Mathematical proofs

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Mathematical Proofs

by *Marco Panza*[*]

The aim I am pursuing here is to describe some general aspects of mathematical proofs. In my view, a mathematical proof is a warrant to assert a non-tautological statement which claims that certain objects (possibly a certain object) enjoy a certain property. Because it is proved, such a statement is a mathematical theorem. In my view, to understand the nature of a mathematical proof it is necessary to understand the nature of mathematical objects. If we understand them as external entities whose "existence" is independent of us and if we think that their enjoying certain properties is a fact, then we should argue that a theorem is a statement that claims that this fact occurs. If we also maintain that a mathematical proof is internal to a mathematical theory, then it becomes very difficult indeed to explain how a proof can be a warrant for such a statement. This is the essential content of a dilemma set forth by P. Benacerraf (cf. Benacerraf, 1973). Such a dilemma, however, is dissolved if we understand mathematical objects as internal constructions of mathematical theories and think that they enjoy certain properties just because a mathematical theorem claims that they enjoy them. This is just my view.

By speaking of general aspects of mathematical proofs, I do not refer to their logical nature, as it could be expressed in one of the artificial languages actually used by modern formal logic, like the usual languages of predicative logic, or the lambda-calculus. I am not interested in suggesting a way of translating the arguments that mathematicians employ or have employed to prove their theorems in a suitable formal language, where these arguments are reduced to chains of formulas connected with each other by syntactical rules of inference, and to describe these chains in general. I am neither interested in suggesting a way of treating these arguments as the objects of a formal theory of proof, nor in considering the possibility of reducing them to executable programmes. Rather, I am interested in studying the internal structure of these arguments taken as such.

The difference between my aim and the aim of the formal theories of proof should be clear enough, and I shall not insist on it. By contrast, I would like to say something more about the difference between my aim and the usual description of mathematical proofs as chains of formulas of a formal language, connected with each other by syntactical rules of inference. I will call these chains "syntactical chains", for short.

I do not want to deny that amongst the arguments that mathematicians employ or have employed to prove their theorems there are some which appear as syntactical chains, once they are understood merely as systems of formulas of a certain language. Simply, I argue: *i*) that this is not the case for all the arguments that mathematicians employ or have employed to prove their theorems; *ii*) that these latter arguments can also be suitably understood otherwise than as systems of formulas of a certain language. Moreover, I maintain that when these arguments are understood in a different manner—as I shall

[*] A preliminary version of this paper was written during a visiting scholarship at the University of Bielefeld (*Institut fuer Didaktik der Mathematik*) from May to August 1997. I would like to thank the IDM and its managing director, M. Otte, for the logistic and financial supports. I am also grateful to P. Cassou-Nogues, A. Coliva, M. Otte and M. Radu, for useful discussions and suggestions.

suggest here to do—they present some characters which are common both to the arguments which appear (once they are understood merely as systems of formulas of a certain language) as syntactical chains and to those that do not. If we would be able to describe these characters well enough, we would recognise that these arguments are mathematical proofs because of these other aspects of them, and not simply because they appear as syntactical chains.

When an argument employed by a mathematician to prove one of his theorems does not appear as a syntactical chain (once they are understood merely as systems of formulas of a certain language), it could be profitable to translate it in this way. However, I am not interested in this sort of translations here. I shall call “ideal formal (mathematical) proof” a syntactical chain issued by a translation of this sort of a given previous mathematical argument. By contrast, I shall call “real (mathematical) proof” an argument a mathematician has actually used, in order to prove one of his theorems.

An ideal formal proof is an idealisation of a real proof, but it is not the only possible idealisation of a real proof. It is quite impossible to study an object in general without idealising it, and I will use in what follows other kinds of idealisations. What is important, when we are using an idealisation of some object, is not to confuse the object itself with its idealisation. The uneasiness of many working mathematicians with respect to the logical formalisation of their arguments just depends, I think, on such a confusion, brought about by a number of logicians or philosophers of mathematics.

A very simple example is enough to illustrate this confusion. Consider a mathematician who is proving that 170,363 is a prime number, and who is applying for that purpose one of the usual algorithms of factorisation of a given natural number. As soon as the computation is finished our mathematician claims: “No prime number less than 170,363 divides 170,363, thus this is a prime number.” It is possible to describe this (real) proof as a syntactical chain, where the rule of *modus ponens* is applied. By supposing that

a =: 170,363
$N(x)$ =: “x is a natural number”
$F(x)$ =: “x has no prime factor less than it”
$P(x)$ =: “x is a prime number”

a logician could describe this proof as follows:

$\forall x\{N(x)\Rightarrow[F(x)\Rightarrow P(x)]\}$	[by definition of P]
$N(a)$	[by definition of a]
$F(a)$	[as showed by the algorithm of factorisation]
$P(a)$	[by two applications of *modus ponens*]

Presumably, our mathematician would refuse to admit that his (real) proof actually makes use of the general syntactical rule called by the logician “rule of *modus ponens*”, and that his theory of numbers should integrate this rule in order to validate such a proof. More generally, he would not recognise his own real proof in the previous ideal formal proof. If the logician claims that his ideal formal proof is such a real proof, and that this proof actually makes use of the rule of *modus ponens*, then the tension is inevitable. But this claim depends on a confusion, I think, since to recognise the previous schema as a possible good translation (that is an idealisation) of a real proof is not the same as asserting that this schema is this proof, and that this proof actually makes use of the rule of *modus ponens*.

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My objective here is to state a number of necessary conditions that a certain object should satisfy in order to be, as such, a real mathematical proof. These conditions are certainly not also sufficient ones, and presumably they are not all the necessary conditions that a certain object should satisfy in order to be, as such, a real mathematical proof. In stating them, I will only try to go as far as I am able in providing a description of some common properties of real proofs, as they have appeared to me in my historical researches.

My starting point is given by a strategic choice. I understand and describe mathematics as a human activity. I am not interested in knowing whether there is a mathematical reality independent from this activity, or whether a mathematical proposition should be meant as true or false with respect to such a reality.

My point is simply that the mathematical *corpus*, as it appears to us today, has been constructed along the history by human beings, working in concrete and determinate situations. I am interested in the modalities of this work. This means that I am interested in the real act of doing mathematics, and not in the illusory structure of an eternal world that, in any case, we could only discern and describe by doing mathematics.

Thus, a real mathematical proof is for me a succession of acts, that is a procedure, that the members of a community could in principle repeat and that they accept as a warrant to assert certain statements called, because of that, “theorems”.

From such a point of view, to define mathematics (to set forth its nature) means to select a particular sort of human activity according to its modalities. When this selection has been performed we can claim that we are doing mathematics if and only if we are acting according to these modalities. There is no way, I think, to justify this definition *a priori* without falling in some sort of circularity. The only possible justification I see for it is *a posteriori*: using it, we are able to explain, in a satisfactory way, the historical phenomena we wanted to explain. And there is no manner to state in general what is a satisfactory way, since it is just one of the aims of the explanation that is proposed to provide us with a criterion of satisfaction. Philosophy does not differ on this point from any natural science.

I. The Functions of Mathematical Proofs

I describe mathematics in general as a human activity directed towards objects. This activity consists in defining these objects, exhibiting them, and looking for their properties.

I.1. *Definitions, Characterisation, and Exhibitions*

The distinction between definition end exhibition is crucial. It does not hold for all sorts of objects. Rather, we could distinguish, in general, between two sorts of objects: the objects which can be both defined and exhibited, and the objects that can only be defined—or for which there is no possible distinction between the act of definition and the act of exhibition.

In my terminology, to define certain objects means to set forth some properties that an object enjoys if and only if it is one such object. It is possible that a definition contains a clause of unicity, but this is not necessary in order to have a definition in my sense. If this happens to be the case, we shall say that such a definition is a definition of a certain object, otherwise we shall say that it is a definition of a certain sort of objects.

It is important to not confuse a definition of a certain sort of objects with a definition of a certain class of objects. The latter is the definition of a certain object (that is the class itself), while the former is the definition of any object of a certain sort, as long as it is just an object of this sort. Notice also that it is possible to define a certain sort of objects without defying or having defined the class of the objects of this sort. This is what we do in general when we define a certain sort of objects, like the chairs, the stars, the triangles, or even the definitions. Sometimes, we refer to a definition of a certain sort of objects—let us say of the a’s—by saying that is a definition of an a. This is possible, however, only if the context enables us not to confuse such a definition with a definition of a particular a, that is of course a definition of a certain object.

In general, we can describe an act of definition of a certain object a or of the a’s as the act of claiming that an object x is a or an a, if and only if $P(x)$. A statement which expresses this claim is a definition. Of course this is an ideal description. To adapt this ideal description to real definitions, we should imagine that “P” denotes any sort of complex property. It might be necessary, for example, to assume that “P” denotes a monadic property constructed by saturating n–1 free places in a n-places relation $R(x, y_1,\ldots, y_{n-1})$, in such a way that if the individual constants $b_1,\ldots, b_{n-1}$ which saturate these places denote some objects that may not have been already defined, in their turn. And it might be also necessary to assume that the relation $R(x, y_1,\ldots, y_{n-1})$ were not already defined, yet. Think, for example, of Peano’s implicit definition of the natural numbers (which is a definition of a certain sort of objects, that is at the same time a definition of a certain class of objects—the set N of natural numbers—and thus a definition of a certain object).

If the term “definition” is used in this sense, by saying that a certain statement is a definition we do not assign to such a statement a particular role in a system of statements. If you open an usual mathematical text, you might recognise, for example, a definition in this sense, both in the statements that are explicitly called “definitions”, and in the statements that are used, more soberly, to present the conditions of a problem.

Ideally, I understand a mathematical problem as a question concerned with a definition. Once a definition is presented, a problem consists in wondering: *i*) which are all the objects that satisfy it; or *ii*) which sort of objects satisfies it; or *iii*) which are some objects (one or more) that satisfy it.

Of course it is always possible to answer a problem by uttering some proper or common names. Not all the names work in the same way, when they are used to answer a problem, however.

Two cases can be easily considered and eliminated as trivial. Imagine **D** is a definition like “x is the a if and only if $P(x)$” and **P** is the problem of the form (*i*) associated to **D**, that is: “which is the object that satisfies the definition **D**?” Or imagine that **D’** is a definition like “x is an a if and only if $P(x)$” and **P’** is the problem of the form (*ii*) associated to **D’**, that is: “which sort of objects satisfy the definition **D’**?” We can answer to **P**, by saying that this object is a, and to **P’**, by saying that these objects are the a’s. In this way, we do not solve these problems, however. We only substitute them with two other equivalent problems: “which is the object called ‘a’?” (that is a problem of the form (*i*)), or “which sort of objects are called ‘a’?” (that is a problem of the form (*ii*)).

