Are the Traditional Philosophies of Mathematics Really Incompatible?

J. Lambek

The Four Schools

Having been under the impression that most mathematicians do not care about the foundations of their subject, I was amazed by the heat generated by this topic in recent issues of the *Mathematical Intelligencer*, particularly in the letters to the editor (see, e.g., Paris [25]). The purpose of this article is to marshal a number of facts that support a certain philosophical thesis, which I hope to persuade at least some readers to share.

I would like to argue that, contrary to widely held opinion, the traditional philosophies, logicism, formalism, Platonism, and intuitionism, *if stated with sufficient moderation*, do not really contradict each other, although I still have some reservations about logicism. This idea was first proposed in our book [17] by Phil Scott and me and elaborated for a philosophical audience in collaboration with Jocelyne Couture [5]. The present discussion owes a considerable debt to both co-authors. For background material on the traditional mathematical philosophies, the reader is referred to the standard references Benacerraf and Putnam [1], Hintikka [10], and van Heijenoort [30].

There are a number of problems a philosophy of mathematics should address. Perhaps the most important of these are: How is mathematical knowledge obtained (epistemology), and why can it be applied to nature? However, we shall here confine attention to another problem: What is the nature of mathematical entities (ontology) and of mathematical truth?

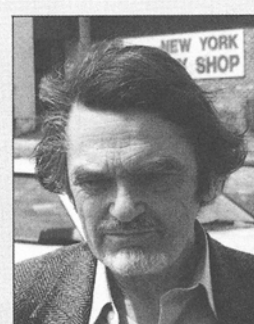
The best-known mathematical philosophies have given different answers to this ontological question (see [5]), which we shall summarize here in rather abbreviated form.

Logicists claim that mathematical entities can be defined in the language of symbolic logic.

Formalists claim that mathematical entities, if they exist at all, are nothing but terms of a formal language (of course, modulo the equivalence relation of *provable equality*, two terms being equivalent, or denoting the same entity, if the formula obtained by putting an equality sign between them is provable in the language).

Platonists claim that mathematical entities exist independently of our way of viewing them.

Intuitionists claim that mathematical entities are mental constructs.



Jim Lambek

After a short career as a lumberjack, *Jim Lambek* spent half a century at McGill, first as a student and then as a teacher, with sabbatical interruptions at Princeton, Zürich, Paris, and Oxford. His interests have ranged from elementary number theory, where he collaborated with the late Leo Moser, to linguistics. Aside from lecture notes, he has written one and a half books: *Lectures on Rings and Modules* and *Introduction to Higher Order Categorical Logic*, the latter coauthored with Phil Scott. He plans to write a few more, but time is getting short.

The different schools also have varied conceptions of what is mathematical truth. Logicists and formalists would claim that mathematical statements are true only when they are provable. Platonists claim that mathematical truths are there to be discovered and intuitionists claim that mathematical statements are true only if they can be known to be true.

These are the more moderate views expressed by these schools. Some adherents of these schools may have more extreme opinions, which we shall mention only briefly. Thus, an extreme logicist might claim that set theory is not part of logic. An extreme formalist might claim that mathematics is a meaningless game and that there is no such thing as a number, that only numerals exist. An extreme Platonist might believe that mathematical entities are ideas in the mind of a supernatural being; I am told that this view was proposed by Nichomachus of Gerasa about 100 A.D. and entertained by many religious thinkers since. Finally, an extreme intuitionist might believe that only those statements are true which are known to be true today; this was, in fact, occasionally asserted by the founder of the school.

It goes without saying that these extremist views cannot be reconciled with one another. Alas, it has become clear from various comments I have received that the more moderate reformulations, proposed in the interest of eclectic conciliation, are rejected by some adherents as well as by some opponents of these positions. However, I believe that, if hard pressed, I could find adherents who will accept the moderate presentations advocated here.

Gödel's Impact

There seems to be a general consensus among logicians that the rather vague concept of "truth" should be replaced by the more precise notion of "truth in a model," and we shall adopt this point of view here. Hilbert's formalist program implicitly contains the proposal that the semantical notion of truth can be captured by the syntactic notion of provability. In a sense, this proposal was carried out by Gödel [1930] in his completeness theorem:

> *a statement in a formal language is provable if and only if it is true in every model of that language.*

This result holds not only for first-order logic, but also for higher-order logic, that is, type theory, as was shown by Henkin [8], and even for intuitionistic type theory [17]. Type theory for us is the language of mathematics.

