Notre Dame Journal of Formal Logic
Volume 28, Number 4, October 1987

Church's Thesis and the Ideal of Informal Rigour

GEORG KREISEL

Introduction IR, short for 'informal rigour', is a venerable ideal in the broad tradition of analysing precisely common notions or, as one sometime says, notions implicit in common reasoning. CT, short for 'Church's thesis', concerns the common notion of effective computability, and is thus a candidate for IR.

As with other ideals and, more generally, with other aims there are two, possibly alternating stages in work on IR: first, the possibilities of *pursuing* IR, and secondly, of *examining* the pursuit, that is, its contribution to the broad area of knowledge to which the notions in question belong. Part of the examination consists in finding proper measures for the contribution, which, at least generally, are not given as part of the data.

There is a good deal of literature on these matters, including odd doctrines about some kind of logical impossibility of pursuit or examination or both; but also about their central place for knowledge. (What else do we have to start with but common notions?) Such frustrated and frustrating antics are not uncommon when there is simply nothing at all rewarding to be said at the level of generality at which the ideas involved are usually discussed. Being specific is no panacea either since counterexamples are liable to evaporate with simple distinctions.

What, if anything, is to be done with such ideas? Occasionally they are best relegated to those foolish things we must learn to forget. But often there is an alternative that has been successful in the scientific tradition, at least when a body of knowledge has accumulated that is, or can be, more or less closely related to some of the ideas in question, and has enough consequences for judging its significance (in contrast to the 'specific' counterexamples alluded to above). Experience shows that interpretations of such knowledge in terms of those ideas occasionally reveal not only new aspects of it, but also more rewarding levels of generality for the ideas. A familiar directive for this kind of investigation is: *dégager les hypothèses utiles.* Since a lot of work has been done around CT it is a candidate for use in examinations of IR too.

It may have escaped the reader's notice that the last paragraph is less innocent than it sounds.[1] For one thing the directive conflicts with the pious hope that *results will speak for themselves*, that is, without any explicit formulation of conditions, the so-called hypotheses. On the other hand, at least as I understand the directive here, it is a matter of experience to *discover whether* the literary forms of mathematical logic or other parts of mathematics provide more effective formulations. Alternatives are metaphors, commentaries, or any of the many other devices available for saying memorably what can be shown only dimly, or vice versa. (*Bon mots* play here the role of theorems in mathematics.) This appeal to experience in choosing the style of formulations is of course in conflict with so-called empiricist doctrines of what 'should' be effective or, for that matter, clear, precise, and what have you.

The material below is organized as follows. First, there are some simple reminders about the master assumption of the tradition mentioned at the outset, and the malaise about it at different levels of sophistication. Readers accustomed to formal expositions should think of those reminders as counterparts of generalities at the beginning of, say, group theory, about left hand and right hand inverses. And if the reminders are a little longer, they are also not quite so trivial as those generalities.

Secondly, there are (more detailed) reminders about CT, and especially about IR applied within mainstream mathematics, serving as background to a more thorough discussion of CT.

The following three sections concern three variants of CT under self-explanatory headings, at opposite extremes as it were.

The final section reviews what has, I think properly, been called the cultural value of work on CT. It is a particular case of a broad view on logical foundations that seems widespread, even if it does not have a catchy name.

1 Background: the master assumption implicit in the undiluted logical tradition(s) concerning common notions that have been sanctified as 'fundamental' The assumption is that provided only such a notion can be made *precise without arbitrary conventions*, the rest will follow automatically. The 'rest' includes the understanding needed to recognize whether and in what way the precise definitions of the notions in question contribute to solving problems that present themselves in the area to which the notions belong. Viewed this way, generality enhances a notion because it eliminates niggling worries about the exact extent of that area.

As already mentioned, the notion of effective rule or, more exactly, in-principle-effective rule, is regarded as fundamental in one tradition. But let there be no mistake. The master assumption operates equally in *would-be competing* traditions; for example, when the separation P/NP replaces recursive/rec. enumerable or, in a different vein, Kolmogorov's (universal) machine replaces Turing's.

There is very widespread malaise about the logical tradition, sometimes described as 'prejudice against logic'. However, the malaise has been expressed most unconvincingly; as somebody said, in barks and grunts.[2] Here are two samples, of somewhat different flavour.

First, there is the superstition that no precise relation can be established between mathematical and nonmathematical (common) notions. There are two weaknesses. The trivial one is the obvious conflict with the 2000-year old tradition in mathematics of defining common geometric notions, from the *circle* to *convexity* in the last century (and more besides; cf. the section on IR below). Less obvious, but more consequential, is that by denying the very possibility of correct and precise definitions, one draws attention away from the more demanding second part of the master assumption—in line with the truism that dubious doubts obscure genuine problems.

Secondly, there is that particularly coarse vulgarity, called 'pragmatism', 'identifying' Truth and Use, preferably, in the social sphere. Almost the opposite is at issue here:

Far from involving any such identification, the (genuine) problem about the master assumption is *whether mere truth is enough*: enough for the additional understanding that, on the master assumption, comes for free with the truth of those precisely formulated theses, dubbed 'fundamental' in the foundational literature. More brashly, the worry is that, at least in general, it's a sight more difficult to find any use for (the truth of) such a thesis than to decide its truth. This is meant in contrast to (hypo)theses in some mature branch of knowledge, which have been established to be significant by consequences that one wants to know about, in short, by uses; for example, the Riemann hypothesis in number theory.

Viewed this way, the word 'pragmatism' could be used for the philosophical doubt above about the *nature of the problems that are most prominent in academic philosophy*. The doubt is familiar from the contemporary philosophical literature (and not even particularly brash compared to expressions like 'bewitchment' or 'mathematics without foundations'). The material below goes further on two scores: First, exceptions, indicated by the words 'in general' in the brash formulation above, are pinpointed. Secondly, it is examined to what extent the formulation applies in a *particular area*—here, CT—*when all available knowledge is taken into account.*

2 Background (ctd.): preliminaries on CT, used here generically for various relations in the literature between the mathematical notion of recursiveness and the common notion(s) of effective computability Originally, numerical computations were meant, now also on abstract (data) structures. More significantly, the relations differ with respect to the idea(lization)s of the systems for which the computation rules are to be effective.

The modern terminology 'data processing' is better than 'computation' since it names the parameters that are neglected in CT, which is concerned with logical ($\forall\exists$) aspects, and thus ignores specifics of the *data* processed, and the structure of the *processors*. Among the latter are not only the digital or, equivalently, discrete electronic kind, but also (at least, according to classical theory, generally nondiscrete) analogue computers, and of course ourselves, the most familiar computers (albeit without any convincing theory of the wetware, as the brain is called in computer jargon). Presumably, our data processing involves both discrete and other elements since some of the information processed is more permanent, and attempts to follow mechanical rules are more prone to error than would seem likely from wholly nondiscrete, respectively wholly discrete processors.