Imagine now that we answer to a problem of the form (*i*) or (*iii*) by saying that the sought objects are $b_1, b_2,\ldots,b_n$ (where n is possibly equal to 1), or to a problem of the form (*ii*) by saying that the sought objects are the b's, provided that these problems are not associated to the definitions of the objects $b_1, b_2,\ldots,b_n$ or of the b's. These answers are essentially different according to the way in which the names “b”, “b_1”, “b_2”,… ,“b_n” have been introduced. If these names have been, in their turn, introduced by an act of definition, then, by answering in such a way, we also substitute the given problems with other ones. To solve the given problems in a proper sense, we have to answer them in some way that is essentially different from referring to previous definitions, even if these definitions are not the ones to which these problems are associated. And this is the case if the names “b”, “b_1”, “b_2”,…,“b_n” have been introduced by an act of exhibition. In this case, the act of uttering these terms could in principle be substituted, as an answer to the given problems, by such an act of exhibition[1].

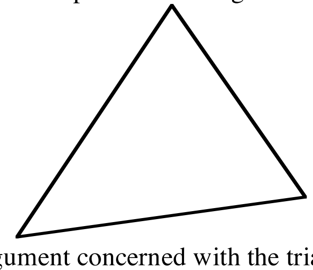
You could think that in such a way I have distinguished between two different ways of answering a problem and clarified such a difference by making appeal to the notion of exhibition and the different sorts of possible exhibitions. This was not my intention, however. By contrast, I claim to have defined, in such a way, the acts of exhibition: they are acts which can provide a final solution to a problem. Objects that cannot be exhibited are thus the objects (whose common or proper names) can never (be uttered to) provide a final solution to a problem.

I am quite sure that many people do not allow that there are objects of this sort, but it seems to me that, at this stage, the problem is nothing but a terminological one, i. e. it is only concerned with our conventions about the use of the term “object”. I am only asking, at this stage, to allow that a constant “a” which can enter a proposition like “$P(a)$” (that we are able to understand and we associate to any sort of assertibility conditions), denotes

[1] Of course, to exhibit some definite objects, like $b_1, b_2,\ldots,b_n$, is not the same as exhibiting a sort of objects, like the b's. If the b's are empirical objects, there is no way to exhibit this sort of objects as such. It is possible, at most to exhibit some b. In this case, to define a certain sort of objets is, on the other hand, the same as defying a certain property that the objects of this sort have to satisfy (the property ‘to be a b’), by reducing it to another property of them. Thus, when we define the chairs, we define the property ‘to be a chair’ by reducing it to the property ‘to be an object on which it is possible to sit down’, for example. It is not the same for mathematical objects, however. This is just one of the mayor points of Kant, who argued that “mathematics can consider the universal *in concreto*” (cf. I. Kant, *Critique of Pure Reasons*, A, 732; B, 760). I do not know whether this is only the case of mathematics, or it is also the case of other human activities. What I know is that in mathematics it is possible to exhibit a sort of objects either by exhibiting a generic object of this sort, or a particular object of this sort we are able to treat as any other object of this sort. As an example of the first case, consider the generic polynome

$$\sum_{i=0}^{n} A_i x^i$$

As an example of the second case, consider a particular triangle



that we consider as a support for an argument concerned with the triangles.

an object. If you accept such a convention, you should also accept, for example, that Othello is an object, even if you do not think that when I answer to the question “who did Iago betray?”, by uttering the name “Othello”, I am properly solving a problem. If you assume also that, when I answer the question “which is the highest mountain in the world?”, by uttering the name “Everest”, I am properly solving a problem, you should agree with me that there are two sorts of objects: the objects that can be both defined and exhibited, like the Everest, and the objects that can only be defined, as Othello. To fix my language, I shall call the first ones “genuine objects” and the second one “fictional objects”.

At this point, you can ask me: “when should one consider that a certain problem has been properly solved?” My answer is simple: there is no general rule for that. I have no idea, for example, if by answering “Othello” to the question about Iago’s I am properly solving a problem or not, and I do not think there is a rule to know or decide it in general. I simply assume that we are dealing with genuine objects only when we possess a criterion to distinguish, with respect to these objects, an act of exhibition from an act of definition. If you want to claim that by dealing with Othello you are dealing with a genuine object, then you have to indicate which acts, or which sort of acts, can be understood as acts of exhibition of Othello. Similarly, if you want to claim that by dealing with the Martians, you are dealing with a sort of genuine objects, you have to indicate which acts or which sort of acts can be understood as acts of exhibition of the Martians.

The same goes for mathematics. According to my definition of mathematics, mathematical objects are genuine objects. This means that you are doing mathematics only if the objects you are dealing with are such that you have a criterion to distinguish the act of exhibiting them, from an act of defining them. To introduce mathematical objects then you have then to define in some way, and to fix the conditions under which such objects are exhibited.

These conditions should specify whether what is exhibited is a definite object or a certain sort of objects. Both in the former and in the latter case, the act of definition and the act of fixing the conditions of exhibition are two very different acts. The second of them must not be confused, however, with an act of exhibition—since to fix the conditions of exhibition of some objects is not the same as exhibiting them. This means that, with respect to genuine objects, and thus to mathematical ones, we have to distinguish between three sorts of acts: the acts of definition, the acts of fixing the conditions of exhibition—which I shall call, for short, “acts of characterisation”—and the acts of exhibition. I assume that we can distinguish different kinds of genuine objects by distinguishing different modalities of realising these three sorts of acts.

Mathematical objects are just a particular kind of genuine objects. Of course, they are not empirical genuine objects. That is to say, the acts of exhibition of them do not consist in ostensive acts directed towards some contents which are spatio-temporally delimited and which consist of the objects that are exhibited as such. This last clause is essential, I think, since genuine non-empirical objects, and particularly mathematical ones, can be exhibited by exhibiting empirical objects that represent or express them in a suitable way. I shall come back on this point later.

I.2. *Theorems*

According to my previous definitions, to have an object or a sort of objets it is sufficient to have some assertibility conditions for a statement assigning some property to something. One could argue that this in not a necessary condition (since there could be objects of which no one is speaking), but it is certainly a condition that is satisfied by objects which have been defined. Any statement "$P(a)$" entering a definition of a or of the a's is in fact an assertible statement about a, or the a's. If a or the a's are also genuine objects, any statement "$Q(a)$" describing their conditions of exhibition is also an assertible statement about them. Let us suppose that the statement "$R(a)$" is the conjunction of all the statements about a or the a's, which are assertible because they enter a definition of them, or describe their conditions of exhibition. Let us suppose that, because of the nature of the complex property R, this statement implies some other statement "$S(a)$", in such a way that there is no need to exhibit a or the a's, or to assure that such an act of exhibition is possible, in order to conclude that such a statement is assertible[2]. This means that just like the statement "$R(a)$", the statement "S(a)" is an assertible statement about a or the a's, and that its assertibility does not depend neither on an act of exhibition, nor on the possibility of such an act. I call the statements of this kind "tautologies about a, or the a's". Moreover, I call "logical" the (genuine or fictional) objects which are such that any assertible statement about them is a tautology about them.

My previous definition of an act of exhibition does not guarantee that when certain genuine objects have been exhibited it is always possible to look at them in order to discover some properties of them that are not ascribed to them by a tautology about them. If you think, at first glance, that there is no problem in supposing that this is always possible, it is probably because of a shifting in the meaning of the term "exhibition". If you think of an exhibition as a presentation to our senses of an empirical object, you are certainly ready to admit that, once the exhibition is realised, it is possible to look at the object which has been exhibited in order to discover same properties of it which are not ascribed to it by a tautology about it. Nevertheless, nothing in my previous definition of an act of exhibition assures us that an act of exhibition is always similar, under such a respect, to an act of exhibition of an empirical object.

This suggests a new distinction. I call "authentic" the genuine objects which are such that their exhibition provides a content that can be considered as a source to ascribe some properties to these objects that are not ascribed to them by a tautology about them. This is a condition which concerns the nature of the acts of exhibition of genuine objects. But there is no general rule which teaches us how to satisfy it in general. This is a question concerned with the edification of any theory which aims to deal with authentic objects.

I claim that mathematical objects are genuine, non-logical, authentic and non-empirical objects.

This makes me able to formulate a more precise definition for mathematical proofs: mathematical proofs are successions of acts that the members of a community could in principle repeat, and that they accept as warrants for asserting a certain statement "$T(a)$"—where "a" is a proper or common name of mathematical objects—which is not a tautology about a or the a's; such a statement "$T(a)$" has been proved if and only if such a

[2] According to the terminology, I have introduced in Panza, 1997*a*, we should qualify the acts for which we pass from the assertion of $R(a)$ to the assertion of $S(a)$ as "analytic acts of reasoning."

succession of acts has been realised; if this is the case, it is called “mathematical theorem”.

I.3. *Proof of Existence, Proof of Non-Existence and Predicative Proofs*

A very natural problem concerning genuine objects, and particularly mathematical ones, is the problem of their existence. To assert that certain mathematical objects exist is the same as asserting that they can be exhibited, *i. e.* their conditions of exhibition can be satisfied by certain acts. If we understand the existence of mathematical objects in such a way, to say that certain mathematical objects exist, or even that they do not exist, is the same as ascribing a particular property to them. The existence and also the non existence of certain mathematical objects are thus two predicates of these objects, and a statement ascribing one of these predicates to these objects is a non-tautological statement about them.

The most natural and obvious way to prove that certain mathematical objects exist is to exhibit them, to realise the acts prescribed by their characterisation. This is not, however, the only way to prove that mathematical objects exist. It is also possible to realise a succession of acts which is accepted as a warrant to assert that it might be possible, in principle, to exhibit these objects. Moreover, there is no other way to prove that certain mathematical objects do not exist than to realise a succession of acts which is accepted as a warrant for asserting that it is not possible to exhibit them. I do not think however that there is something as a general criterion of possibility that can be used as a guideline for these sorts of proofs. To fix these conditions of possibility (or non possibility) is a matter for any particular mathematical theory.

When certain mathematical objects have been proved to exist by exhibiting them, it is possible to look at them—*i. e.* to the content provided by the act of their exhibition—in order to prove that they enjoy some properties that are not ascribed to them by a tautology about them.

