main topics: *Local Observables and Constructive Field Theory.* Much thought has been given to the formulation of axioms in terms of local observables (e.g. a lattice of algebras of bounded operators) whose physical interpretation includes all possible experimental measurements which could be carried out in a space (or space-time) region. This section covers much early work of Haag, Kastler and Araki, but covers only a small part of the interesting results on superselection rules established by Haag, Doplicher and Roberts. The second aspect of Part 6 is an introduction to the existence problem for fields satisfying the axioms and to the analysis of detailed properties of solutions to model equations. This "constructive field theory" has been another major focus in the study of quantum fields over the past ten years. The authors devote the final section of their book to a brief but comprehensive survey of this work up to 1971, when their manuscript was completed.

All in all, the book provides a readable introduction to a large area of mathematical physics. In trying to include many things, the authors are occasionally incomplete or sloppy in minor ways. However, the book complements well the older books on axiomatic field theory by Jost and by Streater and Wightman, and a recent review by Streater in *Reports of progress in physics.* A mathematician interested in physics must be willing to learn some of the language and definitions of the physicist. This book is a good place to begin.

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Proof theory, by Gaisi Takeuti, Studies in Logic and the Foundations of Mathematics, vol. 81, North-Holland/American Elsevier, Amsterdam, Oxford, New York, 1975, vii + 372 pp., $35.50.

Takeuti places himself squarely in the line of development of Hilbert and Gentzen, which we begin by retracing. Proof theory was conceived by Hilbert as the means to carry out his grand program to secure the foundations of mathematics by purely mathematical means which were to be of the most elementary and evident kind. The logical structure of mathematical practice had been successfully mirrored within a variety of formal systems S for algebra, geometry, number theory, analysis, and set theory, all logically based in the predicate calculus (the logic of propositional connectives and quantifiers). The consistency of each such S is a combinatorial proposition which may be shown to be equivalent to a number-theoretical statement of the form (for all natural numbers n) $f(n) = g(n)$, where f, g are effectively computable. Hilbert expected to be able to derive such statements using only quantifier-free logic, recursive definitions of functions and proof by induction. Each derivation of a specific numerical statement by these means has a finite model. This differs from the situation where quantifiers are essentially involved in the reasoning; thus even where the variables of S range over natural numbers one may say that application of the reasoning of the predicate calculus in S implicitly involves infinitistic concepts.

To elaborate a bit: Hilbert spoke of the quantifier-free numerical statements as the "real" ones, and of statements involving noncombinatorial concepts or quantifiers as the "ideal" ones. He likened the use of the latter to the use of imaginary quantities or ideal divisors; the introduction of those had been justified by "consistency" proofs which showed how they could be eliminated in favor of combinations of familiar objects. Similarly, Hilbert aimed to eliminate the use of infinitistic concepts and logic in derivations of finitistically meaningful statements. For this purpose, formal consistency of S would be sufficient as long as S contains a bare minimum of arithmetic; the reason is that in this case for each particular n_0 with $f(n_0) \neq g(n_0)$, S proves $f(n_0) \neq g(n_0)$; hence if S is consistent and $f(n) = g(n)$ is provable in S, then for each $n_0, f(n_0) = g(n_0)$ must be true. The proof of consistency itself would have to be carried out entirely by finitary mathematical reasoning if the program were not to be circular.

In addition to these general aims, Hilbert devised a specific syntactic scheme, called the *ε-calculus*, to carry through his program. In that scheme quantifiers are eliminated in favor of *ε-terms*; at first sight it appeared reasonable to eliminate ε-terms entirely from derivations of end-statements not containing them. Preliminary successes with the ε-calculus were made in the 20's for the predicate calculus and for a fragment of Peano's axioms for elementary number theory by Ackermann and von Neumann, cf. [**H, B**]. Also Herbrand found another very original and appealing but complicated reduction of the theorems of the predicate calculus to the propositional calculus (cf. [**H**] or [**vH**, pp. 525–581]). However, the system *PA* of Peano's arithmetic itself turned out to provide an unexpectedly difficult obstacle, for reasons soon revealed by Gödel's incompleteness theorems: no consistent extension S of *PA* could prove its own consistency. Thus finitist means as conceived up to that point could not possibly work for *PA*; nor was there any real evidence that finitist methods in their full extent could somehow reach beyond *PA* let alone beyond systems of set theory.

