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Von Neumann and impossibility, from Gödel to EDVAC

1 Introduction

Jesper Lützen's idea of a long-durée exploration of impossibility results and their role in the history of mathematics provides a very original and fresh perspective from which to analyze important developments in the discipline. Several impossibility results have played, at various historical stages, different roles in shaping the organization of entire branches of mathematics. They have also led to new, specific results and to reconceptualization of fundamental ideas. They have led to both dead ends and creative innovations. In Lützen's own words:

> [D]espite their negative nature, impossibility theorems rarely bar progress. On the contrary, they often result in vigorous activity. For example, the discovery of incommensurability (the first rigorous impossibility proof) led the ancient Greeks to a great number of important theories. Similarly, the discovery of the unsolvability of the quintic led to the development of Galois theory and much of modern algebra. In the same vein, much of the activity in voting theory was a reaction to Condorcet's paradox, and the development of social choice theory can be (and has been) considered a response to Arrow's impossibility theorem. To circumvent the impossible has always been a strong driving force in many areas of life. [1, p. 85]

Inspired by Lützen's broad perspective I explore the narrower question of how an individual mathematician, John von Neumann, reacted throughout his career to four different situations that involved impossibilities of various kinds. These are: (1) Gödel's incompleteness theorem; (2) the impossibility theorem of hidden variables in quantum mechanics; (3) Turing's undecidability proof for the *Entscheidungsproblem*; (4) impossibilities related to the design, implementation, and use of electronic computers. Von Neumann was an extremely versatile mathematician and, as we shall see now, he was also versatile in his reactions to these four situations.

2 Göttingen and Gödel

After graduating as a chemical engineer from ETH Zurich in 1926, while simultaneously completing the requirements for his PhD in mathematics from the University of Budapest, von Neumann went to the University of Göttingen for research work under David Hilbert. The vibrant scientific atmosphere at Göttingen and the personal-

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ity of Hilbert were for him a source of unparalleled attraction, much as they were at the time for many among the most ambitious and talented aspiring young scientists in Europe and beyond. Von Neumann felt particularly attracted to Hilbert's current involvement with foundational questions of mathematics and physics, and the axiomatization program. In his application for the Rockefeller Foundation Scholarship that would bring him to Göttingen, von Neumann explicitly wrote:

> Research over the bases of mathematics and of the general theory of sets, especially Hilbert's theory of uncontradictoriness [. . .], [investigations which] have the purpose of clearing up the nature of antinomies of the general theory of sets, and thereby to securely establish the classical foundations of mathematics. Such research renders it possible to explain critically the doubts which have arisen in mathematics. (Quoted in [2, p. 46])

Hilbert and Courant were thrilled about the prospects of adding this promising talent to the gallery of local rising stars. In spite of his youth, von Neumann was considered to be "a completely exceptional personality . . . who has already done very productive work . . . and whose future development is being watched with great expectation in many places" [3, p. 336]. Soon after his arrival, von Neumann became strongly involved in the many fields of activity pursued by members of the Göttingen community, while joining forces with Hilbert's young collaborators, Wilhelm Ackermann and Paul Bernays.

Beginning in 1926, von Neumann published a series of important papers where he introduced his own axiomatic system for set theory, and initiated a well-elaborated attack on the question of the consistency of first-order arithmetic [4–6]. Relying on the techniques developed by Ackermann and inspired by the finitistic approach of the Hilbert school, he first succeeded in proving the consistency of a fragment of the arithmetic of natural numbers, via a restriction of the kind of induction allowed, and then continued to look for a general proof of the consistency of larger parts of classical mathematics using methods from proof theory as known then [7]. Before 1930, he had published 32 major papers, including an important cluster on the foundations of set theory. With the foundational debate then at its apogee, von Neumann sided with Hilbert, while explicitly opposing the intuitionist views of Brouwer and Weyl and their willingness to "sacrifice much of mathematics and set theory." He likewise criticized the logicists' "attempts to build mathematics on the axiom of reducibility."

This highly successful part of von Neumann's early career came to an end, however, following the events of September 1930, at the Second Conference for Epistemology of the Exact Sciences, held in Königsberg. This was the place where Kurt Gödel, then a totally unknown young logician from Vienna, reported on his first incompleteness theorem. He showed, as it is well known, that in any consistent and sufficiently strong formal theory, such as the one typically taken to define arithmetic, there are true statements that are unprovable within the system, or, otherwise stated, there are statements which can neither be proved nor disproved in the system.

Von Neumann was in the audience and soon acknowledged the significance of Gödel's result for Hilbert's program and started to think about it in greater detail. In a famous letter exchange with Gödel, von Neumann indicated that he had found the evident corollary to the incompleteness result, but at the same time he reassured Gödel that he would not publish on the topic, as it was the latter who had established the theorem as a natural continuation and deepening of his earlier results. Gödel had found the same result already independently, which is what became known as the second incompleteness theorem. It states that such a formal system, if indeed consistent, cannot prove its own consistence. Upon studying Gödel's paper in early 1931, Bernays too realized the consequences of the incompleteness theorem presented in Königsberg (For details see [8, pp. 135–137; 9, pp. 327–330]).

There is some room for debate concerning the details of how von Neumann, starting from the early versions of Gödel's first incompleteness theorem, concluded the impossibility of proving the consistency of arithmetic [10]. It is clear, at any rate, that von Neumann believed that the methods developed by Ackermann in working out the details of Hilbert's metamathematical ideas had helped Gödel come up with what was "the greatest logical discovery in a long time." Von Neumann saw these results as undermining the entire finitist consistency program as envisioned by Hilbert. Jacques Herbrand – who, in a paper written before his untimely death at age 23, had established the consistency of a fragment of arithmetic by elementary metamathematical means, and was thus the other mathematician who prominently contributed to this thread – agreed with von Neumann in this regard. Gödel and Bernays, in turn, believed that, to the contrary, these results actually opened the way for a continued, fruitful development of a well-elaborated discipline of proof theory [11–13].

In terms of von Neumann's own career and the research directions he went on to pursue, however, this disagreement was beyond debate. After the correspondence where he interchanged ideas with Gödel concerning the technical details and the overall significance of the incompleteness and consistency results, von Neumann in early 1931 definitely abandoned his active involvement with the foundations of mathematics. He did further work on logic, but in different directions, namely in relation with quantum logic and the foundations of the theory of finite automata.

Von Neumann's reaction to Gödel's result was clear-cut and straightforward: it signaled the end of von Neumann's own activities in a field that, inaugurated by Hilbert and his collaborators, reached a dramatic turning point when Gödel announced his impossibility result. In the case of quantum mechanics however, things worked differently as we will see now.

3 Göttingen and quantum mechanics

While still working on the foundations of set theory in the late 1920s, von Neumann also devoted efforts to questions related with the foundations of the recently created field of quantum mechanics, for which Göttingen counted as one of its most active hotbeds. An important result arising in this part of von Neumann's scientific activities is a famous and often controversial impossibility result, known as the impossibility of formulating an extended version of the theory that would include "hidden variables." Remarkably, very much as with his involvement with logic and set theory, this part of von Neumann's scientific career is strongly marked by the influence of Hilbert's ideas, and specifically with the program for the axiomatization of physics, about which a brief clarification is in order here, before explaining some details about von Neumann's contribution the field.