Imagine that a certain object *a*, or a certain sort of objects, let us say the *a*’s, have been defined and characterised, and that their existence has been proved, without their having been actually exhibited. It is possible that the definition of *a* or the *a*’s assures us that *a* or the *a*’s are objects of a certain sort. Let us say that they are some *b*’s. Imagine also that we are able to exhibit the *b*’s. We can then suppose that *a* or the *a*’s have been exhibited in their turn, and work on them as if they were particular *b*’s, that is some *b*’s that are different from any other *b* because of the particular properties which are ascribed to them by their definition. This is just what mathematicians called, for many centuries after Aristotle, “analysis” (cf. Panza, 1997*b*). By working in such a way, we can also discover some properties of *a* or of the *a*’s which are not ascribed to them by a tautology about them.

To prove some mathematical theorems about certain objects whose existence has been already proved is the aim of another sort of mathematical proofs which we could call “predicative (mathematical) proofs”. By means of these proofs we pass from the (effective or supposed) exhibition of certain mathematical objects to the ascription to them of certain properties, that are not ascribed to them by a tautology about them.

II. The Nature of Mathematical Proofs

I take mathematics to be an activity of communication: an intellectual activity of individual subjects who communicate with each other by means of different kind of systems of signs, essentially by auditive and graphical signs.

With the term “sign”, I mean a class of equivalence of empirical objects. It is not possible to define in general the relation of equivalence on empirical objects which produces a sign. It depends on the particular functions of this sign. What is clear is that two distinct empirical objects belong to the same sign if and only if they are considered as two distinct occurrences of it in space and time.

In order to be an occurrence of a sign, an empirical object has to be invested, by a community of subjects (eventually by only one subject) with at least one of the three following functions: the function to be a symbol, the function to be an icon, and the function to be an index. An empirical object is a symbol when it is taken as the expression or the representation of something else. It is an icon when it is recognised as an example of a certain form. Finally, it is an index when it is distinguished from any other empirical object because of its position in space end time. Of course, this is not due to the intrinsic nature of such an object. What makes of an empirical object a symbol, an icon or an index is exactly the intentional attitude that the members of a community have towards this object. This attitude has to be the same when two distinct empirical objects are taken as two distinct occurrences of the same sign in space and time. Thus, these three functions are also distinct functions of a sign. And no class of equivalence of empirical objects can be considered as a sign if no one of these functions is ascribed to it.

I will come back later on the distinction between these three functions of a sign. The previous short remarks should be sufficient for now.

Between the systems of signs which enter mathematics, languages, and specially natural languages, play an essential role. We would need too weak a definition of language in order to assume that the systems of signs that are used in mathematics are nothing but suitable sorts of languages. Nevertheless, no matter how we define them, languages and specially natural languages—supplemented with suitable stocks of technical terms—do enter mathematics in order to formulate its definitions and theorems. Thus, the other systems of signs that enter mathematics have to be connected with natural languages. A statement in a natural language can be understood as a suitable configuration of signs belonging to such a language. Hence, such a connection should consist in the possibility of going from a certain statement in a natural language to a certain configuration of signs of another system of signs, and *vice versa*.

This possibility to pass from a system of signs to another one—possibly from a language to another language—seems to me a condition of possibility of a mathematical proof. I shall clarify this point later.

As a part of mathematics, a mathematical proof is also an activity of communication. It consists in a succession of elementary acts, taking place in time. These acts are structurally similar to each other, one of them differing from another both in respect to the time in which it is realised and in respect to its particular content. At a certain time t_i a certain configuration of signs is given, a subject considers it and reacts to it by producing another configuration of signs. This new configuration of signs is thus given at a time t_{i+1}, where the same subject or another one considers it and reacts to it by producing a new configuration of signs. A proof consists in a succession of acts like these.

II.1. *Mathematical Proofs and Formal Deductions*

A simple example of such a succession of acts is formal deduction, that is nothing but the activity consisting in producing a syntactical chain or, more generally, a chain of formulas belonging to a certain formal system. My point is just that we could describe a mathematical proof as a sort of generalised formal deduction.

This is no news. Nevertheless, this idea can take very different forms according to the way the generalisation is understood. Normally, people think that a generalised formal deduction is something like a weakened deduction, where some formal inferences are substituted by “intuitive” or “conceptual” arguments, or even expunged, leaving a gap that can be fulfilled thanks to something such as practical ability. This is not at all my point. I have never understood what these “non formal” arguments should be, in general. I even suspect that to speak of them is nothing but a way to hide a local difficulty in reducing a real mathematical proof to a formal deduction. This difficulty is obvious to anyone who is familiar with the history of mathematics, but it cannot be solved simply by supposing other sorts of inferences whose logical nature is necessarily ambiguous.

This is not the only problem related with the conception of a mathematical proof as a weakened formal deduction, however. Another one is concerned with the understanding of a formal deduction. From my point of view, we should not confuse a formal deduction, that is a succession of acts, with the chain of formulas it produces. A problem with this understanding of a formal deduction as a chain of formulas of a formal system is that it hides the role and nature of the rules of inferences.

Imagine that f_1, f_2 and $\#(f_1,f_2)$ are formulas of a formal system **S**. To pass, in this system, from the chain of formulas

$$\begin{array}{l} \#\left(f_1,f_2\right) \\ f_1 \end{array} \tag{1}$$

to the formula

$$f_2 \tag{2}$$

we should refer to a rule of inference like the following

$$\frac{\begin{array}{l} \#\left(X,Y\right) \\ X \end{array}}{Y} \tag{3}$$

If R is the name of this rule, we could express this by writing

$$\begin{array}{ll} \#\left(f_1,f_2\right) & \\ f_1 & \\ f_2 & [R:\ \#\left(f_1,f_2\right)\ ;f_1] \end{array}$$

In this way we hide the essential point, however. We hide that the rule (3) allows to pass from the chain (1) of formulas of **S** to the formula (2) of **S** only if it is neither a formula of **S**, nor merely a meaningless formula. This is quite clear when you observe that we can make appeal to (3) to pass from (1) to (2) only if in (3) the symbols “X” and “Y” refer to any formula of **S** and the symbol “#(–,–)” refers to a formula like #(–,–) of **S**. Thus, either

(3) does not enter the previous inference or this inference cannot be expressed merely as a chain of formulas of **S** or, more generally, as a chain of meaningless formulas.

This remark seems to me quite obvious. It amounts to nothing more than the observation that the rules of inference which govern the passage from a certain chain of formulas of a formal system **S** to another formula of **S** in a formal deduction are essentially different from any formulas of **S**. But no formal deduction is possible without rules of inference. Thus, no chain of formulas of a formal system should be confused with the formal deduction itself. I will call any formula which enters a chain of formulas of a formal system produced along such a formal deduction an “output” of such a formal deduction.

If I am right, we cannot describe a formal deduction simply by listing its outputs in due order. Rather, we should indicate how these formulas are obtained according to some rules of inference, and we cannot do that without considering at least two systems of signs, one of which is necessarily not a purely formal system, since its formulas refer to the formulas of the other system. The latter elementary formal deduction of the formula f_2 of **S** should, for example, be described in the following way:

1) Both the configuration (1) of formulas of **S** and the configuration (3) of formulas of another system **S'**—whose formulas refer to the formula of **S**—are presented;

2) A subject recognises in the configuration (1) a particular instance of the two first lines of the configuration (3), which he judges to be a rule of inference;

3) He recognises in the formula f_2 of **S** the appropriate instance of the formula Y of **S'**, as it appears in the third line of the configuration (3), when the first two lines of it are taken as instantiated by the configuration (1);

4) He utters the formula f_2 of **S**.

Of course, this is a description of an elementary formal deduction as a human activity. We can construct machines that are able to carry out such a formal deduction too. But this is possible only as long as these machines are able to react to some internal state of them in a convenient way. This is just the constitutive principle of a Turing machine. Thus, even when it is carried out by a machine, a formal deduction is not simply a question of listing formulas of a formal system. It needs an interaction between two systems of signs, one of which is referring to the other.

When I speak of generalised formal deduction, I mean formal deduction as a human activity. The generalisation I am thinking of consists in admitting the possibility that: *i*) different systems of signs—possibly other that formal systems or systems of formulas referring to the formulas of a formal system—enter the procedure; and *ii*) the function fulfilled by rules of inference in a formal deduction could be also fulfilled by other sorts of instructions expressed in different possible ways by suitable configurations of signs produced along the procedure itself. To say that a mathematical proof can be described as a generalised formal deduction is thus to say that it can be understood as a orderly procedure consisting in producing certain configurations of signs belonging to different systems of signs, some of which refer to other ones of them. This is a very general description, however, which needs some refinements.

In order to be a mathematical proof, a orderly procedure of this type must be applied in order to ascribe certain properties to certain mathematical objects. Customarily, this ascription is expressed by means of a statement belonging to a natural language. This procedure provides then a warrant for the assertibility of this statement. I shall consider only this customary case, here.

II.2. *An Example: the Proof of the Commutativity of Addition in Peano's Arithmetic*

Let us start with a simple example: the proof of the commutativity of addition on natural numbers in Peano's arithmetic. This is a real proof that mathematicians generally understand as a formal one, because it depends essentially on formal deduction. This does not mean, however, that this proof consists merely in a formal deduction.

This is a proof of a property of addition defined in Peano's arithmetic. This is not the same as arguing that this is a proof of a property of natural numbers, or even of the addition between natural numbers. Strictly speaking, there is no mathematical object like the natural numbers, but only distinct systems of mathematical objects, responding to different definitions, which we recognise as structurally equivalent, and we call by the same name. And any proof we consider as concerned with natural numbers, is in fact concerned with one of these systems (and not all of them). This is to say that it is internal to a particular arithmetical theory.

The term “theory” is generally used in mathematics in a very weak sense to refer to very different sorts of things. Later, I will try to assign a precise meaning to this term. Provisionally, I use it to refer to something as a limited context where certain orderly procedures of the sort I have just described (rather than other ones) count as mathematical proofs. Thus, when I speak of the proof of the commutativity of addition on natural numbers in Peano's arithmetic, I refer strictly to a property of an operation defined in a precise way with respect to a domain of objects called “Peano natural numbers” implicitly defined by Peano's axioms. Here, I assume that these objects are defined and we have proved that they exist, in my previous sens. For short, I will refer to them, in the presentation of my example, simply with the name “natural numbers”. I also suppose we have defined the internal operations on the elements of a certain set. My example is just concerned with a particular internal operation on the elements of a certain set, i. e. the addition on natural numbers, and with the proof of its commutativity.

The definition I shall consider is the following:

> **Def.** If n et m are two natural numbers, the addition is the internal operation $+ \colon \mathrm{N}^2 \rightarrow \mathrm{N}$ on the elements of the set N such that:
>
> $$n + 0 = n$$
> $$n + m' = (n + m)'$$

(remember that in Peano's Arithmetic, if x is a natural number, then x' is its successor).