Remark: Accordingly, some of the (most satisfactory) work reported below is better described by the old-fashioned idiom of computability in a *finite number* of blatantly unspecified *steps* than by the more usual 'effective computability'.

There is a great deal of literature on the idea of the perfect digital computer, which will therefore only be touched below. Instead, some neglected variants of CT are gone over, mainly in Sections 4–6, naturally with frequent reminders of problems which are or are not sensitive to the details of the processors considered.

Warnings of conflicts with the literature: Below, similarities between CT and other work in the tradition of IR are stressed. Some of the most gushing passages in the subject do the opposite; for example, von Neumann saw the exceptional contribution of Gödel's incompleteness theorem, obviously pertinent to CT, in its allegedly unheard-of *generality*. Yet the humble idea of *continuous* function of a real variable is not an iota less general, and useful to boot. An exception to this raving is René Thom's *aperçu* about a single differential equation sometimes having more content than a general logical notion. This is neatly illustrated by comparing Kleene's familiar normal form $U[\mu_y T(e,x,y)]$ in recursion theory with the manifold of solutions to, say, Laplace's equation, especially when the parameters—domain and initial conditions—are restricted (only) in the now usual ways.

In a different vein, the interest here in a *few variants* of CT conflicts with a couple of cherished principles in the foundational literature, at opposite ends of the scale as it were. Thus, the words 'essence of computation' are a directive to look for one variant or, equivalently, to what is common to all those considered here. On the other hand, the familiar homily 'it all depends' (on situations, purposes, etc.) suggests the need for an endless array of variants, illustrated for example in endless distinctions in certain parts of so-called analytic philosophy. Admittedly, there is apparently no guarantee that

relatively few variants could be adequate for *relatively many* situations

that present themselves. But experience in mathematics with relatively few so-called basic structures—and even with *privileged descriptions* of them (in the usual axioms)—is encouraging, tacitly provided discretion is exercised by remembering exceptions.

3 Background (ctd.): the rise and decline of IR in mathematics As mentioned in the introduction, IR is involved in the 2000-year-old tradition of analysing common notions in mathematical terms (current at a given time). In particular, definitions and other properties of such notions are established in rigorous ways.[3] When this way consists of inspection by the mind's eye, one speaks of 'axioms' in the old sense of this word, now called 'informal'. When this way consists of formal deduction from established knowledge, one speaks of formal rigour.

Taken literally, the domain of notions that are 'common' varies in space and time (and other dimensions). The idea is that there is a hard core; cf. the expression 'natural language' where, for example, literally most natural metaphors or kenningars are ignored. In any case, until about 50 years ago the tradition in question flourished. Occasionally a new word was used for a common notion, for example 'Riemann integral' for 'area under the curve $y = f(x)$' (at least, if $f(x) \geq 0$ in the interval considered so that 'under' makes sense). Here the properties *monotonicity* and *additivity* of the common notion of area are established by the kind of inspection mentioned, and then formally shown to determine the measure of this area (for suitable f).

In the 19th century this kind of rigour was cultivated in elegant, though by now half-forgotten, representation theorems. Incidentally, not only geometric notions were analysed with IR, 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.

During the last century this style receded, as a glance at the mathematical literature shows. Instead of common words, tacitly with their common meanings, like 'convexity', 'dimension', etc., neologisms were introduced like 'homotopy', or words without any meaning at all, like 'well-behaved'. Certainly, for this kind of word there can be no question of establishing definitions to be *correct* (in the sense of IR)! CT has the glitter of unheard-of originality (like the generality at the end of Section 2), only if viewed against this recent background, which became dominant just about the time when CT was formulated. It pays to pause a moment, and review the literature in the light of what has just been said, even if such matters are too much intertwined to speak of cause and effect.

If the possibility of establishing CT is discounted, and if, nevertheless, evidence is to be provided for CT, it is perfectly fitting to have lax standards; recall the curious 'evidence' allegedly provided by equivalences between various definitions, as if not every notion had many definitions. All the gushing about natural or simple definitions distracts from the *new* problem of evidence when IR is rejected (or, more generally, no longer regarded as adequate):

> To find evidence for the *use* of a notion or even of a particular definition (= description) of it.

As already stressed in connection with the master assumption in Section 1, this matter is much more demanding than mere IR.

The last 50 years of mathematics Put abstractly—and therefore simply, but also superficially—work on previously dominant common notions turned out to reach *points of diminishing returns*, at least in any directions remotely like those that had been pursued. The tradition of IR may fail when its notions are applied to uncommon areas or uncommon questions about familiar areas. Less trivially, it fails when a simple question is *discovered* to be related to unfamiliar matters. It fails, not because of lack of rigour, but because the common notions considered are just not suited to all matters—phenomena in science, tasks in technology—that have become topical.

Let there be no mistake about the loss involved. At least, without doctrinaire fetters of formal rigour, knowledge of common notions and formal deduction can be *combined* to establish (new) mathematical properties. Perhaps even more significantly, experience with common notions leads painlessly to (common) areas that we want to know about where those mathematical properties contribute to effective knowledge. In short, *for these notions the master assumption is satisfied*: relative to our limited appetite for knowledge.

What next? As might be expected, when the 'solid body' of common notions reaches points of diminishing returns the questions about these notions at the beginning of this section become decisive. Among those of us who have become familiar with a generally unfamiliar area, new notions become common, sometimes long before they get a name, with an obvious extension to unfamiliar questions about familiar objects. Certainly, thoughtful mathematicians see things that way, for example, Manin at the beginning of [12] with quotations from others. Admittedly, in his discussion he does not refer to the tradition of IR, not only not to the name, but not to such literary forms as *representation theorems*, mentioned earlier, which seem to provide more efficient formulations of knowledge about the new common notions than the current fashion.

Conflicts with the literature: in line with the refrain of this article about wide-spread practice[4] contradicting abstractly innocent home truths Over the years Gödel attempted to draw attention to a new direction in set theory (over and above formal deductions from knowledge codified in current axioms); or, more to the point, to continuing the old direction that had established the existing axioms. He mentioned, innocently enough, familiarity with the subject; in particular, for progress with unfamiliar questions about large cardinals of familiar sets generated by transfinite iterations of the power set operation. There was wide-spread scepticism long before he tried to 'argue' a case with mouthfuls like 'platonism' (and its touching association with belief in other abstract objects like ghosts; cf. document 040411.5 of the *Nachlass*, on the modern development of the foundations of mathematics in the light of philosophy, which Gödel divides into a right and a left wing tradition. The right wing includes both platonism and spiritism, apparently in the hope that the association would make both more credible).

