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ON THE KIND OF DATA NEEDED FOR A THEORY OF PROOFS

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Part II of this article responds to the question implicit in the title: there are reasonably adequate data for a natural history of proofs, but not for a systematic science. The distinction between natural history and systematic or fundamental science is elaborated in the Introduction to Part II. Part I (§§1-3) prepares for Part II by listing some striking successes of early proof theory and the diminishing returns of later elaborations. The present article complements recent publications (Kreisel (1976) and (in press)) which stress negative aspects of current proof theory.

PART I: THE PAST

INTRODUCTION

Over the last half century proof theory has made a good deal of progress of permanent interest. One need only compare what we know now with the impressions current in the twenties concerning such general issues as Hilbert's programme; cf. §1 below for two extreme examples. Also quite specific questions raised by mathematicians about the content of their own proofs, have been answered with the help of early work in proof theory; cf. §2. This was considerably elaborated in the sixties, both w.r.t. the analysis of various informal notions of proof in the foundational literature and w.r.t. the mathematical techniques used. Some of the elaborations show unquestionable logical or mathematical wit, but none has excited broad (active) interest in the silent majority of logicians; or, perhaps, a little more pointedly, what authors regarded as interesting about their elaborations, that is, the results stated, left the silent majority unimpressed. Of course, every science produces some dull stuff; the point here is that the harder results in proof theory are so to speak systematically dull -- with reason (cf. §3 and below).

The common-place view of the (sociological) facts just described is that the silent majority lacks the philosophical sensibility needed to appreciate the full inwardness of those elaborations. It can hardly be expected that the silent majority expresses its sensibility very well. But -- and this is perhaps the main point of *Part I* of this article -- there is also another side to the matter. The elaborate results are *stated* in language proper to (philosophical) aims which themselves make *dubious assumptions*. For example, the aim of Hilbert's programme was to eliminate abstract methods because it assumed that these methods are unreliable or otherwise 'unjustified' (or, at least, more so than elementary methods). Now, expectations derived from this assumption have been refuted explicitly by work on Hilbert's programme, and implicitly by general mathematical experience. This has corrected false first impressions, and thus constitutes *philosophical* progress. Recognition of such progress (by the silent majority) would show philosophical acumen, not insensibility.

Put differently, the concepts which *existing* foundational schemes (suggested by first impressions) consider to be basic are simply off the mark.

To get some perspective on future work, it is natural to look at the past of some successful sciences, and to match up the early stages in their development with those of proof theory over the last 50 years. Two old branches of physics, kinematics and the mechanics of continuous media, illustrate the kind of thing that has happened in the history of several sciences. Initially only qualitative impressions of the world are available, but this is often sufficient to speculate on the *true nature* of the phenomena involved. In the branches of physics mentioned, those early ideas were expressed by phrases like *perfect shape* or *ideal fluid,* and made more explicit by use of the corresponding branches of mathematics (Euclidean geometry, Laplace's partial differential equations). As time went on, these ideas or conceptions of the world were elaborated by more advanced mathematics -- or else radically revised.

Sometimes a conception can be rejected on purely mathematical grounds simply by developing it to a point where it conflicts with very general qualitative or otherwise familiar experience. In such cases, the literature speaks of imagined (Gedanken) experiments: they need only be mentioned, not carried out since their outcome is not in -- genuine -- doubt. In the theory of ideal fluids the standard example is the result that a steady current exerts no drag (on objects of arbitrary shape). For similar negative examples in logic see §1; for positive ones, see §2, and successful applications of elementary geometry or mechanics. As to the elaborations of §3 they may perhaps be compared to the notorious applied mathematics of the Cambridge Tripos at the turn of this century; cf. Littlewood (1953). This comparison is of course immensely optimistic: present-day geometry and mechanics have gone well beyond the level of the Cambridge Tripos.

The parallel above will be continued in the introduction to *Part II.* -- In *Part I* the main use of the parallel concerns the passage from (informal) proofs to adequate formalizations; for example, it can no more be *assumed* that mathematical texts provide exactly those data which are significant for proofs than that descriptions by sailors of waves in the sea provide the data which are hydrodynamically significant.

EARLY SUCCESSES: FORMAL SYSTEMS AND FORMALIZATION

1. About 50 years ago there was widespread interest in the need for analytic methods in number theory; cf. Ingham (1932). Opinions varied. In accordance with Hilbert's programme some thought that references to reals, sets of reals, etc., were mere shorthand for appropriate approximations, and straightforwardly eliminable. Others thought that the opposite was true. And, above all, both sides thought that it was a matter of 'opinion' that would never be settled. Work in proof theory showed that they were especially wrong where they agreed.

(a) *Gödel's incompleteness theorem,* and especially his interpretation of it in footnote 48a of Gödel (1931), corrected the assumption about the general innocence, that is, eliminability of set theoretic methods, even for proving number theoretic results. Incidentally the assumption was widespread even among those who did not know its precise formulation in the form of Hilbert's programme. -- NB. The correction was *discovered* in connection with Hilbert's proof theoretic programme. Today it is best to use different results, splitting the notion of formal system into 2 parts: (definability in a) *formal language* and *formal rules* (for the consequence relation). The results for the restriction to formal operations are corollaries of general results in recursion theory; (invariant) definability and the consequence relation are best studied in model theory without this restriction. Also the implications of axioms of infinity asserting the existence of sets of high type (in Gödel's footnote 48a) are valid without any restriction to formal systems; cf. p. 182, ℓ.-10 to ℓ.-6 of Kreisel and Krivine (1971).

(b) *Actual mathematical practice,* in contrast to the possibilities pointed out by Gödel, turned out not to use analytic methods in an essential way. As early as 40 years ago, Gentzen pointed out that number theoretic practice did not use the full force of first order arithmetic (and so his consistency proof was not needed to 'justify' actual, only possible number-theoretic reasoning).

cf. p. 136, 170 and 200 of Gentzen (1969). In the fifties I reformulated his point more generally as follows: one uses the *language* of set theory or analysis, but '*weak*' (existential) *axioms*, conservative over arithmetic. In fact, imagination was needed to find any specific result of analytic practice which *cannot* be proved by quite elementary methods: I had to turn to such curiosities as the theorem of Cantor-Bendixson (which will come up again at the end of §4 below). In short, the pious business about the collapse of mathematics if set theoretic or even all non-constructive methods are excluded, turned out to be hollow.

