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THE SIMPLE THEORY OF TYPES

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In the first part of this paper I review the early history of the simple theory of types; in the second part I discuss some questions raised by the work (published and unpublished) of A.M. Turing on type theory.

EARLY HISTORY

I have nothing new to say about sources. A rather superficial skimming through Church's bibliography and the known sources suggested that the papers which are usually cited, by Russell, Chwistek and Ramsey, are indeed the sources from which the simple theory of types was developed. These papers are listed in the bibliography for this paper. But many of the historians, or logicians making historical remarks, seem to me to have misunderstood and misreported these early papers. So a review, a REview, may be of some use.

The simple theory of types provides a straightforward, reasonably secure, foundation for the greater part of classical mathematics. That is why a number of authors (Carnap, Gödel, Tarski, Church, Turing) gave a precise formulation of it, and used it as a basis for metamathematical investigations. The theory is straightforward because it embodies two principles which (at least before the advent of modern abstract concepts) were part of the mathematicians normal code of practice. Namely that a variable always has a precisely delimited range, and that a distinction must always be made between a function and its arguments. In this sense one might claim that all good mathematicians had anticipated simple type theory. [Indeed Turing made this claim for primitive man]. The claim goes too far, but it does draw attention to the fact that the justification for the theory is not to be found by considering it as a formal device to avoid Russell's paradox. This point is very clearly put by Church in his interesting paper 'Schröder's anticipation of the simple theory of types':

> Speaking in more general terms we may say that Schröder regarded a class as somehow built of its members, as a wall is built of brick or (to adopt Frege's simile) as a wood is composed of trees. From such a point of view, according to which members of a class are prior to the class, something like a theory of types does appear natural and inevitable.

[In discussion at the conference Michael Dummett argued that Church claims too much for Schröder. Schröder's stipulation that an individual in a domain D must not, if it is itself a domain, have elements in common with D is, as Church recognises a technical necessity. There is hardly a hint of the type structure, let alone any suggestion that it might form a foundation for mathematics]. Mathematicians at that time did not have too clear a general notion of 'set', but the sets with which they were familiar were certainly constructed by 'assembling' previously given elements.

I find it extraordinary - even after making allowance for the climate of metaphysical abstract in which, philosophically speaking, Bertrand Russell grew up - that nowhere in his writings (?) certainly nowhere in the Principia, does Russell adopt, or even refer to the kind of mathematical common sense described in the previous paragraphs. And his blindness was inherited by many other logicians.

I consider next the theory of types which Russell outlines, and rejected, in appendix B to the Principles of Mathematics. This is a simple rather than a ramified theory. He defines, in effect:

X is a type if it is of the form $\{x:\phi(x) \text{ is significant}\}$;

X is a range if it is of the form $\{x:\phi(x)\}$;

Russell is here very naive about what propositional functions $\phi(x)$ can be used. He allows $V = \{x:x \text{ is a range}\}$ and $U = \{x:x=x\}$ as ranges so that $V \in V$ (and presumably, though he does not remark the fact, $U \in V$ and $V \in U$). From his paradox he concludes that the Russell class is not in the range of significance of $x \in x$ but gives no other reason for excluding it. The reason which he gives for doubting the correctness of this theory of types is sufficiently curious to be worth reproducing. Let $\text{Prop} = \{x:x \text{ is a proposition}\}$, and let $\bigwedge P$ stand for the (infinite) conjunction of all the propositions in the set P. Russell supposes:

(a) $P \subseteq \text{Prop} \Rightarrow \bigwedge P \in \text{Prop}$;

(b) $\bigwedge P = \bigwedge Q \Rightarrow P = Q$.

Now let

$$Q = \{p \in \text{Prop}: \exists P \subseteq \text{Prop}.\ p = \bigwedge P \wedge p \notin P\},$$

and let $q = \bigwedge Q$. Then, using (b) $q \in Q \Leftrightarrow q \notin Q$. He then says:

> It is possible of course to hold that propositions themselves are of various types and that logical products must have propositions of only one type as factors. But this suggestion seems harsh and highly artificial.

