The significance of Hermann Weyl's *Das Kontinuum*

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Abstract

In his 1918 monograph "Das Kontinuum", Hermann Weyl initiated a program for the arithmetical foundations of mathematics. In the years following, this was overshadowed by the foundational schemes of Hilbert's finitary consistency program and Brouwer's intuitionistic redevelopment of mathematics. In fact, not long after his own venture, Weyl became a convert to Brouwerian intuitionism and criticized his old teacher's program. Over the years, though, he became more and more pessimistic about the practical possibilities of reworking mathematics along intuitionistic lines, and pointed to the value of his own early foundational efforts. Weyl's work in *Das Kontinuum* has come to be recognized for its importance as the opening chapter in the actual development of predicative mathematics, whose extent has been plumbed both mathematically and logically since the 1960s.

The main reference of Hermann Weyl that I am going to be talking about is his 1918 monograph *Das Kontinuum*. Other things that I might be referring to include a rather late volume, *Philosophy of Mathematics and the Natural Sciences*, (1949) (which is an expansion of an encyclopedia article that he wrote in the late 1920s), his four volumes of collected works, and various articles. Weyl is a very interesting and many-sided person, and he has been studied from many different perspectives. So, in particular, about *Das Kontinuum*, I have written an article "Weyl vindicated" (1988) which

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has now been reprinted in my volume of essays, *In the Light of Logic* (1998). I should also mention Dirk van Dalen's article on Weyl's intuitionistic mathematics (1995). (All these items are included in the list of References below.)

Hermann Weyl lived from 1885 to 1955. He obtained his doctor's degree in Göttingen in 1908, working with Hilbert. After a few more years in Göttingen, Weyl moved to a position in Zürich and remained there for a number of years. Hilbert kept trying to get Weyl to come back to Göttingen as his successor. Weyl finally returned in 1930 but then, when the Nazis came in and the Mathematical Institute started to crumble, he left it and went to the Institute for Advanced Study in Princeton. Weyl stayed there for the rest of his academic life; he retired in 1951, and then traveled back and forth between Zürich and Princeton until the year of his death. In his mathematical work, Weyl was just about as wide and as original as Hilbert in all areas of mathematics: number theory, algebra, geometry, analysis, mathematical physics, logic, philosophy of mathematics, as well as the philosophy of science. Weyl also was a very cultured person; he had a strong interest and background in art, literature and philosophy. I think he is also of interest as a literary stylist. His writing can be ornate at times; I do not know if that is the right word, as I do not read the German language that well. But it seems to me that he is more of a stylist than Hilbert, for example.

Weyl's own contributions to logic and the foundation of mathematics are not that many in number. At one point I counted in his collected works a total of 160 items on all subjects; of those only about a dozen concerned logic and the foundations of mathematics. And, really, of technical contributions I count only two or possibly three: an early paper "Über die Definitionen der mathematischen Grundbegriffen" (1910), then the 1918 monograph, whose full title is *Das Kontinuum. Kritische Untersuchungen über die Grundlagen Der Analysis*, and finally the break to intuitionism in the paper from 1921, "Über die neue Grundlagenkreise der Mathematik". Subsequent papers mostly dealt with his views of foundational developments in one way or another. It is not that he forgot any of these early things, by no means. But as technical contributions these three are the ones to concentrate on.

The 1910 paper I think is sadly overlooked, because what he does there is provide an explanation of—what we call in modern terms—the notion of definability over any relational structure, and which anticipates Tarski's famous contribution in that respect. Tarski's own paper on definability in the real numbers did not come until 1931, and his full explication of satisfaction, truth and thence of definability was not published until a few years later. So we credit Tarski for the full spelling out of that, but if you look at Weyl's paper it is quite clearly right there. One of the things he was after had to do with a question in Zermelo's axiomatization of set theory concerning the Axiom of Separation. As Zermelo formulated it: if you take any *definite property* $P(x)$ of elements x of a set a, then there is the set of all x's in a which satisfy the property $P(x)$. There were various questions about Zermelo's axioms: what did they mean, why should one accept them, and so on. But one question in particular from a logical point of view concerned the separation axiom. The idea of a definite property was itself indefinite, and one should really explain just what one had in mind. As a solution to this, it was natural to propose: as a definite property we simply mean all those properties which are definable within the language of set theory. And this was in fact Weyl's proposal in 1910. He said, well, we now have a notion of definability over any structure consisting of a domain and some relations. If we apply this in particular to the domain of sets with the membership relation (and, optionally, the equality relation), then we can generate all definable relations in that sense. Then just those properties are the ones that are to be counted definite and are to be used in the Separation Axiom. I don't know why Weyl does not get proper credit for this. People usually claim that Skolem or Fraenkel did this in the 1920s in order to explain just how to make the Separation Axiom precise. But there it is already in Weyl's 1910 paper[1].

