KREISEL'S CHURCH

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Contents

"Church's thesis has, within logic, a similar function to dogmas and doctrines within the Church. The faithful get excited at the cost of being ridiculous to outsiders."

In *Classical Recursion Theory* (Odifreddi [1989]) I dedicated 20 pages to a discussion of Church's Thesis. It was Kreisel (K. from now on) who alerted me at the subtleties of the subject, and at the insufficient treatments available in print. Suffering of a seemingly widespread illness, I understood only part of what he said or wrote; and even of what (I thought) I understood, I made use for my own purposes.[1] However, his name in boldface in a number of places in that discussion may have created an illusion that I was reporting on his views: making him uncomfortable, and others confused.

I intend to attempt such a report here, letting as much as possible K.'s original words and formulations speak for themselves; in the hope not of satisfying him (an obviously impossible task), but of dispelling such a confusion.

Since K.'s range of interests (even in the limited area of concern to this article) is quite wide, the reader is advised to browse among the various topics (perhaps with the help of the table of contents), looking for ones of his or her own interest.

1 General Remarks

Observations about general aspects of Church's Thesis are scattered in K.'s papers and reviews, especially in:

- *Analysis of the general concept of computation* (1.(c) and 4.(c) of [1971]),
- *Principal distinctions* (II.(a) of [1972]),
- *Church's Thesis and the ideal of Informal Rigour* ([1987]).

The following unstructured selection somehow reflects the occasional character of those observations.

[1]As a related example of how things may come to be used in a (purposely) distorted way, I quoted on p. 2 of my book a parallel that K. had made in §4 of [1985], between Euclid's *Book X* (which classified irrationals by means of a notion of degree) and the theory of Turing degrees.

His purpose was to draw attention to the fact that the shift from the classification of *Book X* to the measures of irrationality in modern number theory (based on diophantine approximations) required an absolute level of imagination, while nothing in (classical) Recursion Theory "approaches the philosophical detachment from the original set-up that was so essential for progress in the parallel from number theory".

My purpose was to claim that Recursion Theory is part of classical mathematics, and I thus presented the parallel as an item of the subject's pedigree, claiming that "degrees were used for the purpose of a classification of reals already in Euclid's *Book X*".

K. has whipped me more than once for "squandering a cute quotation".

1.1 Informal rigour

K. states in [1987] that Church's Thesis is a candidate for informal rigour, "a venerable [2000-year-old] ideal in the broad tradition of analysing precisely common notions or, as one sometimes says, notions implicit in common reasoning [at a given time]". He repeatedly warns us, at the end of [1987], about the obvious risks involved in any such enterprise; specifically:

- First, "there is no end in sight to the possibilities of coherent and imaginative analysis in the tradition of informal rigour". What is in doubt is the adequacy of the common notions to be analysed, not only to the phenomena for which they are intended, but even to our practical knowledge of them.

- Second, "even if the contributions [to informal rigour in general, and Church's Thesis in particular] were more central than they are, the market would be limited by the background knowledge needed for more than an illusion of understanding. It is a hallmark of philosophical questions that they present themselves to those of us who do not have such knowledge (and even as not requiring any)".

It is then of no surprise to know that K. sees work about Church's Thesis, and more generally informal rigour, as an answer to the question: How to talk in the face of ignorance? Specifically, an answer taught by philosophy to those not satisfied with the easiest answer: stay silent.

Be that as it may, the common notion to be analysed here is of course effective computability, and work around it is a candidate not only for the *pursuit* of the ideal of informal rigour, but also for the *examination* of the pursuit itself. In other words, not only to show that informal rigour can be *achieved*, but also to discover if and how it can be *used* when achieved: and "it's a sight more difficult to find any use for (the truth of) such a thesis than to decide its truth".

§2 of [1987] reminds us that there are two opposite principles in the foundational literature: on the one hand, "the words 'essence of computation' are a directive to look for *one* variant or, equivalently, to what is common to all [of them]"; on the other hand, "the familiar homily 'it all depends' (on situation, purposes, etc.) suggests the need for an endless array of variants". K.'s position, supported by experience in mathematics with relatively few so-called basic structures, is to look for "*relatively few* variants [that] could be adequate for *relatively many* situations".

1.2 Variants of Church's Thesis

K. proposed in 2.7 of [1965] to consider the variants of Church's Thesis obtained by specifying 'effectively calculable' as: mechanically, constructively, humanly, and physically realizable.

As noted in 2.(c).(iii).(β) of [1966], "at the time [of Church], one would have been prepared to regard *effective, intuitionistic, constructive, mechanical, formal* as equivalent when applied to rules! After all, less than ten years before Church formulated his thesis, von Neumann and Herbrand took it for granted that *finitist* and *intuitionistic* had the same meaning! But, perhaps, it would be historically more correct not to call it Church's Thesis; for, once alerted to the difference between intuitionistic and mechanical rules, he would surely have formulated the thesis for the latter".

[1972] however points out that "it seems safe to say that the *sensational* aura around references in popular philosophy to Turing's analysis or to Church's Thesis reflect the - conscious or unconscious - assumption that the humanly effective, not only the mechanically effective definitions are in question".

In any case, 2.715 of [1965] states that explicit support for Church's Thesis exists only in the case of mechanically computable functions. Precisely, it "consists above all in the analysis of machine-like behavior and in a number of closure conditions, for example diagonalization", for which K. refers to the discussion in Kleene [1952].

1.3 Equivalent characterizations of recursiveness

In 2.715 of [1965] one finds the first statement of a point that K. will often repeat,[2] asserting that support for Church's Thesis is *not* to be found in empirical evidence such as the equivalence of different characterizations of recursiveness: "what excludes the case of a *systematic* error?". For comparison, he quotes "the overwhelming empirical support for: if an arithmetical identity is provable at all, it is provable in classical first order arithmetic; they all overlook the principle involved in, for example, consistency proofs".

In 1.(c).(i) of [1971] K. stresses the fact that "the mathematics has here much the same role as in the natural sciences: to state rival hypotheses and to help one deduce from them a consequence, an *experimentum crucis* which distinguishes between them; one will try to avoid artefacts and systematic error. Equivalence results do not play a special role, simply because one good reason is better than 20 bad ones, which may be all equivalent because of systematic error". A note adds that "the familiar emphasis on stability or equivalence results is not rooted in some kind of 'common sense', but in a positivistic philosophy of research which rejects the objectivity of [informal rigour]. An equivalence result allows one to act in accordance with this doctrine without formally adopting it: the result allows one to evade the issue (for the time being)".