This was a great shock for the Hilbert program, but not a complete reversal. The pieces were picked up by Gentzen who transformed the subject in a series of original and penetrating papers from 1934–1943 (now conveniently translated and collected in [**G**]). This was accomplished in three ways: (1) by introducing new kinds of logical systems to facilitate the studies, namely the so-called *sequential calculi*; (2) by isolating for these as chief result the so-called *cut-elimination theorem*, guaranteeing that every derivation could be replaced by a direct one; and (3) by introducing elementary forms of *transfinite induction* as a principal means to go beyond number theory while staying within a quantifier-free formalism.

Hilbert's ideas had developed during a period felt as that of a crisis in foundations, particularly following the appearance of the paradoxes in set theory. At the same time Brouwer challenged the whole platonist-realist conception of mathematics, rejected most of it as meaningless or unjustified and proceeded to rebuild mathematics on the basis of intuitionistic (constructive) tenets. Hilbert had hoped to "save" *all* of classical mathematics by his program and at the same time aimed to use only the most concrete intuitionistic methods. However, once the dam was broken, it was no longer clear which constructive methods were to be admitted in extensions of the program.

Takeuti's general point of view in this subject is perhaps best revealed by the following quotation from his book (p. 96).

"Comparison of our standpoint with some other standpoints may help one to understand our standpoint better. First, consider set theory. Our standpoint does not assume the absolute world as set theory does, which we can think of as being based on the notion of an "infinite mind". It is obvious that, on the contrary, it tries to avoid the absolute world of an "infinite mind" as much as possible. It is true that in the study of number theory, which does not involve the notion of sets, the absolute world of numbers 0, 1, 2, ... is not such a complicated notion; to an infinite mind it would be quite clear and transparent. Nevertheless, our minds being finite, it is, after all, an imaginary world to us, no matter how clear and transparent it may appear. Therefore we need reassurance of such a world in one way or another.

Next, consider intuitionism. Although our standpoint and that of intuitionism have much in common, the difference may be expressed as follows.

Our standpoint avoids abstract notions as much as possible, except those which are eventually reduced to concrete operations or Gedankenexperimente on concretely given sequences. Of course we also have to deal with operations on operations, etc. However, such operations, too, can be thought of as Gedankenexperimente on (concrete) operations.

By contrast, intuitionism emphatically deals with abstract notions. This is seen by the fact that its basic notion of "construction" (or "proof") is absolutely abstract, and this abstract nature also seems necessary for its impredicative concept of "implication". It is not the aim of intuitionism to reduce these abstract notions to concrete notions as we do.

We believe that our standpoint is a natural extension of Hilbert's finitist standpoint, similar to that introduced by Gentzen, and so we call it the Hilbert-Gentzen finitist standpoint.

Now a Gentzen-style consistency proof is carried out as follows:

(1) Construct a suitable standard ordering, in the strictly finitist standpoint.

(2) Convince oneself, in the Hilbert-Gentzen standpoint, that it is indeed a well-ordering.

(3) Otherwise use only strictly finitist means in the consistency proof."

An opposing point of view is taken by Kreisel in a long series of critical examinations of the subject (cf. **[K1]** and **[K2]** particularly). He starts simply by asserting that there is not the shadow of a doubt as to the *consistency* of Zermelo-Fraenkel set theory let alone analysis or number theory. He argues that the paradoxes had nothing to do with the axiom of infinity, but stemmed rather from a confusion between sets and properties. Zermelo separated these and offered a perfectly clear explanation of what his axioms were supposed to be about, namely the cumulative hierarchy of sets (the result of iterating the cumulative power set operation $a \mapsto a \cup \mathcal{P}(a)$). Finally, and more generally, proofs which use abstract principles are not only more intelligible but, as a matter of fact, more reliable. From this point of view, the professed aim of proof theory to secure the foundations of mathematics is an ideological hangover which is no longer tenable. The conclusion drawn from all this is not completely negative; Kreisel does not doubt that there is an interest in past and current proof-theoretical work, but has constantly been searching for further concepts and results (one might even say "mini-programs") to bring that out. While he has not made much headway with the more traditional proof-theorists (such as Schütte and Takeuti), I suspect most logicians (and mathematicians generally) will be sympathetic to this view and will be puzzled by some of the contortions of the present book. It goes against the grain to prove results by specially restricted methods when they are recognized already to be true, unless one sees sharpenings of the conclusions or interesting side-products. I shall try to suggest a point of view intermediate between Takeuti's and Kreisel's at the conclusion of this review.