Now, when we talk about Weyl's philosophy of mathematics, we are faced with shifting positions. We all change our minds, or most of us do, about things over periods of time and Weyl was certainly no exception. It is hard to identify him with a clear-cut philosophy of mathematics or foundational program in the same sense as we identify Brouwer with constructivism and Hilbert with his finitist consistency program. I think part of the reason that Weyl's program in 1918 was overshadowed by Brouwer's and Hilbert's programs was that there is not this clear identification, nor did he plug it in the way that Hilbert and Brouwer plugged their programs. But, as we shall see, he never really gave up the achievements of his 1918 monograph.

I am not quite sure whether his view of the foundations of mathematics was set-theoretical in 1910, but you might read the fact that his work then contributed to the foundations of set theory as in some sense an acceptance

[1] See the introductory note to Fraenkel's 1922 article on the notion of "definite" in van Heijenoort (1967) p. 285.

of it. But at some time, at least by 1917, he became critical of set theoretical foundations. In the preface to *Das Kontinuum* (I will present to you an English translation, which is not too precise) he says:

> It is not the purpose of this work to cover the firm rock on which the house of [mathematical] analysis is founded with a rigid wooden skeleton [hölzernen Schaugerüst] of formalism and then persuade the reader and finally oneself that this is the real foundation.

With respect to "formalism", it is not clear whether Weyl is directing this specifically at Hilbert or instead an axiomatic presentation of analysis. At any rate (he continues), it is not his purpose to cover the house of analysis with this kind of formalistic structure, a structure which can fool the reader and ultimately the author into believing that it is a true foundation. Rather

> I shall show that this house is to a large degree built on sand. I believe that I can replace this shifting foundation with pillars of enduring strength. They will not, however, support everything which today is generally considered to be securely grounded. I give up the rest since I see no other possibility.

Now it may be argued whether Weyl did indeed show that it was a house built on sand. But there are some fundamental criticisms he made of basic assumptions that are implicitly involved in analysis. That led him to reject those assumptions, in a way that I shall explain at length.

But first let me give a quick survey of Weyl's shifting views on the foundations of mathematics.

- 1910 Contribution to Zermelo's set theory
- 1917 Critical of set theoretical foundations
- 1918 Definitionism (à la Poincaré)
- 1920 Joins Brouwer's intuitionistic program and criticizes Hilbert's program
- 193? Gradual disillusionment with intuitionism
- 193? Reaffirmation of value of 1918 contribution

- 1953 Torn between "constructivity" and "axiomatics"

The 1910 work and its contribution to set theory has already been mentioned. But from 1917 on, Weyl was critical of set theoretical foundations, and he seems to have remained critical in that respect for the rest of his life. On the other hand, in his own mathematical practice he seemed to accept set theory to a certain extent, and it is not clear how he resolved the two. It is not uncommon, of course, that people will say one thing during the week and something else on Sundays; perhaps this is another aspect of the same thing.

In 1918 Weyl published his monograph *Das Kontinuum* in which he produced his own approach, and which represents a form of definitionism, according to which all mathematical objects that one deals with have to be defined; but there is an irreducible undefinable minimum and that irreducible minimum is simply the structure of the natural numbers. So it is a notion of definitionism, modulo (or given) the natural numbers; in that he followed Poincaré.

In 1920 Weyl made his break with his own approach and shifted radically to Brouwer's intuitionistic program, and, also at the same time, criticized Hilbert and his program (cf. Weyl (1921) and van Dalen (1995)). As you can imagine, that upset Hilbert quite a bit in his quarrels with Brouwer, since Weyl was his former student, and I guess Hilbert had thought of him as an ally. Weyl's criticism of Hilbert in this respect also lasted apparently all through his life.

