# IS THE NOTION OF MATHEMATICAL OBJECT AN HISTORICAL NOTION

*Marco Panza*[1]

1

Both[2] historians and philosophers of mathematics frequently speak of mathematical objects. Are they speaking of the same or of similar things? Better: are they appealing to the same notion or to similar notions?

If we judge according to the way they actually use the term 'mathematical object', or the term 'object' tout court, we should answer in the negative.

Of course, we cannot describe the situation by saying that there are two distinct and well established notions, and the historians appeal to one of them and philosophers to the other. Many historians and many philosophers speak of mathematical objects without bothering to specify what exactly they are referring to, and when they try to clarify their languages, they are quite far from describing two notions that we could respectively identify with the historians' notion and philosophers' notion.

Moreover, the very distinction between historians and philosophers of mathematics is hard to establish and the situation becomes much more complicated if mathematicians are considered as well.

It seems, thus, that the best way to describe the situation is to say that mathematicians, historians of mathematics, and philosophers of mathematics speak of mathematical objects in many quite different ways, often without trying to specify what they mean.

Still, it seems to me that among the respective meanings that mathematicians, historians of mathematics, and philosophers of mathematics actually ascribe, or seems to ascribe, to the term 'mathematical object', there are some which are quite frequent.

The following claims display some of these meanings:

$O.i$) Mathematical objects are what mathematical theorems ascribe properties to;

$O.ii$) Mathematical objects are what mathematicians are able to prove the existence of;

[1] CNRS, REHSEIS (CNRS and Université Paris Diderot – Paris 7).

*2 The text was presented for the first time at the REHSEIS, in the context of the workshop Philosophy of mathematics as an interpretative enterprise: how much history of mathematics should the philosophy of mathematics be able to account for?,, held on Jun 18th-19th 2007. I also presented at the San Jose State University, on October 9th, 2007 and at the University of Urbino, on December 11th 2007. I thank all the colleagues that heard my talks and made me precious suggestions, specially Mario Alai and Alessandro Afriat that took the time to correspond with me about a first version of my paper. I also thank Edward Zalta for having read my text and having discussed it with me.*

$O.iii$) Mathematical objects are what it is possible to operate with in doing mathematics;

$O.iv$) Mathematical objects are what a mathematical theory or different mathematical theories are about;

$O.v$) Mathematical objects are the outcome of a process of objectivation of mathematical procedures, or thematisation (as Cavaillès said);

$O.vi$) Mathematical objects are places in structures;

$O.vii$) Mathematical objects are either constructive or correlative;

$O.viii$) Mathematical objects are abstract objects that encodes exactly the properties that they exemplify in an appropriate mathematical theory;

$O.ix$) Mathematical objects are what singular terms, occurring in appropriate true statements (or in appropriate statements that may warrantably be claimed to be true), stand for;

$O.x$) Mathematical objects are abstract individuals that exist and are as they are independently of what we know and assert about them.

Needless to say, many other claims like these could be added and each of them might and should be better specified, for many further differences or equivalences to appear.

But, for my present purpose, this list is large enough. I have ordered its items so as to go from claims that I would expect to be accepted by a mathematician to claims that I would expect to be accepted by a philosopher, passing through claims that I would expect to be accepted by an historian.

In spite of that, several claims which, in this list, have positions that are far-away from each other, admit understandings under which they are very similar. The most clear example is that of claims ($O.i$) and ($O.x$) that are subject to understandings under which they are perfectly equivalent. This is not surprising, since many philosophers that accept claim ($O.x$) argue that it corresponds to the 'spontaneous Platonism' of many mathematicians.

Similarities and/or equivalences between the previous claims might not, however, mask differences in the reasons that different people could have for endorsing them. These reasons are what I'm mainly interested in, here.

Rather than distinguishing mathematicians, historians of mathematics, philosophers of mathematics and their respective conceptions about mathematical objects, I suggest to distinguish three distinct motivations for appealing on the notion of mathematical object.