In II.(a).(v) of [1972] the mantra is chanted again: "equivalence of different notions such as definability in λ-calculus or by Post rules is often said to provide evidence: *evidence for what?* Such equivalences may indeed provide evidence

[2] Such repetitions were obviously needed, and should be contrasted with the repetitions of the argument of equivalence in practically every textbook of Recursion Theory, till this day.

of *some* interest. But they cannot provide evidence for equivalence to a notion which is *not* among those considered! And if the intended notion is explicitly *included* among the notions considered then there is no need for equivalence proofs - on the principle that one good reason is better than 20 bad ones".

Finally, in 1.2.1 of [1990a]: "the conventional 'evidence' neglects safeguards against systematic errors, and thus the axiom of experimental science (derived from experience, not only doctrine) that the most insidious errors are not at all random, but systematic".

1.3.1 A shift of emphasis

§3 of [1987], while recalling "the curious 'evidence' provided by equivalence between various definitions, as if not every notion had many definitions", introduces a new twist: "those totally absorbed in pursuing such equivalences do not ask whether the schemes are all equally sensible or equally silly. Less obviously, they do not ask whether the details that are left out in the matching are significant; enough to make one scheme practically superior, at least occasionally".

As adumbrated in the last quotation, in recent years K.'s criticism has taken a positive side too, well expressed in 3.(b).(ii) of [1987a]: "the drivel about evidence for Church's Thesis obscures a genuine virtue of having many equivalent definitions or, more simply, descriptions of the same notion (whether or not they define the originally intended matter). When solving problems about the notion, use can be made of knowledge of the different concepts involved in those descriptions. ... It is an object of research to *discover* which description suits particular problems, even though it may well be that other descriptions tend to force themselves on us".

This shift of emphasis (from what equivalent descriptions have in common to their different potentials) is repeated in 1.2.1 of [1990a]: "those different schemes are not viewed as different, so-called informal analyses of a familiar notion, but are simply the logical aspects of different (mechanical) processors".

1.4 Church's Superthesis

Mere equivalence of characterizations hides a neglected aspect, for which K. introduced in 4.(c).(i) of [1971] a special name. He called Church's *Superthesis* a stronger version of Church's Thesis, in which one not only claims that certain mathematical tasks are *equivalent* to recursive ones, but rather that each such task is *equal* to some program for an idealized computer: "to each mechanical rule or algorithm is assigned a more or less specific programme, modulo trivial conversions, which can be seen to define the same computation *process* as the rule".

K. has been attentive to positive evidence for the superthesis. In particular:

- In [1972] he notes that Turing's analysis of the notion of computability does establish a version of the superthesis, for the notion of mechanically computable function.

- In [1972a] and §4 of [1987] he reports on work by Barendregt,[3] establishing the superthesis for: reduction of terms in the λ-calculus, execution of programs by Turing machines, and evaluations according to the computation diagrams for partial recursive functions given by Kleene. Thus "not only the classes of functions defined by the different familiar schemes are equal, but the definitions themselves match so as to preserve computation steps".

§5 of [1987] stresses the value of the emphasis on the superthesis in another direction. "Common sense says: If you want to find out about things, for example, processes, don't hide them in black boxes! Try to look at them. Specifically, in connection with a refutation of Church's Thesis, don't rely on the off-chance of some process being grossly non-mechanical; so much that not even its effects that strike the eye, the so-called output, can be computed mechanically from the input".

1.5 Turing's analysis

§3 of [1987] recalls that among the equivalent characterizations of recursiveness, the one in terms of Turing machines has a particular intrinsic value: "Turing's description of computations, by the rules of his universal machine, is so vivid that it would establish a common notion together with its elementary properties even if it were not present before. ... This constituted essential progress for informal rigour, and is not changed by the many defects of the notion", some of which are at issue here.

As noted in II.(a).(i) of [1972], the distinction between mechanically and humanly computable functions was clearly presupposed in Turing's attempt [1936] to establish that "a machine can reproduce all steps that a human computer can perform".[4]

However, Gödel [1972] noticed a problem in the details of Turing's assumptions about distinguishable states of mind (which, however, does not invalidate his analysis of *mechanical* instructions). Precisely, Turing proposed a compactness argument to establish that the number of such states is finite, but according to Gödel he disregarded the fact that mind develops, and thus that such number (though finite at any given moment) may tend to infinity. Gödel's remark is

[3] In the supplementary Part II of his unpublished dissertation, briefly discussed on p. 43 of the second edition of his book [1981].

[4] In II.(b).(ii) of [1972] K. notes how Turing's introduction of progressions of formal systems (called by him 'ordinal logics') may be taken as showing that he did not take his own claim too seriously.

accepted by K. in II.(a).(i) of [1972], Note 4.(c).(ii) of [1987a], and Appendix I of [1990].

In II.(a) of [1972] K. finds that "an even more important error in Turing's argument consists in a kind of *petitio principii* assuming that the basic relations between (finite) codes of mental states must themselves be mechanical". While "in the case of (Turing) machines whose states are finite spatio-temporal configurations it is quite clear how to code states by natural numbers, ... coding (mental) states of the human computer is a much more delicate matter". In particular, "even if we assume a coding by *finite* configurations, ... what is the *arithmetic* character of the relation (between codes) induced by meaningful relations between the mental states considered"?

In a word, the problem here is that "the human computations are more 'complicated' or, better, more abstract than the objects on which they operate (our thoughts may be more complicated than the objects thought about)". In contrast, "the mechanical computations and their arguments are on a par".

Acording to Note 4.(c).(iii) of [1987a], Gödel's view on K.'s objection that the coding operators may be non-recursive, "was that we know so little about the details that only very simple assumtpions can be convincing. But here, in contrast to his reaction to other, apparently comparable cases, ... he rejected the thought that we may know too little for *anything* convincing".