A sentiment which may help to explain why it took him so long (5 years) to arrive at the ramified theory of types.

I now consider Chwistek's contribution. His motives in proposing a simple theory of types were disingenuous; he did not believe that it could provide a true

foundation for mathematics. To understand his point of view it is necessary to particularise. In (1912) Chwistek claimed to have derived a contradiction from the axiom of reducibility; the arguments are repeated in (1921) and (1922). It is customary to dismiss this claim as based on a misunderstanding. I think Chwistek's argument deserves more attention than it is usually accorded; I will describe it briefly.

In Principia the universe of individuals, the primitive relations over them, and the resulting atomic propositions are taken as given, and as not being part of logic. Russell, supported by Wittgenstein's Tractatus seems always to have held that in principle the universe of all existent things could be completely described by atomic propositions. I call this universe the ultimate universe of discourse. The logical constructions of Principia are built up from the primitive terms in the description of this universe, and the truth of non-logical statements such as the axiom of infinity is determined by the atomic proposition which are true of it. (Russell often refers to the truth of the axiom of infinity as a matter of empirical fact.) The range of a variable of any (ramified) type consists of just those propositional functions which can be defined by the means appropriate to that type from the primitive relations of the ultimate universe of discourse.

Now, to illustrate Chwistek's argument, suppose that there were only finitely many primitive relations. (This might be false, but it is not absurd - indeed both Carnap and Russell believed at one time that similarity was the only primitive relation.) Then one could enumerate all predicative properties of the natural numbers (construed, say, as classes of classes of individuals) by a single relation of appropriate type. So, by diagonlisation, one could define a property of the natural numbers which differed from all predicative properties, thus contradicting the axiom of reducibility. So the axiom of reducibility (which appears to be of a logical nature) has entirely non-logical consequences - that there are not only finitely many primitive relations. Indeed if the axiom of reducibility is true there cannot be any logically definable enumeration of the primitive relations of the ultimate universe of discourse. This last conclusion was totally unacceptable to Chwistek, and that is why he claims that the axiom of reducibility leads to contradiction. I quote from (1921).

> If we accept the Axiom of Reducibility we have to give up once and for all the logic of constructive propositions and functions, that is of such functions as may be constructed by means of letters or symbols given beforehand by enumeration, and we have to resort to functions which cannot be thus constructed. This seems to be inconsistent with the basic conception of logic which apparently should be independent of metaphysical assumptions such as the existence of non-constructive functions.

In his later work Chwistek went even further; the expressions (for constructive functions and so on) themselves became mathematical objects, rather than being treated metamathematically. From all this it will be seen that the simple

theory of types was eminently not Chwistek's cup of tea. But he did point out that it was an alternative way of escaping from the unacceptable consequences of the axiom of reducibility. In both (1921) and (1922) he remarks that if one is ready to ignore the semantic notions involved in Richard's paradox, then one can jettison the hierarchy of orders and classify propositional functions simply and solely by the type of their arguments. In (1921) he writes

> It should be noted that the elimination of these elements from the theory of types of Whitehead and Russell would render this theory exceedingly simple and manifest.

But, apart from asking (in (1922)) whether the resulting theory would be consistent, he pursued the matter no further.

Now I turn to Ramsey (1926). The standard thing to say about Ramsey's paper is that it distinguished between the set-theoretic and the semantic paradoxes, and that it pointed out that the simple theory of types suffices to avoid the known instances of the former. This is, of course, true. But, like so many of the received ideas trotted out by philosophers and logicians when they write about the foundations of mathematics, it misses the main point, the point which as the quotation below shows - Ramsey himself put first. Ramsey's head was sometimes in the clouds, but his feet remained firmly on the ground of mathematical experience. In this paper the clouds include an extreme platonism ('our inability to write propositions of infinite length which is logically a mere accident'), but through them he has a clear view of that creation of the mathematical imagination which distinguishes Cantorian set theory from the logic of classes: the concept of an arbitrary subset existing only in extension. I quote.

