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Wittgenstein's lectures on the foundations of mathematics, *Cambridge*, 1939, from the notes of R. G. Bosanquet, Norman Malcolm, Rush Rhees, and Yorick Smythies, edited by Cora Diamond, Cornell Univ. Press, Ithaca, New York, 1976, 300 pp., $18.50.

W, the favorite pupil of Bertrand Russell, became famous after World War I for a slim volume *Tractatus logico-philosophicus*. Apart from the obviously impressive flair and vigor of the style the book remains an outstanding example of the heroic tradition of Western philosophy, with its questions about the general structure of knowledge or the correct analysis of (all meaningful) propositions. Since the questions certainly occur, to anyone, prior to any detailed intellectual experience, more or less the same is expected of the answers. *Tractatus* is quite remarkable in this respect: no appeal to anything that would ordinarily be called a discovery, about physical or mental phenomena, barely any use of new intellectual let alone material tools except–of all things–truth tables for propositional logic. Offstage there was a discovery, chemical atomism: the correct analysis of substances in terms of atoms and the chemical bonds between them. The general idea of *Tractatus* is that there is a (tacitly:finite) supply of simples corresponding to atoms, and so-called elementary propositions about them, independent of each other, as in elementary probability theory, but in contrast to chemical bonding. Arbitrary propositions are then to be analyzed in terms of elementary ones. Sense corresponds to chemically possible combinations of atoms. Actually there was very little about mathematics in *Tractatus* except for a brief reference to an operational analysis–in contrast to the set-theoretic analysis in *Principia*, which is also in the heroic tradition, but a less pure example.

In the decade after *Tractatus* was completed, W turned away from academic life, renounced an immense fortune, and did other romantic things. (He also retained, to the end of his life, a freshness of mind quite unusual even among those not desiccated by an academic atmosphere nor preoccupied with finance.) During that period he became disillusioned with *Tractatus*; so much so that a perfectly trivial objection by a friend triggered his decision to spell out his misgivings, not so much about his personal contribution as about the whole heroic tradition. (The objection concerned the inadequacy of *Tractatus* for a correct analysis of some expressive Italian gesture–as if this were a principal weakness of *Tractatus*.) W found that quite elementary mathematics provided excellent illustrations of weaknesses of traditional foundations, t.f. for short.

Trivially, W's revised views are 'revolutionary' for t.f.; but they are quite close to those of many thoughtful mathematicians, for example, in Bourbaki's most interesting (and no longer well-known) manifesto: *L'architecture des mathématiques* **[1]**.[1] A principal problem was to put those views into words convincingly, and W was aware of this fact; thus the last sentence of the book under review stresses [*The seed I am most likely to sow is*] *a certain jargon*; cf. **[2]**. The main aim of this review is to restate the complaints of W and

[1] [*n*] means the *n*th note at the end of this essay, containing historical and bibliographical details.

Bourbaki about t.f., with due regard for the discoveries of mathematical logic (which those authors neglected [3]). By and large, at least in the reviewer's view, the discoveries of logic support the principal complaints. For balance, some local virtues of t.f. will be mentioned at the end. To get generalities out of the way, it is best to begin with a couple of obvious distinctions.

Strategy and tactics (of W and Bourbaki). Naturally, their principal, though not their only target is the best-known branch or 'school' of t.f., familiar from the *formal-deductive presentation* of mathematics in a *universal system*, of the kind often found in the first chapter of a mathematical text (but barely referred to later). W was most familiar with so-called logistic foundations going back to Frege-Russell, Bourbaki with its (set-theoretic) variant going back to Cantor-Zermelo. Usually the universal system has a single 'primitive' symbol, $\in$, besides the logical operations; in logistic interpretations, $P \in Q$ is read as: P has (the property or 'structure') Q, and as: P belongs to (the set) Q in set-theoretic ones. It is then claimed that this sort of presentation provides the 'fundamental' analysis of mathematical notions (and proofs), and, at least occasionally, for example, by Russell, that the use of a single primitive reflects the 'unity' of mathematics.

Probably the most obvious difference between W's and Bourbaki's tactics in discussing such traditional claims is this: Bourbaki refer to wide experience in mathematics, while W uses very elementary examples. The latter are elegant (and popular because most foundational issues present themselves when we know little), but leave open to what extent they are representative of wider experience, too.

A more basic difference is strategic. Bourbaki simply record their impression (of set-theoretic foundations) on p. 37: 'This is only one side of the matter, and the least interesting at that', and then go on to describe a better alternative with the same *general* aim: to exhibit, in terms of Bourbaki's basic structures, what is vaguely called the nature of mathematics (Bourbaki speak of 'unity' too though there are several such structures). Bourbaki's strategy leaves professional philosophers cold who *assume* that the proper way to achieve that aim must use the notions prominent in t.f., and dismiss Bourbaki as mere mathematicians lacking the higher sensibility needed for a true interest in t.f.! In contrast, W attempts to convert the fundamentalists by 'deflating' the notions and thus the so-called fundamental problems of t.f., stated in terms of those notions. In W's words, he wants to *show the fly the way out of the fly bottle*. He does this with much ingenuity and patience, and some overkill, while Bourbaki ignore the fly which does not see the way out by itself (by the light of the basic structures).