Conclusions for CT As meant 50 years ago, CT belongs squarely to the tradition of IR, which certainly allows distinctions within common notions, of the kind made at the beginning of Section 2 concerning different systems for which a program is to be effective. There is the separate question about the adequacy of the common notion(s) considered for data processing. And, finally, there is the possibility of *discovering* other areas, in pure mathematics or its applications, where the mathematical equivalent, recursiveness, is suited to describing the facts. 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, and is a model of *evidence for the use of a notion* to tell us what we want to know about (groups); cf. also the end of Section 6 on new questions.

Additional comments on the early literature concerning CT For the tradition of IR the presentation by Church in [4] is not at all satisfactory, incidentally, reflected in the malaise of some contemporaries; cf. footnote 18 on p. 356 of [4]. Today it is easy to elaborate.

There is a whole arsenal of notions that are now known to satisfy the laws of Church's λ-calculus. But no description of any one of them bears the hallmarks of common notions, which are the target of IR. This simple fact is not changed by the discovery that some elements of that arsenal seem to be quite well suited to some areas of data processing by some processors. On the contrary this fact underlines the limitations of IR as knowledge expands.

In contrast, 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 (and, as somebody said, such distinctions between discovery and manufacture are difficult because generally they make as little difference as here). This constituted essential progress for IR, and, again, is not changed by the many defects of the notion for understanding data processing. For example, quite often the *particular list of basic rules is an artifact*, not only for computations in parallel on the latest wonder in the field of hardware, but also for the familiar kind including wet ware. Whole 'subroutines' can be realized by 'units', not only single basic steps. *Reminder:* Banal as this observation may be, it puts in question a major part of would-be applied (theoretical) computer science on so-called complexity theory, where the number of basic steps is used as the measure of complexity. Of course, all this is not new in the history of human civilization, being reminiscent of—impeccably precise!—Tripos problems on would-be applied mathematics such as the hydrodynamics of perfect fluids.

The topic above—of making some or avoiding any *choice of rules*—lends itself to rewarding analysis in the tradition of IR, by use of a particularly modest variant of CT, though usually not viewed in this way. It is the subject of the next section.

4 Effective definability by equations: the priority of computability over computation (rules) A prototype here is definition by recursion; for example, exponentiation, n^m, by use of the auxiliary functions of addition and multiplication. The constants 0 and s are interpreted as zero and successor. Below, finite systems of equations are denoted by $E(f,\vec{f},x)$ where f stands for the function to be defined, $\vec{f}$ for the auxiliaries, and $\vec{x}$ is the sequence, say, $x_1, \ldots x_N$ of variables (instead of n and m above). They range over the natural numbers.

As already mentioned, f is defined by E if it is the only function that satisfies $\exists\vec{f}\forall x E(f,\vec{f},x)$. There is nothing effective or mechanical about this property. Today this is clear from the fact that every hyperarithmetic function is defined in this way by a suitable E. But even without such background knowledge the nonmechanical character is clear from $E_0(f,f_1,x)$ below:

$$f_1(0) = 0, f_1(sx) = ssf_1(x) \text{ for doubling; } f(x) = f_1 f(sx).$$

An *argument* is needed to see that only f: $x \mapsto 0$ satisfies E_0 for all x. Specifically, if $f(0) = p$ then $f(p) = p/2^p$, and thus not integral unless $p = 0$. Alternatively, if one thinks of partial functions, E_0 is satisfied also if f is not defined at all. Viewed this way, the noneffective character of the definition lies in the fact that $f(0)$ and, more generally, $f(\alpha)$ for other specific numerals α, are not determined by (a finite number of) *substitutions* α_i for x in E. This is in contrast to the case of, say, f_1 where the value of $f_1(\alpha)$: $\alpha = s\alpha'$, is determined by: $f_1(0) = 0$, and $f_1(s\beta) = ssf_1(\beta)$ for $\beta = 0$, $\beta = s0, \ldots \beta = \alpha'$. In symbols:

$$\forall f_1\{[f_1(0) = 0 \wedge \ldots f_1(\alpha) = ssf_1(\alpha')] \rightarrow f_1(\alpha) = 2\alpha\};$$

in fact, the value is *unique*: $\forall x \forall f_1\{([f_1(0) = 0 \wedge \ldots f_1(\alpha) = ssf_1(\alpha')] \wedge f_1(\alpha) = x) \rightarrow x = 2\alpha\}$.

Corresponding substitution instances of $f(x) = f_1[f(sx)]$ are satisfied by all f such that, for $0 \leq x \leq \alpha$: $f(x) = 2^{\alpha+\beta-x}$, with $\beta \geq 0$. The words 'determined' and 'satisfied' have here their common meaning: the variables range over the natural numbers, respectively the set of all number-theoretic functions. So the additional requirement is expressed by the common notion of f being *finitely determined* by E. In symbols:

For each natural number α there is a *unique* β such that

$$(*) \quad \forall f \forall \vec{f}\left\{\left[\bigwedge_i E(f, \vec{f}, \vec{\chi}_i)\right] \rightarrow f(\alpha) = \beta\right\},$$

where, for some P_α, $1 \leq i \leq P_\alpha$, and each $\vec{\chi}_i$ is a suitable numerical substitution for $\vec{x}$.

$(*)$ is not yet effective since $(*)$ looks 'transcendental'. But a little exercise shows how, for any given α, β, *and* $\vec{\chi}$, $(*)$ can be decided; cf. [10], improved in Statman's contribution to [1] by better quantitative information. The latter can be interpreted either as measuring the complexity of the decision method or the range of functions (of finite support with bounds on their values) that form a so-called basis for $(*)$.

In any case the decision procedure is recursive in the usual sense, for arbitrary E and numerical substitutions. As a corollary: if f is finitely determined then f is recursive because, for any α, β can be found by trial and error, by trying out all β and sequences $\vec{\chi}$.

Bibliographical comments As is well known, for example from Kleene's exposition of recursion theory under the proprietory name 'Herbrand–Gödel', the general project of equational definability goes back to a suggestion by Herbrand, improved by Gödel, and published in his Princeton lectures in 1934. The relevant correspondence, which is now available, provides a vivid picture of bright ideas and false starts, red herrings and new directions that is by no means valid only for the present parochial topic, let alone for the people involved.

Herbrand's original letter, of 7 April 1931, has a crucial requirement (3) on the equations to be used: intuitionistic proofs showing that it is possible to compute the values of the functions (uniquely: *eindeutig zu berechnen*). Gödel's reply of 25 July, the eve of Herbrand's death, is, very understandably, preoccupied with the scope of intuitionistic methods, but without a word on the means of computation envisaged. As shown by the example $f(x) = 2f(sx)$ above, an intuitionistic proof that f is defined, and hence a means of *computation from the proof*, does not ensure that f is finitely determined. Here it should be remarked that the μ-operator is defined by equations superficially similar to the example above.