At a certain time in or by the 1930s (and that is why I put 193? in the dates above) Weyl became disillusioned with the Brouwerian development; let me read you a passage from the *Philosophy of Mathematics and Natural Science* (1949), where he says:

> Mathematics with Brouwer gains its highest intuitive clarity. He succeeds in developing the beginnings of analysis in a natural manner, all the time preserving the contact with intuition much more closely than had been done before. It cannot be denied, however, that in advancing to higher and more general theories the inapplicability of the simple laws of classical logic eventually results in an almost unbearable awkwardness. And the mathematician watches with pain the larger part of his towering edifice which he believed to be built of concrete blocks dissolve into mist before his eyes. (Weyl (1949), p. 54)

Concerning the second shift in the 1930s indicated above, if you read Weyl (1949), you see various passages—and I quote them in my 1988 paper—in which, in effect, he says: "Well I really did do something important in 1918, something that was valuable and something that still has permanent value"—he is being modest about it but reaffirms his earlier accomplishment.

Finally, Weyl gave a lecture in 1953 that was published posthumously in 1985 in the *Mathematical Intelligencer*, in which he talks about being torn between constructivity and axiomatics. He seems there to take constructivity in a rather general sense as a kind of genetic creation of mathematical objects. By axiomatics he meant mathematics developed on systematically organized axiomatic grounds such as in group theory, Hilbert space theory, topology, and so on; he did not necessarily mean axiomatics in the sense of mathematical logic.

Let us now return to Weyl (1918) and to its relations with the ideas of Poincaré and of Russell, who took up the criticisms of Poincaré. People were very much concerned about the paradoxes which emerged towards the end of the 19th century and the beginning of the 20th century. The naive idea was that given any property $\phi(x)$ there is associated with it its extension: the set or the class of all x's which satisfy ϕ, $\{x|\phi(x)\}$. As we know, a simple application of this leads to Russell's paradox: if you take the set r of all x's such that x is not an element of x, i.e. $\{x \mid x \notin x\}$, then r is both an element of itself and not an element of itself, which is a contradiction.

So, in 1905 Russell asked the question: which predicates determine extensions? He introduced the word *predicative* for a property which can be predicated in such a way that it has an extension. From that we derive the word *impredicative*, or *non-predicative*, for predicates which look as if they express reasonable properties, but of which we cannot say that they have an extension. Then the question is: how can we characterize the predicative properties; alternatively, which are the impredicative properties?

Poincaré had an answer that came from his analysis of the paradoxes. As a paradigm, Poincaré referred mainly to the paradox found by Jules Richard, which was a kind of paradox about definability in natural language. It concerns the set of definable real numbers: assumedly, you can enumerate all definitions of definable real numbers, but then by diagonalization you can define a number which is not in that enumeration, so you have a contradiction. Poincaré saw in that paradox, and other paradoxical arguments, a *vicious circle*—as he put it: in such paradoxes you define an object which essentially assumes the existence of a totality which contains that object as a member.

But, according to Poincaré, in order to define an object by reference to a totality, that totality must *precede* the definition. If not, the definition is unsound, there is a vicious circle, and the definition is impredicative.

Now, we have many real-life definitions which are impredicative. For example, if we talk about the tallest person in this room, then we are singling an object out of the totality of objects in this room, by reference to that totality. But the question is: is that the only way in which to determine that individual? No, in that case we have other ways, and we can identify such a person in many different ways. Similarly in number theory. For example there are the famous theorems that every natural number is the sum of at most four squares, and every natural number is a sum of at most nine cubes. And, more generally, by Hilbert's solution to the Waring problem, there is for each k a number m, such that every natural number is the sum of at most m k'th powers. What is the least number m for which that is possible? On the face of it, that is an impredicative definition, since it picks out that number m by reference to the totality of natural numbers. In the case of $k = 2$ it is 4, and in the case of $k = 3$, m is 9. We don't know the value of m for $k > 3$, but we believe that it has another description, that the number exists prior to having been picked out in this way. So this is not an essential impredicativity. But in set theory we are dealing with much more general notions, and if you have the idea that sets are introduced by definitions, and by definitions only, then you have to look for what is the source of impredicativity in forming an extension, that is, the class of x satisfying $\phi(x)$, $\{x|\phi(x)\}$, or in Russell's symbolism $\widehat{x}\phi(x)$.

Russell agreed with Poincaré that the source of the logical and mathematical paradoxes lay in the appearance of a vicious circle: in each case there is a presumed totality out of which one member is singled out by reference to that totality in a way that depends essentially on the presumption of that totality. For example, Russell's paradox presumes that the totality of all classes x exists, in order to single out the class of all those classes x for which $x \notin x$. As we saw, Poincaré argued that such apparent definitions are improper: an object is to be defined or determined only in terms of prior objects, notions and totalities; only those are predicative. Russell turned Poincaré's proscription of impredicative definitions into his *Vicious Circle Principle* (VCP):

> No totality can contain members defined [only] in terms of itself.