Even granted W's pedagogic aim above, his style does not seem efficient. In the reviewer's opinion, current mathematical logic, which has developed several notions of t.f. (has, so to speak, given them rope), seems much better, and some of those developments have positive interest to boot; cf. [4].

General complaint: deceptive abstractions of t.f. When we know little (but want to make general, 'big' statements), we necessarily use superficial generalities, so-called abstractions. As an example Bourbaki cite, on p. 37, general talk about the 'experimental method' being the link between physics and biology, in contrast, as we know now, to the general laws of molecular biology, which, quite literally, are not superficial at all. Being unquestionably venerable, all branches of t.f. concern principally early generalities; for example, they all agree in regarding such superficial 'defining' properties as *validity* (of proofs) or *existence* (of mathematical objects) as principal subjects of study, refined by almost equally obvious (and early) subdivisions into broad categories: (i) constructive and nonconstructive validity or (ii) concrete and abstract, in particular, infinite objects. Different branches of t.f. usually differ in picking on one category or the other as comprising all (justified) mathematics, or in the exact boundary they draw; cf. logicist and set-theoretic foundations mentioned above, or finitist and intuitionist (constructive) foundations.

Debates over t.f. generate much heat, an occupational hazard (or attraction) for philosophers, who trade in justifications and similar hot commodities. More importantly, those debates obscure what, on general scientific experience is most suspect: the assumption that those broad categories, which are certainly on the surface, should serve for a 'fundamental' theory of (mathematical) phenomena which, on the surface, strike us by their diversity. This suspicion is further obscured, in effect if not by intention, by, so to speak, the opposite assumption, that (most of) those early ideas are not only unrewarding, but simply incoherent or, at least, very difficult to make precise. This is simply false. Quite a number of traditional so-called informal notions have been analyzed precisely and convincingly; both 'grand' and 'modest' ones; of course, not only in t.f., but also in traditional physics (rigid body, ideal fluid, perfect gas, etc.): it just so happens that most of these often very appealing concepts have turned out not to serve very well for a fundamental theory (and others less easy to develop, like chemical composition, were more essential); cf. [5]. Of course, precision by itself is rarely enough to inspire universal confidence; *astrology* is a good (extreme) example, as venerable as t.f., and a model of precision and clarity. And thorough or even brilliant justifications of definitions can be quite sterile; thus analyses of the *area of a triangle* in Hilbert's *Foundations of Geometry* have not helped with the (genuine) problems of defining the area of a *surface*.

In short, the general complaint (of W and Bourbaki) is that t.f. may be *poor philosophy*, in the broader popular sense of 'philosophy', specifically, if in practice the general aims of foundations are better served by alternatives, for example, by ordinary careful scientific research and exposition. In the reviewer's opinion this alternative is particularly superior to t.f. with regard to reliability, at least, in the bulk of mathematical practice. This conclusion is of course quite consistent with [3] and [5] which show that some developments of t.f. have occasional use (and appeal; being easy to handle, like other developments of simple-minded notions).

Principal complaint: better current ideas than t.f. (not, of course, for the specific problems most prominent in t.f., but for the broad general aims behind t.f.). For all branches of t.f. the matter of *explicit definitions* is utterly trivial: for validity because such definitions can be systematically eliminated from proofs, and for so-called ontology because no existential assumptions are involved. Both Bourbaki and W emphasize–of course in accordance with ordinary mathematical experience–the *choice of explicit definitions*, as incomparably more significant than the glamorous preoccupations of t.f., not only for discovery, a 'mathematical' affair, but also for intelligibility, a principal factor in reliability, and hence a more strictly 'foundational' business. (In brutal terms: an idealization, of reliability, for which this factor is trivial, is a poor idealization.)

Bourbaki treat explicit definitions at length on pp. 42–43, at least, implicitly, in connection with the use of basic structures for solving 'concrete' problems. The scheme is this: (i) A structure S is explicitly defined in 'concrete' (say, number-theoretic) terms. (ii) S is shown to be a basic structure, for example, (shown to satisfy the axioms for) a group or a unitary group. (iii) Known properties of the basic structure are used to yield number-theoretic information. Without exaggeration: experience shows that the conscious use of the scheme literally alters our view of mathematics, and so, in the popular sense: our philosophy of mathematics.–NB. The scheme *would* be significant for t.f. *if* in striking applications the means used to establish (ii) and (iii) were foundationally problematic, not 'reducible-in-principle' to the usual methods of number theory. Not only logicians but, occasionally, also mathematicians *assume* that this must be so. They are wrong. The scheme above is unquestionably effective in existing practice also where the assumption is demonstrably false, as shown up to the hilt by the work referred to in [4], for a wide range of precise formulations of the notions involved.

W stresses the importance of explicit definitions in so many words, specifically, in connection with logistic foundations of numerical arithmetic (a principal topic of his own, limited study of logic). Here structures are explicitly defined in logical terms, and shown to satisfy familiar arithmetic laws. (This kind of thing calmed Frege's indignation at the 'logical scandal' of being left speechless by: What is the number 1?) W stresses the following aspect of logistic foundations (which Frege and Russell, and of course W in his youth, had ignored as being trivial 'in principle'). If the logical formula F_A expresses the arithmetic theorem A, knowledge of A is needed not only to recognize this fact, which goes without saying, but simply to prove F_A *convincingly*. An analogue to this is used frequently in current algebra; if A is a theorem about ordered fields, and F_A the corresponding logical formula, then F_A is in fact proved by using set-theoretic or algebraic operations on ordered fields. Sure, by the completeness theorem, F_A has a proof using only the rules of ordinary predicate calculus, too; but this fact, which is certainly fundamental *if* the assumptions of t.f. are granted, has turned out to be quite marginal in practice. In short, as a matter of empirical fact, arithmetic does more for logic than logic for arithmetic; cf. [6].