Hilbert's program for the overall axiomatization of mathematics and physics is sometimes conflated with his finitistic program and they are mistakenly conceived as being one and the same thing. The former, however, pervaded most of Hilbert's career and was conceived as a much broader undertaking, whereas the latter was circumscribed to a specific task to which Hilbert devoted concentrated efforts with his collaborators, beginning around 1920. As I have discussed in great detail the meaning of the axiomatization program and how Hilbert implemented it through the years (see, e.g. [14]), I will limit myself here to state that what in von Neumann's view had reached a dead end in the wake of Gödel's results was the set of very specific questions related with the possibility of proving the consistency of arithmetic using finitary methods. His decision to abandon ongoing efforts connected with this task did not at all imply that he would relinquish any endeavors related to the broader axiomatization program, and much less that he would forsake the overall vision of science that this program envisaged. He approached the current, dramatic developments taking place in the attempts to understand basic questions about the structure of matter by following a tradition that Hilbert had inaugurated since his arrival in Göttingen in 1895, and in a more focused fashion in his seminars on these topics in Göttingen, beginning in 1914 [15, pp. 287–310]. Hilbert's own famous incursion into General Relativity Theory had also derived from this very source [14, Ch. 7].

A main foundational issue that arose right from the early days of the new theory concerned the indeterministic character of the wave function lying at its core, particularly in Schrödinger's version of the theory, and the concomitant probabilistic interpretation introduced by Max Born in 1926. Born, for one, was "inclined to give up determinism in the world of atoms," and he considered this issue to involve "a philosophical question for which physical arguments alone are not decisive" [16, p. 866]. Soon thereafter, Bohr and Heisenberg developed the Copenhagen interpretation, according to which the probabilistic nature of quantum mechanics is *unavoidable*, with Heisenberg's uncertainty principle allowing for a quantitative expression of the bounds of indeterminacy. Einstein, to the contrary, opposed any kind of indeterministic understating of the physical world, and he famously asserted that quantum mechanics – as then known – was an incomplete theory. "Quantum mechanics – he declared – is very worthy of respect. But an inner voice tells me this is not the genuine article after all" [17, p. 403].

A possible way to reach a deterministic version of quantum mechanics and to avoid quantum indeterminacy was to consider the current theory as representing a statistical approximation of a yet unknown, extended, deterministic one. Any elaboration of this approach would imply that all observables in the theory have defined values which are determined by unknown, "hidden variables." Assuming the existence of such hidden variables would provide answers to other concerns related with the new theory, such as the difficulties raised by the idea of measurement in quantum mechanics, the existence of non-local effects of entangled states, and the unexplained transition from a microscopic world dominated by probabilistic laws to a macroscopic one perfectly described by the deterministic laws of classical mechanics. The celebrated paper of 1935 by Einstein, Podolsky, and Rosen contained important suggestions on how to develop such ideas.

The beginning of von Neumann's own involvement with quantum mechanics was marked by a seminal paper authored in 1928 in collaboration with Hilbert and his assistant Lothar Nordheim [18]. It presented an axiomatization in the spirit of Hilbert's program for physical theories and opened the way to all subsequent axiomatic treatments of quantum mechanics, be it by von Neumann himself or by others. This was followed by a series of brilliant papers, written while still in Göttingen, which eventually converged in the famous treatise *Mathematische Grundlagen der Quantenmechanik* [19]. Subsequently, he also published additional important work in collaboration with Pascual Jordan, Eugene Wigner, and Garret Birkhoff.

In his book, von Neumann presented a new impossibility result concerning the existence of hidden variables in quantum mechanics. It was meant to show that the theory as defined in his own axiomatized version could not be extended to include additional variables, and that no theory of hidden variables could faithfully reproduce all the experimental predictions of quantum mechanics. In the introduction to the book, von Neumann stated his aims and explained the way in which axiomatic considerations would underlie his entire approach:

> In the analysis of the fundamental questions, it will be shown how the statistical formulas of quantum mechanics can be derived from a few qualitative, basic assumptions. Furthermore, there will be a detailed discussion of the problem as to whether it is possible to trace the statistical character of quantum mechanics to an ambiguity (i.e., incompleteness) in our description of nature. [. . .]. This explanation "by hidden parameters," as well as another, related to it, which ascribes the "hidden parameter" to the observer and not to the observed system, has been proposed more than once. However, it will appear that this can scarcely succeed in a satisfactory way, or more precisely, such an explanation is incompatible with certain qualitative fundamental postulates of quantum mechanics. [20, p. 2]

Without going into any detail about the proof or the reactions it elicited, it is important to indicate that as early as 1935 Grete Hermann presented her criticism to the result and its implications, but her views had little impact. Until the 1960s the proof was considered to establish the certainty of the Copenhagen interpretation. This started to change in 1952 with the publication of David Bohm's new theory of hidden variables, which was followed in 1966 by John Bell's suggestions on how to reintroduce such variables while criticizing the validity of von Neumann's proof. The entire discussion has been recently reappraised (see, e.g. [21]). What I want to stress here, however, is the nature of von Neumann's own views on the issue.

As Jesper Lützen has indicated in his book (following Dieks), von Neumann did not claim that his proof contradicted the possibility of introducing hidden variables in quantum theory. Rather, he proved the impossibility of doing so *in von Neumann's own axiomatized formulation of the theory*, in which the observables are described in very specific mathematical formulations, namely in terms of Hermitian operators on the Hilbert space. Lützen also points to the following quotation appearing in von Neumann's 1932 [19] book, which is crucial for understanding this point:

> The only formal theory existing at the present time which orders and summarizes 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 serious lacunae, and it may even be that it is false, although this latter possibility is highly unlikely in view of the theory's startling capacity in the qualitative explanation of general problems and the quantitative success of calculations relating to special problems. In spite of the fact that quantum mechanics agrees well with experiment, and that it has opened up for us a qualitatively new side of the world, one can never say of the theory that it has been proved by experience, but only that it is the best known summarization of experience. However, mindful of such precautions, we may still say that there is at present no occasion and no reason to speak of causality in nature. Because no experiment indicates its presence: the macroscopic are unsuitable in principle, while the only known theory which is compatible with our experiences relative to elementary processes – quantum mechanics – contradicts it. [20, p. 213]

The most important point to notice is the extent to which von Neumann's early work on quantum mechanics, of which his impossibility proof is part and parcel, was in fact a thorough elaboration of the principles underlying Hilbert's program for the axiomatization of physics as put forward in his 1900 address and variously realized throughout his career. An axiomatic analysis such as the one lying at the basis of von Neumann's analysis was not one to be applied to a formal game of meaningless symbols, as developed in the Hilbert school as part of the attempt to prove the consistency of arithmetic by finitistic means. Rather it was an analysis that should apply to a *well-elaborated physical theory so that it becomes a fully mathematical theory* on which significant mathematical results can be proved. As early as 1898–99, in a course on mechanics taught at Göttingen, Hilbert had formulated this idea very clearly in relation with geometry, in the following terms:

> Geometry also [like mechanics] emerges from the observation of nature, from experience. To this extent, it is an *experimental science* . . . But its experimental foundations are so irrefutably and so *generally acknowledged*, they have been confirmed to such a degree, that no further proof of them is deemed necessary. Moreover, all that is needed is to derive these foundations from a minimal set of *independent axioms* and thus to construct the whole edifice of geometry by *purely logical means*. In this way [i.e., by means of the axiomatic treatment] geometry is turned into a

pure mathematical science. In mechanics it is also the case that all physicists recognize its most *basic facts*. But the *arrangement* of the basic concepts is still subject to a change in perception . . . and therefore mechanics cannot yet be described today as a *pure mathematical* discipline, at least to the same extent that geometry is. We must strive that it becomes one. We must ever stretch the limits of pure mathematics wider, on behalf not only of our mathematical interest, but rather of the interest of science in general. (Quoted in [14, p. 90]. Italics in the original.)

One important idea that dominated Hilbert's view in this regard is that adding a new assumption to an existing physical theory – even if it seems plausible in and by itself – may lead to serious problems if its logical compatibility with the theory is not examined carefully. Thus, one of the aims of the axiomatic analysis of physical theories is precisely to allow for such an analysis. Hilbert wrote about this in his correspondence with Frege in 1900:

After a concept has been fixed completely and unequivocally, it is on my view completely illicit and illogical to add an axiom – a mistake made very frequently, especially by physicists. By setting up one new axiom after another in the course of their investigations, without confronting them with the assumptions they made earlier, and without showing that they do not contradict a fact that follows from the axioms they set up earlier, physicists often allow sheer nonsense to appear in their investigations. One of the main sources of mistakes and misunderstandings in modern physical investigations is precisely the procedure of setting up an axiom, appealing to its truth (?), and inferring from this that it is compatible with the defined concepts. One of the main purposes of my [book on the Foundations of Geometry] was to avoid this mistake. (Quoted in [14, p. 114])

As I have shown in detail elsewhere, significant manifestations of the influence of this crucial aspect of Hilbert's axiomatic program for physics continued to dominate much of the efforts of the Göttingen community over the coming decades. Minkowski's analysis of the principle of relativity, vis-à-vis mechanics and electrodynamics is a prominent example of this [14, Ch. 4]. A less known instance worthy of mention here relates to the work of Constantin Carathéodory in 1925 on the foundations of thermodynamics. To be sure, this work had a rather reduced impact among physicists, but it nicely illustrates, from a different perspective, the mindset followed by von Neumann in his analysis of the role of hidden variables. Carathéodory explained the aims of his analysis using the same kind of rhetoric typical of Hilbert, as follows:

If one believes that geometry should be seen as the first chapter of mathematical physics, it seems judicious to treat other portions of this discipline in the same manner as geometry. In order to do so, we are in possession since ancient times of a method that leaves nothing to be desired in terms of clarity, and that is so perfect that it has been impossible ever since to improve essentially on it. Newton felt this already when trying to present his mechanics also in an external form that would fit the classical model of geometry. It is quite remarkable that with even less effort than in mechanics, classical thermodynamics can be treated by the same methods as geometry.

This method consists in the following:

1. Create thought experiments, as in the case of geometry, constructing figures or moving around spaces figures already constructed.
2. Apply to these thought experiments the axioms that the objects considered are supposed in general to satisfy.
3. Extract the logical conclusion that follows from the given premises. [22, pp. 176–77]

The logical conclusions that both Minkwoski and Carathéodory investigated in their respective domains, and also von Neumann did for hidden variables along the same tradition in the framework of his axiomatized presentation of quantum mechanics, concerned in the first place the contradiction, or lack thereof, obtained when attempting to add a new, significant assumption into an existing, well-elaborated physical theory. Once again this is echoed in a passage taken from his 1932 book (p. 136):

> Explanations by means of hidden parameters have (in classical mechanics) reduced many apparently statistical relations to the causal foundations of mechanics. An example of this is the kinetic theory of gases . . . Whether or not an explanation of this type, by means of hidden parameters, is possible for quantum mechanics is a much-discussed question. . . . If it were correct, it would brand the present form of the theory provisional, since then the description of states would be essentially incomplete. . . . Until a more precise analysis of the statements of quantum mechanics enables us to prove objectively the possibility of introducing hidden parameters . . . we shall abandon this possible explanation [and] . . . admit as a fact that the natural laws which govern elementary processes (i.e., the laws of quantum mechanics) are of a statistical nature.

But there is also another aspect that must be mentioned in the framework of this analysis, and that is the somewhat elusive idea of “completeness.” On the one hand, it is remarkable that this issue is not at all mentioned in von Neumann’s important articles of the late 1920s, and it only appears for the first time in the book, published in 1932, namely after he became acquainted with Gödel’s results in the Königsberg meeting and through their correspondence. One might thus argue that the very idea of discussing incompleteness in the framework of a physical theory was strongly motivated by what von Neumann had learnt from Gödel’s work. This is totally unlikely, however. The kind of completeness discussed in von Neumann’s analysis of quantum mechanics and in his exclusion of hidden variables is not the kind of *formal completeness* that Gödel introduced in his analysis of arithmetic. Rather, it is a kind of “*pragmatic completeness,*” in the sense that his axiomatic system establishes all that is needed for developing the expected results of the other. This had been from the beginning one of the requirements that Hilbert thought as fundamental for any successful axiomatic treatment of geometry and of the various physical disciplines originally envisaged in his program. In a course on the foundations of geometry taught back in 1893–94, he had formulated this idea as follows:

> The problem can be formulated as follows: What are the necessary, sufficient, and mutually independent conditions that must be postulated for a system of things, in order that any of their properties correspond to a geometrical fact and, conversely, in order that a complete description and arrangement of all the geometrical facts be possible by means of this system of things. (Quoted in [14, p. 87].)

Hilbert required that an adequate axiomatization of a mathematical discipline should allow for an actual derivation of *all* the theorems already known in that discipline. This was, Hilbert claimed, what the totality of his system of axioms did for Euclidean geometry or, if the axiom of parallels is ignored, for the so-called absolute geometry, namely that which is valid independently of the latter. In the second edition of *Grundlagen der Geometrie*, moreover, published in 1903, Hilbert added a new axiom, the so-called “axiom of completeness (*Vollständigkeitsaxiom*), meant to ensure that, although infinitely many incomplete models satisfy all the other axioms, there is only one complete model that satisfies this last axiom as well, namely, the usual Cartesian geometry. Even when he started to work specifically on the issue of the foundations of arithmetic, Hilbert never envisaged the possibility of a situation of formal incompleteness such as Gödel’s results brought to the fore.