There have been considerable discussions about just what Russell's VCP says and there have been various criticisms of it. Just to mention two references, Gödel had a very interesting article (1944) on Russell's mathematical logic that contained an extensive critique; another useful source is the book by Charles Chihara, *Ontology and the Vicious Circle Principle* (1973). I do not want to get into any elaboration of the VCP or try to justify it but you should just have the understanding that if you are going to pursue a definitionist philosophy of mathematics, in the sense that you believe that all objects are supposed to be—in some sense—defined eventually in terms of certain basic ones, and that among the objects you want to deal with are classes, then you are going to confine yourself to definitions where everything that occurs in the definition is prior to what is actually being introduced by that definition.

Russell's main struggle was to turn the VCP into a logically useful criterion for which definitions of classes $\widehat{x}\phi(x)$ (in present-day symbolism, $\{x|\phi(x)\}$) are to be admitted as predicative. He identified the source of possible violations of the VCP in the unrestricted use of bound variable in $\widehat{x}\phi(x)$; in Russell's terminology, these were called *apparent variables*. Thus, one reformulation he took of the VCP was:

> Whatever contains an apparent variable must not be a possible value of that variable.

Such apparent variables have two kinds of occurrences in expressions of the form $\widehat{x}\phi(x)$: first in the variable 'x' itself, and second within any quantifiers '$\forall y$' or '$\exists y$' that occur in ϕ. Russell used this observation to employ the VCP in a positive way in an axiomatic system for predicatively defined classes, in what is called the *Ramified Theory of Types* (RTT). That is the basic system of the magnum opus of Whitehead and Russell, *Principia Mathematica*, published in three big volumes between 1910 and 1913.

Two kinds of restrictions were made there in order to meet the second form of the VCP enunciated above. One is a division into types in the ordinary sense of the word, and the other is into orders or levels of definition. The basic picture is that one starts with a collection of individuals at type 0, then we have a collection of classes of individuals, which would be type 1, then classes of classes of type 1—that would be type 2, and so on. A natural restriction is that you can only ask whether an object of type n belongs to an object of type $n+1$. Further, we have to talk of *levels of definitions*: we assign a level to an abstract $\widehat{x}\phi(x)$ in such a way that its level is greater than the levels of all bound variables that occur in ϕ, including 'x' itself (without this second distinction as to levels of definition, the system obtained would just be the *Simple Theory of Types.*) Within the ramified theory, the type/level classification can be combined to a single ordering of variables of different sorts.

What Russell aimed to accomplish in *Principia Mathematica* was an execution of the logicist program, more or less along the lines that Frege had hoped to achieve, reducing all of mathematics to logic[2]. However, Russell had to modify the logicist program in order to go along with the Vicious Circle Principle and thus to set up his formalism in terms of Ramified Type Theory. Now, what did the logicist program come to in the case of the natural numbers? The notion of equinumerosity is the basis of the concept of natural number: two classes are equinumerous if they are in one-to-one correspondence, and cardinal numbers are simply the equivalence classes under this equivalence relation of equinumerosity. And, finally the natural numbers are just particular kinds of cardinal numbers, namely, the finite ones. So, in that way the notion of number comes out of a general theory of classes in a kind of Fregean approach.

Classes were to be conceived of as logical objects, as extensions of predicates. To the extent that Russell's explanation of mathematical notions and results in RTT would succeed, that would constitute a reduction of mathematics to logic. But now we do not have classes without restriction, only classes of type 0, of type 1, of type 2, and so on. Moreover, in each type we have classes of different levels. So we do not have one definition of natural number, but we have natural numbers in every (appropriate) type and level. In more detail: when we ask what the natural numbers are, we have to start with the number 0, and we have also to define the successor operation on cardinal numbers, and, finally, define the natural numbers as the smallest class which contains 0 and which is closed under successor. But, when one says "smallest class", one has a quantifier ranging over classes: something belongs to the smallest class satisfying a certain condition only when it belongs to all classes satisfying that condition. But in RTT we cannot talk about all classes unrestrictedly, we can only talk about classes of a certain type and level. Therefore we have not just one notion of natural number, but a notion of natural number in each appropriate type and at each appropriate level of (predicative) definition.