The first motivation is what I call 'mathematical': the appeal to the notion of mathematical object can be useful for describing some mathematical states of affairs. For example, in the meta-language (or better, a meta-language) used for exposing a certain mathematical theory, it may be convenient to say that it has been proved that a certain condition holds for a certain class of objects, or that certain objects have been proved to exist.

The second motivation is what I call 'historical': the appeal to the notion of mathematical object can be useful for reconstructing a piece of the history of mathematics. For example, one may argue that two theories differ because the objects they are about are different, or the same objects have been studied, in different theories, according to a different perspective, or, also, that in a certain stage of the evolution of mathematics a certain operation or relation has been transformed into (or understood as) an object.

The third motivation is what I call 'philosophical': the appeal to the notion of mathematical object can be useful for accounting for some relevant features of mathematics, conceived either as an activity of a certain sort, or as a corpus of statements of a certain nature. For example, one can argue that in mathematics some objects are brought into being, or that mathematical knowledge is knowledge about abstract objects, or that mathematical theorems are true statements and they are so for they ascribe to some objects the properties that these objects actually have.

All these three motivations are genuine, I claim. Moreover, though they are certainly different, there is a way to understand them according to which they appear to be strictly connected.

On the one hand, one could argue that, under an appropriate – or, at least, a legitimaten – understanding, the philosophical motivation should neither be concerned only with some selected pieces of contemporary mathematics – like set theory or different versions of formal arithmetic – nor with mathematics as a whole, conceived as a mere ideal-type occasionally illustrated by some examples, but rather with real mathematics as it is and it has been along all its history, which I shall call, for short, 'real all-time mathematics'.

If this is admitted, the historical motivation might be understood as being part of the philosophical one: the reconstruction of some relevant pieces of the history of mathematics can be conceived as a way to provide the material that the philosophical motivation is about. Hence, the appeal to the notion of mathematical object can be useful for shaping this material in a convenient form.

On the other hand, one could also argue, that, under an appropriate – or, at least, a rightful – understanding, the historical motivation is concerned with the mathematical states of affairs that the different theories that have followed each other along the history of mathematics have displayed.

If this is admitted the mathematical motivation might be understood as being part of the historical one: the description of some mathematical state of affairs can be conceived as a way to depict the results of these theories. Hence, the appeal to the notion of mathematical object can be useful for representing these results in a convenient way.

According to the this understanding, the historical motivation includes the mathematical one, and the philosophical one includes both the historical and the mathematical ones. But this is not to say, of course, that mathematics and history of mathematics are included into philosophy of mathematics. If this were a consequence of such an understanding, this same understanding would be ipso facto refuted because of a *reductio ad absurdum*.

The inclusion of the historical and mathematical motivations into the philosophical motivation reects rather the conviction that, insofar as philosophy of mathematics aims to account for some relevant features of real all-time mathematics, it cannot act alone: it needs the help of history of mathematics and of mathematics itself. Hence, if philosophy of mathematics appeals to the notion of mathematical object in order to pursue this aim, this notion has to be consistent with the notion of mathematical object that also history of mathematics and mathematics could appeal to.

My purpose here is to take seriously this last methodological principle, and thus to look for a possible construal of the notion of mathematical object that could remain invariant under the occurrences of these notions in philosophical, historical and mathematical contexts.

2

Let us begin by considering the help that history of mathematics should give to philosophy of mathematics.

According to the previous understanding of the philosophical motivation, the purpose of the philosophy of mathematics is to account for some relevant features of real all-time mathematics. This is, of course, a very broad characterisation of such a purpose. But it is clear enough to suggest that philosophy of mathematics has a structural affinity with empirical sciences.

Take the example of physics. One could justifiably argue that its purpose is to account for some relevant features of physical reality. To admit that the purpose of the philosophy of mathematics is to account for some relevant features of real all-time mathematics is thus the same as admitting that philosophy of mathematics is to real all-time mathematics like physics is to physical reality.

Let us develop such an analogy.