Von Neumann’s reliance on the mathematical force of the axiomatic method to shed light on the structure of physical theories is forcefully illustrated in two letters where he emphasized the idea that his work on quantum mechanics moved in the borderlines between disciplines, without however crossing the borders that would take him into the work of the physicists, properly said. First is a letter of December 1947, to Ramón Ortiz Fronaguera, the translator of his book into to Spanish:

> Your questions on the nature of mathematical physics and theoretical physics are interesting but a little difficult to answer with precision in my own mind. I have always drawn a somewhat vague line of demarcation between the two subjects, but it was really more a difference in distribution of emphasis. I think that in theoretical physics the main emphasis is on the connection with experimental physics and those methodological processes which lead to new theories and new formulations, whereas mathematical physics deals with the actual solution and mathematical execution of a theory which is assumed to be correct per se, or assumed to be correct for the sake of the discussion.
>
> In other words, I would say that theoretical physics deals rather with the formation and mathematical physics rather with the exploitation of physical theories. However, when a new theory has to be evaluated and compared with experience, both aspects mix. [23, pp. 118–119]

And a similar idea arises in a letter of October 1949 to H. Cirker, president of Dover Publications, concerning the difficulties expected to arise around the translation of his book to English. Von Neumann thus wrote:

> The subject-matter [of the book] is partly physical-mathematical, partly, however, a very involved conceptual critique of the logical foundations of various disciplines (theory of probability, thermodynamics, classical mechanics, classical statistical mechanics, quantum mechanics). This

> philosophical-epistemological discussion has to be continuously tied in and quite critically synchronized with the parallel mathematical-physical discussion. [23, pp. 91–92]

And the interesting point is that the ideas expressed here by von Neumann continually permeate all of Hilbert's involvement with physics. A remarkable example that illustrates this direct connection is found in the notes of a course taught in the winter semester of 1916–17 in Göttingen, on the General Theory of Relativity. This was the first systematic course ever taught in a German university on the topic and at that opportunity Hilbert told to his students:

> In the past, physics adopted the conclusions of geometry without further ado. This was justified insofar as not only the rough, but also the finest physical facts confirmed those conclusions. This was also the case when Gauss measured the sum of angles in a triangle and found that it equals two right ones. That is no longer the case for the new physics. *Modern physics must draw geometry into the realm of its investigations.* This is logical and natural: every science grows like a tree, of which not only the branches continually expand, but also the roots penetrate deeper. . . . the physicist must become a geometer, for otherwise he runs the risk of ceasing to be a physicist and vice-versa. The separation of the sciences into professions and faculties is an anthropological one, and it is thus foreign to reality as such. For a natural phenomenon does not ask about itself whether it is the business of a physicist or of a mathematician. On these grounds we should not be allowed to simply accept the axioms of geometry. The latter might be the expression of certain facts of experience that further experiments would contradict. (Cited in [14, p. 379]. Italics in the original.)

The case of von Neumann and the impossibility result related to hidden variables in quantum mechanics is thus clearly different from what we saw for the case of Gödel. In the first place, here it was von Neumann himself who came up with the result. This was for him not the end of a stage in his career, but rather, in important senses, the starting point of a very fruitful one. Von Neumann's devoted great amount of energy to the broad cluster of research areas related with quantum mechanics and considered that to be one of his most important contributions to science. At the same time, however, it is remarkable that the two impossibility results discussed thus far were strongly connected with the work of Hilbert though from different directions and with contrasting consequences. For von Neumann, the correspondence between the available empirical supports for quantum mechanics was so strong that there was no room for calling into question its basic assumptions and theoretical structure. At the same time, his mathematical analysis led to the impossibility result, based on a certain axiomatic formulation of the theory. Along the lines of Hilbert's axiomatization program, in its broader sense, von Neumann's result did not cross the lines into the core of the realm of physics and it left open the possibility of alternative, perhaps improved formulations of quantum mechanics, where the Hilbert Space perspective might be removed from its centrality and the impossibility result would be in need of further reconsideration.

4 Princeton and Turing machines

New impossibility results on computability arose in 1936 in the work of Alan Turing on Hilbert's *Entscheidungsproblem*. This is yet another context where von Neumann's attitude is of interest, if only because his name is often mentioned in conjunction with that of Turing whenever reference is made to the question of the "fatherhood of the modern electronic computer," and particularly to the crucial idea of the stored-program architecture. The architecture, typically named after von Neumann, was described in 1945 in the famous "First draft of a report on the EDVAC" [24]. Both the EDVAC and the IAS machine, built between 1945 and 1951 at Princeton, embodied many of the principles that von Neumann had first introduced in his Draft of 1945. At nearly the same time, Turing became involved at Manchester in the design and construction of the ACE machine, based on a stored-program architecture, which he described in a document called "Proposed Electronic Calculator" [25].

An interesting historical debate revolves around the purported connection between the idea of the Universal Turing Machine (UTM), as introduced in the seminal article [26], and the design and implementation of these two machines. In several accounts, the latter are unambiguously described as direct implementations of the former. The following quotation expresses this idea forcefully:

> What Turing described in 1936 was not an abstract mathematical notion but a solid three-dimensional machine (containing, as he said, wheels, levers, and paper tape); and the cardinal problem in electronic computing's pioneering years, taken on by both 'Proposed Electronic Calculator' and the 'First Draft,' was just this: How best to build a practical electronic form of the universal Turing machine? [27, p. 73]

In a recent article [28], I have argued that in order to attain a complete and balanced historical picture of this issue, one must explicitly abandon the idea of a straightforward (let alone necessary) transition from the mathematical idea of 1936 to the physical machine (or even the design of that machine) in 1945. Moreover, to the extent that early stored-program computers of the mid-forties can be seen as physically embodying ideas discussed in "Computable Numbers," this is mostly a result of hindsight and it says little about Turing's ideas before the war. This is not the place to go into the details of my argument or of some possible reactions to it (on which see [29]). What is relevant for the present account is what can be said in relation with this topic, about von Neumann's attitude towards Turing's early work, and in particular towards the proof that a general solution to the *Entscheidungsproblem* is impossible (of which an equivalent result was given by Alonzo Church at nearly the same time, using a different approach [30]).

Still as a graduate student during September 1936 to July 1938, Turing accepted an invitation to Princeton, where since 1933 von Neumann had been professor of mathematics at the Institute for Advanced Study. Turing had become aware of Church's result very soon after its publication in1936, and he sent his own paper for publication in August 1936, with an appendix proving the equivalence of both approaches and of the ensuing results. Church, in turn, published a now famous review of Turing's results [31], where he also introduced the term "Turing Machines." Turing's trip to Princeton materialized through an invitation of Church. Turing attended his lectures and wrote his PhD dissertation under Church's supervision.