1.6 Perfect fluids vs. perfect computers

In §3 of [1987] K. reminds us that in the 19th century "not only geometric notions [such as area] were analysed with informal rigour, but also those belonging to the aptly named subject of rational mechanics, with notions of uneven scientific value, including the notoriously imperfect notion of perfect liquid".

Experience with such a notion was considered in §2 of [1985] as an object lesson, not to be forgotten in the context of perfect computers: "progress was made by shifts of emphasis away from the original context. The two dimensional motion of such liquids provides a valid description of - not merely, as is sometimes said, a metaphor for - the notion of function of complex variable. The latter is firmly established in mathematics, even used in parts of mathematical physics, but just not primarily in successful hydrodynamics".

The exceptionality of the idea of perfect fluid is spelled out by K. in unpublished notes of 1989:

- "First, by and large it is a very imperfect idealization compared to, say, celestial mechanics of the planets. There is no area of familiar experience of fluids where the neglected aspects - viscosity, compressibility, turbulence, etc. - are absent to a comparable degree as, say, friction and air resistance in outer space.
- Secondly, the mathematical properties of that idealization belong to function theory, one of the jewels of mathematics. Specifically, the potentials

> and stream lines of these ideal motions are simply - given by - the real parts of functions of one complex variable.
>
> The same theorem is both one of the most useful mathematical tools and, applied to the idealization, one of its severest limitations. It is Cauchy's Theorem (on the vanishing of integrals round closed curves), which implies, for such ideal flow, that a stream does not exert any drag of any cylinder".

In the words of §3 of [1987], the example of perfect fluids shows that, when a common notion has been shown to have a mathematical equivalent by means of informal rigour, "there is the possibility of *discovering* other areas, in pure mathematics or its applications, where the mathematical equivalent is suited to describing the facts".

Tacitly, in the case of recursiveness 'other areas' means ones not directly connected to computability.

1.6.1 A first success: Higman's theorem

§3 of [1987] states that, "as is (or should be) well known, the prototype of such a discovery is Higman's answer to the question: Which finitely generated groups can be embedded in finitely presented groups? It is given in recursion-theoretic terms,[5] and is a model of evidence for the use of a notion to tell us what we want to know about (groups)".

§6 adds that "a mere corollary to [Higman's] positive answer is a finitely presented group with unsolvable word problem; in other words, something of concern to Church's Thesis. So Higman's answer shifts attention away from the latter. The answer, in terms of recursiveness, is *tested* by its contribution to the demands of group theory; not primarily by the validity of Church's Thesis in any of its versions".

1.7 Infinitistic character of recursiveness

K. has noted in recent years that an obstacle to simple-minded applications of recursion theory to problems such as those discussed below (especially in Sections 3 and 4) lies in the infinitistic character of the notion of recursiveness, which makes all finite sequences of numbers automatically recursive.

This was adumbrated in §6 of [1987], where he noted that "it is generally assumed that there [is no experimental consequence of the existence of irrational numbers], and it seems very plausible that there is no single measurement that could be interpreted to establish irrationality; or rationality, for that matter. For the record, I am not persuaded that (ir)rationality results have no experimental implications at all. ... Be that as it may, problems of similar flavour come up

[5] Namely: exactly the recursively presented ones, i.e. those for which the set of words equal to 1 is recursively enumerable, are embeddable.

with the two demarcations, between rational/irrational and computable/non-computable".

The suggestion becomes explicit in Appendix I.3 of [1990]: "a problem comes from the ordinary separation between observational knowledge and its theoretical interpretation(s): on the one hand, data of the observational kind are (hereditarily) finitely described; on the other hand, any such (necessarily finite) set of data is recursive. Evidently, only the most coarse-minded would conclude from this that the mathematical property [of being recursive] is without *any* scientific significance. An obvious question is: where, if anywhere, is such a significance? In other words, recursiveness is an *infinitistic* property, and so its interpretation is more demanding (in imagination)".

The point is reiterated in [1992]: "It is well known that *infinitesimal* properties like irrationality or (repeated) differentiability have no place in so-called phenomenological interpretations, that is those that strike the naked eye. Now, computability in the logical sense is quite coarsely *infinitistic*: every finite sequence of (hereditarily finite) data is computable in that sense. This does not exclude a physically suitable interpretation, for example, by reference to some appropriate micro-theory, but this matter is demanding".

For direction and comparison, K. refers in Appendix I.3 of [1990] to experience with partial differential equations, where "conditions on solutions being once or twice differentiable are, often demonstrably, mathematically significant; most simply, for admitting or excluding a particular P.D.E. as (even) a candidate for a theory of (the aspects of) the phenomena considered"; for example, as noted in [1982], "the most visible features of many phenomena obeying the wave equation, such as caustics (images) in optics, occur only with weak solutions", i.e. their second derivative exists but is not continuous. "But, again, every observational set of data is consistent with those conditions and also with their negation".

2 Constructive Mathematics

Church's Thesis for constructive mathematics was discussed by K. in:

- *Church's Thesis* (2.7 of [1965]),
- *Church's Thesis: a kind of reducibility axiom for constructive mathematics* ([1970]),
- *Church's Thesis for effective definitions of number theoretic functions* (Part II of [1972]),
- *Laws of thought: this side of the pale* (§5 of [1987]).

In its simplest setting, it amounts to saying that "every constructive number theoretic function has an equivalent definition by means of a certain kind of computation procedure".[6]

The reason to consider constructive mathematics is recalled in §5 of [1987]: "originally Church's Thesis was intended and understood in the sense [that] effectiveness for the ideal mathematician was meant. The recursive undecidability results were advertized under the slogan: what mathematicians cannot do. ... In view of how little is known about the outer limits of mathematical imagination, Church's Thesis in its original sense is simply beyond the pale. If anything remotely like it is to be pursued, some shift of emphasis is required, ... and the intuitionistic variant presents itself at least as a candidate. ... The link with the common notion in question is the meaning of intuitionistic logic as originally explained by Brouwer and Heyting: in terms of mental constructions (of the ideal mathematician)".

2.1 Mechanical and constructive rules

K. notes in §1 of [1970] that there is an issue here, since constructive and mechanical are not equivalent: "it is almost banal that we understand non-mechanical rules; on the contrary too detailed, that is 'too' mechanical rules only confuse the human computer".