Presumably, this solution is not acceptable to a Platonist, who feels uneasy with the plurality of models and wishes to single out a distinguished model, let us call it the *real world of mathematics*, in the hope that truth in this model alone should suffice.

Gödel was a Platonist and believed in a real world of mathematics. In the semantic version of his famous incompleteness theorem [7], he apparently showed that the Platonists' hope is incompatible with Hilbert's proposal: There are mathematical statements true in what he thought was the real world, yet not provable in a language adequate for arithmetic, it being assumed that the set of proofs in that language is recursive. But wait a minute, let us look at Gödel's argument more closely. (See also [4].)

Gödel constructed a formula G of the form $\forall_{y \in N} \varphi(y)$ such that G is not provable, yet *true in every ω-complete model*. By this, we shall mean that $\forall_{y \in N} \varphi(y)$ is true in the model whenever $\varphi(0), \varphi(S0), \varphi(S(S0))$, etc. are all true. (For some details in this argument, the reader is invited to consult Appendix II.) It follows that the following two statements are incompatible:

(a) the real world of mathematics is ω-complete;
(b) truth in the real world implies provability.

Classical Platonists may assert (a), whereas Hilbert presumably hoped for (b). So who is right?

Brouwer to the Rescue

Curiously, it would seem in retrospect that the intuitionist Brouwer comes to Hilbert's rescue here, even though both Hilbert and Brouwer had perceived a conflict between their respective positions, formalism and intuitionism, prior to the publication of Gödel's epoch-making paper (see [29]). Moreover, to allow himself to be rescued, Hilbert would have to sacrifice the *principle of the excluded third*, which is not essential to a formalist position.

Brouwer would certainly deny (a) and, although he cannot be accused of favouring Platonism, I shall argue that his position can be interpreted as defending (b), thus removing the apparent contradiction between formalism and Platonism. On the one hand, he would allow us to assert the truth of $\forall_{y \in N} \varphi(y)$ only if the truths of $\varphi(0), \varphi(S0), \varphi(S(S0))$, etc., can be established in a *uniform* way. This would fail to be the case, for example, if the lengths of the proofs of $\varphi(S^n 0)$ were unbounded as n varies over the natural numbers. On the other hand, he would insist that a mathematical statement is true only when it can be *known*, which we will take the liberty of interpreting to mean that it can be *proved*.

It would appear that Brouwer himself later softened his stand against formalism and that his present-day followers, on the whole, have adopted formal proof theory as a tool to investigate his principles. On the other hand, intuitionism has been accepted into the mathematical mainstream, even if not always as an exclusive position, by constructivists, logicians, categorists, and, for some purposes, by computer scientists. (See, e.g., Troelstra and van Dalen [28].)

What Distinguished Model?

There remains the question whether there is a distinguished model which suffices for the discussion of mathematical truth. We shall not follow Tait [27], who believes that Platonism can survive even without this "Model-in-the-sky," as he calls it. Having accepted the intuitionistic viewpoint, we must insist that this model should not only exist in the classical sense, but that it should actually be constructible.

Different answers have been given to this question. Plato himself would have said that the real world is inhabited by *ideal* objects, of which we can only observe the shadows. Leibniz would have said that the real world, namely, *this world*, is the *best of all possible worlds*. A modern logician would be tempted to construct the distinguished model as the *term model* of pure intuitionistic type theory: The entities in it are closed terms modulo provable equality. In particular, a statement is true if and only if it is provably equal to $\top$, that is, if and only if it is provable. A categorist might attempt to bring Leibniz up to date, albeit in a watered-down version, but one that is immune against Voltaire's criticism, by suggesting that the distinguished model is an *initial object* in the category of all models.

It turns out that the term model when suitably presented as a category, which might reasonably be called the *Lindenbaum–Tarski category*, is an initial object in the category of all models, even in the category of all *toposes* (with *logical* morphisms) and has been called the *free topos*. (See Appendix III for a discussion of toposes.) It so happens that the intuitionistic version of model, generalizing Henkin's classical *nonstandard model*, is a special kind of topos. It is true, though not at all obvious, that the term model is a model in this sense.

