HIGHLIGHTS OF THE HISTORY OF THE LAMBDA-CALCULUS

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1. Early beginnings. The lambda-calculus originated in order to study functions more carefully. It was observed by Frege in 1893 (see van Heijenoort 1967, p. 355) that it suffices to restrict attention to functions of a single argument. For, suppose you wish a function to apply to A and B to produce their sum, A + B. Let ⊕ be a function, of a single argument, which when applied to A alone produces a new function, again of a single argument, whose value is A + B when applied to B alone. Note that ⊕ is not applied simultaneously to A and B, but successively to A and then B; application to A alone produces an intermediary function ⊕(A), which gives A + B when later applied to B alone. That is, A + B = (⊕(A))(B).

This is the way computers function. A program in a computer is a function of a single argument. Many people think, when they write a subroutine to add two numbers, that they have produced a program that is a function of two arguments. But what happens when the program begins to run, to produce the sum A + B? First A is brought from memory. Suppose that at that instant the computer is completely halted. What remains in the computer is a program, to be applied to any B that might be forthcoming, to produce the sum of the fixed A and the forthcoming B. It is a function of one argument, depending on the fixed A, to be applied to any B, to produce the sum A + B. It is Frege's intermediary function ⊕(A).

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Apparently Frege did not pursue the idea further. It was rediscovered independently (see Schönfinkel 1924 [BSM]) together with the astonishing conclusion that all functions having to do with the structure of functions can be built up out of only two basic functions, K and S. Let us adopt the notation that has been in vogue since then. Instead of writing the value that one gets by applying the function F to A as F(A), we write (FA). Omission of the outside parentheses will be usual. When more than two terms occur, association shall be to the left; thus MNP denotes ((MN)P), but M(NP) denotes (M(NP)). Then the sum of A and B would be written ⊕AB.

The functions K and S are such that

(1.1) KAB = A

(1.2) SABC = AC(BC).

For a proof that all functions can be built up of K and S, one can consult the original Schönfinkel paper. Or one can consult the two early papers, Curry 1929 [ALS] or Curry 1930 [GKL].

Suppose we have two functions, F and G (built up out of K and S, of course) such that by means of (1.1) and (1.2) one can show that

(1.3) FX = GX

for each X. This means that F and G take the same value whenever they are applied to each X whatever, and so they ought to be the same function. That is, one should have

(1.4) F = G.

In general, one cannot prove (1.4) by means of (1.1) and (1.2). Curry (see Curry 1930 [GKL]) contrived additional axioms such that one can prove (1.4) whenever (1.3) holds for each X. A system like this is said to have the extensional property.

Curry added axioms to enable him to prove additional equalities. Did he go too far, so that now any two functions can be proved equal to each other? He did not. Indeed, he was careful to prove a weak form of consistency, in that there are many pairs of functions that cannot be proved equal to each other. And, especially, $K = S$ cannot be proved.

This gave a workable system, which illuminated many properties of functions. For instance, let M be built up from K, S, and the variable x. One can, using only K and S, build up a function F such that one can prove that

(1.5) $$Fx = M$$

by means of (1.1) and (1.2). However, F turns out to be a mixed up combination of K's and S's. Just from looking at F, one would not have the least clue that (1.5) should hold.

Church (see Church 1932 [SPF.I]) proposed that the F in question be called λxM. In this, M is intentionally part of the name of the function, so that by inspection you can see what you would get if you apply the function to x. For his construct, Church decreed that

(1.6) $(\lambda xM)N = M[x:=N]$;

by $M[x:=N]$ we mean the result of replacing each occurrence of x in M by N.

If one starts with the left side of (1.6) and replaces it by the right side, this is called a β -reduction. One is equally entitled to start with the right side of (1.6) and replace it by the left side of (1.6); this is called a β-expansion.

Thus, to produce the $\oplus$ that we had earlier, Church would use $\lambda x(\lambda y(x+y))$. By (1.6), one would have

(1.7) $(\lambda x(\lambda y(x+y)))A = \lambda y(A+y)$.

By (1.6) again, one has

(1.8) $(\lambda y(A+y))B = A+B$.

So taking $\lambda x(\lambda y(x+y))$ to be $\oplus$, one has

(1.9) $$\oplus AB = A+B$$

by two β-reductions.