To say that the purpose of physics is to account for some relevant features of physical reality is not the same as saying that physical reality is the subject-matter of physics. The subject matter of physics is rather something like a system of descriptions of different fragments of physical reality. The elaboration of these descriptions is a crucial task of physics itself. Still, the achievement of this task cannot be the same as the adoption of appropriate conventions. These descriptions should, of course, be appropriate for the aims of physics itself, but they should also be faithful and form a system that is large enough to leave out no crucial aspect of physical reality.

This holds also for philosophy of mathematics, I suggest: to admit that the purpose of philosophy of mathematics is to account for some relevant features of real all-time mathematics should not be the same as admitting that real all-time mathematics is the subject-matter of philosophy of mathematics; the subject matter of philosophy of mathematics should rather be a large enough system of appropriate and faithful descriptions of different fragments of real all-time mathematics, whose elaboration should be a crucial task of philosophy of mathematics itself.

The requirements of faithfulness and sufficient size are essential for physics. To say that the purposes of physics is to account for some relevant features of physical reality is just a way to emphasise these essential requirements.

I suggest that these same requirements should also be essential for philosophy of mathematics. To admit that its purpose is to account for some relevant features of real all-time mathematics is a way to advance these requirements.

But how can it be decided that a system of descriptions of different fragments of physical reality or of real all-time mathematics complies with these requirements? This is a crucial problem both for physics and for philosophy of mathematics, and analogous problems arise for any empirical science.

In the case of physics and of any other physical science these problems are never solved – and they could never be solved – at once, by providing some general conditions of faithfulness and sufficient size and some connected criteria. Any attribution of faithfulness and sufficient size to a system of descriptions of different fragments of physical reality, or of any other sort of empirical reality depends on local decisions which are always open to revision. The essential task of the experimental component of an empirical science is just that of helping in taking these decisions.

Also in this case, the analogy between philosophy of mathematics and empirical sciences holds. Any attribution of faithfulness and sufficient size to a system of descriptions of different fragments of real all-time mathematics should depend on local decisions which should be always open to revision. And these decisions should be taken with the help of history of mathematics.

Still, the requirements of faithfulness and sufficient size have to go together with a requirement of appropriateness: the system of descriptions of different fragments of real all-time mathematics that is supposed to provide the subject matter of philosophy of mathematics should be appropriate for providing a basis for a philosophical account of some relevant features of real all-time mathematics.

Such an account cannot certainly be expected to be based on a detailed and accurate reconstruction of the totality of the history of mathematics, or, at least, of a quite large part of it. Even if it were possible, a similar sort of reconstruction would include too many different data for providing a useful basis for such an account.

Of course, this is not to say that historians should not aim to get detailed and accurate reconstructions of as much history of mathematics as they can, or to argue that there is no utility in looking for new details, or for a more fine-grained view on historical material. This is the normal job of historians, and I'm certainly not willing to deny its usefulness and its interest.

My point is rather that the mere accumulation of a number of quite detailed and fine-grained independent descriptions concerned with distinct fragments of real all-time mathematics can provide a basis for no philosophical account of real all-time mathematics. These descriptions should rather form a system, and this system should provide, so to say, a compact delineation of real all-time mathematics.

The analogy between philosophy of mathematics and physics is again useful here. Insofar as it is supposed to be the subject matter of physics, a system of descriptions of different fragments of physical reality consists in a system of values assigned to a number of parameters that are fixed beforehand together with the relations that are supposed to link them to each other. The form of the system is thus fixed before the data concerned with any particular fragment are established.

Something analogous should hold, I think, for philosophy of mathematics. The system of descriptions of different fragments of real all-time mathematics should comply with an appropriate format.

This is not the same as requiring that this system consist of rational reconstructions, in Lakatos' sense. This is neither the same as requiring that a general model for the evolution of mathematics – similar to Popper's or Kuhn's models for the evolution of empirical sciences – be established, and that historical research be merely intended to provide a collection of case studies to confirm or refute this model.