In his lectures at Princeton in 1934 Gödel simply avoided the question that had preoccupied him, by presenting a particular calculus, later modified by

Kleene (and thereby facilitating certain metamathematical arguments, for example, by use of the normal form mentioned already). All this is very much in the particular mathematical tradition of getting on with the job, which can disturb even those who are not underprivileged; for example, Wittgenstein complained of impatience in this connection, Goethe put it more poetically, and so forth. 'Particular' because there is the recent tradition of setting out elementary distinctions under the mildly affected heading 'foundations' of the subject at issue. But read right it is often very good value. After this digression, we return to equational definability, and Gödel's calculus in 1934.

Simply no common notion in sight was even a candidate for being described by that calculus; no more than for the λ-calculus at the time, and much less than now (cf. the end of the last section). In particular, there was no idea(lization) of a system for which the calculus supplied the effective rules or programs. Perhaps there was, and certainly there could be generated, a *feeling* of confidence in its wide applicability, simply by remembering Gödel's own arithmetization of formal deductions; not only, of course, the numbering of formal objects, which Cantor knew too, but the arithmetic representation of relations between such objects, for example, between the numbers of a derivation and the derived formula. They were defined by equations. In other words, the 'small-minded' preoccupation with unquestionably uninspiring equational rules is broadened when the mind is allowed to roam over the *unlimited possibilities of different interpretations* of the equations.

This feeling is condensed as it were into a proposition by—Kleene's proof of—the equivalence to Turing computability. The proof is straightforward (by use of arithmetization), but essential for IR: it provides access to the resources of the common notion involved in Turing's scheme. In other contexts the properties of Kleene's calculus that are severe blemishes for the logical tradition, in particular, its blatant incompleteness with respect to arbitrary E, are not even defects. The calculus provides a *new description*, which, as Kleene showed, is quite well suited for a number of problems about the common notion in question.

As just mentioned, the calculus does not satisfy the demands of the logical tradition. If authors can judge such things themselves, Kreisel and Tait [10] wanted to correct that blemish. By 1960 the strategy was clear. One needed a so-called semantic notion, called 'finite determination' above, and then *established* the completeness of a suitable calculus for a suitable class of formulas. After 25 years I should stress a different aspect, touched on already at the end of the last section, and summarized in the subtitle of the present section.

Not completeness or incompleteness of a particular calculus is the issue, but the artifacts produced by the choice of *any* calculus; tacitly, of any universal calculus (for the whole class of formulas considered). This is to be compared to experience in ordinary logic where the word 'provability' obscures the *assumption* that proofs or even certain rules of proof are primary. This is made explicit by shifting attention to the common notion of validity. In the next section this matter will be taken up in a suitably broad context.

Diverse comments The last paragraph can be restated as a possible *conflict* between classes of problems (here, wff in some logical language) that have rewarding qualitative solutions (formulated in terms of validity), and those that lend themselves to algorithmic treatment. It is simply lack of scientific experience[5] to assume that the former, being about the coarser notion of validity, constitutes a *first step*, tacitly, *in the right direction. Reculer pour mieux sauter* is a good warning, even if it does not tell us which direction among 180° to take. As to complexity theory, the practical defects mentioned at the end of Section 3 constitute a philosophical contribution in the *examination* of logical ideals: they are striking proofs of the conflict above; for example, for the choice of classes of polynomial equations in real algebra or of linear diophantine equations (cf. footnote 2a to the review of [6]). Naturally, these conflicts do not exclude the possibility of elaborate, of course coherent, theories, in line with the truism that mere truth and, a fortiori, mere coherence, is not enough by a long chalk.

About 15 years ago, in the unpublished part of his dissertation, H. P. Barendregt elaborated what was then called 'Church's superthesis' (for a brief report, c.f. top of p. 43 in the 2nd edition of [1]). Not only the classes of functions defined by the different familiar schemes are equal, but the definitions (= terms) themselves *match so as to preserve computation steps*. More precisely, this applies to those schemes that are felt to be natural, and not, for example, to Kleene's deterministic calculus (in contrast to his later rules S1–S9). *Digression* for specialists: A different project, but in a similar vein, has come to be known under the heading 'stability of E-theorems', so to speak 'super E-theorems' for the usual intuitionistic systems. Instead of stating only that, for some numeral α, $A[x/\alpha]$ is a formal theorem if $\exists xA$ is one (and is closed), now the terms α_π^σ: $A[x/\alpha]$ are associated to proofs π of $\exists xA$; σ refers to various schemes of interpretation, realization, normalization, and so forth. Now the α_π^σ for different σ match up. Here a now no longer familiar exception is the scheme implicit in Hilbert's substitution method for arithmetic, which has a similar flavour to Kleene's deterministic calculus. Naturally, 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.

IR and flexibility (of rules) It is, perhaps, no more than a pun to confront formal or informal rigour and flexibility. But in the particular case of the common notion of rule, definiteness is simply part of the idea(l). It leaves open, to repeat what cannot be repeated too often, the master assumption: to what extent the ideal contributes in a particular area. Once an appropriate range of indefiniteness is known it can, as always, be handled by use of various literary devices. Different ranges are illustrated concretely, for example, on pp. 144–145 of [9]. They involve different normal forms or only selection from traditional computation procedures. Another digression (for the same specialists as in the last paragraph): Contrary to a widespread superstition, *nonconstructive* proofs π of $\exists xA$, for quantifier-free A, possibly with parameters (in other words, for typical algorithmic problems) also provide programs, but they are less definite. Specifically, in classical logic we have a disjunction: $A[x/t_i]$, $1 \leq i \leq N_\pi$, with the terms t_i depending on π; in contrast, an intuitionistic proof provides a single t_π.

This looks good if one forgets that, generally, different t_i, among those for which $A[x/t_i]$ is correct, are efficient for different values of the parameters. Intuitionistic idea(l)s force us to be inflexible by making a uniform, and therefore, for most values, inefficient choice.

5 Laws of thought: this side of the pale The last section belongs to what the English used to call the 'tame' tradition, especially in mathematics. One begins with the innocent notion of finite determination, dots the i's and crosses the t's, and expands by means of novel interpretations; in other words, by discovering relations between this notion and other things (from computer programs to finitely generated groups). One of the most glamorous disciplines in this tradition is probability theory, which started with frivolous games of chance, and became an integral part of some fundamental theories of nature.

