# WEYL'S ATTEMPTED MEDIATION BETWEEN INTUITIONISM AND FORMALISM

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Hermann Weyl was a universal mathematician continuing the tradition from Poincaré and Hilbert, but in the next generation. He wrote his thesis with Hilbert in 1908 and succeeded Hilbert on the chair in Göttingen in 1930, although he could only remain until 1933 for political reasons. Like Poincaré, he cared to write for the general public, so first-class scientific reading with a minimum of technicalities, and hence, in some way at least, accessible to a more general public.

Weyl's work is divided into the part consisting of the scientific articles and the other part consisting of the books, and among the books are some in mathematics, like *The Concept of a Riemann Surface* and the book on symmetry, for instance; at least one in mathematical physics, *Group Theory and Quantum Mechanics*; and then in our area, by which I mean logic, foundations and philosophy of mathematics, in the first place *The Philosophy of Mathematics and Natural Science*, which was published in German in 1927 and then translated into English in 1949 with a considerable number of appendices; and then some lecture series of his were edited as little booklets: *The Open World*, from 1932, for instance, and *Mind and Nature*, from 1934. This is perhaps not an exhaustive list, but it gives an idea of what he wrote that is accessible to the more general public.

Out of his scientific articles, less than ten percent, between five and ten percent, are in our area of logic, foundations and philosophy of mathematics, and I have concentrated on this part entirely. Among these papers—there are around ten, or a dozen, of them—one stands out in particular as the most important one. That is shown also by the fact in the *Selecta Hermann Weyl* that were edited for his 70th birthday in 1955, there is one paper in our area, and that is precisely the 1921 paper on the novel foundational crisis in mathematics, "Über die neue Grundlagenkrise der Mathematik". It has some novelties in it that remain of great value, in the first place the constructive explanation that he attempts of the universal and existential quantifiers, introducing the notion of judgement abstract, Urteilsabstrakt. Weyl's attempted interpretation does not work, we would say nowadays, since it differs from the Brouwer–Heyting–Kolmogorov interpretation. On the other hand, Weyl's explanations of the universal and existential judgements have found their way into type theory as explanations of the meaning of universal judgements and existential judgements, rather than propositions. So that is a novelty of Weyl's in this paper. But the reason why the paper is so important is that it put Hilbert on fire. Weyl has the merit, if one wishes to call it a merit, of having precipitated the foundational debate that stretched from this paper of his in 1921 up until 1928, a fight that concerned such fundamental matters that it has been going on during the rest of the 20th century, and only now, relatively recently, does it seem reasonable to say that we have a sufficiently good understanding of the problem that was there a hundred years ago now.

This meeting has the title *Das Kontinuum – 100 Years Later*, but I found out that I did not have anything sufficiently interesting to say about *Das Kontinuum*, and instead I will concentrate eventually on another contribution of Weyl's, namely his notion of symbolic construction. Before we get to that point, however, let me say that a year after Weyl had finished *Das Kontinuum*, he met Brouwer during the summer vacation in Engadine in Switzerland and, as a result of that, converted to intuitionism. He said quite explicitly in the 1921 paper that from this point on he follows Brouwer in pure mathematics. In particular, he was impressed by Brouwer's conception of the continuum, what Errett Bishop much later called a revolutionary, semi-mystical theory of the continuum—I do not agree with the semi-mystical part, but certainly, it is a conception of the continuum which is quite radically different from the standard definition of a topological space.

So he says in 1921 that he gives up his own attempt on the continuum from 1918 and is now in favour of Brouwer's notion, based on Brouwer's notions of spread and choice sequence. In view of this, celebrating 100 years this year is doubly appropriate, that is, not only because of Weyl's *Das Kontinuum*, but also because of Brouwer's conception of the continuum, which was expanded on in print in a long article, the first of Brouwer's Begründung papers, from 1918. (The second one is from 1919.) We are therefore also celebrating Brouwer's notion of spread and choice sequence with 100 years.

As I already hinted at, the most important effect of the paper was that it set Hilbert on fire and caused him to conceive of a way of defending classical mathematics, which he was so attached to, in particular because of his own non-constructive existence proofs in the 1890s. We all know what the strategy was: to formalize classical mathematics and prove consistency of the formal system, so that even if the formal system does not have a clear intuitive interpretation, we can still work with it consistently. This consistency problem turned out to be radically more difficult than Hilbert thought: the consistency proof for classical analysis, say, or classical mathematics as a whole, is still outstanding today, and we know a certain amount of why that is so.