The beauty of this is that at all stages of the process, one can tell by a simple inspection what the reduced form of $\oplus AB$ is going to be. This is the famous lambda-calculus of Church. (Henceforth, we will write LC for "lambda-calculus.") It does involve one with having to be careful about free and bound occurrences of variables. In $\lambda y(x+y)$, the occurrence of x is free and both occurrences of y are bound.

One has to be careful not to make manipulations which change free occurrences of a variable into bound ones. Thus, suppose one writes $\oplus y$. In this configuration, the observed occurrence of y is free. A blind adherence to (1.6) would give

$$\oplus y = \lambda y(y+y),$$

so that

$$\oplus yz = z+z.$$

This is certainly not what is intended for $\oplus$. The trouble is that the y, which originally existed as a free occurrence of a variable in $\oplus y$, has been put into $\lambda y(y+y)$ where its occurrence is now bound. Actually, when Church enunciated the rule (1.6) he was careful to impose the restriction that it should not be used if some variable with free occurrences in N should have those occurrences bound in $M[x:=N]$. In order to cope with this contingency, Church instituted the α-step

(1.10) $\lambda yM = \lambda z(M[y:=z])$.

NOTE: To avoid confusion of free and bound variables here, one must put the restriction that there are no free occurrences of z in M. Now we have

(1.11) $\oplus y = (\ \lambda x(\lambda z(x+z)))y = \lambda z(y+z)$;

the intermediate formula is got from the left one by an α-step . So one has

(1.12) $\oplus yx = (\ \lambda z(y+z))x = y+x$

which is just what $\oplus$ is supposed to do.

John McCarthy worked several ideas of the LC into LISP. He clearly recognized procedures as functions of one argument. In LC, such functions can be applied to each other and give such functions when applied. In LISP, it is possible to apply one procedure to another, and on occasion get another procedure.

The K and S, and things built exclusively of them, are called combinators. Can we commingle combinators and lambda-expressions? Yes indeed, with no trouble whatever.

Church had decided that one should form λxM only in case there are free occurrences of x in M. Thus, Church could not get a lambda-expression to correspond to K. It could be done if one relaxes the requirement that there be at least one free occurrence of x in M to form λxM. So the LC, as originally set up by Church, seems a trifle weaker than the combinatory calculi of Schönfinkel and Curry. For present day applications, either would serve perfectly well (this takes some proving) and the difference is just something to niggle over, quite insignificant. Originally, this was not known, and Rosser (see Rosser 1935 [MLV]) invented a couple of other combinators, in place of K and S, with which he set up an exact equivalent of the LC. Like Curry, his system had the extensional property and a weak form of consistency. Hence the LC has these attributes also.

The LC (and hence, the combinatory calculi) has a fixed point theorem:

Given a function F, one can find a Φ such that

(1.13) $$F\,\Phi = \Phi.$$

Proof. Take

$\Phi = \phi\phi$, where $\phi = \lambda xF(xx)$.

2. A debacle. The LC and the combinatory calculi were fairly promptly embedded in systems which had some of the earlier attributes of logical systems. See Church 1932 [SPF.I] and Curry 1934 [PEI]. The results turned out to be inconsistent. This was first proved in Kleene and Rosser 1935 [IFL] by a variation of the Richard paradox. Later, Curry got a simpler proof related to the Russell paradox. See Curry 1942 [IFL]. This has the following simple form. Suppose we have the two familiar logical principles:

(2.1) $$P \supset P$$

(2.2) $$(P \supset (P \supset Q)) \supset (P \supset Q),$$

together with modus ponens (if P and $P \supset Q$, then Q). We undertake to prove an arbitrary proposition A. By the fixed point theorem, we construct a Φ such that

(2.3) $$\Phi = \Phi \supset A;$$

for the benefit of those not too conversant with the fixed point theorem, we take

$\Phi = \phi\phi$, where $\phi = \lambda x(xx \supset A)$. By (2.1), we get

$$\Phi \supset \Phi.$$

Applying (2.3) to the second Φ gives

$$\Phi \supset (\Phi \supset A).$$

By (2.2) and modus ponens, we get

$$\Phi \supset A.$$

By (2.3) reversed, we get

$$\Phi.$$

By modus ponens and the last two formulas, we get

$$A.$$

This is usually referred to as the Curry Paradox.