Propositional algebra is a striking example of the opposite direction, especially if rules of inference are added to form a calculus. It was put on the map by a book of Boole, actually with 'Laws of Thought' as (principal part of the) title. Later in this section there will be a few general reminders on the kind of steps that have brought progress to this tradition; evidently not by expansion, but by cutting the original (cl)aims down to size as it were; again by novel, though of course different, interpretations. As always, there are those rare exceptional disciplines, where one starts at what seems to be the top, and never looks back.

Despite a lot of judicious wording, originally CT was intended and understood in the sense of the second tradition. In terms of Section 2, effectiveness for the ideal mathematician was meant. Thus recursive undecidability results were advertized under the slogan: what mathematicians cannot do. This is an impeccable subject for IR inasmuch as there is a common notion or idea(l) of 'the mathematician'. Once again, it is a separate question whether this common notion or norm is suited to the phenomena at issue. According to Section 1—about dubious doubts obscuring genuine problems—the discussion below certainly removes doubts about the mere possibility of any analysis of the ideal mathematician; in the first place, ideal according to the intuitionistic faith.

Remark on the superficially arbitrary choice of this ideal (for other nonbelievers; cf. end of Section 4): In view of how little is known about the outer limits of the mathematical imagination, CT in its original sense is simply beyond the pale. If anything remotely like it is to be pursued, some shift of emphasis is required (by the title of this section), and the intuitionistic variant presents itself at least as a candidate, for the obvious reasons recalled in the next paragraph. What, if anything, is gained by the pursuit is most efficiently considered after the facts are in; cf. the *Manifesto* at the end of this section, and the last section.

Recursive realizability and other interpretations of formally intuitionistic systems 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). Viewed this way, the formal laws are of course assertions about those constructions; in other words, about certain aspects of *thought-processes*. Certainly the laws are not discovered, let alone justified, by referring to psychological or physiological, so-called inductive evidence, but then the laws of perfect liquids are not discovered by poking around in water or pitch either.

Given such formal systems there are straightforward, and by now very familiar, ways of formulating CT; both when variables, say for (lawlike) functions, are and are not present:

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

and

$$\forall x \exists y A \rightarrow \exists e \forall x \exists z \{T(e,x,z) \wedge A[y/U(z)]\},$$

where T is Kleene's T-predicate. For a convincing *refutation* of CT, proofs are required of $\exists f \forall e \exists x \forall z \neg [T(e,x,z) \wedge f(x) = U(z)]$, respectively, of both

$$\forall x \exists y A \text{ and of } \forall e \exists x \forall z \{T(e,x,z) \rightarrow \neg A[y/U(z)]\},$$

because a mere formal negation is weak (in intuitionistic logic).

Recursive realizability is well known to exclude not only such refutations of CT, but even weak ones, because the consistency of CT is established. (For the record, other interpretations have been used too.) In a similar vein, formal work has established what has come to be called Church's *rule*: if $\forall x \exists y A$ is a formal theorem so is $\exists e \forall x \exists z \{T(e,x,z) \wedge A[y/U(z)]\}$. As in the digressions at the end of the last section this work has been refined to establish the corresponding 'super rule'.

In the last 15 years the subject of recursive realizability has become a Dutch garden, in which the answers to a great variety of formal questions can be seen almost at a glance. So to a large extent my report on such matters in Part II of [7] is now obsolete. But, as so often in such situations, the subject is not closed; some of the most useful points loc. cit., at odds with later preoccupations, have not become known, and some pertinent questions, incidentally in the tradition of IR, were overlooked there. Here are a couple of samples.

First, there is the dilemma for refutations of CT: if a system has the (numerical) E-property *and* is formal, there is a mechanical procedure, say e_0, which extracts, from any proof of $\forall x \exists y A$ and any n, some m_n such that $A[x/s^n 0, y/s^m 0]$ is a formal theorem, with the usual refinements when the E-property is established by restricted metamathematics for suitable parts of the formal system. It is not a theoretical dilemma since sound systems need not have the E-property. Specifically, if $\exists y A[x/s^n 0]$ is a formal theorem, $A[x/s^n 0, y/s^m 0]$ must be provable for some m, but not necessarily in the same (formal) system. And a restriction to formal systems 'on principle' would be no less than a *petitio principii* when the validity of CT is at issue. The dilemma is practical because there are no rewarding candidates of systems in sight that can be established with IR to hold for the constructions of the ideal mathematician, but do not have both the two properties.

Secondly, and this is, I believe, much more central, and (therefore?) much more widely neglected, the fanfares in the intuitionistic literature about taking constructions or processes into account seem a *sham*. The logical laws use little more of proofs or constructions than the relation between a proof and the theorem proved, respectively, between a construction and its outcome. And when an attempt is made, as in Brouwer's admittedly clumsy embellishments of his bar theorem, to introduce more delicate aspects of the constructions involved (definitions or proofs, no matter), later work soon suppressed the new elements. History repeats itself here. Russell started off trying to make an inventory of the mathematical zoo, but soon found himself analysing abstract notions 'away'.

These developments were not merely a matter of weak backbone, even if matters looked like that to Gödel in his essay on Russell's mathematical logic (and in effect had similar results as the thoughtless cult of Ockham's razor or, in modern jargon, of black boxes). It is just objectively difficult to *discover* rewarding elements inside those black boxes (as Gödel himself had trouble finding rewarding axioms of infinity). 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 CT, don't rely on the off-chance of some process being grossly nonmechanical; so much that not even its effects that strike the eye, the so-called output, can be computed mechanically from the input. A formal counterpart of this view, of the crudity of CT, is the

Conjecture: There are simple conditions, easily verified for current intuitionistic systems, that imply easily the consistency of CT and closure under Church's rule.

A proof would supersede current proofs, which are often laborious, and would thereby merely embarrass the tame logician. But, philosophically, it would be progress by confirming the suspicion above of the proposal to use the intended interpretation of intuitionistic logic for the topic of CT. (It would show that the project is an oversight, not a brave try, refutable only by detailed studies like ingenious realizability interpretations.)

Reminders: On the, incidentally, fairly common view that the original interpretation does not make sense, the project doesn't either. Recursive realizability is then taken to give sense to the formal machinery. (A much simpler alternative is to ignore the latter altogether.) There are also other ideals of 'the mathematician', for example, Gödel's, requiring a relentless search for new axioms of infinity.

Bibliographical remarks on laws of thought The pioneers, in particular Frege, had of course a lot to say about the distinction above between thought processes and their results. He called the latter 'objective' thoughts, thereby hanging his observations on pegs supplied by some hoary 'isms'. He saw a principal use of his objective analysis (ignoring subjective processes) in the greater security it gave to common reasoning. But he never recognized the *assumption* in supposing that security-in-principle, that is, security of principles, was a dominant factor in the area (of mathematical certainty). An instructive example of abstraction from processes that was rooted in experience, not in a dubious assumption, goes back some 150 years, and is associated with Dirichlet.

