theory [i.e. such a model] is simply whether it can, by a simple and elegant classifying and correlating scheme, cover very many phenomena, which without this scheme would seem complicated and heterogeneous, and whether the scheme even covers phenomena which were not considered or even not known at the time when the scheme was evolved. (These two latter statements express, of course, the unifying and the predicting power of a theory). (von Neumann 1947, p. 6)

This is the basic idea of the opportunistic methodology of science. Hence we can arrive at two more conclusions which reveal Hilbert's influence (Israel and Millán Gasca 2008, ch. 3): mathematics is appointed to play a leading role in all that is science, because it demonstrates «an enormous flexibility in the formation of concepts» (von Neumann 1954, p. 482), *and* the axiomatic method is appointed to play a leading role in all that is mathematics, because it is precisely this method that permits the greatest degree of flexibility (Stöltzner 2001, pp. 36, 53). Indeed, the axiomatic method is a fundamental tool because it can be applied to investigate both primitive and well-developed scientific theories. Let me quote a passage from Hilbert which I do not think needs further comment:

> I believe: anything at all that can be the object of scientific thought becomes dependent on the axiomatic method, and thereby indirectly on mathematics, as soon as it is ripe for the formation of a theory. […]. In the sign of the axiomatic method, mathematics is summoned to a leading role in science. (Hilbert 1918, p. 1115)

Asserting that any method in science is primarily opportunistic means, according to von Neumann, identifying it with the axiomatic method. Indeed, this method can be carried out in a pragmatic way; that is, it can be applied differently depending on the maturity of the field to be axiomatized. The literature has staunchly defended this claim primarily with regard to physics – in particular to von Neumann's axiomatization of quantum mechanics (Lacki 2000; Stöltzner 2001; Rédei 2005; Rédei and Stöltzner 2006) – but I think that it can also be applied to economics, as shown in *Theory of Games and Economic Behaviour* (1944) by von Neumann and Oskar Morgenstern.

At the time economics was in a primitive stage: it contained considerable conceptual gaps, for example in the definition of crucial notions such as "utility" and "rational behaviour"; it lacked an adequate empirical background, for instance as compared to physics; and consequently it had not received an appropriate mathematical treatment, except for «mere translations from a literary form of expression into symbols, without any subsequent mathematical analysis». For these reasons, the authors said that the standpoint of the book had to be mainly opportunistic and the method to be used was axiomatic, applied «in the customary way with the customary precautions» (von Neumann and Morgenstern 1944, pp. v-x, 2-9). That this method is suitable to the maturity of the field to be investigated is shown by the following paragraph in which von Neumann and Morgenstern are searching for a satisfactory theoretical background, which they then found in game theory:

> [W]hat is important is the gradual development of a theory, based on a careful analysis of the ordinary everyday interpretation of economic facts. This preliminary stage is necessarily *heuristic*, i.e. the phase of transition from unmathematical plausibility considerations to the formal procedure of mathematics. The theory finally obtained must be mathematically rigorous and conceptually general. Its first applications are necessarily to elementary problems where the result has never been in doubt and no theory is actually required. At this early stage the application serves to corroborate the theory. The next stage develops when the theory is applied to somewhat more complicated situations in which it may already lead to a certain extent beyond the obvious and the familiar. Here theory and application corroborate each other mutually. Beyond this lies the field of real success: genuine prediction by theory. It is well known that all mathematical sciences have gone through these successive phases of evolution. (von Neumann and Morgenstern 1944, pp. 7-8)

I shall conclude this section by returning to the main point. Of course, I do believe that there is a difference between Hilbert's and von Neumann's foundational standpoints. Yet they share some crucial ideas. I believe that von Neumann's epistemology develops, after Gödel's results, through the attempt to place the axiomatic method within a new coherent methodology of science. But it was not achieved by rejecting Hilbert's spirit. Instead, von Neumann's great achievement consisted in ascribing a key foundational task to success. It seems to me that success, as the criterion of justification for theories formulated in an axiomatic fashion, also provides – at least in von Neumann's reflections – an appropriate answer to the crucial epistemological problem left open by the incompleteness theorems. In this way he bridged the gap introduced by Gödel in Hilbert's program.