Fitch proposed to avoid this by weakening the LC (or equivalent combinatory calculus) so that the fixed point theorem fails. He also weakened modus ponens a bit. See Fitch 1936 [SFL] and Fitch 1952 [SLg]. He has proved consistency for his system, but it is much too weak to be considered as a foundation for mathematics. He and his students have continued intermittently to the present to come out with improvements, but it still remains extremely weak.

Ackermann proposed keeping the full strength LC, but badly crippling implication. See Ackermann 1950 [WFL] and Ackermann 1953 [WFT]. He proved the consistency of the system, but it was hopelessly weak as a foundation for mathematics, and I know of no recent interest in it.

Curry kept the full strength combinatory calculus, but added a fragmentary theory of types and a weakened version of implication. He introduces a notion of functionality F, such that if Z is a function and X and Y are types, then $FXYZ$ is to denote that if U is of type X, then ZU is of type Y. See Curry 1934 [FCL] and Curry 1936 [FPF]. It turned out that the "natural" axioms for functionality lead to a contradiction, as shown in Curry 1955 [IFT]. However, by imposing suitable restrictions, all is well. See Curry 1956 [CTF]. Since then, Curry and his students have made extensive developments. Two major works, Curry and Feys 1958 and Curry, Hindley, and Seldin 1972, are landmarks. Even so, adoption of the system as a foundation for mathematics has not progressed at all, though the system has some capability in that direction; see Cogan 1955 [FTS].

3. Where do we go from here? As we said, Fitch and Curry are continuing to develop their systems. However, there is

no likelihood that either will be adopted as a foundation for mathematics. Originally, it was expected that the LC or a combinatory calculus should be a part of such a system. Surprisingly enough, the LC (or a combinatory calculus) has turned out to be of importance in its own right. So one has to ask the questions that are asked about any logical system.

1. What about consistency?

2. What about completeness?

3. What about models?

4. What about the connection with computers?

At the time when the LC and the combinatory calculi were being developed, one did not ask the fourth question. Computers had not yet been invented!

4. What about consistency? We observed earlier that the LC and the combinatory calculi have a weak consistency, in that one cannot prove that both of any pair of functions are equal to each other. Fairly early on (see Church and Rosser 1936 [PCn]; reproduced in Church 1941 [CLC]) a considerably stronger form of consistency was proved for the LC, embodied in the Church-Rosser Theorem:

Suppose X_o red X_1 and X_o red X_2. Then there is an X_3 such that both X_1 red X_3 and X_2 red X_3.

Here A red B signifies that there is a succession (possibly null) of β-reductions and α-steps that will take one from A to B. The Church-Rosser Theorem will be referred to hereafter as C-R-T.

A lambda-formula is said to be in normal form if it has no part on which one can perform a β-reduction . A lambda-formula X is said to have a normal form Y if Y is in normal form and one can get from X to Y by some succession of operations, each of which is a β-reduction, a β-expansion or an α-step. We can prove the following theorem.

If X has a normal form Y, then X red Y and Y is unique, except possibly for a few cosmetic α-steps.

One proves this by induction on the number of operations. The idea is as follows. Suppose one goes from X to W_1 by a β-expansion, next from W_1 to W_2 by a β-reduction, and finally from W_2 to Y by a second β-reduction. Then W_1 red X and W_1 red Y. So, by C-R-T, there is a W such that X red W and Y red W. But Y is in normal form, so that in the reduction from Y to W there cannot be any β-reductions; only α-steps. So, except for cosmetic uses of α-steps, Y is W, and we had X red W. For uniqueness, suppose Z is another normal form of X. Then it is also a normal form of Y. So Y red Z. But Y is in normal form. So the reduction from Y to Z can consist only of α-steps.

The lambda-formulas of interest mostly have normal forms. These normal forms constitute a foundation, on which is erected an elaborate superstructure. However, each normal form has its own individual superstructure, not overlapping the superstructures of the other normal forms.

Because formulas of the LC can be identified with formulas of a combinatory calculus and vice versa, there are superstructures in the combinatory calculus corresponding to those of the LC.

In (1.1) and (1.2), one can consider going from left to right as a reduction. So one can look for parallels to C-R-T, one can define normal forms, etc. There was much investigation of these questions.