The *graph* of a function neglects, that is, abstracts from, particular definitions and computation rules. It was a genuine discovery. For centuries, functions had been thought of as rules. But the theorems actually stated about them at the time depend only on their graphs. There were immediate mathematical payoffs. Moreover, in effect, if not on purpose, this led to philosophical progress as follows.

When the fundamentalists, born-again with the grace of intensionality, gush about those neglected elements they forget the difficulty of making a rewarding selection. For example, some of the popular parts of would-be applied computer science already mentioned are presented as if half the battle were won by looking inside black boxes at all; meaning here complexity measures inside graphs of functions. The measures are expected to sound familiar and to be precise, but are rarely tested for adequacy (recall the end of Section 2 on Turing's basic rules). Sometimes the possibility of such tests is denied, and so it is not realized how demanding a proper choice is, comparable to the situation with IR earlier on. The tame tradition does better by adding extra structure to the graph only if felt to be needed. Thus this tradition is *more* philosophical by paying attention, automatically as it were, to a significant selection of such structures (= *useful* 'intensional' elements).

Deeper waters: reflections on the old impression of a conflict between logical and psychological (or other biological) matters such as thought processes. More pedantically, the impression is that logical aspects, which are of course present, are rather marginal for these phenomena. Accordingly, 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 be 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.[6] *Neglected corollaries.* First, given that the genetic code violates those idea(l)s most brutally, the success of genetic engineering refutes—once again, and strikingly— a tacit assumption behind them; specifically, that human intellectual capacities are at their best or, at least, efficient when using logical knowledge. Experience shows, as somebody said, that reason is not reasonable (in the sense that Simple Simon gives to this word). The second corollary concerns A.I., a part of *another new branch of engineering*, which uses other programs (not the genetic code). By almost common consent among experienced engineers, success in their subject(s) requires flexibility in the use of available resources, not systematic rules in the logical tradition, even when the latter may have helped in building up the resources of material and intellectual technology.

Conclusions for CT There is no end in sight to the possibilities of coherent and imaginative analysis in the tradition of IR; specifically, of the common notion(s) of the ideal mathematician and thus of CT, as interpreted in the present section. What is in doubt is the adequacy of these notions, not only to the phenomena of mathematical thought (processes), but even to our practical knowledge of them. Without much exaggeration, current theoretical literature contributes infinitesimally to that knowledge. *Corollary.* Differences between different parts of that literature, for example, pro and contra A.I. (or even differences from talkshows on TV) are not apparent if measured by the absolute value of their contributions.

Manifesto Existing *theoretical* knowledge of our own data processing is so limited that (even) the logical aspects of the common notions here considered remain principal elements of that knowledge. Those of us who recognize this may still want to analyse the little we know with IR, and feel that, as long as we do not stray beyond the pale, we can afford this luxury in our age of intellectual affluence. As for selecting suitable parts of the current literature, by the corollary above the choice cannot rely on differences in the *absolute value* (of their contributions). But their *ratios* usually separate remarkably sharply the parts beyond the pale from those that are still on this side.

Readers satisfied with the manifesto may wish to stop here. Those curious only about my conception of the luxury in question should skip the next section, which is intended for readers who, like myself, wish to muse about consequences for CT of knowing a bit more about the laws of thought; naturally, by reference to a suitable parallel.

6 Theories in natural science: rational and computable laws The parallel meant in the last paragraph concerns CT applied to, say, physical systems or, more precisely, to theories of them. The words 'input' and 'output' are used below for initial conditions given in nature or under experimental control, respectively for observable behaviour; in both cases: *according to theory*. The former correspond to rules or programs, even though in general there need not be—according to theory!—any *discrete steps* that terminate in the observed behaviour.

There is another parallel to help keep our feet on the ground, when mouthfuls like computability-in-principle begin to make us light-headed: the thesis attributed to Pythagoras about *number* being the *measure of all things*. This is to be compared to: programs for a universal Turing machine being such a measure. In particular, when, as originally intended, Euclidean geometry is thought of as a theory of physical space there are *dimensionless quantities* that are not rational; for example, the ratios of diagonal to length of the square or of circumference to radius of the circle. In the case of processes, represented by functions, there is a distinction between those that are not ratios of polynomials (with rational coefficients), and those that do not take rational values for some rational arguments; for example, $\sin x$: $0 < x \leq 2\pi$.

The two results, of the irrationality of $\sqrt{2}$ and of π, were discovered more than 2000, respectively 200, years ago. So there is plenty of experience to reflect on when wondering about realistic consequences; both for experimental science and, above all, for the style of later theory.

Noncomputability results, corresponding to irrationality on the parallel mentioned, have accumulated in the last 50 years; in work by Pour El and Richards with special emphasis on questions arising, more or less, in mathematical physics (cf. for example, their most recent papers [14] and [15]). In particular, the latter has a neat description in terms of bounded and unbounded linear operators, where previously one had (endless) lists of typical, familiar examples, as at the beginning of the review of [13]. But, again by experience with the parallel, a much more demanding job is the *proper interpretation* of the mathematical results; in hackneyed terms: not to forget the physics behind the mathematics.

Experimental consequences (of the irrationality of $\sqrt{2}$ or π) It is generally assumed that there are none, 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; for example, in so-called fundamental theories characterized by relations with very diverse phenomena, and admitting ingenious statistical interpretations. Be that as it may, problems of similar flavour come up with the two demarcations, between rational/irrational and computable/noncomputable. Much more can be said about the other kind of consequence, for new kinds of solution.

New kinds of solution, say, of differential equations, and new questions about them It would be close to affectation to belabour ancient history; the passage from rational solutions to elementary expressions, built up by adding trigonometric functions, exponentiation and their inverses; going on to (effectively) convergent series of such expressions. Perhaps less familiar is the shift of emphasis to a qualitative classification; not according to the manner of convergence (proof), but by such related properties as analyticity or continuity in parameters. And the discoveries of so-called weak solutions and of distributions are part of living memory. Incidentally, these things are not confined to marginal refinements, but used in central parts of mathematics and mathematical physics.

On the other hand, as an advertisement about technology used by United Technology put it, nothing is ever lost completely. Once unexpected implications of good old rationality were recently discovered in connection with the Yang-Mills equations, and duly advertised, too. Here, 'in connection with' has to do with new questions.