Historical Remark. The logical status of set theoretic principles actually used in mathematical practice is not widely known even now; cf. Bishop (1967) and Stolzenberg's review, which quote the pious business mentioned above and other impressions current in the twenties, that is, *before* the work of Gentzen and Gödel. By implication, Bishop's exposition is presented as being the first refutation of those impressions! -- NB. Of course, as Gentzen certainly realized, these facts about (the limitations of) current practice are not relevant to Hilbert's original programme, which was concerned with the nature of all (possible) mathematics, not with perpetuating current defects. At most, those facts added to the plausibility of the programme (provided one started off with the relevant general assumptions of formalist or nominalist epistemology).

EARLY SUCCESSES: UNWINDING INDIRECT PROOFS

2. Though the incompleteness and conservation results in §1(a), resp. §1(b) were discovered in proof theoretic contexts, they do not refer to the proofs themselves but only to the set of provable theorems. As mentioned already, many such results are better treated by use of recursion theory (in the case of formal systems) or model theory (for generalizations to arbitrary sets of axioms). We now turn to results that involve operations on proofs. As is to be expected from experience in mathematics, especially category theory, close attention to the choice of data used to represent proofs is needed here, for example, to ensure efficiency of the operations involved. More generally, as mentioned at the end of the Introduction, the details of the passage from informal proofs to their formalizations are more significant here than in §1.

Historical Remark. The methods used below were discovered in connection with doubts about the 'legitimacy' of non-constructive proofs, sometimes called 'indirect' in the case of existential theorems $\exists x F$. The purpose was to show (for example, for Σ_1^0 formulae) that for *some* t, F[x/t] *can* be proved by 'legitimate' methods. This purpose is out of date as soon as the 'legitimacy' of the indirect methods is recognized. But the question remains whether or not a given (indirect) proof π of $\exists x F$ provides an instance t:F[x/t]; and, if it does, to determine t *mechanically*, that is, to find mechanical or, equivalently, formal rules: $\pi \to t_\pi$. In short, the original question of *justifying* proofs (that is, principles of proof) is replaced by questions of *handling* proofs (mechanically).

(a) *Two examples of mechanical unwinding*. They need only proof theory of first order predicate logic. They span a period of nearly 30 years. They answer genuine questions, (actually) asked by excellent mathematicians about their own work.

(*i*) *A theorem of Littlewood* (proved in 1914, refuting a guess of Riemann): $\pi(x) - \ell i(x)$ changes sign infinitely often where $\pi(x)$ is the number of primes $\leq x$ and

$$\ell i(x) = \int_e^x dx/\log x .$$

Of course, this means that there is some (recursive) method of determining a, in fact the least natural number n such that $\pi(n) > \ell i(n)$; since $\ell i(n)$ is not integral, we can compute $\pi(n) - \ell i(n)$ to sufficient accuracy to determine whether $\pi(n) \lessgtr \ell i(n)$, and then take the least $N:\pi(N) \geq li(N)$. Littlewood's proof consists of two parts, one assuming the (still open) Riemann hypothesis, the other its negation. As Littlewood (1953), p. 113 puts it, 'it appeared later that this proof is a pure existence theorem and does not lead to any explicit numerical value [of N].' Unquestionably, the proof is indirect, since it applies the law of the excluded middle to an undecided proposition. Nevertheless, an essentially routine application of proof theory (ε-theorems or cut elimination) applied to Littlewood's original proof, extracts a bound for N; cf. Kreisel (1952). Of course, this use cannot be expected to give optimal bounds for which further ideas are needed, and it doesn't: it gives a bound of about

$$10^{10^{10^{34}}} \quad \text{compared to } (1.65)10^{1165}$$

found in Lehman (1966). Actually, as Littlewood realized (p. 113, l. 13-14), the size of N is not the only issue here: by (iv) on p. 115, a 'further idea' was alleged to be needed to extract a bound from Littlewood's proof, for example, by the switch from π to ψ. This impression is refuted, beyond a shadow of doubt, by the proof theoretic analysis which makes a *routine* application (to Littlewood's original proof for π) of a *general* (logical) method.

Historical Remark. To be precise, the ideas involved in the general proof theoretic procedure were applied, not the procedure itself. For one thing, such a procedure operates on a formalized proof, and, of course, Littlewood's original proof was not. But also, at the time the relevant procedures had not been worked out for formal systems very close to the language of ordinary analysis. It turned out that what were obviously the only critical steps in Littlewood's proof could be transcribed into the formalism of first order predicate logic, to which then-current proof theory applies; cf. Remark 5.2 p. 171 of Kreisel (1958). A significantly more systematic use of proof theory here would have to make the passage from informal proofs to their formalizations a principal object of study: this was premature before the advent of high-speed computers (at least, if the general impression is right that formalizations of such proofs as Littlewood's are too complex for humans); cf. §4(b).

(ii) *Unwinding a proof of Roth.* A. Baker brought up the following point in a conversation about Roth's theorem:

$$\forall n \forall \alpha \exists q_o (\forall q \geqslant q_o) \forall p \left(|\alpha - p/q| > q^{-(2+\frac{1}{n})}\right),$$

where α ranges over the irrational algebraic numbers (and the other variables over the natural numbers). Baker felt morally certain that the proof in Roth (1955) could be 'unwound' to yield *some* bound for the *number* of exceptionally close approximations to α, that is, the number of the set $E_{n,\alpha}$ (of rationals r):

$$\{r : |\alpha - r| \leqslant q^{-(2+\frac{1}{n})}\} \quad \text{where } r = p/q \text{ (in its lowest terms)},$$

the bound depending on n and the height of α. But he also felt that this bound would be insufficient for the use he made in Baker (1964) of the bound by Davenport and Roth (1955), which requires a 'further idea.'

As it happened the matter of unwinding had been considered in the literature, in Kreisel (1970), pp. 135-136, as an application of Herbrand's analysis of logical theorems of the form $\exists x \forall y\, R(x,y)$,

$$R(t_1, y_1) \vee R(t_2, y_2) \vee \ldots R(t_k, y_k),$$

where the terms t_i do not contain any variable y_j with $j \geqslant i$. This applies in an obvious way to Roth's theorem for fixed n and α; with q_o in place of x and the pair (q,p) in place of y. Inspection then shows that k yields a bound on the

the number of $E_{n,\alpha}$. This supports the first part of Baker's impressions. Modulo the uncertainty about the passage from Roth's informal proof to its formalization, discussed at the end of §1a(i), I have convinced myself that the $k_{n,\alpha}$ supplied by Herbrand's analysis are indeed too large for the application in Baker (1964). Incidentally, this example provides one of the few useful applications of Herbrand's own formulation, specifically, in contrast to the no-counter example -- interpretation which introduces fuctionals; in the case of $\exists x \forall y\, R(x,y)$ above, a new function symbol f is introduced (as a 'counter example' $\forall x \neg R[x,f(x)]$) and one gets Terms T_i, containing f, such that

$$\neg\{R[T_1,\ f(T_1)]_\wedge\ R[T_2,f(T_2)]_\wedge\ \ldots\ R[T_k,f(T_k)]\}$$

It will not have escaped the reader's notice that the interests of both Littlewood and Baker were so to speak contemplative, not activist. They *had* bounds -- and (as mentioned) there are *better* ones than those supplied by the original proofs considered. Contrary to activist propaganda, those interests are no less permanent than the search for better results, which, after all, will be superseded, too.