One can even make a comparison between Hilbert's and von Neumann's foundational standpoints in mathematics and the mathematical sciences. Indeed, Hilbert firmly believed that his program could be extended to all the exact sciences, first of all to physics (Majer 2006). According to his general scheme, after a given informal scientific theory has received an adequate axiomatic formulation, it will find its proper justification through the method of relative consistency proofs; that is, through the direct consistency proof of (a certain finitist part of) classical mathematics – for instance arithmetic – to which the consistency proof of the axiomatic theory can be finitarily reduced. Hilbert thought that the complex problem of the foundations of sciences could indeed be reduced to the more simple problem of the foundations of classical mathematics. I like to call his move the *internalization* of the epistemological problem: i.e. science has to be justified through mathematics and mathematics has to be justified through finitist mathematics, which is a proper part of classical mathematics. Von Neumann made just the opposite move: he claimed that classical mathematics has to be justified through the success it achieves in science and science has to be justified through the success it achieves in the realm of applications. I like to call this move the *externalization* of the epistemological problem.

One further remark as a bridge to the next section. Referring to success, von Neumann would say that «this is a formalist, aesthetic criterion, with a highly opportunistic flavour» (von Neumann 1955, p. 498). Nevertheless, it has been rightly noted that he did not consider mathematics, or science, to be either a conventional or a sociological construction (Stöltzner 2001, p. 50; Rédei and Stöltzner 2006, p. 246), because he thought that mathematics «establishes certain standards of objectivity, certain standards of truth» (von Neumann 1954, p. 478). Of course, there is something that needs to be explained here: how can we reconcile the idea that a mathematical model is constructed to satisfy certain opportunistic conditions – i.e. its descriptive and predictive power and formal adaptability for correct extensions – with the thesis that the model, at the same time, satisfies certain standards of objectivity, or even certain standards of truth?

As yet I have found no satisfactory explanation for this. However, I do have an hypothesis that needs to be confirmed. I believe that we can reconcile these claims if we assume that Hilbert and von Neumann also share some ontological assumptions about the nature of mathematical thought. I also think that this suggestion can be supported by textual evidence.

5. Ontological Assumptions on the Nature of Mathematics

Hilbert believed that there is a non-Leibnizian pre-established harmony between thought and nature and that this is sufficient to explain the embodiment or the realization of mathematical thought. This is the very famous thesis expressed in *Logic and the Knowledge of Nature* (1930): «But even more striking is an occurrence which is virtually an embodiment and realization of mathematical thoughts, and which we shall call, in a sense different from that in Leibniz, pre-established harmony» (Hilbert 1930, p. 1160). From this perspective the success achieved by an opportunistic mathematical model in the realm of applications is not a miracle, since, in virtue of a pre-established harmony, any mathematical construction will be realized soon or later. So, at least in principle, certain standards of objectivity or certain standards of truth are guaranteed. Notice that von Neumann seems to use indifferently objectivity and truth, since he does not make any epistemological distinction between the two concepts.

But any critical mind will wonder what explicitly supports this belief in a pre-established harmony, which seemed to many of Hilbert's contemporaries a purely mystical conviction. The answer has to be sought in the nature of mathematics since, according to Hilbert, it is mathematics that bridges the ideal gap between thought and nature: «The instrument which mediates between theory and practice, between thought and observation, is mathematics; it builds the connecting bridges, and makes them ever sounder» (Hilbert 1930, p. 1163). However, in his 1930 paper Hilbert seems to defend a radical empiricism towards the laws of natural science, but he does not express any clear thesis about the nature of mathematics. One has to go back to *Mathematical*

Problems (1900) to find a specific claim about mathematics which mirrors the later one about natural science. That is, mathematics – or at least mathematical problems – comes from two interrelated sources: human experience and pure thought (Hilbert 1900, pp. 1098-99).