A topos $\mathcal{T}$ is called a *model* if it shares the following properties with the usual category of sets:

1. $\bot$ is not true in $\mathcal{T}$;
2. if $p \vee q$ is true in $\mathcal{T}$, then p is true or q is true;
3. if $\exists_{x \in A}\varphi(x)$ is true in $\mathcal{T}$, then $\varphi(a)$ is true for some arrow $a : 1 \to A$ in $\mathcal{T}$.

These properties have an algebraic translation, first pointed out by Peter Freyd, concerning the terminal object 1 of $\mathcal{T}$:

1. 1 is not an initial object;
2. 1 is indecomposable;
3. 1 is projective.

Here "projective" has exactly the same meaning as in module theory. Moreover, model toposes are the analogues of local rings, and the completeness theorem can be sharpened to yield an analogue of the representation of commutative rings by continuous sections of sheaves of local rings [13].

Constructive Formalism

What we are suggesting here is that the free topos is a suitable candidate for the real (meaning *ideal*) world of mathematics. It should satisfy a moderate formalist because it has been constructed from terms of a language and because it exhibits the correspondence between truth and provability. It should satisfy a moderate Platonist because it is distinguished by being initial among all models and because truth in this model suffices to ensure provability. It should satisfy a moderate intuitionist, who insists that "true" means "knowable," not only because it has been constructed from pure intuitionistic type theory, but also because it illustrates all kinds of intuitionistic principles [17]. The free topos would also satisfy a logicist who accepts pure intuitionistic type theory as an updated version of symbolic logic and is willing to overlook the objection that the natural numbers have been postulated rather than defined.

It is by no means a trivial matter to show that the Lindenbaum–Tarski category is a model in our sense. Some fancy metamathematics or category theory has to be used to prove this (see, e.g., [17]). The three properties of truth in a model are certainly principles that Brouwer would have insisted on. [The arrow $a : 1 \to A$ of property (3) is just a term in the internal language of $\mathcal{T}$; see Appendix II.] He might also be happy that truth in the free topos coincides with provability, even if the latter is only a formalist's attempt to interpret "knowability."

It also turns out that in the free topos every natural number is *standard*, namely, equal to one of 0, $S0$, $S(S0)$, etc. In view of property (3) in the definition of "model," it then follows that the free topos is ω^**-complete* in the following sense: if $\exists_{y \in N}\psi(y)$ is true in the topos, then one of $\psi(0), \psi(S0), \psi(S(S0))$, etc., is true. This property is equivalent to ω-completeness classically, but not intuitionistically. We may, therefore, subscribe to the following revised form of (a):

(a*) the real world of mathematics is ω^*-complete.

In this connection it should be pointed out that the *free Boolean topos*, namely, the Lindenbaum–Tarski category for *pure classical type theory*, is not a model because for Gödel's sentence G, $G \vee \neg G$ is true, but neither G nor $\neg G$ is, thus violating property (2). One may, of course, obtain a model of pure classical type theory from the free Boolean topos, as a first step with the help of an ultrafilter of arrows $1 \to \Omega$, by declaring all these to be equal to $\top$; but I doubt whether any such ultrafilter can be described constructively.

If we look at Gödel's incompleteness theorem for pure classical type theory, we thus obtain a classical (Boolean) model which is not ω-complete. It follows from Gödel's argument (see Appendix II) that there is a formula $\varphi(y)$, y a variable of type N, such that $\varphi(0), \varphi(S0), \varphi(S(S0))$, etc., are all true, but also $\exists_{y \in N}\neg\varphi(y)$ is true. This allows us to construct a nonstandard natural number $n \equiv \mu_{y \in N} \neg\varphi(y)$ in this classical model, which is *a fortiori* a model of pure intuitionistic type theory. The existence of nonstandard natural numbers in a model is what bothered Nyikos [24]. Would he be satisfied with the distinguished model discussed above in which all natural numbers are standard?

To sum up, we suggest that to a moderate intuitionist there should be no contradiction between formalism and Platonism. Moreover, he ought to be willing to accept the free topos as one candidate for the real world of mathematics, at least of elementary mathematics. It is not necessary for our argument that the free topos is *all there is*, only that its existence shows the compatibility of apparently conflicting views.