(b) *Hilbert's 17 th problem:* a case of a *knotty unwinding*. Artin's solution of Hilbert's problem was stated for archimedean formally real, that is, orderable fields K in which every positive element is a sum of $\leqslant$ k squares.

> If p is a polynomial with coefficients in K and $p \geqslant 0$, then $p = \Sigma(p_i/q_i)^2$, $1 \leqslant i \leqslant N$, where p_i and q_i are also polynomials with coefficients in K -- How large is N?

Because of the algebraic character of Artin's proof, it was clear that some such bound for N would depend only on k, the number n of variables and the degree of p; and not on, say, arithmetic properties of K (which might, however, permit 'better' bounds; cf. the end of §2).

Historical Remark. In seminars Artin had raised the problem of finding bounds, ever since his original proof in the twenties. He mentioned the problem to me some 20 years ago. As it happened, around the same time, the matter was also considered by A. Robinson, but in model theoretic terms. His proof appeared in Robinson (1955), not long after the proof theoretic solution had been found. Model theory supplied a recursive method of determining a bound (by trial and error applied to derivations in predicate logic), proof theory a bound involving n-fold *exponentiation*.

As in §1(a), or, for that matter, also in comparable uses of model theory in the fifties, Artin's proof was first transcribed into (first order) predicate logic, but with this difference: two not altogether trivial modifications had to be made.

(i) *Restatement of Artin's theorem*. In contrast to the cases in §1(a), Artin's formulation has to be modified since the condition that K be archimedean is not of first order. Inspection of Artin's argument showed that it was only used to ensure that $p \geqslant 0$ in all real closed extensions of K. So it could be replaced by this weaker condition. Incidentally, as observed in Henkin (1960), given this replacement a more elegant formulation is obtained for *ordered* K without the assumption that positive elements are sums of $\leqslant$ k squares: Under the new hypothesis, or, equivalently, if $p \geqslant 0$ in the (unique) real closure $\bar{K}$ of K, then

$$p = \Sigma c_i (p_i/q_i)^2 \quad \text{where} \quad c_i \varepsilon K \quad \text{and} \quad c_i > 0.$$

As usual, the proof was uniform for given n and d. So there are polynomials p_i^j, q_i^j in both the variables and coefficients of the *general* polynomial p, and polynomials c_o^j, c_i^j in the coefficients only $(1 \leqslant j \leqslant j)$ such that, for each j,

$$p = \sum (c_i^j/c_o^j)(p_i^j/q_i^j)^2; \text{ and if } p \geq 0 \text{ in } \bar{K},$$
$$\text{for some } j, \text{ each } (c_i^j/c_o^j) \geq 0$$

(ii) *Restatement of Artin's proof.* Very much in contrast to §1(a), it can hardly be claimed that the passage from Artin's own proof to a *proof* in the *language of predicate calculus* is altogether obvious, even after a reformulation of his theorem is given, as in (i) above. Artin uses an infinite tower of field extensions; determining a bound on the relevant finite portion of it, is tantamount to finding N: and one needs that finite portion before one can even begin to formalize a version of his proof in predicate logic. -- Actually two quite different (first order) proofs of (i) are sketched in Kreisel (1960). One keeps quite close to Artin's proof, the other combines the algebraic identities used in his proof with the completeness of the axioms for real closed fields and the so-called uniformity theorem of logic, a corollary of Herbrand's theorem.

The last five years have seen a considerable extension of proof theory; in particular, cut-elimination was extended to (impredicative) type theories in such a way that all cut-free proofs of first order theorems are themselves of first order. In other words, to an arbitrary (higher order) derivation of a first order theorem is associated a *particular* first order derivation: its cut-free (normal) form. Cut-elimination is then the procedure which mechanizes the mathematician's *unwinding* of proofs (or is, at least, a candidate for such a procedure). But the matter is not yet settled. Sure, if one insists on replacing Artin's ordinary (algebraic) language by the 'logical' language of type theory mentioned in the last paragraph, the passage from his own exposition to a formalization of his proof in type theory is pretty unambiguous; certainly as much as for the parallel passages in §1(a). However, this insistence may involve a strategic mistake, in accordance with the ordinary mathematician's instinctive resistance to the language of type theory and to other 'foundational' languages. This issue is a principal topic of §4(b) below.

Remark. At least at present the issue is also contemplative, and not activist (in the sense of the remark at the end of §2(a) above). In particular, numerically better bounds for N itself have been obtained in Pfister (1967) independently of d when K itself is real closed. (Both the proof theoretic and model theoretic methods yield bounds simultaneously for N *and* the degrees of everything in sight.) More interestingly, for suitable topologies on (suitable) K, one would expect *topological* versions of Artin's solution, where the p_i, q_i, c_i, c_o depend continuously on the coefficients of p or where c_o and q_i never vanish at all when $p \geqslant o$ in $\bar{K}$. Such results are certainly not to be obtained by unwinding Artin's proof.

ELABORATIONS AND DIMINISHING RETURNS

3. The main proof theoretic results on first order predicate calculus used in §2 are cut elimination and functional interpretations. They have been extended to stronger systems (including infinitary ones), usually by use of some kind of transfinite iteration or hierarchy, for philosophical reasons discussed in (a) below. When the process which is iterated is 'elementary', the dominant factor is (i) the 'length' or *ordinal* of the iteration; tacitly, together with a 'control' at limit ordinals by means of so-called fundamental sequences. When already each step of the process involves some kind of 'collection', one has (ii) *functionals of higher type;* tacitly, together with some defining schema for the latter. -- NB. The distinction between (i) and (ii) is quite well illustrated by the familiar (i) constructible and (ii) natural hierarchies of sets generated by iterating the so-called predicative, resp. the full power set operation; 'quite well', not perfectly since in the case of proof theory there are additional relations. Specifically, the computability of terms (for functionals) in (ii), is proved by transfinite induction on (orderings with) the ordinals in (i); conversely, 'definitions' by transfinite recursion in (i) are, *demonstrably,* satisfied by suit-

able terms in (ii). -- NB. Many of the elaborations contain material of considerable mathematical charm, as illustrated very well by the lectures of Girard and Ershov at this conference, which state results in a form independent of the original proof theoretic aims. Make no mistake about it: though perhaps exceptionally polished, Girard's material is quite typical of the kind of ordinal structures developed in proof theory (categories of not necessarily well-founded orderings with suitable maps and functors between them), and Ershov's is typical of -- what are nowadays called -- cartesian closed categories used for functional interpretations when certain informal continuity properties are needed (made precise by Ershov's topology or by appropriate limit space structures); cf. footnote 1 of Kleene (1959) or the 'principal result' (2.4 and 5.1) of Kreisel (1959).

