CONSTRUCTIVE VALIDITY

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This paper is a preliminary report on work in progress and is an expanded and revised version of the lecture given at the conference. The author is indebted to N.G. de BRUIJN, N. GOODMAN, G. KREISEL and A.S. TROELSTRA for many kinds of help, information and advice as well as stimulation. In particular KREISEL has been very patient over the years in repeating time after time points not taken in and in offering extended criticism of faulty attempts at understanding what he calls "non-set-theoretic" foundations. The author is also indebted to D. LACOMBE for bringing a formal decidability problem to his attention, and to G. KREISEL for discussions on the significance of this problem. (See postscript).

BACKGROUND

A quote from HEYTING (8) will set the stage as well as could be desired:

One of BROUWER'S main theses was that mathematics is not based on logic, but that logic is based on mathematics. This is easily seen to be an immediate consequence of his point of departure. If mathematics consists of mental constructions, then every mathematical theorem is the expression of a result of a successful construction. The proof of the theorem consists in this construction itself, and the steps of the proof are the same as the steps of the mathematical construction. These are intuitively clear mental acts, and not applications of logical laws. Yet an intuitionistic logic has been developed, and thus the question of its significance was raised. The older interpretations by KOLMOGOROFF (as a calculus of problems) and HEYTING (as a calculus of intended constructions) were substantially equivalent. In a later paper HEYTING interprets logical theorems as simply mathematical theorems of extreme generality. There is no essential difference between logical and mathematical theorems, because both sorts of theorems affirm that we have succeeded in performing constructions satisfying certain conditions.

BROUWER based his considerations on a complex philosophical standpoint and a thorough psychologistic view of the nature of mathematics. Our purpose here will be to reexamine the idea of the calculus of constructions. A formalization of this calculus will be presented, and it will be applied to the problem of interpreting logical formulas in a way that, to the author at least, seems to carry out the program outlined by HEYTING above word for word. When this is done it would appear that the psychologism has been reduced to a minimum: one only has to agree that the theory of constructions has intuitive appeal. And one particular advantage of the theory we shall examine is that it has many interpretations of varying degrees of constructivity. Now there will of course remain the questions of whether BROUWER would have considered the theory at all reasonable and of whether some essential part of his idea of mathematics has been lost. But the author feels that until the intuitionists arrive at a greater degree of clarity in formulating their principles, the conclusion must stand that the notion presented here is indistinguishable from the intended meaning on the basis of current practice, of intuitionistic mathematics.(This statement is incorrect; see postscript.)

These remarks do not apply directly to BROUWER'S theory of choice sequences, but the present state of the art (cf.(19) and the objections of MYHILL (17)) indicates that choice sequences are eliminable. Thus, however pleasant they may be in theory (and natural in intuition), one cannot claim for them at the moment any more fundamental rôle in analysis than, say, that of the infinitesimals of (classical) non-standard analysis. For both kinds of analysis these various remarkable reals have properties that aid our understanding through the regularity of their laws, but strictly speaking they are not needed. This situation may very well change in the light of future developments; hence the cautious reader may be reluctant to call the author's theory intuitionism.

The calculus we shall develop here did not occur as a bolt out of the blue but has a long history involving many people. In the first place we have HEYTING'S original work. The author's own contact with the problem came through KREISEL'S formulations in (10) and (11). Subsequently interest was revived in consulting with GOODMAN on the thesis (5) (cf. also (6), and more on this later. In the meantime we had the work by LÄUCHLI (14) and LAWVERE (15) who both provided interpretations that are closely related. Their approach has one serious defect from our point of view: neither of them formalized their theories of functions (constructions) and both of them think rather non-constructively. (I hope they will forgive me for this remark.) Therefore the foundational (as distinct from mathematical) content of their interpretations is not evident. Hopefully the present theory will make it possible to view their results in a new light. LÄUCHLI was motivated by KLEENE'S realizability interpretation (cf.(9)) and considered his notion as an abstract generalization thereof. The exact relation of the present interpretation to realizability is not clear yet. KLEENE'S particular use of recursive functions introduces anomalies (sometimes formally useful!) that make comparison difficult. GOODMAN discusses this in (5), but we shall not be able to do so here.

Getting back to KREISEL, he wanted to formalize the "intended" interpretation in such a way that proofs (in an abstract sense) were objects of the theory of the same status as constructions. This is reasonable from the psychologistic approach which accepts mental acts as objects of mathematical investigation. There were some difficulties in bringing KREISEL'S theory to a precise enough state to allow metamathematical results, and this problem was the point of departure for GOODMAN. He reformulated KREISEL'S theory and obtained several results, but his version was not exactly what KREISEL had wanted. KREISEL felt that in view of decidability of various features of proofs, the functions should be total functions. GOODMAN did not find this requirement convenient because operations on constructions were to be given by general combinators (in the sense of Curry-Church), and these necessarily lead to partial functions. GOODMAN gave a quite neat treatment of a calculus of partial functions, and aside from this divergence carried out KREISEL'S plan in satisfactory detail. It will be noted, however, that neither KREISEL nor GOODMAN gave an analysis of the structure of abstract proofs, and they enter in a (to the author) mysterious way simply to allow certain properties to be decidable.

This was how matters stood at the time the author came to Amsterdam in the fall of 1968. Soon thereafter he met Professor de BRUIJN, who explained to him his language AUTOMATH (cf. the paper of de BRUIJN at this conference). The feature of his language what was of special interest to de BRUIJN was the possibility of writing a computer program for practical proof checking but that will not concern us for the moment. What was highly suggestive to the author was de BRUIJN'S conceptual framework. He had been, of course, personally influenced by BROUWER and wanted to present a suitably constructive notion of proof. He achieved this, not surprisingly, by means of constructions for interpreting the logical notions. He distinguishes between constructions (functions) and categories (certain sets or species) of constructions and places the burden of proof on showing that a given compound construction belongs to the desired category. The particular conventions of language for writing such proofs, which are essential for computer work, need not be discussed here.

As the reader can well imagine, at this point the author made the connection with KREISEL, GOODMAN, LÄUCHLI, and LAWVERE, and he set out to formulate a system of his own. Instead of the natural deduction style of de BRUIJN, it seemed more succinct to use the calculus of sequents employed by GOODMAN for foundational considerations. (This also seems better than the two-valued propositional connectives of KREISEL, since one is only interested in certain implications in any case.) Next the distinction between constructions and species used by de BRUIJN seemed very convenient, though as we shall see this does not require notational distinctions. When one does this one finds that partial functions can be avoided by having each function defined on a "principal" domain and then making function values arbitrary outside this domain (a plan of KREISEL). Next de BRUIJN made good use of cartesian products of species (formation of function spaces) in connection with the universal quantifier - an idea also familiar to LAWVERE and to a certain extent to LÄUCHLI - and the author took this at once. Now dual to products (as LAWVERE knows) are disjoint sums which must be used for the interpretation of the existential quantifier (cf. KREISEL - GOODMAN). These sums were not employed by de BRUIJN, but it would be easy to add them to his system.

Now that we have functions and species and sums and products, we take certain primitive species (a one-and two-element species, and the species of natural numbers) and their implied functions (ordered pairs and definitions by recursion) and combine and recombine them as much as we please obtaining the basis species of constructive mathematics (cf. the discussion in TROELSTRA (20)). These are finally used for the interpretation of logic.

Several points should be noted:

(1) We never have occasion to form species of species. Why? Well since we can form functions of functions of ... of functions of species,

the species of species do not seem to be needed. If we can think of some use for them, the format of the theory will allow for them.

(ii) General species variables (quantification over species) are not allowed, though the effect can be produced by some simple primitives. This is not a defect, because there may be arguments against quantification over arbitrary species.

(iii) We have no abstract proofs only constructions and species of constructions. When the author finally obtained his formalism the proofs-as-objects vanished. May be they should be brought back in, but for the present the author's system seems to be simpler than KREISEL - GODDMAN'S (and to a certain extent, de BRUIJN'S) and to be adequate. Thus it seems more reasonable to try it out first in some detail; only then will one be able to appreciate whether abstract proofs are desirable. (But see postsript.)

(iv) The general combinators are not used. This has the advantage that models are conceptually easier to obtain (total vs. partial functions, as mentioned earlier). Furthermore, one is forced to make explicit all the basic modes of formation, and they are remarkably few.

Let us digress for a moment to discuss the category-theoretic approach of LAWVERE. In category theory we axiomatize a calculus of functions under composition. We do not, however, have (what seems to the author) a convenient axiomatization of which infinitary operations (such as direct product) actually exist. Usually we consider a category as a class and talk about (arbitrary) indexed families of objects. Thus the existence of these families is thrown back to set theory.

If there were an axiomatization of the "category" of "all" categories, this would not be necessary, but in the author's opinion this all-inclusive theory does not yet exist. Even if it did, it would most likely not be a constructive theory. If one likes, one can view the author's theory as an attempt at axiomatizing in a constructive way a theory of both functions and families of sets of functions. Whether this approach could have any effect on category theory is a matter of speculation.

At this point, mention should also be made of TAIT'S paper [18]. He called his work "Constructive Reasoning" and seemed to make a conscious effort <u>not</u> to define <u>validity</u>. He does of course discuss the GÖDEL interpretation, but that is not the same thing. Also he uses in an essential way <u>definitional equality</u> which we have <u>not</u> found necessary, though it is a notion favored by KREISEL. Furthermore, TAIT'S use of combinators leads to a theory of species that does not seem as elementary as the one presented here. The author does, however, agree with TAIT on the introduction of species of <u>trees</u> used to index quite general <u>iterations</u> and will discuss this in detail below.

In summary, then, based on the motivations and contacts just explained, we are going to propose a theory of constructions and species and to show how it applies in making precise the meanings of the logical notions. This theory involves the primitive ideas of sums, products and iterations applied to the finite species to generate the basic species which provide the background for constructive mathematical thought.