The original proof of C-R-T was fairly long, and very complicated. In Newman 1942 [TCD], the point was made that the proof was basically topological. Newman generalized the universe of discourse, and defined a relation $\longrightarrow$ with properties similar to a β-reduction. He proved a result similar to C-R-T by topological arguments. Curry, in Curry 1952 [NPC], generalized the Newman result, with the intention that it would be relevant to similar considerations in the combinatory calculi. Unfortunately, it turned out that neither the Newman result or the Curry generalization entailed C-R-T in the intended systems because the systems did not satisfy the hypotheses of the key theorems. This was discovered by David E. Schroer, whose counterexample is recorded in Rosser 1956. In Schroer 1965 is derived still further generalizations of the Newman and Curry results, which indeed do entail C-R-T in assorted systems. As Schroer 1965 is 627 typed pages, this hardly contributes to the cause of shorter and simpler proofs of C-R-T.

Chapter 4 of Curry and Feys 1958 is devoted to a proof of C-R-T for the LC, and to related matters. It is not recommended for light reading. In Hindley 1969 and Hindley 1974 are discussions of proofs of C-R-T for the LC and systems closely related thereto.

These various proofs all stemmed generally from the Newman approach, with an emphasis on the topological structure. However, lambda-formulas and combinations have a marked, though specialized, tree structure. Mitschke 1973 used the tree properties a bit in deriving a proof of C-R-T. Rosen, in Rosen 1973, really went overboard. He worked with general trees, and relationships between them. As lots of things have a tree structure, his results have applications beyond proving C-R-T. He applies his results to the extended McCarthy calculus for recursive definition (see McCarthy 1960), and verifies a conjecture in Morris 1968. He also applies his results to tree transducers in syntax-directed compiling. With all that, the proof of C-R-T did not come easy. He had to prove C-R-T's for several related systems, and then derive the C-R-T for the LC by some trickery.

Meanwhile, a genuine simplification for the proof of C-R-T had come in sight. See Martin-Löf 1972. It is agreed that Martin-Löf got some of his ideas from lectures by W. Tait. An exposition of the proof of C-R-T according to Tait and Martin-Löf appears as Appendix 1 in Hindley, Lercher, and Seldin 1972. A shorter exposition appears on pp. 59-62 of Barendregt 1981. We will give what seems to us a still shorter and more perspicuous proof of C-R-T.

What seems to be the main difficulty of the proof? Let us look at the minimal case. Suppose X_o has two parts, $(\lambda xW_1)V_1$ and $(\lambda xW_2)V_2$. Let X_o red X_i by performing a λ-reduction on $(\lambda xW_i)V_i$, for $i = 1, 2$. If $(\lambda xW_1)V_1$ and $(\lambda xW_2)V_2$ reside in totally disjoint parts of X_o, there is no trouble. To get X_3 we perform a β-reduction on the $(\lambda xW_2)V_2$ that still resides in X_1 and on the $(\lambda xW_1)V_1$ that still resides in X_2.

Note that the reductions from X_1 to X_3 and from X_2 to X_3 each use exactly one β-reduction.

But suppose that $(\lambda xW_2)V_2$ is part of V_1. X_2 will contain $(\lambda xW_1)V_3$, where V_3 is the result of a β-reduction of $(\lambda xW_2)V_2$ inside V_1. As a candidate for X_3, we perform a β-reduction on the $(\lambda xW_1)V_3$ of X_2. How about getting from X_1 to X_3? Where X_o had $(\lambda xW_1)V_1$, X_1 will have $W_1[x:=V_1]$. If there had been only one free occurrence of x in W_1, then $W_1[x:=V_1]$ will contain a V_1; we change this to V_3 by a β-reduction on $(\lambda_2W_2)V_2$, and we have arrived at X_3. But W_1 may well contain several free occurrences of x. Then $W_1[x:=V_1]$ will contain several V_1's. We can go through, and change them one after another to V_3's, which will result in X_3. But there is no way we can get from X_1 to X_3 by a single β-reduction.

In the language of Barendregt 1981, p. 54, β-reduction does not have the diamond property.

The difficulty is that it may take several β-reductions to get from X_1 to X_3. This should have suggested working with a string of β-reductions, instead of only one. Why it took more than thirty years for this to occur to anyone is a mystery.

If we call a β-reduction or α-step a step, then a string of them will be a walk. But we cannot allow just any old string. What we are aiming for is that if X_o walk X_1 and X_o walk X_2, then there is an X_3 such that X_1 walk X_3 and X_2 walk X_3. If we put the right restrictions on the steps allowed in a walk, we can do this.

