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Reflections on Church's Thesis

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Over fifty years after I first heard Church propose his thesis, about which I have meanwhile often written, can I find anything more to say concerning it? I have been introduced to much of the recent literature in which Church's thesis is discussed by the excellent scholarly volume [29] of Judson Webb.[1] Its bibliography, which of course covers many topics besides Church's thesis, includes over 300 items, about half of them published since 1960. It is nevertheless not quite complete; thus Post [25], Markov [21] and [22], and Smullyan [26] are not listed, although they are devoted to expounding some of the newer equivalent versions of Church's thesis. Also, a new book from the Russian school has just appeared: Markov (posthumous) and Nagornyi [23].

It is a recurrent theme in Webb [29] that Gödel's (first) incompleteness theorem of [8] gave "protection" to Church's thesis; thus, if, contrary to the incompleteness theorem, a system F such as Gödel considered were complete (i.e., for each closed formula A, either $\vdash_F$ A or $\vdash_F \neg$A) and gave correct results (say, satisfied Gödel's hypothesis of ω-consistency), then in Kleene's effective enumeration (with repetitions) $\phi_0(x), \phi_1(x), \ldots, \phi_z(x), \ldots$ (where $\phi_z(x) = U(\mu y T_1(z,x,y))$ of all the 1-place partial recursive functions (including all the 1-place general recursive functions), we could effectively complete the definitions of all the functions which are not total (leaving those that are total unchanged) getting $\bar{\phi}_0(x), \bar{\phi}_1(x), \ldots, \bar{\phi}_z(x), \ldots$, by putting

$$\bar{\phi}_z(x) = \begin{cases} U(y) & \text{if } T_1(z,x,y), \\ 0 & \text{if } \vdash_F \forall \mathrm{y} \neg \mathrm{T}_1(\boldsymbol{z},\boldsymbol{x},\mathrm{y}). \end{cases}$$

That is, for given z and x, we search effectively through the numbers $y = 0,1,2,\ldots$ for the first one such that *either* $T_1(z,x,y)$ holds (on finding which we put $\bar{\phi}_z(x) = U(y)$) or y is the Gödel number of a proof in F of $\forall \mathrm{y} \neg \mathrm{T}_1(\boldsymbol{z},\boldsymbol{x},\mathrm{y})$ (on finding which we put $\bar{\phi}_z(x) = 0$).[2] Now by diagonalizing we would get $\bar{\phi}_x(x) + 1$ as an effective total 1-place function which is not general recursive,[3] contradicting Church's thesis. So, as Webb correctly stresses, if we hadn't the

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"protection" of Gödel's incompleteness theorem, it would have been foolhardly in 1936 to have proposed Church's thesis. For, by the argument just given,[4]

(1) {not-(Gödel's incompleteness theorem for F)} $\rightarrow$ {not-(Church's thesis)}.

Of course, if in 1936 mathematicians had been ignorant of Gödel's incompleteness theorem, one could have proposed Church's thesis and let it lead one to Gödel's theorem. The implication (1) is exactly the contrapositive of

(2) {Church's thesis} $\rightarrow$ {Gödel's incompleteness theorem for F}.

In fact, historically, beginning immediately after Church's thesis became public, Kleene (in his [13], and more simply in [15], [16]: cf. [17], Sections 60, 61) used Church's thesis to give proofs of Gödel's incompleteness theorem (in the positive form[4]), fulfilling this implication (2), for a large class of Fs, in fact generalizing Gödel's theorem to any formal system (maybe remote in its details from the ones Gödel talked about) meeting Hilbert's demand for effectiveness in the concept of proof and formalizing (with the correctness as hypothesis, just as in Gödel's statement) a small piece of elementary number theory. Indeed, Kleene's treatments each constituted a schema for establishing Gödel's incompleteness theorem independently of Church's thesis for any specific formal system (embodying the necessary small piece of elementary number theory) for which we have verified that being the Gödel number y of a proof is a general recursive predicate (the Gödel number of the formula of which it is a proof is then given by a primitive recursive function of y under Gödel's method of numbering) and that (e.g., for the [15] version) the Gödel number of the formula $\forall \mathrm{y} \neg \mathrm{T}_1(\boldsymbol{x},\boldsymbol{x},\mathrm{y})$ expressing $(y)\overline{T}_1(x,x,y)$ is a general recursive function of x.