The book is divided into three parts. Part I reviews Gentzen's approach to the first-order predicate calculus and *PA*. Part II is anomalous; it deals with second-order, finite order, and infinitary systems by nonconstructive methods, since no constructive means of the kind sought by Takeuti have been found to work in these cases. Part III presents a detailed constructive consistency proof of a certain subsystem of second-order arithmetic ("analysis").

The Gentzen sequential calculus *LK* differs from the familiar kind where one generates formulas A from initial formulas (axioms) by rules of inference of the form: from $A_1, \ldots, A_n$ infer A. Instead, one generates *consequence relations between formulas*, indicated by $A_1, \ldots, A_n \rightarrow A$ or simply $\Gamma \rightarrow A$ where $\Gamma = A_1, \ldots, A_n$; this is read: A is a consequence of Γ. Now the axioms are certain special $\Gamma \rightarrow A$ and the rules of inference take the form: from (hypotheses) $\Gamma_1 \rightarrow A_1, \ldots, \Gamma_n \rightarrow A_n$ infer (the conclusion) $\Gamma \rightarrow A$. Initially, Gentzen set up a system *NK* of *natural deduction* which closely follows ordinary reasoning. There the paradigm case is for implication $\supset$, with the two rules:

($\supset$-*introduction*) from $\Gamma, A \rightarrow B$ infer $\Gamma \rightarrow (A \supset B)$, and

($\supset$-*elimination*) from $\Gamma \rightarrow A$ and $\Gamma \rightarrow (A \supset B)$ infer $\Gamma \rightarrow B$.

Gentzen found this awkward to deal with for his proof-theoretical work and shifted to *LK*, in which one derives more generally (and more symmetrically) *sequents* $\Gamma \rightarrow \Delta$ where $\Gamma = A_1, \ldots, A_n$ and $\Delta = B_1, \ldots, B_m$; $\Gamma \rightarrow \Delta$ is interpreted as: the disjunction of $B_1, \ldots, B_m$ is a consequence of the conjunction of $A_1, \ldots, A_n$. Now the rules for $\supset$ take the form:

(*left* $\supset$) from $\Gamma \rightarrow \Delta, A$ and $B, \Gamma \rightarrow \Delta$ infer $(A \supset B), \Gamma \rightarrow \Delta$.

(*right* $\supset$) from $A, \Gamma \rightarrow \Delta, B$ infer $\Gamma \rightarrow \Delta, (A \supset B)$.

Each connective and quantifier has, similarly, characteristic left and right rules. The axioms (in the predicate calculus) are all those of the form $A \rightarrow A$. In addition, one has structural rules (permutation, contraction, etc.), and the rule

$$(cut) \text{ from } \Gamma \to \Delta, A \text{ and } A, \Gamma' \to \Delta' \text{ infer } \Gamma, \Gamma' \to \Delta, \Delta'.$$

Except for this, each rule has the *subformula property*: every formula in any hypothesis of the rule is a subformula of some formula of the conclusion. Thus any *cut-free* derivation has a *direct* character. On the other hand, the cut-rule is needed to establish the equivalence of LK with usual (complete) calculi for the predicate calculus in an elementary way. Gentzen's principal result for LK was the *cut-elimination theorem*, which shows how to replace each derivation $\mathfrak{D}$ of a sequent $\Gamma \to \Delta$ by a cut-free derivation $\mathfrak{D}^*$ of the same sequent. The passage $\mathfrak{D} \mapsto \mathfrak{D}^*$ is effective and proceeds by induction on a certain measure of complexity of $\mathfrak{D}$ (compounded from the locations of each cut-rule applied in $\mathfrak{D}$, together with the complexity of the "cut-formula" at that point).