One of P.A.M. Dirac's disarming *aperçus* is worth quoting: "If I want to know something about a differential equation, I derive the answer directly without solving the equation." In other words, generally he did not merely want to know the graph of a solution, nor does anybody else. Since the turn of the century, the subject of *qualitative* dynamics has grown up, concerned with topological, that is, nonlocal, properties of solutions. More recently a shift in a different direction had to be made when, as the title of the readable exposition [11] puts it, the failure of predictability in Newtonian dynamics was recognized; not of predictability-in-principle (= determinism), but of the kind of order which makes it rewarding to bother about it. Beyond the so-called horizon of predictability, properties of the solution are used that are familiar from statistics; as occasionally notions from probability theory are applied in number theory to impeccably (primitive) recursive sequences.

The moral for the version of CT discussed in the present section—or, equivalently, for the question whether current theories of nature are mechanistic in the sense of [8]—is compelling. The answer to the question is one of the many things that one might want to know about such a theory. And if it is negative, one just has to look for suitable problems with solutions that are not only computable-in-principle, but in practice. At least, to start with, the words 'suitable' and 'in practice' are not as clear as 'mechanistic'. But then, like mere truth, mere clarity is not enough by a long chalk. A clear meaning of those words is part of the solution; cf. Schiller's *Spruch des Confucius*: *Nur die Fulle bringt die Klarheit.*

Remarks on another shift of emphasis in the general area of recursiveness, and away from the preoccupations of CT: The model here is Higman's answer, touched on in Section 3, to the (new) question: Which finitely generated groups can be embedded in finitely presented (f.p.) groups? A mere corollary to his 'positive' answer is an f.p. group with unsolvable word problem; in other words, something of concern to CT. 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 CT in any of its versions. Incidentally, much the same kind of shift is conveyed by the subtitle of the chapter in [3] on Hilbert's 10th problem about diophantine equations: positive aspects of a negative solution. It remains to be seen whether theories in natural science will benefit from this kind of shift too.

7 Conversation pieces: a wider market for the literary forms of mathematical logic, and especially of IR? wider than the market for their contributions to science and technology Even if these contributions 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).

As I read him, Bertrand Russell expresses a somewhat related view somewhere in the introduction to his *History of Western Philosophy*: philosophy is to teach us how to act in the face of uncertainty. This is a tall order. The variant here proposed puts 'talk' for 'act'. NB: This is in direct conflict with one of the most famous passages in Marx (as it was meant, that is, without quibbling about the changes that can result, for example, in the world of academics, from a different style of talking). The idea that philosophers, that is, those who have enjoyed a philosophical education, should be particularly well prepared for changing the material world too, is one of the many odd *assumptions* of that (to many, compelling) writer. To me he is not persuasive. Thus in this article there is no trace of measuring value by labour; rather by the opposite, of interest of result *divided* by labour! We now return to the question above:

How to talk in the face of uncertainty (= ignorance)? The easiest answer is *in dubio non*, expressed in, often vocal, advice to be silent. This advice is not followed. Certainly not by Wittgenstein, who has given the most famous formulation of that answer in our century. Nor by the so-called silent majority, which breaks its silence (see Note 2) with barks and grunts, not housebroken as it were by proper breeding; cf. Aristotle's *Met* Γ4, 1006a, 6-9. To paraphrase Pascal: if you try to be a silent angel you'll finish up neighing like an ass (*L'homme n'est ni ange ni bête . . .*).

How then might IR and mathematical logic help? The idea is in the *Manifesto* at the end of Section 5; here are a couple of samples (related to CT); at least, of the existence of various possibilities, if not of their (all-important) *weight*.

The first sample concerns A.I. and the laws of thought, a topic touched earlier on. The question is, how to talk about this topic; for example, in a circle of like minds, not necessarily as a missionary. Most discussions I know (pro or contra, no matter) seem to me spoilt not only by clumsy exaggerations, but above all by the following fact: There is a glamorous aura of electronic computers, with a hardware that is, admittedly, one of the wonders of the modern world, but no mention of its details. Without exaggeration, the discussions rely on Simple Simon's idea of a computing machine, in short, on this common notion. The words come from the world of engineering, but the thoughts straight from the logical tradition.

Viewed this way the discussions are likely to benefit from explicit reference to the unquestionable *logical progress*, including attention to IR, that has been made since the discussions between the (same) pros and contras 100 years ago, who were then called 'formalists', respectively, 'antiformalists'.

Bibliographical remarks and a disclaimer In the last 20 years of his life, Wittgenstein was surely sensitive to the banality of (as opposed to logical mistakes in) both the kind of popular discussions above, and their counterparts in academic jargon. But the dramatic style of his objections seems (to me) to jar, as it were objectively, with their content. There may be something dramatic about claims—true or false—to have found 'the general form of propositions'. A correction is surely salutary. But is it dramatic? (Cf. also refutations of Hilbert's program.) Do a few starry-eyed fans of A.I. constitute evidence of bewitchment (by the admittedly facile language of that subject)?[7] For the record I too am impressed by the phenomenon of otherwise thoughtful scientists repeating empty phrases like a mantra in discussions on some broad philosophical question. But I have thought of this as the 'natural' consequence of their feeling obliged to have an opinion when in fact they had none. (Experts may attribute that sense of obligation to some wicked witch.) The colloquial style above requires a disclaimer, in the following reminder.

Reminder on our practical knowledge, here, of thought processes: It is an *assumption* that those general discussions benefit significantly from adding to practical knowledge any specialized, let alone academic, experience such as logic. For example, everyday experience of creative and mechanical thinking (= following mechanical rules) 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 Σ_n: $1 \leq n \leq 100$ 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.)

If this colloquial style of thought, not only of words, is found to be as good as any then the academic style is a ritual, at best useful as occupational therapy. Fully aware of this, I now turn to the second sample of using the logical style.

Meaning and use Questions suggested by this pair of words turn up almost anywhere; from charming situations with bricklayers and their mates, to erudite texts on modern semantics or on pragmatism of blessed memory. As long as the questions are sufficiently general, by the nature of the case they can be discussed 'in principle' in all those contexts; for example, questions about conflict or correspondence, but also about the relative contribution of establishing formal properties of meaning and of pin-pointing uses. Here the very modest Section 4 seems to me good value, of course, in the non-Marxist sense.

There is no conflict there between meaning and use. Different meanings of 'equational definability' correspond to different uses, rigorously to boot. As to relative contributions, they differ for different meanings. The formal side was more demanding in the case of straight definitions, needing knowledge of hyperarithmetic functions. On the other hand, once the use of finitely determined equations had been recognized the formal exercises were routine. Reminder: My own experience is too lopsided to judge what the material above is worth on the market envisaged in this section. In particular, I am not familiar enough with the many alternatives (to the logical style and IR), which, as in an earlier warning, are all-important for such judgments.