There are competing candidates for the "real world of mathematics," for instance, Gödel's *universe of constructible sets* and Martin Hyland's *realizability topos*; but I have not investigated to what extent either of these notions would support the attempt of eclectic conciliation.

The proposal to accept as the real world of mathematics the term model of pure intuitionistic type theory, or perhaps of some more powerful language, has been called *constructive nominalism* in [5]. It is my belief that this proposal can be extended to natural languages to construct the everyday world of "shoes and ships and sealing wax, of cabbages and kings" [2]. There may even be different such worlds for different linguistic cultures. I suspect that similar views are held by a number of philosophers, linguists, and anthropologists.

The real world of mathematics should not be confused with the real world of physics. Not being ultrafinitists, who believe that numbers greater than $10^{10^{10}}$ (say) do not exist, we take the world of mathematics to be infinite. According to the present state of physics, there is no conclusive evidence that the material universe is infinite in the large, although ever since Zeno, it is generally believed that every interval contains infinitely many points, but even this has been doubted, for example, by Coish [1959].

What About Logicism?

The problem with logicism is not its compatibility with the other positions, but whether it is defensible in the first place. The usual mathematical entities are natural numbers, pairs of such, sets of such, sets of sets of such, and so on. If we want to reduce mathematics to logic, as chemistry has been reduced to physics, we must surely include the machinery of set theory into what we call logic, thus allowing for the notions of equality and membership and some form of the comprehension scheme.

This much seems to be taken for granted by all logicists. The difficulty arises when we want to construct the natural numbers as sets. For this, we need an *axiom of infinity*, which asserts the existence of an infinite set. But then we may as well adopt Peano's axioms in the first place. This entails, in particular, that we include symbols for zero and successor in our language. There seems to be a general feeling that this is contrary to the logicist program, hence that logicism has failed.

There is, however, a glimmer of hope that logicism may be resurrected, in view of recent developments in categorical computer science (e.g., [15], [16]). Following the lead of Church, one can construct the natural numbers object in a category as the retract of the *formal* product $\Pi_X (X^X)^{(X^X)}$ or $\Pi_X X^{(X^{X+1})}$, where X is an indeterminate object and where the retract is constructed with the help of equalizers. Unfortunately, such formal products exist neither in the usual category of sets nor in the free topos, so some difficulties still have to be ironed out. Anyway, if logicism is to be salvaged, this may have to be with the help of categorical logic. See the discussion below.

Some Objections

It is not likely that the proposed compromise among three, or perhaps four, major philosophical schools will put the controversy about the foundations of mathematics to rest. One reason for this is that some people's favourite positions have been ignored in this discussion, for example, predicative mathematics, ultrafinitism, and quasiempiricism. Others believe that formalism and Platonism are both wrong. This is the opinion of Saunders Mac Lane (expressed privately, but see also his lecture [20] and his book [21, Chapter XII]). Finally, I may as well admit that, in presenting the traditional philosophies in moderate form, I have distorted each of them a little. It is debatable whether Plato, Frege, Hilbert, and Brouwer would acknowledge my version of Platonism, logicism, formalism, or intuitionism, respectively.

Mac Lane has also criticized the prominence given to the free topos, or, what amounts to the same thing, to pure intuitionist type theory. Of course, other models (equivalently, applied type theories) should be studied too, and one may even look at them simultaneously, for instance bundled up in a sheaf [13]. The question then arises: Where do these models live? Well, in the category of sets, of course. But what is the category of sets? According to a constructive nominalist, it is the free topos. Yet there are other candidates for the category of sets and these are models of pure intuitionistic type theory, so we are back where we started.

Let me anticipate another criticism, which shows that constructive nominalism, the position defended here, is guilty of the same circularity. In constructing the free topos linguistically, we have taken the number 2 to be the class of all closed terms of our formal language which are provably equal to $S(S0)$. Now the terms of this language are elements of the free monoid generated by a finite set of symbols. However, the exact nature of these symbols is of no importance; it does not matter whether they consist of chalk marks or of sound waves, or whether they are written in blue or red ink. What matters is that the elements of the free monoid can be put in one-to-one correspondence with the natural numbers, as in Gödel's well-known arithmetization. If we pick one such coding of terms by numbers, we end up with the conclusion that 2 is a set of natural numbers! This is hardly an illuminating conclusion.

