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MATHEMATICAL LOGIC: TOOL AND OBJECT LESSON FOR SCIENCE

ABSTRACT. The object lesson concerns the passage *from* the foundational aims for which various branches of modern logic were originally developed *to* the discovery of areas and problems for which logical methods are effective tools. The main point stressed here is that this passage did not consist of successive refinements, a gradual evolution by adaptation as it were, but required radical changes of direction, to be compared to evolution by migration. These conflicts are illustrated by reference to set theory, model theory, recursion theory, and proof theory. At the end there is a brief autobiographical note, including the touchy point to what extent the original aims of *logical* foundations are adequate for the broad question of the *heroic* tradition in the philosophy of mathematics concerned with the 'nature' of the latter or, in modern jargon, with the architecture of mathematics and our intuitive resonances to it.

1. INTRODUCTION

The starting point of this lecture is the consensus, of the (silent) majority of mathematicians and the (vocal) bulk of traditional philosophers, that logical analysis contributes little to the epistemology of mathematics. Bourbaki and Wittgenstein are well known spokesmen for those majorities. Both would regard (imaginative) reflection on broad mathematical experience to contribute more than detailed analyses of logical aspects. The main difference is tactical. Wittgenstein believed that quite elementary mathematical experience is enough, and that higher mathematics may even distract attention from essentials, and Bourbaki doesn't. More about this later.

Over the last few years I have tried to set out at length the case for this consensus but with a special twist. Instead of *ignoring* mathematical logic, which would be the practical man's conclusion from the consensus, I drew on my experience in several branches of logic; having given them rope as it were for nearly 40 years; consciously! cf. the reference to 'calculated risks' in *Dialectica* **12** (1958) p. 369. Most recently, on the occasion of Brouwer's centenary, this was done, in a joint paper with A. MacIntyre, for constructive logic. It seems appropriate to use a different style on the present occasion.

The stress here will be less on the defects of mathematical logic for its original aims, but on *broad successes* for different scientific aims, as *tools*

Synthese **62** (1985) 139–151. 0039–7857/85.10

of reasoning, *extending* our possibilities of (mathematical) reasoning rather than on analyzing what – realistically speaking – is wholly expected. The successes are so broad that readers have to be merely reminded, not instructed by detailed lessons, to acquire a realistic perspective or, more simply: to regain a sense of proportion.

Digression for Specialists

These reminders create a feeling of panic because a great many elaborations of mathematical logic, which are indeed central for the original foundational aims, are seen to be sterile; perhaps, sterile by any standards, but certainly when compared to the broad successes of elementary logic as a scientific tool. This panic is itself the result of scientific immaturity because it ignores the many parallels where aims had to be changed radically, but experience gained in pursuit of those aims was fruitful as soon as *new questions were asked* and the old experience was *combined with more successful directions of research.* Astrologers had acquired experience in observing the stars, and did better when they dropped their original aim: of using knowledge of the stars primarily to predict human destiny. Alchemists had acquired many skills in manipulating matter, and used them to better effect when they dropped their original aim of turning base metals into gold (with the tacit hope that the price of gold would not be affected by the law of supply and demand).

Returning to logic, I checked in my longer recent papers mentioned above that there is plenty of scope for specialist experience in logic provided (i) new questions are asked and (ii) that experience is combined with more specific knowledge.

2. SETS: REASONING BY ANALOGY

Looking back 100 years the most stunning use of (elementary!) properties of sets is the *transfer of knowledge* about one structure to others, made possible by use of the set-theoretic notion of isomorphism: not all knowledge, but that which is expressed set-theoretically in terms of the relations preserved by the isomorphism. (The word 'structure' is used to indicate that one ignores all properties of the objects considered except those involved in the relations mentioned.) Of course the idea had precursors. Counting establishes the crudest kind of isomorphism, preserving only identity and difference, between finite collections with the same number. In geometry rigid translations preserve so-called intrinsic geometric properties. But the general idea of set-theoretic isomorphism has a so to speak qualitatively wider range of application. NB. We have here a narrow sense of 'analogy'; in compensation: there is nothing imprecise about this kind of reasoning by analogy.

Without exaggeration, the whole development of axiomatic mathematics was concerned with the question: Which sets, and possibly, which isomorphisms are rewarding objects of study? By and large the choices were not made by logical analysis.

Evidently, some general knowledge of sets, in other words, some set *theory* was needed, just as some arithmetic laws, for example, commutativity laws, are needed to make counting an effective scientific tool. But often the crucial step is seeing *what* to count, and in the case of sets, which kinds of sets to consider.