> The possibility of indefinable classes and relations in extension is an essential part of the extensional attitude of modern mathematics... that it is neglected in Principia Mathematica is the first of the three great defects in that work. The mistake is made not by having a primitive proposition asserting that all classes are definable, but by giving a definition of class which applies only to definable classes, so that all mathematical propositions about some or all classes are misinterpreted. This misinterpretation is not merely objectionable on its own account in a general way, but is especially pernicious in connection with the multiplicative axiom which is a tautology when property interpreted, but when misinterpreted after the fashion of Principia becomes a significant empirical proposition.

The contrast between Ramsey's approach and Chwistek's could hardly be greater. But the whole weight of mathematical experience from the eighteenth century onwards is surely on Ramsey's side. Euler's view that one should allow arbitrary continuous initial configurations for vibrating strings - as opposed to D'Alembert's insistence that they must be analytic: Fourier's work on discontinuous functions: the pathological counter-examples given by Weierstrass: Hilbert's work on Dirichlet's problem: Schwarz's theory of distributions: in each of these developments the important thing is not a mere increase in generality but the opening, by the enlargement of our concept of a function, of gates to new paths

of mathematical thought. The most significant objection to the imposition of criteria of constructibility on the foundations of mathematics as exemplified, for example, in the writings of Bishop and Martin-Löf is that it prevents us, or at very least strongly discourages us, from using our mathematical imagination to create or discover new, beautiful and applicable mathematics. (Cantor wanted pure mathematics to be called free mathematics.) These flights of mathematical imagination are not so high in the air that they have nothing to contribute to the concrete mathematics of computation. Suppose, for example, that D'Alembert had successfully imposed his views. Then there would have been no stimulus to the development of the theory of Fourier series and integrals, and no need to look beyond power-series for the solution of differential equations. I am not arguing that constructive ideas should not be taken seriously - for example, Gödel discovered the constructible sets by taking the notion of predicativity seriously. What I am saying is that mathematicians do well to disregard stern moral injunctions dogmatically issued. Better be a thief than an honest toiler in a labour camp.

To return to the simple theory of types. Ramsey's contribution should be seen then as a liberating, not a restricting, step in the development of a foundation for classical mathematics.

Neither Chwistek nor Ramsey gave a detailed formulation of the theory, though evidently they could have done so if they had wished. The first detailed formulation is by Carnap (1929) who cites Ramsey but not Chwistek. Meticulous formulations are given in Tarski (1931) (who cites Chwistek and Carnap but not Ramsey) and in Gödel (1931) (who cites only Principia Mathematica).
A rather eccentric system, based on the simple theory of types, was published by Quine (1934); this certainly influenced Church's 1940 formulation. [Indeed Church gives Quine's paper a star (for importance) in his bibliography - surely one of the master's very few lapses from impeccable judgement.]

The formulation in Church (1940) differs from the previous formulations in a number of important ways: (1) there is a type of propositions; (2) for any two types there is the type of functions from one to the other; (3) terms for functions are formed by lambda-abstraction. The last feature allows one to dispense with the comprehension axioms and in conjunction with the rules of lambda conversion simplifies the procedure for substituting complex expressions for predicate letters. [A procedure which was ignored in Principia and incorrectly described in the first two additions of Hilbert-Ackermann and in many other text books - including some published as late as the 1950's.] A reformulation of Church's system in Henkin (1963) has the engaging feature that the only primitive constants are the relations of identity for each type.

TURING'S WORK ON TYPE THEORY

First I give a brief account of the available material, published and unpublished.

[1] Letters to M.H.A. Newman. There are 5 of these, handwritten, covering 31 sides; 4 were written in 1940, one in 1942.

[2] (with M.H.A. Newman) 'A formal theory in Church's theory of types', J.Symb.Logic Vol.7 (1942) pp.28-33, (received by the editors, 9 May 1941).