<u>LANGUAGE</u>

We shall distinguish as usual between <u>terms</u> and <u>formulas</u>; however, only the terms will be compounded not the formulas. Thus as formulas we have:

$$\sigma \in \tau \quad \text{and} \quad \sigma = \tau$$

where σ and τ are terms. The first means that the (construction) σ <u>belongs to</u> the (species) τ ; while the second is an <u>equation</u> (between constructions or species). There seems to be no need at all to have a two-sorted theory (indeed later it would actually be inconvenient), so we have just <u>one</u> type-free sort of variable (usually, lower-case Roman letters with the Greek letters reserved for metatheory) ranging over both constructions and species, variables are terms.

Among the terms we mention next the <u>constants</u>, namely:

$$\mathbf{0}, \dot{0}, \mathbf{1}, \bar{0}, \bar{1}, \mathbf{2},$$

of these $\mathbf{0}$, $\mathbf{1}$, and $\mathbf{2}$ are thought of as species and the others as (atomic) constructions. (One may guess the membership relations now, but they are made explicit later by axioms.)

Further we have some simple compound terms, namely:

$$\sigma(\tau),\ \sigma_0, \sigma_1, [\sigma \wedge \tau],\ O(\sigma), \sigma^+, \mathbb{T}(\sigma),$$

where σ and τ are previously obtained terms. Explanations of meanings now deserve special sections below.

Finally we have the complex compound terms which involve bound variables:

$$\forall x \varepsilon \alpha[\sigma],\ \exists x \varepsilon \alpha[\sigma],\ \mathbf{P} x \varepsilon \alpha[\sigma], \quad \text{and}\ \mathbf{R} v[\alpha, \beta, \sigma].$$

Here α, β, σ are terms and in place of x and v we may have any other variables. One should not worry now about the use of ε: it could be just another punctuation mark. The reason for the placement of brackets is that our's is more a postfix rather than a prefix notation. This could be modified, but it makes formulas even less beautiful. The reason for writing α, β is that our convention is such that the variable (x or v) is bound only in the σ not in the α or the β. One defines free and bound occurrences of variables in the usual way as well as the notion of rewriting bound variables.

We shall often have to indicate the substitution of a term σ for all free occurences of a variable (x, say) with the implied rewriting of variables free in σ if they occur bound in the context (τ, say) into which we substitute. We use the notation

$$[\sigma / x][\tau]$$

(sometimes without the second pair of brackets) and remark that this is a notation of the metalanguage not the object language.

INFERENCE

Connections between formulas are asserted by sequents

$$\Delta \vdash \delta$$

where Δ is a (finite) sequence of formulas and δ is a single formula. The meaning is clear: the conjunction of the formulas in Δ implies the formula δ. We provide no brackets because this implication is never iterated.

A stock of these assertions is provided later by the axioms; while the theorems are derived from them by these well-known rules:

(I1) $\dfrac{\Delta \vdash \delta}{\Delta, \varepsilon \vdash \delta}$ (Weakening)

(I2) $\dfrac{\Delta, \varepsilon, \varepsilon', \Delta \vdash \delta}{\Delta, \varepsilon', \varepsilon, \Delta \vdash \delta}$ (Interchange)

(I3) $\dfrac{\Delta, \varepsilon \vdash \delta \qquad \Delta \vdash \varepsilon}{\Delta \vdash \delta}$ (Cut)

(I4) $\dfrac{\Delta \vdash \delta}{\Delta \vdash \delta'}$ (Rewrite)

(I5) $\dfrac{\Delta \vdash \delta}{[\sigma/x]\Delta \vdash [\sigma/x]\delta}$ (Substitution)

where in the rewrite rule, δ' results from δ by rewriting a bound variable. (It may be possible that (I4) follows from substitution if we made the substitution conventions really precise, but never mind.)

The author does not know whether it is true - or even interesting to suppose that there is a "cut-free" formulation of the theory. This question might be related to some decision problems mentioned below.

EQUALITY - Two axioms are required:

(E1) $\vdash x = x$

(E2) $x = y, \delta \vdash [y/x]\delta$

these hardly need explanation.

We note these obvious consequences:

$$\delta \vdash \delta$$
$$x = y,\ x = z \vdash y = z$$
$$x = y \vdash y = x\ .$$

It is possible that equality could be eliminated from the system, but it does not seem pleasant to do so.

FUNCTIONS

We mean by $f(x)$ the ordinary function value f of x. In as much as functions can take functions as values, we can write $f(x)(y)$ for functions of two arguments, and similarly for more arguments. All our functions are total, so that $f(x)$ always means something even if x

is outside the principal domain of f. In that case, we would, if we so desired, let $f(x) = f$, but we shall formulate no axiom to that effect leaving the matter open.

For the most part a theory of functions is quite uninteresting unless there is some method for introducing new functions by (explicit) definition. We provide such definitions through functional abstraction. Thus if σ is a term (with the variable x free in σ, say) and if a is a given species, then we can think of the function f defined on a with value σ for $x\varepsilon a$. Our notation for this function is:

$$f = \forall x\varepsilon a[\sigma] .$$

Most people will consider the author slightly mad to use the universal quantifier for functional abstraction. Nevertheless, there is method in his madness as will be clear in the next section. In the meantime, the reader may rub out the $\forall$ and replace if by λ, if that makes him happier. The idea of functional abstraction is formalized in the axiom of conversion:

(F1) $$f = \forall x\varepsilon a[\sigma],\ x\varepsilon a \vdash f(x) = \sigma.$$

Note the variations of the axiom that can be obtained by substitution.

To the non-constructive mind it would seem reasonable to adjoin at this point the rule of extensionality;

$$\frac{\Delta,\ x\varepsilon a \vdash \sigma = \tau}{\Delta \vdash \forall x\varepsilon a[\sigma] = \forall x\varepsilon a[\tau]}$$

where x is not free in Δ. Exactly why this is unreasonable the author cannot argue convincingly at the moment. However, to leave open possible "intensional" interpretations of the axioms (the same functions may be given by different GÖDEL numbers, say), it seems better to avoid it. In any case it was not required for the interpretation of logic.

We make one apparently harmless concession to extensionality though:

(F2) $$f = \forall x\varepsilon a[\sigma] \vdash f = \forall x\varepsilon a[f(x)] .$$

This may not really be needed, but the equation on the right is a way of saying that the principal domain of f is a. (We also considered having an operator $Df = a$ for computing domains, but dropped the idea as unnecessary.)

So much for single functions, we must now turn to the consideration of species of functions.

PRODUCTS

Familiar from set theory, topology, and algebra is the cartesian (direct) product. Familiar too is its fundamental rôle, and so it will be here. Given species $\sigma(x)$ indexed by $x \varepsilon a$, we consider all functions f defined on a such that $f(x) \varepsilon \sigma(x)$ for all $x \varepsilon a$. These form a (basic) species, whose existence we wish to postulate. First being influenced by ordinary mathematics, we might call it:

$$\mathsf{X} x \varepsilon a [\sigma(x)].$$

But let us stop to think a moment. We have distinguished between species and functions, because we must give the functions a special place. (In mathematics the idea of function really is more primitive than most other notions.) In particular there is absolutely no reason to identify a function with a set of ordered pairs as is usual in (pure) set theory. For one thing such an identification is not particularly constructive; for another, our species are rather more restricted that those allowed in set theory. So another plan may be considered.

Let us reason as follows: for the moment functions and species are separated. Maybe an identification can be reestablished that is even more convenient than the usual one. (An identification is a simplification - hopefully - that avoids proliferation of entities.) In our case the product $\mathsf{X} x \varepsilon a [\sigma(x)]$ is completely determined by the function $\forall x \varepsilon a [\sigma(x)]$. Conversely, assuming as we do that no species is known to be empty, then the product also determines the function (this point is not too essential). Hence, no one can stop us from making the identification:

$$\mathsf{X} x \varepsilon a [\sigma(x)] = \forall x \varepsilon a [\sigma(x)],$$

and we therefore drop the X notation. Of course, it remains to be seen whether the identification (which was partly suggested by Professor de BRUIJN'S style) is actually useful.

Now that we have the idea of products as functions, we can formulate the abvious axioms. In the first place we have an analogue to (F1):

(P1) $$f \varepsilon \forall x \varepsilon a[\sigma],\ x \varepsilon a \vdash f(x) \varepsilon \sigma \ .$$

Next we also take an analogue to (F2):

(P2) $$f \varepsilon \forall x \varepsilon a[\sigma] \vdash f = \forall x \varepsilon a[f(x)] \ .$$

Finally we must assume what would be an analogue to the rule of extensionality:

(P3) $$\frac{\Delta,\ x \varepsilon a \vdash \sigma \varepsilon \tau}{\Delta \vdash \forall x \varepsilon a[\sigma] \varepsilon \forall x \varepsilon a[\tau]} \ ,$$

where x is not free in Δ. Axioms (P1) and (P2) tell us that the elements of a product have the proper character; while (P3) expresses the fact that any function of the proper kind must belong. In contradistinction to extensionality, this rule is harmless: even though there may be several "copies" of the same function (given by different definitions, say), we can obviously demand that all the copies belong to the product. Note that (P3) is very much like the rule of universal generalization, and this apt analogy will be exploited later.

Once we can form products, they can be specialized to what are usually called powers. For reasons that will eventually become apparent, we use this definition (which may be considered as a new axiom by prefixing the $\vdash$):

DEFINITION

$$[a \to b] = \forall x \varepsilon a[b] \ .$$

As a function $[a\to b]$ is the constant function on a, as a species the use of the notation $[a\to b]$ does not differ too much from the use of the usual category-theoretic notation $f : a \to b$, but we have to write $f \varepsilon [a\to b]$. We also find that $\to$ does indeed behave like (intuitionistic) implication, but before we discuss this in detail the reader might try this theorem as an exercise:

$$x \varepsilon a \vdash \forall f \varepsilon \forall x \varepsilon a[\sigma][f(x)] \varepsilon [\forall x \varepsilon a[\sigma] \to \sigma] \ .$$

The assertion results from (P1) by the rule (P3), and the part underlined should be considered as a whole. What is interesting is to the right of the principal ε, an expression reminding one of the logical axiom of universal instantiation.