At the same time, however, given their mutual scientific interests and their joint later prominence in the story of the modern computer it may be natural to assume that this was a period of intense intellectual interchange and collaboration between Turing and von Neumann. In particular, it may sound natural to assume that ideas about computing were a main topic of common interest for them. A closer look at the record, however, tells a completely different story. Direct evidence for the nature of their relationship at the time appears in a letter written by Turing on October 6, 1936, soon after his arrival in Princeton:

> The mathematics department here comes fully up to expectations. There is a great number of the most distinguished mathematicians here. J. v. Neumann, Weyl, Courant, Hardy, Einstein, Lefschetz, as well as hosts of smaller fry. Unfortunately there are not nearly so many logic people here as last year. Church is here of course, but Gödel, Kleene, Rosser and Bernays who were here last year have left. I don't think I mind very much missing any of these except Gödel. (Quoted in [32, p. 149].)

As we can see, Turing did not include von Neumann among the "logic people," and this is obviously related to the fact that right after becoming aware of Gödel's results in 1930, von Neumann abandoned all active involvement with logic. This applied to Turing's work as well and it explains his lack of interest in Turing's result concerning the *Entscheidungsproblem*, which was at the core of Hilbert's involvement with logic. But the case is that also the other prominent mathematicians listed by Turing in his letter showed no particular interest in the newcomer from Cambridge and in his work. As a matter of fact, Alonzo Church was Turing's only real interlocutor on logic while at Princeton. Moreover, at the personal level both Turing and Church were far from the extroverted style of von Neumann and all evidence indicates that there was no friendly or professional relationship with him.

Church made some active attempts to make Turing's work better known in Princeton, but these turned out to be unsuccessful. Shortly before "Computable Numbers" was about to be published, Church urged Turing to deliver a talk before the distinguished local mathematical community. Obviously, Turing was thrilled about the opportunity and thought it might bring greater attention to his work. However, it all ended up in disappointment, as we read in one of his letters at the time:

> There was rather bad attendance at the Maths Club for my lecture on Dec. 2. One should have a reputation if one hopes to be listened to. (Quoted in [32, p. 157].)

When Turing's article was finally published in the *Proceedings of the London Mathematical Society* by late 1936, he was badly disappointed by the rather limited reaction –

besides Church's review essay – it aroused. Only two persons are known to have requested off-prints. It seems that he did not even expect von Neumann to react in any way to his paper, and besides the letter quoted above, von Neumann is not mentioned in any of the letters that Turing wrote from Princeton in 1936–37, to either his mother or to his teacher in Cambridge, Philip Hall. Turing was particularly disappointed by the lack of reaction on the side of Hermann Weyl, a most prominent member of Hilbert's inner circle and a main figure in the late 1920s debates around the Hilbert program. Weyl never made a single remark to him about his paper and about his research on this topic [32, p. 158].

It is also well-known that in April 1938, von Neumann approached his younger colleague to offer him a job as assistant. This is sometimes taken as further evidence of their common interests at the time and as reinforcing the assumption of a putative interchange of ideas around computing and computers. It is also known that Turing turned down the tempting offer. Turing's fellowship at Cambridge had been just renewed, and he was not eager to remain in the USA anyway. These may have been among the main reasons for Turing's decision. But what about von Neumann's motivations? Hodges [32, pp. 183–184] suggests, that by this time he "was aware of *Computable Numbers*, even if had not been a year earlier." While there is no direct evidence for this claim (which is nevertheless somewhat likely), it is more than evident that the offer had nothing to do with a direct interest in Turing's work on computability and logic, either as developed in the now famous article of 1936 or as then pursued as part of his PhD dissertation. Indeed, back in June 1937, von Neumann had written a letter of recommendation on behalf of Turing and there he indicated that Turing "had done good work in branches of mathematics in which I am interested, namely: theory of almost periodic functions and theory of continuous groups." Indeed, Turing had recently published two related articles [33, 34]. Von Neumann, let me emphasize once again, had completely abandoned his interest in logic and there is no indication that at the time he had in anyway started to think about computing machines or even about mathematical topics related with massive calculations.

One cannot outright dismiss the claim that "von Neumann had learned of the universal Turing machine before the war," but there is certainly no evidence that, if he did, he devoted much attention to it, certainly not in the sense of leading to the idea of an electronic, stored-program physical machine. The very term "machine" has led to much confusion about Turing's work. Turing's "machines" of 1936 provided a *mathematical model* meant to analyze the nature and scope of the calculations that individual *human computers* could and did effectively perform. And if we look at the PhD dissertation he wrote while at Princeton, this non-material aspect of his approach becomes even more perspicuous. In the dissertation, Turing introduced the innovative idea of "oracle" that "cannot be a machine" (in the sense of the 1936 article) and which, by definition, involves "some unspecified means of solving number theoretic problems." With the help of the oracle, Turing said, "we could form a new kind of machine (call them *o*-machines), having as one of its fundamental processes" the ability to solve certain number theoretic problems (such as the twin prime conjecture and Fermat's last theorem) [35]. There is nothing in the way that "machines" are referred to in Turing's PhD thesis that may be taken to suggest the idea of building an actual device. Much less does the text suggest that a UTM should be taken as the most appropriate basis for building some kind of "general purpose," or "stored-program" calculator. In exploring the capabilities of the *o*-machines, Turing meant to explore aspects of mathematical proof and of calculation that would not be covered by the "machine" as defined in 1936.

We come across a third instance, then, of impossibility results that can be mentioned in relation with von Neumann's scientific career, namely the results associated with Turing's pioneering work on computability. In this case von Neumann's reaction was one of total indifference and lack of attention. Von Neumann and Turing came across each other while at Princeton but their interaction seems to have been rather limited in scope and intensity. To the extent that there was such an interaction, there is no indication that they devoted any time to discussing Turing's ideas on computability, oracles, and the consequences of his undecidability proof concerning the *Entscheidungsproblem*. There is no factual basis to the claim that they shared during these years anything like a "dream of building a universal stored-program computing machine" (as claimed in [36]).

Moreover, when von Neumann visited England in 1943, as part of his involvement with war-related activities, he most likely did not meet Turing or visited Bletchley Park. Von Neumann was not aware of Turing's current work on cryptography [37, p. 259], nor was code-breaking the direct motivation behind von Neumann's confession, in a letter of March 1943 to Oswald Veblen, that he would return home "a better and impurer man," after having developed "an obscene interest in computational techniques" [37, p. 27]. Rather, his entrance into the world of computing-intensive mathematical problems and the task of building actual computing machines to solve them – the next stage in von Neumann's career – arose from concerns that lay (at that time) far away from the idea of Universal Turing Machines or stored-program architectures. It is true, nonetheless, that much later and in retrospect, von Neumann acknowledged the importance and originality of Turing's 1936 paper as a precursor of the idea of the general purpose, stored-program computer as it came to be embodied in von Neumann's and Turing's respective electronic machines, but even then, Turing's undecidability result concerning the *Entscheidungsproblem* was not the issue that von Neumann indicated as truly important in that early work [38].