Specific complaints about some glamor issues of t.f. W's pet aversions will be illustrated here by just two examples; references to mathematical developments of his complaints are given in [7].

W had a particularly strong aversion to one of the more dramatic topics of t.f. : the matter of *contradictions* as in the paradoxes, or their absence, *consistency*, as in Hilbert's program. Incidentally, at least by implication, Bourbaki too are unimpressed; treating consistency (or the existence of some model) as a by-product; for example, the model of the –theory for the field **C** of–complex numbers furnished by the Euclidean plane, which was originally hailed for 'legitimizing' $\sqrt{-1}$, is reinterpreted on top of p. 43 as a useful property of the plane. Be that as it may, the familiar dramatics about consistency, etc., are unconvincing. For one thing, one is accustomed to oversights or blindspots which result in straight errors and possible contradictions; cf. Hilbert's recipe for proving Fermat's conjecture (finitistically!): *so lange herumrechnen, bis man sich endlich verrechnet*. Also there are confusions between notions: when P is true for one of them, and $\neg P$ for another, it is simply futile to ask for a precise location of 'the' error. On the other hand, generally speaking, consistency alone is not too reassuring (from experience with skillful liars). W had a pet complaint: Why not ensure consistency trivially, by modifying the rules in the obvious way? Though he asks, in effect, what would be lost by this, he doesn't really stop for an answer. Actually, his point is well illustrated in a paper by Rosser on mathematical logic, well before 1939 (without having been recognized explicitly by its author); cf. (i) of [7].

Another matter which had long been prominent in t.f., and especially in the writings of W's teacher, Bertrand Russell, is the topic of *higher* (*infinite*) *cardinals*. W was particularly offended by the use of the harmless diagonal construction to support heavy infinities; 'harmless' inasmuch as–to W–the point of the construction was perfectly well illustrated by proving that the set of (ordinary) polynomials in one variable cannot be enumerated by a polynomial in 2 variables. W preferred to use the construction in the context of rules (for partial functions), specifically for proving Gödel's incompleteness theorem, incidentally without appeal to the liar paradox. W considered rules $\rho_1, \rho_2, \ldots$ for sequences of 0 and 1, where some ρ_n says: put 0 or 1 at the mth place iff ρ_m tells you to put 1, resp. 0 at the mth place. (So ρ_n says: put nothing at the nth place–and the value of $\rho_n(n)$ is undecided; cf. also (ii) in [7].) Bourbaki's manifesto does not seem to commit itself on the matter of higher cardinals. But it seems fair to say that they would study problems about the natural numbers by means of (suitable generalizations to) finite fields rather than, say, by means of infinite ordinals (which Sierpinski attempted to do, for example, in his counterexample to an analog of Fermat's conjecture).

While the substance of W's complaints is certainly eminently reasonable, the ordinary style of mathematical logic is more efficient (as mentioned on p. 80): one reformulates the theorems involved (as in [7](i) for the case of Gödel's second incompleteness theorem, and in [4] for debunking the 'logical strength' of languages of higher type). In the reviewer's opinion, W is too tolerant of t.f., for example, far too soft on *formalization* as a (necessary) condition for mathematical rigor. Thus, in an exchange with Turing on the subject of making ordinary proofs 'more' formal, W does not question this aim but merely assumes (p. 127, l.-13) that it would be 'easy' to do, incidentally, contrary to an almost universal opinion. The business of formalization is at least as prominent in t.f. as W's pet aversions (and perhaps not so easy to put into perspective; cf. [8]).

W's complaints live up to one of his quotable quotes (on p. 68): Don't treat your common sense like an umbrella. When you come into a room to philosophize, don't leave it outside .... Of course, 'philosophy' (of mathematics) is not meant here in its academic sense, of t.f., but rather in its popular sense as on p. 81. We shall return to possible inadequacies of common sense at the end of the review.

W's advice in place of the kind of analyses proposed in t.f. When confronted–or, in W's terms, 'puzzled', as on p. 266–by a philosophical problem about (mathematical) notions or proofs, we should see what we *do* with them, how we *use* them (in the lectures under review W concentrated on uses outside mathematics but no longer in conversations in the forties mentioned in [**2**]). This is like the familiar advice in ordinary mathematics to try and see what makes a proof *work* or, more formally, *dégager les hypothèses utiles*. Fair enough, compared to other elastic advice on conduct (*ad majorem gloriam dei* or its 'enlighted' up-date: *pour l'honneur de l'esprit humain*). But in really doubtful cases, usually more imagination is needed to find the concepts, the *cadre*, for stating a satisfactory answer than to think of the troublesome notion or proof in the first place. As to 'puzzles', many solve themselves in the ordinary course of nature, for example, by means of memorable counterexamples, in particular, ($\mathfrak{F}_R$ in [**7**]) for W's complaint about consistency. As to his other specific complaint, what was there to see in 1939 *à propos* of higher cardinals? Work that has been attributed to W's or related advice is pretty varied, and of uneven interest [**9**].