We frame our restrictions for a walk as follows.

1. A walk may contain no steps at all.

2. It may contain α-steps at will.

3. If a number of parts $(\lambda xW_i)V_i$ fail to overlap at all, the corresponding β-reductions may be done in any order.

4. If $(\lambda xW_1)V_1$ and $(\lambda yW_2)V_2$ are candidates for β-reductions in a walk, and $(\lambda yW_2)V_2$ occurs as a proper part of $(\lambda xW_1)V_1$, then the β-reduction of $(\lambda yW_2)V_2$ must precede that of $(\lambda xW_1)V_1$.

The relation $\twoheadrightarrow_1$ on p. 60 of Barendregt 1981 is likely closely related to our motion of a walk, but it is not exactly the same. For the key lemma, Barendregt uses something like induction on the number of steps from X_o to X_1 whereas we use induction on the number of symbols in X_o. This makes quite a difference.

Lemma. Suppose X walk Y. In X, change all bound variables by an α-step to a set of distinct variables that have no occurrences in X or P. This gives X^1, for which there is a Y^1 such that X^1 walk Y^1 by essentially the same β-reductions as were used for X walk Y. Then $X^1[x:=P]$ walk $Y^1\,[x:=P]$ by a completely parallel series of β-reductions.

Proof by induction on the number of steps used in X^1 walk Y^1. If there are none, it is obvious. So assume the hypothesis of the induction. Let us say that

X^1 has got down to Z^1, on the way to Y^1, and $X^1[x:=P]$ has got down to $Z^1[x:=P]$.

There will be no α-steps in going from X^1 to Y^1, since any necessity for them was disposed of in going from X to X^1. If there were, the conclusion would be obvious. So let the next step be from $(\lambda yW)V$ to $W[y:=V]$ in Z^1. If y is x, then there were no free occurrences of x in Z^1, so that Z^1 and $Z^1[x:=P]$ are identical, and no x's get changed to P from there on. With y different from x, the y's in W and the x's therein are quite disjoint. When the V's are put for the y's, the x's and the V's will be quite disjoint. So it does not matter if all x's are then replaced by P's, after the reduction, or before the reduction, as will happen when one makes the corresponding reduction in $Z^1[x:=P]$.

Note that this lemma is very nearly the same as proposition 2.1.17(i) on p. 28 of Barendregt 1981. It is also closely related to proposition 3.1.16 on p. 55. In Barendregt's terminology, our lemma says that a walk is substitutive.

Diamond Property. If X_o walk X_1 and X_o walk X_2, then there is an X_3 such that X_1 walk X_3 and X_2 walk X_3.

In other words, there is an X_3 which is the fourth vertex of the diamond, with a walk along each edge.

Proof by induction on the number of symbols in X_o.

Case 1. If X_o has a single symbol, it is immediate.

Case 2. Let X_o be λxM_o. Then X_i must be λxM_i for $i=1,2$. Clearly we have M_o walk M_i for $i = 1,2$. So there is a M_3 such that M_i walk M_3 for $i=1,2$. Take X_3 to be λxM_3.

Case 3. Let X_o be M_oN_o.

Subcase 1. X_i is M_iN_i with M_o walk M_i and N_o walk N_i, all for $i=1,2$. Then there are M_3 and N_3 with M_i walk M_3 and N_i walk N_3, both for $i=1,2$. Take $X_3 = M_3N_3$.

Subcase 2. M_o is λyW_o, X_1 is $W_1[y:=N_1]$, and X_2 is $(\lambda yW_2)N_2$. By restriction 4, the last β-reduction in X_o walk X_1 had to be from $(\lambda yW_1)N_1$. So we have W_o walk W_i and N_o walk N_i, both for $i=1,2$. Then there are W_3 and N_3 such that W_i walk W_3 and N_i walk N_3, both for $i=1,2$. Then we have X_2 walk $(\lambda yW_3)N_3$, and hence we take X_3 to be $W_3[y:=N_3]$. There are various N_1's in $W_1[y:=N_1]$, but they are non-overlapping. So we operate on each in turn, and have X_1 walk $W_1[y:=N_3]$. By our lemma, $W_1[y:=N_3]$ walk $W_3[y:=N_3]$, the latter being X_3, except for some α-steps. Now the steps we took in going from $W_1[y:=N_1]$ were all on N_1's that had been put for y's in W_1. So none of them could violate restriction 4 as we go on down to $W_3[y:=N_3]$ from $W_1[y:=N_3]$. So we can put these two walks together to conclude X_1 walk X_3.