Here is another simple exercise:

$$\vdash \forall x \varepsilon a [\forall y \varepsilon b [x]] \ \varepsilon \ [a \to [b \to a]] \ .$$

Is this not also suggestive? Would you care to fill in the blank in the theorem:

$$\vdash \underline{\qquad\qquad} \ \varepsilon \ [[a \to [b \to c]] \to [[a \to b] \to [a \to c]]] \ .$$

(The format is deceptive because considerably more space for writing the answer is required than is indicated !) Once you have the idea such examples may be multiplied at will. (This was clear to LÄUCHLI and LAWVERE, for instance.)

SUMS

Dual to products are (disjoint) sums. At this stage we cannot expect any new, clever notational inovations because our previous identifications have exhausted the raw material provided by the functions. So if $\sigma(x)$ are species for $x \in a$, the disjoint sum (union) of these species will be denoted by a new symbol:

$$\exists x \varepsilon a [\sigma(x)].$$

(Note that we can usually omit the "of x" by considering x a free variable of σ ; this is a more formal approach but a little harder to read.) In ordinary set theory the disjoint union is identified with:

$$\bigcup_{x \in a} (\{x\} \times \sigma(x)),$$

but in our theory the ordered pairs cannot be combined in such an arbitrary fashion. For one reason, we are trying to keep our species disjoint (basic species are very much like - a generalization of - the types in the theory of types), and so the same ordered pair cannot belong to distinct species. For another reason, the reduction of disjoint union to ordinary (cumulative) union is not constructive (information is lost in a cumulative union). These considerations thus lead to a related but independent analysis of the notion.

What we seem to have to do next is to provide a separate notion of ordered pair for each district sum. This is just a bit clumsy, but the author could not find a simpler device. Thus the pairing function appropriate to the sum $\exists x \varepsilon a[\sigma(x)]$ is called:

$$\mathbf{P}x \varepsilon a[\sigma(x)] \ ,$$

and we can state the first two axioms governing this pairing function:

(S1) $f = \mathrm{P}x\varepsilon a[\sigma],\ x\varepsilon a,\ y\varepsilon\sigma \vdash f(x)(y)\varepsilon \exists x\varepsilon a[\sigma]$

(S2) $f = \mathrm{P}x\varepsilon a[\sigma] \vdash f = \forall x\varepsilon a[\forall y\varepsilon\sigma[f(x)(y)]]$.

Obviously, the import of these axioms is that the pairing function <u>is</u> a function of the correct type with values in the desired species. This does not yet characterize the values as ordered pairs, however.

It seemed necessary to provide a distinct pairing for distinct sums, because the coordinates of the pair do <u>not</u> determine the context of their occurence (at most the a and the <u>one</u> $\sigma(x)$ is determined by $x\varepsilon a$ and $y\varepsilon\sigma(x)$). On the other hand, we are quite free to assume that the resulting <u>pair</u> $(\mathrm{P}x\varepsilon a[\sigma](x)(y))$ does indeed completely determine not only the coordinates but the whole sum. Hence we can now simplify matters by introducing <u>universal inverse pairing operations</u> that require no special mention of context. The notation is given by the (bold-face) subscripts 0 and 1, and we have these straight-forward axioms:

(S3) $f = \mathbf{P}x\varepsilon a[\sigma], x\varepsilon a, y\varepsilon\sigma \vdash f(x)(y)_{\mathbf{0}} = x,$

(S4) $f = \mathbf{P}x\varepsilon a[\sigma], x\varepsilon a, y\varepsilon\sigma \vdash f(x)(y)_{\mathbf{1}} = y,$

(S5) $\mathbf{f} = \mathbf{P}x\varepsilon a[\sigma], z\varepsilon\exists x\varepsilon a[\sigma] \vdash f(z_{\mathbf{0}})(z_{\mathbf{1}}) = z,$

(S6) $z\varepsilon\exists x\varepsilon a[\sigma] \vdash z_{\mathbf{0}}\varepsilon a,$

(S7) $z\varepsilon\exists x\varepsilon a[\sigma], x = z_{\mathbf{0}} \vdash z_{\mathbf{1}}\varepsilon\sigma$.

It takes several statements, but all we have said here is that the elements of $\exists x \in a[\sigma]$ really do correspond to ordered pairs with well-behaved coordinates.

If the reader wishes a simple exercise, he may prove:

$$\vdash \underline{\mathrm{P}x\varepsilon a[\sigma]}\,\varepsilon\forall x\varepsilon a[[\sigma \to \exists x\varepsilon a[\sigma]]] \ .$$

GENERATORS

As of now our theory is empty, because we have not yet introduced any species, and so there are no domains on which to define functions. This gap we now fill by providing the finite species from which all the other basic species will be generated. In view of the products and sums, we will only need the first few of these species: $\mathbb{0}, \mathbb{1}$, and $\mathbb{2}$. These seem to be independent, and the author doubts that any further simplification is possible.

The species $\mathbb{0}$ is to be empty, but we shall not assume the axiom

$$x \in \mathbb{0} \vdash \sigma$$

at the present. The author cannot put his finger on the precise reason, but somehow this assumption is too strong (there is some connection here with extensionality). Instead we merely remain silent: no axiom or theorem will ever produce an element of $\mathbb{0}$. (This cautiousness is actually unnecessary.)

Hence, if we find one in a hypothetical proof, we know something is absurd or trivial. So much for $\mathbb{0}$.

The species $\mathbb{1}$ is to be the one-element species. It has a much more "positive" character than , and so its axioms are clear:

(G1) $\vdash \mathring{0} \in \mathbb{1}$

(G2) $x \in \mathbb{1} \vdash x = \mathring{0}$.

Thus $\mathring{0}$ is the only element of $\mathbb{1}$. (There is absolutely no real saving in mixing types by the identification $\mathbb{0} = \mathring{0}$, as we do in set theory.) Note that we can easily define functions on $\mathbb{1}$, since such a function has only one value (y, say) and can be defined as:

$$\forall x \in \mathbb{1}[y] = [\mathbb{1} \to y] \ .$$

It is quite possible that it is sensible to generalize (G2) to the following instance of extensionality.

(G2) $f = \forall x \in \mathbb{1}[f(x)] \vdash f = [\mathbb{1} \to f(0)]$.

This is generalization because (G2) can be derived by substituting

$\forall x \varepsilon \mathbf{1}[x]$ for f. (The reader may carry out this simple exercise.) Extensionality of function on <u>finite</u> species seems unexceptional. (The reader may also use (G2') to derive the rule:

$$\frac{\Delta,\ x\ \varepsilon\ \mathbf{1} \vdash f(x) = g(x)}{\Delta \vdash \forall x\ \varepsilon\ \mathbf{1}[f(x)] = \forall x\ \varepsilon\ \mathbf{1}[g(x)]}$$

where x is not free in Δ).

The species $\mathbf{2}$ is to be the <u>two-element</u> species. Since it must be kept (potentially) disjoint with $\mathbf{1}$, it has its own elements $\bar{0}$ and $\bar{1}$. Thus:

(G3) $$\vdash \bar{0}\ \varepsilon\ \mathbf{2},$$

(G4) $$\vdash \bar{1}\ \varepsilon\ \mathbf{2}.$$

To say that these are the only elements, we must resort to a rule:

(G5) $$\frac{x = \bar{0},\ \Delta \vdash \delta \qquad x = \bar{1},\ \Delta \vdash \delta}{x\ \varepsilon\ \mathbf{2},\ \Delta \vdash \delta}\ .$$

So much is clear; what is not yet clear is how to obtain functions on $\mathbf{2}$. <u>Constant</u> or <u>identity</u> functions are already at hand, but the function that <u>interchanges</u> $\bar{0}$ and $\bar{1}$ is not. In general we must obtain with the aid of a <u>new</u> primitive operation the arbitrarily defined function on $\mathbf{2}$. Suppose its values are to be a and b corresponding to $\bar{0}$ and $\bar{1}$, then we call this function $[a \wedge b]$ and assume:

(G6) $$\vdash [a \wedge b](\bar{0}) = a,$$

(G7) $$\vdash [a \wedge b](\bar{1}) = b,$$

(G8) $$\vdash [a \wedge b] = \forall x\ \varepsilon\ \mathbf{2}[[a \wedge b](x)],$$

(G9) $$f = \forall x\ \varepsilon\ \mathbf{2}[f(x)] \vdash f = [f(\bar{0}) \wedge f(\bar{1})]\ .$$

This last is extensionality for the species $\mathbf{2}$.

Here is a useful exercise: prove the following:

$$[x \wedge y]\ \varepsilon\ [a \wedge b] \vdash x \in a,$$
$$[x \wedge y]\ \varepsilon\ [a \wedge b] \vdash y\ \varepsilon\ b,$$
$$x \in a,\ y \in b \vdash [x \wedge y]\ \varepsilon\ [a \wedge b]\ .$$

Thus the reader can see that not only does $[a \wedge b]$ play the rôle of an ordered pair, but it is also the same as the <u>finite cartesian</u> product we usually call $[a \wedge b]$. (But the ordered pairs are necessarily distinct from the pairs we needed for sums; sad but true.) If he likes, the reader may also fill in the blanks in these theorems:

$$\begin{aligned}
&\vdash \underline{\hspace{6cm}} \ \varepsilon\ [[a \wedge b] \to a],\\
&\vdash \underline{\hspace{6cm}} \ \varepsilon\ [[a \wedge b] \to b],\\
&\vdash \underline{\hspace{6cm}} \ \varepsilon\ [a \to [b \to [a \wedge b]]].
\end{aligned}$$

Suggestive?