Obviously, this should have been disturbing to Russell. To get around

[2]Actually, Frege did not include geometry in his program.

that, he had some idea of what he called typical ambiguity, which was: whatever you do in one of these types and levels of definition looks the same as what you do at every other type and level. A case could be made for that in arithmetic, but it is when we come to the real numbers that we get into serious problems about the logical foundations of mathematics in RTT. Real numbers may be identified—if one is going to reduce them to something more basic—in terms of rational numbers, either as Cauchy sequences or as Dedekind sections of rational numbers. Rational numbers can, of course, be built up by pairing from natural numbers or integers, so there is no problem about that, assuming a suitable notion of pairing. But real numbers then appear as objects of a higher type than rational numbers, basically one higher type, and so if we have the natural numbers as equivalence classes in types $n \geq 2$, then the real numbers will be in the next higher type on up, so again one does not have one notion of real number, but a notion associated with each (appropriate) type and level. Now, when one comes to verifying the *least upper bound axiom* for real numbers, one finds that if you are dealing with a class of real numbers of a certain level then the least upper bound axiom has to take you to a higher level though within the same type. The invocation of typical ambiguity does not serve to deal with this problem. You cannot stay within the level, you always have to go to higher levels in order to satisfy the least upper bound axiom. And since that seems to be a basic essential principle of analysis, RTT proves to be unworkable mathematically. For that purely pragmatic kind of reason, Russell introduced what is called the *Axiom of Reducibility*, which basically says: anything which is definable at a higher level is coextensive with something which is introduced in the most basic level. Formally, for type 1 variables, this takes the form

$$\forall X^{(j)} \exists Y^{(0)} \forall n [n \in X^{(j)} \Leftrightarrow n \in Y^{(0)}]$$

where $X^{(j)}$ is a variable of type 1 and level j, $Y^{(0)}$ a variable of type 1 and level 0, and n is a variable of type 0.

In effect this eliminates the distinction between levels of definition and wipes out Ramified Type Theory in favor of the impredicative Simple Theory of Types. Russell, in the introduction to the second edition of *Principia Mathematica*, said that he realized that pragmatic necessity was not a good fundamental reason for accepting the Axiom of Reducibility. But he seemed to feel that some such justification could eventually be given for it.

Weyl, as I said, allied himself with Poincaré, and did not accept the logicist program of trying to reduce all of mathematics to logic, which would have to start with a definition of the natural numbers. Instead, he said that the natural number system was an irreducible minimum of mathematics and that we would have to accept proof by induction and definition by recursion as basic principles. He also rejected the Axiom of Reducibility on the grounds that it was "künstlichen und unbrauchbar", i.e. artificial and unworkable. Weyl also said of the Axiom of Reducibility, that it was this "chasm" which separated him from Russell.

So, Weyl was a predicativist in the sense that he was only going to deal with things that were introduced by definition, but not an absolute predicativist in the sense that everything had to reduce to purely logical principles—rather, a predicativist given the natural numbers. Now, there are different ideas as to what predicativity means and I do not want to go into those, but one could think of it as a kind of a relative stance: if one understands or grants certain concepts, then what is predicative given those concepts is that which is obtainable by successive definition from them.

Weyl's system, reconstructed. I want to turn now to the axiomatics of Weyl's system. What he is doing here, you will see, is to work with the assumption that we have the natural numbers, that those are given and that basic constructions on the natural numbers are accepted. The main question then is: which sets (or classes) of natural numbers are we to deal with? Since Weyl does not want to use a ramified system because that leads to unworkable formulations of notions, he basically confines himself to level 0 sets definable in terms of the natural number structure. That is, we are not going to use—in the definitions of sets of natural numbers—bound variables ranging over sets of natural numbers of various levels; we are only going to use bound variables ranging over the natural numbers. On the other hand, we should be able to talk (by quantification) about *properties* of "arbitrary sets". So from a modern point of view, the syntax of Weyl's system looks as follows.