Balancing the account on the positive side of t.f. In the reviewer's opinion the weaknesses of t.f., inherent and compared to available alternatives, mattered less to W than the style of t.f.: (i) the almost staggering banality of 'fundamental' notions and problems compared to the ambitious general aims, and (ii) the–basically pretentious–simple-minded language used to formulate the results of t.f. For obvious reasons, elaborated at the end of [**9**], it is far beyond the scope of any review–and certainly of this reviewer–to try and assess the pedagogic or heuristic value of the stylistic feature of t.f. just mentioned. But it is worth remembering the *possibility* of such a value; perhaps best by reference to related aims, notions and problems which are of about the same vintage as those of t.f., but have made much more progress; specifically, the ideas of the Greeks about physics, in particular, space, time, matter. ([**10**] contains some documentation concerning the rest of this review.)

The first examples one thinks of are, of course, spectacular, such as Einstein's most artistic presentation of the special theory of relativity in traditional philosophical terms, in particular, of the *skeptical*, so-called positivist or operational tradition. As a result of relativity theory there are now masses of data which would admit a 'purer', purely mechanical presentation (without bringing in light, that is, electromagnetic phenomena at all). But Einstein's presentation seems to have a kind of permanent pedagogic appeal even to those of us who, like Einstein, have grown weary of positivism (which tells us to begin with operational definitions, as in [**9**](ii)a, when in fact theory is needed for their choice). As far as mathematics is concerned, the switch from (i) the nineteenth century's version of the axiomatic method, used by Frege, Dedekind and others to set up categorical axioms (cf. [**1**]) to (ii) its current first-order version (including most of Bourbaki's basic, in particular, algebraic structures) continues to be introduced in so-called formalist terms, formalism being intended as the specialization of positivism to mathematics. In short, the particular features of knowledge on which positivism concentrates, seem to be occasionally central, at least for pedagogy. Evidently, spectacular successes are few and far between.

For the present (by **[9]**: necessarily statistical) purpose it is more interesting to look at modest, but appealing uses of t.f. (and their counterparts in physics). For balance, two illustrations from the *speculative* tradition will be given. Both are due to Gödel (who has shown more discretion and above all more flair than most exponents of that tradition). In physics the search for ghosts has so far not proved generally rewarding. But it can lead (one) smoothly to cosmological solutions of Einstein's field equations in general relativity theory with cyclic time; cf. **[10]** for possible alternatives. In mathematics, the search for open problems, say about the natural numbers, which are settled by means of (axioms about) large cardinals, has not been very rewarding either. In fact–and this is of course the principal conclusion of **[4]**–the superficial impression that anything like nondenumerable cardinals is used in existing analytic number theory, is simply false. But there is certainly a pedagogic interest in the *possibility* of any effective use of higher set theory for number theory, discovered and stressed by Gödel. What is more, the possibility is 'revolutionary' in the sense that it does not seem to be even remotely suggested by the bulk of mathematical practice.

It seems to the reviewer that, used with much discretion and a little flair, the ideas of t.f. provide *occasional* checks and balances on the strategy of relying on the 'needs' of current practice (Bourbaki) or on current uses (W), presumably, most often when the matters considered are far removed from current scientific study and uses. (By **[9]**(i), mathematicians tend to avoid such matters, like free choice sequences and large cardinals, to mention minor topics from the constructive, resp. nonconstructive branch of t.f.) When musing about the virtues and limitations of t.f., readers may wish to recall the memorable successes of natural science in this century (which we can, perhaps, view with more detachment than our own subject). Some of the early ones in the first quarter, say on atomic and cosmological matters, have a distinct flavor of t.f. Others which have, literally, changed our view of the world even more (like Rutherford's on the structure of the atom), and the extraordinary advances of the last 25 years, which have changed our view of ourselves too, do not. Naturally, the faithful either disregard those advances as not 'fundamental', or assume that things would have gone even better if the early preoccupations had persisted.

NOTES

[1] Bourbaki's manifesto appears on pp. 35–47 of *Les Grands Courants de la Pensée Mathématique*, edited by F. Le Lionnais (Cahiers du Sud, Paris, 1948; MR **10**, 239). Despite the title, by p. 42, Bourbaki are also concerned with mathematical activity, our 'intuitive resonances' to the 'architecture', that is, to Bourbaki's basic structures–at least within mathematics; by p. 46, Bourbaki regard such 'resonances' to structures occurring outside mathematics as problematic, in line with what is nowadays called the 'unreasonable effectiveness' of mathematics for physical theory. (Would it be *obviously* more 'reasonable' if we were not effective in thinking about the external world in which we have evolved?) Occasionally, one has to read between the lines. Thus the negative remarks on pp. 45–46 about categorical axioms (in contrast to the axioms for basic structures which are realized in many structures) are naturally interpreted in opposition to a preoccupation of t.f., the analysis of so-called informal notions, discussed further in **[5]**. Some of the best-known analyses of this sort have been of little use: Would Gauss' *Disquisitiones* have been better if he had started with Peano's axioms? Less trivially, in practice, both in pure and in applied mathematics, the particular informal notions we start with often turn out to be unmanageable or otherwise unrewarding, and it is simply better to axiomatize which properties of such notions have been used (for some striking conclusion). What Bourbaki actually say, about the 'sterility' of categorical axioms, is a bit glib. By neglecting such axioms altogether, one loses (pedagogically) useful explanations of the choice of familiar axioms in algebra and of so-called formal independence results; cf. Proc. Sympos. Pure Math. **28** (1976) on Hilbert's problems, **[HP]** for short. (For specialists: pp. 101–102, resp. top of p. 103 are meant.)