There is no formal conflict between these rewarding questions and the two principal foundational questions:

> Frege/Russell: How to define set-theoretically familiar mathematical structures (up to relevant isomorphism)?
> Cantor: What more can be said about the crudest isomorphism (mentioned earlier)? Specifically, when applied to infinite sets, that is, infinite cardinal arithmetic.

There is a practical conflict because (a) for reasoning by analogy it is *demonstrably* immaterial whether or not the structures themselves or the isomorphisms are defined set-theoretically; what is needed is that the knowledge *about* those structures should be so expressed (and not 'intensionally', that is, it should not involve any properties other than those listed in presenting the 'structure'), (b) cardinal arithmetic with its stress on a *single* kind of isomorphism and on studying *all* sets draws attention away from the much more delicate, but more rewarding matter of selection (of sets and isomorphisms to study). Put differently, the progress of mathematics would be affected if too much weight were to be given to flashy logical theorems; flashy because, consciously or unconsciously, the problems of selection are hidden. As a corollary, one gets a false perspective of mathematics, a wrong philosophy in the popular sense of the word, if one is blinded to the central question of selection by the flashy 'generality' or 'clarity' of the logical notions.

As always, the foundational work can be put to *pedagogic* use, to remove a blind spot, for example, of attributing the weakness of higher set theory to (brutal) incoherence rather than to the lack of imagination in the questions asked about it.

3. MODEL THEORY OF FIRST ORDER LOGIC: A BROADER KIND OF REASONING BY ANALOGY

The most successful use of model theory is as a refinement of (2). By restricting the set-theoretic 'knowledge' considered, specifically, to first order assertions about the structures involved, one widens the class of structures to which such knowledge can be transferred.

Reminder. First order properties of the (field of) real numbers to real algebraic numbers.

The most obvious direction of development is to discover more or less general, more or less delicate operations on structures which preserve first order properties (and more besides!)

Reminder. Various kinds of delicate ultraproducts which preserve, for example, measurability.

Warning. Of course ordinary mathematics also has its transfer principles, for example, Hasse's (local/global) principle. By definition, it is not a logical principle because it applies to specific objects. But, by a realistic measure of generality, it certainly competes with model-theoretic principles, for example, in the *number of non-trivial applications*. And once again there is a practical conflict, between the *choice* of notions and problems required by the aims just mentioned on the one hand, and by the foundational questions which led to (model theory of) first order logic originally on the other. Perhaps the most obvious, least imaginative foundational question which suggests itself is: What is logic? The point to stress here is that, contrary to a widespread misconception, the question can be made precise in perfectly natural ways, and, equally inevitably, leads to a substantial body of results. For example, so called abstract model theory contains many pretty, and some ingenious results. The same applies to a precursor of this subject, Tarski's theory of theories in the fifties, which aimed at specifying the 'privileged place' or significance of first order formulae (or of syntactic subclasses among them) by describing the *closure* *properties* of classes of models defined by them. It is fair to say that one of the first substantial uses of ultraproducts was made in this area, by Keisler and Kochen (later improved by Shelah), to describe classes of models which can be defined by a first order formula. This was obviously, a fine piece of *logic*.

But, philosophically – as always, for perspective – it is dubious when it draws attention away from less obvious, nonlogical classifications, and thus from the question of comparing the relevance of superficially quite different, but genuinely competitive schemes. (Reminder: the superficial generality of logical transfer principles draws attention away from the, realistically, quite competitive generality of Hasse's principle.) What is more, the question: What is logic? and all the paraphernalia surrounding it draw attention away from the possibility of a convincing

> evaluation of logical classifications *without* answering the general question,

for example, by finding many structures with obviously *significant differences* (for the domain of phenomena considered) of *logically obviously the same type*. As in §2, the foundational work had certainly good *pedagogic uses* of correcting false impressions, or, at least, ratifying formally corrections which were obvious from experience. For example, it had become clear in the sixties that some (logical) extensions of first order logic were useful at the cost of giving up certain 'short cuts', such as the use of diagrams and compactness. Lindstrom's theorem shows that the 'price' was right. Trivially, but contrary to superficial impressions, his theorem does not answer the question: Why predicate logic? but: Why *not* rely on the most popular metamathematics of predicate logic (compactness and Skolem/Loewenheim)?

Reminder. At a very early stage the most highly advertised aspect of first order logic was its formal precision: a formal grammar and formal rules of inference. But this is better discussed in the more general context of the next section.