Two obvious questions are prompted by what has just been said:

(i) Have we simply not elaborated *enough?* not put enough resources into the traditional aims? or,

(ii) Have the elaborations *only* heuristic value? by having led (some of us) to ideas which are effective only when divorced from the original, misconceived aims?

Before giving -- what seems to me -- convincing evidence for (ii) in the case of current proof theory, readers may wish to have some

Examples concerning (i) and (ii) from successful parts of logic. *Ad* (i): McIntyre's lecture at this conference illustrates very well (my) misjudgments on the use of elaborations in model theory, specifically, in connection with Morley (1965), usually considered to be the first piece of 'hard' model theory. Some ten years ago, I had high hopes of glamorous applications; cf. p. 152 of the original (French) version of Kreisel and Krivine (1971). These were not fulfilled in the next five years, and so the reference to Morley (1965) was dropped from (p. 186 of) the latest (German) version. McIntyre's results more than fulfill the original hopes, when the new 'hard' model theory is *combined* with results for suitable mathematical structures (groups, not, e.g., rings, as McIntyre mentioned), and emphasis is shifted away from simple-minded 'logical' cardinality properties to stability and the like.[1] -- *Ad* (ii): Gödel's work on the constructible hierarchy, L, mentioned earlier, provides an excellent object lesson here. L is an (elegant) extension of the ramified hierarchy introduced by Poincaré and Russell for their programme of predicative *foundations*. But as stressed in the clearest possible terms on pp. 146-147 of Gödel (1944), his work drops a requirement which is absolutely essential to that programme, since the relevant properties of the iteration process are *not proved predicatively* (but in axiomatic set theory itself, which is enough for Gödel's relative consistency results). A somewhat similar twist w.r.t. fundamental sequences for 'proof theoretic' ordinals was used successfully in Jensen (1972). Gödel's somewhat nostalgic description of his twist, *loc. cit.* as having primarily mathematical, not its original, philosophical interest seems (to me) to overlook the principal *philosophical* issue: Is the predicative programme, admittedly a philosophical affair, also of philosophical *interest?* And *one* test is to compare the results obtained by modifying it (as Gödel did) with those obtained by respecting it, as in certain autonomous transfinite progressions; cf. the survey in Feferman (1968). Of course, outside logic there are much more impressive examples of (ii), for example, from the familiar theory of ideal fluids,

[1] The need for such combinations is also familiar from applications of 'soft' model theory (p-adic fields) and of recursion theory (finitely generated groups), and of course from §2 above. -- *Warning*. The 'contemplative' results of §2 are to be compared to applications of model theory in the fifties which consisted in easy (general) proofs of easy (general) theorems, not to the more substantial applications in the sixties. In the fifties, model theorists familiar with the material of §2 found it to be as promising as then-current model theory (an impression which has not been justified so far); cf. A. Robinson's review of Kreisel (1958).

mentioned in the Introduction. This theory -- or, as one says, this 'idealization' -- is simply not adequate for its original aim, for hydrodynamics. But the ideas that came from the development of the theory, especially in the two-dimensional case, have *permanent* value provided they are suitably *separated* from the original aim (functions of a complex variable or harmonic functions, used to describe the potential and the flow of -- hydrodynamically pretty useless -- ideal fluids).

After these illustrations concerning the alternative between (i) and (ii) above we return to our principal concern, proofs. Here, as promised, is evidence for (ii), both w.r.t. (a) the particular hierarchies mentioned at the beginning of §4, and (b) traditional proof theoretic aims in general.

(a) *Two strategic assumptions* in the construction of hierarchies. The first concerns justifications (of principles P of proof), and is *restrictive:* P should be justified 'from below', via a hierarchy, by some kind of reduction to more elementary principles than P. The second is *permissive*: with any (reduction) step, an arbitrary finite iteration is taken to be 'given' too; in short, ω-iterations are not counted, nor their ω-iteration, and so forth; for a fairly, but by no means absurdly broad sense of 'and so forth', cf. Girard's lecture. -- NB. Trivially, idealizations are involved here. This is not the issue at all. What is questionable is the implicit assumption that they are even approximately adequate, for example, for studying reliability of proofs. In fact, there is a radical alternative:

Don't we do better by reversing the strategy altogether?

specifically, by not building up P from below at all, but by reducing length, the (finite) number of iterations of any one step. For example, suppose that, for some given P, the passage, in §2, from derivations d to explicit realizations t_d becomes complicated. What use is then the *possibility* of such a passage? One would actually look for P^+ which permit -- realistically -- simpler proofs than P, at the -- realistically negligible -- cost of losing that possibility altogether. A less extreme example is familiar enough from elementary arithmetic, where it is certainly futile to reduce numerical terms to numerals (0, s0, ss0, ...); instead one looks for new, more efficient *notations;* for an instructive application in 'advanced' arithmetic, cf. Feferman (1971) and its review where the aspect relevant here is pointed out. -- For reference in §4: Statman (1974) reverses the -- usual -- aim of eliminating cuts in order to reduce not length, but *genus*. More generally, one might try to mechanize a good deal of the related, familiar routine of introducing suitable lemmas for 'cleaning up proofs': after all, we learn to do this sort of thing almost automatically.

Viewed in the light of the considerations above, the successes of §2 may well constitute a kind of limit to useful applications of the guiding ideas of current proof theory; a kind of optimal value for the ratio:

additional information/additional effort

in a traditional proof theoretic analysis. To be a little more specific, we conclude *Part I* of this article with some generalities about proof theoretic aims.

(b) *Mathematical reasoning and mathematical objects: a discovery.* Trivially, the moment we make it our business to be self-conscious about our knowledge (of anything!), the so-called subjective elements of this knowledge become most prominent: they are thought of as particularly close to the thinking subject. In the case of mathematical knowledge, definitions and proofs -- as opposed to the objects defined or to the theorem proved -- are among those elements. As a matter of historical fact, whenever some branch of mathematics began to be analyzed, the first distinctions that came to mind concerned methods: projective and metric methods in geometry, algebraic and differential ones in analysis, and the like. It was a *discovery* that the particular differences mentioned were more profitably interpreted by reference to the different, possibly novel notions or structures for which results proved by different methods are valid.[2] In other words, we have discovered an unexpected adequacy of 'objectivist' analysis.