Subcase 3. Like subcase 2, except with X_1 and X_2 interchanged. Make suitable interchanges in the proof of subcase 2.

Subcase 4. M_o is λyW_o, and X_i is $W_i[y:=N_i]$ for $i=1,2$. By restriction 4, the last β-reduction in X_o walk X_i had to be from $(\lambda yW_i)N_i$, both for $i=1,2$. So we have W_o walk W_i and N_o walk N_i, both for $i=1,2$. So there are W_3 and N_3 such that W_i walk W_3 and N_i walk N_3 both for $i=1,2$. There are various N_i's in $W_i[y:=N_i]$, but they are non-overlapping. So we operate on each in turn, and have X_i walk $W_i[y:=N_3]$, both for $i=1,2$. By our lemma, $W_i[y:=N_3]$ walk $W_3[y:=N_3]$, both for $i=1,2$, except for some α-steps . X_3 is $W_3[y:=N_3]$. To get from X_i down to X_3, we have to combine two walks, both for $i=1,2$, but the argument for this goes as in subcase 2.

Now we prove something that looks like C-R-T.

If X_o goes to X_i by a succession of walks, both for $i=1,2$, then there is an X_3 such that X_i goes to X_3 by a succession of walks, both for $i=1,2$.

The proof is so easy that, if we carry out the details for a special case, the whole thing becomes obvious. So, let X_o walk W_1 walk W_2 walk X_1 and X_o walk W_3 walk X_2. By the Diamond Property, we can fill in W_i's to be corners in Figure 1.

$$\begin{tikzcd}
 & & & X_o \arrow[dl, dash] \arrow[dr, dash] & & \\
 & & W_1 \arrow[dl, dash] \arrow[dr, dash] & & W_3 \arrow[dl, dash] \arrow[dr, dash] & \\
 & W_2 \arrow[dl, dash] \arrow[dr, dash] & & W_4 \arrow[dl, dash] \arrow[dr, dash] & & X_2 \arrow[dl, dash] \\
X_1 \arrow[dr, dash] & & W_5 \arrow[dl, dash] \arrow[dr, dash] & & W_7 \arrow[dl, dash] & \\
 & W_6 \arrow[dr, dash] & & W_8 \arrow[dl, dash] & & \\
 & & X_3 & & &
\end{tikzcd}$$

Figure 1

As each β-reduction or α-step taken alone is a walk, C-R-T follows by the previous result.

5. What about completeness? At first sight, it appears that the LC is so weak that it is absurd even to raise the question. However, as indicated in Church 1932 [SPF.I] and amplified in Kleene 1935 [TPI], the positive integers can be defined in the LC. If n is a positive integer, we let

$$\lambda f(\lambda x(f(f(\ldots(f(fx))\ldots)))),$$

where there are n f's, denote the integer n. This makes one form of recursive definition easy. If $F(n)$ is to be defined by

$$F(1) = GA \tag{5.1}$$

$$F(n+1) = G(Fn), \tag{5.2}$$

then we can take F to be

$$\lambda n(nGA). \tag{5.3}$$

With this definition,

$$F1 \ \text{red} \ GA$$

$$F2 \ \text{red} \ G(GA)$$

$$F3 \ \text{red} \ G(G(GA))$$

etc.

However, there is no zero in this system. One would prefer the recursive definition to be given by

$$F(1) = A \tag{5.4}$$

$$F(n+1) = G(Fn). \tag{5.5}$$

In Kleene 1936 [LDR], Kleene worked out a way to do this. This opened the door to still more general recursive definitions. More and more definitions of functions from integers to integers were discovered. Some never published investigations by Rosser disclosed so many that in about 1934 Church was led to conjecture that every effectively calculable function from positive integers to positive integers is definable in the LC. It was known from Church and Rosser 1936 [PCn] that every function from positive integers to positive integers that is definable in the LC is effectively calculable. So Church enunciated what is now known as "Church's Thesis."