[3] 'Some theorems about Church's system', 20 quarto pages of typescript (1940?).

[4] Pages 37-43, 60-73 of an untitled quarto typescript (perhaps a continuation of an earlier draft of [3]).

[5] 'The use of dots as brackets in Church's system', J. Symb.Logic Vol.7 (1942) pp.146-156, (received by the editors 17 June 1942).

[6] 'A practical form of type theory I', 30 typescript quarto pages; a first draft of [10].

[7] 'A practical form of type theory II', 81 typescript quarto pages with some extra pages of handwritten insertions.

[8] 'Outline of proof of metatheorem p.38 of "Practical forms" ', 20 pages manuscript, (the metatheorems referred to are labelled (1) and (2) on the last page of [10]).

[9] 'The reform of Mathematical Notation and Phraseology', 14 pages quarto typescript; there appear to be 4 pages missing in the middle.

[10] 'Practical forms of type theory', J.Symb.Logic Vol.13 (1948) pp.80-94, (received by the editors Jan.6 1947).

[Turing bequeathed the manuscripts to me. They are deposited in the library of King's College, Cambridge.]

Turing had learnt about Church's type theory when he was at Princeton (1936-1938). Almost all the work covered by the above material must have been done between 1940 and 1946. (As has recently been revealed, in 1940 he was engaged in breaking the code of the war-time Enigma encoding machine; later on (1944-1945) he had a less taxing job - building a speech encoder of his own invention.)

Neither mathematically nor philosophically is there anything very surprising or exciting in the manuscripts. [A fairly complete account will appear in my edition of his collected work which is now, as ever, in active preparation.] In view of the current interest in normalization theorems it is perhaps worth recording that the first page of [3] contains a proof of normalisation for the typed lambda calculus. [3] and [4] contain various simple relative consistency proofs for additions to and modifications of Church's system. (Also a purported relative consistency proof of the axiom of infinity in the absence of extensionality: the howler in it is almost of the schoolboy variety.) [7] gives a proof

of the completeness of the cumulative type system of [10] when the axiom of infinity is replaced by axioms of finitude, and a proof of the equivalence asserted in section 3 of [10]. The topics discussed in [9] are (1) the distinction between free and bound variables, (2) the use of hypotheses and the deduction theorem and the role played by variables which are restricted by hypothesis, (3) the application of the theory of types in ordinary mathematical practice.

As might be expected from his other mathematical work, Turing's attitude to the theory of types is common-sensical and down-to-earth; closer to the view attributed by Church to Schröder than to the philosophical positions of Russell Chwistek and Ramsey. Some quotations will give the flavour. From [6]:

> The author wishes to repudiate any implication that may be suggested by this paper to the effect that he believes Russell's philosophy of mathematics to be the truest. He does believe however that it is the one which is most easily understood, and also that it describes most closely the accepted forms of mathematical thinking.

From [9]:

> Actually it is not difficult to put the theory of types into a form in which it can be used by the mathematician-in-the-street without having to study symbolic logic, much less use it. The statement of the type principle given below was suggested by lectures of Wittgenstein, but its shortcomings should not be laid at his door.
>
> The type principle is effectively taken care of in ordinary language by the fact that there are nouns as well as adjectives. We can make the statement 'All horses are four-legged', which can be verified by examination of every horse, at any rate if there are only a finite number of them. If however we try to use words like 'thing' or 'thing whatever' trouble begins. Suppose we understand 'thing' to include everything whatever, books, cats, men, women, thoughts, functions of men with cats as values, numbers, matrices, classes of classes, procedures, propositions,... Under these circumstances what can we make of the statement 'All things are not prime multiples of 6'. What do we mean by it? Under no circumstances is the number of things to be examined finite. It may be that some meaning can be given to statements of this kind, but for the present we do not know of any. In effect then the theory of types requires us to refrain from the use of such nouns as 'thing' 'object' etc., which are intended to convey the idea 'anything whatever'.