5 Electronic machines

The next impossibility situation that von Neumann faced in his career was one of a completely different kind, namely, that associated with the *technical* limitations encountered in working with specific computing machines. It can be dated to the outbreak of WWII, which signified for von Neumann, as it did for many other scientists, a watershed in his career. Wartime activity and its aftermath covered the last period of his life, which was particularly fruitful, and led him to the exploration of entirely new fields of activity before his untimely death at the age of 53. Over this period von Neumann became increasingly involved with questions of applied mathematics and numerical analysis. He became closely associated with government agencies and large war projects and with issues related to electronic computers. The latter strongly attracted his attention as a powerful tool for addressing immediate practical needs (war needs in the first place) but also as the key to opening new horizons in scientific research. Beginning in the mid-forties, von Neumann also further developed his interest in questions related to the possible application of mathematics to the social sciences. He explored the possibility of representing rational subjective human behavior in mathematical terms and, in strong connection to it, the challenge of studying the human mind and understanding it with the help of mathematics.

In fact, the global transition from pre- to post-WWII science is nowhere most clearly embodied – both at the material and the symbolic level – than in the iconic figure of von Neumann. This is true, in the first place, concerning the rise and consolidation of the USA as the new world-leading center of science. This is no less true concerning the displacement of the center of gravity in mathematics from the pure and analytic towards the more applied and computing-intensive. And it is perhaps even more significant, when it comes to the rise of a new kind of alliance between scientists and the main world centers of power, both economic and military [39, 40, 2, pp. 266–300].

But, as already suggested, von Neumann's enthusiastic embracing of electronic computers went beyond their possible use in practical contexts, and he was fully aware of their importance for research in pure mathematics and in basic science as well. It is interesting in this regard to look at a letter of October 1945 to Commodore Louis L. Strauss, a longtime admirer and supporter, who helped raise funds for the IAS machine, and later served as Chairman of the Atomic Energy Commission during the years 1953–58. Von Neumann explained to him:

> I feel sure that an electronic machine of the most advanced conceivable type should be constructed, not for use on specific applied mathematical or physical or engineering problems, but with the purpose of experimentation with the machine itself in order to develop new approximation and counting methods, and generally to acquire the mathematical and logical forms of thinking which are necessary for the really efficient orientation of such a device, with the methods it will have brought into existence. I have no doubt whatever that we are here on the threshold of very important developments both in pure mathematics and in its applications and that a pure research institute should spend several years in building a machine and experimenting with it. If we devote in this manner several years to experimentation with such a machine, without a need for immediate applications, we shall be much better off at the end of that period in every respect, including the applications. [23, p. 235]

In addition, von Neumann was also thrilled by the potential afforded by open classical problems of mathematics as a source of challenging tests for the computing power of the new machines (as well as for the programming skills of those in charge of operating them). In 1949, for instance, he suggested to use ENIAC to calculate values of π and e up to many decimal places [41]. Against the background of his interest at the time in questions related to randomness – and particularly in developing possible tests for checking randomness – he viewed the decimal expansions of these two numbers as useful sources of random sequences of integers where such tests could be initially tried.

In this new world of electronic computing, Neumann became aware, right from his early involvement, that issues of "impossibility" would inherently arise, but they would sensibly differ from those he had met in the early phases of his career, and which were discussed in the foregoing sections. He became now concerned with impossibilities associated with performing in actual machines specific calculation tasks within reasonable periods of time. In the letter to Louis L. Strauss cited above, he also commented on this matter:

> All existing methods of computing, or more broadly speaking of "approximation mathematics," as they were developed in the course of the last 150 years, are essentially conditioned by what was practically feasible during the period that is by the speeds of computation which were possible. These speeds changed considerably during this period, but at its close, that is in the immediate past, the fastest procedure that was at all assimilated by large group of mathematicians and computers, was the electrical "desk" computing machine, or the standard I.B.M. multiplier, both of which require about 10 s for 10-digit multiplication. [23, p. 236]

Such concerns had already arisen as part of von Neumann's earlier, war-related, computing-intensive involvement, namely, the need to solve partial differential equations, which were crucial for understanding the hydrodynamics phenomena behind explosions and implosions, and particularly those related with the development of the atomic bomb at Los Alamos. In fact, hydrodynamics soon became paradigmatic in von Neumann's quest for using computer-related methods of numerical analysis, particularly in addressing non-linear problems. In a memorandum written in March 1949 to Oswald Veblen, he described how his wartime work on nuclear reactions led him to realize that:

> Many problems which do not prima facie appear to be hydrodynamical necessitate the solution of hydrodynamical questions or lead to calculations of the hydrodynamical type. It should be noted that it is only natural that this should be so since hydrodynamical problems are the prototype for anything involving non-linear partial differential equations, particularly those of the hyperbolic or mixed type, hydrodynamics being a major physical guide in this important field, which is clearly too difficult at present from the purely mathematical point of view. (Quoted in [40, p. 401].)

In a letter of 1949 to Vannevar Bush at MIT, von Neumann expressed his concerns about the fact that "many organizations are now asking for computing machines, although they will be completely unable to use them and have only the most amorphous ideas as to how those might be useful for them" [23, p. 77]. More interestingly, he indicated that the "entire computing machine is merely one component of a greater whole, namely, the unity formed by the computing machine, the mathematical problems that go with it, and the type of planning which is called for by both."

One of von Neumann's remarkable papers on numerical methods, published in 1947 in collaboration with Hermann Goldstine (and its sequel of 1951) dealt with the all-important question of inverting matrices of higher orders, and the calculation of rigorous estimates of the errors that arise in such processes [42]. Beyond the remarkable techniques they introduced and the great impact they had on the entire field, these articles discussed general issues of principle related with their use, with special attention to the impossibility (again: material, not mathematical impossibility) of avoiding all the possible errors inherent in the use of the computer.

They considered four categories of errors that may happen independently and "that aggregate with one another as part of complex numerical calculations." The first two types relate to the very idea of mathematically modelling a physical situation, an issue of fundamental importance for von Neumann's overall scientific outlook. Inherent to such models is the fact that *only some phases of reality*, and not reality itself, can be represented using them. This concern is directly related to the idea of "Panmathematics," which, in many respects, summarizes the gist of von Neumann's current conceptions about mathematics and nature. The classical idea of mathematizing nature – originating in the seventeenth century with the works of Newton, Laplace and their likes – underwent in the hands of von Neumann a meaningful transformation towards the idea of modeling, which became manifest, in particular, in the form of computer-based modeling [43, pp. 57–76].

These two types comprise: (A) limitations derived from the kinds of idealizations, simplifications, and neglections that are unavoidable when mathematically formulating a physical situation; (B) observational errors in measuring the basic parameters of a given model. The third kind of problem, (C), relates to the issue of approaching by elementary arithmetical operations that the computer can handle directly, and in a finite number of steps, transcendental functions like sin or log, as well as operations like integration or differentiation. Any limiting process, "which in its strict mathematical form is infinite, must in a numerical computation be broken off at some finite stage, where the approximation to the limiting value is known to have reached a level that is considered to be satisfactory."

Von Neumann and Goldstine indicated, concerning errors of type (C), that their analysis applies to any kind of "computer" (p. 1025):

> Digital computing by human operators, by 'hand' and by semi-automatic 'desk' machines, also computing by the large modern fully automatic, 'self-sequenced,' computing machines. Fundamentally, however, it applies equally to those 'analogy' machines which can perform certain operations directly, that are 'transcendental' or 'implicit' from the digital point of view.