Church's Thesis. Effectively calculable functions from positive integers to positive integers are just those definable in the LC.

As "effectively calculable" is an intuitive notion, Church's Thesis is not susceptible of proof. However, it states a strong, and quite unexpected, version of completeness.

In about this era, Gödel and Kleene were trying to get a definition for "general recursive function." Kleene gives a definition in Kleene 1936 [LDR]. He attributes it to Gödel. Gödel thought that general recursiveness should be taken as the criterion of effectively calculable. However, in Kleene 1936 [LDR] it is shown that general recursiveness is the same as being definable in the LC. This lent strong support to Church's Thesis.

Independently, Turing had been developing the abstract idea of a computer, the so-called "Turing machine." See Turing 1936 [CNA]. Turing thought that "effectively calculable" should be taken to be the same as calculable on a Turing machine. But in Turing 1937, he proved that that is the same as being definable in the LC. This result explains why the lambda-calculus and the combinatory calculi can (and do) play such an important role in the theory of computer programming, and such matters.

Independently, in Post 1936, Post had developed ideas very similar to those of Turing. Turing published first, by a very few months. Later, in Post 1943 [FRG], still another definition of "effectively calculable" was proposed, which turned out to be equivalent to those already given. Still later, in Markov 1951 [TA1], Markov gave yet another definition, which was also proved to be equivalent. A translation of this appears as Markov 1961.

With the development of actual computers, which are finite approximations for a universal Turing machine, interest in all these matters has been much intensified. In Kleene and Vesley 1965, on p. 3, the authors list 150 contributions to the subject by Oct. 15, 1963. By now there are far more.

I mentioned before that Kleene made great progress in the notions of recursion theory. This was important for computing. However, over more than forty years, Kleene has generalized and extended the ideas far beyond what is of use in computing. Already, in Rogers 1967, which is a book of nearly 500 pages devoted to the subject, the author says it is not intended to be comprehensive. In the summer of 1982, beginning on June 28, there was a three week meeting at Cornell of experts concentrating on recursion theory. Essentially none of the material discussed at the meeting would be of use in computing. And, though it still makes much use of notations from the LC, it really has diverged far from it. To get a notion of the subject matter for talks at the meeting, read "Recursive functionals and quantifiers of finite types revisited II," by S. C. Kleene. This is the first article in Barwise, Keisler, and Kunen 1980.

But, to get back to our original topic, Church's Thesis is by now not questioned by anyone. It certainly expresses an important, if unorthodox, version of completeness for the LC.

6. What about models? There is a classic theorem that says that, if a logic is consistent, it will have a model; indeed a denumerable one. However, the LC is so different in structure from the usual logics that the theorem does not apply to it.

Why does one wish a model? If one has a framework with a lot of structure, and the logic is isomorphic to some part of the framework, then the structure in the framework can contribute to your understanding of the logic. One can always manufacture a very superficial model by taking equivalence classes of objects in the logic. The only structure this has is what is forced on it by the logic itself. So no additional understanding can come from studying the structure of the model. Such a model does little good.

For a very long time, this was the only kind of model that was found for the LC. Finally, with help from Strachey, Dana Scott hit on a way of making some really useful models. They could be constructed either in the category of topological spaces or in the category of lattices. An exposition, "Outline of a mathematical theory of computation," appears in pp. 169-176 of the Proc. Fourth Annual Princeton Conf. on Information Sciences and Systems, 1970. In case this is inaccessible, another exposition appears as the final article in Engeler 1971. In Barendregt 1981 is given a model similar to the Scott one, but in a still more general framework, namely the category of complete partial orders. In one sense, this is good since one can derive still more properties of the LC in this more general category. However, suppose one would like just to see a model without having to learn all the algebra involved in topological spaces, lattices, or complete partial orders. Some people have been working in that direction, to get a model without all the algebraic baggage. This is mostly available only in unpublished material, such as Plotkin 1972 and Meyer 1982; the latter gives a fairly complete and coherent account. We give the Meyer model.

Start with a nonempty set, A; the unit class consisting of the ordered pair $\langle\phi,\phi\rangle$ will do, where ϕ is the null class. Enlarge A to the least set B containing A and all ordered pairs $\langle\beta,b\rangle$, where β is a finite subset of B and b is in B.