One sometimes encounters statements asserting that Gödel's work laid the foundation for Church's and Turing's results, as for example in Webb [29], p. 26, lines 6–7. It seems to me that the truth is that Church's approach through λ-definability and Turing's through his machine concept had quite independent roots (motivations), and would have led them to their main results even if Gödel's paper [8] had not already appeared.

Church's formal system (which subsequently appeared in his [2], [3]), which had as a subsystem the λ-calculus, had already been presented in his logic course in the fall semester of 1931–1932 (in which I was a student), before Church and the rest of us first learned of Gödel's results [8] (through a lecture by von Neumann). So can there be any question but that the program of investigating the λ-definability of number-theoretic functions (which actually Kleene initiated, diverging from Church's start toward using descriptive definitions mixed with λ-definitions in developing the theory of positive integers in his system; cf. [19] pp. 55–56), the results of which led to Church's first entertaining his thesis, would not have gone ahead under its own momentum even if Gödel's [8] had not existed? The only question I see is whether, without the example in Gödel of the use of what we now call Gödel numbering,[5] Church would have thought of the arithmetization of the metamathematics of the λ-calculus which he used in applying his thesis in his [4] to obtain some undecidability results (what Webb calls unsolvable mass problems) in the theory of λ-definability,[6] continuing in [5] to establish the unsolvability of the famous Entscheidungsproblem for the first-order predicate calculus. Whether or not one judges that Church would have proceeded from his thesis to these results without his having been exposed to Gödel numbering,[7] it seems clear that Turing in [27] had his own train of thought, quite unalloyed by any input from Gödel.[8]

One is impressed by this in reading Turing [27] in detail. Already while an undergraduate at Cambridge University, which he entered in 1931, Turing

> started to build a machine for computing the Riemann Zeta-function, cutting the gears for it himself. His interest in computing led him to consider just what sorts of processes could be carried out by a machine: he described a 'universal' machine which, when supplied with suitable instructions, would imitate the behavior of any other; and he was thus able to give a precise definition of 'computable', and to show that there are mathematical problems whose solutions are not computable in this sense. The paper [27] which contains these results is typical of Turing's methods; starting from first principles, and using concrete illustrations, he builds up a general and abstract argument. (Robin O. Gandy in *Nature*, 18 September 1954, p. 535.)

> It was, perhaps, a defect of his qualities that he found it hard to accept the work of others, preferring to work things out for himself. (M. H. A. Newman in the *Manchester Guardian*, 11 June 1954.)

A given machine $\mathfrak{M}$ for Turing will have, say, R possible "machine configurations" $q_1, \ldots, q_R$, and m symbols $S_1, \ldots, S_m$, any one of which can be printed on a square of the tape (only finitely many squares being printed upon at each moment of time). In writing the standard description of $\mathfrak{M}$, Turing represents q_i by the letter "D" followed by the letter "A" repeated i times, and S_j by "D" followed by "C" repeated j times. In fact, he succeeds in writing his *standard descriptions* using only the six letters "A", "C", "D", "L", "R", "N" and the semicolon ";". Thereby, the inputs for a machine $\mathcal{E}$ to operate on the standard description of any machine $\mathfrak{M}$ are finite printings using symbols from a fixed finite list (of just 7) symbols. The standard description (S.D) of a machine $\mathfrak{M}$ becomes the arabic numeral for its *description number* (D.N) upon replacing the seven symbols by the digits "1", . . . ,"7". Thus Turing uses a totally different method of numbering linguistic objects than Gödel—one perhaps more natural to a person who is steeped in machines.

This should not at all disparage Gödel's achievement. "Kurt Gödel's achievement in modern logic is singular and monumental—indeed it is more than a monument, it is a landmark which will remain visible far in space and time" (von Neumann in the *New York Times*, March 15, 1951, p. 51). It only argues that Church and Turing had their own independent inspirations, leading them to equally significant results (the Church–Turing thesis, and their first examples of the unsolvability of some decision problems). Whether (and in what form) without Gödel's having already done it, they would have come via (2) (perhaps deduced from (1)) to the incompleteness of formal systems I hesitate to judge.[9]

Let me now address an argument reported in [29], p. 222 (after Wang [28], p. 325): "Gödel . . . objects that Turing 'completely disregards' that

(G) 'Mind, in its use, is not static, but constantly developing.'