To prove that the addition on natural numbers exists, we have to prove that these two conditions are not contradictory and that they are sufficient to associate any (ordinate) pair of natural numbers with one and only one natural number, which is called the “sum of them”. This is easy to do and I shall consider that it has been done too.

In order to prove that such an operation is commutative we have to prove that if n and m are natural numbers, then

$$n + m = n + m \tag{4}$$

* * *

Before considering this proof, let me make a short digression. It could be argued that in order to prove the commutativity of addition on natural numbers we should rather prove that *for all* natural numbers n and m, we have (4). Of course, we may chose to formulate the property of commutativity of the addition on natural numbers in such a way, but I claim that the latter formulation of such a property is a good one if and only if we interpret it as a reformulation of the former one.

This depends on the fact that mathematics is an explanatory and not a descriptive science. In a descriptive science, we deal with objects which are already given to us. To define the objects of such a science is nothing but to suggest a possible classification of these given objects. So, when we assert in a descriptive science that all the a's are P, either we are definying the a's, by supposing that the property P enters a criterion of classification, or we are asserting that the domain of the given objects is such that all the particular a's are P. In the later case, nothing but an account of the properties of all the a's, taken one by one, could support our assertion. By contrast, in an explanatory science, we deal with objects we have generated, and the definition of these objects is part of the generation of them. Thus, when we assert that all the a's are P, either we are defining or characterising the a's, or we are claiming that the sort of objects we call "a" is a sort of objects which are P. In this second case, we may support our assertion by a proof bearing not on all the different a's but on the a's as a sort of objects. If the a's are infinitely many, there is no other way to prove our assertion. In such a case, what we prove is nothing but an implication: to be an a is a sufficient condition in order to be P. In formal logic this is generally understood as an open formula like "if x is an a, then it is P". In transforming this open formula in a closed one, by adding a universal quantifier, we merely adopt a *façon de parler* we judge more convenient. Nevertheless, this *façon de parler* hides the logical nature of our proof and potentially deceives us, by suggesting that our assertion is an infinitary one and needs something as an infinitary proof. But there is nothing like an infinitary proof in mathematics, nor is there any need of it[3].`

* * *

Let us come back to our example. You could think that (4) is nothing but a formula of a formal system, and thus argue that in order to prove our theorem we have simply to produce this formula as an issue of a formal deduction whose outputs are formulas of such a formal system. This is not right, however. If (4) were nothing but a formula of a formal system, it would not make any sense to assert that we have to prove *that* (4). At most we could say that we have to prove (4). The propositional form of our statement suggests that (4) is not taken here as a formula of a formal system. It is rather a statement written in a convenient language, where it claims that the sum of n and m is also the sum of m and n. This is obviously also the case for the equalities which appear in the

[3] Taking this conclusion together with my previous interpretation of a mathematical theorem of existence, we should conclude that when formulas like $\forall x(\mathcal{A}_x)$ and $\exists x(\mathcal{A}_x)$ (where $\mathcal{A}_x$ is an open formula with respect to x) refer to mathematical statements, they are equivalent to singular formulas and express non quantified statements. For example, if we use the symbol "$\equiv$" to indicate the relation of identity, and the symbol "E" to indicate the predicate "to exist", and suppose that y runs over the range Y, then a formula like $\exists x \forall y R(x, y)$ is equivalent to a formula like $\{(x \equiv a) \Leftrightarrow [(y \in Y) \Rightarrow R(x, y)]\} \wedge E(a)$, whereas a formula like $\forall x \exists y R(x, y)$ is equivalent to a formula like $(y \in Y) \Rightarrow \{[(x \equiv a) \Leftrightarrow R(x, y)] \wedge E(a)\}$. The quantifier $\forall$ and $\exists$ play thus no essential role in mathematics. I have made the same point with respect to universal formulas referring to statements of empirical explanatory sciences in Panza (1988).

definition of the addition: they are statements claiming respectively that the sum of n and 0 is n and the sum of n and the successor of m is the successor of the sum of n and m. The fact that these statements can be expressed by formulas of a formal system is essential for our proof, however. It gives us the possibility to prove our theorem by means of some formal deduction.

I shall consider here to possible versions of this proof.

In the first version, we associate to the statements which appear in the definition of the addition two formulas of a suitable formal system, let us say **S**, and try to deduce formally from these formulas the formula of **S** that is associated in the same way to (4)[4]. In order to do that, we need to associate to the fifth axiom of Peano a rule of inference like the following (as it can be written in a formal system **S'** whose formulas refer to the formulas of **S**):

$$\frac{\begin{array}{l}F(0)\\ F(X)\Rightarrow F(X')\end{array}}{F(X)} \tag{5}$$

Here “$F(0)$”, “$F(X)$” and “$F(X')$” refer to different formulas of **S** which have been associated respectively to three statements claiming that 0, any natural number and its successor enjoy the same property. The formula “$F(\mathrm{X}) \Rightarrow F(X')$” refers to a chain of formulas of **S** constituting the outputs of a formal deduction, whose starting and final formulas are respectively $F(\mathrm{X})$ and $F(X')$.

Once this rule has been accepted and it has been applied to get the formula of **S** associated to the statement (4), as the final output of a convenient formal deduction, the proof of our theorem is not finished yet. We have also to pass from this formula to the statement (4), that is, we have to understand this meaningless formula of **S** as a warrant to assert this statement. And this is no more a question of formal deduction. An output of a formal deduction has here to be taken as a warrant to assert a statement of a language that is essentially different from the formal system to which such a formula belongs.

Let us consider now the second version of the proof. Instead of looking for a formal deduction of a formula of **S** corresponding to (4), and use the rule (5) as a rule of inference applied along this formal deduction, we can interpret the fifth axiom of Peano as an instruction to organise and conclude our proof: “if you have proved that 0 enjoys a certain property and you have proved that when a natural number enjoys the same property, then also its successor enjoys this property, then you my claim that it is sufficient to be a natural number in order to enjoy this property.” According to such an instruction, we first prove that if m is a natural number, then: $0 + m = m + 0$. Afterwards, we prove that if n and m are two natural numbers then: if $n + m = m + n$, then $n' + m = m + n'$. Finally, we assert that if n and m are two natural numbers, then: $n + m = m + n$, so that the addition on natural numbers is commutative. Each one of these two preliminary sub-proofs may make use of a formal deduction of a formula of a formal system **S**, but the proof of the theorem, taken as a whole, composes these formal deductions using an instruction that is not, as such, a rule of inference expressed in a formal system **S’** whose formulas refer to the formulas (or the chains of formulas) of **S**.

[4] The fact that usually we write the formulas of **S** exactly in the same way in which we write the corresponding statements should not make us conflate them.

Thus, this second version of our proof, taken as a whole, is essentially different from the first one.

The two versions have, however, a common structure. A final output of some formal deduction provides an evidence that is considered as a warrant to assert a statement claiming that a mathematical object enjoys a certain property. The first version makes use of only one formal deduction and its final output provides a warrant to assert the theorem we had to prove. The second version makes use of two formal deductions whose final outputs provide respectively a warrant to assert two statements which, when they are token together, provide in their turn a warrant to assert the final theorem.

II.3. *A common structure for mathematical proofs*

My claim is that this common structure is just a particular instance of a more general structure that is the common structure of any mathematical proof.

A statement is a configuration of signs and a formal deduction is a succession of acts each of which produces a configuration of signs as a reaction to the presentation of another configuration of signs. Thus, I argue that a mathematical proof is a suitable composition of different elementary acts which have always the same structure: a certain configuration of signs provides an evidence that is considered as a warrant to produce a new configuration of signs. A formal deduction is nothing but a possible form that a sub-succession of these acts can take in some cases. However, no mathematical proof can entirely consist of a formal deduction, since the outputs of a formal deduction are nothing but meaningless formulas, and no meaningless formula can claim that certain mathematical objects enjoy a certain property.

To make such a description more precise, we have to clarify at least two points: What is a configuration of signs? What does it means that a certain configuration of signs provides an evidence that is considered as a warrant to produce a new configuration of signs?

II.4. *Configurations of Signs*

I have already said that I call “sign” a class of equivalence of empirical objects which are invested with three sorts of functions. A configuration of signs is a collection of signs arranged in a certain spatio-temporal order. Thus, it is a class of equivalence of ordered collections of empirical objects. The particular functions that the signs entering a collection of signs fulfil depend on their order in such a collection of signs. However, the particular nature of the empirical objects which belong to a certain sign, that is the internal feature of this sign, is not irrelevant for the functions that this sign fulfils.

An example will clarify this point. Let us go back to the elementary formal deduction we have considered above, and in particular to the rule of inference (3). When we say that (3) is a rule of inference, we do not refer of course to the configuration of graphical marks that you can find above, on the sheets of paper you hold in your hands, at the left of a graphical mark like “(3)”. Nor do we refer simply to the class of equivalence of the configurations of graphical marks we recognises as the same sign, to which both the graphical marks I have typed above and those you can now find on one of the sheets of paper you hold in your hands belong. However, I had and I have no other way to indicate this rule of inference without producing a configuration of graphical or vocal marks we are able to recognise as occurrences of a configuration of signs. This configuration of signs is not a rule of inference, but it refers to a rule of inference. Thus it is a symbol, in its turn. Take now the elementary components of this configuration of signs, for example the sign “*X*” which occurs in the first line of this configuration of signs. It refers to some formulas of the formal system **S**. Thus, it is a symbol too. This is also the case for the sign “*X*”, which occurs in the second line of this configuration of signs. At a first glance, one could think that these are not, actually, two distinct signs, being rather two distinct occurrences of the same sign. This is not the case, however. In fact, it is easy to understand that between these two “*X*”'s there is not the same relation that holds between two distinct graphical marks which are taken as distinct occurrences of the same sign. The fact that in our configuration of signs two distinct signs like “*X*” occur in distinct places is essential to make this configuration of signs able to refer to a certain rule of inference. These signs are thus two distinct indexes, because of their place in the configuration of signs to which they belong. Consider now the relation between these two distinct signs. As symbols, they are supposed to refer to the same formula of **S**, and we understand they do that, just because they are equivalent in some way. It means that the referential equivalence we assign to them depend on a previous equivalence of them. This original equivalence concerns the internal feature of these signs that makes possible for them to be considered as two different examples of the same graphical form. Thus, these signs are icons (each of them being an icon representation of the other) both because of their internal feature, and because they are two distinct signs belonging to the same configuration of signs.

To produce a sign, we have to produce an empirical object, usually a graphical or a vocal mark. Thus, if I am right in my description of a mathematical proof, there is no mathematical proof without production of empirical objects, like graphical or vocal marks, that are understood as occurrences of signs.