In particular, Hilbert believes that «the first and oldest problems in every branch of mathematics spring from experience and are suggested by the world of external phenomena». This is how all the empirical methods of mathematics, such as rules of calculation with integers, were discovered and even more complex problems arose, such as solutions for numerical equations, rules for the differential and integral calculus, etc. Be this as it may, a second phase soon appeared in the evolution of mathematical thought: «[T]he human mind, encouraged by the success of its solutions, becomes conscious of its independence. It evolves from itself alone, often without appreciable influence from without, by means of logical combination, generalization, specialization, by separating and collecting ideas in fortunate ways, new and fruitful problems, and appears then itself as the real questioner». For example, this is how mathematicians discovered problems in number theory and answered most of the finer questions on the theory of functions. Only later, according to Hilbert, did experience take its own revenge: «[W]hile the creative power of pure reason is at work, the outer world again comes into play, forces upon us new questions from actual experience, opens up new branches of mathematics, and while we seek to conquer these new fields of knowledge for the realm of pure thought, we often find the answers to old and unsolved problems and thus at the same time advance most successfully the old theory». Here the point is that Hilbert explicitly says that this «ever-recurring interplay between thought and experience» generates the harmony mathematicians so often perceive in various branches of science.

Hence I shall conclude that, according to Hilbert, the realization, or embodiment, of mathematical thought in science is due to its double nature, which is to be found both in pure thought and in human experience. In particular, mathematical constructions – i.e. models – achieve successful applications because mathematics itself maintains a more or less remote link with its natural origin, i.e. human experience. From this perspective success must not be considered a miracle.

It has been claimed that von Neumann «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 hand, the ontological problem became a pragmatic one» (Rédei and Stöltzner 2006, p. 246). I am only partly satisfied with this claim. I think that, as a general attitude, it is true that in von Neumann's hands the ontological problem becomes pragmatic; likewise it is true that he never expressed himself using mystical terms such as «pre-established harmony». And yet, I do believe that there is enough evidence to affirm that von Neumann shares with Hilbert the same ontological assumptions on the nature of mathematics which, in the end, lend support to – at least in Hilbert – the thesis of the pre-established harmony between thought and nature. Let me review this evidence in more detail (von Neumann 1947, pp. 1-9).

Von Neumann is firmly convinced that «much of the best mathematical inspiration comes from experience». By the word «experience» he means, besides the experience of everyday life, the essential background related to any science «which interprets experience on a higher than purely descriptive level». He provides three key examples which document this remote empirical origin with regard to certain mathematical disciplines: i.e. geometry, calculus and the foundations of mathematics. While the evolution of the first two examples – i.e. geometry and calculus – would only apparently manifest a drastic and progressive separation from empiricism which, to tell the truth, has never been entirely completed, the controversy over the foundations of mathematics has undoubtedly proven, according to von Neumann, that in all that is mathematical one cannot assume «the existence of an absolute, immutable concept of mathematical rigour, dissociated from all human experience». Thus, something else besides abstraction – something that comes from experience – must enter into the makeup of this science.

However, von Neumann is also aware that in various important areas of modern mathematics this «empirical origin is untraceable, or, if traceable, so remote that it is clear that the subject has undergone a complete metamorphosis since it was cut off from its empirical roots». Algebra represents a good example in this sense: its symbolism was invented for everyday use but it has been practiced in a very modern and abstract fashion which seems far from its initial empirical ties. Even more significant are differential geometry and group theory, because both have been pursued in a radical non-applied spirit, but strangely enough they have turned out to be, in the end, very useful in physics.

