602
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Review of Gödel's 'Collected Works, Volume II'

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Preamble All prominent results of Gödel's writings in this volume and many of its asides have been gone over in the literature, for readers of differing background; cf. Note 1 below (from: **Notes: Mainly Beyond the Academic Pale**). As in the review of Volume I, the emphasis below comes from a broader view, which relates those writings to other traditions, but with a difference. The material in Volume I is squarely in one tradition, going back to Hilbert's *Foundations of Geometry*. (Through this work not only (young Gödel's kind of) mathematical logic, but generally the axiomatic method in its modern sense, was put on the map.) The same is true of Volume II up to p. 101 or, equivalently, of the writings from the first half of Gödel's life (till his mid thirties). But the later part, after p. 119, belongs to an older tradition, variously known as logic chopping or exact philosophy (in the academic sense of this word), which, in turn derives from the heroic perennials familiar from philosophy in its more popular sense.

With this contrast explicit in the writings themselves, they will be cited more often than those of Volume I in the earlier review.

Background: Hilbert's agenda at the turn of the century This is not to be confused with his later programme, in which so-called finitist parts of—what had come to be called—metamathematics were privileged (with the usual consequences of such practice). The often tedious literary forms of logic chopping in the foundations of mathematics were to be replaced by those of mathematical logic with emphasis on the idea(l)s of *consistency, completeness*, and *decidability*. These household words were applied by Hilbert to formal objects defined independently of any further interpretation. The scheme recalled—the best of—rational mechanics beginning in the 17th century, which both replaced logic chopping concerning matter and motion and gave scope to the armchair (applied) mathematician. Results in the literary forms of mathematical logic were expected to 'speak for themselves' too.

Gödel's contributions to this line of business remain (among) its most memorable successes.

Gödel's later agenda: What is lacking (in his earlier work)? Specifically, 'lacking' if logic is to be a science prior to all others, which contains the ideas and principles underlying all sciences (p. 119). In less academic terms, logic is to be a seed from which the tree of knowledge grows, and the logical order of priority is the corresponding (tree) ordering.

Roughly speaking, Gödel saw the best prospects for this idea(l) in going back to the older tradition, in particular, to elements that are, as it were, prematurely disregarded in Hilbert's scheme. Gödel presented such elements at various levels of sophistication, both in mathematical and other literary forms.

Agenda for this review: To balance the account with due regard to the editorial notes. First, the internal coherence of Gödel's view is emphasized, with some formal consequences of those neglected elements from the last 40 years. But secondly—and this is of course of broader interest—there are reminders of genuine alternatives to that heroic perennial of knowledge growing like a tree from a seed; made particularly memorable, I believe, by *contrast* with (Gödel's) pursuits of that idea.

1 How adequate are those would-be fundamental metamathematical notions? The adequacy meant here is a common-or-garden variety; viewed neither as a mere matter of principle (of being ever or never adequate), since this is not in doubt, nor of course according to the heroic ideal of logic on Gödel's agenda (since this is on trial here). The particular notion, used as a sample, is (relative) *consistency* prominent in Gödel's own titles (pp. 26, 27, 28, 33) for his work on *GCH*. The details are familiar enough to rely on the following:

Reminders. First, it is a common place that the work in question is more adequately described by different labels, for example, 'inner model constructions', 'absoluteness' (albeit relative to the ordinals), or 'conservation'. Pedantically, results, stated in such different terms, are obtained as corollaries to the work by means of general logical theorems, which correspond to so-called abstract nonsense in current mathematical jargon. Then contributions of the work to effective knowledge are discovered to follow from those different descriptions, but not from Gödel's titles. Secondly—and this keeps the first point topical—the later literature, for example, relating axioms of infinity and determinacy, continues to rely on relative consistency (as if it were an adequate description).

This alone would be enough to illustrate vividly Gödel's reservations about following Hilbert's ideal (of course, not despite, but because of the fact that they conflict with Gödel's own practice in his 'salad days'). A closer look underlines the point as follows.

Finite axiomatizations. If relative consistency (and the metamathematical methods of proof used) were the first order of business and such alternatives as (inner) model constructions only a means then finite axiomatizations would have compelling consequences. For example, mere validity of those constructions ensures a relative consistency proof by quite elementary methods, while, in general, this is not so (even) for r.e. systems of axioms. But, at least by pp. 12–13, the trade dealing in relative consistency results does not generally regard finite axiomatizations as so privileged.

Reminder. The points above, about labels and formal incongruities, would be as irrelevant as the name 'rose' in horticulture, if mathematical logic could be viewed as a similar trade with an established market. Such a view may well apply, by now, to certain parts of logic and their markets (dealing with finitely generated groups, finite fields and a few more). But those markets had only just been discovered at the time of Gödel's agenda, and certainly are not prominent in it; nor in this review, except for Appendix II.

Almost as a corollary there is a *positive side* to all this, at least for those readers who are looking for object lessons from experience with problematic aspects of the logical enterprises. This need not be a parochial interest, since those aspects are as it were chemically pure specimens of those found throughout the *commerce of ideas*; cf. Note 2 on this metaphor.

Be that as it may, nothing (positive) has yet been said about traditional logic chopping, the second item on Gödel's agenda. The particular alternatives above to relative consistency, which speak better 'for themselves' than Gödel's titles, do not come from that tradition. This is the next order of business.

2 Logic chopping: elementary samples Whatever the literary defects of the essay on Russell's mathematical logic (pp. 119–141, cf. also Note 3), the topic itself was, and remains, perfect for anybody who has anything to add to the logical literature. Russell had an exceptional talent for formulating memorably almost any thought that could cross anybody's mind, and used it freely. So he has left us plenty of pegs, one of them being his paradox, on which to hang—actually, often salutary—additions; obviously, to consolidate, not to introduce broad ideas. In the present case the broad idea involved is of course this: logical results, which do not speak (well) for themselves, may do so better when supplemented by some traditional logic chopping.

After nearly a century of experience it is fair to say that some of the items thrown up by people thrashing about for something to say about that paradox are more rewarding here than the latter, let alone Gödel's oft-quoted, would-be dramatic comment on p. 124.

Logical reminders. First, for arbitrary (binary) relations $R, \neg\exists x\forall y[R(x,y) \Leftrightarrow \neg R(y,y)]$; in fact, even for the special case, when $y = x$, $\neg\exists x[R(x,x) \Leftrightarrow \neg R(x,x)]$. Secondly, Frege had written down an axiom scheme for (his idea of) predicates and classes, which includes $\exists x\forall y[R(x,y) \Leftrightarrow \neg R(y,y)]$ as a special case when $x \in y$ is taken for $R(x,y)$.

Frege's scheme, which had been totally ignored in scientific trades of the commerce of ideas before Russell's paradox, gained a little notoriety by it. More high-minded (and less experienced) traditions assumed—as it were, as a matter of course—that there was a specific error (to understand) in Frege's scheme, so to speak, 'the' root cause of its failure (in some tree of ignorance), for example, the following:

Demons: (i) infinity, (ii) self-reference, (iii) impredicativity.

Questions: Are they really demons? Whatever the answer(s): Just where are they in the result above?

Gödel does not go into (i). Readers should recall 'the' barber (in some hypothetical finite village) with its embarrassingly blatant abuse of the definite article. For the record, Weyl did not remember this in his comment on the paradox on p. 211 of his review [19].

(ii) Self reference came up in the review of Volume I; cf. the *Remark* there on p. 173 about psycho-analysts. (In the reminder above, it is the specialization of y to x.) Gödel ridicules the assumption—that self-reference is a demon—by a more subdued reminder (on p. 130): *every sentence (of a given language) contains at least one relation word.*

For the record, (by temperament and) by contrast with my earlier experience of traditional logic chopping I—continue to—find this example of the genre compelling. Readers interested in such matters can find more in Note 3.

(iii) Impredicativity is of course related to the broad topic of self-reference since, pedantries aside, it is about defining an object by reference to a totality containing it. So what?

Of course, such definitions do not fit the metaphor of knowledge growing like a tree: *any cycle vitiates any* (tree) *order*. For the agenda of this review, when the metaphor is on trial, it is an open—and main—question whether this conflict is evidence for or against it (and such other items on Gödel's agenda as his logical order(s) of priority). But—for those of us grateful for small mercies—it is relief that he raises at least the less demanding question:

Just where is there any impredicativity in Russell's paradox?

(The predicate used, $\neg y \in y$, contains no quantifiers, which could be said to 'refer' to a totality.) Gödel focuses on the *range* of the variable (y) itself, in other words, something left implicit in the logical notation. Incidentally, Cantor's criticism in his review [4] of Frege's *Grundlagen*, more than 15 years before Russell's paradox, also focuses on the indefinite range, albeit in different (medieval) terms.

Here too Gödel's (compelling) point, which does not use formal constructions, fits the academic tradition of philosophy; as, for example, in Kant's *aperçu* (A713): philosophy analyses and mathematics build up concepts. (The *aperçu* is, as so often, useful provided only it is *not* taken literally! There are plenty of mathematical analyses of concepts, and there were some at his time, too.) But a sound perspective, here, on logic chopping, requires the following elementary distinction, and above all attention to its neglected consequences.

Corrections of errors and contributions to effective knowledge. The samples (i)-(iii) correct errors; both in the traditional literature and on the would-be 'purely' mathematical side. It is a common place that it would be merely high-minded (and thus liable to be simple-minded) to assume that, in some given area of knowledge, the correction of errors *must* contribute rather than simply distract; more generally, that extended logic chopping must (help to) contribute; in A716 and A718 Kant ridiculed the assumption by the example of geometry and analysing the concept of triangle. One simply may do better by making a fresh start. But it should also be added—and this is illustrated throughout the review: errors are not automatically corrected by contributions (in the area considered); the latter do not generally 'speak for themselves'; not enough for the corrections in question. Readers not interested in more specifics should skip the following:

Reminder (about (ii), but with a shift of emphasis away from the 'self' in 'self reference'). As stressed on pp. 176–177 in the review of Volume I, contemporary mathematics provides a good deal of effective knowledge on *representing* one kind of thing by another, aka as choice of data. Such representations are then used to *refer* to those other things. Sometimes recondite facts about geometrical coordinates are used in that review for illustration, and also for comparison with Gödel's use of Cantor's numbering (= representation) of finite sequences of numbers by numbers. Incidentally, the parallel comes up again in Appendix I (footnote 2). As in the general topic of representation including reference it goes without saying that its mental aspects, for example, intentions involved in reference, strike the mind's eye most vividly. But it also goes without saying that this fact does not guarantee that those aspects lend themselves well to theory; tacitly, as always, by anything remotely like current means. Of course, we know a lot about them; it's just not theoretical knowledge. For reassurance:

Inanities about reference (in so-called theories of meaning), which 'identify' it *either* with its mental aspects or the software, that is, mathematical aspects of representations, *or* with the wetware (data processing in the brain) will fall into place at the end of the review. It would be premature to agonize over them here.

The next section has some samples from Volume II, which *combine* formal constructions and logic chopping.

3 Absolutes: a top priority in the logical tradition On pp. 150–153 Gödel emphasizes this aspect of such venerable notions as *definability* and *provability*. This is in sharp(est) contrast with Hilbert's scheme, which applies them only to some formal system or 'language'. The adequacy of such choices is left open (or at most paraphrased; cf. for example, pp. 122–123 of [18] in the case of 'completeness'). *Reminder*. Model theory, central to mathematical logic – today, not 45 years ago –, also concerns aspects ignored in Hilbert's scheme (models or structures), but not those stressed by Gödel. Thus model-theoretic definability is generally about, aka relative to, a language, and hence *not absolute* (in the sense used by Gödel here).

He introduces the topic by reference to (mechanical) *computability* and to its analysis by Turing. Quite explicitly – but so innocently that this too sounds absolute – he assumes that the (mathematical) concept of recursiveness itself derives its importance from its absolute character, that is, the independence of this definition (of computability) from any particular formalism. Now, computability evidently involves both definability and provability; by routine verification. So, after Turing's success Gödel proposes to go the whole hog, and analyse the absolute idea(l) of the two perennials above.

Correction of Gödel's flourish on p. 150, about Turing's analysis being a first in human history. Classical propositional logic too is – not only deductively, but also – functionally complete; in other words, the adequacy of its formalism is established. For the agenda of this review the correction above is less innocent than it may sound. actually on two scores.

First, it takes some of the glamour out of any logic chopping that may be used in establishing such absoluteness. Secondly, it shows that doctrinaire (formalist) objections to Gödel's proposal – namely, that the notions involved are 'essentially' relative to some formalism – are below any threshold of informed discussion. Once grasped these insights (may help to) shift the emphasis to –

naturally, more demanding—matters above threshold, for example, incompatibilities between the aspects, of definability and of provability, required by the logical orders of priority and by effective knowledge. But this is quite another—and outside the logical trade—only too familiar story.

To end on a positive note there is the following:

Cheerful news (from pp. 151-152). For one thing, at least for suitable variants of absoluteness and suitably adjusted expectations Gödel's remarks on definability, say, of sets of natural numbers have been checked; for example, for such variants it has been proved that only countably many are so definable (and naturally there is no enumeration that is so defined). Since this matter is raised in the editorial footnote *r* on p. 115, there is a little (autobiographical) detail on it in Note 4. In particular, incidentally like the whole of this section, no *attachment* to any ism is involved; it's just a matter of temperament whether you get attached to everything you fancy.