4. FORMAL RULES: NONNUMERICAL DIGITAL DATA PROCESSING

The clear recognition of just how much reasoning – that is, as far as results are concerned, never mind the processes – can be mechanized is surely the most outstanding contribution of 20th century logic *sub specie aeternitatis*. It is not necessary to speculate on cause and effect to find the following facts memorable.

By 1937 the German engineer Zuse had patented an electromechanical computer, programmed by means of a punched film strip. He was aware, independently of Shannon, of the possibility of breaking down rules into a succession of Boolean operations which he could implement electromechanically. During World War II he enriched his programming language by adding first order operations (and the Germans had such high hopes of his engine that they called it V4). What he did not have was any implementation of codes operating on codes. For example, when a conditional program proceeds one way, if the outcome of a calculation is, say 1, another way if it is 0, the punched film strip actually splits!

Till the mid-fifties his computer was competitive with the electronic computers using the programming scheme introduced by von Neumann (with codes operating on codes); for example, it was used by the ETH at Zurich. But as the speed of computers increased, the advantages of having codes operating on codes became overwhelming.

Warning. A particular case of this is so called self-application when a code operates on itself. While this case can be made particularly memorable, philosophically it would be an error to single it out for emphasis. A much more relevant point is this: for mechanization one gains a lot by including partial operations *at all*, contrary to the most common everyday meaning of 'well defined rule' which requires a specification of the domain of definition. Naturally, when it comes to further development one will look for gains, for example, in computational *efficiency* when a sufficiently simple specification is actually available.

Of course, one needs the rudiments of recursion theory to exploit codes operating on codes, for example, Kleene's remark called $S(n, m)$-theorem, to be compared to knowing some arithmetic for an effective use of counting. (As mentioned so often, without commutativity, we should not be able to check a count by simply recounting.) But only scientific immaturity would lead one to expect that therefore more elaborate theory is likely to lead to more effective exploitation of codes operating on codes; least of all the kind of elaborations required by such foundational aims as exploring the actual possibilities of (human) reasoning in terms of computer processing of digital data.

Remark. Since there are plenty of scientifically immature people around, one must expect that such sterile elaborations will be quite popular. But it is worth being more specific, by looking at the various foundational aims involved.

One is *precision* and *reliability*. Certainly, *if* one is operating on masses of meaningless symbols as one says, then the possibility of formalization or mechanization is a pretty obvious prerequisite for precision. But it would be odd, so to speak illogical, to conclude from this that the reliability of an argument is enhanced by ignoring everything one knows about its subject matter, and by treating it as a manipulation of meaningless symbols! In fact, the practically absolutely essential method of *cross checks*, comparing steps in an argument with one's background knowledge or reinterpreting formulas (in Hilbert's terms: by using 'impure' methods), gives *new meaning* to the argument.

Remark. Readers will be familiar with claims for a special reliability of finitist and other constructivist methods (of proof, not calculation). This *was* unquestionably true when, about 100 years ago, people began to discover the use of other methods. But as the latter became more familiar, things changed. As somebody pointed out, the frequency of errors in so called finitist consistency proofs was staggering, at least before Gödel's second theorem provided cross checks. The talk about some kind of 'idealized' reliability is drivel because it ignores the possibility that an *inappropriate idealization*, suggested by a *false conception* of the possibilities of reasoning, has slipped in. Given how little we know about those possibilities, it would be odd if we had an even remotely correct conception.

Reminder. Of course, being special, finitist proofs do have some special properties including virtues. It just so happens that (special) reliability is not among them. This is best seen by looking at cases where Hilbert's program *has* been carried out, and obviously contributed nothing to reliability.

Another foundational aim concerns the question whether *all* 'effective' procedures have been codified, with distinctions between mechanical, physical or human effectiveness. Obviously the success of the 100 billion dollar computer industry – responding to 'human' needs – does not depend on the answer (since we don't know it), but on the fact that so much can be mechanized. Not the question whether computers can do 'in principle' everything that humans can do, but whether in practice computers can do a lot of things *better* than humans is decisive. Far from being a purely philistine reaction this switch takes into account a *principal lesson of* scientific experience.

Granted (as I believe) our concept of effectiveness, or, more pedantically, our few concepts listed above are unambiguous and capable of precise formulation, the more significant question is whether these concepts are appropriate to the phenomena concerned. Evidently, the less we know about the actual mechanisms envisaged, the more significant is this question.