This is an important historical point, since it stresses the fact that analog computers continued to be a viable, legitimate alternative to the idea of automatic computing well into the 1950s [44], and so was the idea that calculation-intensive tasks may be performed by groups of “human computers” [45]. Additional developments were needed before digital electronic computers became established as the dominating paradigm. Hence, it was only natural that von Neumann and Goldstine would refer to all these alternatives, while pointing out that “the differences are only in degree (number of processes that rate as ‘elementary’ and ‘explicit’) but not in kind. Such differences, by the way, exist even among digital devices: one may treat square rooting as an ‘elementary,’ ‘explicit’ process, and another one not, and so on.”

The fourth type of errors (D) mentioned in the article complement those of type (C), as they refer to the fact that the machine cannot be fully rigorous and faultless at the level of the individual, “elementary operations” that they perform. Of course, the kind of “noise” and “rounding-off” problems that arise at that level in either digital or analog machines are somewhat different from each other, but still they arise in both cases.

These joint articles by von Neumann and Goldstine were highly influential and repeatedly cited, in conjunction with a no less famous article by Turing [46]. Given the many parallels between von Neumann and Turing in matters related with post-war design and implementation of electronic computers, it is quite remarkable that Turing would also devote efforts to developing methods of matrix inversions. Turing’s main concern at the time was, however, “with the theoretical limits of accuracy that may be obtained in the application of these methods, due to rounding-off errors.” Working at the National Physical Laboratory in England, Turing in 1946 had presented his plans for the ACE computer, the first model of which became operational in 1950 [36]. In November 1947 he submitted for publication his method of matrix inversion, citing among others the paper of Goldstine and von Neumann.

Neither was von Neumann oblivious to the very practical limitations that would arise in the actual building of electronic computing machines. In a letter of November 1945 to William Overbeck, an engineer at MIT, von Neumann explained the intended uses of the IAS machine for solving problems related to the theory and practice of partial differential equations, fluid dynamics, turbulence, atomic physics, and other applications, and then remarked:

> We visualize that the greatest difficulty in building such a machine is to provide it an adequate “inner” memory – it seems that in order to work efficiently on the type of problem which seems important, it is necessary that the machine should be able to store about 100,000 binary digits. This storage must be such that a digit can be stored, or sensed, or cleared, in times which never exceed a millisecond and which in certain critical situation may have to be up to 30 times shorter. Considering the size of the memory indicated (100,000 digits), ordinary vacuum tubes arrangements would lead to forbidding numbers of vacuum tubes and forbidding switching problems. However, the television and the radar industries comprise devices which will probably meet this problem adequately. [23, pp. 203–204]

An interesting outcome of von Neumann's involvement with practical concerns surrounding the limitations in the design and implementation of actual electronic computers is reflected in the way he started to develop ideas related to a new logic of automata. In traditional formal logic, von Neumann stated in a lecture of 1948, the only important consideration is "whether a certain result can be achieved in a finite number of elementary steps or not," and the number of the steps, large or small, is irrelevant. It does not even matter if the sequence of steps is so large that "it couldn't possibly be carried out in a lifetime, or in the presumptive lifetime of a universe as we know it." The focus of interest in the case of automata, he explained, is very different in this respect, as what matters here is "not only whether it can reach a certain result in a finite number of steps, but also how many such steps are needed" [47, p. 303].

On the face of it, von Neumann was formulating here the basic concern that would lead, about two decades later, to the development of complexity theory as we know it nowadays in the framework of theoretical computer science. An important difference, however, must be noticed. While in the current conception, the *theoretical* model that describes the machines considers them to be, in principle, error-free, von Neumann's concern at the time was with *actual* machines and the issues arising in programming and implementing them. Indeed, the questions he discussed in his recent joint articles with Goldstine related to *physical reliability* and error control. Moreover, as stressed by [51, pp. 519–520], von Neumann did not even formulate at the time the problem in a fashion that would later become the canonical one, namely, in terms of Turing machines as a main theoretical model. This aspect of von Neumann's perspective on complexity at the time is explicitly manifest in the way he defined the two main principles dominating the behavior of the automata, as follows:

> First, automata are constructed in order to reach certain results in certain pre-assigned durations, or at least in pre-assigned orders of magnitude of duration. Second, the componentry employed has on every individual operation a small but nevertheless non-zero probability of failing. In a sufficiently long chain of operations the cumulative effect of these individual probabilities of failure may (if unchecked) reach the order of magnitude of unity – at which point it produces, in effect, complete unreliability. [47, pp. 303–304]

Another remarkable consequence of this point of view – closely related also to the primacy he accorded to hydrodynamics as the paradigmatic model for computing-intensive methods in numerical analysis – is that von Neumann's conception of the new logic of automata placed it closer to the analytic mathematical disciplines than to those belonging to the discrete (or combinatorial) side of the spectrum, closer to classical mathematical logic, precisely that side of mathematics from which von Neumann had decided back in 1930 to distance himself. In his own words:

> There are numerous indications to makes us believe that this new system of formal logic will move closer to another discipline which has been little linked in the past with logic. This is thermodynamics, primarily in the form it has received from Boltzmann, and is that part of theoreti-

> cal physics which comes nearest in some of its aspects to manipulating and measuring information. [47, p. 304]

By 1949 von Neumann had a clear view of the stored-program architecture and, in retrospect, of its connection with Turing's idea of the universal machine. And in this regard, he became increasingly aware of the specific challenges involved in complexity of algorithms and the "crucial question . . . of the time required for solution" of specific problems addressed with specifically designed vs. general purpose machines. Once again it is useful to look at his correspondence, this time a letter of February 1949 to J.C.C. McKinsey at the Rand Corporation:

> Machines will be constructed in order to solve problems with "large" numbers of steps. The concept of "large" must, however, be interpreted somewhat specifically. In the present context it should mean the following: The number of steps in question must be impractical for human operation, but it must become manageable through the acceleration affected by a fast machine. Since this acceleration is likely to be in the order of 10^4 to 10^5 or so, the above statement can also be formulated as follows: In order to justify the use of a fast machine, the number of steps involved must be too large for human operation, but not too large by more than a factor of at most 10^5. The real "figure of merit" for the possibilities of a fast machine with respect to a certain class of problems is therefore this: How wide a family of special cases of that problem involves numbers of steps which lie within the five powers of 10 immediately above the domain of reasonable human operation? [23, p. 178]

The fourth context, then, where von Neumann met impossibility, was quite different from the first three. It involved acknowledging that technical limitations and possible errors related to specific electronic computing machines – their architecture, their programming, and their material components – need to be taken into account in any attempt to solve practical, scientific and mathematical problems. Such limitations, if not properly understood and addressed head-on, would hamper the likelihood of fulfilling the aims for which the machines were implemented in the first place. At the same time, but to a lesser degree in terms of where von Neumann's own interest lay at the time, also the limits imposed by the mathematically analyzable complexity of the algorithms should be acknowledged. However, the most interesting and peculiar aspect of von Neumann's encounters with the hurdles, difficulties, and impossibilities that arose in this fourth context was that none of them discouraged him from devoting increasing efforts, thoughts, and resources in this direction. Rather, they only enhanced the direct motivation and the actual energies devoted to further refining the architectures, improving the qualities of the material components, and developing the professional capabilities of programmers and technical personnel involved in the construction and operations of the machines.