Of course, mental proofs are possible, but they are nothing but proofs where signs are imagined rather than effectively produced.

II.5. *A Configuration of Signs as an Evidence*

Taken as such, this is a very weak condition for a mathematical proof. To pass from such a very general necessary condition to a more precise description of a mathematical proof, we have to answer the second of the previous questions, namely: what does it means that a certain configuration of signs provides an evidence that is considered as a warrant to produce a new configuration of signs?

When I speak of an evidence that is considered as a warrant to produce a certain configuration of signs, I use the term “evidence” in a way similar to the one in which it is used, for example, in logicist theory of probability, where it designates the information we have when we assign a certain value of probability to a certain event. I use this term to designate the configuration of signs to which a subject reacts, along a mathematical proof, by producing a new configuration of signs. The act of producing a new configuration of signs works as a sort of operator which applies to a certain configuration of signs and produces a new configuration of signs. I take the first one of these configurations of signs as an evidence for the act which produces the second one. Such an act is thus a complex act, a part of which consists in constituting the evidence for it. The constitution of such an evidence is in its turn a complex act. A part of this act consists in delimiting a certain configuration of empirical objects in the larger domain of available empirical data. Another part of it consists in recognising a certain configuration of signs in such a configuration of empirical objects. When the evidence has been constituted, it appears as an indisputable datum. This indisputable datum is manifested by means of empirical objects, but it is not as such an empirical datum. The act of constitution of the evidence selects certain empirical data and transforms them into a configuration of signs, by ascribing to them the functions of indisputable symbols, icons or indexes.

Even if, once the evidence has been constituted, it appears as an indisputable datum, there is no way to be sure that it has been correctly constituted, since no general standard of correctness is available for the act of its constitution. An evidence presents itself as a source of certainty the constitution of which depends on some capabilities shared by the members of a community. A part of these capabilities consists in recognising the particular situation in which one is operating, i. e. the mathematical context where the proof takes place (later, I shall describe this context as a mathematical theory).

This last point is very important within my argument, and I will illustrate it with an example that I will also use to introduce the next point in my description of a mathematical proof. The example I refer to is Hilbert's contentual elementary arithmetic (cf. Hilbert, 1922 and 1926).

According to Hilbert, contentual arithmetic is a mathematical theory where mathematical objects are directly exhibited (without using any sort of axioms). These objects are integer positive numbers and they consist of the “signs [*Zeichens*]”: 1, 1+1, 1+1+1, etc., “whose form we are generally and surely able to recognise independently of the place and time of their production and of the minor differences in their execution” (cf. Hilbert, 1922, p. 163). It is not perfectly clear what Hilbert means with the term “sign [*Zeichen*]”, to which sometimes he adds the adjective “concrete [*konkrete*]”. I will come back to this point later. Firstly, I will present Hilbert's proof of the commutativity of addition on integer positive numbers, by using this term in quotation marks, in order to indicate that it has to be understood in Hilbert's sense, whatever this sense is.

Suppose (cf. Hilbert 1922, 164) that n and m are two distinct numbers (to avoid cumbersome formulations, I will use, in my discussion of Hilbert’s proof, the term “number”, without any adjective, instead of the complex term “integer positive number”). It means that they are two distinct “signs” like 1+1+…+1. It is then clear that one of them contains the other one as a part of it. Suppose that m contains n, then there will be a number h (that is a “sign” like 1+1+…+1) such that $m = n + h$. Hence, instead of $n + m = m + n$ we can write $n + n + h = n + h + n$ which is equivalent to $n + h = h + n$. It follows that $n + m = m + n$ if and only if $n + h = h + n$, where h is less than m. By continuing in this way, we will arrive to reduce the statement $n + m = m + n$ to a statement like $k + k = k + k$, which claims that a certain “sign” like 1+1+…+1 (that is a certain number) is equal to itself.

According to Hilbert, this is a contentual proof of the commutativity of addition on numbers. And because it is contentual, this proof is sure.

A serious argument against Hilbert's pretension has been advanced by P. Kitcher (cf. Kitcher 1976). Hilbert's proof bears essentially on collections of strokes; it proves the community of addition on numbers as a consequence of the proprieties of the collections of strokes. But, how is this possible? Kitcher (cf. *ibid.*, 107) considers two plausible answers: *i*) these collections of strokes “are the numbers”; *ii*) they “are accurate representations of the numbers.” The problem is that neither (*i*), nor (*ii*) seem to be good answers to Kitcher. The difficulty with (*i*) is obvious, when this answer is understood literally; the difficulty with (*ii*) would be, according to him, that this answer implies that we have an independent access to numbers in order to judge that the representation is accurate. I quote Kitcher (*ibid.*, 111):

> Let us suppose that we are coming to know some arithmetic by following the method which Hilbert takes to be basic. We draw some stroke-symbols in thought. Assume that we draw them in black. Are we entitled to infer from our inspection that all numbers are black? Presumably not. But why not? Perhaps the answer will be that we recognise that we could have drawn the symbols in some other colour and that they still would have been essentially the same symbols. Yet now we must ask how we recognise that the symbols are “essentially the same”. True enough, drawing the symbols in red does not affect their arithmetical properties—but our knowledge of this depends on our knowledge of arithmetic!

The difficulty is not solved by supposing, as Hilbert seems to do, that the members of a certain community are able to recognise different collections of strokes as different occurrences of the same “sign”, and that they are thus working not on strokes but on their forms, when they are doing contentual arithmetic. If we interpret Hilbert's argument in this way, we assign to the term “sign”, as used by him, a meaning that is not so far from the meaning I have previously ascribed to it. Now, it is quite legitimate to suppose that the members of a certain community are able to do that. And this seems to me also a condition of possibility not only for Hilbert's arithmetic, but of mathematics as a whole. But this supposition is not sufficient as such to solve Kithcher's problem. This problem concerns in fact neither the availability of a criterion of identity for Hilbert's signs, nor our ability to apply such a criterion in order to recognise such a sign. Rather, it concerns the justification of such a criterion.

This is exactly an example of the situation I have described in general. At any stage of Hilbert’s proof, we operate on an evidence we consider as an indisputable datum. Nevertheless, this evidence is constituted according to a competence or an ability which is just questioned by Kitcher’s argument.

By supposing (and recognising) that different graphical marks are distinct occurrences of the same signs, we recognise some common nature to them. But we do not grasp this common nature *a posteriori*, as a feature that all the elements of a given class of graphical marks partake. By contrast, we recognise in certain graphical marks a nature with which we are familiar *a priori*. When we work on these graphical marks, we are not really operating with them. We are operating with this common nature, that is just the nature of numbers. Hence, despite what Hilbert says, his signs (that are just classes of equivalence of collection of strokes) are not the numbers, but they represent them. Thus, we can suppose that we are proceeding contentually only if we suppose that the content on which we are operating is just this nature, that has to be given to us before the collection of strokes. The problem is thus clear: how is this common nature given to us?

Kitcher argues that our “knowledge” of this nature “depends on our knowledge of arithmetic.” This is not far from a correct description of the situation, I think. Nevertheless, despite Kitcher's worries, I do not see any problem in that. The point that seems to escape Kitcher and to many other scholars is that nobody begins to make mathematics with an empty mind. Of course, Hilbert knew arithmetic before conceiving his proof. And this is also the case for any other known mathematician which invented a new way to do arithmetic. Any one of these historical versions of arithmetic depends on a previous knowledge of arithmetic and simply aims to reformulate already known results in a new formal context. But, you could insist: what about the unknown first mathematicians which invented arithmetic *ex novo*? And, to stay closer to us, what about children that are learning arithmetic? The answer is not so different, I think. They also did not invent or do not learn mathematics with an empty mind. For them, mathematics was or is nothing but a way to answer to a previous problem. And they attained or attain it by realising what I understand as an objectivation of a previous concept.

Where so many philosophers have seen the necessity of a first mysterious intuition of an eternal object, I see, much more prosaically, the necessity of a previous piece of knowledge.

I do not know of any document describing the first invention of arithmetic and I do not know of an indisputable account of the way in which children learn arithmetic. But I can imagine a possible path bringing to it.

Suppose you are able to count. Obviously, this does not mean that you are able to repeat in your language the succession of the names of the numbers: one, two, three, four, etc. Rather, it means that you are able to operate on a given collection of objects in quite a standard way. First of all, you are able to fix the limits of this collection. Second, you are able to distinguish among them the elements of this collection. Third, you are able to remove, or to mark, or simply to consider these elements one after the other, in such a way that the elements you have already considered are not considered again. If you are able to do that, you are able to know whether two given collections (small enough) are “equinumerous”, that is, whether they are such that it is possible to associate any element of one of them with one and only one element of the other one, in such a way that no elements of the first collection is associated to an element of the second one to which another element of the first collection is also associated. You can perform this association by counting (in the previous sense) the two collections alternatively. You consider an element of one of them, then an element of the other one, then another element of the first collection, then another element of the second collection, and so on. If the two collections are exhausted at the same stage, then the one-to-one association of their elements is possible, otherwise it is not.

In so doing, you are in no way doing arithmetic, you are only solving, by using a standard procedure you have learnt quite well, a practical problem. Suppose now that you decide to look for a way to associate to any collection of objects you could consider a distinctive sign, in such a way that two collections are associated to the same sign if and only if they are equinumerous. You could start by fixing a suitable and ordered collection of different signs and to associate each of them to the different stages of the procedure of counting. Then, you can associate any collection with the sign associated to the stage in which you consider its last element. You will understand very soon that in this way you are able to compare with each other the signs you have associated to any collection, without coming back to the collection itself, only if the signs you have chosen are related to each other by suitable laws. You may choose these signs as you please. What is important is that you arrive to invent a way to work on these signs in order to know the relational properties of the collections of objects to which they are associated, without coming back to these collections themselves. It is just at this very moment that you begin to do arithmetic, even if your arithmetic is still a very elementary one.

What does this story teach us? It teaches us that arithmetic can be viewed as founded on some everyday acts, but that it is not an everyday practice, or an everyday experience. It just begins when we leave our everyday practices aside and construct a system of signs on which it is possible to operate in essentially a new way. This construction is guided both by a task and by a previous piece of knowledge about collections of objects. In my account, it is guided by the capacity to order these classes in classes of equinomerous classes. Thus, it is guided by the concept of equinoumerous classes. In my account, the apparently so mysterious nature of numbers is nothing but the properties of the signs 1+1+…+1 that make these signs able to represent the relational properties of the classes of equivalence of equinomerous collection of objects. What we recognise in the collection of stokes we judge as occurrences of these signs is nothing but the possibility to use them as representations of these classes. To be able to make such a recognition, we need some capabilities. These are exactly the capabilities we acquire by working on our original equinomerous collections of objects.