The second cheerful item is Gödel's own blithe disregard for his own idea(l) of absoluteness when he goes on to muse about—the possibility of a complete set of (what are now called)—axioms of infinity. Here, quite cheerfully, *completeness for the* **ordinary language** *of set theory* is meant. Again it is a matter of temperament whether, like (the older) Gödel, you like to 'aim for the stars'. Ever since my teens I have been told that, in this way, 'you may hit the moon' (and even long before there were astronauts I wondered whether this was really good advice to those who have a chance of actually going to the moon).

Be that as it may, a later generation had a few successes with a few axioms of infinity; in accordance with some, but certainly not all, elements prominent in Gödel's musings. The next section is quite down to earth.

4 Selected thoughts about sets: choice, comprehension, replacement They come mainly from scattered footnotes in Gödel's piece on Cantor's *CH* (pp. 176-187 with additions on pp. 266-270), which has been reviewed many times (and described as 'marred' in one of the reviews cited in Note 1). It would be futile to repeat this material here. But a few words on Gödel's own emphasis are in order lest the shift below cause unnecessary malaise; 'unnecessary' today, not 40 years ago when I at least knew nothing simple enough about it (or set-theoretic foundations generally) to dispel (anybody's) malaise.

Gödel's own perspective and the agenda of this review. His first order of business is the question whether the concepts used to state *CH* are well-defined, and, at least, his answer is in terms of 'some well-determined reality' (p. 181), aka philosophical realism. There are many, albeit partial, results which he relates to his answer. But, as for everything else in the world, this is not—and certainly does not remain—the only compelling emphasis; in fact, not even remotely so. (By the same token, the constructible sets also constitute *some* well-determined reality.) For the record, 40 years ago my (and my chums') malaise with Gödel's piece was simply compounded by all that heavy breathing about 'reality'; recalling *Hamlet*: The lady doth protest too much, me thinks. Today I can be more explicit; partly by reference to specific logical discoveries in the meantime. They show that the results about the continuum—established (and rewarding) in geometry—are even logically independent of *CH*. Probably, more convincingly, at least for those with broad research experience, there are *general reminders*.

When a question is of little consequence, in other words, has few consequences—of interest in the area considered, like *CH* above in geometry—it is likely to be both difficult *and* unrewarding to decide. In terms of (erudite) isms, the assumption implicit in Gödel's emphasis is tantamount to the most vulgar form of pragmatism: what exists is useful, here, in the commerce of ideas.

But just as at the end of the last section, here too there is cheerful news. Despite his unpromising general perspective Gödel's piece has some almost equally memorable points of obviously lasting use.

Terminology. Instead of Gödel's 'set in the sense of arbitrary multitude' or 'extension of definable property' (footnote 2 on p. 177), corresponding words familiar to readers of this journal are used below; in particular, sets in (segments of) the cumulative hierarchy V_α generated by the power set operation, constructible sets in L_α etc. Generally, 'kind of set' is used without agonizing whether the kinds involved are restrictions of some (more) general kind. In terms of the refrain about the growth of knowledge, here, knowledge of sets: without agonizing whether such a general kind functions like a seed or is the result of (growth by) accretion.

In any case throughout this section the emphasis is on sets in suitable V_α's. The emphasis has an obvious parallel in the case of *number* (in place of *set*), when axioms, for example, for rings or fields, are explained by reference to familiar or otherwise easily described kinds of numbers. As with any emphasis something is lost; for example, we have no parallel here for Conway's numbers-large-and-small [5]. We return to sets.

In terms of the properties listed among axioms of familiar set theories, all α or all limit ordinals α may be 'suitable', depending on which V_α have the property in question: for example, all V_α satisfy the axiom of union, for each limit ordinal α, V_α is closed under pairing, and of course under its generating (power set) operation. A few reminders, incidentally neglected in the volume under review, are salutary here before going into the properties in the heading of this section, let alone, into any problematic axioms of infinity.

Some home truths, half truths and untruths. Above all, in the first place, V_α for particular α are meant. Where conditions (and ways) have been spotted for extending results to 'all' ordinals, it is sensible to do so. It is not sensible merely because one 'wants' to; cf. Dirac on sensible mathematics [6]. A *rough* parallel is a child's experience with finite α on the one hand combined with general, aka indefinite properties on the other; pedantically, of indefinite extension (not in the sense of p. 3 of the Volume).

But parallels between V_ω and V_α, for specific $\alpha > \omega$, generally reach the point of diminishing returns quite soon; much sooner, as it were, than those between, say, $\mathbf{Z}$ and the rings of algebraic integers in number fields (cf. Appendix II).

Samples. (i) In V_ω—what since Cantor are called—cardinals and ordinals satisfy the same arithmetic laws, but not beyond. While, for cardinals a, $2^a > a$ holds generally, for each n, $1 < n < \omega$: $2^a > a^n$ is a consequence for all $a \geq \omega$, but not for all $a < \omega$. The *GCH* ($2^a = suc\ a$) is false in V_ω, except for $a = 0$ and $a = 1$, but it would be as pathetic to draw any conclusion from this about V_α for $\alpha > \omega$ as from the fact that 1 is weakly, but not strongly inaccessible. (ii) V_ω, like everything else in the world, has many descriptions, all of which can be generalized (again like everything else in mathematics); but not necessarily to V_α for $\alpha > \omega$. Thus V_ω is generated from $\varnothing$ by: $x, y \mapsto x \cup \{y\}$. In old-fashioned terms this satisfies *l'esprit fin* (of algebra or its infinitistic analogue in L) in contrast to *l'esprit géometrique* of the (impredicative) power set operation. Thus ω has already 2 descriptions as the closure ordinal of each of those generating operations, of course, without (the usual) accumulation at limits.

For the metaphor of a tree of knowledge there is, as always, some (logical) order of *evidence* or at least of *justification*. But its use (p. 177) or, for that matter, niggling about it (p. 145) simply distracts from effective knowledge of those phenomena in familiar experience that have the labels above (and are genuinely in demand in the commerce of ideas). Specifically, the logical order imposed on descriptions—where one is privileged as a definition, and the others are deduced—produces artifacts; certainly for the historical order. For example, the Greeks managed well with their geometric (impredicative) descriptions.

Reminder. The reminders above, including the last one about effective knowledge—serve to—correct errors. They are not contributions to effective knowledge (of V_α for $\alpha > \omega$). But, compared to the logic chopping in earlier sections, they rely more on mathematical constructions than on other thoughts. (The latter are meant as in *Tractatus* 6.21, on p. 164 in the review of Volume I.[1])

(a) *Axiom of choice*. Footnote 2 on p. 177 recalls that it is valid for the V_α's (there called 'arbitrary multitudes'); actually, at each α, at least in its multiplicative version. One flourish in the footnote—about its being 'exactly as well-founded as . . . the other axioms'—has just been alluded to; realistically, it is *more* evident than, say, replacement; cf. (c) below. (Historically, it was used freely; to be compared to axioms for *order*, which are used in Euclid, but not stated there either.) Secondly, Gödel notes that it also holds for L, in other words, for sets defined from the ordinals by familiar logical operations and accumulation. This flourish misses a couple of opportunities:

Logical hygiene concerning the sets, say, in $V_{\omega+1}$, which are definable in the absolute sense adumbrated in his bicentennial lecture (where this topic is presented as a first order of business). Now, whatever doubts there may be about the scientific sterility of this sense there is no doubt that it is among the first thoughts that cross anybody's mind. For those sets the axiom of choice is quite dubious. Viewed this way, the fuss about choice is not merely thoughtless and thus simply embarrassing (as it is in the bulk of the literature).

Reminder concerning the contemporary sense of the word *axiom* (for the motto: *dégager les hypothèses utiles*). Suppose the property P satisfies some general conditions which ensure $\exists xP$ by the axiom of choice, and—the lemma, as it were—$\exists xP$ is enough to infer Q. Then, given a proof of Q from $P[x/f]$ with some more or less elaborate definition of (a choice function) f, the motto requires *either* the use of the axiom of choice *or* some rewarding strengthening of Q which follows from $P[x/f]$, but not from $\exists xP$ alone. The traditional preoccupation with the validity of choice for airy-fairy notions of sets distracts from the sensible use of 'axiom' above.

Viewed this way, well known conservation results applying to Q of suitable logical form are in accordance with the motto; at least, when utility for formal derivability is meant.

(b) *Comprehension*; cf. footnotes 12–14 on p. 180 about—what is there called—the operation 'set of x's'. For the record Gödel did not object to the use which I make below of those footnotes, and made in many conversations with him; but cf. (ii) on p. 158 of [18] before drawing any conclusions about his views (even at the time). In a nutshell the use is—not, of course, to agonize over paradoxes but—to find a memorable interpretation of the literature: on Frege's oversight, which Cantor called '*unglücklich*' (an unfortunate idea, that is, $\exists y \forall z[z \in y \leftrightarrow P(z)]$) and on Zermelo's $\forall x \exists y \forall z(z \in y \leftrightarrow [z \in x \wedge P(z)])$, which has superseded it. Mere (mathematical) survival certainly does not depend on reviving Frege's fossil in the evolution of ideas. The (cl)aim is merely about a *suitable* place for it in a museum of them, if we can afford it in our age of intellectual affluence.

Zermelo's clause '$z \in x$' gets a memorable interpretation in Gödel's thought of—here, y being—a set of x's. Actually, in conversation I went the whole hog, and interpreted '$z \in x \wedge P(z)$' as a *property of* x's, too. On the other hand, rightly or wrongly, the relation to Cantor's sense of *definite* (applied to extensions of properties, here, to elements $\in x$) was to me too obvious to require mentioning it (to Gödel); as opposed to: undetermined for some particular z. After all, in connection with impredicativity in Section 2, he himself had drawn attention to the (indefinite) range of the membership relation used in Russell's paradox. And if this relation were undetermined in set theory, what on earth should be determined there? Be that as it may, comprehension holds for all V_α.

Reminder on the other kind of property (not 'determined without arbitrariness'; cf. p. 3). Suppose, say, a quantified, possibly first-order formula of ordinary set theory is presented as a definition of P for some given $x \in V_\alpha$, but without stating the range, say V_β, of the quantifiers in the formula. In general, the property so defined does depend on β, and if the choice of β is regarded as arbitrary, the property is indeed not determined without arbitrariness.

(c) *Replacement and (uncountable) strongly inaccessible cardinals.* By (Zermelo's) [20], for infinite α, V_α satisfies replacement for arbitrary, in other words, second-order, functional relations iff α is strongly inaccessible; naturally, well defined relations are meant. (Incidentally, in [20] Zermelo uses the homely words 'definite property', but without would-be erudite explanations (on p. 3), to be compared to the use of 'finite' without any ritual of set-theoretic definitions.) Now, the replacement axiom differs in many down-to-earth respects from the others, in particular, form choice and comprehension. For one thing, it is *not true for all* α; in fact, not for any α ($>\omega$) usually encountered in the commerce of ideas, where it is, realistically speaking, an axiom of infinity. For another, its first-order and second-order versions differ in a more brutal way than that illustrated in the *Reminder* at the end of (b) above. There simply are accessible α such that the former version holds for V_α (but of course not the latter). As to Gödel's own perceptions of evidence they required education over several years after his correspondence with Zermelo in 1931. There he alluded to his malaise with [20], but could not (politely) pursue the matter since Zermelo did not take him up on it. At least according to what Gödel told me, he was ill at ease with replacement. This malaise had a practical consequence for him in his work on the constructible hierarchy. He had the general idea for his proof of *GCH* for L as a student, and even lectured on L in 1936. But with his malaise he hesitated to use von Neumann's ordinals of the required high types, and, without them, tiresome definitions of well-orderings are needed up to ω_ω in $V_{\omega+\omega}$ (cf. p. 31). This delayed the final publication for a couple more years; until he had satisfied himself about inaccessibles.

For the record I once asked him for his later thought(s) on these things. His answer—those inaccessibles are implicit in the concept of ordinal—was to me a reminder: the series of ordinals is—conceived as—absolutely unending. I did not have sufficiently specific questions to have a chance of getting much from any specific answers (he might have). So I did not pursue the matter at the time. Only later did I see in the many Pyrrhic victories of set theory in general, and especially of set-theoretic foundations, a *fertile subject of cultural interest*; too late to become sufficiently steeped in it for genuine interest, let alone, contributions. But much less is enough for the following:

Correction of errors; actually at two extremes. First, so to speak on the negative side, not every instance (of replacement) that holds for a particular V_α, provides support for the axiom itself. For example, $\alpha = \omega$ does not; not surprisingly, after what was said in the *Samples* above about (other) parallels with ω. If $\alpha = \omega$ then, for arbitrary functional relations restricted to V_α, domain and range are finite. Thus if the range consists of ordinals it actually contains its supremum. But accessibility of α can be problematic only if the supremum of a set (of x's $< \alpha$) may exceed all elements (and be $= \alpha$). Strong accessibility involves the exponential function, which is not well understood for $\alpha \geq \omega$ (in contrast to $\alpha < \omega$, from which fact the literature both on computation and on certain non-standard models of arithmetic distracts). In simplest terms, in the present case, parallels with $V_\alpha : \alpha > \omega$ are spoilt by incomparably greater experience with V_ω; including the impredicative knowledge of facts about V_ω, which are described by use of the idea *finite*.

At an opposite extreme is the superstition that replacement is suspect merely because it is a kind of (infinitistic) 'closure' condition, and hence tainted by Frege's 'closure' condition on properties and sets: $P \mapsto \{z : P(z)\}$. It is familiar that Frege thus obliterates the traditional distinction between properties and sets, which applies the membership relation only to the latter; Cantor spoke of 'arbitrary varieties' in contrast to those 'grasped as unities'. Pertinent, but less familiar are the following:

Reminders. (i) Some 2500 years ago (in *Physics* III 6, 206b 33–35), Aristotle made a distinction, admittedly in clumsy terms, under the heading: the infinite. (It is exactly the opposite to what is [tacitly, sometimes or, perhaps, usually] said. The infinite is not that which admits nothing outside itself, but that which always admits something outside [each part] of itself.) Applied to strong inaccessibility, viewed as an axiom of infinity, this emphasis on 'parts' corresponds to the use of *strict* order:

$$\text{If } \beta < \alpha \text{ and } \gamma < \alpha \text{ then } \beta^\gamma < \alpha.$$

This would be obviously false with $\leq$ in place of $<$. (Frege's condition does not have a corresponding bound.)