Historical Remark. Traditionally, and more dramatically, one speaks here of conflicting views of the nature of (mathematical) reality, of a grand *conflict* between: objective and subjective. This becomes much less dramatic when specialized to familiar examples: after all, there are projective and metric planes on the one hand, *and* there are projective and metric methods on the other. Far from being presented with a conflict, with a choice between different views on what there is, we have a very close relation between methods and objects, so close that the objects concerned can be characterized in terms of the methods; cf. the distinction between those physical objects which are, and those which are not visible to the (idealized) naked eye. The distinction is objective enough (and not particularly hard to make precise). But -- on present evidence -- it is weak simply because visibility is not a significant factor in most physical phenomena at all (for which we have viable theories). In short, there is a very real issue here, but much more subtle than the hackneyed business of reality.

Do the reservations (a) and (b) above finish the subject?

(of proof theory). Surely not, provided we look for phenomena of mathematical reasoning in which proofs are -- likely to be -- principal factors; in short, if proofs are to be *principal objects of study*; if we do not insist on standing on our heads, and think of proofs principally as a means, for example to analyze the 'meaning' of theorems.

PART II. A FRESH START

INTRODUCTION

To continue the view of scientific progress presented in the Introduction to *Part I*, readers should recall that, at least in existing sciences, the mathematical elaboration of early conceptions and the restriction to imagined experiments soon reached a point of diminishing returns. Generally -- this seems to be a fact of scientific life -- progress in the successful sciences really picked up only when striking laws were discovered which *had* been in doubt or had not even been suspected. In other words, they were found by genuine, not merely imagined experiments. For the present purpose it is not necessary to distinguish between such experiments and what are called observations: the latter concern phenomena that turn up in the course of nature, while experiments involve observations of phenomena in specifically designed situations. Of course, as in all matters of knowledge, observers are not passive: in experiments the external circumstances are manipulated (so to speak 'interfered with' before the observation), in observations a *selection* is made among the raw data. This selection involves, in effect if not by intention, the notions to which early speculations, discussed in the Introduction to *Part I*, had drawn attention; in particular, in the branches of physics considered there, one looks for phenomena exhibiting 'perfect' shapes,

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[2] For readers interested in constructive mathematics, the corresponding reinterpretation concerns the particular (new) species of operations for which the theorems hold -- as opposed to the methods of proof used to establish that the identities hold which the operations are claimed to satisfy. -- Occasionally, there are candidates for the reverse procedure, for example, when Brouwer came up with a characterization of functions F on choice sequences f in terms of his 'fully analyzed' proofs of $\forall f \exists n\, R(f,n)$, and F satisfies the identity $\forall f R[f,F(f)]$. As at the end of (a) above, one expects only a narrow class of cases where the *possibility* of 'extracting' F is useful: in general, F must be so simple that it is worth writing out a definition in full.

resp. properties of 'ideal' fluids. Incidentally, even if this *normative* terminology, of perfection and ideals, is inappropriate if taken to mean what 'ought' to be the case, it occasionaly makes good sense as a hint on what ought to be studied. In short, early speculations come up with such hints.

Natural history will be used below for those parts of science which formulate laws close to our ordinary conception of the phenomena involved. This is somewhat broader, but more appropriate than the usual meaning of 'natural history' which excludes all but the most rudimentary mathematical developments of the (ordinary) conception.

As is well known, some early speculations introduced also extraordinary views. The best known such view goes back to the Greeks: the atomic structure of matter. (It is so far removed from our ordinary views that we do not see the world as atomic even after we have absorbed atomic theory). However -- and this is perhaps the principal point of *Part II* -- the mere introduction of this extraordinary view was sterile. What was needed was *massive progress in natural history*, specifically the discovery of laws relating data belonging to sciences which are quite different for our ordinary conceptions, for example, laws in spectroscopy relating data from optics and chemistry. (Massive progress was needed, and not merely isolated relations of this kind; for example -- the substance with the composition of -- glass has long been known to be transparent).

Naturally, a *systematic* science, covering phenomena from different branches of natural history has to use extraordinary conceptions (or combinations of ordinary conceptions, which are so complicated that the difference in degree becomes enough of a difference in kind; enough to be consistent with the assumption in force, that we have to do with different branches of natural history). In general, laws relating different branches do not determine a particular extraordinary conception. But they pinpoint an area where such a conception may be *tested*.

The simple observations above will guide the exposition and the interpretations of *Part II*.

PROOFS AS PRINCIPAL OBJECTS OF STUDY: NATURAL HISTORY

4. *Explicit definitions* have turned out to be fruitful here. They provide the sort of striking phenomena (in the area of mathematical proof) which are needed for natural history in the sense of the Introduction above. And, as required at the end of §3, these phenomena cannot be analyzed in 'objectivist' terms, that is, by anything like current reinterpretations of the theorems proved. -- NB. This article confines itself to valid proofs though, presumably, fallacious arguments are also important for a study of reasoning (as illusions are for perception). Incidentally, fallacies are often adequately analyzed in objectivist terms, for example, in those paradoxes which come about when some of the axioms and inferences are valid for one notion, say, of 'set' or 'predicate', others for another notion; cf. the *discovery* discussed in §3(b).

Uses of explicit definitions are conveniently grouped into those where (a) notions are *introduced* by means of such definitions, and (b) already understood notions with many familiar properties are analyzed by being shown to be *equivalent* to others that are explicitly defined (in some given language). Roughly speaking, in (a) it will often be obvious, in (b) some 'foundational' work is needed to show that the explicit definitions can be eliminated (at all): but the quantitative differences between (a) and (b) will turn out to be quite delicate.

Examples. (a) An important strategy in applying modern abstract mathematics, say group theory to number theory, is this: (i) An operation O is defined number theoretically, say on $\mathbb{Z} \times \mathbb{Z}$ or modulo some prime. (ii) O is shown to satisfy the group laws. (iii) Specializing general facts about groups to O leads to number theoretic results. As a matter of empirical fact this strategy has often been strikingly successful; probably the most spectacular use is in Weil (1967) which brought to light a computational oversight in Siegel (1952). At the same time, here and elsewhere in current practice, the strategy is *logically trivial*, that is, eliminable in the following precise sense: Proofs of (ii) have turned out to be formalizable in elementary number theory (though, by incompleteness, this has to be checked), and the general facts used in (iii) are naturally proved in conservative extensions of that theory (by completeness of predicate logic this is automatic if the facts involved are of first order). Thus the uses of this strategy in current practice are candidates for the programme at the end of §3. -- (b) Much of traditional mathematics, for example, Euclid's geometry was dominated by the search for correct definitions of familiar notions (circle, tangent) in Euclidean terms, for which familiar (geometric) properties of those notions can be formally derived from Euclid's axioms. Clearly, these properties correspond to the facts about groups in (iii) above, though -- in contrast to the case of groups -- geometric properties were used long before there was an 'official' list of axioms for those familiar notions.