Of course, I am not so naive to believe that by telling the previous story, I have exposed the real nature of numbers. I have simply exposed a possible psychological origin of a pre-mathematical concept that we can associate retrospectively to numbers. Another scholar could imagine another one, and a good historian of ancient civilisations could explain both to me and to my colleague that the historical origins or arithmetic should be described in quite a different way. This means that there is nothing as *the* true nature of numbers that historians, philosophers, logicians or psychologists should discover. There are a large number of quite equivalent theories we call "arithmetic", and a large number of quite analogous possible reconstructions of the earlier (historical, logical or psychological) origins of them. Our task, however, is not as simple as that we can claim to have realised it once we have chosen one of these reconstructions, and taken it as disclosing us the original nature of numbers.

My story only aimed to illustrate with a simple example a general feature of mathematics: the generation of mathematical objects never takes place in a knowledge vacuum. In my example the previous piece of knowledge was not a mathematical one. But normally, in the history of mathematics, this is the case. The generation of mathematical objects depends on a previous piece of mathematical knowledge.

This apparently obvious remark seems to me to suggest an answer to a more general problem than the problem we started with. We started with the problem of the constitution of evidence. We have come to the problem of the generation of mathematical objects, of which the first problem is nothing but a small part. The two problems interact not only because one of them contains the other, but also because they have similar answers. My story suggests that both the constitution of the evidence and the generation of mathematical objects depend on a previous piece of knowledge, and that such a previous piece of knowledge is not simply contingent. It is just the piece of knowledge that motivates and makes possible the constitution of a mathematical theory. This is all we may say to answer these problems in general. If we want more accurate answers, we have to consider these problems not in general, but in particular, i. e. with respect to some particular evidence or same particular domain of mathematical objects. Then, the answers will be particular—i.e. historical—answers which will depend on the particular nature of the mathematical objects that will be in question.

II.6. *The Problem of the Justification of Mathematical Deduction*

What I have just called the problem of generation of mathematical objects is nothing but an aggregate of four different problems: the problem of the definition of mathematical objects; the problem of their characterisation; the problem of being sure of their existence; and, finally, the problem of the determination of their properties.

In the example of Hilbert's contentual arithmetic the first of these problems is solved in quite a simple way, by saying that 1 is a number and that “a sign which begins with 1 and ends with 1, in such a way that in the interval 1 is always followed by + and + by 1, is equally a number” (cf. Hilbert, 1922, 163). Hilbert should have added: nothing else is a number. As a matter of fact, this clause is implicit in his argument.

We have already seen that this definition cannot be taken literally. What Hilbert means is that a number is the common form (of any sort of equinumerous collections of objects) we can represent by a sign like “1+1+…+1”. The conditions of exhibition of a number are thus the conditions of exhibition of this form. If we suppose to be able to recognise a sign like this (and thus the form it represents) in any collection of strokes, then these conditions are simply satisfied by the exhibition of any collection of strokes. Once the signs like “1+1+…+1” are ordered in the natural way,

1
1+1
1+1+1
1+1+1+1
....

and it is taken for granted that we can add the sign “+1” to any sign like “1+1+…+1”, we have also proved the existence of the infinite set of numbers. We understand then why the problem of generation of numbers arose quite naturally from our discussion of the problem of the constitution of an evidence like “1+1+…+1”. To constitute such an evidence is *ipso facto*, to exhibit numbers, in the Hilbert sense.

After having proved that numbers exist, it is easy to define addition on them. Hilbert does it implicitly by supposing that the sum of any two numbers represented by the signs “n” and “m” is the number represented by the sign “$n + m$”, i. e.: $+(n, m) = n + m$. The existence of addition is also easy to prove, and Hilbert takes it for granted. As an example of possible contentual proofs on numbers, he proves, as we have seen above, its commutativity. Let us consider now this proof. It consists in arguing that for any pair of numbers n and m, it is possible to pass from the configuration of signs “$n + m = m + n$” to a certain configuration of signs “$k + k = k + k$”, where k is a convenient number.

Of course, the presence of the sign “=”radically changes the nature of these two last signs with respect to the previous ones. Each one of these two last signs represents a statement about addition and not a number. Nevertheless, Hilbert works on the symbol “=” as it were the representation of a well known form, too. Simply he supposes that this sign, combined with the signs “+”, “m”, “n”, “h” and “k” (or even with others signs likes “2”, “3”, …) comes in handy “for the communication of assertions [*die zur Mitteilung von Behauptungen*]” about the numbers (cf. Hilbert, 1922, p. 163). And he understands his proof as if it consisted merely in operating on numbers. This is possible only because of the presence of a previous piece of knowledge too. Nevertheless, it is clear that when the previous piece of knowledge that Hilbert takes for granted is actually shared by the members of a community, no doubt is possible about the correctness of Hilbert's proof.

This is also the essential point of Hilbert's well-known argument on foundation of mathematics.

The question is thus the following: what does it mean to say that Hilbert's proof is correct, and what is it the logical nature of our certainty about it? This is another form to state the problem I have posed above by asking: what does it means that a certain configuration of signs provides an evidence that is considered as a warrant to produce a new configuration of signs? If we accept that a mathematical proof is a particular sort of deduction, this is also a way to wonder what counts as a justification for mathematical deduction. This is the question I have to tackle now.

Once we understand a mathematical proof as a succession of human acts, the problem of its justification seems much less mysterious than when we understand a mathematical proof as a chain of sentences or statements which necessarily imply each others, and wonder how to justify such a necessity.

It is possible that the physical structure of human beings is such that certain acts necessarily go together with other acts, that is: the former always go together with the latter. For example, it is possible that we cannot sleep without dreaming, or eat without digesting, or think of a red rose (to be in the physical state corresponding to such a thought) without thinking of redness. But it is quite clear that we can produce any configuration of signs and then produce any other one. There is no intrinsic necessity that forces us to write “B” once we have written “$A \Rightarrow B$” and “A”. We could perfectly write A instead, or even “Zuruzuz”. Thus, the succession of the acts that constitutes a mathematical proof is in no way intrinsically necessary. At most it is necessary with respect to a certain task[5].

The necessity with respect to a task is a final necessity however, and has no causal efficacy. Thus, the problem we face when we have to pass from a certain stage in a mathematical proof to another one is a problem of choice, and we are always free to choose as we please. The question is thus the following: imagine that you face a certain evidence—that is a certain configuration of signs—and you have to choose a configuration of signs and produce it; how do you make your choice? We may suppose of course that between the infinity of configurations of signs you could produce, there are some that are selected, as being correct with respect to the given evidence. To say that a certain evidence is a warrant to produce a certain configuration of signs would thus be the same as saying that the latter configuration of signs belongs to the class of the correct ones with respect to the given evidence. A mathematical proof would consist thus in passing successively from given configurations of signs to other ones in such a way that any new configuration of signs is correct with respect to the evidence wich precedes it.

[5]You could argue that this is not the point and retort that even if there is of course no necessity which links the act of writing “$A \Rightarrow B$” and “A” together with the act of writing “B”, there is a necessity that makes that B is the case when A and $A \Rightarrow B$ are the case. If you think in this way, you should also think that a mathematical proof is not a succession of acts which ascribes a property to certain mathematical objects, but an argument that discloses an intrinsic property of these objects. This point of view is just opposite to my starting point. I do not reject it because I consider it as wrong. Simply, I do not understand what an intrinsic property of a mathematical object is, and I argue that no supposed explanation of mathematics which presupposes something as mysterious as an external domain of mathematical objects with some intrinsic properties we should disclose is actually an explanation. You can believe in that, as you believe in God, if you please, but you cannot pretend to have explained something simply by making appeal to such a personal belief.

This is an obvious description of the situation and of course, I agree with it. It does not answer the problem, however, since it does not tell us how the selection of correct configurations of signs with respect to a certain evidence is made.

To find an answer to such a question, let us go back to Hilbert's proof of the commutativity of addition on numbers. Suppose that the signs “m” and “n” did not refer to Hilbert's numbers, but to Peano's ones (that is, they refer to natural numbers, as they are defined in Peano's arithmetic). Then the passages from the evidence

$$m = n + h$$
$$n + m = m + n$$

to the configuration of signs

$$n + n + h = n + h + n$$

and, from here, to the other configuration of signs

$$n + h = h + n.$$

would be incorrect, unless we had already proved that addition is also associative. If you grasped the logic of Hilbert's argument you would understand easily that it would be senseless to make the same point with respect to such an argument. The reason is clear and depends on Hilbert's definition of numbers and addition on them. In Hilbert's arithmetic the sign “$x + y$”, where x and y are numbers refers to a number as such and not simply to a number taken as the sum of two others numbers, as it happens in Peano's arithmetic, instead. Thus the sign “$n + n + h$” is not ambiguous at all[6].

Of course, from this difference between Hilbert's and Peano's arithmetic we cannot conclude that Hilbert and Peano are using contradictory rules of inferences and applying thus two different logics. This would be a very odd way to describe the situation. But it would be very odd too to remark that Hilbert and Peano were not very precise in defining their formalism, since they did not distinguish with care between addition and sum. Of course, we can make this distinction explicit, but by the only fact the we can do it very easily, it follows that these accounts are not ambiguous at all, for any one who is familiar with mathematics. We could express the same point in another way: of course, we could transform both Hilbert's and Peano's real proofs in ideal proofs written in a convenient language of classical formal logic, but this does not explain why these proofs are correct. These proofs are not correct because they apply some general rule of classical logic, although they can be transformed in ideal proofs applying these general rules because they are correct.

This conclusion should not surprise us. Once a mathematical proof is described as a succession of acts and these acts are described as reactions to the occurrence of a particular situation (the presence of a certain evidence), it is obvious to conclude that there is no universal criterion for its correctness. The fact that a certain configuration of signs is correct with respect to a given evidence cannot depend only on universal rules. Suppose you have a set of universal rules you could apply in any situation. Again, you do not have a general rule which tells you which of them you have to apply in any particular case and how you have to do it. At most, you could have a general criterion for that, but in order to apply your criterion you have to judge the particular case in which you are, and this judgement cannot be but a particular one.

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[6] To understand the point, you have to observe that in Hilbert’s proof the signs “n” and “h” refer to other signs like “1+1+…+1”, so that the sign “$n + n + h$” refers to a sign like “1+1+…+1+1+1+…+1+1+1+…+1”, which is just a sign like “1+1+…+1”.