Now that we have finite products we should also try to obtain finite sums. Fortunately, these can naturally be identified with combinations already available. By analogy with (G8) we have this definition instead of a new axiom:

DEFINITION

$$[a \vee b] = \exists x \ \varepsilon\ \mathbf{2}[[a \wedge b](x)].$$

We find here a new quality of the expression $[a \wedge b](x)$. In fact, this is what is usually called in computer science now the <u>conditional expression</u>. If $x = \bar{0}$, the value is a; otherwise if $x = 1$, the value is b. Ready for your exercises? Fill in the blanks, please:

$$\begin{aligned}
&\vdash \underline{\hspace{5cm}} \ \varepsilon\ [a \to [a \vee b]],\\
&\vdash \underline{\hspace{5cm}} \ \varepsilon\ [b \to [a \vee b]],\\
&\vdash \underline{\hspace{5cm}} \ \varepsilon\ [[a \to c] \to [[b \to c] \to [[a \vee b] \to c]]].
\end{aligned}$$

After so many of these exercise, surely the reader is getting the point and can begin to guess at a general statement.

It would be possible to identify $\mathbf{2}$ with $[\mathbf{1} \vee \mathbf{1}]$, but note we could not define $\vee$ without the aid of $\mathbf{2}$. Thus, this circularity does not seem to get us anywhere. However, if we needed the species, we could define

$$\mathbf{3} = [\mathbf{2} \vee \mathbf{1}],\ \mathbf{4} = [\mathbf{3} \vee \mathbf{1}],\ \text{etc.}$$

The reader should check (and it is not all that pleasant to do so) that <u>arbitrary</u> functions can indeed be defined on these finite species in terms of the constructs already available.

TRANSFINITE CONSTRUCTIONS

The mathematician has the advantage over the "ordinary" mortal (a finite mind) of grasping (some of the properties of) <u>infinite species</u>. Or at least that is his conceit, and the author has no desire to argue against that attitude. Neither does he want to waste the time to glorify this ability but rather wishes to make "visible" the underlying mechanism. What is about to be formulated is hardly original, but the new theory in which it occurs does seem to possess advantages over previous proposals.

The idea is simple. Suppose one is given a species a, then many functions can be defined on this species (assuming some elements are known !). In particular suppose we have a special object 0 (better: $0(a)$ to show its dependence on a) which will be an element of a yet-to-be specified species $\mathbb{T}$ (better: $\mathbb{T}(a)$). Now we can at one form a function from a into $\mathbb{T}$, namely the constant function $[a \to 0]$. Call this function u for the moment. Why not put $u \in \mathbb{T}$ and assume u and 0 are distinct? This way we can try to form even more functions in $[a \to \mathbb{T}]$, because some new values for functions are now available. And then we want to put those functions in $\mathbb{T}$ and to continue this process in an iterative fashion. There is only one defect with the plan: the functions u already belong to certain species $(u \in [a \to \mathbb{T}])$ and are not available for other jobs. The solution is easy: we send instead of u a proxy called u^+. To be precise, we assume these axioms:

(T1) $\qquad \vdash 0(a) \in \mathbb{T}(a),$

(T2) $\qquad u \in [a \to \mathbb{T}(a)] \vdash u^+ \in \mathbb{T}(a)\ .$

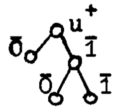
So far the axioms give positive results about certain elements belonging, but we need more: a principle of (transfinite) induction to assure that these are the only elements (cf.(G5) in the finite case). Here is a possible formulation:

(T3)

$$\frac{\Delta \vdash [0(a)/t][\sigma] \varepsilon [0(a)/t][\tau] \qquad \Delta,\ u\varepsilon[a\to\mathbb{T}(a)],\ \forall x\varepsilon a[[u(x)/t[\sigma]] \varepsilon\ \forall x\varepsilon a[[u(x)/t][\tau]] \vdash [u^+/t][\sigma]\varepsilon[u^+/t][\tau]}{\Delta,\ t\varepsilon\mathbb{T}(a) \vdash \sigma\varepsilon\tau}\ ,$$

where u,t are not free in Δ. We can also take = instead of ε, but this only seems useful for finite a. Nevertheless, let us assume it as (T3'). The lack of extensionality for functions on infinite a may make the use of these axioms somewhat less interesting.

Intuitively (following Tait [18]) the elements of $\mathbb{T}(a)$ can be considered as trees. Thus 0 is the null tree and $[a \to 0]^+$ is the tree of rank $\mathbf{1}$. There are (in general) many trees of rank $\mathbf{2}$. Suppose we take $a = 2$, then any diagram such as :



can be considered as determining a function. Here $u = [0 \wedge [0 \wedge 0]]$ and u^+ is our (abstract) tree of rank $\mathbf{2}$. An so on to the higher ranks.

The case $a = 2$ leads to the more beautiful diagrams, but the case $a = \mathbf{1}$ is also of interest (while $a = \mathbf{0}$ is not). Indeed $\mathbb{T}(\mathbf{1})$ can be considered to be the species of <u>integers</u>. Let us write $\mathbb{N} = \mathbb{T}(\mathbf{1})$ and, for the moment, 0 for $0(\mathbf{1})$ and n^+ for $[\mathbf{1} \to n]^+$. Then a moments thought shows us that (T1), (T2), (T3), and (T3') specialize to obvious <u>closure</u> and <u>induction</u> principles for the integers.

Just as with finite sets, we do not have enough functions on the new species unless we assume some additional axioms. In the case of an inductively defined species such as $\mathbb{T}(a)$, the proper method is to supply functions defined by <u>recursion</u>. That is the purpose of the operator $\mathcal{R}$:

(T4) $f = \mathcal{R}\, v\,[a,b,\sigma] \vdash f(0(a)) = b$
(T5) $f = \mathcal{R}\, v\,[a,b,\sigma],\ u \in [a \to \mathbb{T}(a)], v = \forall x \in a[f(u(x))] \vdash f(u^+) = \sigma,$
(T6) $f = \mathcal{R}\, v\,[a,b,\sigma] \vdash f = \forall t \in \mathbb{T}(a)[f(t)].$

This completes the list of fundamental axioms.

As an example of a definition by recursion, we define the important notion of the <u>nodes</u> of a tree. We will define an operation $nd(a) = \forall t \in \mathbb{T}(a)[nd(a)(t)]$, whose values are species. Obviously, what we want are these two theorems:

$$\vdash nd(a)(0(a)) = \mathbf{1}$$
$$u \in [a \to \mathbb{T}(a)] \vdash nd(a)(u^+) = [\mathbf{1} \vee \exists x \in a[nd(a)(u(x))]].$$

The way to obtain them is to define:

<u>DEFINITION</u>

$$nd(a) = \mathcal{R}\, v[a, \mathbf{1}, [\mathbf{1} \vee \exists x \in a[v(x)]]] \;.$$

Then the desired theorems will be derived from (T4) and (T5). We can then define such notions as a labeled tree: that is a tree $t \in \mathbb{T}(a)$ together with a function $l = \forall n \in nd(a)(t)[l(n)]$ and so on. (The tree we employ seem less general at first sight than those of TAIT [18], but the idea of a labeled tree is actually more general than TAIT'S notion.)

It may be that the operator $\mathcal{R}$ is defective, because the author can see no way of defining a function (call it pred, say) such that pred $(0) = 0$ and pred $(u^+) = u$. That is, we have not allowed our recursive functions at u^+ to depend not only on the preceding function values, but also on the preceding arguments - the operation pred is a simple example. In the case of $\mathfrak{a}$ finite this seems to be no problem, but for infinite a one sees no easy reduction of the more general kind of recursion to the simpler one. If this is so, we should probably replace $\mathcal{R}$ by $\mathcal{R}'$ with an appropriate axiom such as:

$$(T5') \qquad f = \mathcal{R}' u,v[a,b,\sigma],\ u \in [a \to \mathbb{T}(\mathbf{a})],\ v = \forall x \in a[f(u(x))] \vdash f(u^+) = \sigma .$$

That may look the same as (T5), but note that u is now a bound variable in $\mathcal{R}'u,v[a,b,\sigma]$: that means that u occurs in the same way on both sides of $f(u^+) = \sigma$ as desired.

In case $\mathfrak{a}$ is a finite species, then $\mathbb{T}(\mathfrak{a})$ is denumerable. On the other hand, if $\mathfrak{a}$ is infinite (say $\mathfrak{a} = \mathbb{N} = \mathbb{T}(\mathbb{1})$), then $\mathbb{T}(\mathbb{N})$ is non-denumerable (the ranks of these trees - classically anyway - would be ordinals of the second number class).

We could then go on to iterate $\mathbb{T}$ and form $\mathbb{T}(\mathbb{T}(\mathbb{N}))$, $\mathbb{T}(\mathbb{T}(\mathbb{T}(\mathbb{N})))$, and so on - even into the transfinite. these new species are ever larger and more complicated, but at the moment the author does not really know how to make good use of anything worse than $\mathbb{T}(\mathbb{N})$ - which seems to have been BROUWER'S limit - at least for ordinary analysis. But we agree with TAIT [18] that there appears to be nothing that will stop us at the second step. This, by the way, seems to answer BISHOP [2], who asks whether there is any structure (of a combinatorial rather than function-space nature) beyond the integers. Thus $\mathbb{T}(\mathbb{N})$ is just the index set one needs for the proper definition of BOREL sets, for example, which in [1] where defined by BISHOP only in an intuitive, non-formal way.

INTERPRETING LOGIC

We have already introduced the operators $\forall, \exists, \wedge, \vee, \rightarrow$, though the reader may not have yet appreciated just why we used symbols from logic in the way we have. All will now be explained satisfactorily, let us hope. But before we do, we need the notation for the remaining logical operators:

DEFINITIONS

$$\begin{aligned} \top &= \mathbb{1}, \\ \bot &= \mathbb{O}, \\ \neg[\alpha] &= [\alpha \rightarrow \bot], \\ [a \leftrightarrow b] &= [[a \rightarrow b] \wedge [b \rightarrow a]] \ . \end{aligned}$$

Let us begin with an example of a (valid) logical formula:

$$[\forall x \varepsilon a[[P(x) \rightarrow Q(x)]] \rightarrow [\neg[\forall x \varepsilon a[Q(x)]] \rightarrow \neg[\forall x \varepsilon a[P(x)]]]] \ .$$

Now as this stands (except maybe for the pedantic bracket conventions and the use of the capital Roman letters P and Q) it can be read <u>both</u> as a formula of ordinary predicate calculus (with variables restricted to a given <u>domain</u> a) <u>and</u> as a term of our theory of constructions. Call the formula $\mathscr{L}$. The reason that $\mathscr{L}$ is <u>intuitionistically</u> (constructively, if you prefer) <u>valid</u> is that there is a specific term τ (involving both of the variables P and Q as well as a) such that the assertion

$$\vdash \tau \varepsilon \mathscr{L}$$

is <u>provable</u> in the theory of constructions.(The blank can be <u>filled in</u>, as we did in several elementary exercises already.) It is just as Professor HEYTING said:
"The proof of the theorem consists in this construction itself, and the steps of the proof are the same as the steps of the mathematical construction".
Of course, we have aided the "intuitively clear mental acts" by our <u>formal rules</u> for operating with constructions. Thus we can ask a machine to check over our proof (as Professor DE BRUIJN wants to do).