This is sharpened in §7 of [1987]: "everyday experience of creative and mechanical thinking shows that the former is simply more congenial to us, less prone to errors, and accordingly more reliable; but also (perhaps disappointingly) the latter can be more efficient. Thus, a modern computer sums $\sum_{1\leq n\leq 100} n$ more quickly - not more reliably - by routine addition than Gauss did at the age of 6 by use of a bright idea. (Computers do *mechanical* work more reliably than people.)"

In 2.35 of [1965], elaborated in §1 and §2 of [1970], K. goes as far as proposing the following as a specific example of a constructive but apparently non-mechanical function.[7] Given a constructively valid formal system $\mathcal{F}$ for arithmetic, constructively enumerate its proofs, and associate to n:

- 0 if either the conclusion of the n-th proof is not an existential assertion, or it is but the proof does not provide an explicit witness for it;
- $m + 1$ if the conclusion of the n-th proof is an existential assertion, and the proof provides m as an explicit witness for it.

Transformation of this (obviously constructive) function into an equivalent mechanical one encounters a number of obstacles: $\mathcal{F}$ does not necessarily have the

[6]Extensions to functions of higher types are also considered in [1965], with the role of the recursive functions variously played by Kreisel's and Kleene's continuous functionals, Gödel's primitive recursive functionals, and Kleene's recursive functionals. A different extension, to partial functions, is considered in [1972]. We don't discuss these extensions here.

[7]Note 9 of [1970] criticizes Kalmar [1959], an inconclusive paper whose title had attracted some attention, and that claimed to contain an example with similar properties.

so-called *Constructive* $\exists$-*Rule* (if an existential assertion is provable, then so is some of its numerical instances); even if it does, $\mathcal{F}$ does not necessarily admit recursive procedures that associate numerical witnesses to provable existential assertions; even if it does, the problem still remains of knowing whether one of such procedures (which are not all necessarily equivalent, in the sense of providing the same witnesses for the same provable existential assertions, unless the system has the so-called $\exists$-*Stability*[8]) is equivalent to the function above, and if so which one.[9]

The main characteristic of this example is isolated in II.(a) of [1972] as "the passage between a formal derivation ... and the corresponding mental act, namely the proof expressed by the derivation". In particular, because of this reference to mental acts the definition above is not even meaningful from a set-theoretical standpoint!

The example was shown in 1.(b) of [1971a] to be mechanically computable (for a large class of formal systems, including Heyting's Arithmetic) by use of normalization techniques. But the mere fact that relatively advanced work was needed to answer the question establishes that the latter was genuinely problematic.

2.2 Formal versions of Church's Thesis

In 2.72 of [1965] K. proposes two formal versions of Church's Thesis, for systems for constructive mathematics:

CT1 $\forall f \exists e \forall x \exists z [T_1(e,x,z) \wedge f(x) = U(z)]$

CT2 $\forall x \exists y R(x,y) \ \rightarrow\ \exists e \forall x \exists z [T_1(e,x,z) \wedge R(x, U(z))]$.

The former, suitable for second-order systems with functional variables (for lawlike functions), says directly (via the Normal Form Theorem) that every function is recursive. The latter, suitable also for first-order systems, expresses (via the axiom of choice, extracting a function from a $\forall\exists$ form) the fact that every function is recursive.

Naturally, there is no question of *provability* of CT1 or CT2 in usual systems for constructive mathematics, since in them the corresponding classical systems are interpretable, and in the latter CT1 and CT2 are false.

[8]Warning: the term 'stability' in this context does not fit the usual meaning of stability w.r.t. (small) changes of data, and is instead applied to changes in interpretation.

This kind of stability is made insignificant, from a proof-theoretical point of view, by the kind of instability (this time in the usual meaning of the word) discovered by Girard's in proofs of a theorem by Van der Waerden, and reported in his book [1986].

[9]An example of a system with both the Constructive $\exists$-Rule and $\exists$-Stability is Heyting's Arithmetic.

2.3 Consistency of Church's Thesis

2.723 of [1965] states that both CT1 and CT2 are *consistent* with the systems for constructive mathematics considered there (intended for treatments of free choice sequences, generalized inductive definitions, and bar recursion), as well as with those in Kleene [1952]. This consistency result is extended in Kreisel and Troelstra [1970] to the theory of species of natural numbers (an intuitionistic analogue of classical analysis with the comprehension axiom and the axiom of dependent choices), and hence to a number of theories (including the ones just quoted) that can be modelled in it.

As Note 9 of [1970] states, "the main purpose of consistency results is to help avoid fruitless lines of research, since our principal interest is the refutation of Church's Thesis": "consistency results exclude even a 'weak' refutation in the sense of showing the absurdity of a proof, not only a 'strong' one in the sense of exhibiting a counterexample" (e.g. of the kind considered in 2.1 above).

In the opposite direction, II.(c).(ii).(β) of [1972] notices that an inconsistency of CT1 or CT2 "would only mean the absurdity of assuming the existence of a proof, and it would not establish a counterexample". In other words, inconsistency would only show that Church's Thesis cannot be proved by methods in the system considered, but it would fall short of providing an example of a constructive function that is not recursive.

2.4 Validity of Church's Thesis

The question of *validity* of CT1 and CT2 is posed in 2.75 of [1965], with the remark that "there is no reason why the question should not be decidable [in the negative] by means of evident axioms about constructive functions", whose discovery is described as "one of the really important open problems" (and, in 2.(c).(iii).(β) of [1966], "one of the more feasible problems at the present time").

Obviously, one is not thinking here of axioms *stated* in the language of constructive mathematics, but justified by explicitly non-constructive, or otherwise arbitrary interpretations. Here are three examples:

- Spector's *bar recursive functionals.*

  2.(c).(iii).(β) of [1966] states, and 2.b.(iii) of [1971a] proves, that they are inconsistent with Church's Thesis. But, "because of excessive extensionality conditions imposed on them, the contradiction is of little interest".

- Various notions of *choice or lawless sequence* α.

  As noted in 4.b of [1970], all known such notions naturally satisfy the negation of Church's Thesis: "for dice α (or lawless sequences) you don't expect to prove that successive values of α will follow a recursive, or for that matter, any law".