After going over this, Takeuti draws some familiar model-theoretic consequences of the completeness of LK without the cut-rule, such as interpolation and definability theorems. However, these only require the existence for each valid sequent $\Gamma \to \Delta$ of *some* cut-free derivation of $\Gamma \to \Delta$. It is shown that the completeness of cut-free LK can be established directly without combinatorial difficulties. (This is said to be by the method of Schütte, though Beth, Smullyan, and others should also be credited.)

Another application given later (pp. 124–126) of the cut-elimination theorem for LK is the nonfinite axiomatizability of PA. This was first proved by Ryll-Nardzewski by a special method; Kreisel and Wang then found proof-theoretical arguments which permitted Montague to extend nonfinitizability to a variety of theories. (No credits are given by the author in connection with any of this work.)

Though Gentzen used the same basic logical apparatus as LK for number theory, the situation was now different since one needed a new rule:

$$(Induction) \text{ from } A(x), \Gamma \to \Delta, A(x+1) \text{ infer } A(0), \Gamma \to \Delta, A(t).$$

When t is a numeral this can be replaced by a finite sequence of cuts, using $A(0), \Gamma \to \Delta, A(1)$; $A(1), \Gamma \to \Delta, A(2)$; etc. However, in general, induction must be applied to get conclusions with variable t. Cut-elimination does not hold in this system. Gentzen only obtained a weaker result, but which is sufficient to establish the consistency of PA, namely: there is no derivation of the empty sequent $\to$. In any such derivation we can replace free variables by numerals and make reductions of induction at least in the "end-piece" of the derivation. Again a measure of complexity is involved, though no longer finite; it is assigned in the set of ordinals less than ε_0 (the limit of $\omega, \omega^\omega, \omega^{\omega^\omega}, \ldots$). Using Cantor normal form, a computable ordering of the natural numbers is naturally set up whose order type is ε_0. Then the principle of transfinite induction up to ε_0 applied to any particular number-theoretic property is equivalent to a statement in the language of number theory. Gentzen showed that with each derivation of $\to$ of ordinal α may be associated a new derivation of $\to$ with an ordinal $\beta < \alpha$. This is all done in an elementary effective way, so the only nonfinitist principle used to show consistency of PA is that of transfinite induction up to ε_0.

It may be noted that transfinite induction is already present in a disguised form in the classical method of descent in number theory. One way to show that there do not exist solutions of $f(m, n) = 0$, is by showing

$$\forall n\{\exists m f(n,m) = 0 \supset \exists n_1 < n \exists m f(n,m) = 0\},$$

or

$$\forall n\{\forall n_1 < n \forall m f(n_1, m) \neq 0 \supset \forall m f(n,m) \neq 0\}.$$

Assigning ordinal $\omega \cdot n + m$ to the pair (n, m), *quantifier-free induction up to* ω^2 allows one to conclude $f(n, m) \neq 0$ for all n, m.

Gentzen showed that for each initial segment $<_\alpha$ of the natural recursive ordering of type ε_0 the scheme of transfinite induction on $<_\alpha$ is provable in *PA*. Repeating this, Takeuti goes to some pains (pp. 87–95) to show how by concrete operations, "Gedankenexperimenten" on such, etc. one establishes that "whenever a concrete method of constructing decreasing sequences of ordinals is given, any such decreasing sequence is finite". The reviewer finds it a bit deceptive that his argument is suggested as somehow more concrete or finitist than full number theory itself, since the central notion $E(f, \alpha, n)$ of an operation f being an (α, n)-eliminator is defined by induction on n as follows:

$$E(f, \beta, n+1) \text{ iff } \forall g \forall \alpha[E(g, \alpha, n) \supset E(f(g), \alpha \cdot \omega^\beta, n)].$$

Hence we are using the whole number-theoretic apparatus with quantifier logic, differing only from ordinary arithmetic in that the logic is intuitionistic. But the reduction of classical to intuitionistic number theory is quite simple by Gödel's "negative" translation (p. 21). Here is the first place where a doubting reader will find it difficult to explain what is accomplished by all this and in what respect our "reassurance" has been increased.