We are a little hasty here : the exact term τ required above has not yet been exhibited. It is rather long to write down, and so we shall arrive at it indirectly. Instead of showing why $\mathscr{L}$ is valid (notation: $\models \mathscr{L}$), we shall rather establish the validity of <u>this</u> sequent of logical formulas:

$$\forall x \in a[[P(x) \rightarrow Q(x)]],\ \neg[\forall x \in a[Q(x)]],\ \forall x \in a[P(x)] \vdash \bot .$$

In general, to establish the validity of a sequent of logical formulas:

$$\mathcal{A}_0, \mathcal{A}_1, \ldots, \mathcal{A}_{n-1} \vdash \mathcal{B}$$

we read them as terms of the theory of constructions and provide <u>both</u> a term τ <u>and</u> a proof in our theory of the assertion:

$$\Delta,\ t_0 \in \mathcal{A}_0,\ t_1 \in \mathcal{A}_1,\ \ldots,\ t_{n-1} \in \mathcal{A}_{n-1} \models \tau \in \mathcal{L}$$

where $t_0,\ldots,t_{n-1}$ are fresh, distinct variables, Δ contains the proper information about the <u>free</u> variables - including the predicate variables - (and more on this later), and where τ may involve all the variables. This means that giving the constructions establishing the $\mathcal{A}_1$ we can always find (in a uniform way by means of τ) a construction which establishes $\mathcal{B}$.

Returning to the example, if we have $t_0 \in \forall x \in a[[P(x) \rightarrow Q(x)]]$, $t_1 \in \neg[\forall x \in a[Q(x)]]$ and $t_2 \in \forall x \in a[P(x)]$, then we can certainly find a term $\tau \in \bot$. Indeed, we can let:

$$\tau = t_1(\forall x \in a[t_0(x)(t_2(x))]),$$

and it is a mildly intersting exercise to verify that this **is** correct. (By the way the use of subscripts here is not to be confused with the subscripts $Z_{\mathbf{0}}$ and $Z_{\mathbf{1}}$ for <u>disjoint sums</u> as these subscripts **0 and 1** will be printed in bold-face type.)

As a second example consider the well-known law of contradiction

$$[P \rightarrow [\neg[P] \rightarrow Q]] \ .$$

We should try to find τ such that

$$t_0 \in P,\ t_1 \in \neg[P] \vdash \tau \in Q.$$

This <u>cannot</u> be done unless $\mathbf{0} = \bot$ is really assumed empty, which we are reluctant to do. So we side-step the **issue** by adjoining (as part of the Δ mentioned above) a side condition on the variable Q: namely

$q \varepsilon [\bot \to Q]$. Then $\tau = q(t_1(t_0))$ gives the answer. This should be done for all the propositional variables in general - and for the predicate variables too. Thus if we wanted to establish the validity of :

$$\forall y \varepsilon a [\forall x \varepsilon a [[P(x) \to [\neg [P(x)] \to Q(y)]]]],$$

we would prove :

$$q \varepsilon [\bot \to Q], \ y \varepsilon a, \ x \varepsilon a, \ t_0 \varepsilon P(x), \ t_1 \varepsilon \neg [P(x)] \vdash \tau \varepsilon Q(y),$$

where $\tau = q(t_1(t_0))(y)$ in this case. Correct? No! we should also have the hypothesis $Q = \forall x \varepsilon a [Q(x)]$ to be able to pass from $q(t_1(t_0)) \varepsilon Q$ to $\tau \varepsilon Q(y)$, but that is all quite reasonable.

We can think of Δ as the complete list of the declarations of the types of the variables. In case of predicate, we must indicate their domains and number of arguments, as well as providing for free such $q \varepsilon [\bot \to 0]$. We require no more formal statement for Δ for the present.

As a third example we establish the validity of a law of double negation :

$$[\forall x \varepsilon a [[\neg [\neg [P(x)]] \to P(x)]] \to [\neg [\neg [\forall x \varepsilon a [P(x)]]] \to \forall x \varepsilon a [P(x)]]].$$

Thus suppose that $t_0 \varepsilon \forall x \varepsilon a [[\neg [\neg [P(x)]] \to P(x)]]$ and that $t_1 \varepsilon \neg [\neg [\forall x \varepsilon a [P(x)]]]$. Then as the reader can easily verify - if he has the patience - the construction that belongs to $\forall x \varepsilon a [P(x)]$ is

$$\forall x \varepsilon a [t_0(x)(\forall u \varepsilon \neg [P(x)] [t_1(\forall v \varepsilon \forall x \varepsilon a [P(x)] [u(v(x))])])] \ .$$

(The main point of this particular example was to show how complicated and overloaded with brackets the terms can become. This is not to be regarded as a conceptual drawback, however.)

One major point has been left unexplained : our examples were compound formulas, but when we exhibited constructions we broke the implications up into simpler parts. The justification of this procedure lies in the proof of the deduction theorem : if

$$\mathcal{A}_0, \mathcal{A}_1, \ldots \models \mathcal{L}$$

is valid, then so is

$$\mathcal{A}_1, \ldots \models [\mathcal{A}_0 \to \mathcal{L}] \ .$$

(Let τ be the term establishing the validity of the first sequent then $\forall t_0 \varepsilon \mathcal{O}\!\mathcal{l}_0[\tau]$ - except for a quibble about subscripts - is the term we need to establish the validity of the second sequent.) It may safely be left to the reader to establish <u>modus ponens</u> :

$$\mathcal{O}\!\mathcal{l}_0,\ [\mathcal{O}\!\mathcal{l}_0 \to \mathcal{O}\!\mathcal{l}_1] \models \mathcal{O}\!\mathcal{l}_1 ,$$

and since the analogues of all the inference rules (I1)-(I5) for logic are also clearly valid, we have all we need for intuitionistic implication.

Furthermore, the reader, without realizing what he was about (or maybe he did!), has verified in the previous sections <u>all</u> the other axioms of intuitionistic propositional calculus. Thus we can take <u>that</u> as firmly established.

Finishing up the predicate calculus, we note that we have already done the <u>axioms of instantiation</u>. Therefore, it only remains to discuss the <u>rules of generalization</u> :

$$\frac{\mathcal{O}\!\mathcal{l}_0,\ \mathcal{O}\!\mathcal{l}_1,\ \ldots \models \mathcal{L}}{\mathcal{O}\!\mathcal{l}_0,\ \mathcal{O}\!\mathcal{l}_1,\ \ldots \models \forall x \varepsilon a[\mathcal{L}]}$$

where x is <u>not</u> free in the $\mathcal{O}\!\mathcal{l}_1$, and

$$\frac{\mathcal{O}\!\mathcal{l}_0,\ \mathcal{O}\!\mathcal{l}_1,\ \ldots \models \mathcal{L}}{\exists x \varepsilon a[\mathcal{O}\!\mathcal{l}_0],\ \mathcal{O}\!\mathcal{l}_1,\ \ldots \models \mathcal{L}}$$

where this time x is <u>not</u> free in $\mathcal{O}\!\mathcal{l}_1, \ldots, \mathcal{L}$. (Let τ be the term establishing the validity of the hypothesis of the first rule. Then $\forall x \varepsilon a[\tau]$ establishes the conclusion. Let σ , on the otherhand, establish the validity of the hypothesis of the second rule. Then $[(t_0)_{\mathbf{0}}/x][(t_0)_{\mathbf{1}}/t_0]\sigma$ is the required term for the conclusion. (Note the bold-face subscripts!))

Though our examples were restricted to monadic formulas, the procedure is quite general; and we can claim that we have given a "mathematical" basis (foundation) for the whole of intuitionistic predicate logic. (By the way, our pedantic notation for binary relationships is $P(x)(y)$, and similarly for more arguments.) It seems quite reasonable to suppose that the proof of LÄUCHLI [14] can be transposed to this theory, and that we can establish the faithfulness of the interpretion (formulas valid on our interpretation are indeed provable in HEYTING'S calculus). The author has not yet had time to work out the details, however, LÄUCHLI did not discuss higher-order logic (though LAWVERE did), and we wish soon to consider a particularly mathematically important theory: higher-order analysis - but without the free choice sequences. Before we do this, however, we must review the progress of our program.

REVIEW

We began by regarding species and constructions as mathematical objects and found that there were some simple axioms governing their properties. It then became slowly apparent that these properties were highly analogous to properties familiar from formal logic. We then turned this analogy into a dogma by insisting that the logical formulas be read (better : interpreted) as (mathematically meaningful) terms of the theory of constructions. This interpretation requires that validity be asserted by the act of giving an explicit construction belonging to the (interpretation of the) formula. Validity is established by giving a proof from the axioms for constructions of the membership assertion.