By speaking of format, I'm rather referring to a grid of possibilities that the different fragments of real all-time mathematics are supposed to realise or not to realise. In my view, a compact delineation of real all-time mathematics is nothing but a system of responses to a number of general questions about the realisation of these possibilities.

In order to state these questions and imagine these possibilities, a system of general interpretative categories is needed, and these have to be chosen beforehand, of course. Call them 'the fundamental notions of philosophy of mathematics'.

There are thus, in my view, three successive stages in the elaboration of the subject matter of philosophy of mathematics.

The first of them consists in the determination of the fundamental notions of philosophy of mathematics, together with their mutual relations.

The second consists in the imagination of the possibilities that the different fragments of real all-time mathematics are supposed to realise or not, and in the framing of a number of questions about the realisation of them.

The third consists in the concrete historical researches that are intended to answer these questions.

This does not mean that the first two stages have to be achieved blindly, before any previous contact with concrete historical stuff, and that their achievements are not submitted to any possible revision. History of mathematics has itself a quite long history, and though it has quite seldom worked in a strict connection with philosophy of mathematics, according to a general strategy as that which I'm trying to describe, it is there to suggest some ways to achieve these stages. Moreover, the results of the third stage can, in any moment, feed back to the results of the previous ones.

Still, a starting point is necessary. I suggest to begin with some general considerations as the following.

On one hand, real all-time mathematics can certainly not be depicted as an harmonious arrangement of homogenous elements. Hence, the format of the system of descriptions of different fragments of real all-time mathematics we are looking for cannot certainly reduce to a general schema together with some rules of composition of different elements that comply with it. On the other hand, real all-time mathematics can no more be depicted as a mere amount of disparate elements. I suggest to understand it as a generative system of elements, each of which is complying with a general schema chosen among a finite number of different though similar ones.

If so, the establishment of the format we are looking for depends, at least, on:

α) An appropriate description of the different schemas that these elements can comply with;

β) An appropriate description of the similarities between these schemas;

γ) An appropriate description of the relations that these elements, once generated, have to each other;

δ) An appropriate description of the process that produces these same elements.

The content of these descriptions, and, more generally, the nature of the elements they are concerned with cannot but depend on the features of real all-time mathematics that the philosopher is willing to account for.

Different choices are possible, here. I suggest that these features should include all those which are relevant for responding to the following questions: can real all-time mathematics be understood as a sort of knowledge? And, if it can, as what sort of knowledge can it be understood?

These questions are quite different from others like: is real all-time mathematics a sort of knowledge? And, if it is, which sort of knowledge is it? Real all-time mathematics is a quite complex phenomenon whose fragments admit different sorts of descriptions and that admits different sorts of compact delineations. Hence, the question is not whether it is so and so, but whether it admits a certain understanding, that is, whether such an understanding is consistent with the requirements of faithfulness and sufficient size.

To answer the previous questions, we have but to try to understand a sufficiently large amount of real all-time mathematics as a sort of knowledge, and evaluate whether this understanding is faithful. This is the way the requirement of appropriateness meets the requirements of faithfulness and sufficient size.

But this is also the way genuine historical inquiries meet genuine philosophical explanations: whereas the satisfaction of the requirements of faithfulness and sufficient size is essentially an historical matter, the satisfaction of the requirement of appropriateness is essentially a philosophical matter.

But the history which is concerned here is history of mathematics. Thus there is no way, I think, to evaluate faithfulness if the mathematical content is not reconstructed as such. The historian has to delve into this content and learn how to do mathematics according to the constraints of the fragment of history that he is studying. She/he has to transform her/himself into a mathematician of the time she/he is considering. It follows that this is also the way how genuine historical inquiries and genuine philosophical explanations meet genuine mathematical practice.

3

Knowledge necessarily has a content, either propositional or not, and for it to be mathematical this content has to be mathematical. A natural choice is to understand this content as a domain of objects (if knowledge is not propositional), or as a system of states of affairs concerning these objects (if it is propositional).