Pursuing this line of reasoning even further, we find that the free topos is an object in the category of sets, for that matter, in any model of our language, even in the free topos. Evidently, we have again gone in a circle. It is like lifting oneself up by one's shoelaces. However, I believe that this kind of circularity is inherent in any attempt to come to grips with basic ontological questions.

Many people share Gödel's belief (a) that the real world of mathematics is ω-complete and that, therefore, his statement G is true but not provable. Because apparently we can *see* that G is true, Penrose [26], following Lucas [19], draws the further conclusion "that our *consciousness* is a crucial ingredient in our comprehension of mathematical truth" and that it is "*not* something that we can ascertain merely by use of an algorithm." For all I know, this conclusion, asserting the superiority of the human mind over the computer, is correct, but I must reject the argument, as I do not believe (a).

Gödel himself drew an important corollary from his incompleteness theorem: *To prove the consistency of any language adequate for arithmetic one has to go outside that language.* This shows that Hilbert's proposal to restrict metamathematics to finitary methods cannot succeed. Indeed, metamathematicians no longer feel bound by Hilbert's proposed restriction. For example, the simplest proof of the consistency of pure intuitionistic type theory consists of pointing to property (1) of some model, say the free topos. Whereas the free topos can be constructed in pure intuitionistic type theory, the proof that it is a model requires more powerful methods.

Our version of type theory, sometimes called the *theory of finite types*, is adequate for elementary mathematics, namely, *arithmetic* and *analysis*. Even if we want to treat these subjects classically, we can do so within intuitionistic type theory by looking at statements of the form $\forall_{x \in \Omega}(x \vee \neg x) \Rightarrow p$. *Metamathematics* and *category theory* require more than the theory of finite types. One may have to admit not only the axiom of choice, but also much higher types, corresponding to Grothendieck universes in Gödel–Bernays set theory or to inaccessible cardinals in Zermelo–Fraenkel set theory. For some purposes, even quantification over types may be required.

Categorical Logicism

It has been argued by Henle [9] and Marquis [22] that logicism should be revived as *categorism* or *categorical logicism*. Without necessarily following these two authors, I see categorical logicism as abandoning the attempt to reduce arithmetic to logic, but realizing instead that, at a very basic level, arithmetic and logic are the same. To say this, however, one has to enlarge one's conception of logic to incorporate *proofs* in place of mere *provability*.

Consider, for example, the arithmetical identities

$$(a^b)^c = a^{(c \times b)}, \qquad a^{b+c} = a^b \times a^c,$$

and compare them with the intuitionistically valid logical equivalences

$$c \Rightarrow (b \Rightarrow a) \leftrightarrow (c \wedge b) \Rightarrow a,$$
$$(b \vee c) \Rightarrow a \leftrightarrow (b \Rightarrow a) \wedge (c \Rightarrow a).$$

The obvious analogy extends to the usual laws of arithmetic: commutative, associative, and distributive laws, and the laws of indices.

What lies behind this analogy is Lawvere's [18] concept of *cartesian closed category*. However, one can also understand the analogy by looking at sets: Replace the natural number a by a typical set of a elements and replace the proposition a by the set of all *reasons* for a. Here "reason" cannot be taken to mean proof, else all unprovable propositions would be replaced by the empty set; but it may be taken to be any deduction $c \to a$, where c is any proposition whatsoever (see [15]).

In this analogy, we have compared arithmetical operations with logical connectives. One can also compare arithmetical operations with logical deductions in the form of Gentzen sequents, because both are special cases of algebraic operations (see [14]). A primitive recursive function $\mathbf{N}^n \to \mathbf{N}$ may be viewed as realizing an operation $N^n \to N$ in a certain algebraic theory, and a Gentzen style deduction $A_1 \cdots A_n \to B$ may be viewed as an operation in a multisorted algebraic theory.

The categorical viewpoint allows us to go beyond mere ontology and ask: Which mathematical entities are interesting, relevant, or important? With Bill Lawvere and other categorists, I share the view that interesting mathematical entities tend to be categories or functors and that the growth of mathematics is often guided by looking for functors adjoint to previously known functors. However, I admit that it may be difficult to convince a number-theorist of this.