[2] The matter of jargon, or style, came up quite often in my conversations with W (from 1942 to his death in 1951), for example , once after W had invited F. J. Dyson, who at the time had rooms in College next to W's, to discuss foundations. Dyson had said he did not wish to 'discuss' anything, because *what* W had to say was not different from anything everybody was saying anyway, but he wanted to hear *how* W put it. W spoke to me of the occasion, agreeing very much with what Dyson had said, but finding Dyson's jargon a bit 'odd'. On another occasion W said: Science is O.K.; if only if weren't so grey. Incidentally, 'style' was not a dirty word in the Cambridge of those days, though its significance was not as forcefully analyzed as nowadays, for example, in (the first part of) Solzhenitsyn's Nobel Prize lecture or Bellows'. At least in my own experience the style of W's conversations on foundations (not on everyday matters!) was very different from his public performances, which were always tense and often incoherent; more detail will be given at a lecture to the Forum Philosophicum Austriacum (September 1977). Without exaggeration: What W actually said in the seminars I attended, did not express at all well his views at the time. This seems very much to the point in connection with the present book, which does not even record what W said in the lectures, but what a bunch of students thought he had said.

[3] W makes passing references to some kind of (mathematical) interest of mathematical logic which had grown out of t.f., but without any hint of what that interest might be. Though this is easier to state now, by 1939 (and especially by 1948, the year of Bourbaki's manifesto), some people with their wits about them had a pretty good idea; for details, see Vaught's story of model theory up to 1945 in Proc. Sympos. Pure Math. **25** (1974), especially about Malcev and Tarski. The principal uses of logic divide into (i) solutions of previously stated problems, and (ii) adequate formulations of natural questions, the latter being, perhaps, of greater interest to philosophers of mathematics. As to (i), the best-known use of logic applies model theory to prove (an asymptotic version of) Artin's conjecture on p-adic fields. To be precise, the proof combined a little logic with a good deal of algebra; but the fact remains that though some kind of relation between p-adic fields and fields of formal power series had been recognized by algebraists, model-theoretic notions were needed to formulate the relation precisely enough to finish the job. (ii) A good example of using logical, in fact, recursion-theoretic notions for stating a theorem (not only for its proof) is in Higman's work on finitely generated groups [Proc. Roy. Soc. Ser. A **262** (1961), 455–475]. This is a good answer to the (natural) question: 'Which' finitely generated groups can be embedded in finitely presented ones? A similar question, with logical answers, is this: What makes algebraically closed or real closed, so-called maximal, ordered fields 'special'? These fields are singled out, among all, resp. all ordered fields (by A. Macintyre) by reference to the elimination of quantifiers ([Fund. Math. **71** (1971), 1–25], resp. in the abstract, jointly with K. McKenna [Notices Amer. Math. Soc. **24** (1977), A28, Abstract #742-02-4]). Unquestionably, this type of work, especially in (ii), is *satisfaisant pour l'esprit*; but it is hardly central. *Though results from logic are of course applied to many areas, within any one area the successes are strictly local*; (if a choice had to be made) one would lose more by neglecting Bourbaki's basic structures than even the most respectable parts of logic such as model theory or recursion theory.

Apart from local uses of logic as in (i) and (ii) above, there is an almost endless list of applications to intimate pedagogy, to answer contemplative, 'useless' questions which thoughtful mathematicians often ask (themselves): What do we know from what we have done so far? (which is 'useless' if we know that later we shall go much farther);cf. **[4]**.