The Grundlagenstreit extended, as I said, until 1928, and the choice of that year is relatively clear: it was in 1928 that Hilbert gave his second Hamburg lecture. He gave two Hamburg lectures in the 1920s. The first one was the immediate response to Weyl's 1921 paper, and that was given in the summer of '21, published in '22, and that is where proof theory is conceived. The second one, which was given in '27 and published in '28, puts an end to this bitter exchange between Brouwer and Hilbert. It was followed up in 1928 by the firing of Brouwer from the editorial board of the *Mathematische Annalen* and Brouwer's withdrawal, essentially, for a long time. This, therefore, is the natural year to count as the end of the Grundlagenstreit.

It is clear from autobiographical material of Weyl's published in the 1950s that Hilbert and Brouwer were the two mathematicians who had made the strongest impression on him—were his heroes so to speak—so he must have been affected more than anyone else by this bitter debate, and that is what will become the topic of my talk here.

Weyl's response, three years after his 1921 paper, so in 1924, was to propose a mediating position, which he stuck to for the rest of his life, essentially just repeated and repeated in many, many places, sometimes almost verbatim, and other times really verbatim—one such passage I will quote in a moment. To change one's mind on such a fundamental matter is a big thing—to do it once is dangerous, and to do it more than once is more than what usually gets accepted—and his solution was stubbornly to stick to this proposal from 1924. The first place where this new proposal appears is a bit difficult to find, because it is not visible from the title of the paper, "Randbemerkungen zu Hauptproblemen der Mathematik", marginal remarks on the main problems of mathematics. From the title this sounds like a continuation of Hilbert's problem paper, which almost all of it in fact is, but on the last couple of pages, Weyl sketches his new position regarding the foundational problems. Then there is a paper from 1925 which follows very closely the '24 paper, and that paper is called "Die heutige Erkenntnislage in der Mathematik", so the present knowledge situation in mathematics. There is nothing substantially new in this. It is essentially a repetition of what was there already in 1924.

I will not quote from the 1924 paper, because it would be a bit too long, but there is a passage from 1931—I think I will refer to papers by their year, so 1931 is "Die Stufen des Unendlichen", the levels of infinity—and it is repeated in 1932 in *The Open World*, and again in 1949, a particularly important place for the notion of symbolic construction, and it is also repeated verbatim in 1953, which is a very late paper, because he died in '55. The 1931 passage reads in English translation—many of his papers in our area have now been translated in two volumes by Peter Pesic, one is called *The Levels of Infinity* and the other is called *Mind and Nature*, and that is the name of his 1934 lectures that I mentioned before—so the quotation in English reads as follows:

> If one takes mathematics by itself,

by which he clearly means pure mathematics,

> one should restrict oneself with Brouwer to the truths of insight in which the infinite enters only as an open field of possibilities;

so he endorses the potential infinite here,

> but there is no motive discoverable that presses farther than that. But in natural science, we touch a sphere which is anyhow impenetrable to intuitive evidence. Here knowledge necessarily becomes symbolic shaping,

symbolische Gestaltung, and this Gestaltung is a bit difficult in English—it is not perfect to translate it with shaping, but most of you probably know German well enough to just take symbolische Gestaltung,

> which is why, when mathematics is taken along by physics in the process of theoretical world-construction, it is no longer necessary that the mathematical let itself be isolated as a particular region of the intuitively certain. From this higher standpoint, which makes

> the whole of science appear as a unity, I consider Hilbert to be right.

This is quite a sensational attempt at reconciling Hilbert and Brouwer. I suppose you are struck by the same thing as I was in the first place: he is going to disrespect the distinction between logical laws and physical laws, or natural laws more generally, because he wants to save excluded middle by treating it along with the physical laws, as if it were a law of physics. If there is a distinction—which, I take it, is the most common view, that there is a fundamental difference—between the logical laws and the physical laws, then there is no doubt that the excluded middle belongs to the logical laws, and to the physical laws we have—well, in classical mechanics we have Newton's laws, and in electromagnetism we have other laws, also equationally formulated, and similarly in relativity theory and quantum theory. These are examples of physical laws, and to group the law of the excluded middle together with them is a very radical proposal, no doubt.

In the beginning of the quotation he distinguished the pure mathematical laws from the physical laws by saying that the physical laws are impenetrable to reason—in his expression—*anyhow*. Since they are anyhow impenetrable to reason, why not have one more thing, namely excluded middle, which is also impenetrable to reason. It is quite comprehensible why he says impenetrable to reason concerning the physical laws as opposed to the pure mathematical laws, which are, in his view, penetrable to reason—provided you limit yourself in Brouwer's way to what is intuitively evident. That is understandable enough. If you think of a pure mathematical law, which is penetrable to reason, like the rule of mathematical induction, then we all know what thought steps we go through in trying to convince ourselves of its validity. It is, however, clear that if you take physical laws, like the laws of electromagnetism, Maxwell's laws for instance, there is no chance that you can sit down, think about what meaning the constants appearing in such a law have, and come to the conclusion that, yes, they must satisfy these partial differential equations that make up Maxwell's laws, and similarly for the other examples of physical laws that I gave.