- Variables for natural numbers: x, y, z, $\cdots$
- Variables for sets of natural numbers: X, Y, Z, $\cdots$
- Primitive recursive functions: 0, $'$, f, g, $\cdots$
- Individual terms: s, t, $\cdots$

- Atomic formulas: $s = t$, $t \in X$
- Formulas: $\neg\phi$, $\phi \wedge \psi$, $\phi \vee \psi$, $\phi \to \psi$, $\exists x\phi$, $\forall x\phi$, $\exists X\phi$, $,\forall X\phi$

In words, we have variables for natural numbers and variables for sets of natural numbers. We have constructions on natural numbers: 0, successor($'$) and various primitive recursive functions like addition, multiplication, and so on. Individual terms, i.e. terms representing natural numbers, are built from variables and the constant 0 by these primitive recursive operations. And the kinds of basic questions we can ask are whether two terms are equal or whether a term belongs to a set. Then we have properties expressible in this system by the logical operations of negation, conjunction, disjunction, implication, existential and universal quantification over natural numbers and over sets. Weyl accepts classical logic here, and, in a sense, accepts the natural numbers as a completed totality for which that is appropriate. In the intuitive sense of the word, they form the only completed totality in his system.

A formula is called *arithmetical* if it does not contain bound set variables. So, an arithmetical formula defines a property which refers only to the totality of natural numbers but does not refer to the totality of sets of natural numbers. This leads to a system which is called $\mathbf{ACA}_0$. 'ACA' is an acronym for the Arithmetical Comprehension Axiom, and the sub '0' indicates a certain kind of restriction on the induction axiom that will be explained.

Axioms for $\mathbf{ACA}_0$

I Peano Axioms for 0, $'$

II Defining equations for f, g, $\cdots$

III Induction Axiom

$$\forall X[0 \in X \wedge \forall x(x \in X \to x' \in X) \to \forall x(x \in X)]$$

IV Arithmetical Comprehension Axiom

$$\exists X \forall x[x \in X \leftrightarrow \phi(x)]$$

for each arithmetical ϕ, where the variable X is not in ϕ.

$\mathbf{ACA_0}$ is a modern formulation of Weyl's system. In words: it takes Peano's Axioms for 0 and successor, i.e. that 0 is not a successor, and successor is a one-to-one function. For each primitive recursive function one takes its defining equations. Induction is given as a single axiom, which says that any set which contains 0 and is closed under successor, contains all natural numbers. The Arithmetical Comprehension Axiom tells us which sets are guaranteed to exist. It is a scheme, each instance of which is given by an arithmetical formula $\phi(x)$, and says that there is a set, X, which ϕ defines. So for those particular kinds of formulas we can speak of this set X as the set of all x's satisfying $\phi(x)$.

The system in which we take, instead of the induction axiom III, the induction axiom *scheme*, which allows us to apply induction to *any* property formulated within the system, is called **ACA** nowadays. This scheme has the form

$$\text{III}' \;\; \phi(0) \wedge \forall x(\phi(x) \rightarrow \phi(x')) \rightarrow \forall x \phi(x), \text{ for each formula } \phi.$$

Note that in III′, ϕ may contain bound set variables. Only those instances of III′ for ϕ arithmetical can be derived in $\mathbf{ACA_0}$, from III and IV.

There is an ambiguity in Weyl's system, because it is not clearly formalized in his 1918 monograph, as to whether he accepts just III or, more generally, III′. In fact, though, for all the mathematical work that he does in his system, III suffices, but that is only verified by careful examination of the proofs. There are other ambiguities in Weyl's formulation that I do not want to go into, which are spelled out in my "Weyl vindicated" article (Feferman 1988).

Given this system, and working informally with its basic principles in mind, what part of analysis can you do? We have the natural numbers, **N**, and pairing on **N** is given by a primitive recursive function. Then we define the integers **Z** as usual, as pairs of natural numbers representing differences, and the rationals **Q** as pairs of integers representing quotients. All this is obtained in a standard constructive way from the natural numbers. So the elementary theory of **N**, **Z** and **Q** still stays at the level of the natural numbers.

It is only when we come to the real numbers that we have to step up in type. And, as I said, we can treat these either by Cauchy sequences of rationals or by Dedekind sections. Weyl chose to treat them in the latter way, and I follow that here. In my third lecture I will talk about a different predicative approach which makes use of Cauchy sequences instead, but the main results are the same.

The idea of Dedekind sections is that every real number is associated with, and is determined by, the set of all rational numbers which are less than it. Now, the construction of real numbers goes by looking at those sets of rational numbers which are closed to the left in the usual ordering, have no largest element and which do not exhaust the totality of rational numbers; that is the notion of a (lower) Dedekind section. Real numbers, then, are identified with Dedekind sections, and you can then define addition, multiplication, and so on for real numbers in a standard way.