(ii) Some 100 years ago much attention was given to a particular class of closure conditions, called 'from below' or 'inductive'. These do admit equality, with a *least* fix point (and others 'outside' it).

To repeat what cannot be repeated too often: (i) and (ii) correct elementary

(= brutal) errors, here, dubious doubts, about inaccessibles, but are not enough for contributions.

Remarks on what may be lacking. First, what is *not* lacking is progress in our understanding of inaccessibles, specifically, in the last quarter of this century. Thus Solovay's use of them for defining kinds of sets (= models of set theory), for which every set of reals is L-measurable is simply qualitatively more substantial than any earlier material. Even more directly pertinent is Shelah's demonstration, as it were in the opposite direction: how knowledge of L-measures for certain projective sets provides new descriptions of ordinals, which establish the latter to be inaccessible in L; to be compared to the impredicative knowledge about ω used in the discussion of V_ω above. Secondly—at least, for the market of which I am representative—what is certainly lacking is an analogue to Gödel's *felicitous expression* 'set of x's' for a *thought* that supplied what was previously lacking in connection with comprehension. Incidentally, when work on axioms of infinity began some 30 years ago, I was, obviously wrongly, convinced that, *at a minimum*, it would lead to an adequate expression for a correspondingly adequate thought in connection with inaccessibles. Thirdly, this last and similar defects may be connected with a lack of a sensible perspective; specifically, in the emphasis on the (logical) need for axioms of infinity for new results, which distracts from the market for their *uses as a better bargain* (in competition with possibly already existing proofs). The standard example is Cantor's cardinality argument for the mere existence of transcendental numbers. This remains a better bargain than the specific transcendental $\sum 10^{-n!}$, which Liouville had produced 10 years earlier; with more work, for a very limited market. For example, when Martin gave his proof of Borel determinacy by use of—what is realistically—an axiom of infinity, the emphasis was *either* on the theorem itself *or* on the logical need of the axiom for proving it; overlooking the *main* novelty of Martin's product: the use of higher cardinals for something that at least remotely resembles some things in demand by the market in question.

So much for Gödel's writings on sets. Readers who have persevered so far may benefit from the following:

Recapitulation. Gödel's titles in the 30's generally emphasized aspects prominent in Hilbert's metamathematics, and the detailed work pursues this emphasis. For reference in Appendix I.1: This applies also to the incompleteness paper, where formal undecidability is emphasized, not, for example, the alternative: provability $\neq$ truth (not even in arithmetic).

Gödel's essays in the 40's shift that emphasis; specifically to traditional idea(l)s of a logical order of priority in general and of a venerable *ism* (realism). This emphasis specialized—most prominently, to compensate for formal undecidability—to the topic of large cardinals, which, by the ism, require an even narrower focus on 'some well-determined reality'.

The review follows Gödel's *shift of emphasis away* from Hilbert's so-called formalist aspects, but with a difference. There are constant reminders of *alternatives* to Gödel's own shifts; alternatives both to the traditional ideals and to Gödel's specific topic, an opposite extreme as it were to (Hilbert's) finitism. In terms of isms and anti-isms, for example, anti-realism, the emphasis in the review is on the *anti* only; in other words, on *not confining* attention to aspects prominent in (any) traditional isms.

Correction of (my) bias. I have attempted to take into account the following background, touched on briefly already. Gödel's essays in the 40s had blinded me so completely by their—to me still atrocious—opening fanfares that, 40 years ago, I declined von Neumann's proposal (via the geophysicist Bullard, for whom I had done some work) to visit the Institute at Princeton for contact with Gödel. The visit would have interfered with some plans for frivolity, which was—as I saw things at the time—more rewarding. Fortunately, actually at the *ICM* at Amsterdam in 1954, I was reassured by a friend, whose views I had found compelling for more than 10 years and who had personal knowledge of all parties concerned. When I met Gödel in 1955 I learnt to see—what still appear to me—gems in those essays; admittedly, perhaps brighter than they are against those 'philosophical' fanfares as a foil. But above all I very soon discovered to my delight—and admittedly again possibly all the greater by contrast with expectations—that, in practice, he took a very catholic view of his pet ism: it did not conflict with giving intuitionism, the subject of the next section, a whirl, too (cf. pp. 104–120 of [18]). You trust in God—represented by philosophical isms—and keep your powder dry: by being both realistic and constructive (in the popular senses of these words).

5 Intuitionistic logic: hitting a (little) moon first, and then dreaming of the stars (pp. 240–251 in both German and English, repeated on pp. 271–280 with additional, particularly ethereal notes). By the agenda of this review this kind of logic is viewed here like the kinds of set in the last section: without agonizing whether any general notion of logic is a seed for some tree of knowledge of which our kind is a (sturdy) branch, or whether any such notion has grown by accretion from components, among which our kind remains a visible entity. For one thing, future research may have something to say about these two options. For another, (premature) agonies about such options are traditional, and those agenda require emphasis on alternatives to the traditions involved.

Reminders. First, there is another kind of logic, which is meant for propositions without incompletely defined terms and with the property that they are either true or false. These aspects were considered by Aristotle; quite explicitly, as necessary to make propositions rewarding objects for study. Since the latter was his trade, these aspects were indeed essential. The sanctimonious expression 'of the essence' is apt if trade interests are sacred (whatever the actual usage of that expression may be). Now, it is a simple fact of life that mere truth is often a distraction, for example, for the accused who—knows he—is guilty (provided only the law happens to consider him innocent unless proved guilty). His business is not truth, but an *irrefutable* defense; formally, double negation is weaker than truth. Intuitionistic logic, which ignores truth (in favour of evidence) altogether, provides a coherent scheme for a corresponding interpretation of the logical particles. It therefore also provides a literary form for underlining that first order of business for the accused. As always it is a separate question how, if at all, theoretical elaborations of intuitionistic logic contribute here. At any rate as always it would be a philosophical mistake to assume that all (effective) thought must be theoretical. Though I can see the broad interest of the rhetorical aspects touched above I am not sufficiently familiar with them to be interested, let alone, to report on details. The second reminder is more parochial (but less than higher set theory in the last section).

In mathematics too, we generally know more about propositions than whether they are true or false (and sometimes want a vehicle for expressing such additional knowledge); most often, *how* they depend on parameters, aka functional dependence in this trade. This is obvious in the case of $\exists xP$ and $p \vee q$, with some (explicit or implicit) parameter, but as a moment's thought shows, also with $p \rightarrow q$. Now *one* option is to make the dependence explicit, when the logical particle involved is simply eliminated. Another option is to adapt the logical laws and thus ensure that (logically proved) theorems are subject to *suitable* dependencies: Logicians are familiar with such dependencies from various kinds of reducibility in recursion theory. So much for background.

In a lecture on 15.IV.1941 (mentioned on p. 217, but not included in this volume), Gödel asked: *In what sense is intuitionistic logic constructive?* Here he meant, roughly, the second option; to be precise he had to explain what dependencies are meant in the case of logically compound expressions like $(p \rightarrow q) \rightarrow r$, which do not occur in ordinary mathematical thought (but do occur in formal systems). For simple expressions $p \rightarrow q$ his option involves different (familiar) reducibilities according to the logical forms of p and q; roughly, one-one reducibility if they are in prenex form *without* alternation of quantifiers (in other words, Skolem functions of a suitable prenex form of $q \vee \neg p$ are used), but not generally. He meant 'constructive' in its usual mathematical sense; with emphasis on the (functional) dependencies, as above; less on the means of showing that the functions do what they are supposed to do. In the example above, of $\exists xP$ with parameter a and an explicit function X (of a), this would require a proof of $P[X(a)]$ (for the range of a considered).

Corresponding to iterated implications, as in the example of the last paragraph, for which intuitionistic logic is notorious, Gödel had operations of higher (finite) type. His answer to his question was an impeccable interpretation of Heyting's arithmetic, a most familiar system of intuitionistic logic. The definitions of the operations used have a very familiar look: except for the higher types (and the corresponding conventions of a typed λ-calculus) the whole scheme looks just like primitive recursive arithmetic. In terms he used 10 years earlier, on incompleteness of the familiar system of *Principia*, the interpretation is easily seen to be typical of 'related systems' (e.g. on pp. 236–240), and so enough for the general picture below.

A pyrrhic success: corollaries for the foundational ideal (in other words, for the metaphor of knowledge growing like a tree). The two outstanding facts here are, first, that Gödel's scheme provides definitions for the operations in the mainstream of constructive mathematics involved in the second reminder above, and secondly that it has not contributed to that main stream (cf. footnote *o* on p. 238). Incompleteness results distract from both those facts. Now with both the scheme and that mathematical experience before one, one can also see *what is lacking* in the former, specifically, concerning its 'functional dependencies'. By experience *separation* of some, albeit relatively few different kinds, is necessary (for significant results), while the scheme concerns what they have in *common*. Refinements, for example, according to the syntactic form of the definitions produce—of course precise—results that are not significant for mathematics; in contrast to such classifications as algebraic or topological dependencies. Now this property of the scheme has a perfect parallel in the common consent among experienced mathematicians about set-theoretic foundations (as being the 'least interesting side' of the business); of course, also where the latter are amply complete. As a corollary, this *neglect of significance* in classifications is not peculiar to any particular foundational scheme—or even ism, with which it may be connected—but is part of the foundational ideal.

Gödel himself does not touch this foundational side. But starting in his first paper (in 1958, in contrast to the lecture; cf. footnote *a* on p. 217), and especially in his later additions, his (cl)aims concern quite different aspects of his scheme: relations to such traditional idea(l)s as *reductive proof* (in footnote *h* on p. 275). Contrary to p. 219 it is not particularly hard to elaborate such matters, as in Note 4 for other traditional notions of proof, and thus 'formulate the philosophical gain achieved', when the 'gain' is measured by the canons of academic epistemology. This leaves open what gain, if any, there is for a more realistic view (of knowledge). Be that as it may, all this shows vividly that, for good or ill, Gödel's attachment to his so-called Platonism did not keep his hands off other isms.

Remark. Abstractly, there is a staggering contrast between Gödel's (i) acuteness and imagination in seeing and exploiting logical (and other mathematical) aspects of quite hackneyed idea(l)s, and (ii) blithe disregard for general scientific experience where the idea in question has proved sterile or false. An extreme case is the idea of God being a mathematician, a (hackneyed) way of saying that spectacular mathematics must be—a guarantee for—'truth'; of a particular interpretation to boot. The example of the theory of a complex variable, which is also the theory of ideal liquids in 2 dimensions, is often quoted. (The ideal of reductive proofs above is a candidate, too; specifically, Gödel swallowed the 'reductive' interpretation of Gentzen's famous results mainly because of their obvious mathematical wit.) But—and this too is a fact of experience, not a mere possibility—Gödel is by no means unique in combining (i) and (ii). After all, (ii) is a simple attachment to ideas learnt in one's teens, when one often really has too little scientific experience to correct them convincingly (cf. *A brief history of time* by Hawking, who wrote an editorial note for a couple of items in the next section, with many memorable examples combining (i) and (ii)).

6 Cosmology and some (even) more ethereal ologies This section has to do with pp. 189–216 around a previously neglected type of cosmological solution of Einstein's equations for gravitation. Gödel's three short papers date from around 1950. For readers with a general mathematical education the knowledge of differential geometry needed here is no more demanding than the logic in some of Gödel's other papers.

On p. 161 Gödel lists some properties of his solution, in particular (b): There exist closed time-like lines (though every world line of matter is an open line of infinite length). Both Gödel (pp. 202–207) and the editors (S. Hawking and H. Stein) treat the solution—quite solemnly, albeit in different literary styles—as a contribution to effective knowledge; of the nature, as one says, of time. For the present (re)view this is below threshold before looking at alternatives, for example, the following:

Analysis of language. This dread idea of the academic tradition is here applied to the language of (theoretical) sciences, in particular, the mathematics of differential geometry. Viewed this way Gödel's solution unquestionably corrects the neglect of solutions of his type. So what?

(a) *Singularities*. Gödel's type has none; admittedly, this fact is not listed in (1)-(9) on pp. 190–191. But also, at least since the 60's there has been interest, in particular, by Penrose and later also Hawking, in the matter of singularities in solutions of Einstein's equations. Whatever else may be in doubt, if something is to be *proved* about classes of solutions with singularities, Gödel's type has to be excluded. If nothing else it is a *complement* to (later) results about such classes. In contrast, it would be simple-minded to assume that it must have (had) so-called heuristic value; 'simple-minded' by overlooking, for example, the possibility that the type was excluded tacitly. Downmarket Hawking's imaginary time, used in his answer to questions about where the universe 'comes from' (the universe just is), gives the flavour of such complements; more cheaply, since it is familiar that his switch turns hyperbolic into elliptic equations, which are much tamer.

For the record the later work has been supplemented by yet another type of solution without singularities, which satisfies most conditions prominent in earlier singularity theorems, but not, for example, the convergence of light rays (cf. [15] or the breezy account on p. 201 in *Nature* 17.V.1990).