Appendix I. A Modern Version of Type Theory

Gödel's incompleteness theorem applies to any formal system, classical or intuitionistic, as long as it is adequate for arithmetic and as long as the set of all proofs is recursive. In fact, the title of his paper [7] referred to classical type theory as formulated by Russell and Whitehead in their *Principia Mathematica*. Personally, I prefer a more modern version of type theory as presented in [17]. We admit the following types and terms, the latter written under their respective types:

$$\frac{1 \quad \Omega \quad N \quad A\times B \quad\quad PA}{* \quad \substack{a=a' \\ a\in\alpha} \quad \substack{0 \\ Sn} \quad \langle a,b\rangle \quad \{x\in A|\varphi(x)\}},$$

where it is assumed that A and B are previously given types, that a and a' are terms of type A, x is a variable of type A, α a term of type PA, n a term of type N, b a term of type B, and $\varphi(x)$ a term of type Ω. We also presuppose a supply of countably many variables of each type.

The usual logical connectives may be defined by writing

$$\begin{aligned} \top &\text{ for } * = *; \\ p\wedge q &\text{ for } \langle p,q\rangle = \langle\top,\top\rangle, p \text{ and } q \text{ being of type } \Omega; \\ p\Rightarrow q &\text{ for } p\wedge q = p; \\ \forall_{x\in A}\varphi(x) &\text{ for } \{x\in A|\varphi(x)\} = \{x\in A|\top\}. \end{aligned}$$

Other connectives $\perp$, $\neg$, and $\vee$ and the quantifier $\exists$ are defined in familiar fashion, for example, by writing

$$p\vee q \quad \text{for } \forall_{x\in\Omega}(((p\Rightarrow x)\wedge(q\Rightarrow x))\Rightarrow x).$$

For a complete list of axioms and rules of inference, the reader is referred to [17]; there are no surprises. Notably absent is the axiom $\forall_{x\in\Omega}(x\vee\neg x)$ or, equivalently, $\forall_{x\in\Omega}(\neg\neg x\Rightarrow x)$; if it is added, one obtains *classical type theory*. We speak of *pure type theory*, intuitionistic or classical, if there are no types, terms, axioms, or rules other than those that have to be there; in *applied* type theory, there may be others.

It is often useful to incorporate into the language of type theory a Russellian description operator. It so happens that this is not needed in pure intuitionistic type theory as formulated here, nor is it needed in the internal logic of a topos to be discussed in Appendix III (see [17]).

Appendix II. Gödel's Argument

Gödel's basic argument may be presented as follows. Let $\alpha_0, \alpha_1, \alpha_2, \ldots$ be a given effective enumeration of all closed terms of type PN, and let $P_0, P_1, P_2, \ldots$ be an effective enumeration of all proofs, regarded as strings of terms of type Ω. Consider the metamathematical statement $R(m,n)$:

$$P_n \textit{ is a proof of } S^m 0 \in \alpha_m.$$

Gödel realized that this is a *recursive* binary predicate, having practically invented the theory of recursive functions to do so. He succeeded in proving that there is a formula (term of type Ω) $\Psi(x,y)$ with free variables of type N such that

(i) if $R(m,n)$, then $\Psi(S^m0, S^n0)$ is provable;
(ii) if not $R(m,n)$, then $\neg\Psi(S^m0, S^n0)$ is provable.

Now consider the closed term

$$\alpha_g \equiv \{x\in N|\forall_{y\in N}\neg\Psi(x,y)\}.$$

If we assume that $G \equiv S^g0 \in \alpha_g$ is provable, say with proof P_n, then we can prove $\Psi(S^g0, S^n0)$; hence $\exists_{y\in N}\Psi(S^g0,y)$, and therefore $\neg G$. If we assume that our formal language is consistent, we may infer that G is not provable and so not $R(g,n)$ for any n; thus $\neg\Psi(S^g0, S^n0)$ is provable for all n. Taking $\varphi(y)\equiv\neg\Psi(S^g0,y)$ in the definition of ω-completeness, we infer that $\forall_{y\in N}\neg\Psi(S^g0,y)$ is true in any ω-complete model, which implies that $G\equiv S^g0\in\alpha_g$ is true in such a model.