- Brouwer's *thinking subject.*

This is an analysis of mathematics into ω stages, and states that every set of natural numbers is constructively enumerable (over the natural numbers). The application one has in mind here is to the set of all (numbers coding) constructive proofs, and hence to the possibility of enumerating such proofs constructively in an ω-ordering.

As noted in 4.c of [1970], "the thinking (freely creating) subject will not convince himself that his (mathematical) behaviour is subject to any law".

The assumption of the thinking subject is actually provably inconsistent with Church's Thesis.[10]

2.5 Church's Thesis as a reducibility axiom

K. points out in 2.74 of [1965] how the consistency results quoted above show that Church's Thesis plays a somewhat similar role in intuitionistic mathematics as Gödel's constructible sets in set theory:[11] "not only is consistent with the known axioms, but it can also be used to show the *formal* character of interesting open questions".

This is quoted not as a mere possibility in principle, but with explicit examples: in particular, the result that "the rules of intuitionistic predicate logic cannot be proved complete [w.r.t. the intended semantics] by any method consistent with Church's Thesis" (a result sketched in [1962] and 2.741 of [1965], and fully proved in Technical Note I of [1970][12]). In particular, as noted in §3 of [1970], this shows that "the notion of constructive validity of first- order formulas depends on problematic properties of the basic notion of constructive function (like second-order validity, but unlike first-order validity in the classical case)".

The parallel with set theory is explored in [1970] and supplemented in (d) of [1971b], where K. compares:

- on the one hand: the abstract notion of set, its basic properties described by the Zermelo-Fraenkel axioms, Gödel's constructible model, the assumption $V = L$, and a non-axiomatizability result for infinitary predicate calculus following from it;

[10] The idea of the proof, due to Kripke and reported in Note 10 of [1970], is the following. If Church's Thesis holds, every constructively enumerable set is recursively enumerable. But in constructive mathematics one can show that there is a set which is not recursively enumerable (for example, the usual complement of the Halting Problem). One thus has a counterexample to the thinking subject assumption.

[11] Incidentally, as noted in II.(a).(i) of [1972], the constructible sets were proposed by Gödel as an analysis of humanly effective definitions (and the letter 'L' stood for 'lawlike'). Later Gödel expanded the analysis to the notion of ordinal-definable sets.

[12] The idea of the proof is the following. In [1962] K. had proved that constructive completeness of predicate logic implies (actually, is equivalent to) a constructive version of König's Lemma. Consider an infinite recursive (hence, constructive) tree with no infinite recursive branch: if König's Lemma holds constructively, such a tree has an infinite constructive branch, which cannot be recursive. One thus has a counterexample to Church's Thesis.

- on the other hand: the abstract notion of constructive arithmetical function, its basic properties described by Heyting's axioms, Kleene's realizability model, the assumption of Church's Thesis, and the non-axiomatizability result for intuitionistic predicate calculus following from it, and quoted just above.

In this context, axioms refuting Church's Thesis would play a role similar to set-theoretical axioms (such as the existence of measurable cardinals) contradicting $V = L$.

2.6 Church's Rule

For systems for which (consistency of) Church's Thesis is not known to hold, or it actually fails, one can restrict attention to (consistency of) closure under Church's Rule, i.e. the assertion that if the premise of CT2 is provable then so is the conclusion.

Technical Note II of [1970] warns that Church's Rule is genuinely problematic: even if CT2 is valid, provability of the premise implies only validity of the conclusion, not necessarily its provability (because of incompleteness of usual systems).

As noted in 2.7231 of [1965], the first *consistency* result of closure under Church's Rule was obtained by Kleene for his system in [1952] (a result strengthened in 2.723 of [1965] to consistency of Church's Thesis).

Closure under Church's Rule of the theory of species of natural numbers without choice was proved in Technical Note II of [1970]. The result was extended to the theory with choice in Kreisel and Troelstra [1970] (a result supplemented there by consistency of Church's Thesis), while the proof was simplified in 2.a.(ii) of [1971a], and (b) of [1971b].

In general, II.(c) of [1972] showed that every sound formal system satisfying the Constructive ∃-Rule (quoted in 2.1 above) is closed under Church's Rule.[13] This implies that for a *refutation* of Church's Rule one can only look at systems that either are not formal or do not satisfy the Constructive ∃-Rule. III.3 of [1974] points out that the insistence on considering formal systems satisfying the Constructive ∃-Rule was a systematic error that precluded the possibility of disproving closure under Church's Rule. This is balanced by §5 of [1987], where it is noted that, however, "there are no rewarding candidates of systems in sight that can be established with informal rigour to hold for the constructions of the ideal mathematician, but do not have both the two properties [of being formal and satisfying the constructive ∃-Rule]".[14]

[13]The proof is the following. If $\forall x \exists y R(x,y)$ is provable, let n be given: then $\exists y R(n,y)$ is provable, and by the Constructive ∃-Rule so is $R(n,m)$ for some m. But the system is formal, and by enumerating its theorems one can find (one such) m. This defines a recursive function f such that, for every n, $R(n, f(n))$ is provable.

[14]Specially concocted intuitionistic formal systems not satisfying the Constructive ∃-Rule do exist by the incompleteness theorems: an explicit example is given in II.(c) of [1972].

The assessment of results about Church's Rule is a delicate matter, discussed in II.(c).(ii).(α) of [1972]: *closure* under the rule refers to provability in the system, and thus if true is significant only for systems complete for constructive mathematics (for which however Church's Thesis would hold), and if false is merely a symptom of incompleteness; on the other hand, *inconsistency* would instead disprove Church's Thesis.

3 Theories of Mathematical Reasoning

The possibility of a theory of mathematical reasoning was touched upon by K. in:

- *Mechanistic theories of reasoning* (§4 of [1966]),
- *Genetic theories of effective definitions* (II.(b) of [1972]).

As a general point, the end of §4 of [1966] states that "the use of technically advanced machinery in analysing reasoning is encouraging; after all, Aristotle thought about reasoning; one would like to see clearly what one has that he did not have! (It is no comfort to know that over 2000 years have passed since his time unless one sees just *how* one has used the experience of these 2000 years.)"