(b) *Combinations* with—equations for—other aspects, besides gravitation, of the phenomena considered. This matter is prominent in the common-or-garden varieties of science, for example, at the beginning of Newton's *Principia*. The development of rational mechanics in the 17th and 18th centuries produced many examples of such combinations (with his equations for gravity, where actually solving the combined equations is another story altogether). Attention to problems arising from such combinations is one particularly striking difference between practice in the common-or-garden varieties of science or, in fact, thought generally, and in those would-be all-encompassing schemes, which (cl)aim to leave out nothing (with which to combine aspects privileged in them). Foundational schemes are of course chemically pure specimens of this idea(l).

It is by now a common place that relativistic—requirements on—equations are hard to combine with others. Dirac's equation for the electron, respecting *special* relativity, became correspondingly famous. Gödel's solution presents itself as a new type of **candidate** *for object lessons* on such combinations; at least by analogy with my own experience, as in the following:

Digression for readers who (still remember some rational mechanics and) are interested in 'objective correlatives' to the *raw* interest of Gödel's solution. The assumption is that the 'ends of time' are irrelevant in at least some situations where the combination in question is a main problem. Given that not much is known about them, what options (as always, if any) are there? One, of course, is to leave those 'ends dangling', another is to 'glue them together'. This has nothing to do with conventionalism or any other ism. In either case some key element may become visible, that is, some (memorable) obstacle to combinations and, with luck, means of removing it, which then opens the way in other situations, too. Incidentally, the obstacle may be—later seen as—a blind spot. Obviously, nobody (in his senses) with my limited experience here would be tempted to pontificate about cosmology. But there are the following:

Easy parallels. Jets in rapid motion, producing cavities, often disintegrate into turbulence (about which not much is known, and the details of which pretty obviously have little to do with the general motion). Experience has shown that, for a successful theory of those broad aspects, the 'ends' of the jets are sent off into a different Riemann surface. But also – still in connection with rapid motion with forces that are very much greater than gravity – the combination with gravity may be *dramatic in permitting a new type of solution*. Specifically boundary conditions, which now determine a solution, have none – for equations – without gravity; for example, an infinite jet with a free surface (at the top) deflected by an infinite plate partially immersed (below that surface). Incidentally, there is a recent claim by Motz and Motz [11] about a similar story for equations of the photon.

Reminder. The points above are – meant to be – *alternatives to the solemn tradition*, and, in fact, anathema for it, especially the motto: nothing but the universe is good enough for us.

For the record the price paid for this motto seems (to me) fair enough: it generally spawns work that, at best, corrects errors rather than contributes to effective knowledge. Exceptions exist (often by Big Science, cf. Note 2), but they tend to be quite costly by any realistic account(ing).

Sundry tit-bits: old and new. Aristotle's primary (measure of) time is cyclic; cf. *Physics*, Book 8, 265a15. His student Eudemus mused about time being cyclic too. They do not seem to have agonized over effects possibly preceding causes (for a local direction, as in (4) on p. 191); perhaps not surprisingly. For one thing Aristotle has little to say about temporal causes; for another, neither of them discusses the possibility of going back in time. Still, I had hoped that some sophist at the time had objected: What happens *if* you go back and kill your father when he was a baby? Better still: And what if East ever met West? Alas, even if anything of such pastimes among sophists is known, it has not come my way.

Fortunately, in this volume we find the question above on p. 189 (but not the bit about the 'twain') by Hawking, solemnly shown by Stein to be a less decisive objection than his brasher co-editor clearly assumes: cf. footnote a on p. 199. To be precise, Stein considers a variant, where you go back and, more simply, murder your own former self; pedantically, your 'spirit' goes back and 'murders' its former self (since, as mentioned, world lines of matter are open and of infinite length). Incidentally he does not refer to p.189 (nor apply a little hygiene by use of ordinary analysis of language). It's all good, clean fun for us more cheerful readers, whatever the editorial intentions may have been. The next and last titbit is a little different. It is about the following:

Markets for literature concerning the universe. As far as ordinary commerce, say, of the book trade is concerned, the facts are striking, and I certainly have nothing to add by way of interpretation. The commerce of ideas is a different matter. At one time venerable theologians constituted an, as it were, captive market. Times have surely changed, but, perhaps, not as much as suggested by C. Sagan in his preface to *A brief history of time*. He considers pre-school children who ask: Where do we come from? He envisaged an answer in cosmological terms; perhaps, the Big Bang (unless 'the universe just is', like the Creator according to theologians when asked: Who created the Creator?). I have heard it said that the idea of quite ordinary bangs, which do not even move the earth for those involved, also finds a market among pre-school children.

It is time to return to Gödel. Related topics came up in our conversations, as I have reported elsewhere; cf. p. 145 (iii) or p. 150 (a) of [18]. His musings were not as coarse—in either sense of the word—as the last paragraph, but they did not consist of solemn logic chopping or erudite references to the ancients either. He just had a general interest in—what were called—ethereal ologies in the heading of this section, for example, theology itself, but also pneumatology (not only of the Holy Ghost, but of ordinary ghosts too) and demonology. Gödel's (broad) interest in these matters is common enough; cf. p. 150 (a) of [18]. For the record, what I find most *satisfaisant pour l'esprit* is the parallel to experience with the logical 'demons' of section 2 (infinity, self-reference, impredicativity): a kind of 'clutching at straws'; cf. Note 5. As would be expected from the broad philosophy of this review, it shifts the emphasis to different aspects; cf. Note 6.

So much for (the main part of) Volume II.

Among the remaining parts there are some complements to Volume I, which are best reviewed in a different style; cf. Appendix I. To conclude this review we go back to the beginning of the earlier review about 'principal questions, which could not have been answered at the time'; but now applied to both volumes.

7 What was lacking? (60, 40 or even 20 years ago). Pedantically, 'lacking' concerning the main refrain of this review about alternatives to the idea(l) of a tree of knowledge growing from (logical) seeds.

To start with, there was just lack of logical experience, in particular, of what else to do (with knowledge of logic; besides growing and trimming trees in logical foundations).

By mid-century one had elementary results, which—at least, when used with discretion (and in sometimes imaginative combinations with more specific knowledge)—contributed effectively. The most striking example remains mechanical computation; naturally, more for what can be done with it than for its limitations. Malcev had published other such combinations about 50 years ago (cf. p. 158 of [18]); but, for the record, nobody had drawn my attention to them even 40 years ago. (I had found for myself some significant combinations with proof theory.) The next two items are less commonplace.

40 years ago certainly those of my chums who took any interest in logic at all, had a view of scientific knowledge that was still dominated by expositions of the relativity and quantum theories according to the ideal of a tree of knowledge (or, in Dirac's *tour de force*, almost along a single branch!). Not even Bourbaki's scheme—of *relatively few* basic structures to be used for *very many* combinations—was presented (by its authors) or recognized (by us) as an alternative to the foundational ideal. Besides, it was not familiar enough (to us at the time) for its effectiveness to be seen (by us).

20 years ago alternatives to that idea(l) had become spectacularly visible through scientific experience. Molecular biology, full of brilliant thoughts, just isn't a theory according to the stone-age ideal. Big science—naturally, by Note 2, when used with skills closer to big business than those in demand by crafts and guilds—had combined successfully, as it were in parallel, ideas, people and technical apparatus. (For the record, 40 years ago it had seemed to me that people would merely get in each other's way in such enterprises.)

Without exaggeration this experience corrected a parochial idea of *understanding*. New possibilities were established, to be compared to the discovery of new kinds, aka concepts, of *solution* (in mathematics). But applied to logic, at least for most, this broader view merely shifted the emphasis *away* from the foundational ideal. One tied up loose ends, by solving (clearly formulated) familiar problems, and combined logic with more substantial mathematics; in commercial terms: with richer resources.

During the last 20 years—even to the outsider—spectacular events in the commerce of material goods and services have shifted the emphasis to aspects that have obvious parallels in the commerce of ideas. In particular, (genuine) *new markets*—beyond, as always, new bandwagons—have become prominent, which, previously, were genuinely marginal or simply ignored by piety towards traditions of the trade (usually shared by management and unions alike). One example is applied to logic on pp. 168–169 of the earlier review in its peroration under the heading: *For a better quality of life*. It is suggested by the discovery that attention to pollution can make—not only for legitimate, but—rewarding business. Here it should be added that it is even more promising in the commerce of ideas. To use (again) the words of Marx, but with opposite emphasis: in contrast to the case of material pollution, a change in interpretation is sometimes enough to change the world (of ideas).

It would not do to end on this note of smugness, as if events in the last 60 years belonged to the best of all possible worlds (of ideas). It would be a missed opportunity—at least for interpreting the past, even if we forget it when it is needed in the future—not to mention the following:

Reminder. Just think of any—thing that strikes the literal or the mind's eye as an—object. Every relation to anything else—not only to what strikes the (mind's) eye as a part—is a property of that object.

When this home truth, repeated in different words for > 2500 years, is remembered, the *broad* ideal of a tree of knowledge is seen as little more than a blind spot; even when only knowledge of the object above is meant. The same applies to related ideals, for example, of a *complete description* (as a seed for that tree). *Remark*. This ideal is not logically defective since it is realized impeccably in Peano's or Dedekind's axioms. They relate the objects considered, the number series generated by the successor, and the field **R**, to all objects in the universe of sets. But by experience the ideal is sterile here; specifically, compared to the alternative of focusing on *suitable incomplete descriptions*, aka abstractions.

For any remotely realistic sense of 'sterile' the qualification 'here' is *obviously* necessary. Euclid's presentation of (his) knowledge of geometry in the form of a tree is not only (eternally!) legitimate. It also had a market among educated Greeks of his time, and had a good run as a blue-chip-investment in the commerce of ideas.

The literary forms of mathematical logic are well suited for *illustrating* diverse possibilities by memorable examples without necessarily contributing to effective knowledge, too. For instance, examples of abstractions *can* be concocted, of which—as Plato's translators say—**R** partakes, but this is best proved by use of Dedekind's axioms and recondite properties of sets. (There are corners in the market for highly touted independence proofs, which establish a *logical* need for such properties.) There is also the separate fact of experience that such examples are *not encountered* often.

The metaphor of a commerce of ideas would be very weak indeed if trades supplying such properties did not advertise their concoctions, and others not dealing in them did not huff and puff. Those others need not be established trades; often they are vendors of other seeds, peddling theirs under labels like 'cybernetics' or 'information theory' (instead of the more venerable logical variety).

Appendix I: Some remarks on the undecidability results

Gödel's (3) remarks occupy barely two pages (pp. 305–306), the editorial notes over twenty (pp. 281–304), but without emphasizing the following facts: (a) Each of the remarks contains, more or less explicitly, some simple point, which, for >50 years, has proved to have—what is called in current mathematical jargon—foundational significance, but is not prominent in the literature. (b) Each of them also contains, more or less implicitly, one of those mind-boggling assumptions of the heroic tradition, on which the main text of this review has focused.

Evidently, (a) and (b) illustrate the contrast remarked on at the end of Section 5. The editorial notes contain some gems (of breath-taking solemnity) in the general area of (b). But even they are probably easier to take (in) after reading the review below.

Terminology. In the first two remarks 'undecidability' applies to particular formulae and their (formal) independence, as in Gödel's incompleteness paper, in the third to classes of formulae; in other words, it is (Turing's) unsolvability. To repeat what cannot be repeated too often (from the review of Volume I): The mathematics used—such as diagonalization—is not only exceptionally simple, but closely related. But the choice of notions and problems in further elaborations is very different in the two cases. Also, by p. 164 (*loc. cit.*), Gödel's pearl does not turn up in the ordinary mathematical literature, while Turing's twist has an established place in, for example, the subject of finitely generated groups.

The titles of the subsections below are Gödel's own (from his remarks, which are footnotes to material in Volume 1).

1 The best and most general version of the unprovability of consistency in the same system With this would-be dramatic title—as with the fanfares about the priority of logic for all science and of the cardinal of the continuum in Gödel's essays on Russell and Cantor's *CH*—the remark itself cannot help being *plus serieux*. Another (catchy?) title would have been (for him and in contrast to Section 1 of this review on would-be fundamental criteria): *How I never had the courage of my convictions expressed at Königsberg in 1930 about consistency as an adequacy criterion* (cf. p. 148 (a) in [18]).

Reminders. First, he pointed out there—tacitly, (even) in the particular case of formal systems for arithmetic—that, at best, consistency is adequate for (ensuring the validity of)—what we now call—formally proved Π_1^0 sentences; 'at best' because some additional condition such as—what we now call—Σ_1^0 completeness is (obviously) required. In current notation, also used on p. 179 of the review of Volume I, this restricted adequacy is expressed by: for all formulae $F \in \Pi_1^0 : (\Box \ulcorner F \urcorner) \Rightarrow F$.

Secondly, as also stressed in the review, his incompleteness paper provides a formula, say, G_0, which *is* $\neg\Box\ulcorner G_0\urcorner$, where $\ulcorner G_0\urcorner$ is his code for G_0. Pedantically, this is done for a particular system *PM*, a particular coding of syntactic objects and particular definitions of the syntactic relations involved. But never mind for the moment the flourish about 'the most general version' (with its innocent disregard of the most elementary conventions about the definite article). Note that (his) $\Box\ulcorner G_0\urcorner$ is in Σ_1^0-form.

Unprovability of $(\Box G_0) \to G_0$, and thus of the adequacy condition above (since, in *PM*, negations of Σ_1^0 formulae are, demonstrably, equivalent to Π_1^0). Since G_0 *is* $\neg\Box\ulcorner G_0\urcorner$, $(\Box\ulcorner G_0\urcorner) \to \neg G_0$ (in *PM*). So, if $(\Box\ulcorner G_0\urcorner) \to G_0$ were provable then also $\neg\Box\ulcorner G_0\urcorner$. Again, since this *is* G_0, G_0 itself would be provable.