The next step is to argue that the interpretation is "correct", but so far all we have done is to check the validity of the expected formulas. Thus the situation must be examined in more detail. For one thing : have we verified BROUWER'S "main thesis" ? Which is prior : logic or mathematics? Well, the answer all depends on what one means by logic (what is mathematical seems much clearer). In order to organize the mathematical properties of the constructions into a coherent body of knowledge, we had to set up some rules of deduction (I1) - (I5) and some general axioms such as (E1) - (E2). This represents simply a codification of hypothetical argument (if such and such conditions are fulfilled, then another condition follows). If one can hazard a guess, these principles are so self-evident that BROUWER may never have ever given them a moments' thought. This is the realm of urlogic, without which (in the author's opinion) mathematics (and even coherent thought) is impossible. All of these principles are used naturally in an unconscious fashion. What BROUWER probably meant by "logic" was the elaborate RUSSELL-WHITEHEAD theory of propositional operators, quantifiers and propositional functions and the kind of logic that is meant when RUSSELL says that mathematics is reduced to logic. The author considers that today there is a general agreement that RUSSELL was wrong (or at least over-optimistic). The type-theoretic / set-theoretic foundation for mathematics is not pure logic, because axioms about abstract entities are required - and this falls on the mathematical side of the line between the subjects.

Now what the author feels he has demonstrated here is that - granted urlogic - the combinatorial aspects of (constructive) logic can be given a "mathematical" foundation by the theory of constructions. Of course, the axioms for constructions are not so different from the axioms for logic ((P3) is a rule of universal generalization). Nevertheless a certain reduction has been effected (implication and quantification out of the same operator $\forall$, for example) and a considerable amount of clarity has been gained : one can prove the various propositional formulas from the more elementary principles about constructions.

Professor CURRY hoped for a similar reduction based on his theory of combinators, but the author does not feel that illative combinatory logic (cf. [3] and [4]) has reached a high enough of development to judge it successful.

In particular the author wonders whether CURRY'S long struggle with the many headed monster of the partial function was a serious tactical error. (Note in this connection the remark in footnote 3, p.296 of [4]: "It seems best to proceed with these features (of partial functions) and introduce refinements later in the illative theory".)

One of the author's statements in next to last paragraph requires further discussion : why are properties of constructions "more elementary" than valid propositional formulas? From the point of view of classical two-valued logic this is simply not so. But one must keep in mind that we are investigating propositions here in a constructive way. Thus a proposition does not simply degenerate to one of two truth values but instead is represented by a complex species of possible constructions that conceivably can be used in its validation. From this constructive point of view propositional formulas are not so trivial.

Now what about the interpretations of the logical connectives : are they "correct"? Take implication first. Assuming for simplicity that no hypothesis of declarations are required, what must be done in order to establish $[\mathfrak{A} \to \mathfrak{B}]$? One must produce a construction together with a proof that this construction transforms every construction that could establish $\mathfrak{A}$ into a construction for $\mathfrak{B}$.

The construction is an object <u>of</u> the theory while the proof is an elementary argument <u>about</u> the theory. KREISEL [13] calls such proofs 'judgements' and asks for an abstract theory of them. We have not provided this because we did not see why such a theory was needed. The reader may decide : have we or have we not carried out the spirit of HEYTING'S interpretation of implication? Of course, this is not new : the KREISEL-GOODMAN theory can be justified by a similar discussion. The author only wants to claim that his theory is simple, and as yet tha t there is no demonstrated need for "abstract" proofs. (But see postscript.)

<u>Conjunction</u> :

To establish $[\mathfrak{A} \wedge \mathfrak{B}]$ one must produce a pair of constructions the first of which provably justifies $\mathfrak{A}$, and the second $\mathfrak{B}$.

<u>Disjunction</u> :

To establish $[\mathfrak{A} \wedge \mathfrak{B}]$ one must produce (another kind of) <u>pair</u> whose first coordinate is either $\bar{0}$ or $\bar{1}$: if $\bar{0}$, then the second coordinate justifies $\mathfrak{A}$; if $\bar{1}$, then $\mathfrak{B}$.

<u>Truth</u> :

The justification of $\top$ is known because $\hat{0} \varepsilon \mathbf{1}$.

<u>Absurdity</u> :

No justification of $\perp$ $(= \mathbf{0})$ is known.

<u>Universal quantification</u> :

To establish $\forall x \varepsilon a [\mathfrak{A}]$ one must produce a construction that maps every element of the domain into a justification of the corresponding instance of $\mathfrak{A}$.

<u>Existential quantification</u> :

To establish $\exists x \varepsilon a [\mathfrak{A}]$ one must produce a pair whose first coordinate is an element of the domain and whose second coordinate provably justifies the corresponding instance of $\mathfrak{A}$. This completes our review and our argument for "correctness" (cf. also discussion in MYHILL [16] and in TROELSTRA [20]).

One last topic before we turn to "real" mathematics. KREISEL has often stressed that the reason for having abstract proofs is to make the proof predicate <u>decidable</u>. Otherwise there is no <u>reduction</u> in logical complexity when one says that implication means that if you have a próof of the hypothesis, then you know a proof of the conclusion. In our theory we have replaced proof by construction and of by membership. But our theory of membership is a completely 'positive' theory, and we have no way of formulating an assertion to the effect that every object is either a member or non-member of a given species. Likewise, we have no superlarge functions to serve as ($\bar{0}$ - $\bar{1}$ - valued) characteristic functions of species. (TAIT and GOODMAN would allow such functions, but then the door is opened to the murky combinators. So the question is (and it is a quite serious question) : are these deficiencies a real defect of our theory and has the attempted formalization of the basis for intuitionistic logic aborted?

The question of decidability was asked by LACOMBE at the lecture. The author cannot at the moment give a definite answer to this question. The best he can do is to formulate a conjecture. You see, from the definition of validity of logical formulas every such assertion can be put in the form :

$$\vdash \sigma\varepsilon\tau \ .$$

(All the variables on the left-hand side of the $\vdash$ can be moved over by (P3) to the right-hand side. Likewise for the side conditions including $p\varepsilon[\perp\to P]$. There only remain the equations of the form $P = \forall x\varepsilon a[P(x)]$; but by (F2) these will disappear, if we substitute $\forall x\varepsilon a[P(x)]$ for P. Of course, the formula is no longer either beautiful or readable, but that is beside the point.) So then, we have the :

FUNDAMENTAL CONJECTURE

There is a (primitive recursive) decision method for the provability in the theory of constructions for assertions of the from $\vdash \sigma\varepsilon\tau$.

Even if the answer is yes to this question, it may not satisfy KREISEL. The decidability is external to the system rather than a condition having an internal formulation. The question may also be related to the "normal-form" problem that de BRUIJN has encountered in his system. It may be that the current proof theoretical work on GÖDEL'S theory $\mathbf{T}$ (by TAIT and HOWARD, among others) sheds light on the problem, because the theories are related. The only thing the author can definitely contribute to the discussion at this moment is that there can be no decision method for assertions of the hypothetical form :

$$\sigma_0 \varepsilon \tau_0, \ \sigma_1 \varepsilon \tau_1, \ \ldots, \ \sigma_{n-1} \varepsilon \tau_{n-1} \ \vdash \sigma_n \varepsilon \tau_n \ .$$

We will prove this result even for the fragment based on (F1) - (F2), (P1) - (P3). (This is a stronger not weaker result, because it seems reasonable to suppose that a theorem in the pure theory of functions and products that is proved with the aid of the other notions can be proved without them. But this has not been formally established.) Now if we allowed equations in the hypothesis, the undicidability is immediate : any calculus of conditional equations between arbitrary functions is undecidable in view of the word problem for semigroups. For example, we can easily prove in our system :

$$f \varepsilon [a \to a], \ g \varepsilon [a \to a], \ \forall x \varepsilon a [f(g(x))] = \forall x \varepsilon a [g(g(f(x)))], \ x \varepsilon a \ \vdash$$
$$f(f(g(x))) = f(g(g(g(f(f(x)))))) \ .$$

In other words, any deduction from generators and relations written as functional equations can be carried out for the "semigroup" of functions on a domain in our calculus. Now we have not dicussed models for the theory and shall not be able to do so in this paper, but with their aid we can see that, conversely, every equational result proved by pure semigroup methods. Hence, there can be no decision method for the calculus.

We note in passing that the equation in the conclusion - which was written between elements and not between functions in view of the lack of extensionality - could even have been eliminated in favor of membership statements. Thus (and this no doubt can be established with the aid of models) an assertion $\Delta \vdash \sigma = \tau$ is provable if and only if Δ, $h(\sigma) \varepsilon b \vdash h(\tau) \varepsilon b$ is provable, where h and b are new variables. Or even if this is not the case in full generality, enough is true to apply to "semigroup" equations; because for them we need only consider domains with a characteristic function for identity. This simple-minded approach does not, however, eliminate the equations in the hypothesis.

To express deductions with semigroup equations entirely with membership statements, we imagine a function e such that for $x, y \varepsilon a$ the value $e(x)(y)$ is $\top$ in case $x = y$, and is $\perp$ otherwise. We will require nothing special about $\top$ and $\perp$ and could just think of them as free variables - in fact, $\perp$ will not even appear but was just mentioned for definiteness. With this idea about e we recognize several correct statements about it (where we omit some tiresome brackets) :

(i) $\forall x \varepsilon a\ \forall t \varepsilon T[t] \varepsilon \forall x \varepsilon a[T \to e(x)(x)]$

(ii) $\forall x \varepsilon a\ \forall y \varepsilon a\ \forall z \varepsilon a\ \forall t \varepsilon T \forall u \varepsilon e(x)(y) \forall v \varepsilon e(x)(z)[t] \varepsilon \forall x \varepsilon a\ \forall y \varepsilon a\ \forall z \varepsilon a[T \to [e(x)(y) \to$
$[e(x)(z) \to e(y)(z)]]]$

(iii) $\forall f \varepsilon [a \to a] \forall x \varepsilon a\ \forall y \varepsilon a\ \forall t \varepsilon T\ \forall u \varepsilon e(x)(y)[t] \varepsilon \forall f \varepsilon [a \to a] \forall x \varepsilon a\ \forall y \varepsilon a[T \to | e(x)(y) \to$
$e(f(x))(f(y))]]$.

Statements (i) - (iii) express that e is very much like an <u>equality</u> relation on the domain - at least in some formal sense. Next we consider a typical (defining) relation between given (generating) functions f and g for our "semigroup" :

(iv) $\forall x \varepsilon a\ \forall t \varepsilon T[t] \varepsilon\ \forall x \varepsilon a[T \to e(f(g(x)))(g(g(f(x))))]$.