3.1 Individual reasoning

In discussing formalist rules of reasoning, K. notes that (in the terminology introduced in 1.4) what is at stake here is the superthesis for (mathematical) reasoning. According to §5 of [1987], "the pioneers, in particular Frege, had of course a lot to say about the distinction between thought processes and their results. He called the latter 'objective' thoughts, ... [and] saw a principal use of his objective analysis (ignoring subjective processes) in the greater security it gave to common reasoning".

However, Note 31 of [1965] remarks that "the conviction, probably, is not merely that such rules happen to generate the provable statements in a particular domain of mathematics, but that ... this is really all that goes on". K. proposes a parallel with the early days of chemistry, where "one did not merely mean that the particular integral ratios in chemical reactions happen to be *formally* explained by an atomic hypothesis, but that there were such things as atoms". And notices that the attraction of formalism "derives at least partly from this: long before electronic computers were thought of, one could see more or less how behavior according to such formal rules could be realized by a *mechanism*, that is an old fashioned mechanism in the sense of a Turing machine".

This is reiterated in 2.(a).(i) of [1966]: "Probably the major attraction of formalization was that it suggested the possibility of a mechanistic theory of human reasoning, in particular, that [mathematical] propositions not only can be decided by means of formal rules, but that something like repeating application of such rules is all that goes on even if we consciously think of reasoning differently; more precisely, that the higher nervous system consists of a mechanism whose behaviour is given by the formal rules, as an electronic computer is a mechanism whose physical behaviour realizes certain mechanical laws (the 'instructions' which it is given)".

4.(a).(i) of [1966] notices however that "it is remarkable how little work was done on this even in areas, such as predicate logic, where the set of valid statements is recursively enumerable. The least one would have to do is to show that there is something mechanical about the *actual* choice of proofs, not only about the set of results".

The point is taken up again in 4.(c).(i) of [1971], where the notion of superthesis is applied to "Frege's empirical analysis of logical validity in terms of his formal rules; the superthesis would then correspond to an assignment of specific deductions, modulo trivial conversions, to intuitive logical proofs. Here ... the theorem proved does not determine the process, that is the proof (*a fortiori*, not the formal description of the process); in fact, not even in propositional logic: thus we have at least two obviously different proofs of the theorem $(p \wedge \neg p) \rightarrow (p \rightarrow p)$, one using $p \rightarrow p$ and $q \rightarrow (r \rightarrow q)$ with $q = p \rightarrow p$ and $r = p \wedge \neg p$, the other using $(p \wedge \neg p) \rightarrow s$ with $s = p \rightarrow p$".

K. notes there that what is now called the Curry-Howard isomorphism (between derivations in intuitionistic calculi and terms in corresponding typed λ-calculi) provides an example of work in the direction of the superthesis in the sense just discussed.

3.2 Collective reasoning

The cooperative phenomenon (in the language of statistical mechanics) of the mathematical community, and its behavior with respect to arithmetic problems, is considered in Note 29 of [1965], and in 4.(a).(ii) of [1966]: "This behavior seems asymptotically stable. We certainly have no *better* theory at present than this: a statement will be accepted if true". Now this theory is certainly not recursive and hence not mechanistic, "but the whole issue is whether reasoning is mechanistic, and so it is a *petitio principii* to require that only mechanistic theories of reasoning are admitted".

In II.(b).(i) of [1972] a shift from truth to provability is made: the possibility of "an all-encompassing formal system $\mathcal{F}$ for the whole of mathematics (or even the part dealing with number theoretic predicates)" is considered, and it is noted that such a formal system would establish Church's Thesis for humanly realizable functions.[15] Equivalently, any example of a humanly realizable, non-

[15]A technical result sketched in [1971c] and proved in Part I of [1972] shows that the same would hold in the weaker hypothesis that "mathematical reasoning is encompassed not by a single formal system, but by a recursive progression on a Π^1_1 path through Kleene's $\mathcal{O}$". See

recursive function would refute the possibility of such a system.

II.(b).(i).(α) shows that Gödel's incompleteness results prove not the impossibility of such a system, but only that we cannot have mathematical evidence of its adequacy.

II.(b).(i).(γ) discusses empirical, non-mathematical evidence, in particular stability of actual practice: systems such as *Principia Mathematica* are as adequate for number theory or analysis today as they were in the 19th century. K. notices that knowledge of Gödel's incompleteness proof would, at least naively, be expected to spoil the adequacy of such systems, but in practice it does not.[16]

3.3 Mind

According to the end of [1980], "throughout his life Gödel looked for good reasons which would justify the most spectacular conclusion that has been drawn from his first incompleteness theorem: minds are not (Turing) machines. In other words, ... the laws of thought are not mechanical (that is, cannot be programmed even on an idealized computer)".

As stressed there, "the popular reasons are quite inconclusive. Certainly, by (Matyasevic's improvement of) the incompleteness theorem, those minds which can settle all diophantine problems are not machines; but we have not found any evidence of such minds. Nor there is the slightest hint of any computer programs which simulate (even in outline) actual proof search; not even for solving problems which do have a mechanical decision procedure (for example, propositional algebra)".

In §4 of [1966] K. proposes a variant to a favourite twist of Gödel's, brought up in conversation: "either there are mathematical objects external to ourselves or they are our own constructions and mind is not mechanical".

The variant differs from Gödel's formulation in two respects: first, K. makes no assumption that "if mathematical objects are our own constructions we must be expected to be able to decide all their properties";[17] second, he would "like to use an abstract proof of the non-mechanical nature of mind ... for the specific purpose of examining particular biological theories".

For the latter purpose, granted a negative result about the mechanical nature of mind, 4.(a).(iii) of [1966] points out that one needs to make specific

Note 4.(b).(iii) of [1987a] for an account of Gödel's role in prompting this result.

[16]He intriguingly remarks that also "knowledge of Freud's interpretation of the dream symbolism would be expected to produce new symbols (to deceive the superego) but, apparently, it does not".

[17]K. has a gift for provocative comparisons, and displays it in this case by adding: "I do not see why one should expect so much more control over one's mental products than over one's bodily products - which are sometimes quite surprising" and, as added at the end of [1980], "can have painfully unexpected properties". According to the last source, "Gödel remained unsympathetic to [this] admittedly tasteless comparison".