Now a minimum condition on $\Box$, used in the incompleteness theorem, is that, if G_0 is provable, so is $\Box\ulcorner G_0\urcorner$. Thus $\neg\Box\ulcorner G_0\urcorner$ and $\Box\ulcorner G_0\urcorner$ would both be provable (and the system considered would be inconsistent). This is all; it would have been perfectly compelling in the early 30s.

Remark. By scientific experience—and contrary to the teenage idea(l) implicit in the flourish above—it would have been premature then to try and establish *suitable* generalizations; in other words, by a refrain from Section 7, relatively few that cover relatively many formal systems (in broad experience). If one tries, as people did, one ends up with inanities about 'natural' systems; cf. the end of Section 4 on p. 167 of the earlier review on the *obvious* poverty of all formal systems considered; poor for representing the phenomena familiar from mathematical experience.

Points to note today: first, by reference to experience with (pretty) provability logic. This uses heavily not only *both* closure under modus ponens *and* completeness for Σ_1^0 (specifically $\Box p$) formulae mentioned already; but also the provability of those properties of the system in itself. (It is an open secret that the contemporary trade of provability logic would be out of a job without these two properties.) The particular (unprovability) argument above, advocated by Gödel, *uses neither property*. This need not be the end of the story, at least not for those prepared to learn from scientific experience, as in the following:

Secondly, emphasis is shifted to the significance, if any, of formal systems that do not have those venerable properties (and thus to the significance of avoiding the latter for a particular result). Cut-free systems are—now, not in 1931!—familiar enough, the other kind less so (but cf. p. 178(c) of the earlier review). Let there be no mistake. This shift is *in conflict* with teen age ideals, especially, of a complete description for the essence of proof.

Thirdly, in terms of the metaphor on pollution, since gushing about (Hilbert's banner) consistency is still around, one may wish to give it attention, and, perhaps, thereby develop immunity. One option is to dot the *i*'s and cross the *t*'s by, first, considering different formulations (by Hilbert which—as he was never tired of emphasizing—are equivalent; tacitly, for systems that *do* have these two properties). Secondly, one lists other systems for which the formulations are not equivalent (a salutary preparation for Hilbert's many tacit or even glib assumptions in this area). All this was done in the 50s and 60s, but (in Gödel's words on p. 305) 'it has not received sufficient notice'. The editorial note (pp. 282–287) certainly contains information on these matters, which is easy enough to find there if one knows what to look for.

Remarks. The note is quite weak or, more precisely, clumsy about a topic that need not be belaboured here because it is treated at length in Section 1 of this review and on pp. 176–178 of the earlier review: *representation*, here of syntactic objects and relations. The fanfare at the beginning—about *how S is represented*—is a bit hollow without the fiction (at the end) about *canonical representations* presenting so-called conceptual difficulties (totally ignoring obvious object lessons from coordinates in geometry[2]). More specifically, in footnote b on p. 282, no attention is given to the *data determining (formal) proofs* or, equivalently, to suitable descriptions of these objects: not only the 'output', the proof *tree* itself, but also (the 'program' for) checking that it is a *proof* tree, is relevant to the questions considered there, and so has to be part of the data; cf. top of p. 179 of the earlier review.

So much for the editorial note. We turn now to Gödel's wilder side:

Legalistic (= debating) points. Here his concern is to 'refute' Hilbert (on the latter's terms) in a court that insists on the letter of the law and relies on precedents. For the record, as is evident from Section 1 of this review, I could not bring myself to do this, but I found the spectacle of Gödel at it simply enchanting. His concern fits the—to the modern reader strange—prominence in the remark of *provability in his equation calculus*, say $\Box_0$, and of the idea(l) of *primitive recursion* (in, of all things, a would-be most general version!).

Reminders. First, Hilbert had a thing about (formal) derivability. The harmless little word 'true' was banned, and not even applied to purely numerical propositions, say, $P(0)$, $P(1)$, ... Gödel respects this little whim by a rephrasing, permitted by a theorem of *PM*: for numerals 0, $s0(=1)$, etc.

$$(\forall n \in \omega)\,[P(n) \Leftrightarrow \Box_0 \ulcorner P(s^n 0) \urcorner].$$

Secondly, in the mid 20s Hilbert committed himself to Ackermann's function as *finitist* (meaning: privileged for the cheerful tradition, legitimate for the other). Now, Ackermann's function enumerates all primitive recursive functions, and so any attempt to make do with less would be teratological; even for that 'most general version' since, tacitly, it is meant for the refutation in question.

Disclaimer. As mentioned earlier, read with ordinary horse sense, Gödel's remark is easy enough to follow. But Gödel's refusal to use the literary forms of mathematical logic for the sake of very compressed solemn language interprets Ezra Pound's recipe for great literature—as being 'simply language charged with meaning to the utmost possible degree'—too innocently. I am not persuaded that the simple thoughts in Gödel's remark are rewarding subjects for great literature at all, let alone, in the particular traditional language favoured by Gödel. That's the way the cookie crumbles.

2 Another version of the first undecidability theorem The wording of this remark, including the title, may be a little strange, at least, if Gödel's poor health at the time (1972) is disregarded. But the thoughts (I read into or) in them have been long familiar to me: the simplest since before I first met him, the wilder ones from our conversations, even in his good old days.

Reminders. Ever since the 50s a little cottage industry has been busy classifying formal systems—pedantically, the corresponding theories (= sets of theorems)—according to their degrees of unsolvability; as in Turing's meaning of 'undecidability'. But also since that time this classification has been known to be insignificant—in the ordinary sense of statistics—for many parts of logic, which were prominent then (and have become more so); for example,—in modern notation—$I\Sigma_n$ for different n, that is, induction restricted to Σ_n formulae. Their theories are all of degree $0'$, but differ, actually even for their Π_1^0-theorems, prominent in the last section. (This was known then for $n > 2$, now even for $n \geq 1$.) Last but not least, these differences were established by—more efficient use of—the ideas in Gödel's own proof of the first undecidability result. Pedantically, this would not be called 'another version' of the latter. But a suitable version, which implies those differences as corollaries, is easily formulated by routine use of the literary forms of mathematical logic, and proved by means of those ideas.

Remark on academic etiquette. This emphasizes the fact that sometimes classification by degree is useful, sometimes by inclusion. It distracts from the (more demanding) thoughts needed to determine what significance, if any, either classification has in a particular area of experience.

Lack of this kind of thought, in the sense of footnote 1, spoils the wilder sides of Gödel's second remark; specifically, on such matters as *understanding*—here, of mathematical concepts and axioms about them—, *complexity* and, at least tacitly, *abstraction*.

Reminders. First, readers of the review of Volume I should recall p. 167, but also the end of its Section 1 (on p. 163) on *what* one wants to understand, as opposed to Gödel's preoccupation (on p. 305 of Volume II) with the mere 'existence of mathematical yes or no questions' with some heroic, traditional property; like being 'undecidable for the human mind'. Secondly, less specifically, the general idea of Gödel's (second) remark is commonplace for ordinary mathematical experience: focusing on formal deductions from given axioms is by no means obviously a first step *towards* a realistic view of mathematical reasoning. Its formal aspects are inadequate not only as far as discovery is concerned, but also understanding, including checking; both of theorems and of proofs. Thus formally unnecessary methods can be essential, for example, for reliability by cross checks (of numerical calculations by use of general theorems; in higher mathematics, of deductions from axioms for real closed fields, which are formally complete, by means of topological methods, for which there is no similarly complete formalization). Thirdly, as so often, though all this is commonplace it is also in conflict with heroic perennials, for example, of 'purity of method'.

Gödel's remark compounds the conflict by uncritical—and possibly even premature—precision about the matters above. It pays no explicit attention to what, if anything, (its) precise meaning for words like 'understanding' contribute by way of effective knowledge; instead it relies on the following:

Logical straws: a kind of numerology; cf. Note 5. To fix ideas, the samples below concern the systems $I\Sigma_n$ mentioned earlier. Gödel trots out familiar (logical) parameters, but now meant as 'measures'. Thus n itself is for (degree of) abstraction, in other words, determined by counting quantifiers[3]; the number of symbols occurring in a formal object for (the complexity of) understanding the thought—theorem or proof—represented by the formal object. Naturally, there is no limit to elaborating this numerology.

But what we know of those matters—never mind the many things we don't know—is enough to show that the measures chosen are at cross purposes to the meaning(s), for which, say, abstraction *does* contribute; for example, the passage from **Q** to abstract fields. This is not all.

A solemn assumption and one alternative; recall also Remark 1 and Gödel's (legalistic) debating points. The pious assumption is that views should be established or defended on terms set by opponents. Here the commonplace view from mathematical experience is meant, and its opponents are proponents of strong AI, who were called 'formalists' 100 years ago. The assumption is enshrined in Turing's test, touched on p. 180 of the review of Volume I (footnote 2). It requires mental capacities to be measured solely by—the sets of—formal *results* obtained, not *processes*; cf. pp. 161–162 of that review. Accordingly, (pious) defences of the ordinary view attempt to rely on incompleteness properties. The weakness of any such attempt spawns (valid) objections, which then attract attention to that weakness and away from the strength of the (commonplace) conclusion. Except for the commotion produced in this way the net result is a step back.

Now, certainly a most obvious alternative is to look at—if you like, just the conscious aspects of—mental processes of human computers, where the *results* generally *do* agree with those of the electronic variety! One simply looks at the execution of formal rules such as substituting one formal expression for another. Human computers have additional resources, including, for example, appropriate, so-called *ad hoc* interpretations of formal symbols. If one wants to know about the (biological) resources available, one will be well advised to look at them; not merely at possibilities of replacing them for (sufficiently) similar results by suitable software engineering.

By Notes 3, 5 and 6 and especially by Section 7 readers must expect many different things to come to mind at this stage (and unless they are very unlucky, all of them more rewarding than those logical straws). Those with a classical background will think of Aristotle's advice in *Metaphysics* Γ 5, 1009a 16–22 on how to treat opponents who object mechanically; perhaps fittingly, if they are proponents of mechanical reasoning. Those used to ologies (of Section 6) may have to be reminded of the common-or-garden varieties of science, where there are lots of familiar things to look at, such as conscious aspects of mental processes mentioned above. Some of them will come up again in connection with the third remark.

But first here are a couple of cozier items.

Remarks, first on the editorial note, especially pp. 288–289. Once the particular interpretations, in the samples above, of those logical parameters are recognized for what they are, that is, as inane, a new question arises: *Do they have another*, naturally more recondite, *significance*? In other words, other implications (which must be expected to demand more imagination). At least for *some* of them, the formal improvements by the editors—of the remark itself, but also of Gödel's note on speed-up in 1936—*may* be useful. Secondly, to illustrate what was said at the start of this section of the Appendix (about Gödel's good old days), here is an anecdote. Gödel's own attachment to those literally superficial measures came up in our conversations; first at a luncheon at Pennington in a cottage, which I shared with Dana Scott. (The occasion was Friedberg's visit to Princeton after solving Post's problem.) Gödel talked with obvious warmth about that little note he had published around 20 years earlier, although the general idea is perfectly clear from the last *Reminder* above. There was an obvious, so to speak solemn, contrast between his faith in those formal(ist) parameters and his reservations, in particular, at Königsberg, about Hilbert's aims (in his programme). But this presented itself, to me—then and now—as a kind of aberration or blind spot. The temperamental side, his *attachment*, has remained, for me, much more vivid, partly because of a coincidence. Less than a year before that luncheon I had learnt from him his interpretation of Heyting's arithmetic (in Section 5). For me a principal attraction of this was *as a change* from the no-counter example-interpretation, with which I had been familiar for barely 10 years (and had used effectively and repeatedly in the meantime). Incidentally, he remained attached not only to his own discoveries, but also to knowledge he had got the hard way, for example, from uncongenial literature.

3 A philosophical error in Turing's work By experience with the academic sense of 'philosophical' the title gives fair warning: an elementary blindspot is meant which—by a refrain of this review, like other, even literally superficial, errors—can have profound consequences. By that same experience, Gödel must be expected to be solemn where Turing was (by ordinary standards, not those of Cambridge at the time) particularly breezy.

Reminder. In what most readers of his 1936 paper would have regarded as an aside, Turing proposed a (compactness) argument to 'establish' that, at any given moment, there can be only finitely many states of mind. His rather quaint hypothesis was that otherwise there would be confusion. For the record I know at least one mind that gave Turing the opportunity (a few years later) to correct his, let us say, ideal of the human mind, and to remember—from his Tripos questions on ideal fluids—that idealizations need not be even first steps towards understanding the phenomena meant.

Gödel's thought. Granted that at any stage there are only finitely many—conscious or unconscious—states of mind (or, in some equally rough sense, of the brain) this leaves open *how* any sequence of subsequent states continues or so to speak grows; especially if the capacity of the mind (which he calls 'faculty' on p. 268) grows, of course, over and above its memory.

As a *correction* of Turing's breezy (strong) AI, Gödel's reminder remains compelling, however weak his *attempts at contributions* to effective knowledge of that faculty may be; cf. Section 4(a) of this, and p. 167 of the earlier, review on doubts about assuming that the discovery of (logically) new axioms is a very rewarding function of that (mathematical) faculty. As matters stand today, Turing's focus on those functions which the human mind has *in common* with (conventional) machines—but perhaps less so with the minds of animals—has been more rewarding; tacitly, as objects of theoretical understanding.