Bibliographical remarks. The examples (a) and (b) above play a basic role in the particular brand of natural history (of mathematics) described, somewhat grandly, on p. 42 of Bourbaki (1948) in terms of 'intuitive resonances' (to mathematical structures). The discovery of (explicit) definitions which are easy to grasp and to handle provides of course critical evidence for such 'resonances' (and for the significance of the notions defined): evidently, this applies to groups in (a) above. Presumably in order to stress the (obvious) similarities between the two kinds of uses of explicit definitions in (a) and (b), Bourbaki play down the differences. Finally, with the relentless logic of epigones, some followers want to suppress altogether the uses quoted in (b) above, for example, in the school curriculum. -- Less grand, but no less emphatic are the references to the importance of explicit definitions in Wittgenstein (1976), for example. p. 33 or p. 111. The idea, in (b) of *recognizing* the equivalence between an understood notion and an explicit definition, that is, of the correctness of the definition is as much anathema to Wittgenstein as to Bourbaki. It is perhaps worth noting that -- apart of course from the obvious literary differences -- there are several other significant similarities in the general views of Bourbaki (1948) and Wittgenstein (1976), above all, concerning the superficial character of the properties of proofs stressed in traditional foundations. Wittgenstein illustrates his points by means of examples from the most elementary kind of numerical arithmetic, Bourbaki from 'advanced' mathematics. Wittgenstein's skepticism extends to set theoretic foundations, while Bourbaki (seem to) pay lip service to the language of set theory on p.37. However, on p. 40, footnote (2), they stress the importance of their basic structures without even mentioning the definability of those structures in set theory. These matters will be elaborated in a review of Wittgenstein (1976) for the Bull. A.M.S., including the errors of both Bourbaki and Wittgenstein due to their ignorance of logic (of the sixties).

Granted then the -- obviously -- striking role of explicit definitions, one looks for *specific* factors involved in the effective use of such definitions; tacitly, always together with the pertinent axioms; cf. (ii) in Example (a) above. The reader should recall here from the Introduction (to *Part II*) that superficial or 'phenomenological', not 'hidden' factors are treated in (any) natural history; further, that their relevance is not expected to be established purely mathematically from familiar experience: the latter only helps one choose a sensible candidate for a (relevant) factor. -- NB. As in other scientific practice, the facts will be stated in terms of any notions we have learnt to use, without insisting on a premature analysis (of 'how' we know, for example, that a particular spectral line is blue); an extreme case is the use of 'metallic look' in early mineralogy.

(a) *Genus of proof figures* and the particular use of explicit definitions illustrated in Example (a) above. First of all, it is to be remarked that none of the familiar measures of *length* is a particularly decisive factor here. The *number* *of steps* (nodes) is reduced only (roughly) linearly by introducing new notions by explicit definitions. The *number of symbols* (in formulae) can be reduced exponentially, but striking uses of the strategy in Example (a) occur also without such shortening. In any case many arguments are strikingly affected by mere rearrangement, if thereby, say intricate cross references are avoided. This observation is, qualitatively, consistent with using genus as a factor, introduced in Chapter 1 of Statman (1974) where details are to be found. (Roughly speaking, the genus of a proof figure is the least genus of any surface in which the figure can be embedded without crossing). Here are a few rather general features of genus which are not discussed in Statman (1974), but illustrate the kind of considerations needed for the natural history of proofs (and which tend to be disturbing to someone brought up on traditional mathematical logic).

(i) Genus is *sensitive to the style* of formalization. Specifically, all derivations in a calculus of sequents have genus 0, in contrast to natural deductions (provided the nodes are joined where an assumption is introduced and used).[3] Far from being a defect, this sensitivity allows one to choose between the styles; cf. the shift to heliocentric coordinates in astronomy at the time of Kepler (which was essential for natural history, despite all the business about the relativity of space).

(ii) Genus is *hard to calculate* from the data: one needs a computer even for quite elementary derivations (and a good deal of effort to convert ordinary proofs into data suitable for high-speed calculation). An anecdote concerning such calculations: At Stanford a body of (natural) deductions had been stored in a computer, left over from the homework by students in a computer-assisted course on axiomatic set theory; information on the course can be found in Suppes (1975). Tarjan's algorithm was efficient enough for dividing this ready-made material into planar and non-planar deductions. The job would be formidable if instead written homework for an ordinary course had to be prepared for the computer.

Far from being a drawback, (ii) seems (to me) promising, granted that genus is a significant factor at all. For one thing, since computers are new, only a short while ago it would have been simply premature to study our subject empirically at all. Less trivially, (ii) reduces a, if not the principal complication in studying human behaviour, whether it be psychological or physiological (which is in practice rarely an important difference). Specifically, *knowledge of a theory* is liable to *influence the subject of the observation*; in contrast to other sciences where of course the influence of such knowledge on the observer, not on the object of the observation, can be critical, and where one needs safeguards against conscious or unconscious faking (by the observer). If a theory involves notions like genus, which are hard to calculate, there is a better chance that the subjects will not even try -- and so simply will *not know the theoretical prediction* (even if they know the theoretical laws). At the very least, the need for *statistical* tests is reduced in the case of such intractable theories. Finally -- and quite superficially -- the phenomena (of reasoning) considered here strike us as *obscure:* so if a theory uses superficial (phenomenological) notions like genus such obscurity would go well with a difficulty in actually applying the theory (to the data); cf. also the last paragraph of the Introduction to *Part II*. -- Incidentally, the actual computations of genus for the corpus of proofs by Stanford students mentioned earlier, were not particularly conclusive; at least partly because the whole material was just a bit too elementary to present any really striking differences to start with.

What has been said so far is quite sufficient to show that there are plenty of data for a natural history of proofs; in fact, whenever we find striking phenomena

[3] This distinction, in Statman (1974), pinpoints for the first time a measure for the difference between the two styles, though it had long been clear that there was *some* significance to that difference (as suggested in a general way by Gentzen's terminology 'natural deduction').

which do not invite analysis in objectivist terms (in the sense of §3). The role of explicit definitions discussed above is of course only one of many such phenomena. As observed in the last paragraph, the presentation of those phenomena for theoretical treatment, that is, the choice of data, is quite critical, especially if notions like genus are involved which are so hard to compute that only automatic data-processing is realistic.