The usual *Least Upper Bound Axiom*, LUB, says that every set of reals which is bounded above has a least upper bound. How does this principle look in Weyl's system? A set S of real numbers is a subset of the set $Ded(\mathbf{Q})$ of all Dedekind sections in the rationals, $S \subseteq Ded(\mathbf{Q})$. If we have a non-empty set of Dedekind sections and it is bounded above then its least upper bound has to be another Dedekind section. Considered set theoretically, the way in which that Dedekind section, $lub(S)$ is obtained from the set S is just by forming the union of S:

$$lub(S) = \bigcup S$$

So, what is wrong with that, if anything? Well, if the set S is given by a property ψ,

$$X \in S \leftrightarrow \psi(X),$$

then the rationals q which belong to the union $\bigcup S$ are just those objects which belong to some element of S, in other words, just those for which there exists a set, X, such that $\psi(X)$ holds and such that q belongs to X:

$$q \in \bigcup S \leftrightarrow \exists X[\psi(X) \wedge q \in X]$$

From Weyl's point of view this is an impredicative definition, because it refers to the totality of subsets of the natural numbers, by quantification over sets. And that is not derivable in his system, which only uses the Arithmetical Comprehension Axiom. So, you cannot derive the least upper bound axiom in its most general form in $\mathbf{ACA_0}$ (or even $\mathbf{ACA}$).

But Weyl observed that you *can* derive the Least Upper Bound axiom, not for *sets* of real numbers, but for *sequences* of real numbers. A sequence of real numbers, $\langle X_n \rangle_{n \in \mathbf{N}}$, in the sense of a sequence of Dedekind sections of $\mathbf{Q}$, is simply given by a subset X of $\mathbf{N} \times \mathbf{Q}$; whose sections are just the terms of the sequence $\langle X_n \rangle_{n \in \mathbf{N}}$, i.e.

$$q \in X_n \leftrightarrow \langle n, q \rangle \in X.$$

The least upper bound of the sequence is then the union, now over natural numbers, of its terms, i.e.

$$lub_{n \in \mathbf{N}}(X_n) = \bigcup_{n \in \mathbf{N}} X_n$$

A rational q, belongs to such a least upper bound simply if there exists a natural number, n, such that $\langle n, q \rangle$ belongs to X:

$$q \in lub_{n \in \mathbf{N}}(X_n) \leftrightarrow \exists n(\langle n, q \rangle \in X).$$

That is just an arithmetical definition, it only involves arithmetical quantification, and it is therefore predicative in Weyl's sense. It is thus acceptable within the system $\mathbf{ACA_0}$.

The natural question then is: if you go back to doing analysis and you do not allow the least upper bound axiom, LUB, for sets, but you *do* have it for sequences, what can be done and how much of what you did before can still be done? You may not accept Weyl's argument for *why* one should restrict oneself to predicative definitions. But given that that was what he wanted to do on philosophical grounds, you cannot do real analysis with the LUB axiom taken in its usual sense. However, you can try to see what you can do if you just apply the LUB axiom to sequences rather than to sets.

There is a little bit of a puzzle here because in everyday reasoning the LUB axiom for sets is equivalent to that for sequences. That the former implies the latter is trivial, but one needs the Axiom of Choice (AC) for the converse. The argument is that if you have a set of real numbers which does not contain a largest element then beyond any real number in the set there is another real number in the set which is greater than it, so using AC there exists a sequence whose limit is the same as the supposedly least upper bound of the set. In that way you reduce the least upper bound axiom for sets to that for sequences, but at a price, namely assuming AC, and that is not justified predicatively. (However, it is, in certain special forms involving an extension of predicativity beyond $\mathbf{ACA_0}$. But even those forms are not enough for the usual formulation of theorems involving the LUB axiom for sets in standard set-theoretical foundations of analysis.)

The following example illustrates the difference between these two ways of spelling out the LUB principle. A basic theorem of analysis is the Heine-Borel theorem, which states that every closed interval of real numbers is compact: i.e., if we have an arbitrary covering of the closed interval by open intervals or more generally by open sets, then a finite sub-covering exists. That is not provable in the system $\mathbf{ACA_0}$. (One can give an independence argument by a suitable model, and there are even arguments to show, in a strong way, why it is an impredicative theorem.) What you *can* prove is the so-called sequential compactness of the closed interval, the Bolzano-Weierstrass theorem, which says that any bounded sequence of real numbers, not necessarily Cauchy, contains a Cauchy sub-sequence. That is a very special consequence of compactness in the usual sense of the word.