As another example, in §8 of [1985] he attacks the "blithe talk about 'natural' notions" by reminding one of "the obvious parallel from botany where perfectly natural and often pretty mushrooms can be addictive or poisonous in other ways".

assumptions about such theories (in addition to the general ones stated below, at the end of 4.1). In particular: "mathematical behavior is regarded as an integral part of the experience to be explained, and not as some corner far removed from the principal activities of the organism" (an assumption whose rejection implies an acceptance of the division between mental and 'ordinary' biological phenomena), and it is "to be explained in terms of the basic laws themselves"; moreover, "the basic laws are such that the laws for co-operative phenomena, i.e. interaction of organisms such as involved in mutual teaching of mathematics, are also recursive".

K. proposes the following as a debating point: "compare the place of mathematical behaviour among biological phenomena to the place of astronomical behaviour among mechanical phenomena; the former is far removed from ordinary life, exceptionally predictable, exceptional both in the sense that the predictions are precise, and also that they were the first to be noted; since astronomical phenomena played an important part in building up physical theories, should one not expect the analogue too?"

As to the role of Gödel's incompleteness theorem, K. states in 4.(b) of [1966] that he does not think that the result "establishes the non-mechanistic character of mathematical activity even under [the assumptions] above without [Gödel's own] assumption that we can decide all properties of our (mental) productions. For, what it establishes is the non-mechanistic character of the laws satisfied by, for instance, the natural numbers: and the theory of the behavior of arithmeticians mentioned above may well be wrong!"

Actually, there is the possibility that "the natural tendency of mathematicians to be finitist or predicativist is significant for the psycho-physical nature of reasoning". And if finitism or predicativism turned out to be the correct description of the behavior of finitist or predicativist mathematicians, one could actually mention problems which neither can decide.

4 Physical Theories

The question of whether physically realizable functions are recursive was first raised by K. in 2.714 of [1965], and discussed in:

- *Mechanism and materialism* (4.(d) of [1966]),
- *Analogue versus Turing computers* (§3 of [1970a]),
- *A notion of mechanistic theory* ([1974]),
- *Theories in natural science: rational and computable laws* (§6 of [1987]).

On the positive side, II.(a).(v) of [1972] states that the (important and neglected) empirical evidence provided the fact that a large class of patently nonmechanical functions turn out to be equivalent to recursive ones should be taken as a sign of the importance of the notion of recursive function.

On the negative side, K. notes in 4.(d) of [1966] and at the end of [1980] that the possibility of physically realizable but non-recursive functions shows on the one hand that "the hypothesis that reasoning is not mechanistic is by no means anti-materialist or anti-physicalist", and suggests on the other hand the possibility that "the notion of machine is not adequate 'in principle' to separate mind and matter".

To avoid misunderstanding, [1974] stresses the fact that it is not *phenomena*, but *theories* about them that are considered here. Accordingly, K. defines a theory as mechanistic if "every sequence of natural numbers or every real number which is well defined (observable) *according to theory* is recursive or, more generally, recursive in the data (which, according to the theory, determine the observations considered)".

As a corollary to this position, "the reader should not allow himself to be confused ... by doubts about the validity of a theory with regard to the phenomena for which it is intended", although obviously "such doubts imply doubts about the relevance (to those phenomena) of any results about the mechanistic character of the theory".

4.1 Positive results

The obvious starting point for the search of mechanistic theories is, as the name implies, classical *mechanics*. In 2.714 of [1965] K. noticed that "(excepting collisions as in the 3-body problem, which introduce discontinuities) the theory of partial differential equations shows that the behavior of discretely described (finite) systems of classical mechanics is recursive".[18]

This is reiterated in Note 2 of [1970], where the possibility of "(finitely specified) physical systems whose most probable behavior is non-recursive" is reconsidered, and the fact that "the theory of partial differential equations gives a negative answer for a general class of systems in classical mechanics" restated (this time, with the comment that "the result is not trivial since we are dealing with the mechanics of the continua and Turing machines are discrete mechanisms").

In §4 (Footnote 1) of [1966] attention is shifted to *probabilistic processes*, and a proof is given of the fact that "if in a stochastic process (with a finite number of states) the transition probabilities are recursive, any sequence of states with non-zero probability is automatically recursive".[19] This is sharpened in 3.(c) of [1970a], where it is shown that the result "can be extended to stochastic processes with an *infinite* number of discrete states and a recursive table of

[18] 'Discrete' means that *all* relevant parameters take discrete values, not only that the systems are finite.

[19] The idea of the proof is the following. If the transition table is recursive, the tree of all possible sequences of states is recursive. If a sequence of states has non-zero probability, it is an isolated branch in such a tree. And any isolated branch of a finitely branching recursive tree is recursive.

transition probabilities".[20]

Finally, in §4 of [1966] K. touches on *biological processes*, and claims that "the stable macroscopic properties of organisms would be expected to be recursive" if, as currently assumed, biological theories will be general schemas for the explanation of biological processes, based on "combinatorial basic steps iterated a (large) number of times" (a characteristic of recursive processes).

This is supplemented in §5 of [1987], where it is noted that "such characteristic aims of the logical tradition as unity by reduction to a few primitives may be misplaced here. Thoughtful biologists are sensitive to those aims, and tell us that they are not compatible with the process of evolution. It selects from a mass of random mutations those specific elements that are adapted to the surroundings in which they happen to be. Quite simply, the process doesn't have a logical feel, and so the laws could not expected to have such a feel either. At most, somewhere on the molecular level the laws might satisfy the idea(l)s of the logical tradition, though often they do not".

4.2 Evaluation of positive results

The result quoted in 4.1 provide *empirical* evidence for the mechanistic character of existing physical theories. III.3 of [1974] draws a parallel with the fact that by the end of the twenties "the huge bulk of the mathematical problems that were regarded as solved had formal, that is, mechanically computable, solutions", and that "even today we do not have any theorem in ordinary number-theoretic practices which cannot be proved in *Principia Mathematica*". Nevertheless, "the non- mechanistic nature of the axiomatic theory of natural numbers was discovered, not by sifting existing applications which accumulated in the course of nature (here: in number-theoretic practice) but by looking for unusual or neglected applications (here: to metamathematical questions): applications specifically chosen for their relevance to questions of mechanization or, equivalently, formalization". This is, according to K., "the lesson to be learned from the experience with axiomatic theories of mathematical objects; for use with our present problem concerning the mechanistic character of (other) scientific theories". The points in 4.3 below have been raised throughout the years with this explicit lesson in mind.