Thoughts by association with the topic of finiteness. The additional background here is the, by now, standard material on *recursiveness* applied in various parts of mathematics that serve as the languages of theoretical sciences. As a corollary, so to speak for the *analysis of language(s)* of this kind, the notion of mechanistic theory presents itself; for such theories all its (scientifically interpreted) aspects are recursive; for example, solutions of partial differential equations. Despite pitfalls in interpretation, for example, of so-called initial conditions (cf. the review of [13]), the general idea of this mathematical property of theories is clear enough. A problem comes from the following:

Reminder. For the ordinary separation between observational knowledge and its theoretical interpretation(s) — never mind high-falutin' (and correspondingly dubious) doctrines about any order of priorities — data of the observational kind are (hereditarily) finitely described. But also, *any such*, necessarily finite, *set of data is recursive*.

Evidently, only the most coarse-minded would conclude from this that the mathematical property above, of being mechanistic, is without *any* scientific significance. An obvious question is: Where, if anywhere?

In other words, recursiveness is an *infinitistic* property, and so its interpretation is more demanding (in imagination).

Reminder from vast experience in classical physics, in particular, PDE. Some *infinitesimal* conditions (on solutions being once or twice differentiable) are, often demonstrably, mathematically significant; most simply, for admitting or excluding a particular PDE as (even) a candidate for a theory of the phenomena — pedantically, of those aspects of them which are — considered. But again, every observational set of data is consistent with those conditions and also with their negation.

More recently, these infinitesimal conditions have been *discovered* to have significance, roughly, when the PDE of continuum mechanics are viewed as (classical) *limits* of theories for molecular or quantum phenomena and the like. Not surprisingly, this requires particular attention to matters that used to be brushed aside by claiming that we understand the classical phenomena 'in principle'. Also not surprisingly, the implications at a microscale of the infinitesimal conditions are, as one says, qualitative, aka as 'matters of principle'; cf. [3].

Viewed this way — and in the absence of any corresponding micro *theory* of the mind or brain — pedantic elaborations concerning recursiveness are premature; as it were, a confusion in kind, not only in degree.

Remarks. The editorial note (pp. 292–304) by J.C. Webb (assisted by Feferman) does not even get the correction in Gödel's (simple) thought above straight, nor anywhere near those thoughts by association. There is a good deal of elaboration of the kind adumbrated in the last paragraph. On pp. 300–301 there is material with interpretations and speculations about a piece I perpetrated in 1972. By a fluke I reported on the circumstances surrounding that piece on pp. 152–156 (especially, pp. 154–155) of the proceedings of the symposium at Salzburg in 1983, which cast a shadow on the editorial material. But since I am eternally grateful to Webb I shall not miss this opportunity to tell the story here.

In the mid 70s — of course, after that aberration in 1972 — he drew my attention to Hilbert's musings in lectures, not in print, on — what is called in this review — a market for his kind of *axiomatic analysis*, especially of elementary geometry. He proposed it as a substitute for pursuing sterile (cl)aims of contributing knowledge of the nature — or, in modern jargon, the concept — of space, for example, by sterile traditional logic chopping. At the time Hilbert did not — and could not very well — estimate the (obvious) *risk*: of such axiomatic exercises distracting attention from more rewarding aspects of — something more or less like — space. But by another fluke, which strikes the (mind's) eye if you keep it open, Hilbert himself had a chance to reassure us about that risk. He was among the first to take up Einstein's *shift of emphasis*: from space and time separately to space-time. (Nothing could be further removed from all that axiomatic analysis, some of which no doubt prettier than Einstein's mathematics.) In short, for those with a modicum of negative capability, the axiomatic analysis is harmless enough; as somebody put it: this side of the pale.

For the record, I have always regarded exercises around Church's thesis (in intuitionistic terms, ordinal logics etc., summarized on pp. 154–155 of [18]) as similar substitutes. But before learning of Hilbert's musings it was tedious to talk about this. Now I just tell the story above.

Appendix II: Non-standard analysis: impressions around 1970

In a little more than half a page (on p. 311) Gödel's view, after a lecture by A. Robinson at Princeton in 1974, is presented, and reviewed in the editorial note on pp. 305–310. This is quite consistent with, but does not emphasize, the aspect of those remarks which is most pertinent for the agenda of this review, and particularly Section 7:

The contrast between traditional logical idea(l)s—generally, and, in particular, applied to mathematics—and scientific experience has become evident, especially in the second half of this century. It becomes particularly vivid in Gödel's remarks, which focus on the (in 1974 even) narrow(er) area of non-standard analysis and arithmetic. The contrast concerns both the interpretation of the results available and expectations of the future; so to speak, concerning the centre(s) of gravity of a growing body, here, of analysis. By our refrain such expectations will differ if knowledge is taken to grow like a seed from a tree or by accretion.

Gödel was singularly well equipped at the time to present that contrast; in effect, not necessarily on purpose. He had been out of touch with developments in mathematics in the preceding quarter of the century, and he had had practice in presenting logical ideals since the 40s. Allowance should be made for strong language, for example, about non-standard analysis being the analysis of the future. But it is certainly no stronger than the fanfares in his essays in the 40s. *Remark*. Of course there is also—in terms used at the end of Section 5—a staggering contrast between, at least, the literal meaning of what he preaches here and his practice in his metamathematical papers. But it is no greater than the contrast, for example, between Hilbert's peroration about purity of method at the end of his *Foundations of Geometry* and his practice in ordinary mathematics.

Three samples will do.

1. *The tree* (of knowledge) *of numbers* becomes a single branch on the logical view; filling gaps (from **Z** to **R**, with **C** regarded as a minor excrescence). Non-standard reals are then the next step, if not the holy grail (and forgetting differences between them and other non-archimedean fields, which are not even mentioned by Gödel; though certainly significant for effective knowledge). But by mathematical experience other omissions are more serious.

For one thing Gödel blithely disregards the risk of a point of diminishing returns in the pursuit of any holy grail, here of filling gaps. More specifically, certainly as far as number theory goes, branches that are totally overlooked by Gödel are at least as prominent in mathematical experience, for example, finite fields or p-adics. (The former, like **C**, differ in not having an ordering compatible with + and ×.) Only the *logical* order of priority puts these objects low down.

2. *Ordering* (theorems, proved at the time by non-standard methods) by *logical implications*. Gödel tacitly applies this to theorems about invariant subspaces for (polynomially compact) operators, and disregards the quality of the 'improvement' of Robinson's result (tacitly in [10]; of course, even without knowing the meaning of the words used, the later result, by Lomonosov, is seen to imply Robinson's, but its quality requires closer attention.)

Gödel also refers to 'other' cases, of which there were few at the time. One of them involved non-standard notions in the statement itself since it connects function fields of basic arithmetic with number fields in (suitable) non-standard models of arithmetic. So on the surface it resembles Higman's gem in Section 3, which (also) connects logical notions and ordinary mathematics; now from recursion theory and the theory of finitely presented groups. But closer inspection of that other case was summed up by a perceptive mathematician as follows: No wonder you get such a connection if you call a lot of strange objects 'non-standard integers'.

In other words, as matters stood at the time, knowledge of function fields told you quite a lot about non-standard models with precious little in return. The assumption that one day the balance of trade *must* be reversed is—by experience and as in ordinary commerce—touching (at least in the young).

Of course, literally, the connection constitutes new knowledge since it cannot even be stated without non-standard concepts. It is a new truth, and by the logical tradition this has priority over choosing among truths.

3. *Concrete numerical problems*, like Fermat's conjecture, were, in Gödel's words, 'left far behind' in then-contemporary mathematics. By implication—and again in accordance with the logical meaning of the words used—*concrete* and *abstract* are seen to be in (would-be fundamental) opposition. Partly, plain ignorance was involved. In 1970 Baker received a Fields Medal; exaggerating very little, for bounds, in terms of k ($\in \mathbf{Z}^+$), on x, y ($\in \mathbf{Z}$) that satisfy $x^2 = y^3 + k$. Partly, it was thoughtlessness: What could be more 'concrete' than finite fields? especially, since metamathematical properties like consistency and other Π_1^0 properties are concrete in Gödel's sense.

Compared to these oversights it is a minor detail that, even today—with many memorable contributions, of which one will come up below—Robinson's *logical* versions of non-standard arithmetic and analysis have not been used to solve such concrete numerical problems. On the contrary, such well-known results as Tchebotarov's theorem have been used imaginatively for work on non-standard models. *Logical* must be stressed since otherwise number fields are also non-standard 'versions' of **Q**. The single most striking difference is of course that logical versions express the analogy involved in terms of logical classifications (of the properties preserved).

Disclaimer. Ever since the 60s I not only had no qualms about non-standard methods, but was in the market for information about them; however, not for such traditional reasons as presented by Gödel. For example, I realized (and this was confirmed by people familiar with the subject) that the problem about invariant subspaces actually solved by Robinson was simply *not a contribution* to effective knowledge in that area. But, to me, it *illustrated* vividly a particular potential of his method: an efficient representation by a single (infinite) nonstandard element of iterated limiting processes. Quite apart from literary talent I could not possibly have expressed this thought as compellingly as van den Dries and Wilkie in the 80s in the introduction to [7]. They had realized this potential by realizing that it contributed to an unquestionably substantial piece of knowledge: Gromov's theorem on finitely generated groups of polynomial growth (a different matter from polynomial compactness of operators).

To conclude, here is a report on impressions of non-standard analysis in a different quarter, actually in the late 60s. Given my own impression described above, also of Robinson's other work, it was natural for me to look for support of my proposal to have him elected a Fellow of the Royal Society; not even a Foreign Member since he had British nationality. It had been already established that there was then no 'prejudice' against logic. By statute, initial support has to come from within the Society. On the face of it this seemed easy (to me). Robinson had not worked on, say, large cardinals or Turing degrees, on which the people involved simply could not be expected to have *informed* views (and I for one did not expect support for my proposal on the basis of uninformed views). Robinson's (cl)aims were stated in ordinary mathematical terms; in several books, at length, and with a good deal of repetition. His style may not have been everybody's cup of tea, but then mine, though different, isn't either. By leafing through his publications it is certainly possible to get the gist of it, provided at least a few results catch one's attention. But the general response to my proposal was very cool.

Probably I had underestimated the extent to which would-be advertizing of Robinson's invariant subspace theorem had become known. It has to be admitted that it involved a quite staggering lack of understanding of the subject in question. Be that as it may this theorem was brought up, and nobody (in the trade) would want to 'disregard the improvement'. I remember the shift of emphasis in the *Disclaimer* above, to illustrating potential, so well because this was acceptable. But it was pointed out that *this* point was not to be found in *his* books.

Somebody with a temperament different from mine might have pursued the matter successfully. As I saw it, it was the Society's loss, not Robinson's. By chance, in a review of Robinson's book on the metamathematics of ideals [14] in the mid fifties, I took the same view of lack of interest in logic: it was their loss, not ours.

For the record Gödel took a completely different view. His personal loyalty was mentioned already in (iv) on p. 146 of [18]. But his professional loyalty was very strong too. I have too little interest in such matters to speculate sensibly in what way, if any, this played a role in his remarks; let alone, whether anybody in the audience later helped in the election of Robinson to the U.S. National Academy of Sciences. If it did, one could learn a lesson about the way this world works (in terms used repeatedly in this review). It can be useful, even if it is not logically necessary, to be able to say with a straight face: Non-standard analysis, in some version or other, will be the analysis of the future.

Notes: Mainly Beyond the Academic Pale

1 Aspects of Gödel's work neglected in this review As mentioned in the *Preamble*, many aspects of his work are familiar and/or presented in the editorial notes. Anything I have to add is to be found in earlier publications; in diverse forms for readers with diverse backgrounds. Thus the obituary was written for—likely readers among—Fellows and Foreign Members of the Royal Society, and the chatty article in [18] for a handful of people who had known Gödel personally and met at Salzburg in 1983 for the purpose described in the introduction to [18] (but not achieved). The review of this Volume in *The Journal of Symbolic Logic* follows its guide lines, in contrast to the (earlier) review of Volume I in this journal (volume 29 (1988), pp. 160–181). This diversity fits the main theme of the present review: on alternatives to the ideal of a tree of knowledge; pedantically, provided the mind is permitted to use its natural capacity for processing data in parallel (not only "systematically").

Here as elsewhere the general idea of that theme is abstractly so familiar as to be banal. But also (as elsewhere) it is in conflict with venerable ideals, for example, of a "definitive evaluation". More information on that conflict—where and how it arises—is in the next few notes and especially in Section 7 of the review itself.

*2 **Commerce of ideas*** Limitations of this parallel (with ordinary commerce of material goods and services) are taken up at the end of this note. Throughout this review it serves to underline aspects—in the "knowledge business", a literal translation of Kant's *Vernunftgeschäft* (A 724), but with a twist in meaning—central for the agenda of this review; more fully, aspects which are neglected by venerable ideas, but correspond to household words in ordinary commerce.

Samples. Above all, there is so-called *marketing* but not confined to door-to-door salesmanship (though this too has its parallels in the commerce of ideas). It requires the *discovery of markets* or at least, gaps in the market, for products over and above their (mere) legitimacy, that is, absence of fraud. The latter correspond to the likes of existence, truth or consistency in our commerce. In a somewhat different vein, there are *different priorities* for those who labour, say, on the shop floor or on arranging shop windows, and more broadly for trade unions and management of corporations. These in turn combine against those concerned with *correcting traditions* common to the "supply side", for example, by (philosophies of) mergers and unbundling. More topically, failures of so-called *Big Science* often recall those of big business conducted according to idea(l)s familiar from—and well established in—experience of craftsmen and village grocers.