We can call (iv) the <u>translation</u> of the 'equation' fg = ggf. Now if we let Δ be the sequence (i), (ii), (iii), (iv), $f \varepsilon [a \to a]$, $g \varepsilon [a \to a]$, then it is fairly simple to see that $\Delta \vdash \delta$ can be proved, where δ is the translation of the equation ffg = ggggff. Furthermore, if δ is the translation of some other equation, then it is intuitively clear that $\Delta \vdash \delta$ is provable in our calculus <u>if and only if</u> there is a semigroup deduction of the equation from the given fg = ggf. Hence, the undecidability result follows with 95% certainty. The status of the conjecture remains open, however. (The undecidability result is not all that interesting, but it <u>is</u> a non-trivial exercise in the theory of constructions that gives some insight into the expressive power of the calculus.)

By the way, in the case where <u>recursion</u> is available in the theory it seems very likely that there is no decision method for assertions $\vdash \sigma \varepsilon \tau$ either. For suppose the term p represents (a standard definition of) a primitive recursive function. Surely there is no way to decide the provability of such assertions as

$$(*) \qquad n \varepsilon \mathbb{N} \vdash p(n) = 0 \ .$$

Now let ζ be introduced by recursion so that

$$\vdash \zeta(0) = 0 \text{ , and}$$

$$n \varepsilon \mathbb{N} \vdash \zeta(n^{+}) = \bar{0} \ .$$

It seems reasonable to suppose that

$$\vdash \forall n \varepsilon \mathbb{N}[\zeta(p(n))] \varepsilon [\mathbb{N} \to \mathbb{N}]$$

is provable if and only if (*) is provable. Hence, there could be no decision method. - This will require some more thought. Maybe there are some sensible restrictions to put on the theory, or maybe one is only interested in special cases of $\vdash_{\sigma\varepsilon\tau}$.

INTERPRETING ANALYSIS

Of course, by analysis we understand higher-order arithmetic, since for foundational purposes we do not need to discuss here the mathematical theory of the real numbers - the reduction of the reals to (sequences of) the integers is assumed known.

We recall that the species of integers $\mathbb{N} = \mathbb{T}(\mathbb{1})$, and that we simplified the notation for 0 and for successors $(n^+ = [\mathbb{1} \to n]^+)$. By recursion we can introduce all the usual primitive recursive functions and prove at least all the basic theorems of primitive recursive arithmetic (eg. GOODSTEIN (7), Chapter V). In particular we can introduce the equality function $\mathrm{E}\, \varepsilon [\mathbb{N} \to [\mathbb{N} \to \mathbb{2}]]$ and prove :

$$\begin{aligned} &\vdash \mathrm{E}(0)(0) = \bar{0} \\ &m \varepsilon \mathbb{N} \vdash \mathrm{E}(0)(m^+) = \bar{1}, \\ &n \varepsilon \mathbb{N} \vdash \mathrm{E}(n^+)(0) = \bar{1}, \\ &n \varepsilon \mathbb{N},\ m \varepsilon \mathbb{N} \vdash \mathrm{E}(n^+)(m^+) = \mathrm{E}(n)(m), \\ &n \varepsilon \mathbb{N},\ m \varepsilon \mathbb{N},\ \mathrm{E}(n)(m) = \bar{0} \vdash n = m\ . \end{aligned}$$

(as will be seen from [7], this is not so easy, but it is elementary.) This allows us to define the predicate of equality between integers :

DEFINITION

$$[n =_{\mathbb{N}} m] = [\top \wedge \bot](\mathrm{E}(n)(m))\ .$$

One can then establish the validity of all the usual logical formulas involving equality over the domain $\mathbb{N}$. It takes a little trouble, but let us assume its done.

The next step is to consider the validity of formulas of higher-order arithmetic. What are these formulas? In the first place they contain variables of several sorts or types. We already have at hand the notation for these types :

$$\mathbb{N},\ [\mathbb{N}\to\mathbb{N}],\ [\mathbb{N}\to[\mathbb{N}\to\mathbb{N}]],\ [[\mathbb{N}\to\mathbb{N}]\to\mathbb{N}],$$

and so on. We can imagine what a <u>stratified formula</u> should be (all the types of arguments and values of functions in terms should match). The atomic formulas are numerical (type $\mathbb{N}$) equations, and we may use constants 0, $^{+}$, and anyother well-known functions.

The main effort here is seeing what the formulas are, because <u>validity</u> is already understood (in theory).

We leave to the (poor) reader the verification of :

(A1) $\models \exists n\varepsilon\mathbb{N}\ [[n =_{\mathbb{N}} 0]]$

(A2) $\models \forall n\varepsilon\mathbb{N}\ [\exists m\varepsilon\mathbb{N}[[m =_{\mathbb{N}} n^{+}]]]$

(A3) $\models \forall n\varepsilon\mathbb{N}\ [\ [0 =_{\mathbb{N}} n^{+}]]$

(A4) $\models \forall n\varepsilon\mathbb{N}\ [\forall m\varepsilon\mathbb{N}[[[m^{+} =_{\mathbb{N}} n^{+}]\to[m =_{\mathbb{N}} n]]]]$,

but they are, after all, rather easy. We shall discuss, however, the <u>induction axiom</u> :

(A5) $\models [P(0)\to[[\forall n\varepsilon\mathbb{N}[[P(n)\to P(n^{+})]]\to\forall n\varepsilon\mathbb{N}[P(n)]]]]$.

For this we must "fill in" the τ of :

$$P = \forall n\varepsilon\mathbb{N}[P(n)],\ p\varepsilon[\bot\to P],\ t\varepsilon P(0),\ u\varepsilon\forall n\varepsilon\mathbb{N}[[P(n)\to P(n^{+})]] \vdash \tau\varepsilon P\ .$$

(For this particular argument we do not require the $p\varepsilon[\bot\to P]$, but the author wanted to state the problems in full.)

The construction of the construction τ will be given by - recursion, which is hardly surprising. The only trick is to know what the values of the function should be. The function we want has values $[n\wedge\tau(n)]$ (supposing for the moment we already knew our τ), because they can conveniently be chosen by recursion : let

$$\tau' = Rv[\mathbf{1}, [0\wedge t],\ [v(\dot{0})(\bar{0})^{+}\wedge u(v(\dot{0})(\bar{0}))(v(\dot{0})(\bar{1}))]]\ .$$

Then $\tau = \forall n \varepsilon \mathbf{N}[\tau'(n)(\bar{1})]$ (In the above formula, the reader is reminded that $\mathbf{N} = \mathbf{T}(\mathbf{1})$ and in (T5), $v = [\mathbf{1} \to f(u(\dot{0}))]$. The "trick" of the recursion is to let the next value of the function depend not only on the previous value but also on the previous argument.) The desired result will then be proved by the induction principle (T3). The reasoning is not really circular : we are showing how to reduce a compound form of induction back to a more primitive kind. Nevertheless, we do not do away with <u>all</u> assumptions. (Likewise, in set theory we still need an axiom of infinity to have a <u>set</u> of integers.)

Next we have the <u>axiom of choice</u> :

$$\text{(A6)} \qquad \forall x \varepsilon a[\exists y \varepsilon b[P(x)(y)]] \models \exists f \varepsilon [a \to b][\forall x \varepsilon a[P(x)(f(x))]],$$

where we are using not only a free binary predicate variable P, but also free "type" variables a and b as well.

Let $\pi = P f \varepsilon [a \to b][\forall x \varepsilon a[P(x)(f(x))]]$. Then for τ take $\tau = \pi(q)(\psi)$, where we assume $t \varepsilon \forall x \varepsilon a[\exists y \varepsilon b[P(x)(y)]]$ and let $\varphi = \forall x \varepsilon a[t(x)_0]$ and $\psi = \forall x \varepsilon a[t(x)_1]$. It is that easy, because the interpretation of the existential quantifiers is so constructive.

Rather more complicated is the axiom of dependent choices, a principle very important for analysis but curiously overlooked until recently (cf.eg. the end of MYHILL [17]) :

$$\text{(A7)} \quad \forall x \varepsilon a[[P(x) \to \exists y \varepsilon a[[P(y) \wedge R(x)(y)]]]] \models$$
$$\forall x \varepsilon a[[[P(x) \wedge Q(x)] \to \exists f \varepsilon [\mathbf{N} \to a][[[P(f(0)) \wedge Q(f(0))] \wedge \forall n \varepsilon \mathbf{N}[R(f(n))(f(n^+))]]]]].$$

With our bracketing conventions, this is an axiom that is harder to write than to understand. In words : if a <u>finite</u> chain of relationships among elements of a having property P can be indefinitely extended, then (assuming we have a constructive verification of the hypothesis) we can find an <u>infinite</u> chain of elements with successive terms of the chain related and the initial element specified. (The reader should not overlook the generalizations of this principle to trees other than those in $\mathbf{T}(\mathbf{1})$, but this is neither the time nor place to discuss them.) The desired sequence of elements is found by recursion, but one must be careful on which species the recursion is done.