Example. For a conventional[4] exposition of elementary logic, one of the best known 'reductions' is the explicit definition of an arbitrary *propositional operation* (tacitly, together with proofs of its axioms) in terms of (the rules for) say $\neg$ and v. What is lost by such reductions? Statman (1974) shows (actually for the implicational calculus, not for $\neg$ and $\vee$) that the *genus* can be increased unboundedly. In a lecture at Clermont-Ferrand (in 1975) he considered propositional quantifiers, and showed how *length* is increased too; those quantifiers allow explicit definitions since $\forall pP$ is 'short for' $P[p/T] \wedge P[p/\bot]$.

Correction. Though Statman (1974) refers to Kreisel (1973), tacitly, p. 267, his (successful) treatment followed totally different lines from what I intended at the time, that is, at the congress at Bucharest in 1971. My idea was that the choice of axioms for explicit definitions would be made with essential help of some kind of traditional *philosophical analysis* of the meaning of the defined notions, and the combinatorial side would look after itself as it were. Indeed, as I stressed explicitly on p. 258 and in the PS, I was looking for sustained philosophical analysis, in contrast to its use in current foundations where it is completely overshadowed by the mathematics (needed to develop the one-liners expressing those analyses). Statman concentrates on combinatorial, not philosophical analysis. -- Incidentally, five years ago I did not think (consciously) of Bourbaki nor of Wittgenstein in connection with explicit definitions, though I had read Bourbaki (1948), and I knew Wittgenstein in the forties. Actually, even today I do not remember his talking about the topic in my presence. Evidently he must have done this in view of the quotations from Wittgenstein (1976) given earlier, and equally evidently, I was not ripe to profit from those remarks.

(b) *Foundational languages: artifacts.* Trivially, the effectiveness of an explicit definition of a given notion will depend on the choice of *language* (to be used in the *definiens*). What seems to be less well-known is that the familiar foundational languages tend to introduce artifacts, in particular, when notions from geometry including descriptive set theory are defined in the usual way in the language of *higher order arithmetic*. The examples below come from my own misjudgments.

(i) There are two standard proofs of the theorem of Cantor-Bendixson; one identifies the perfect kernel of a (closed) set F (of reals) with its set of condensation points, the other with the limit of its derived sets. *If* one insists on transcribing those proofs into second order arithmetic, when F is coded by its set of complementary intervals (with rational end points), then the difference between the proofs is most obviously expressed in model theoretic terms: the former uses Π_2^1 - comprehension (applied to the predicate of being a condensation point), the latter uses only Π_1^1 - comprehension; cf. Kreisel (1959A).[5] This difference is illusory inasmuch as a so-to-speak more geometric formalization of the first proof (with additional primitives for real numbers, etc.) can be modelled

[4] For example, in contrast to an exposition based on normalization where the data include normalization rules, and so a 'reduction' must preserve normalization steps, too.

[5] The main result of Kreisel (1959A) does not concern the difference between the two proofs mentioned, but the relative complexity of F and of (any second order definition of) the perfect kernel of F.

by use of Π_1^1 - comprehension too. This was discovered by Friedman in recent developments (to third order theories) of his careful formalizations in Friedman (1976).

Corollary. The (striking) difference between -- careful formalizations of -- the two proofs above does not invite analysis in objectivist terms after all, and is thus a candidate for the programme at the end of §3.

Remark concerning the logician's (so far practically unsubstantiated) impression of a 'need' for *higher types* in ordinary mathematical practice. Of course, by footnote 48^a in Gödel (1931) mentioned already in §1(a), the *possibility* of a logical need exists: some arithmetic theorems of the (usual impredicative) theory of type $n+1$ just aren't provable in the theory of type n. But the *actual* basis for that impression may not be logical at all, but structural; cf. the structural improvements introduced by Friedman's *third* order language with axioms that are *weaker* (than those in the original second order formalization). It is certainly of interest for the natural history of proofs to present *alternative hypotheses* for such impressions (as was done above by distinguishing between logical and structural needs).

(ii) Many theorems which are formulated in the language of first order logic (and therefore, by completeness, *can* be proved by the usual rules of predicate calculus), are in fact proved by use of geometric and/or analytic methods. An obvious problem for the natural history of proofs is this: Are these mathematical methods needed to make the proofs manageable (by reduction of length, genus or whatever)? In Kreisel (1976) a candidate from the theory of real closed fields was suggested: the theorem established by Schütte and van der Waerden (1953) in their solution of Newton's problem of the 13 points (on the surface of the unit sphere). My specific suggestion was to formalize their solution in usual type tyeory where, in particular, trigonometric functions are explicitly defined. But closer inspection shows that elementary facts about geometric and topological notions (area of spherical triangles, or Euler's theorem on polyhedra) have quite unmanageable proofs in the language I had in mind. In other words, a representation in that language introduces complexities that are not at all intrinsic to the solution of Newton's problem.

Remark. It cannot be assumed that to every solution of Newton's problem in a suitable geometric language there corresponds a particular proof in first order predicate calculus (in contrast to solutions in type theory where such a correspondence is provided by normalization); cf. the transcription of Artin's own work on sums of squares into first order logic discussed in §2(b), as an issue where the 'instinctive resistance' of ordinary mathematicians to foundational languages is relevant.

PROOFS AS PRINCIPAL OBJECTS OF STUDY: SYSTEMATIC SCIENCE

5. To put first things first: How much natural history of the kind suggested in §4 do we need? Can we not learn from the history of successful sciences how to find a systematic scheme without wasteful detours *via* natural history?

Historical Remarks. Much work in natural history was indeed scientifically wasteful; it was superseded by later, fundamental science without being used. One example was already menioned in (the bibliographical remark of) §4: before X-ray crystallography, the natural history of minerals was preoccupied with shapes and colours or with the distribution of particular minerals in odd corners of the globe. The information obtained was sound enough, but simply does not lend itself to theoretical study. (The same applies to much of botany or zoology). A more topical example is provided by the subject of 'natural languages'. It has not inspired much confidence, perhaps because -- as somebody said -- these particular languages do not tell us much about the genuine *possibilities* (for human language), and even less about the *actual world* which is supposed to be described by language. -- Certainly, there have been bright ideas in the parts of natural history mentioned (perhaps as imaginative as much more successful ones); for example, d'Arcy-Thompson's on growth and form, or Chomsky's on transformational grammars. Not despite, but rather because of their familiarity and generality, these ideas do not even smell like germs for a systematic theory. -- As a memorable contrast: The superficially very special idea in genetics, to concentrate on bacteria and viruses, was not only successful for a systematic science of genetics, but immediately convincing: here one could observe -- in a humanly short time -- so many more generations that the difference in degree could be expected to produce a difference in kind (of the data involved).