These simple home truths, concerning the emphasis on different aspects, are in conflict with erudite, traditional—here, economic—*isms*, for which certain aspects alone are privileged. Effective use of those home truths requires the following:

Disclaimers. Generally, especially for the agenda of this review, the appropriate pay-off in the commerce of ideas is *not monetary* (which is relatively prominent in ordinary commerce; but recall the striking exceptions in the pretty theory(!) by Kreps under the heading "bounded rationality", tacitly, in Simple Simon's sense of "rationality"). A venerable alternative pay-off, just knowledge, is memorably described in Goethe's letter of 18.VI.1795 to A.v.Humboldt, specifically, by reference to his experience with science; according to him in contrast with works of art.

More specifically, concerning my own use of the parallel (not only in this review), I have nothing to say about changing the world, here, of our commerce, only about interpreting it. These words come from one of Marx's (many) popular dicta: The philosophers have only interpreted the world in various ways; the point is to change it. **This is not my point.** I do not see that I know enough—or even that enough is to be known—about *predicting* the world (or history) to make Marx's philosophy above even remotely plausible; pedantically globally. (One's backyard is another matter, as Candide might have added.) For the record, ever since my teens I have viewed that dictum—of course, not as the single most distinctive, but—the shrillest element in—what I have learnt of—Marxist thought. Viewed this way—and contrary to Engel's quote from Marx ('All I know is that I am not a Marxist')—not only he was very much a Marxist, but so are many so-called anti-Marxists and specially ex-Marxists.

The economic side of the ism has always seemed a much more delicate matter (to me). In recent terms (late 80s), General Motors and Yugoslavia were both planned economies with much the same annual turn over. High executives of GM certainly had, realistically speaking, less freedom of speech about their daily concerns than, say, Croatian peasants. A more striking difference (to me) is that GM looked for markets, the other was—thought to be—committed to a particular market.

Passing thought. Temperament is involved in my particular brand of anti-Marxism.

The following note looks at this thought more closely (and not primarily in the field of economics).

3 Matters of temperament, background and literary forms Such matters arise of course everywhere, also in the commerce of ideas, but with *different weight in different sectors.* With luck, they will be correspondingly emphasized, ignored or suppressed in the traditions of the trades and unions involved. According to temperament and background this may or may not suit a particular individual, pedantically, when engaged in a particular job.

Speaking for myself (and for some, by no means all, congenial spirits) attention to those matters has enriched my experience with mathematical logic; naturally measured by the (for me) appropriate pay-off function. This is all the more vivid since it does not apply to my experience in the Dutch garden of function theory, where, for example, proofs of Bieberbach's conjecture were presented both with and without relations to those matters, and the latter kind were much the better bargain. In any case, in such a narrow sector of the market as function theory, and a union with such strict rules the variety of temperament, background and (even) literary forms encountered is, realistically speaking, quite limited.

At this stage everyday experience of ordinary commerce—with its more striking effects than in our commerce, for example, crashes—is useful. It discourages general pontification about the topics in the headings of this section, but also it provides plenty of specific situations, where emphasis on the corresponding aspects is rewarding; again, by the easier measures established in commercial experience. This simple point is—by experience in our commerce—so central that it is worth underlining; below, by reference to those notorious *paradoxes*, treated disdainfully in Section 2. The difference is simple.

There their *logical* interest—more fully, the interest of their logical aspects—was the focus. Here, the interpretation is different. What has been said about the paradoxes is taken to establish *some* interest. But the focus is on alternatives to *assuming* that it is primarily related to their logical aspects. For one thing, paradoxes—in other words, refutations of familiar, implicit or explicit, more or less thoughtless assumptions—are commonplace; especially, around (ab)uses of the definite article, as in Russell's "the class of . . . ", but also in "the greatest integer". (In the latter case, "the" is misplaced both for the usual sense of "integer" and, for, say, integers mod p, when the usual order is incompatible with the ring operations.) In short, the logical interest is dubious.

Samples appropriate to this review. On p. 124 Gödel refers to "the amazing fact that our logical intuitions (. . . concerning such notions as: . . . being . . .) are self-contradictory". On p. 103 and in Section 2 above Hermann Weyl's malaise about all this, of which more in (c) below, is mentioned.

(a) What is regarded as *amazing* is obviously, at least partly, a matter of temperament. Less obviously, this applies to *our* intuitions, for example when a solitary temperament does not get an opportunity to compare personal impressions with wider experience. (Of course, dim-witted people have little chance of benefitting from such experience even if they have a different temperament.) Once again, an abuse of the definite article—here, in "the problem of paradoxes", when the latter have, obviously, many different aspects—helps to bring in tacit assumptions about "the" solution, for example, that amazing facts *must* have great inwardness; never mind, whether as sources of profound wisdom or as the work of demons (or in another quarter, witches; so to speak as a matter of sexual preference). The tacit assumption is that they are *not* simply blind spots.

(b) Needless to say, matters of temperament and background are often difficult to disentangle (since one may not wish or be able to manipulate either). But in some cases such aspects of background as general education are more visible, and thus easier to document and convey, even to solitary temperaments. In the case of Gödel's "amazing fact" there is the historical record of Cantor's and specially Frege's complaints about being ignored. In other words, for philistine intuitions topics like being—Gödel mentions also truth, concept, class—weren't even candidates for (mathematical) study. "Convey", not discover, since experience of men and events, not available to solitary temperaments, is needed to use the historical record sensibly (or even to have an inkling of possible snags). The historical record, here, a little literary background, also helps with the next item.

(c) As mentioned there aren't all that many options of literary forms for presenting a proof of Bieberbach's conjecture. A horse of a different colour is involved when it comes to formulating general—especially, would-be fundamental—thoughts concerning all sectors of the commerce of ideas (even though the literary form of mathematical logic may be one's hobby horse).

One of the most famous quotations, concerning two broad options, is from Boswell's *Johnson* (17.IV.1778, by Oliver Edwards): I have tried too in my time to be a philosopher; but I don't know how, cheerfulness was always breaking in. The contrast with—what is called in this review—the "solemn tradition" is not only still topical, but is recognised by academic philosophers, for example Peacocke, in the prologue to his piece [12] in a publication for the humanities generally. He solemnly commits himself to relentless pursuit, a well tested ideal when a target is clearly in sight, but hardly compelling when the target is the whole commerce of ideas (or the ordinary kind).

Disclaimer. Especially the cheerful variety of literary forms is, generally, not easy enough to be generally recommended. In this sense the fashionable, austere doctrine of deconstructionism of literary "theory"—or of *The Journal of Symbolic Logic*'s guide lines to reviewers, on being "directly concerned" with the text—will be better for those who do not have the discretion needed for a broader view. What, if anything, is accessible without such discretion depends of course on the particular case.

Before concluding I return to the case of Weyl's malaise with Gödel's essay (in Section 2(i) and in the *samples* above), which will also illustrate my unorthodox use of "literary form". Weyl had a choice between the literary forms of ordinary mathematics, of which he was a master, of mathematical logic, of metaphors and similes and others from the ordinary literary tradition. He chose the latter, calling the essay "the work of a pointillist". This simply does not fit what he wanted to say. A pointillist worth the name uses points, which are individually of no weight, to produce the impression of a recognizable object with global features. But then Weyl goes on to say, of course in different terms, that he had not derived any global idea from Gödel's points. (By Section 2 he had not perceived their individual weight either.) An alternative available at the time was to use details of the constructible hierarchy (if only for a metaphor).

Reminder; cf. footnote 11 on p. 179. Weyl's—only, but well-known—"intervention" in the foundational debate was his emphasis on the first level of ramified analysis, obviously related to the ramified "theory" of types up to ω considered by Whitehead and Russell. Gödel's essay emphasizes the (close) connection to the constructible hierarchy (p. 136). So Weyl's literary lapse seems (to me) a sound enough peg on which to hang the kinds of comments above.

A familiar conclusion, but with a twist. Inasmuch as the kind of logic around the paradoxes is typical, *logic just isn't mathematics*. More soberly, (a)-(c) above draw attention to many other aspects of phenomena around the paradoxes. So it is (just) simple-minded to *assume* that their mathematical aspects are rewarding, let alone decisive. The twist is two-fold. For one thing, (a)-(c) are full of reminders how marginal, especially higher, mathematics is; not only here, but generally in the broad commerce of ideas.[4] Secondly—and on this score unions and management close rank across the board (in the world of academic disciplines)—, *traditions of a trade are not sacrosanct in (a)-(c)*; specifically, not those of academic philosophy, which claims to know the extent to which the *raw* potential of a commodity is enhanced by its own resources; here, potential of the paradoxes.

4 Autobiographical remarks: absoluteness scaled down (with particular reference to footnote *r* on p. 115). Though I saw Gödel's remarks to the Princeton bicentennial conference (pp. 150–153) only in the mid 60s, the general drift was clear enough from his conversations 10 years earlier. (As usual, he did not breathe a word about his earlier, here, oral, publication; cf. (a) on p. 148 of [18]).

At the time I was equally ill at ease with popular claims for *and* against general, so-called epistemological notions (p. 150). But also I had no idea how to formulate the malaise; not even, for example, in terms of the obvious incompatibility between the logical order of priority and orders discovered to fit the facts of experience better (which is a refrain of this review). So I could not, and of course did not take Gödel's words literally. However, they struck several chords.

(a) *Finitist provability*. (This too is "absolute" for those so benighted that other proofs are inaccessible to them.) Now, footnote 2 on p. 242 of [8], which I had quite forgotten by the time I met Gödel (and nobody including him had challenged), is spoilt by a blind spot. I had assumed there that a (satisfactory) definition of finitist provability should also be *established* finitistically; in other words, that it should be satisfactory to a finitist, too. Now, whatever malaise I had with Gödel's absolutes taken literally, what he said about them was enough to remove my blind spot.

His (admittedly arresting) terminology jarred with the view I took of finitism. Given my temperament it would not do (for me) to sanctify benighted shortcomings by terms like "absolute". So instead I used the (admittedly colourless) word "informal"; for example, in my lecture to the ICM in 1958; cf. footnote 4 on p. 242 (of Volume II).

(b) *Predicative provability*. As described elsewhere, by a fluke I came across Kleene's papers on hyperarithmetic predicates at about the same time. Of course there was nothing wrong with them. But, to me, they became more rewarding when related to the traditional literature on predicativity. Later I noticed a more specific use for analysing different proofs of Cantor–Bendixson, which I had learned in my teens in Littlewood's lectures, the only course I liked at Cambridge. (There was a corresponding shift of emphasis in the questions asked about hyperarithmetic objects.)

(c) *Intuitionistic provability*. As in (a), in the course of conversations with Gödel about his system T, I came to see some merits in this idea (which were indeed enormous compared to my earlier expectations!). In contrast to the topic of sets here was lots of virgin territory, starting with the possibility of completeness theorems without concocted semantics. In Gödel's terms, one had now absolute results on intuitionistic provability (albeit only about propositions stated in familiar formal languages); "positive" ones for propositional logic, "negative" ones for predicate logic. Most memorably, at least for me, results on new propositional operators served as a foil to the functional completeness of classical logic, a specimen of absoluteness ignored by Gödel, as mentioned in Section 3 of this review.

(d) *Calculated risks*. Nothing in (a)–(c) had shown (to my satisfaction) that the informal notions considered are particularly suited to describe the facts of (here, mathematical) experience. I was aware of this risk at the time (cf. p. 369 of [9]), and others noted my awareness (cf. the introduction to [1], but dropped from the second edition). Once again it is a matter of temperament whether correcting widespread misconceptions, here, about the specific potential of traditional informal notions of proof, is or is not adequate pay-off; to be compared to correcting the idea that butter, which tastes good, is bad for you, as opposed to discovering that seal fat, which stinks, is good for you. (In both cases you have to look at those fats.)

Corrections (for specialists) to 3 items in footnote *r* on pp. 114–115. (i) My interest in predicative *definability* (related to *minimal models*, for example, in the ICM lecture) did not "peter out" particularly. Around 1970 I drew H. Friedman's attention to the impredicativity of ω (cf. Section 4), naturally in connection with (non-standard) *general* models. His result, that there are no such minimal models for a broad class of arithmetic systems, gives so much more detailed information that it would have been poor salesmanship to present it in relation to (mere) impredicativity. (ii) Since Gödel thought of the isms in (a)–(c) above as "opposed" to realism, *his* realism could have led to my interest in the others only out of perverseness (which I do not wish to exclude; but then: Who am I to judge my unconscious motives?). However, I was totally conscious of the fact that what he had to say not only revived my earlier interests (mentioned in [8]), but consolidated them. (iii) Since Lorenzen was—and remains—*committed* to his ism, what was a blindspot on my view of the ism in (a) is a focus for him: he does not allow himself a place from which to look at such limits.

5 Clutching at straws, and straws to clutch at For one of the parochial concerns in this review,—the growth of—knowledge of sets, three such *straws* are: infinity, self-reference and impredicativity; recall Section 2(i)–(iii). They were *clutched* at by people thrashing about for something to say after Russell's paradox. Preoccupied with the latter (and, generally, with Frege's naughty axiom) they neglected, by and large, more rewarding questions about sets, for example, about the replacement axiom and (some of) those called axioms of infinity; cf. Section 5a. Obviously, straws have properties too, and so can be perfectly legitimate objects of (precise) study, as in [N4b]. Clutchers differ in style and power, and so they too can be subjects of musings, for example, in [N3c] on Weyl. This note shifts the emphasis away from such, as it were, internal properties of the straws to one of their outward, hence, literally superficial aspects that strike the mind's eye. Biologists would speak of *functions*—or, as far as knowledge is concerned, dysfunctions—of the straws, in contrast to their (internal) structure. Roughly, experience with Russell's paradoxes and reactions around it is summarized in the following:

Pun on the word "philosophical". For its popular (though perhaps not for its literal) sense, clutching at straws is decidedly *un*philosophical. At the same time, in the area around Russell's paradox, the straws involved are of the bluest blood for the philosophical tradition in the academic sense.