The model consists of all subsets of B. For two members, C and D, of the model, define

(6.1) $(CD) = \{b\varepsilon B \mid \langle\beta,b\rangle\varepsilon C \text{ and } \beta \subseteq D\}$.

To show that this contains a model of the combinatory calculus, we identify two elements K and S:

(6.2) $K = \{\langle\alpha,\langle\beta,b\rangle\rangle \mid b\varepsilon\alpha \text{ and } \alpha,\beta$ finite subsets of $B\}$

(6.3) $S = \{\langle\alpha,\langle\beta,\langle\gamma,b\rangle\rangle\rangle \mid b\varepsilon\alpha\gamma(\beta\gamma)$ and α, β, γ finite subsets of $B\}$.

One verifies fairly easily that

(6.4) $KCD = C$

(6.5) $SCDE = CE(DE)$

for all elements of the model. A close relative of the extensional property holds; see Meyer 1982, p. 19.

Of course, this model is nondenumerable. To get a model isomorphic with the Curry combinatory calculus, we restrict our attention to the objects built up solely out of K and S.

By the equivalence (developed in the thirties) between the LC and the combinatory calculi, one must be able to get a model of the LC from this. In any case, Meyer gives details.

7. What about the connection with computers? This proceeds in two directions. One can use computers to manipulate combinators or formulas of the LC, or one can use properties of combinators and the LC to help in programming or to develop ideas of use for computers.

Looking to the first, the obvious approach would be to represent the combinators, or formulas of the LC, as lists or arrays in the computer memory. In fact, these formulas are tree structures, and might better be represented so on the computer. Knowing the location of only the root of the tree then suffices to reconstruct the entire tree. So the trees (entire formulas) can be identified by single memory locations, instead of by elaborate diagrams or linearizations thereof.

The idea is very simple. Suppose A and B are combinators, and we have put their roots at memory locations a and b. Then we represent C = (AB) by locating its root at memory location c; in c we put the ordered pair of numbers a and b. The person who wishes to know the structure of C is told to look at location c. There he finds <a,b>, which tells him that C has the form (AB), and that to know the form of A he should look in location a, and similarly for B.

Besides the convenience in referring to a formula, this allows economies of memory which are not possible when a formula is represented by a list. For an extreme example, suppose B = ((AA)(AA)), where A requires 1000 memory locations for its representation. To represent B as a list would require four repetitions of the listing of A, together with attendant parentheses; a total of 4006 locations. With the tree representation, let A have its root at a; we may still suppose that the entire representation of A fills 1000 locations. At some convenient memory location, d, we put <a,a>, which denotes D = (AA). Then at another empty memory location, b, we put <d,d>, which denotes (DD). But (DD) is ((AA)(AA)) = B. Thus, with A represented in 1000 memory locations, we require only 1002 locations to represent ((AA)(AA)).

The generalization to representations of formulas of the LC is quite trivial.

These, and many related matters, are taken up in Petznick 1970. Consider a typical program on a computer, say for computing an approximation to the square root (two integers, a mantissa and an exponent). If one inputs an approximation for a real number (a mantissa and an exponent) the program will generate and output an approximation for the square root. So the program defines a function. Naturally, it is a computable function. So (by one of the equivalences supporting Church's thesis) this function must be expressible by means of a combinatory formula. If suitable hardware, or software simultions thereof, is available, the calculation can be done solely by combinatory manipulations.

Something of the sort had been proposed for lambda-formulas by Landin. See Landin 1965 or "A formal description of ALGOL 60," pp. 266-294 in Steel 1966. However, this involved him in a very difficult problem of handling the complicated substitutions properly. If he had used combinatory formulas instead, this problem would be much simplified. Also, Landin tried to superpose the lambda-formulas on top of the usual computer software. This produced a greatly complicated assignment problem. If one would dispense with the usual computer software, and work only with combinatory formulas stored in the memory (preferably as trees) the assignment problem would simply disappear.

Petznick's thesis, Petznick 1970, showed that it is possible to design a computer to work exclusively with combinatory formulas, stored as trees. There is no assignment problem, and application takes the place of substitution. As application is the basis of the tree structure, it is handled automatically. The hardware one would have to build to handle this would be quite simple. Or it can be handled with present hardware by a suitable software simulation.

To operate such a system requires considerable dexterity with combinators. And there are questions about the best way to handle numbers. So far, no one has tried to carry the matter past where Petznick left it.

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