Despite all the excitement about the discovery of formalization, at present there do not seem to be any even mildly promising ideas for a systematic or fundamental science of proofs. Worse still, the two pillars of all current work, (a) and (b) below (which -- at the present stage -- are surely sound for the natural history of proofs), seem -- to me -- quite weak.

(a) We look for *conscious* elements in (mathematical) reasoning, or, at most, the very mild extension to those elements which *we are able to make conscious to ourselves*. As usual, traditional philosophy discusses pedantic doubts (here: doubts about the reliability of introspection, existence of mental objects and the like), and thereby -- consciously or unconsciously! -- draws attention away from the critical issue,namely this: Do those (sub)conscious elements constitute *adequate data* for anything like a systematic theory? cf. the situation in mineralogy before X-ray crystallography (in the Historical Remarks above), and more generally the inadequacy, for systematic physical theory, of the domain of phenomena that can be made visible.

(b) Impressed by its unquestionably distinctive features, we separate *mathematical* reasoning from other intellectual activities; in humans, and of course in other species. Again, the issue is not the (official) alleged difficulty of making some such distinction *precise*, and even less the -- equally often alleged -- *prejudice* against assuming intellectual abilities in subhuman species (after all, before very recent advances in electronic technology, we could not even begin to record the more intimate features of their behaviour). The issue is, once again, whether this particular part of intellectual activity, which as it were strikes the mind's eye particularly vividly, is right for (building a fundamental) theory.

To repeat: (a) and (b) do not cast doubt on the possibility of progress with the natural history of proofs, where we make do with what we have. -- Let me conclude with a comparison between *stages in the study* of (what is usually called: the nature of) proofs and of matter. The comparison seems quite effective for orientation; of course, it is not suggested that proofs are material substances.[6]

A parallel: What is proof? and *What is matter?* (i) An important step in the development of the atomic theory was the discovery of *chemically pure* substances, among the mostly impure substances which dominate natural history. This allowed the distinction between atoms and molecules. Bourbaki (1948) mentioned already in §4(a), sounds very much as if their *structures - mères* (basic structures) are to be compared to chemically pure substances, in particular, to atomic ones. But as stressed in §4(b), foundational analysis has not yet taken the discovery of those analogues to chemical purity into account: By insisting on *one* foundational language, such as set theory or type theory, one enforces a literal, but purely

[6] The comparison is also useful for expanding the discussion, at the end of the Introduction, of the heuristic role of traditional philosophy. After all, the idea of an atomic structure is one of the most striking contributions of *speculative* philosophy -- perhaps matched by the idea of the relativity of space and time, made prominent by *critical* philosophy.

formal unity on mathematics. (On pp. 36-37 Bourbaki too begin with a fanfare on the 'unity' achieved by means of their basic structures -- but eight pages later they stress that there are several such structures, and also that quite a lot of mathematics, for example, what is left out of their treatise, is not 'composed' of those structures anyway). Bourbaki, surely quite properly, do not mention in this context the hackneyed business about an objective or subjective view of mathematical reality. After all, even granted the objectivity of those basic structures, the fact that they contribute to the 'profound intelligibility' of mathematics (p. 37) concerns specifically our intellectual equipment. (ii) Building on the discovery in (i), *chemical* atomism could be developed, and the molecular diagrammes of elementary texts in chemistry. This required only a quite crude idea of atomic structure, the *valency* of particular atoms; in fancy language, needed for a pun (on the representation of proofs by graphs, as in §4(a) above), molecules were represented by a

purely graph theoretic structure.

This can hardly be said to tell us what matter is (possible, as it were), since it leaves open an enormous number of combinations of atoms which never turn up at all. (iii) A really significant advance was made by relating valency to the *internal* structure of atoms, in Rutherford's theory refined by use of quantum theory. This not only made 'sense' of valency (as is well known). It did much more when Pauling derived from it a

metric structure,

specifically, the *lengths* of and *angles* between chemical bonds. This additional structure cut down the number of possible combinations sufficiently to make it an *effective scientific tool;* probably, the most spectacular uses were made in molecular biology. -- It would be a little too facile to compare the use of an internal structure of atoms to going beyond conscious elements in (a) above; or the quite essential information provided by such unfamiliar elements as radium to the need for broader experience in (b). However, it seems quite clear that in one direction the current graph theoretic representation of proofs has to be enriched (even though, presumably not by a conventional metric) since a proof is rarely convincing if the conclusion depends on something which is too 'distant' in our memory. In short, one expects that *some* hypothesis about memory structure is needed here (and, as mentioned already, the difficulties are much the same whether one thinks of memory in physiological or psychological terms). But also, after we have grasped a proof of a proposition, we use the proposition more easily: as somebody said, we know more when we have proved a proposition than when we merely know it is true. The functional interpretations mentioned in §§2-3 make explicit *some* such additional information (which, by §2, is occasionally useful). What is missing is a convincing *test* for deciding whether this particular addition even approximates the factors which are actually operative.* As so often, the trouble is not at all that we cannot think of any theory; if anything, we can think of too many which are roughly consistent with familiar experience (of mathematical proofs).

HEURISTIC VALUE OF TRADITIONAL AIMS: A DISCLAIMER

6. The view of scientific progress adopted in this article and described in the introductions to *Parts I* and *II* disregards the principal specific claims of traditional philosophy -- and the conscious or unconscious hope that something like this tradition would provide a short-cut to a fundamental systematic science, without detours via natural history. In particular, the view disregards the claim that traditional analysis is needed to *correct errors* in our ordinary conceptions, and the associated hope that corrections would so-to-speak automatically lead to the extraordinary conceptions needed for a basic systematic science. In the case of proofs, our idea of a *valid* argument is claimed to be in need of correction (or at least of some kind of analysis). And the hope mentioned is implicit in the so-called logical priority of validity which, being the 'essence' of proof, is assumed to be a basic element of any systematic theory of proofs. (A pun may be involved

here too, since unquestionably, discussions of validity were temporally, that is, literally prior to theories of proof.) Granted all this, the hope rests on the further assumption that an *analysis* of validity (tacitly, in terms realistically available at the present time) would be rewarding. Of course, the parallel to validity in the case of physics is the business of reality, which -- in its originally intended generality -- has not been of much consequence for the progress of physics. -- The claims just mentioned are disregarded in this article, but not dismissed nor rejected. After all, we have evolved in the world in which we live. So why shouldn't our built-in so-called a priori conceptions be a very good guide in science, both efficient and reliable, and not primarily a limitation, an obstacle between us and the Ding an sich? Certainly, a mild form of paranoia is needed to concentrate *only* on the limitation -- either with pride in our impotence or in horror of it.

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