Evidently, the pun involves much the same point as the heading of Section 1 on (its) would-be fundamental notion(s), with one difference. There one small branch of the tradition was considered, here the broad tradition is meant.

Reminder. It is a foregone conclusion that there are more or less limited areas where—like consistency in Section 1—other would-be fundamental notions live up to expectations, too. It's just unwise to rely on it without specific tests.

One of the more glamorous candidates (for a straw) among heroic perennials is the following—allegedly fundamental—"opposition":

Objective and subjective (knowledge). Just as in the case of sets above, (scientific) experience presents—in fact, many—questions *around* the glamour issue above, for example, generally:

(a) The *effectiveness of mathematics in the natural sciences* across the board, that is, both of phenomena that strike the eye and elsewhere, and specifically around:
(b) aspects of quantum mechanics under the umbrella: *collapse of the wave packet*.

Now, when (a) and (b) are raised — or, more accurately, gushed about — they are traditionally related to the opposition above. This is at least a *candidate for a straw*, by distracting from alternatives, as follows.

As to (a), from general experience, that (innocent) effectiveness *differs in different areas*. It is quite limited in many of the common-or-garden varieties of science (and differs with the branch of mathematics considered). As to — what are called (by the trades dealing in such commodities!) — fundamental theories, the question of Section 1 about would-be fundamental notions arises even if it has different answers; from Section 1 and for different such theories.

As to (b), at least for my temperament, I have not thought enough about it to have an opinion. But my impression is that a closer look at the so-called *classical limit of wave functions for many particles* is — by a refrain of this review — an alternative option; especially since the work of Berry [2]. After all, only after 300 years of classical mechanics was its chaotic behaviour established: even if this particular neglected possibility does not occur in quantum mechanics proper, another comparably decisive (mathematical) property may have been missed in the last 70 years.

Certainly, for the broad philosophy (in the popular sense) of this review, the focus on some opposition between objective and subjective knowledge, which is of course high-minded, is above all simple-minded (and probably even below any threshold of informed discussion). The general idea of clutching at straws is perfectly commonplace. What is stressed here is that this idea applies to — and may be even adequate to specify principal errors in — would-be sophisticated enterprises.

Matters of terminology. At one extreme (going back to the pun), there is a question of a name for the trade(s), if any, whose business it is to distinguish between straws, pegs, seeds and other alternatives; either across the board or in specific branches of knowledge. For the linguistic convention used throughout this review — with emphasis on the tradition of trades — "philosophical" is *not* suitable in the specific cases mentioned so far in this note. Staples of the "bluest blood" — infinity, self-reference, impredicativity for our parochial concerns, the opposition between objective and subjective for wider concerns — are indeed given attention here, but with a view of determining where, if anywhere, they are (not) straws. In ordinary commerce this corresponds to the attention given to the stock-in-trade of a corporation by, say, unbundlers, who are not — generally considered to be bona fide — members of unions or management. Viewed this way, the best that could be said about the business in question, which shifts the emphasis away from the perennials of (academic) philosophy, is (in terms of the Maréchal Bosquet): *C'est magnifique, mais ce n'est pas la philosophie*.

At another extreme, there is the question of alternatives to the linguistic forms used here. To repeat what cannot be repeated too often. The general idea of clutching at straws is banal, the principal problem is to remember it when it applies. So a most obvious alternative option is a poetic wording, for example, in Keats' letter of 21.XII.1817 to his parents, specifically: *negative capability: when a man is capable of being in uncertainties, mysteries, doubts without any irritable reaching after facts and reason.* (A more prosaic version of "any . . . after" is: impatient clutching at straws of.)

Naturally, it would be a *missed opportunity* to "press on", and not to give second thoughts to such memorable words, both in accordance and in conflict with decontructionist canons, as in the following:

Samples. (i) The word "mystery" itself need not be used thoughtlessly or as meaning a particular feeling. (Though one would certainly like to know more about the latter, for example, how it can be induced by drugs and the like, these matters are difficult, and costly. For the record I have never been prepared to take the steps, in particular, drugs, which might be objectively (!) necessary for conclusive answers.) In my experience—and whatever its intended meaning(s)—the word is often used in situations when it is not even known whether a satisfying understanding *requires recondite study* or is obtained by *removing a blind spot.* It is a commonplace that such elementary uncertainties are apt to produce strong feelings whether or not of the kind meant above. In quite a different vein, (ii) Keats's letter of 22.XI.1817, just about a month earlier, to John Bailey contains a splendid example of clutching at—the perennial straw of—truth in a memorable objection to—what was later called—strong AI: *I have never yet been able to perceive how anything can be known for truth by consecutive reasoning—and yet it must be so.* In the contemporary trade, "formal" or "mechanical" is used rather than "consecutive".

Now, (mere) truth is a straw here—pedantically, for supporting the weight of Keats' thought—because many things are verified formally, for example, by calculation. But there are many aspects of reasoning *around* the consecutive kind, which is of course one side, that—God knows, not only—Keats wanted to know about, and which are not a by-product of (mere) truth of the end result of reasoning.

The subject of AI has come up already in Appendix I of both the earlier and of this review. They differ in the following respect from the popular literature on AI. They concern specific, albeit parochial, successes and limitations of formal reasoning, and need specifically logical information. The popular literature—for and against AI—may mention high-speed electronic devices, but its general conclusions require no more than the background to Keats' thought above. That literature—was ridiculed in the earlier review, and—is ignored here by the motto: *dégager les hypotheses utiles.*

Disclaimers (again in terms of a refrain of this review). The idea of "clutching at straws" is not thought of (here) as a seed from which a tree of knowledge grows. So, as matter of practical politics, it—is of course a legitimate topic of analysis or what have you, but—is not *assumed* to be a rewarding object of recondite or otherwise extended study. Metaphors for alternatives abound. At one extreme there are pegs—with luck, not straws—on which diverse items (of knowledge) are assembled, which would otherwise float in thin air, and be inaccessible. At another there are (mathematical) attractors in chaotic dynamics; so to speak, steady states presenting a more or less adequate idea(l) in turbulent surroundings.

A—to me—particularly attractive metaphor comes from biology. (F. Hodgkin, "Sex determination compared in *Drosophila* and *Caenorhabditis*," *Nature* 344 [1990], 721–728.) Here one has *both* striking outward resemblances (concerning sex determination) in those flies and nematodes *and*—this is of course more demanding—radical differences on the molecular level. So what is common to both will often—of course, not always—be *le côté le moins intéressant*. Hodgkin's word for these resemblances is colourless (convergence), but the thoughts are not.

Remarks. First, the opposite is true of a witless—and correspondingly popular—metaphor by Wittgenstein; obviously meant for concepts applying to phenomena related by such "convergence": *family resemblances*. "Witless" because family resemblances are—and are thought of as—connected with *common genes* (and billion dollar projects of mapping genomes of different species). Bringing in composite photographs is so contrived that it adds a negative quantity. Secondly, by a fluke the leading article (on p. 705) of the same number of *Nature* presents the concept of *complexity*, compellingly and elegantly, as an example of the kind above; needless to say, without reference to Hodgkin's convergence.

6 Logical aspects of ologies This note, which supplements Section 6, may be of use to intellectually cheerful readers. It goes into (a) some options for "unbundling" (some of) the solemn tradition, which can be rewarding provided (b) wide-spread but rarely mentioned *assumptions* of that tradition are remembered.

(a) The general idea follows from a refrain of this review. Though everything has logical aspects, they will be most visible where they are not overshadowed by other (more rewarding) aspects. Furthermore, by experience, at least occasionally, neglect of logical aspects can be costly (in the commerce of ideas, too). The following examples come from (familiar) theology.

(i) Various ontological arguments, preferably in Latin (which has no articles at all), concerning *the perfect Being* illustrate abuses of the definite article, which are not covered by Russell's *the present king of France*. For example, by the tradition of theology (of their day), critics of those arguments could not assume that there was no such being. Nevertheless they were able to make their point *without any ritual of formal "paradoxes"*. (Both *the present king of France is bald* and *the present king of France is not bald* are false on Russell's analysis, which conflicts with—a first reading of—Tarski's "adequacy" condition for truth: $\mathbf{T}(\ulcorner \neg p \urcorner) \leftrightarrow \neg \mathbf{T}(p)$; cf. p. 123.)

The word "ritual" is meant to underline—not only the obvious possibility, but—the fact of experience that formal contradictions are neither the only defects of reasoning nor particularly instructive. Specifically, Cantor's review [4] of Frege's *Grundlagen* specified a convincing defect in Frege's naughty axiom, and, when all is said and done, Russell's paradox continued to attract attention to itself; not, for example, to the definite article in: *the class of all classes not belonging to themselves*.

(ii) One of the properties required of *the perfect Being* in (i) above, is omnipotence or, in terms of Section 3, *absolute* power. Cusanus had some formally very simple closure conditions on his idea(l) of omnipotence: not only (the power to create) an immovable material object, in his case, a stone, but also the power to move all (material) objects, including stones. For reference below: it appears that Cusanus *wanted* a Creator with such absolute power.

Now, not only did Frege impose closure properties, on such logical objects as predicates and classes, which have a prima facie similarly absolute flavour, and so are suspect *if* Cusanus is remembered. (As so often with such general, elementary points it is also enough to read Aristotle right, for example, *Metaphysics* 998 b 22–27 refuting the idea(l) of a highest type, aka genus.) In any case more than 50 years after Russell's paradox, the faithful—in a then-new sect devoted to categories—blithely "wanted" a category of all categories (without any of the many qualifications, which present themselves after a moment's thought).

In short, there seems (to me) a gap in the market for supplementing the literary forms of mathematical logic, which are used for making such particularly elementary logical properties as above memorable; memorable enough to be remembered when they present themselves (not only abstractly). The theological literature is one (re)source, and a good bargain too, since many possess—knowledge of—it already; naturally, not for those who are determined either to remain committed to the solemn tradition or to stay away from it altogether.

For the rest of us it pays to know something of its conventions. What to do with such knowledge may depend on temperament; whether we want to interpret or change the world (of this sector in our commerce); in the latter case, whether by merger, take-over or unbundling.

(b) Above all, the emphasis in (a)—on particularly crass logical errors and using them, as it were, as vaccines for immunity in more delicate situations (where related errors occur)—conflicts with those conventions.

Samples. (i) The *style* of (a) is a breech of good manners in solemn circles, most simply, by its lack of respect for what is ***holy***. It is of course a matter of routine to avoid it *if* one wants to do so. This is of no consequence for interpreting the world. *How* to avoid it, if at all, may well have different answers for mergers, take-overs and unbundling.

(ii) The solemn tradition *assumes* that that shift of emphasis *risks a permanent loss* by distraction (from the full inwardness of higher aspects); a kind of mirror image to the refrain in this review about distractions from effective knowledge (by clutching at straws). In fact, that assumed risk is often presented as involving a loss of effective knowledge, too, especially by Gödel, albeit in an exceptionally innocent manner (pp. 140–141). This overlooks at least two snags. First, in such complex situations it is simple-minded to assume that relations of cause and effect are appropriate at all, and even more to rely on flash judgement. Secondly, it is equally simple-minded not to balance the account of such (assumed) gains against the cost (of futile pursuits of solemn idea(l)s). This is the "theoretical" side.

More significantly: What do we know of the probability that the risk in question materializes? For example, in terms of Note 5, negative capability could—as a matter of experience, not only possibility—intervene; cf. the end of Appendix I on Hilbert's logical exercises in geometry.

Even for interpreting the solemn tradition it is, by experience, good policy to correct for the lopsidedness which its preoccupation with that risk introduces. As before, changing it would be a more difficult matter, requiring the practical skills of unbundlers. Their policy is to give their targets rope: they are a better bargain if they have pursued their idea(l)s further, when it is easier to see (that is, cheaper in the commerce of ideas) what to keep.[5]

NOTES

1. *Addendum. Tractatus* is (best regarded as) an ode to propositional calculus, when its otherwise irritating exaggerations become perfectly acceptable instances of poetic license.

2. *Addendum* to the earlier review. Differences, not noted there, are (i) for example, in provability logic one has iterated codes (of codes . . .), but, in geometry, practically never: coordinates of coordinates, and—again in contrast to geometry, but now in an opposite direction—(ii) the literature on (Gödel) numberings is rarely explicit about the 'structure' on finite sequences (to be represented).

3. For specialists. Another favourite parameter relates abstraction to *higher types*; not 'type' in the ordinary sense (of 'sort' or 'kind'), but as in 'functions of higher type' in Section 5 or in axioms of infinity.

4. Even those who realize that demand is limited—like A. Weil: mathematics does not lend itself to popularization—rarely recognize specific obstacles that stare one in the face; cf. the motto of [17] about number theory being *abstruse*. (This is supposed to be obviously quaint.) Now, "abstruse" does not apply to the good old natural numbers, barely mentioned in [16], but may apply to *particular* properties considered, say, in arithmetic geometry. In other words, they may not be what the market wants to know (even) about numbers.

5. In terms of the present metaphor it is legitimate to ask: What business have, say, Notes 3 and 5 or (b) above in a trade journal like *Notre Dame Journal of Formal Logic*? Nowadays, it is standard for such journals—even those directed at the shop floor, involved in "participatory decision making"—to go occasionally into so-called structural weaknesses of the sector concerned; not necessarily written by "interested parties". It is a different question whether this has been done well in the present review.

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