Other literary forms are available for 'talk in the face of uncertainty', except of course to those hamstrung by some doctrine, for example, of formal precision, according to which only formal theorems have substance, and all else is packaging. The alternative usually mentioned is the discursive philosophical style with its big abstract words (and its venerable tradition). Another much more recent alternative is the philosophical novel, although it is rarely seen this way (least, in connection with foundations).

Here *aperçus* and *bon mots* take on the role of theorems in the logical style; and slogans, which are to be used repeatedly, correspond to lemmas. As I see it, a principal difference from the older style is that those *aperçus*, etc., are not introduced in isolation and dissected, but appear 'live' in memorable situations, and thus help us learn where they apply. Viewed this way, staged dialogues and comic strips in the contemporary foundational literature are minor variants of the old style. Perhaps a closer parallel to the style of the philosophical novel is found in the oral scientific tradition. Here memorable proofs and definitions are 'interrupted' by perceptive comments if and where this is appropriate. Again they appear live, and thus casual in form.

Remark: Some of the pioneers of mathematical logic, as far back as Leibniz, hoped that its style would bridge the gap between formal slogging (ours is not to reason why) and the understanding expressed in literate comments. Its style could be more easily learnt since it does not require literary talent. Once again, it is left open how effective this style is for communicating the (additional) understanding where to apply the logical knowledge; 'once again' as with the master assumption in Section 1.

An anecdote: Recently Gödel's incompleteness result was brought to the attention of Austrian politicians; successfully in the sense that one of them quoted it during a 'discussion' sponsored by a movement called *Nova Spes*. His purpose was to underline the, incidentally, quite obvious weakness of some panacea meant to bring health, wealth, and other ingredients of happiness; in short, new hope. Though the proposal was surely simple minded, as bad luck would have it, it was not formal nor did it give any hope of solving any recursively unsolvable problem. A clever man was led to *faire la bête.*

Let there be no mistake. *Some* learning was conveyed by learning the truth of Gödel's result; it just was *little* learning. Empirical-minded, not to be confused with so-called empiricist philosophers, might be interested in looking for more such empirical evidence.

NOTES

1. Below there are other warnings against such appearance of innocence. They are prompted by a complaint of one member of the audience at a lecture in June 1986 at Utrecht where an earlier version of this article was presented. He found it uncontroversial. (Of course, 'boring' was meant since there is no virtue in being controversial except perhaps among professional dissidents.) I take this opportunity to thank not only this critic, but also the logicians at Utrecht for their hospitality, and the Ford Foundation for a grant supporting work on the paper.
2. The phrase refers to A. E. Houseman's oft-quoted, albeit odd, epistemological ideal of an Irish terrier. When asked to define 'poetry', A.E.H. referred to terriers that can smell a rat, but not define 'rat'. In different parts of the world one has pigs that grunt when they smell a truffle.
3. A property P of a notion, say, N, is a definition of N if N is the only object (considered) that satisfies P. The passage to formal rigour is standard, once P has been established to be a mathematical analysis of P, and $\exists !N P(N)$ has been proved.
4. Also widespread, would-be revolutionary *obiter dicta*: for example, about an alleged 'relativity' of truth (of theses), which would be spectacular, in place of change in their marginal utility with increased knowledge, which is indeed important but a matter of course.
5. It is of course in the nature of lack of experience that particular native talent is needed even to suspect the *need for more experience*: cf. Goethe's remark to Eckermann on the high cost of *bon mots*, derived from experience bought, literally, by seeing more of the world.
6. Cf. [5]. This scientific failure contains a splendid piece of IR in its analysis of the notion of *rational code* (given the structure of DNA). The actual codes, both for mitochondria and for the rest of us, are quite different.
7. Big words like computable-in-principle may be addictive, and thus seem bewitching, too. Reminder: Mathematicians live with them, for example, 'imaginary' or 'transcendental' applied to numbers; originally, for reasons now considered (by most of us) to have been misguided. Short words like 'real' have a way with them, too. Hilbert got a lot of 'mileage' out of his real and ideal elements when it would have made good sense to pause after he had gone an inch.

REFERENCES

[1] Barendregt, H. P., *The Lambda Calculus*, 2nd revised edition. North Holland, Amsterdam, 1984.

[2] Barwise, K. J. (ed.), *Handbook of Mathematical Logic*, North Holland, Amsterdam, 1977.

[3] Browder, F. E. (ed.), "Mathematical developments arising from Hilbert's problems," *Proceedings of the Symposium on Pure Mathematics*, vol. 28, American Mathematical Society, 1976.

[4] Church, A., "An unsolvable problem in elementary number theory," *American Journal of Mathematics*, vol. 58 (1936), pp. 345–363.

[5] Crick, F. H. C., J. S. Griffith, and L. E. Orgel, "Codes without commas," *Proceedings of the National Academy of Sciences*, vol. 43 (1957), pp. 416–420.

[6] Fitting, M. C., *Fundamentals of Generalized Recursion Theory*, North Holland, 1981; rev. *Bulletin of the American Mathematical Society*, vol. 13 (1985), pp. 182–197.

[7] Kreisel, G., "Which number-theoretic problems can be solved on Π_1^1-paths through O?," *The Journal of Symbolic Logic*, vol. 37 (1972), pp. 311–334.

[8] Kreisel, G., "A notion of mechanistic theory," *Synthese*, vol. 29 (1974), pp. 11–26.

[9] Kreisel, G., "Proof theory and the synthesis of programs: potential and limitations," *Springer Lecture Notes in Computer Science*, vol. 203 (1985), pp. 136–150.

[10] Kreisel, G. and W. W. Tait, "Finite definability of number-theoretic functions and parametric completeness of equational calculi," *Zeitschrift für mathematische Logik und Grundlagen der Mathematik*, vol. 7 (1961), pp. 28–38.

[11] Lighthill, J., "The recently recognized failure of predictability in Newtonian dynamics," *Proceedings of the Royal Society*, A407 (1986), pp. 35–50.

[12] Manin, Yu. I., "New directions in geometry," *Russian Mathematics Survey*, vol. 39, no. 6. (1984), pp. 51–83.

[13] Pour El, M. B. and I. Richards, "The wave equation with computable initial data such that its unique solution is not computable," *Advances in Mathematics*, vol. 39 (1981), pp. 215–299; rev. *The Journal of Symbolic Logic*, vol. 47 (1982), pp. 900–902.

[14] Pour El, M. B. and I. Richards, "Computability and noncomputability in classical analysis," *Transactions of the American Mathematical Society*, vol. 275 (1983), pp. 539–560.

[15] Pour El, M. B. and I. Richards, "Computability and noncomputability in analysis and physics: A complete description of a class of noncomputable linear operators," *Advances in Mathematics*, vol. 48 (1983), pp. 44–74.

Salzburg, Austria