In the syntactic version of his incompleteness theorem, Gödel assumed that the language is ω-consistent and deduced that $\neg G$ is not provable either. Rosser later showed that ω-consistency here can be replaced by consistency (see [12] or [11]).

Appendix III. On the Notion of Topos

The notion of a *topos*, actually of an *elementary topos*, was introduced by Lawvere in collaboration with Tierney, following a lead by Grothendieck. It is a category which resembles the familiar category of sets in having finite products, exponentiation, like the object B^A of all functions from A to B, and a *subject classifier* Ω, resembling the set $\{\top,\perp\}$ in classical set theory, inasmuch as it allows one to characterize subsets of A by their characteristic functions from A to Ω. For our purposes, to the regret of all logicists, we must also stipulate a *natural numbers object* N, resembling the usual set of natural numbers.

This is not the place to describe in detail the *internal language* of a topos $\mathcal{T}$. Suffice it to say that its closed terms of type A are arrows $a: 1\to A$ in $\mathcal{T}$, where 1 is the terminal object (empty product) and A is any object. In particular, closed formulas are arrows $p: 1\to\Omega$. In general, the internal language of a topos is intuitionistic and there may be more than the two arrows $\top,\perp: 1\to\Omega$. To say that *p is true in $\mathcal{T}$* means that the arrows p and $\top$ from 1 to Ω coincide.

Conversely, from every language, that is, type theory, one can form the *topos generated* by it, alias its Lindenbaum–Tarski category. Its objects are closed terms α of type PA, A being any type, and its morphisms $\alpha\to\beta$, β of type PB, are closed terms of type $P(B\times A)$ about which it can be proved in the language that they denote functions from the set denoted by α to the set denoted by β.

When we say that a model topos $\mathcal{T}$ is a model of a language $\mathcal{L}$, we are implicitly referring to an interpretation of $\mathcal{L}$ in $\mathcal{T}$. (It so happens that, when $\mathcal{L}$ is pure intuitionistic type theory, there is exactly one such interpretation.) An *interpretation* of $\mathcal{L}$ in $\mathcal{T}$ may be viewed either as a morphism (*translation*) in the category of type theories from $\mathcal{L}$ to the internal language of $\mathcal{T}$ or, equivalently, as a morphism (*logical functor*) in the category of toposes to $\mathcal{T}$ from the topos generated by $\mathcal{L}$. (The equivalence follows from the fact that the processes "topos generated" and "internal language" are adjoint functors, see [17].)

A closed formula p of $\mathcal{L}$ is *true* in the topos $\mathcal{T}$, under the given interpretation, if the translation sends it onto the arrow $\perp$: $1\to\Omega$ in $\mathcal{T}$. Before Gödel proved the incompleteness theorem, people had need of a special term for "true in every model"; for example, "semantically true," and I seem to recall that Carnap used "analytic."

As we already hinted in the section entitled "What Distinguished Model?", a model of pure classical type theory is precisely a nonstandard model in the sense of Henkin, showing that this concept is not at all contrived, as some people seem to think.

It may be worth pointing out in support of nominalism that every topos is equivalent to the topos generated by its internal language. On the other hand, the internal language of the topos generated by a language is merely a conservative extension of the latter. Although it must be conceded to extreme formalists that, at first sight, pure intuitionistic type theory is not *about* anything, it has a conservative extension, the internal language of its free topos, which is about the free topos, the proposed candidate for the real world of mathematics.

Appendix IV. Some Recollections of Brouwer

I wish to take this opportunity to share some personal recollections of L.E.J. Brouwer. When he visited Canada, quite a few years ago, to address the Canadian Mathematical Congress (now called "Society") on his ideas, he defended his notion of "twoity" against H.S.M. Coxeter's criticism that it should be called either "twoness" or "binity." He also came to my house and became quite interested when I told him that he had influenced two people in rejecting Aristotelian logic, the founder of General Semantics, Korzybski, and the science-fiction writer Van Vogt. Somehow the conversation turned to Wittgenstein, and Brouwer doubted whether the latter had made any contributions to logic. I mentioned that Wittgenstein had invented truth tables, although I now know that they go back to Philo of Megara, about 300 B.C. Brouwer then asked: "What are truth tables?" I was naïve enough to attempt to explain them to him.

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