To verify (A7), note that the hypothesis is equivalent (in a sense to be made precise in a moment) to the "formula" :

$$\forall u\varepsilon\ \exists x\varepsilon a[P(x)][\ \exists v\varepsilon\ \exists x\varepsilon a[P(x)][R(u_o)(v_o)]\ .$$

The sense of equivalence is simply this : given a construction belonging to the hypothesis of (A7), we can find a construction belonging to the above - and conversely. (This is the meaning of $\leftrightarrow$, too.) So let t be a construction belonging to the above. As in the argument for (A6) consider the two functions $\varphi = \forall u\varepsilon\ \exists x\varepsilon a[P(x)][t(u)_o]$ and $\psi = \forall u\varepsilon\ \exists x\varepsilon a[P(x)][t(u)_1]$. From the assumption on t we note that we can prove in the theory the conclusion

$$\psi\varepsilon\forall u\varepsilon\ \exists x\varepsilon a[P(x)][R(u_o)(\varphi(u)_o)]\ ,$$

which is just a more explicit version of our main hypothesis if we also remember

$$\varphi\varepsilon[\ \exists x\varepsilon a[P(x)] \rightarrow\ \exists x\varepsilon a[P(x)]]\ .$$

Next suppose that $Z\varepsilon a$ and $p\varepsilon P(Z)$ and $q\varepsilon Q(Z)$. We then iterate φ by recursion obtaining $\tilde{\varphi}$ so that

$$\tilde{\varphi}\varepsilon[\mathbb{N}\rightarrow\ \exists x\varepsilon a[P(x)]]$$
$$\tilde{\varphi}(0)_o = Z,\ \tilde{\varphi}(0)_1 = p,\ \text{and}$$
$$\tilde{\varphi}(n^+) = \varphi(\tilde{\varphi}(n)),$$

for $n\varepsilon\mathbb{N}$. (We are speaking informally, but this can all be done in the system.) Next we let $f = \forall n\varepsilon\mathbb{N}[\tilde{\varphi}(n)_o]$, and we find

$$p\varepsilon P(f(0)),\ q\varepsilon Q(f(0)),\ \text{and}$$
$$\psi(\tilde{\varphi}(n))\varepsilon R(f(n))(f(n^+))$$

for $n\varepsilon\mathbb{N}$. By using all manner of pairing functions all these facts can be put together to obtain a term which can finally be shown to belong to the conclusion of (A7). (A note to the reader who tries this : remember $\tilde{\varphi}$ contains Z and p as free variables and that you will have to apply functional abstraction to them.)

Very much analysis can already be carried out on the basis of (A1) - (A7) (using intuitionistic logic!) and we have BISHOP [1] as evidence. However, such topics as BOREL sets and continuous functions on BAIRE space (eg. functions of type $[[\mathbb{N}\to\mathbb{N}]\to\mathbb{N}]$) bring up definitions by recursion on the second number class or better : on $\mathbb{T}(\mathbb{N})$. We shall only discuss one topic here : the definition of the predicate K on $[[\mathbb{N}\to\mathbb{N}]\to\mathbb{N}]$ that determines the continuous functions and which is obtained by recursion.

Before giving the recursion on $\mathbb{T}(\mathbb{N})$, it is convenient to introduce by an ordinary recursion on $\mathbb{N}$ that operator $*$ such that

$$m\varepsilon\mathbb{N},\ f\varepsilon[\mathbb{N}\to\mathbb{N}]\ \vdash\ [m * f](0) = m\ ,$$

and

$$m\varepsilon\mathbb{N},\ f\varepsilon[\mathbb{N}\to\mathbb{N}],\ n\varepsilon\mathbb{N}\ \vdash\ [m * f](n^{+}) = f(n)\ .$$

This is simple and we need not give the explicit definition for $*$. One can think of $*$ as a kind of translation operator on BAIRE space.

To define K we define an auxiliary operator $\mathring{K}$ by recursion on $\mathbb{T}(\mathbb{N})$ such that :

$$\vdash\ \mathring{K}(0(\mathbb{N})) = \forall k\varepsilon[[\mathbb{N}\to\mathbb{N}]\to\mathbb{N}][\exists n\varepsilon\mathbb{N}[\forall f\varepsilon[\mathbb{N}\to\mathbb{N}][[k(f) =_{\mathbb{N}} n]]]],$$

and

$$u\varepsilon[\mathbb{N}\to\mathbb{T}(\mathbb{N})]\ \vdash\ \mathring{K}(u^{+}) = \forall k\varepsilon[[\mathbb{N}\to\mathbb{N}]\to\mathbb{N}]$$
$$[\forall m\varepsilon\mathbb{N}[\mathring{K}(u(m))(\forall f\varepsilon[\mathbb{N}\to\mathbb{N}][k([m * f])])]].$$

Then K can be defined by the equation :

$$K = \forall k\varepsilon[[\mathbb{N}\to\mathbb{N}]\to\mathbb{N}][\exists t\varepsilon\mathbb{T}(\mathbb{N})[\mathring{K}(t)(k)]].$$

The intention of the definition is that a construction in K(k) when $k\varepsilon[[\mathbb{N}\to\mathbb{N}]\to\mathbb{N}]$ gives the direct evidence of why k is continuous. The motivation for the definition is based on the well-known inductive analysis of continuous functions on $[\mathbb{N}\to\mathbb{N}]$. Unfortunately, we do not have the time to discuss the notion further here but can only mention the axioms that can be validated, namely those of <u>closure</u> and <u>induction</u>:

(A8) (i) $\vdash \forall n\varepsilon\mathbb{N}[K([[\mathbb{N}\to\mathbb{N}]\to n])]$,

(ii) $\vdash \forall k\varepsilon[\mathbb{N}\to[[\mathbb{N}\to\mathbb{N}]\to\mathbb{N}]][[\forall m\varepsilon\mathbb{N}[K(k(m))] \to$
$K(\forall f\varepsilon[\mathbb{N}\to\mathbb{N}][k(f(0))(\forall n\varepsilon\mathbb{N}[f(n^{+})])])]]$,

$$\text{(iii)}\ \forall n\in\mathbb{N}[P([[\mathbb{N}\to\mathbb{N}]\ \to n])],$$
$$\forall k\in[\mathbb{N}\to[[\mathbb{N}\to\mathbb{N}]\ \to\mathbb{N}]][[\forall m\in\mathbb{N}[P(k(m))]\ \to$$
$$P(\forall f\in[\mathbb{N}\to\mathbb{N}][k(f(0))(\forall n\in\mathbb{N}[f(n^+)])])]]$$
$$\models \forall k\in[[\mathbb{N}\to\mathbb{N}]\ \to\mathbb{N}][[K(k)\to P(k)]]\ .$$

Clearly, if we do not soon introduce some abbreviations, our formulas will be quite impossible to read. The worst part of the above (A8) is the clumsy restrictions of the variables.

Suppose we let $\mathbb{B} = [\mathbb{N}\to\mathbb{N}]$ and $\mathbb{F} = [\mathbb{B}\to\mathbb{N}]$. Further let us define $\vec{f} = \forall n\in\mathbb{N}[f(n^+)]$. Then for example (A8) (ii) reads :

$$\models \forall k\in[\mathbb{N}\to\mathbb{F}]\,[\forall m\in\mathbb{N}[K(k(m))]\to K(\forall f\in\mathbb{B}\,[k(f(0))(\vec{f})])],$$

where we have also left out some brackets. It could have been shortened even further if we had given a special name to the transformation :

$$\forall k\in[\mathbb{N}\to\mathbb{F}]\,[\forall f\in\mathbb{B}\,[k(f(0))(\vec{f})]]\in[[\mathbb{N}\to\mathbb{F}]\ \to\mathbb{F}].$$

Another approach to practical readibility would be to have conventions that certain variables were to range over certain species. It is hard to stick to these conventions when our alphabet is so finite, however.

This completes our brief survey of the foundations of analysis based on the theory of constructions. What we have given should have been enough, though, to convince the reader that our theory is a sufficiently strong and fertile one.

CONCLUSION

We have tried to present here with adequate motivation a theory of constructions and to show how it is in harmony with BROUWER'S program - at least as the program has been explained by HEYTING. We consider the attempt rather successful, but much remains to be done. For example, we have not discussed quantification over species (better : subspecies of a given species). This can be done in a convenient way within the framework of the present theory, though it is necessary to adjoin new primitive notions. Such considerations bring up problems of consistency, and we have not had time here to investigate the many interesting models that can be (non-constructively!) fashioned for the theory. Especially interesting is a model (of which the author is 85% sure that it can be defined) that has the property that all functions are continuous. This thesis, which is certainly related to BROUWER'S view - except we are not making use of choice sequences - ought to have rather interesting consequences. But there are other thesis possible too : we might want to assume that all basic species are countable - in the sense that they can all be mapped one-one into $\mathbb{N}$. Clearly that thesis also would have different but far reaching consequences. And then there is CHURCH'S Thesis and KRIPKE'S Schema, and these should all be investigated further. What we have accomplished here is the providing of a good context within which to compare these assumptions.

In another direction, we find a host of proof-theoretical problems. One must transpose LÄUCHLI'S proof to this context as well as the results of GOODMAN'S Thesis. A point to think about is whether the author has made the transfinite part of the theory too strong. Are there theorems of first-order arithmetic that can be validated using constructions based on $\mathbb{T}(\mathbb{N})$ but not without it? And what about $\mathbb{T}(\mathbb{T}(\mathbb{N}))$? And what is the strength of the theory with only finite species? A different question : does the constructive proof of GÖDEL'S Incompleteness Theorem suggest any reflection principles that could be added to the theory preserving its constructive character? Would this be a way in which an "abstract" theory of proofs might become interesting again? There seem to be quite a number of things to think about in this area, and the theory of constructions - in this form or another - gives us a way of making the questions and answers precise.

POSTSCRIPT

After further discussions with KREISEL and GÖDEL it has become clear that the attempt to eliminate "proofs" (as abstract objects) and to concentrate on the "pure" constructions is not successful : the decidability problems definitely show that the desired reduction of logical complexity has not been obtained. Therefore, the theory must be revised. (For an exact formulation of a relevant "adequacy condition", as KREISEL calls it, see Problem 10 of his [13].) The author is still unable to formulate any "abstract" theory of proofs that would seem convenient, but he has a suggestion that might be sufficient for the purpose of an adequate theory of constructions. Namely, we replace the elementary assertions $\Delta \vdash \delta$ by assertions $\varepsilon : \Delta \vdash \delta$ where ε is a term denoting a construction which measures the stage at which $\Delta \vdash \delta$ can be proved. Many people have considered stages of evidence, and it seems as though the constructions can easily be used to index these stages and to formalize the idea. For one thing proofs (and ordinals) can be related to trees (as BROUWER did himself) and as we noted above, the constructions can also be thought of as trees. The idea will require some development, and the author did not want to publish this paper until he was more certain that the approach is reasonable. But maybe the details we have outlined here can be of some inspiration to others.

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