Complexity 'Theory'

Unquestionably, the general points just made are familiar; for example, current interest in *sub*classes of the class of recursive procedures fits in here. But the choice of problems does not! Specifically, the classes of problems for which upper and lower bounds on their computational complexity are given, are selected by *purely syntactic conditions*: degree and number of variables of polynomials, length or quantifier complexity of formulas. This is in conflict with the bulk of mathematical experience in this century (which is full of more sophisticated and more successful classifications). It is also one of the relatively rare instances where Wittgenstein's slogan: der unheilvolle Einbruch der Logik in die Mathematik, is not wildly exaggerated.

Pedagogic uses of elaborations of recursion theory abound. Perhaps the least well known use concerns generalized recursion theory (on ω_1^{rec} and other so called admissible ordinals). At the beginning of the sixties, as I remember well, people expected precious little from looking at infinite ordinals other than a few small ones, and cardinals. Correspondingly, looking at the *fine structure* of L seemed to most people *Kleinarbeit* if not a mere connerie. Generalized recursion theory, and – this is perhaps even more important – its lively presentation by Sacks created confidence in the possibility of doing something with the fine structure. Consciously or not, depending on whether they were on intimate terms with their unconscious, set theorists picked this up, and used it to excellent effect.

It is a quite separate matter whether the subsequent development, of using Jensen's results on the fine structure of L for elaborating generalized recursion theory still further, was philosophically sound. It was not if it drew attention away from the discovery that

> the methods developed originally for recursion theory are more fruitful in the context of L than in their original context.

Exaggerating only a little one can compare this to using the methods of X-ray Astronomy, useful for studying the composition of stars, for elaborating astrology.

5. PROOF THEORY

Since I have published a good deal on this subject in recent years along the lines of this lecture it is superfluous to repeat it here, except for one comment. It concerns an aspect of Girard's work which he did not stress in his lecture. It is an *improvement* (not refinement!) of the classification of functions defined by something like traditional transfinite recursion.

Instead of using just *one* parameter, the ordinal of the canonical ordering involved (and allowing again definitions of descent functions by recursion on this ordering), he has *two* parameters: the ordering, and a particularly manageable class of descent functions τ:

> $f(0) = a_0 \quad f(n+1) = g(n, f[\tau^*(n)])$, where $\tau^*(n) = \tau(n)$ if $\tau(n)$ precedes $n+1$, otherwise $\tau^*(n) = 0$.

This is not a refinement because (i) orderings are *increased*, but (ii) descent functions are *restricted*. Only if one thinks of (ii) as fixed, does one get a refinement. It seems to me likely, though this has not yet been verified, that his improvement is fruitful.

Warning. It would be a philosophical error to hide this general direction for using Girard's ideas by claiming that they contribute to the heroic tradition, to understanding our intuitive resonances to the architecture of mathematics; an error analyzed further in the note below.

Autobiographical Note

Like everybody else, as a student, I was familiar with the skepticism of mathematicians (and other scientists) about logical analysis, called 'logic-chopping' in England. And so to speak by accident I was also familiar with Wittgenstein's presentation of their reservations. Presumably, at least unconsiously, I must have had my doubts about this skepticism because, like everybody else at the time, I was immensely impressed by the special theory of relativity, and Einstein's arguments had much the same flavor as logical analysis (and were more successful than anything the skeptics I knew had done). I could not have known how singular this success would turn out to be when we look at it now, and think of the role of technology in the second half of this century; not only in astronomy, but also biology.

Quite consciously, I was impressed by the passage from, admittedly absurd foundational claims to scientific tools. One would imagine that this could have been learnt from the historical record. But it is most convincing if one discovers it for oneself. In my case, it started with the logical atrocities of Hilbert concerning consistency as a criterion of soundness, and consistency proofs. (Very similar reservations about consistency published by Gödel and Gentzen had not become well known.) Yet, when one reversed Hilbert's aim, literally: ignoring Π_1^0 sentences which were his principal concern, one immediately found what is sometimes called the mathematical significance of consistency proofs.

A second nagging doubt about that skepticism was aroused by Wittgenstein's particular stress on *lack of precision* of traditional foundational concepts, so to speak, on their being ill thought out. Realistically speaking, they were not one bit less well thought out than, say, Democritus's ideas of an atomic structure. (I do not have the particular philosophical gift for making a drama out of an oversight; specifically, of first regarding a notion or aim as being 'clear' because it is familiar, and in fact clear enough in familiar circumstances, and then being shocked when the notion is found to be ambiguous elsewhere or when new technology allows us to choose a more sensible aim.) Be that as it may, there was a kind of logical itch to make precise the problematic notions bandied about in my immediate surroundings. Specifically, when I was still a student, the notion of elementary or direct proof in number theory, later of finitist or predicative proof.