This is clearly what Weyl refers to by saying that physical laws are impenetrable to reason. That does, however, not mean that they are totally impenetrable: you must have quite a limited notion of reason if you want to say that they are impenetrable to reason. It would be better to say that we can convince ourselves of them, or give support for them, but in a different way from the pure mathematical-logical laws. What lends support to physical laws? I take it to be the received view that they give predictions that are confirmed by our experiments. If no experiment whatever has been performed, then there are no restrictions that we can put on the laws, because the laws do not have to fit anything, but as soon as we have experimental results, which we have to be able to reproduce by calculation in our model, or in our theory, then we have limitations. Of course, when these physical laws have been discovered, it has been partially as a result of experiments—not only, but at least experiments have played a role in the justification of these laws.

Summing up this part: Weyl's proposal was to justify classical mathematics as based on the law of the excluded middle by amalgamating it with theoretical physics, treating the law of excluded middle on a par with the physical laws, and thereby ignoring the fundamental distinction between logical laws and physical laws.

There is a notion that he introduced already in the 1924 paper that is repeated in all the following writings of his in our area, and that is the notion of symbolic construction. I personally have known for quite some time that there was a notion called symbolic construction introduced by Weyl, but I have simply taken for granted that it was the same as Brouwer's notion of mental construction, with the change from mental to symbolic to make sure that these constructions are not primarily thought constructions, in which case mental construction is a natural term, but they are linguistic constructions, constructions in symbols. From Weyl's own papers on this, however, it is clear that that is a much too simple-minded interpretation of the term: he puts much more weight on it than that. It becomes in a way his key concept, his key contribution in this area.

First, concerning the choice of term: construction was, of course, already there from Euclid, and mental construction was there in Brouwer, but symbolic here is a novelty. I tend to believe that it was inspired by the appearance of Cassirer's *Philosophie der symbolischen Formen*, the philosophy of symbolic forms, in three volumes, published in 1923, '25, and '29. Weyl got inspired by this and saw that what goes on in logic, mathematics and physics shows the same kind of pattern that Cassirer was interested in as a more general cultural framework. I have no direct evidence for this, but the year is there—'23 and '24—and, moreover, Weyl refers very favourably to Cassirer, and Cassirer's general philosophical outlook was probably quite congenial to Weyl. That is my guess as to why symbolic appears here.

If someone introduces a new notion, a new concept like this in an area that is so fundamental as the one we are engaged with now, the natural immediate reaction is: is a new term needed here, do we not have enough terms to talk about theories and the difference between purely mathematical theories and physical or applied theories? How would one try to express what Weyl wants to express using familiar terms? It seems to me that the standard way of speaking about these matters is to use the notion of a mathematical model. That notion appears in the end of the 19th century, and it is possible, I think, to understand why. If you go back further in time to, say, the 18th century or earlier, then there was no clear distinction between pure and applied mathematics. In classical mechanics one thought of the equations that one writes down as directly referring to the moving bodies and their trajectories and so on. When you got the distinction between pure and applied mathematics firmly in place, during the 19th century, then as a side product you have got the possibility of taking a piece of applied mathematics and forgetting about everything that has to do with the application and look at what remains as a piece of pure mathematics. What remains as a piece of pure mathematics, that is what we nowadays call the mathematical model.

The notion of model has also the other sense, which has a different origin, as far as I can judge, namely the model of a theory, the model-theoretic notion. We also know where that comes from, namely non-Euclidean geometry. You had new geometries, and they had their models, the Klein model, the Poincaré model, and so on, so that is the first place where we have different models of one and the same theory. Although the notion of model in these two cases has a different origin, I think that the model-theoretic notion has a lot to contribute to the discussion that I am engaged in. If we look at examples of mathematical models, what do we think of? Well, in physics, we think of the model for the falling body in a gravitational field or the model of the swinging pendulum, and in probability theory we think of probabilistic models like the Brownian motion, where if we by Brownian motion mean the actual erratic movement of the pollen particles that are suspended in the liquid, then the mathematical model is no longer, of course, the pollen particles: the mathematical model is the mathematical capturing, or the mathematical description, of this, and in this particular case it is common to call it the Wiener process. In this case, therefore, we have both a term for the mathematical model and for the phenomenon that the mathematical model is supposed to describe.