By examining the arguments in more detail, some consequences beyond mere consistency are drawn, such as Kreisel's (uncredited) characterization of the provably recursive functions of *PA* by number-theoretic forms of recursion up to α for each $\alpha < \varepsilon_0$.

The first half of Part II is devoted to a proof of the cut-elimination theorem for simple-type theory. This was conjectured by Takeuti some 25 years ago, and he attempted proof-theoretical attacks on it with only partial success (the greatest being represented in Part III). A positive solution by semantical methods was finally obtained by Tait for second-order logic (in 1966) and independently by Prawitz and Takahashi for full finite order logic (in 1967). These solutions were based on the semantical reformulation of the problem by Schütte (though not mentioned in this connection) as one of extending certain partial to total valuations. The significant improvements by Girard and thence Martin-Löf and Prawitz (cf. [SL], 1971) are mentioned, but only as producing "a variant and somewhat more elegant form of cut-elimination". It should have been explained that an essential improvement of these over the semantical results consists in showing that certain natural reduction procedures always lead from given derivations $\mathfrak{D}$ of $\Gamma \to \Delta$ to cut-free derivations $\mathfrak{D}^*$ of

$\Gamma \to \Delta$. On the other hand, the semantical proofs merely give the existence of some $\mathfrak{D}^*$ from the existence of $\mathfrak{D}$. The semantical proof by Prawitz and Takahashi is gone over in the book; it would have helped to have more informal explanation of its workings.

The main obstacle to an extension of Gentzen-style arguments to type theory is that there is no useful means of complexity by which a formula $\exists\varphi \cdot A(\varphi)$ has greater complexity than $A(V)$, where V is an abstract, say $V = \{x^\tau | B(x^\tau)\}$. The trouble is that B may have greater complexity than A. As pointed out by Kreisel, this is also the principal obstacle to the usefulness of the cut-elimination theorem for second- (or higher-) order logic: the subformula property is not met in any useful sense by the rule:

$$(right\ \exists) \text{ from } \Gamma \to \Delta, A(V) \text{ infer } \Gamma \to \Delta, \exists\varphi A(\varphi)$$

(nor, similarly, by the left $\forall$ rule). On the other hand, it can still be shown that consistency of higher order arithmetic (analysis) is a consequence of this cut-elimination theorem.

Next the book moves on to infinitary systems (using infinitely long conjunctions, disjunctions, and quantifier sequences). One can establish cut-elimination theorems directly analogous to the result for *LK*. There is only the vaguest hope expressed that this may help with the official Hilbert-Gentzen program adopted by the author. Rather, work on these is of interest in connection with model theory and set theory. The latter enters the discussion of the logic with so-called heterogeneous quantifiers, e.g., $\forall x_0 \exists x_1 \forall x_2 \exists x_3 \cdots A(x_0, x_1, x_2, x_3, \ldots)$ (the "game" quantifier). For the obvious semantics of this in terms of Skolem functions we do not have the negation of the preceding equivalent to

$$\exists x_0 \forall x_1 \exists x_2 \forall x_3 \cdots \neg A(x_0, x_1, x_2, x_3, \ldots)$$

unless a form of the axiom of determinateness *AD* holds. The matter is delicate, if not of dubious value, since *AD* conflicts with the axiom of choice.

Part III is devoted to a proof of the consistency of a subsystem of second-order arithmetic based on the Π^1_1 (one set quantifier) comprehension principle, and related systems. For this purpose, special kinds of recursive orderings are set up called *ordinal diagrams*. In contrast to the ordering $<_{\varepsilon_0}$ which corresponds naturally to the generation of ordinals under ε_0 by $+$, $\cdot$ and $\exp_\omega$ (and also considerable extensions of such), these orderings seem to be suggested only by their use in proof-theoretical reduction procedures. It is shown by intuitionistic arguments involving iterated generalized inductive definitions that these orderings are well founded. However, the author says that the proofs presented are "not very constructive", and promises improvement in a future publication. The proofs of consistency using ordinal diagrams are themselves quite complicated. It would be completely unpersuasive to claim that these give us greater reassurance as to this part of formalized analysis. Some applications of the proof are given in the conclusion, among them bounds for the provable recursive well-orderings and provable recursive functions of these systems as classified by the orderings of ordinal diagrams;

it is not claimed that these bounds are best possible.