There was an even greater itch to find ways of settling specific questions conclusively without giving precise definitions – for cognoscenti: so called basis results, in particular, completeness and incompleteness theorems for intuitionistic logic (without introducing some far-fetched semantics with fanfares of precision out of all proportion to what we know of constructive operations or proofs). I don't know whether removing an itch contributes *ad majorem gloriam dei* or *pour l'honneur de l'esprit humain*. But it surely does something for the dignity of man.

Before Wittgenstein's particular objection to lack of precision of traditional logical categories was met, I just felt ill at ease about simply ignoring them; unlike more perceptive, and presumably more experienced mathematicians. For example, as I learnt much later, in connection with so called constructivist foundations, C. L. Siegel pointed out, by implication, that other measures are just more significant than the logical variety (specifically, *reducing* the set of parameters or changing it as opposed to logical 'reduction' of definition principles or *rate of growth* of solutions). Occasionally the methods used for providing precision could again be turned into scientific tools; but elaborations, also by others, very soon reached the point of diminishing returns.

Evidently, there is no guarantee that nothing could be gained by adding even more of the same: after all, when, say, a socialist economy fails, a frequent suggestion is that it was not socialistic enough. I myself drew a different conclusion, by switching from

(kinds of) provability, principles of proof

to

proofs themselves, in particular, structural properties,

but still with the aim of contributing to a study of mathematical reasoning. Only when it became clear that the most essential features of proofs, the memory structures at work, were not even roughly represented in anything like our representations of proof, did I look for applications of experience in (structural) proof theory for limited technological uses. First, for the mechanical *transformation* of proofs and programs (as opposed to the glamorous goals of discovery, synthesis or verification), and, secondly for the unwinding or unpacking of genuinely complicated proofs, with special attention to the effect of mathematically 'trivial' changes on the process or even on the result of the unwinding.

A Distinction and a Disclaimer

Subjectively speaking, the most striking *general* object lesson I believe I have learnt from logic is really little more than a confirmation of common sense, the distinction between

bright ideas and germs of theories;

in the sense that the former function best as *remarks*[1] (constatations in French, Konstatierungen in German), and simply do not lend themselves to much theoretical elaboration, while the latter do. (NB. The latter need not be more useful than the former, by any realistic measure of usefulness.) Evidently, this remark does not support so called epistemological anarchy because it involves a distinction between two nonempty classes. For the same reason it conflicts with the canons of so called exact methodology. It is only a remark because it does not pretend to help us recognize whether we have to do with a bright idea or a germ of a theory. It goes without saying that bright ideas are likely to be needed *in combination with* genuinely rewarding theoretical elaborations. My impression is that the bulk of (effective) logical analysis belongs to the category of bright ideas just as, for example, the bulk of dimensional analysis in physics. This impression is entirely in harmony with another impression, namely, that you can't often expect to get something for nothing, logical ideas (des idées immédiates) being suggested by experience that forces itself on us.

The disclaimer concerns the unqualified rejection of mathematical logic, rather common among those who share the broad philosophy of the last paragraph. Their reasons are clear enough. They see the hollowness of the popular claims for mathematical logic. But they overlook the fact that the basic (= elementary) *notions of mathematical logic are often the perfect vehicle for formulating certain bright ideas memorably*; notions which can be learnt and handled easily while a good deal of literary talent would be needed to convey the same ideas reliably without the help of the logical vocabulary.

Wherever this function of mathematical logic is central, elaborations can be worse than useless. They throw dust in one's eyes, and *draw attention away* from the principal prerequisite for using the bright idea effectively, namely, to judge where it is relevant. The imagination needed for this is so to speak the price for getting something out of logic. Of course, almost any elaboration can be useful to others by providing striking *proof* that the point of diminishing returns has long been passed.

A word of reassurance: logic is certainly not unique in the respects attributed to it here. For example, the bright ideas of d'Arcy Thompson's *On Growth and Form* (or on wave packets near the Cornish or Pacific coast) seem to be in the same class: a flea jumps as high as a lion, but a turtle does not. In contrast, the simple ideas of Mendel in genetics do lend themselves to elaborate theoretical, in particular, statistical, development in population dynamics.

NOTES

Note added in proof. A much more fully documented presentation of the main points in this article is contained in the lecture I gave at Stanford in June 84, with the title *Logical Foundations: A Lingering Malaise*. Dana Scott originally suggested that I give such a lecture, since then arranged for a typed record, and has helped in many other ways.

[1] Or even: reminders; recall Dr. Johnson's well known reminder: It is not sufficiently considered that men require more often to be reminded than to be informed.

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