The first examples I gave here were examples of models from classical mechanics, so classical mechanical models, and later I gave this example of a probabilistic model, so that is a model of probability theory rather than of classical mechanics. We thus have examples of theories, namely classical mechanics—it could be optics, it could the theory of electromagnetism, etc.—and models of that. This distinction is the same as the distinction that Bourbaki tried to capture by using the notion of structure and structure of a kind, structure d'une espèce in French: structure instead of model and kind of structure instead of theory.

Once we have the distinction in place between model, in particular, mathematical model, and a theory that it is a model of, the question is with symbolic construction now. What is it? Is it the mathematical models which are symbolic constructions, or is it rather the theories which are symbolic constructions in Weyl's sense? If one reads Weyl carefully it becomes clear that it is the theory that he has in mind with his notion of symbolic construction. In these examples, it is thus classical mechanics considered as a theory which is a symbolic construction—symbolic construction corresponding to the movement of bodies under forces—and similarly in the other examples that I gave: it is the theory of probability which is the symbolic construction, and the various models are models of that theory.

This can be supported, on the one hand, by the heaviest paper where he discusses the notion of symbolic construction, the 1949 paper, "Wissenschaft als symbolische Konstruktion des Menschen", science as symbolic construction by man. If you just consider that very title, science as the symbolic construction of man: well, all of science is not a model, science is a theory, not only one theory, science consists of many theories, but all those theories are symbolic constructions in Weyl's sense. I have two more quotations to support what I say about symbolic construction corresponding to theory rather than mathematical model. One is from a very interesting paper, almost the last one he wrote, called "The unity of knowledge", from 1954. He says,

> Following Galileo, one may describe the method of science in general terms as a combination of passive observation refined by active experiment with that symbolic construction to which theories ultimately reduce.

So it is theories that become symbolic constructions. Another quotation from the following page of the same paper:

> It is to be admitted that on the way to their goal of symbolic construction scientific theories pass preliminary stages...

Theories become more and more generalized and become the ultimate symbolic construction.

With this I have exhausted what I want to say about symbolic construction at the moment, but let me take up one point which occupied Weyl noticeably, and that is that he had basically a Kantian background. He interpreted the notion of immanence and transcendence in a Kantian way, so immanent means within the limits of possible experience, and what is transcendent is what goes beyond possible experience. Of course, in Kant, what is beyond possible experience is precisely what we cannot know anything about. Roughly, that is what Kant uses the an sich for. Weyl has a beautiful idea here: that the symbolic construction is the representation of what is transcendent. What is within experience are the results of our experiments, the data that come in. But we also have what we speak of as lying underneath or behind the phenomena and that gives rise to the phenomena, and that is what Kant used the term transcendent for. This is what Weyl proposes to identify with the theory, the theory which is the symbolic construction. It is a considerably more Platonic view than the Kantian view. The way Weyl expresses himself about this is in terms like: das Transzendente darzustellen, or symbolische Darstellung des Transzendenten, symbolic presentation of the transcendent.

This seems to me to be a substantial contribution to philosophy, actually, that he gives. He says quite explicitly in one place that "I spent a lot of time and energy on physical and philosophical speculations, but I do not regret it." This quotation shows that he did not consider himself professional either in theoretical physics or in philosophy, but that is where his heart was, so to say. Here we have an example where, to my mind, he does contribute something also from a professional philosophical point of view.

I said early on in this talk that no-one was probably more affected than Weyl by the Hilbert–Brouwer controversy, since it was a fight between the two that he considered most highly amongst mathematicians he knew. It is natural because of the delicacy of the matter that not much is published on this—it would be interesting to know if there is correspondence or something like that in the archives, but I have no idea—but in the published writings there are two places that have struck me. One is in the 1925 paper, "Die heutige Erkenntinslage in der Mathematik", where he says

> standing myself in the middle of the fight between the parties, I have tried *sine ira et studio* to depict the present situation.

This is typical of Weyl's style: "sine ira et studio"—of course, one has to look up what this is. It is a quotation from Tacitus and means without anger or bias, which is to say, impartially, so he says that in this paper he has tried impartially to describe the different positions. Then, in 1946, in a paper called "Mathematics and Logic", and which was meant as an introduction to the Schilpp volume on Bertrand Russell, and now it is fortunately in English, he says,

> Like everybody and everything in the world today, we have our crises.

(This is just after the end of the War.)

> We have had it for nearly 50 years. Outwardly it does not seem to hamper our daily work, and yet I for one confess that it has had a considerable practical influence on my mathematical life. It directed my interest to fields that I considered relatively safe, and has been a constant drain on the enthusiasm and determination with which I pursued my research work.

It shows very clearly how affected he has been by this foundational fight, and how unsolved it was for him, still, in 1946.