Reference to the literature is scanty. Schütte's book [S] (a new revised and English translation of which is forthcoming) is mentioned particularly, but there is intentionally little overlap even though [S] also works heavily with sequential style calculi. The main difference is that Schütte systematically employs infinitary systems as a tool to treat finitary systems. Ordinals have an *intrinsic* role for these, as lengths of derivations. Thus, for example, PA is embedded in a system $LK^{(\omega)}$ with the infinitary ω-rule; with each derivation $\mathfrak{D}$ of PA is associated a derivation $\mathfrak{D}^+$ of $LK^{(\omega)}$. The cut-elimination theorem holds in full for $LK^{(\omega)}$ just as for LK. It turns out that for the succession of transformations $\mathfrak{D} \mapsto \mathfrak{D}^+ \mapsto (\mathfrak{D}^+)^*$ with $\mathfrak{D}$ in PA and $(\mathfrak{D}^+)^*$ cut-free, the final derivation has length $< \varepsilon_0$. Moreover, these derivations can be handled finitistically, by effective descriptions of their form. There is no loss in this approach for any consistency results and related applications such as given in this book while there is a real gain in transparence and understanding. (The approach is briefly indicated on pp. 123–124 and in §30, but with no discussion of the advantages.)

Before concluding there are a few further small criticisms I would like to make:

pp. 30–31. The cut-elimination theorem is used to prove the consistency of LK. It should be mentioned that its consistency may be trivially established with the one-element model.

p. 38. The nonderivability in intuitionistic logic of some classically valid statements is left to the reader with no indication of proof; these would seem to need some sort of realizability interpretation, which is nowhere mentioned in the book.

p. 75. Gödel's theorem on the definability of all primitive recursive functions in terms of $+$ and $\cdot$ is stated without proof or reference to the literature, as if it were routine–which it is not.

p. 142. The following proposition is curiously asserted: "If the cut-elimination theorem holds for G^1LC, then G^1LC is consistent." The reason given for putting it this way is that "the proof of cut-elimination for G^1LC is nonconstructive, and hence, on the basis of our finitist standpoint, we cannot claim the consistency of G^1LC from that proof." But (cf. the remark above for p. 30), the consistency of simple type theory is immediate using a one-element model, as Gentzen realized long ago [**G**, #5]. Of course, the situation is quite different if one adds an axiom of infinity in some form or other.

pp. 292–293. The discussion of programs of "quasi" foundations is vague and uninformative; there are no references to the literature. Roughly speaking, Takeuti is arguing (rightly) against semiconstructive developments of *analogues* of classical analysis, such as recursive or hyperarithmetical analysis. However, no mention is made of systems with weak comprehension schemes which have recursion-theoretic or hyperarithmetic interpretations and in which classical analysis is actually *generalized*. Particular (predicative) systems of this kind were discussed by the reviewer in papers going back to 1964; for a more up-to-date approach, see [**F2**].

In general, throughout the book credit for individual results and reference to the literature is casual and haphazard.

Returning finally to the general question of aims, and against which accomplishments are to be measured, one may say that Takeuti's viewpoint is not convincingly supported by his work. The opposite approach of Kreisel [**K1**, Part II] and [**K2**] is persuasive, at least to the following extent: proofs themselves should be the central objects of interest; proof theory is developing precise theories related to informal ideas about proofs and which illuminate our daily experience with them. Secondarily, proof theory gives us means to interrelate formal systems (e.g., by conservative extension results) and to characterize the power of formal systems (provable recursive well-orderings, functions, functionals, etc.) which cannot be obtained by other metamathematical methods. Finally, the theory should be mentioned as of value in connection with mechanical proof-procedures.