4.3 Where to look for negative results

4.(a).(iii) of [1966] notices that, unlike discrete classical systems, *co-operative phenomena* are not known to have recursive behavior.

[20] One has to use an appropriate definition of 'sequence of states with non-zero probability' to prove that the latter is recursive, since an argument as in note 19 would only prove it is hyperarithmetic.

Note 2 of [1970] hints at the possible non-recursiveness of a collision problem related to the *3-body problem*,[21] and suggests as a possible consequence "an analog computation of a non-recursive function by repeating collision experiments sufficiently often". A more explicit discussion of this example is in IV.2 of [1974].[22]

But, as stated in 2.714 of [1965], the natural place where to look for non-recursive behavior is "the *quantum theory*, for example, of large molecules". Here are two examples proposed by K.:

- 4.(d) of [1966] notes that "it is not known whether there exists a physical system with a Hamiltonian H such that, for instance, $\sigma(n)$ is the set of possible spins in the n-th energy state, $\sigma(n)$ finite for each n, and $\sigma(n)$ is not a recursive function of n".

- 3.(a) of [1970a] suggests the possibility of "large molecules whose spectrum (or: to have a dimensionless quantity, the ratio of the first spectral line to the second) is not recursive".

However, K. seems to have reached a negative impression about these examples, and in 3.(a) of [1970a] conjectures that "Kato's theorem could be used to give arbitrarily close recursive approximations".[23]

[21]The critical question is whether or not the masses collide (during the interval of time considered). If they don't, it is obvious that their paths can be computed as precisely as one wants. A physically meaningful formulation of the computability of this matter of collision must then refer not to points in phase space (in other words, precise positions and velocities), but neighbourhoods.

More precisely, one does not ask for a recursive decision procedure to determine whether, for arbitrary times t, a collision occurs exactly at time t (or $\leq t$). Rather, one asks for such a procedure to determine, for arbitrary t, an interval $(t - t_0, t + t_0)$ with small t_0 (possibly depending on t), such that one of the following happens: either there is no collision before $t - t_0$, or there is a collision at some time after $t + t_0$.

[22]K. states in [1976] that the formulation of this problem in [1974] is "distinctly better" than in [1970]. Judgments of this sort, both positive and negative, abound in K.'s (a bit schizophrenic) self-reviews, and the following choice may give a flavour.

In [1971b] he describes the arguments and formulations of [1974] as "unnecessary, ... inconclusive ... and practically useless". He mocks himself, by noticing that "the author, who does not usually avoid self-reference, forgets to quote [one of] his own observation[s]", and that "the author's objections ... seem to the reviewer much stronger that the author can have realized".

In [1971c] he complains that "the title [of [1970a]] is misleading", and that "the discussion trails off feebly instead of referring to the relevant literature". Moreover, "the author [amazingly] fails to stress what is perhaps the most obvious relevant contribution", and "is unsympathetic to his own subject". However, he "has a number of very interesting concrete suggestions".

In [1972a] he depicts the discussions in [1971] as "hesitant and discoursive", and the explanations as "incompetent but convincing".

Finally, in [1976] he plays on his double role by noticing that "the author gives no reference ... and the reviewer does not know any reference either".

[23]Kato's theorem provides upper and lower bounds for arbitrary Schroedinger's equations.

A different example, using sequences of eigenvalues, has been suggested by Pour El and Richards [1989]: the example has the flavour of a Hamiltonian, but does not seem to satisfy the conditions of Schroedinger's equation (in particular, Kato's theorem may not apply).

4.4 Physical relevance

In [1974], K. considers the step from obtaining constants by empirical (usually approximate) measurements to calculating them theoretically, and suggests the possibility that this extension of the notion of physical theories is "liable to introduce non-mechanistic elements in a perhaps non altogether trivial way".

But K. notes that the exhibition of a problem without recursive solutions would not be enough to show the non-mechanistic character of (related) physical theories. Further work would be needed: in particular, "it would be necessary to describe (an ensemble of) experiments and their statistical analysis for which the most probable outcome of the experiments is determined by the solution to the problem. In other words, if the problem has no recursive solution the most probable outcome of the experiments should be non-recursive too".

4.4.1 The wave equation

§IV of [1974] discusses examples of non-recursive objects with a physical look, but without physical relevance: for example, recursively continuous curves which do not attain their maximum at any recursive point.

A step forward in this direction was made by Pour El and Richards [1981]: they proved that for certain choices of recursive data (initial conditions) the wave equation has a unique, but not recursive solution.

K.'s review [1982] discusses this result: on the positive side, the *equation* itself is provided by current physical theory; on the negative side, the *data* are not (known to be) generated by recursively describable phenomena.[24]

This criticism is reiterated in [1992]: "naturally, some of the operators considered appear in theoretical physics. But not all their formal properties have a physically sensible interpretation!"

Thus the further work quoted above, needed to step from mathematical to physical relevance, is still lacking.

4.4.2 Hadamard's principle

IV.2 of [1974] discusses Hadamard's principle, restricting the class of meaningful (physical) theories to ones providing functions *continuous* in their data. In

[24]In passing, K. suggests in [1982] a possible shift of emphasis from recursiveness to subrecursiveness: "the realistic potential of (suitable) analogue computers for cheaply and reliably solving problems that are costly for Turing machines, is at least as significant as that of doing a recursively unsolvable job" (at issue here).

particular, K. suggests a refinement, requiring functions to be *recursive* in their data, and presents the collision problem (related to the 3-body problem) quoted in 4.3 as an example satisfying Hadamard's principle, but not known (even today) to satisfy this refinement.

In Note 4.(a).(ii) of [1987a] K. says that he had actually thought for some time that even the refinement had been a tacit assumption for people working on problems in mathematical physics; forcing them, as a consequence, to miss non-recursive solutions.

The work of Pour El and Richards [1981] discussed above provided a refutation to this impression, and showed in particular that this had not been the case with Kirchhoff's solution of the wave equation.

Nevertheless, "non-recursive solutions are often indeed unsatisfactory as they stand. But, once recognized, they may be explicitly excluded for physical reasons, ... or they may suggest new questions that have more manageable solutions".

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