[Notes of the lectures by Wittgenstein referred to have recently been published: 'Wittgenstein's lectures on the foundations of mathematics', ed. Cora Diamond, Harvester Press 1976.]

He goes on to propose the term 'noun class' for a permitted range of a variable and mentions operations (e.g., the power-set operation) which can be used to form one noun class from another. In [10] a more formal definition of noun class is given: any definable subset of some level in the cumulative hierarchy of types. I cannot resist one further quotation (from [6]).

> There is a close connection between the role that these formulae play and the use of nouns in ordinary language, so much so that one tends to feel that Russell's type theory was largely anticipated by prehistoric man.

But this <u>is</u> going too far; everyday conventions for avoiding the use of un-

restricted variables are not sufficient if one wishes, for example, to handle transfinite ordinals and cardinals in a trouble-free way. And this brings me to a last question: why did Turing prefer type theory to set theory as the ground on which the relations between mathematical practice and symbolic logic should be discussed? I think I can give a series of partial answers, which do add up to an adequate answer.

1. Turing, I am sure, was not familiar with Zermelo's cumulative type structure. So set theory will have seemed to him (as to many others) merely a formal system, quite lacking the sort of quasi-concrete interpretation which he needed if he were to handle a subject successfully.

2. In the same spirit he did not much care for 'general abstract nonsense'. He liked the kind of mathematics which is concerned with specific, more or less concrete problems. But this kind of mathematics, certainly then, perhaps still today, can be formulated within the finite theory of types. [In his paper on ordinal logics he showed that the Riemann hypothesis can be formulated as a two-quantifier sentence of number theory.]

3. One might be surprised that he seems never to have shown much interest in the transfinite alefs. [This same lack of interest is manifest in Frege and Russell.] One obvious possible reason is that he had genuine doubts about their consistency. But, at least equally important, is the fact that in the 1930's very few interesting things were known about them, and there had not been, I think, any significant applications of them to other branches of mathematics.

BIBLIOGRAPHY

A. Sources

F.W.K.E. Schröder (1890). Vorlesungen über die Algebra der Logik, Vol.I, Leipzig 1890.

B. Russell (1903). The principles of mathematics, Cambridge 1903.

L. Chwistek (1912). Zasada sprzeczności w świetle nowszych badań Bertranda Russella, Rozprawy Akademii Umiejetności (Krakow), Wydział historyczno-filozoficzny, 2s. vol 30 pp.270-334.

——— (1921). Antynomje logikiformalnej, Przegl.fil.vol 24 pp.164-171: translated in Polish Logic 1920-1939 (Ed. Storrs McCall), Oxford 1967, pp.338-345.

——— (1922). Über die Antinomien der Prinzipen der Mathematik, Math. Zeitschrift vol 14 pp.236-243.

F.P. Ramsey (1926). The foundations of mathematics, Proc.Lond.Math.Soc.2 ser vol 25 pp.338-384.

R. Carnap (1929). Abriss der Logistik, Vienna 1929.

A. Tarski (1931). Sur les ensembles definissable de nombres réels I, Fund.Math. vol 17 pp.210-239.

K. Gödel (1931). Uber formal untentscheidbare Sätze der Principia Mathematica und verwandter Systeme I, Monatsh.Math.Phys. vol 38 pp.349-360.

W.V. Quine (1934). A system of logistic, Cambridge, Mass. 1934.

A. Church (1940). A formulation of the simple theory of types, J. Symb.Logic vol 5 pp.56-68.

L. Henkin (1963). A theory of propositional types, Fund.Math. vol 52 pp.323-344.

B. Other references

A. Church (1936). A bibliography of symbolic logic, J. Symb.Logic. vol 1 pp.121-218.

——— (1937). Review of L. Chwistek's 'Überwindung der Begriffrealismus', J. Symb. Logic vol 2 pp.168-170.

——— (1939), Schröder's anticipation of the simple theory of types, Preprinted for 5th Int. Congress for the Unity of Science, Cambridge, Mass. as from Jour.of Unified Science (Erkenntnis) vol 9, 4 pages.