II.7. *The Local Character of a Mathematical Proofs*

What I want to suggest is that any step of a mathematical proof is correct because of a local criterion. This criterion can of course be informed by any sort of general principle, but it cannot simply consist of a number of general rules. It depends on the evaluation of the situation and in particular of the subject-matter of the proof.

Brouwer made essentially the same point in a number of very famous papers (cf. for example Brouwer, 1908, 1912 and 1923). Here it is how Detlefsen (cf. Detlefsen, 1992, p. 232) summarises Brouwer's point:

> Basically the idea is this: one’s knowledge of a mathematical truth *p* is *mathematical* to the extent that it is based on a “local” familiarity [...] with the mathematical subject(s) to which *p* belongs. This emphasis on the “local” character of mathematical knowledge seems to be but another way of putting Brouwer’s point concerning the “autonomy” of mathematical thought, which was that we ought to be careful to distinguish the connections between propositions which arise from the *linguistic representation of* mathematical reasoning from connections between propositions which characterise that reasoning itself.

This does not mean, of course, that we should not use language in order to conduct mathematical proofs. Brouwer's point is that linguistic formulations of our thoughts are connected together because of the content of these thought and not because of linguistic rules. No linguistic rule can be, according to Brouwer, a guideline for a mathematical inference.

Brouwer's view suffers from an obvious problem. For Brouwer also, mathematics is an activity, but it is an introspective activity whose guideline is a sort of intimate state (or sentiment) that he calls “intuition”. Thus, the autonomy of mathematics with respect to any sort of “linguistic logic” is for him the autonomy of intuition with respect to language. It is not clear at all what Brouwer actually means when he speaks of intuition as an intimate state, however. One might suspect that in speaking of intuition in such a way he is falling in the same mistake he ascribes to formalist mathematicians: to be led by an empty verbalism.

Nevertheless, it is not necessary to follow Brouwer in his very obscure description of our acts of reasoning in order to agree with him that mathematics can be described as an activity and it should not be reduced to the outcome of a certain logic. By contrast, a number of modern intuitionists seem to think that Brouwer’s ideas suggest a possible logic of mathematical proofs. An essential part of Brouwer's programme consisted in fact in evaluating the different principles of classical logic to check whether we can go astray, by following them instead of our intuition. His conclusion of such an inquiry is well known: we can rely on all the principles of classical logic except the principle of the excluded middle (and its consequences). This, however, is a very strange conclusion issued from a very strange programme for someone who argued for the “local character of mathematical knowledge”, however. But it is just from this conclusion that Brouwer's partisans, starting with Heyting, have constructed intuitionistic logic and intuitionistic theory of proof and understood them as “a kind of ‘algebra’ in the domain of intuitionistic constructions” (cf. Detlefsen, 1992, 235).

Detlefsen himself has proposed a very perspicuous argument showing that “the role of intuitionistic logic in intuitionistic mathematical reasoning is quite suspect” (cf. *ibid.*, p. 240). Consider the rule of conjunction-introduction as it is generally set forth in intuitionistic theory of proof: if χ_A is a proof of A, χ_B is a proof of B and $\chi = <\chi_A, \chi_B>$, then χ is a proof of A&B. Here is what Detlefsen writes (cf. *ibid.*, p. 237):

> The fact that one has a local proof of A and a local proof of B does not imply that one either does or can have a local proof of A&B [...]. This is particularly clear if A and B are drawn from different local settings, but it is to be generally expected even for A and B drawn from the same local setting, since the reasoning according to which the local proof of A and the local proof of B are to be bound together is not local but rather global reasoning. Hence, it is not by local insight that the two proofs are bound together, and this may be enough to deprive the compound proof of status as local reasoning.

Consider the inferential schema

$$\frac{\begin{array}{l} A^* = \gamma(A) \\ B^* = \gamma(B) \end{array}}{< A^*, B^* > = \gamma(A \,\&\, B)}$$

It seems clear that this schema does not apply in general. It seems to apply if A and B are statements and, for any statement X, $\gamma(X)$ is the proposition *that X is true*: by composing the proposition *that A is true* and the proposition *that B is true* we obtain the proposition *that A&B is true*. It does not apply if A and B are empirical contents and, for any empirical content X, $\gamma(X)$ is the proposition *that X is perceived by a certain subject* α: by composing the proposition *that A is perceived by* α and the proposition that *that B is perceived by* α we do not obtain the proposition *that A&B is perceived by* α. This should be also the case with intuition in the sense of Brouwer, whatever this sense were. By composing the proposition *that we have the intuition of A* and the proposition *that we have the intuition of B* we do not obtain the proposition *that we have the intuition of A&B*. Thus, the rule of conjunction-introduction can be accepted in an intuitionistic theory of proof as a rule of inference, only if it is supposed that to have the intuition of X is a sufficient but not a necessary condition in order to have a prove of X. Thus, to conclude that the rule of conjunction introduction does not foul us, we should rely on some general condition we consider as necessary in order to have a proof of something. Thus the problem arises quite naturally: what is this general condition?

Detlefsen mentions an alternative strategy in order to get a justification for the rule of conjunction-introduction, originally due to Dummet (cf. *ibid.*, pp. 238-239). We should admit that a proof of *A&B* is not something different from a proof of *A* and a proof of *B*, rather it is just a combination of these two proofs. According to such a point of view, we have a proof of *A&B* when we have a proof of *A* and a proof of *B* and we are able to consider them together. As Detlefsen remarks, this view does not change the situation, however. If you accept that it is not the same to have the proofs of *A* and *B* and being able to consider them together, you should also reject this rule as a rule of inference, since from the fact that we have a proof A^* of A and a proof B^* of B it does not follow that we have a proof $<A^*, B^*>$ of *A&B*. If you accept that to have a proof of *A&B* is just to have a proof of *A* and a proof of *B*, then you have to justify your view otherwise than by making appeal to Brouwer’s intuition.

The conclusion is obvious. If we accept that the source of certainty of a mathematical proof is nothing but intuition, we cannot admit that the rule of conjunction-introduction works as a guideline for a mathematical proof. At most, it can be helpful in our *a posteriori* description of a mathematical proof.

Suppose now that we discard Brouwer's conceptions and simply ask if an apparently certain rule such as conjunction-introduction can provide a general guideline to conduct a mathematical proof, when the latter is understood in the way I have proposed.

Suppose that you have both an evidence A^* that you consider as a warrant to produce a configuration of signs A and an evidence B^* that you consider as a warrant to produce a configuration of signs B. Do you have, then, also of an evidence $<A^*, B^*>$ that you have to consider as warrant to produce the configuration of signs $A\&B$? Detlefsen's argument suggests that the answer is "no". In fact, to arrive to such a conclusion, it is not necessary to argue that only intuition can provide a convenient warrant in mathematical reasoning, to accept that a proof is a sort of mathematical reasoning, and to add that intuition is an intimate and local state. It is enough to observe that to have an evidence A^* and to have an evidence B^* is not a sufficient condition in order to have a third evidence, whatever it is. For there is no general guarantee that assures that you are able to put A^* and B^* together.

Suppose now that you have the evidence A^* and the evidence B^* and you are able to put them together and you have than the evidence $<A^*, B^*>$. This does not mean that you are able to put together the configurations of signs A and B to form a new configuration of signs like $A\&B$. And, even if you are able to do that, you can do that in different ways. Thus, the configuration of signs $A\&B$ is not univocally given, according to a general rule, once the configurations of signs A and B are given. If A and B are formulas of a formal system which contains the logical constant &, the passage from A et B to $A\&B$ is merely a question of applying the rules to compose sentences in such a language. But, this is not the general case of an inference. Thus, with the only exception of formal deductions, the rule of conjunction-introduction, can be understood as a rule of inference, only if we assume to be able to infer $A\&B$ from A and B. And this rule should then tell us how to do it.

These are two different arguments to conclude that even the rule of conjunction-introduction cannot provide a general guideline to conduct a mathematical proof.

The second of these arguments can be repeated, *mutatis mutandis*, for the rule of *modus tollens* or any other rule concerned with negation. Even if we accept that the configurations of signs to which these principle are supposed to be applied are statements, so that the negation of them gets a precise sense, we should notice that there is no universal rule to decide what counts as the negation of a certain statement in any given situation. On this point, intuitionistic logic seems to provide a better description of the situation, however, by defining the negation of a statement A by means of the implication $A \Rightarrow \perp$, where $\perp$ is a contradiction. In fact, as long as it is taken as a primitive notion (that is, it is not defined in terms of negation), the notion of contradiction is necessarily internal to a local domain. Thus, this definition translates a strong idea: what, in a certain mathematical theory counts as a negation, depends on what counts here as a contradiction, and the latter depends on the specific subject-matter of the theory. Let us take a simple example: the conjunction of two statements "$a = b$" and "$a = c$", where the signs "b" and "c" denote two distinct objects, is a contradiction in arithmetic, if b and c are two distinct and thus different numbers, but it is not a contradiction in geometry, if b and c are for example two distinct segments.

The conclusion we can draw from these remarks is the following: we can obviously use the principles of a certain logic to describe *a posteriori* certain inferences in a mathematical proof, but this does not mean that these principles enter as such in this inference.

II.8. *Mathematical theories*

Once we have discarded the intimate authority of intuition and the outer authority of (some) logic as guidance for conducting a mathematical proof, should we conclude that mathematical theorems are nothing but arbitrary stipulations on which the members of a social community agree? Of course not. Where does the legitimacy or necessity of mathematics lie, then?

I have said above that the constitution of any mathematical theory is motivated and made possible by a previous piece of knowledge. But I have not said what I take a mathematical theory to be.

By speaking of a previous piece of knowledge I want to refer to a previous accepted description of a certain reality, that is a domain of objects with their properties and relations we take as given and known. As rich as it could be, it is possible that such a description of this reality is not a convenient one with respect to certain tasks. It is possible, for example, that it is rich enough to suggest some problems, but also too poor to provide us with a convenient answer to them. So, we need a richer description. There are several ways to get it. The first one is typical of naturalistic sciences. It consists in looking to our reality with a stronger microscope, in order to grasp some differences or similarities between the already given objects or to perceive new objects beyond or between the given ones. The second one is typical of explanatory sciences. It consists in generating new objects by passing trough the ascription of a new order to the objects, properties or relations of the given reality. Of course different explicative sciences differ on the basis of the nature of the objects they introduce. I have tried here to describe some specific features of mathematical objects. My point is now that the generation of these objects meets the exigencies of the task or the problem that motivates their introduction. In this sense it is an intentional act and these objects are intentional. A mathematical theory is just, in my terminology, the abstract space of the conditions which govern this generation. It can be described as a structure composed by a domain of objects (that is a system of definitions and characterisations) and a family of acts which count as inferential procedures that are accepted as means to establish the existence and the properties of these objects.