6 Concluding remarks

Von Neumann's scientific career was astonishingly multi-faceted. His encounters with impossibility results and his interactions therewith were, to a considerable extent, also many-sided. First was the encounter with Gödel in Königsberg in 1930. His immediate grasping of the implications of the first incompleteness theorem over Hilbert's quest for proving the consistency of arithmetic by finitary means was then followed by a clear decision to refrain from any further involvement with research in logic and foundations of set theory and arithmetic, in this sense of the term. Next came his own proof of impossibility, this one related to hidden variables in quantum mechanics. This encounter was clearly different from the previous one, also in the sense that it did not mark the end of a stage in his career, but rather the starting point of new and very fruitful research directions. Common to the first two encounters, however, was the fact that they were connected to different, but related aspects of previous work by Hilbert. The third impossibility result mentioned above is also related to the work of Hilbert, via Turing's work on the *Entscheidungsproblem*. This case is more adequately described as a misencounter, as von Neumann's reaction to Turing's work was one of total indifference and lack of attention, in spite of the fact that one might have assumed a confluence of interests in this regard.

The fourth context discussed above, where von Neumann met issues related to impossibility, was quite different from the first three, as it related to the world of computing-intensive mathematical problems and the task of building actual computing machines to solve them. It involved the understanding of how technical limitations and implementation errors (both physical and related to programming) threaten the degree of reliability that can be associated with the use of machines as a significant tool for scientific research, including research in areas of pure mathematics. One of the outcomes of his preoccupation with this issue was his proposal for the elaboration of a new kind of logic of automata, meant to examine the complexity of algorithms and the difficulties expected to arise in their implementation in actual machines. The way in which theoretical computer science later became consolidated and approached these issues was not the same that von Neumann suggested at the time.

At variance with the three previous ones, the fourth encounter with inherent difficulties and perhaps impossibilities did not have any negative impact on von Neumann's subsequent decisions concerning directions of research and scientific activity. To the contrary, his energies were now increasingly devoted to refining the architectures, improving the qualities of the material components, and developing professional capabilities of programmers and technical personnel involved in the construction and operations of the machines, and he expected that all of these efforts would turn out to be fruitful not only for the sake of practical (mainly defense-related) field of interest, but also, with a very special focus, for the sake of more purely oriented scientific research.

Still it seems that starting with the Gödel episode and going on through the other ones, the cumulative effect of these encounters was to reshape von Neumann's basic views about truth and certainty in mathematics. In 1947, he retrospectively recalled how learning about Gödel's result in 1930 (and by implication, about the inherent presence of impossibility and incompleteness in mathematics) had taught him to be cautious "against taking the immovable rigor of mathematics too much for granted. This happened in our own lifetime, and I know myself how humiliatingly easily my own views regarding the absolute mathematical truth changed during this episode" [48, p. 187].

To conclude, I want to cite here a letter of Gödel to von Neumann, written in February 1957, one year before von Neumann's death (and that came to public light only in 1989). In the letter, Gödel posed the following question:

> One can obviously easily construct a Turing machine, which for every formula F in first order predicate logic and every natural number n, allows one to decide if there is a proof of F of length n (length = number of symbols). Let $\Psi(F, n)$ be the number of steps the machine requires for this and let $\varphi(n) = \max_F \Psi(F,n)$. The question is how fast $\varphi(n)$ grows for an optimal machine. One can show that $\varphi(n) \geq k{\cdot}n$. If there really were a machine with $\varphi(n) \sim k{\cdot}n$ (or even $\sim k{\cdot}n^2$), this would have consequences of the greatest importance. Namely, it would obviously mean that in spite of the undecidability of the *Entscheidungsproblem*, the mental work of a mathematician concerning Yes-or-No questions could be completely replaced by a machine. After all, one would simply have to choose the natural number n so large that when the machine does not deliver a result, it makes no sense to think more about the problem. Now it seems to me, however, to be completely within the realm of possibility that $\varphi(n)$ grows that slowly. Since it seems that $\varphi(n) \geq k{\cdot}n$ is the only estimation which one can obtain by a generalization of the proof of the undecidability of the *Entscheidungsproblem* and after all $\varphi(n) \sim k{\cdot}n$ (or $\sim k{\cdot}n^2$) only means that the number of steps as opposed to trial and error can be reduced from N to $\log N$ (or $(\log N)^2$). However, such strong reductions appear in other finite problems, for example in the computation of the quadratic residue symbol using repeated application of the law of reciprocity. It would be interesting to know, for instance, the situation concerning the determination of primality of a number and how strongly in general the number of steps in finite combinatorial problems can be reduced with respect to simple exhaustive search. (Cited in [49, p. 478].)

This letter since its publication attracted much attention on the side of the theoretical computer science community, as the question posed here may be retrospectively seen as inquiring about the computational complexity of an NP complete problem [50, 51]. But while Gödel and von Neumann, working separately, anticipated ideas related to computational complexity, the time was not yet ripe for a consolidated development of a theory of complexity as known since the early 1970s. More than 20 years after their first encounter in 1930 in Königsberg, Gödel and von Neumann were now colleagues at the IAS in Princeton. They had great admiration for each other, which continued to grow throughout the years; yet it nonetheless seems rather unlikely that they were in some kind of ongoing dialogue (scientific or personal) in general, and on the issue raised by Gödel in the letter in particular. Gödel may have had good reasons to believe that this kind of question would attract von Neumann's attention, but there is no evidence of any reply on his side (as there is no evidence that Turing's ideas on computability had attracted his interest back in 1937, while they were both in Princeton). During and after the war, von Neumann was just too busy in his consulting assignments for government and industry. Beginning in the early 1950s, von Neumann spent more time at Princeton, but his energies and personal interactions were devoted mostly to game theory and the relevant community, which included Oskar Morgenstern and John Nash, among others [2, pp. 345–340]. And in any case, by the time of Gödel's letter, von Neumann was already seriously ill. So, the letter did not actually signal the coming of a 30-year-old full circle, which was opened at the time of their first, rather dramatic encounter.

In the same letter Gödel also referred to von Neumann's health situation. He wrote:

> With the greatest sorrow I have learned of your illness. The news came to me as quite unexpected. Morgenstern already last summer told me of a bout of weakness you once had, but at that time he thought that this was not of any greater significance. As I hear, in the last months you have undergone a radical treatment and I am happy that this treatment was successful as desired, and that you are now doing better. I hope and wish for you that your condition will soon improve even more and that the newest medical discoveries, if possible, will lead to a complete recovery.

But that was not to be. Unfortunately, recovering from his illness became for von Neumann the last real impossibility situation he faced in his life.

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