[4] Good examples of a fly in a fly bottle (or of a 'useless' question in the sense of [3]) come from the debates about the axiom of choice or about so-called nonelementary proofs in number theory of purely number-theoretic theorems. (In the twenties such proofs were distinguished by the use of function-theoretic methods; more recently, by the use of l-adic cohomology, as in Manin's problem I(a) on p. 36 of [**HP**]. Of course, some proofs which happen to be elementary are of interest; the issue is whether this 'raw' interest derives from their elementary character.) Practically speaking, except to doctrinaires, knowing how to use the axiom of choice and other nonelementary (civilized) methods is obviously a good thing. But even a nondoctrinaire reflective mathematician may simply want to know if these methods are eliminable from the proofs considered, as in Serre's question whether 'such' uses of the axiom of choice as in his study of homotopy groups are logically necessary. (Though asked in the thrifty fifties, the question was not intended to be useful; for example, it was not expected that a general answer to this question would help in, say, the actual computation of homotopy groups. In short, no illusions were involved.) Inspection of Gödel's work on–what he called–the relative consistency of the axiom of choice gives an easy negative answer to Serre's question, and, of course, a precise formulation of a whole class of 'such' uses; cf. p. 165 of [British J. Philos. Sci. **7** (1956)]. (Consistency was not the main issue because the axiom of choice is true for the (only) notion which, at present, serves to make the consistency of the remaining axioms evident.) Concerning the business of nonelementary proofs, logicians have spent a good deal of time showing that those occurring in *current* number-theoretic practice can be eliminated. In the process we have dotted the *i*'s and crossed the *t*'s by making distinctions between 'direct' and other elementary proofs, introducing suitable definitions of 'logical strength', and finding formal languages progressively closer (than that of set theory) to those used in the branches of mathematical practice concerned. In this way it became progressively easier to verify that the usual nonelementary proofs can be mechanically converted into–obviously–elementary ones (although, or course, there are purely arithmetic theorems in current *meta*mathematics for which the analogue is not true; cf. pp. 112–113 of [**HP**]). Put differently, the set-theoretic principles used (implicitly) in actual practice are of low 'logical strength' inasmuch as the replacement schema and other schemata are applied only to formulae of low logical complexity, cf. pp. 108–109 of [**HP**] or the more detailed exposition by Friedman [Ann. of Math. **105** (1977), 1–28]. But since the instances of those schemata which happen not to be used, are not in doubt either, we have little more than a modest discovery of a temporary feature of contemporary practice: we have not yet learned to use other instances efficiently, just as it took time to learn to use efficiently the law of the excluded middle applied to, say, the Riemann hypothesis. In fact, with the elimination before our eyes we see how little is gained by it. As a consequence, and as an example of how mathematical logic actually supports the doubts of W and Bourbaki about t.f., mentioned on p. 81, the issue of nonelementary proofs or, more formally, of 'logical strength' is discredited, and thus the claims of those branches of t.f. for which the issue is central, are refuted; cf. a similar use in [**HP**], top of p. 116 for consistency proofs of obviously consistent systems. Admittedly, the effort involved recalls Bertrand Russell's description of *Principia* as 'a parenthesis in the refutation of Kant' (on p. 75 in *My philosophical development*, New York, 1959). But we have not stopped at such refutations. We have gone on to look for factors which, unlike logical strength, do distinguish between elementary and prima facie nonelementary proofs, and above all measure what is *gained* by the latter; cf. p. 127 of [**HP**] concerning the reduction of the *genus* of proof figures as a possible measure of the difference.

[5] In this century mathematical logic has provided analyses of traditional concepts (logical language, formal rule, etc.) by means of definitions which are at least as convincing as famous definitions in 'old fashioned' mathematics, for example, of the notion of *length of curves* or, at the other end of the scale, (planar flow of) *ideal fluids*, by use of calculus, resp. function theory.–NB. Both definitions express correctly the notions intended: this is not the issue at all. Both notions are of course so-called theoretical idealizations: only, length does, and the other one does not isolate a dominant factor in (the bulk of) geometry, resp. hydrodynamics; cf. p. 81 about *correct* definitions of the particular notions prominent in t.f. and (lack of) *interest* of anything like those notions for the general aims of foundations. Incidentally, it is now generally recognized that the notion of formal or, equivalently, (idealized) mechanical rule in the sense of recursion theory is a poor idealization for the study of computers; but, for example, by [3](ii) it is a good tool in algebra (and number theory)–to be compared to those parts of function theory which were originally developed for the analysis of ideal fluids, and have found impeccable uses elsewhere; cf. also the end of this review.

[6] In connection with logistic foundations, W overreacts to, admittedly, exaggerated claims; he speaks of 'the disastrous invasion of mathematics by logic'; cf. [Acta Phil. Fennica **28** (1976), 166–187]. Bourbaki observe the academic proprieties; on p. 37 they are respectful about the language (of set-theoretic if not logistic foundations). But when, in footnote 2 on p. 40, they come to the importance of their basic structures, they do not even mention that such structures can be defined in the language of sets (and membership, $\in$). Thus by implication they dismiss the familiar claim, mentioned on p. 80 above, that this definability constitutes the 'unity' of mathematics.

[7] W's 'specific complaints' have been developed into mathematical theorems in the literature. (i) W's idea of modifying any system $\mathfrak{F}$ into an obviously consistent one, $\mathfrak{F}_R$, is treated systematically on pp. 46–48 of [Dissertationes Math. Rozprawy Mat. **118** (1974)], the subscript 'R' standing for Rosser who used a similar idea in [J. Symbolic Logic **1** (1936), 89–91]. If $\mathfrak{F}$ itself is consistent, $\mathfrak{F}_R$ has not only the same theorems as $\mathfrak{F}$, but even the same proofs. What is 'lost' by the passage from $\mathfrak{F}$ to $\mathfrak{F}_R$ becomes clear by stating the hypotheses of Gödel's second incompleteness theorem properly. In the usual systems $\mathfrak{F}$ the consistency of $\mathfrak{F}$ cannot be proved. In $\mathfrak{F}_R$ (the consistency of $\mathfrak{F}_R$ can, but) the adequacy of $\mathfrak{F}_R$ for *numerical* arithmetic cannot be proved even when $\mathfrak{F}_R$ is adequate. (ii) W's use of a variant of the diagonal construction to establish incompleteness (in conversation in the forties, not in his writings) was reported in footnote 4 on p. 281 of [Fund. Math. **37** (1950)]. To test his (rough) idea, one also considers a rule, ρ_p, which says, so to speak, the opposite: put at the mth place what the mth rule tells you to put there. One would expect to be able to write anything at the pth place. But this is not altogether adequate as seen by considering a problem of Henkin's [J. Symbolic Logic **17** (1952), 160]. The upshot is that the general character of the inferences by means of which ρ_p 'tells' you what to do, is critical: for so-called cut-free systems we have one answer, for the usual systems another; for a precise exposition, cf. 1.7, 2.7 and pp. 45–46 of [Dissertationes Math. Rozprawy Mat. **118** (1974)].