. . . Gödel granted that

(F) The human computer is capable of only *finitely* many internal (mental) states.

holds '*at each stage* of the mind's development', but says that

(G)′ '. . . there is no reason why this number [of mental states] should not converge to infinity in the course of its development.' "

If one chooses to believe (G)′, I can see that it would imply that there need be no end to the possibilities for the human mind to invent stronger and stronger and stronger . . . formal systems that would, in the face of Gödel's ever-renewing incompleteness theorem, decide more and more and more . . . number-theoretic propositions (e.g., of the form $(Ey)T_1(x,x,y)$).

But I reject that (G)′ could have any bearing on what number-theoretic functions are effectively calculable. (Indeed, Webb so argues.) For, in the idea of "effective calculability" or of an "algorithm" as I understand it, it is essential that all of the infinitely many calculations, depending on what values of the independent variable(s) are used, are performable—determined in their whole potentially infinite totality of steps—by following a set of instructions fixed in advance of all the calculations. If the Turing machine representation is used,[10] this includes there being only a finite number of "internal machine configurations", corresponding to a finite number of a human computer's mental states. We are dealing with discrete objects (the arguments and the result included)—it is digital, not analog, computing. Maybe human thought as well as human sense perception can encompass a continuous infinity of qualities. But in digital computation we have abstracted from that continuity to deal with discrete mental states and objects. Indeed, Markov [22], Chapter I, Section 1, emphasized the abstraction by which we obtain *abstract letters* (what his algorithms work with) as equivalence classes of observed *concrete letters*. I think the like applies to instances of a given mental state, which is supposed always to lead to the same operation in a given abstract situation. As Turing [27] says in paragraph 6 of Section 9, "If we admitted an infinity of states of mind, some of them will be 'arbitrarily close' and will be confused." Hardly appropriate for keeping things straight digitally!

Let us have a try at making sense out of there being a potential infinity of states of mind by a human computer with an expanding mind in applying an algorithm. So I encounter Smarty Pants, who announces to me, "I can calculate the value of a certain function $\phi(x)$, for each value of x for which it is defined, by rules already fixed which will determine my every step, so that what function ϕ is is already determined. But *I can't tell you*, Wise Acre, *how*, because the rules have to tell how I will respond in each of an infinity of ultimately possible states of my expanding mind." I would reply, "Phooey! If you can't tell me what your method is, it isn't effective in my understanding of the term!" How can S.P. know about all those future states of his infinitely expanding—should I say exploding?—mind?

The notion of an "effective calculation procedure" or "algorithm" (for which I believe Church's thesis) involves its being possible to convey a complete description of the effective procedure or algorithm by a finite communication, in advance of performing computations in accordance with it. My version of the

Church–Turing thesis is thus the "*Public-Processes Version*" of Hofstadter [10], p. 562 (which is phrased there for deciding a total one-place number-theoretic predicate $P(x)$ rather than for calculating say a partial one-place number-theoretic function $\phi(x)$). Smarty Pants' method of calculating $\phi(x)$ (or deciding $P(x)$) must by the "*Proviso*" be one that "can be communicated reliably from one sentient being to another [of reasonable mathematical aptitude] by means of language", or I don't accept it as being effective and thus coming under the Church–Turing thesis.

What has Smarty Pants offered me except his say so that *he has* a method (which he is keeping secret from me)? How do I know that his claim is right that his method "always yields an answer within a finite amount of time", if we are dealing with a total predicate $P(x)$, or, if we are dealing with a partial function $\phi(x)$, for each x for which $\phi(x)$ is defined, "always . . . the same answer for a given number" as value of x? I could try to test S.P. by trying some values of x on him (and try to trap him by trying the same value sometimes a second or third time). But by observing his performance for any finite length of time (and observing that he always gives the same answer each time I repeat a value of x), I really won't ever finally know that his claim is right.

Particular objects in mathematics, such as predicates and functions, and methods for deciding or calculating an answer to each of a class of questions, are by my book ones that are fully specified for any mathematician to comprehend. Smarty Pants' supposed method (depending on the future statuses of his supposedly infinitely expanding mind), and the predicate or function which it is alleged to decide or calculate (unless he has given me another definition of that) are mystical objects, not mathematical objects.