Now the point is to ascertain how this applicability to science comes about. As for me, I would like to stress the fact that von Neumann, just like Hilbert, believes that there is this «quite peculiar duplicity in the nature of mathematics», namely that its origin is to be found both in human experience and in pure thought. In other words, besides its abstract nature, mathematics has also an empirical nature. Otherwise, he would have talked about the success that mathematics – and through mathematics, science – achieves in applications in terms of a miracle or an unreasonable effectiveness. In *The Mathematician* (1947) he writes:

> I think that it is a relatively good approximation to truth […] that mathematical ideas originate in empirics, although the genealogy is sometimes long and obscure. But, once they are so conceived, the subject begins to live a peculiar 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. […]. It becomes more and more purely aestheticizing, more and more purely *l'art pour l'art*. […]. [A]t a great distance from its empirical source, or after much "abstract" inbreeding, a mathematical subject is in danger of degeneration. […].
>
> In any event, whether this stage is reached, the only remedy seems to me to be the rejuvenating return to the source: the reinjection of more or less directly empirical ideas. (von Neumann 1947, p. 9)

To sum up, I do not seek to claim that von Neumann shares with Hilbert the faith in the notion of a pre-established harmony between thought and nature. My only aim is to show that there is enough evidence to state that they share the same ontological assumptions on the nature of mathematical thought. Only in a second stage do these assumptions end up, in Hilbert, forming the basis for the pre-established harmony hypothesis. However, I think that they could provide a reasonable explanation of the most enigmatic point – at least for me – in von Neumann's methodology of science: how one can reconcile the idea that a mathematical model is constructed to satisfy certain opportunistic conditions with the thesis that the model satisfies, at the same time, certain standards of objectivity, or even certain standards of truth.

6. Concluding Remarks

What I have tried to do is to describe John von Neumann's opportunistic methodology of science. His closeness to Hilbert's spirit is evident. Von Neumann always shared with Hilbert a conservative attitude towards science, combined with a strong faith in the axiomatic method. Yet he never became dogmatic. After Gödel's remarkable results he began a profound revaluation of the concept of mathematical rigour in order to find a solution to the epistemological problem left open in Hilbert's program, i.e. the justification of mathematical and scientific theories formulated in an axiomatic fashion. He found the solution in the criterion of success – not alien to Hilbert's school – to which he ascribed a key foundational task. With regard to Hilbert's program he made exactly the opposite move: he thought that through the externalization of the epistemological problem, mathematics finds its justification in science, while science finds its own in the realm of applications. In spite of his radical opportunism, von Neumann also believed that mathematical models satisfy certain standards of objectivity. However, in his view these two apparently contrasting ideas – opportunism and objectivity – do not contradict each other, but can be reconciled on the basis of certain ontological assumptions on the nature of mathematics which he, once again, shared with Hilbert. Mathematics has a double origin, which is to be found in pure thought and in human experience. Once this has been accepted, success in the realm of applications is not a miracle, but is probably due to the original empirical source, sometimes remote and obscure, latent in all mathematical thought. I would say that John von Neumann was a pioneer faithful to tradition.

7. References

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8. Table

Comparison between Hilbert's and von Neumann's foundational standpoints.

	Hilbert *Methodology of the* *Finitist Program*	**von Neumann** *Opportunistic* *Methodology of Science*
Foundations *Of mathematics*	*a)* – Axiomatic presentation of informal mathematical theories *b)* – Justification through finitist consistency proof	*a)* – Axiomatic presentation of informal mathematical theories *b)* – Justification through the criterion of success in science
Foundations *of sciences*	*a)* – Axiomatic presentation of informal scientific theories *b)* – Justification through relative consistency proof in mathematics	*a)* – Axiomatic presentation of informal scientific theories *b)* – Justification through the criterion of success in applications
Outcomes	*Internalization* of the epistemological problem: finitist mathematics is a part of classical mathematics	*Externalization* of the epistemological problem: mathematics in science and science in applications