What is less persuasive in Kreisel's view is the *ex cathedra* manner of acceptance of current systems of analysis and set theory. Not that the mental picture of the cumulative hierarchy is not convincing enough to warrant believing the consistency of set theory–indeed this picture provides quite an infallible guide when working within ZF; rather, it is that we really do not know what set-theoretical statements *mean* in the same way that we know what arithmetical statements mean. For, we have a complete mental picture of the totality N of natural numbers as generated from 0 by the successor operation, but no such picture of the supposed totality $\mathcal{P}(N)$ of subsets of N. Thus the meaning of a statement A containing quantifiers $\forall x \in \mathcal{P}(N)(\cdots)$ does not have the same definite character in our understanding as that of an arithmetical statement. Since the basic principle asserted for $\mathcal{P}(N)$ is the existence, for any given property $A(n)$, of a set $X \in \mathcal{P}(N)$ such that $\forall n[n \in X \equiv A(n)]$ (i.e., of $\{n \in N | A(n)\}$) we have here the typical *impredicative* situation where an object (X) is supposed to be defined by reference (via second-order quantification in $A(n)$) to a totality ($\mathcal{P}(N)$) of which it is a member. No interpretation of $\mathcal{P}(N)$ as the set of all subsets of N definable in previous terms can satisfy this principle. (Gödel's interpretation in the constructible sets requires for its explanation the impredicative concept of arbitrary countable well-ordering or ordinal.)

In view of the intuitive difference for us of number theory and set theory, it is thus natural to try to isolate that part of mathematics whose meaning is reducible to that of the natural numbers (so-called predicative mathematics) and to measure its practical extent. Recent results of Friedman and of the reviewer are of interest in this connection. Friedman found a fragment S of set theory in which all of Bishop's constructive analysis [**B**] can be formalized, and yet such that S is a conservative extension of (intuitionistic) PA; a similar result was then found by me for a fragment of one of my systems of 'explicit mathematics'. The practical significance comes from the fact that [**B**] provides constructive versions of all of classical analysis and a good deal of modern analysis; moreover, as Bishop remarks [**B**, p. 9], each of his theorems implies the corresponding classical result when one adds decidability of numerical quantification. (This part of mathematics is thus completely predicative.) Even the Π_1^1-comprehension axiom goes far beyond what is needed for foundations of actual analysis, though it does enter with descriptive set theory (Borelian mathematics). Practically speaking then, this part of mathematics rests not only on a secure but computationally and concretely meaningful basis. Without denying the possibility (by Gödel's theorem) that set-theoretically abstract principles may really be needed to make advances on concrete open problems, there has yet to be found a mathematically interesting example of such. This explains perhaps the source of confidence in daily mathematics, and at any rate the irrelevance of most of proof theory to any qualms about that. On the other hand, it is the logicians' task *qua* logician to investigate broad underlying concepts and points of view about mathematics such as finitist, intuitionist, predicativist, and platonist-realist, and to formulate these in precise terms and in as great generality as possible. One then tries to illuminate our understanding of these by myriad models, interpretations, and interreductions.

Despite my many criticisms of this book I believe it should be on the shelf of any serious student of proof-theory, alongside the classic source papers in [vH] and [G], the classic text [H, B], and the more up-to-date [S]. In addition, one should mention Prawitz [P] (cf. also [SL, pp. 235–307]) to see Gentzen's systems of natural deduction put to work, Troelstra [T] for the metamathematics of intuitionistic systems and for the very important functional interpretation of Gödel, and [F1] for infinitary systems and model-theoretical applications. The present book has much that is not to be found in these other references, and does show how far one can push Gentzen's direct approach. In particular, the work on Π_1^1-comprehension reveals Takeuti as a master of reductive syntactic proof-theory, and provides a challenge yet to be met by other and hopefully more transparent methods. Unfortunately, the student will not be able to go far in this subject before plunging into the current literature, which is both extensive and difficult. One may start with [K1], [K2], and [F2] as guides and proceed to the collections [IPT], [SL], and [PTS] for much representative work. Despite confusions and conflicts of aims, or perhaps because of them, there is still much to be learned and done.

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