This raises the question: which theorems depending on compactness can be done using only sequential forms? For example, if you read standard introductions to analysis, you find the theorem that a continuous function on a closed interval is uniformly continuous. On the face of it, that uses the Heine-Borel theorem, but in fact you can prove in this case that you only need the Bolzano-Weierstrass theorem. The theory of Riemann-integration of continuous functions depends on their uniform continuity, and you can proceed in a standard way from there.

That is one point as to how to get off the ground in $\mathbf{ACA_0}$. The second point is that if we are going to talk about functions of real numbers we have in general to go one type higher, because a function is an operation from Dedekind sections to Dedekind sections. However, if we are only talking about continuous functions we can manage without higher types. A continuous function Y=F(X) (say on an interval) is determined by its values at rational numbers. The rationals are embedded in the reals, considered as Dedekind sections via the association (for $q \in \mathbf{Q}$):

$$q \mapsto X_q = \{r \in \mathbf{Q} \mid r < q\}.$$

Then the function F is determined by

$$\lambda q. F(X_q).$$

The graph of the function F at rational arguments to Dedekind sections is then given by the set

$$Z = \{\langle q, r\rangle \in \mathbf{Q} \times \mathbf{Q} \mid r \in F(X_q)\}.$$

Therefore, we can reduce the properties of continuous functions to properties of subsets of pairs of rational numbers. In other words, for continuous functions we do not have to raise the type, we can stay at the level of type 2, the type of sets of natural numbers.

Examples of theorems. Listed next are some examples of theorems that Weyl proved in *Das Kontinuum* and which can be formalized in $\mathbf{ACA_0}$[3]. Basically he showed that all the properties of stepwise continuous functions that have been established in the 19th century foundations of analysis hold in his system, and he showed how various results in applications to mathematical physics could be derived within the system. These include:

- Max and min for continuous functions on an interval $[a, b]$
- Mean-value theorem for continuous functions on an interval $[a, b]$
- Uniform continuity of continuous functions on an interval $[a, b]$
- Existence of Riemann integral of (stepwise) continuous functions
- Fundamental Theorem of Calculus

Which particular functions can we deal with in Weyl's system (as given by $\mathbf{ACA_0}$)? Well, all the familiar functions, e.g. trigonometric, exponential, and all that we can represent by power series, and all that can be represented in sequential forms, etc., can be reconstructed within the system. Various functions represented by Fourier series can be treated there, and so, basically, all reasonable 19th century analysis can be reconstructed, or redeveloped, on the basis of Weyl's system.

Limitations of Weyl's system. However, we cannot live with 19th century analysis in modern applications of analysis; these require extensive functional

[3]The recent volume, Simpson (1998), contains a substantial body of information about what mathematical theorems can be proved in $\mathbf{ACA_0}$. In continuation of the "Reverse Mathematics" program inaugurated by Harvey Friedman, many of these are shown to be equivalent to the Arithmetical Comprehension principle over a weaker system.

analysis and various kinds of higher function spaces: Banach spaces, Hilbert spaces, etc. These spaces make use of possibly very discontinuous functions, for instance various kinds of measurable functions.

To begin with, we have to deal with classes of functions from the real numbers to real numbers, such as the continuous, Lebesgue measurable, L_2 functions etc., which form reasonable spaces, in some sense, of modern analysis. But once we go beyond continuous functions, it is not so obvious that we can deal with these at the type level of continuous functions, which can be reduced to type 2. Moreover, when we deal with *functionals* applied to such spaces, we are going to a still higher type. This leads to obvious limitations of Weyl's system. Natural 20th century mathematical talk requires us to deal with abstract function spaces and with functionals and operators on these spaces. And it is necessary to deal with the spectral properties of operators on these spaces and many other advanced topics. It is not obvious how all this can be developed in Weyl's system. However, these problems are handled in modern extensions of Weyl's system, and that is what I will devote part of Lecture 3 to.

As a logical footnote to that, the system $\mathbf{ACA_0}$, which I described here, is a conservative extension of Peano Arithmetic, even though it employs second order concepts. We would like to see whether there are more flexible systems which allow us to deal with notions of higher type, and which are reducible to Peano Arithmetic in a similar way. In the next lecture we shall see that there exist many new predicative and constructive systems, which extend the ideas of Weyl and Brouwer, and that it is possible to prove very strong reduction results for such systems.

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