Now, my point is simply that a mathematical proof is always internal to a mathematical theory, that is: it is a particular succession of acts, between all the successions of possible acts that belong to it. This succession of acts is guided by two sorts of tasks. It is guided both by the general task that presides the generation of the theory where it takes place, and by a more particular task that motives our inquiry into the properties of certain mathematical objects. In this sense a mathematical proof can be understood as “a fulfilment of a mathematical intention” (cf. Tieszen, 1992, 59).

It is just because of this fact that Kitcher's problem about Hilbert's arithmetic is solved. When we operate on certain signs in our proofs, we only act according to certain possible procedures. Thus, if we infer nothing from the colour of our signs, it is just because no inference based on this colour is accepted as possible into the theory in which we are working: no possible procedure into the theory applies to a certain sign because of its colour.

You can object that this is only a way of displacing both Kitcher's problem and the problem of the justification of mathematical deduction, and you are right, in fact. But this displacement is essential, since it explains the sense in which we have "an account of the nature" (cf. Kitcher 1976, 108) of the objects on which we are working when we conduct a mathematical proof. This does not depend on a mysterious relation with them, due to a special sort of faculty. It simply depends on an essential character of mathematical activity: the fact that it takes place in the presence of a previous piece of knowledge and aims to the fulfilment of a certain task or the solution of a certain problem. A mathematical proof is just a procedure informed by this knowledge and guided by this task or this problem.

II.9. *Mathematical Proof and truth*

As I understand it, the intentional nature of mathematical objects explains the natural sympathy that mathematicians have for Platonism. They feel that these objects are there with their properties before they study them, and that the statements ascribing to them certain properties or relations just ascribe to these objects their properties and relation and they are thus true of them. The belief in the previous existence of mathematical objects is explained in my description as an equivocal account of the presence of a previous piece of knowledge that motives and guides the generation of a mathematical theory. I have already remarked that this piece of knowledge may both be mathematical or pre-mathematical .

In the case of Hilbert's contentual arithmetic, it is probably pre-mathematical, and possibly concerns any sort of collections of objects ordered in classes of equivalence under the relation of equinumerosity. If we understand these collections as objects of which it is possible to say that something is true or false, we should also be disposed to admit that the statement that describes their properties and relations are true of them. Thus, if we consider Hilbert's contentual arithmetic as a theory of signs which represent these collections, we should also be urged to consider its theorems as true statements about these signs.

The situation is quite different when the previous piece of knowledge is mathematical. This means that such knowledge consists of a number of theorems about a certain domain of mathematical objects. If we accept the standard internalist view about knowledge, this is possible only if these theorems are taken as true statements about these objects. Suppose we are ready to admit that the statements that form the previous piece of knowledge that motivates and guides a certain mathematical theory are true in some sense. And suppose also that we are ready to admit that this is a sufficient condition for the theorems that are proved within this theory to be true in a plausible sense, too. Then, we could argue that any mathematical theorem is true because of the truth of some original statement that enters something as an original knowledge. Thus, we could speak without any trouble of mathematical knowledge and even agree with the Platonist account of mathematics on an essential point. This is quite a attractive strategy and it is, moreover, similar to other strategies that have been used to support different sorts of reductionist programmes in philosophy of mathematics (cf., for example, Kitcher, 1983; Resnik 1981-1982, 1992 or 1997; and Tieszen, 1989). Unfortunately, no plausible account is available for this transmission of truth from basic levels to higher ones in mathematical theory, and it seems also questionable to me that mathematical theories could be ordered in such a hierarchic structure.

Should we conclude that a Platonist is wrong when he argues that there are mathematical truths (and theorems some of them)? Or that we can legitimately speak of mathematical knowledge only if we abandon an internalist account of knowledge? I do not think so.

In fact it seems to me that if we accept my account of a mathematical proof, we have a simple argument to argue that mathematical theorems are true in a plausible sense. This plausible sense just refers to a definition of truth *à la* Tarski, even if this definition should be understood for that purpose in a non-classical way.

In his more classical form, such a definition stipulates that a statement “p” is true if and only if p. This is the famous schema T of Taski. According to Tarski’s original account, this definition is satisfactory only if we confine ourselves to “formalised languages” (cf. Tarski 1969, p. 68). But, according to Tarski, formalised languages are not necessarily “formulated entirely in symbols”, rather they can be, and they very often are (as in a large part of mathematics) “fragments of natural languages [...] (provided with complete vocabularies and precise syntactical rules)” (*ibid.*). Consider a statement of a certain formalised language L and a meta-language L', and suppose they are two different fragments of the same natural language L_N. Suppose that

$$\{\text{the, is, are, true, if and only if}, P, a\text{'s}, P(a)\} \subset L_N.$$

and the term “$P(a)$” designates in L_N the statement “the a’a are P” of L_N. If

$$\{\text{the, are}, P, a\text{'s}\} \subseteq L \subset L_N.$$

$$\{\text{the, is, are, true, if and only if}, P, a\text{'s}, P(a)\} \subseteq L' \subset L_N.$$

then we can utter in L' the statement of the form T

$$P(a) \text{ is true if and only if the } a\text{'s are } P$$

which is a definition of the truth of a statement of L. Of course, this is not, at the same time, a criterion of truth for this statement, but it assure us that if the statement “the a’s are P” is assertible in L', than we can say, in L' again, that the statement “the a’s are P” of L is true. But L' is a fragment of L_N, thus we can suppose that any statement of L' which is also a statement of L_N is assertible in L' if it is assertible in L_N. Suppose now that L_N is the natural language we use to formulate some mathematical theorem and that the statement “the a’s are P” of L_N is just one of this mathematical theorems. It follows that this statement is assertible in L_N and thus it also assertible in L'. Hence, we can say in L' that the statement “the a’s are P” of L is true. It is then sufficient to suppose that a statement of L that is also a statement of L_N is true, when we can say of it that it is true in L', to conclude that the mathematical theorem “the a’s are true”, formulated in L_N is true, and that it is true just because it is a mathematical theorem.

This seems to me a very simple argument. It is so simple that it cannot support a strong thesis. I use it, in fact, to support a very weak one: Tarski’s definition of truth is compatible with a stipulation according to which any mathematical theorem is true, just because it is a mathematical theorem. If we also suppose, as I do, that a mathematical theorem is a statement about one or more objects, we should conclude that this statement is true of these objects. We can thus admit that there is something as mathematical knowledge, even if we accept an internalist account of knowledge. Moreover, under the previous stipulation, mathematical theorems are not only true and known, but they are also such that we know that they are true and thus we know that we know them. A mathematical proof is thus a means of knowledge and it is possible just because of the presence of a previous piece of knowledge with respect to it.

Bibliographical references

Benacerraf, P. (1973) “Mathematical Truth”, *Journal of Philosophy*, **70**, 1973, 661-680.

Brouwer, L. E. J. (1908) “De Onbetrouwbaarheid der logische Princips”, *Tijdschrift voor Wijsbegeerte*, **2**, 1908, 152-158 (English trans.: “The Unreliability of the Logical Principles”, in A. Heyting (ed.) *L. E. J. Bouwer: Collected Works*, North-Holland, Amsterdam, 1975-1976 (2 vols.), vol. 1, 107-111.

Brouwer, L. E. J. (1912) “Intuitionisme en formalisme”, Inaug. Lect., Amsterdam, 1912 (English transl. quoted, by A. Dresden: “Intuitionism and formalism”, *Bullettin of the American Mathematical Society*, **20**, 1913-1914, 81-96).

Brouwer, L. E. J. (1923) “Über die Bedeutung des Satzes von ausgeschlossenen Dritten in der Mathematik, insbesondere in der Funktionentheorie”, *Journal für reine und angewandte Mathematik*, **154**, 1923, 1-7.

Detlefsen (1992) “Brouwerian Intuitionism”, *in* M. Detlefsen (ed.), *Proof, and Knowledge in Mathematics*, Routledge, London, New York, 1992, 208-250.

Hilbert, D. (1922), “Neubegründung der Mathematik. Erste Mitteilung”, *Abhandlungen aus dem Math. Seminar der Hamburger Univ*., **1**, 1922, 157-177 (quoted ed. in D. Hilbert, *Gesammelte Abhandlungen*, Berlin, 1932-1935 (3 vols.), vol. III, 157-177).

Hilbert, D. (1926) “Über das Unendliche”, *Mathematichen Annalen*, **95**, 1926, 161-190 (quoted from the partial English trans., by E. Putnam and G. J. Massey, in P. Benacerraf and H. Putnam. *Phylosophy of Mathematics*, 2nd edition, Cambridge Univ. Press, Cambridge, etc., 1983, 183-201).

Kitcher, P. (1976) “Hilbert Epistemology”, *Philososphy of Science*, **43**, 1976, 99-115.

Kitcher, P. (1983) *The nature of Mathematical Knowledge*, Oxford Univ. Press, Oxford, 1983.

Panza, M (1997*a*) “Mathematical Acts of Reasoning as Synthetic *a priori*”, *in* M. Otte and M. Panza (ed.), *Analysis and Synthesis in Mathematics. History and Philosophy*, Kluwer, Boston Studies in the Philosophy of Science, Dordrecht, Boston, London, 1997, 261-326.

Panza, M. (1997*b*) “Classical sources for the concepts of analysis and synthesis”, *ibid*., 365-414.

Panza, M. (1988) “Le falsificationnisme de Popper et le malentendu de l’induction”, *Sciences et Techniques en perspective*, 2nd ser., **2**(1998), pp. 61-81.

Resnik, M. (1981-1982) “Mathematics as A Science of Patterns”, *Noûs*, 1981, **15**, 529-550 (“Ontology and Reference”), and 1982, **16**, 95-105 (“Epistemology”).

Resnik, M. (1992) “Proof as a Source of Truth”*in* M. Detlefsen (ed.), *Proof and Knowledge in Mathematics*, Routledge, London and New York, 1992, 6-32.

Resnik, M. (1997) *Mathematics as a Science of Patterns*, Clarendon Press, Oxford, 1997.

Tarski, A. (1696) “Truth and Proof”, *Scientific American*, **220**, n° 6, Jun 1969, 63-77.

Tieszen, R. (1989) *Mathematical Intuition. Phenomenology and Mathematical Knowledge*, Kluwer, Dordrecht, Boston, London, 1989.

Tieszen, R. (1992) “What is a proof”, *in* M. Detlefsen (ed.), *Proof, Logic and Formalisation*, Routledge, London and New York, 1992, 57-76.