As I wrote in [18], p. 337, our "idea of an algorithm is sufficiently real that in example after example . . . it separates cases when mathematicians agree that a given procedure constitutes an algorithm from cases when they agree that it does not." Historically, our idea of algorithms has involved their being procedures that mathematicians can discuss with one another.

Gödel continued from (G)′ above, saying, "Now there may exist systematic methods of accelerating, specializing, and uniquely determining this development, e.g., by asking the right questions on the basis of a mechanical procedure" (Wang [28], p. 325). If Gödel meant that we could uniquely *effectively* determine the development (including the responses the expanding human computer will make to each of a potential infinity of future (mental state)–(observed symbol) pairs), our *procedure* for doing this being of the sort that Turing conceives, then I think it is clear that the *combination* of *effectively determining* the infinite future expanding computer's mind and *applying it* would be an effective (finitely describable) procedure from the beginning, coming under the Church–Turing thesis. But of course, as Gödel admits, "the precise definition of a procedure of this kind would require a substantial deepening of our understanding of the basic operations of the mind". For the present I have to characterize what he is contemplating as pie in the sky. So far as I can predict, the pie will remain stratospheric.

In conclusion, I will recall that I was present at the Amsterdam Colloquium of 1957 when my good fried László Kalmár presented his argument against the plausibility of Church's thesis [11] (cf. Webb [29], p. 209); I immediately concluded, as fast as I heard it, that he had not given an effective procedure for deciding as to the truth or falsity of $(x)\overline{T}(n,n,x)$. He would not be able to tell me in advance in a finite communication (no matter how long we both should live) what set of atomic rules would completely govern the concrete steps in his search for proofs by "arbitrary correct means" of $(x)\overline{T}(n,n,x)$. I refrained from embarrassing him at the Colloquium by asking him for them on the spot.

NOTES

1. It is not true, as *seems* to be stated on p. 212 of Webb [29], that, *when* Kleene "undertook the detailed study of number theory in the λ-system" (the results of which he published in [12]), "Church had, in fact, already suspected that these 'λ-definable' functions might provide a good approximation of effectiveness". In fact, Church came to this suspicion from contemplating Kleene's results. Thus in January or early in February 1932 when Kleene showed Church his λ-definition of the predecessor function, Church acknowledged that he had just about convinced himself that there wasn't any ([19], pp. 56–57). For a full account of the origins of Church's thesis, etc., see Kleene [19] and Davis [7].

2. Exactly one of these will be found, assuming that F, which embodies a bit of primitive recursive number theory, is ω-consistent as well as complete. Thus if $(Ey)T_1(z,x,y)$, then simple consistency assures that not-$\{\vdash_F \forall y \neg T_1(\boldsymbol{z},\boldsymbol{x},y)\}$. If $(\overline{Ey})T_1(z,x,y)$, i.e., $(y)\overline{T}_1(z,x,y)$, then $(y)[\vdash_F \neg T_1(\boldsymbol{z},\boldsymbol{x},\boldsymbol{y})]$, so ω-consistency assures that not-$\{\vdash_F \neg\forall y \neg T_1(\boldsymbol{z},\boldsymbol{x},y)\}$, so by completeness $\vdash_F \forall y \neg T_1(\boldsymbol{z},\boldsymbol{x},y)$.

3. Of course, you and I know that (assuming the ω-consistency of F) we are arguing under a false assumption (that F is complete), so we should not be surprised to get strange results. In fact, from our assumptions of the ω-consistency and completeness of F, it follows that, for each z, the function $\bar{\phi}_z(x)$ is general recursive (indeed, $\bar{\phi}_z(x)$ is a general recursive function of z,x). We need only use the fact that, for each of the F's considered, $\{y$ is the Gödel number of a proof in F of $\forall y \neg T_1(\boldsymbol{z},\boldsymbol{x},y)\}$ is a general, in fact primitive, recursive predicate of z,x,y. So we have a contradiction to the result of Kleene [17], p. 324, example 1, that the partial recursive function $\mu y T_1(x,x,y)$ which is $\phi_{z_0}(x)$ for some z_0) is not potentially recursive (so $\bar{\phi}_{z_0}(x)$, which is $\epsilon y T_1(x,x,y)$, is not general recursive). In fact, we thus get a (new?) proof of the absurdity of the completeness of F if F is ω-consistent.