[8] For perspective on formalization: First of all, there is the empirical fact that the compactness theorem (which has turned out to be very useful, cf. [3]) was first stated by Gödel as a consequence of his completeness proof for Frege's rules. (A separate step is needed to *recognize* just where that theorem is effective; cf. Vaught's article cited in [3].) In short, one has by-products of formalization, of the kind familiar from [5] *in fine*. But the detour via formal rules is not necessary, and not used in modern (model-theoretic) proofs of the compactness theorem. Not unexpectedly, problems about formal rules are of *permanent* interest, not when mere existence is in question, but detail, for example, in a choice among complete sets of rules (and its effect on the geometric structure of the corresponding formal derivations of given theorems). By the nature of the case the choice of **relevant** *detail* requires more than the kind of general, superficial impressions on which the notions and problems of t.f. are based, for example, notions of (formal) rigor. The most obvious nondoctrinaire need for formalization comes from the application of computers to proofs: trivially, computers operate only on formal data (here: formalized proofs). And computers are certainly needed when measures of proofs are relevant which are hard to calculate by hand, for example, the *genus*, mentioned at the end of [4].

Remarks. (i) The use of computers for operating on, or, as one says, for unwinding 'given' proofs is of course less glamorous than the better-known business of automatic theorem proving (which many of us do better than computers), where one starts with a formula, a conjecture, not with a proof. (ii) The passage from a 'given' proof, say, in a mathematical text, to a formalization should be compared to other processing of 'raw' data for theoretical treatment; for example, to apply physical theory, (physically) significant data are needed, including the correction for artifacts. Correspondingly, the drill or ritual involved in mathematical texts is a likely source of artifacts (like a stylized description of a physical situation by someone not familiar with the relevant theory). (iii) W's obviously offhand comment mentioned on p. 83, to the effect that the passage in (ii) is 'easy', overlooks not only the general problems mentioned, but even the distinction between (absolute) effort and the ratio: effort/reward, familiar from economics (utility and marginal utility). As far as effort is concerned, W may be right, simply because one's subjective judgment tends to be bad! Specifically, at least this reviewer's estimates of the number of lines in a formalization tend to be unreliable even *after* it has been carried out. On one occasion writing down formalizations of 1 and 2 pages felt like 10 resp. 50 pages.

[9] Here are two examples of work attributed to W's advice about 'uses'. Both are extreme, the first in banality, the second in literalmindedness. (i) When proving results about a wrongheaded project, one may stumble over something of interest, as in work on Hilbert's consistency program or, more specifically, the elimination of nonelementary methods in number theory discussed in [4]. Then one tries to formulate that interest. This is familiar enough from the study of false hypotheses in the sciences, less so in the mainstream of pure mathematics which tends to be very conservative (confining itself to obviously relevant or well-tested notions). (ii) Probably the most literalminded interpretation of W's advice, and hence very much more 'philosophical' in the sense of t.f., is elaborated in so-called *operational semantics*, for example, of logical particles. Here the meaning of a word is determined by its 'use'; in mathematics, (tacitly) by formal rules for the use of the word. (The matter was very much in the air in the thirties; most logicians are familiar with it not from W's advice, but from a passing remark by Gentzen, on p. 80 of his collected papers.) Lorenzen has given two versions of operational semantics, one in his book *Operative Logik*, the other in his *Dialogspiele*, terminology which goes well with W's 'language games'. *Dialogspiele* are 2-person games associated with (many of) the usual logical systems in which the players choose formulas alternately: a formula F is said to be valid, if the proponent of F has a winning strategy; for a detailed exposition, see Kuno Lorenz [Arch. Math. Logik Grundlagenforsch **11** (1968), 32–55, 73–100]. Evidently, as the name suggests, a rather special side of reasoning, scoring debating points, is emphasized here (where, incidentally, the rules of the games are heavily biassed in favor the proponent–as if intended for people who like to talk a lot). Nothing is said about (a) the original choice of the usual logical systems, nor (b) the fact that even after formal rules have been formulated we continue to reason logically without remembering them; (a) has its parallel in the case of many definitions in mathematics, but not (b). In short, operational semantics goes against (empirical facts about) Bourbaki's intuitive resonances.

Remark. There is a quite separate question whether the work in (i) or (ii) was not only 'attributed' to, but whether, realistically speaking, it was influenced by W's advice. (W was clearly interested in the question, specifically in the *heuristic* and *pedagogic* value of what he had to say–as in his talk about 'disaster' in [6], or about 'tenacious misunderstanding difficult to get rid of' on top of p. 15.) Dramatics aside, these matters are sociological and hence severely statistical, difficult to judge not only because of the hackneyed business about interpreting statistical data, but because of the various skills needed to compile and process them. Abstractly, W was very sympathetic to an 'impersonal', statistical view of sociological matters. But in practice, he did not even try to see whether his particular views could be examined with existing resources; instead he expressed feelings, like ordinary mortals; cf. strong language about 'paradoxes' at the beginning of Bishop's review in this Bulletin (March 1977) when recording the failures of some starry-eyed projects for mass education (or unsuccessful investors complaining about the 'paradoxes' of the market).