4. Actually we have just shown that, for F a formal system such as Gödel considered or the system of formal number theory of Kleene [17],

$$\{F \text{ is } \omega\text{-consistent and complete}\} \to \{\text{not-(Church's thesis)}\},$$

Thence by propositional logic, classical or intuitionistic (Kleene [17], *12, *12, *49b, *58b),

$$\{\text{not-(if } F \text{ is } \omega\text{-consistent, then } F \text{ is incomplete)}\} \to \{\text{not-(Church's thesis)}\},$$

which is (1) with a negative form of Gödel's incompleteness theorem (asserting the absurdity of completeness, rather than giving a specimen of an undecidable formula).

5. Compare Church's [4] footnote 8 (which I introduce with material paraphrased from the text): "[The equivalence of each of an important class of problems in the theory of well-formed formulas to a problem of elementary number theory obtain-

able by means of the Gödel representation] is merely a special case of the now familiar remark that, in view of the Gödel representation and the ideas associated with it, symbolic logic in general can be regarded, mathematically, as a branch of elementary number theory. This remark is essentially due to Hilbert (cf. for example, *Verhandlungen des dritten internationalen Mathematiker-Kongresses in Heidelberg*, 1904, p. 185; also Paul Bernays in *Die Naturwissenschaften*, vol. 10 (1922), pp. 97 and 98) but is most clearly formulated in terms of the Gödel representation".

6. Including the undecidability of the two equivalent problems stated in his footnote 23, "(1) to find an effective method of determining of any two formulas **A** and **B** whether **A** conv **B**, (2) to find an effective method of determining of any formula **C** whether it has a normal form, [which] were both proposed by Kleene to the author . . . about 1932", the positive solution of which (as Church remarks in his preceding text) would, as is clear from results of Kleene [12], entail the solution of "most of the familiar unsolved problems of elementary number theory" (cf. Church's Theorem XIX).

7. Church's paper [4] uses, besides the Gödel [8] numbering method, the Herbrand–Gödel concept of general recursive function from Gödel [9] (a bit modified after Kleene [13]). But Church remarks in footnote 3, "With the aid of methods of Kleene [12], the considerations of the present paper could, with comparatively slight modification, be carried through entirely in terms of λ-definability, without making use of the notion of recursiveness".

8. I consider the diagonal method, used by both Church and Turing, to have been so widely known since Cantor introduced it in [1] as not to have to be considered an input from Gödel (cf. the footnote at the beginning of Section 8 of Turing [27]).

9. Cf. the fourth paragraph in Section 11 of Turing [27].

   Of course, Gödel [9] set the stage for Kleene [13]; but, as with Church [4] (cf. this paper's note 7), Kleene's normal form theorem could have been established directly for λ-definability (cf. Kleene [19], p. 60, right column, lines 11–16). Kleene learned of primitive recursive functions (in the terminology introduced in Kleene [13]) from Gödel [8] (what Gödel called there simply "recursive" functions); but they were also available through a series of publications going back to Dedekind and culminating in Rósza Péter's writings. Kleene chose to offer his [13] to *Mathematische Annalen* because it had published Péter [24]. That elementary number theory with only $0, ', +, \cdot$ as individual and function symbols suffices for representing every primitive recursive function, so that it becomes an example of an F for the incompleteness results, comes by Gödel's use of the Chinese remainder theorem in proving his [8], Theorem VII (and I do not know another source for it).

10. It is perhaps not always noticed that Turing's detailed treatment in [27] of his machines used them for computing decimal expansions of real numbers. What he briefly indicates in the first paragraph of his Section 10 as "possibly" the "simplest" way of using his machines to compute (1-place) number-theoretic functions can't be used for partial functions that are not total. But Turing's brilliant analysis of the possibilities in the functioning of a mechanical or human computer are all adapted in Kleene [17], Chapter XIII, to apply to the computation of partial (including total) number-theoretic functions.

    The gains for the theory of effective procedures obtained by Kleene's introduction of *partial recursive functions* in [14] (separating the question of effectiveness from the questions for given arguments whether the function being computed is defined), particularly his "recursion theorem" (also in his [17], pp. 352–353) are brought out well in Webb ([29], pp. 214–219). (Kleene lectured on the use of his recursion theorem in analyzing von Neumann's self-reproducing automata, cited in Webb, p. 233, at the RAND Corporation in the summer of 1951.) Kleene similarly used his "first recursion theorem" ([17], p. 348) in his [20].

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