[10] This note contains background material assumed in the last section of this review (pp. 84-85). (i) Concerning the (natural) philosophy of the Greeks, its most obvious actual or potential value has been to make posterity familiar with imaginative, so-called revolutionary ideas in a *general* way, for example, the idea of a *few* elements or of *cyclic* time (Aristotle's *Physics*, Book 8, 265a, 15 or 265b 10 on 'primary' time and motion). But, as so often with–obviously–premature enterprises, most attempts at more precise or explicit formulations were hopelessly off the mark; for example, Aristotle was unhappy with Anaximander's 'elements': earth, water, air, fire, but didn't even connect them with: solids, liquids, gases, energy as in a course on physics; instead he had the business of: dry, wet, cold, hot (things). (ii) Concerning lack of discretion in the use of t.f., Cantor speculated about higher infinite cardinals bringing us closer to the Almighty, with inconsistent manifolds keeping us at a respectful distance. Perhaps it should be added that Brouwer's pious doubts, in the skeptical tradition, of our ordinary conceptions are, realistically, hardly any less dubious than Cantor's beliefs; doubts which led to Brouwer's equally pious trust in what he thought he saw in (his) deepest consciousness. Incidentally, in the reviewer's view, Gödel's own proposals for the use of higher cardinals are, at present, *under*valued for an apparently quite accidental reason. He happened to propose them for settling (number-theoretic problems and, above all) the generalized continuum hypothesis, which, demonstrably, is not decided by anything remotely like the cardinals presently considered. (Earlier he had proposed to use the continuum hypothesis itself to settle number-theoretic problems: his own work could

be used to refute this proposal; as in [4].) But on the borderline of game theory, in the subject of so-called Borel games, the use of uncountably many iterations of the power set operation has turned out to be demonstrably essential for solving problems about **R** (at least, if 'subsystems' of set theory are to be used, as in Martin's proof of the determinateness of all Borel games [Ann, of Math. (2) **102** (1975), 363–371]). Of course, this is a far cry from number theory and from those 'extravagantly' large cardinals which Gödel had in mind. (iii) To supplement the text where (a) spectacular uses of the skeptical, and (b) modest uses of the speculative tradition are given: (a′) in physics, the atomic theory is the standard example of a success fitting into the speculative tradition (atoms being hardly much more plausible than ghosts, from ordinary experience); in mathematics, nonconstructive methods. (b′) Modest uses abound of course; cf., for example, my review of Brouwer's work in **83** (1977) of this Bulletin (around p. 88).

Remark. It cannot have escaped the reader's notice that there is no counterpart in current foundations to what is surely the most glaring difference between modern natural science and the early speculations referred to in (i): the skillful use of a massive amount of empirical data. Certainly the history of mathematics–not, of course, mere snippets as in (i)–(iii) above–would seem to provide, at present, the most obvious source of empirical data for the general questions behind t.f., and, in particular, for a scientific study of Bourbaki's 'intuitive resonances' (in [1]). Of course, precautions are needed against overliteral interpretations of the data (cf. end of [2] about misplaced textual criticism) and artifacts (cf. *Remark* (ii) in [8]); as in all sciences, only more so because here the influence of the observer on the observation is particularly strong. The use of statistical data, as in [9], over long periods provides *one* way of taking precautions. It may well be that this historical perspective would be bad for mathematical practice (with busybodies drawing premature 'practical' conclusions from ill-digested data). But in the reviewer's opinion it is certainly good for foundational research, specifically, for *opening up this subject to* (genuine) *problems raised by recent computer-assisted proofs*: (a) Historically–and scientifically, if not artistically–speaking, such proofs, for example, of the 4-color conjecture, involve incomparably more progress than, say, the use of large cardinals in (ii) above. Compare the effort which would be needed to explain large cardinals to Archimedes with getting him to understand, let alone put together the largish computer used by Haken and Appel (and compare the general interest of the four color conjecture with that of Borel determinacy). (b) There are genuine doubts about the reliability of computer-aided proofs not resolved by the particular idealizations of reliability, that is, the doctrines of rigor in various branches of t.f. Inasmuch as reliability is a principal topic of foundations, these new proofs present novel data for foundations: it would seem premature (to put it mildly) to *assume* that these new data are less fundamental than the matters of 'principle' stressed in t.f.

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The theory of numbers, S. Iyanaga, ed., (translated by K. Iyanaga), North-Holland, Amsterdam; American Elsevier, New York, 1975, xi + 541 pp., \$51.95.

The Legendre symbol $(\frac{a}{p})$ is defined for any odd prime p and any rational integer a that is not divisible by p. It is equal to $+1$ or to -1 according as the congruence $x^2 \equiv a \bmod p$ does or does not have a solution in the ring of rational integers **Z**. The quadratic law of reciprocity then states that the equations

$$\left(\frac{p}{q}\right)\cdot\left(\frac{q}{p}\right) = (-1)^{(p-1)/2\cdot(q-1)/2}$$

and

$$\left(\frac{-1}{p}\right) = (-1)^{(p-1)/2}, \qquad \left(\frac{2}{p}\right) = (-1)^{(p^2-1)/8},$$