If this is admitted, the fundamental notions of philosophy of mathematics should include that of a mathematical object and that of a domain of mathematical objects. And these notions should be supplemented with a number of connected notions capable of characterising what could count as knowledge about mathematical objects, and to distinguish different sorts of knowledge about mathematical objects.

The elements composing the generative system of real all-time mathematics should moreover be somehow intrinsically connected with different domains of mathematical objects.

One possibility would be of course that these elements coincide with these same domains. But if it were so, one should then add an account of the way these objects could be known or the way it could be known that these objects are so and so. Moreover, one should also explain how it happens that they are so and so, at least if claim $(O.x)$ is not admitted, that is, it is not admitted that mathematical objects exist as such, independently of us.

It seems thus more convenient to admit that the elements of the generative system of real all-time mathematics are not simply domains of mathematical objects, but are more complex items capable of determining these domains in some way, by establishing the properties and relations of the objects that belong to them in such a way that knowledge is possible.

I suggest these items – that is, the elements of the generative system of real all-time mathematics – be understood as mathematical theories, and to admit that any mathematical theory determines a domain of objects, though different mathematical theories may do it in different ways.

More precisely, I suggest that the schemas that (α) is about be schemas of mathematical theories and that they mainly differ for the way as the theories complying with these schemas determine the corresponding domains of objects.

These schemas are similar insofar as they are all schemas of mathematical theories. (β) should thus primary describe, in general, mathematical theories. But it should do more than that, I suggest. It should also describe the similarities between the different domains of objects that different theories determine either in the same way or in different ways. I suggest that this description should include an account of mathematical knowledge.

If this is admitted, only (α) and (β) are relevant for responding the two previous questions about mathematical knowledge. In what follows, I shall limit myself to say something more about them.

4

Mathematical theories have often been understood as formal theories, that is, as deductive closures of a system of axioms. This understanding has an evident advantage of clarity, but it seems to have two different limitations.

The first one is quite evident: it plausibly applies only to a quite restricted number of mathematical theories. This is a short way to expressa quite complex circumstance. Another way, a bit more precise, is the following. History of mathematics teaches us that real all-time mathematics includes many fragments that it seems plausible to understand as mathematical theories, provided some other fragments are so understood, and that can hardy be regarded, or even reconstructed as formal theories.

Take the example of Euclid's and Hilbert's plane geometries. It seems plausible to admit that neither one is a mathematical theory, or that they both are. Still, whereas the latter can easily be taken to be a formal theory, the former cannot, and it can also be quite hard to reconstruct as a formal theory: the requirement of faithfulness does not allow us to do so.

The second limitation is quite different in nature. If a mathematical theory is merely understood as a corpus of statements together with a number of deductive rules to derive certain ones from others, and real all-times mathematics is understood as a system of theories, then two possibilities are open.

Either real all-times mathematics reduces itself to a complex corpus of statements formed by different sub-corpuses – the different theories – that do not communicate (and whose possible connections can also be detected from outside), or real all-time mathematics includes some sort of rules of connection between different theories that cannot be deductive rules applied to appropriate axioms.

In the first case, real all-times mathematics cannot be understood as a generative system of theories, since no new theory can be produced on the basis of others in virtue of some component of this same system. In the second case, the generative nature of the system has to be explained by adding to it an essential element that is not included in mathematical theories.

This suggests a change of perspective. Instead of understanding real alltimes mathematics as a corpus of statements, one could understand it as an activity suited to produce such a corpus.

It seems to me that this activity should then be understood as being twofold: on the one hand, it should consist in establishing a net of instructions, possibly justifying them with respect to certain aims; on the other hand, it should consist in the application of these instructions to the production of certain statements.

A mathematical theory could thus be understood as a quadruple (S, R, S, A) composed by: a net S of stipulations (typically assumptions and instructions for deriving conclusions by relying on these assumptions); a system R of reasons for the justification of these stipulations with respect to a certain aim; a corpus S of statements derived according to such stipulations, generally called 'theorems' or more generally 'results'; and an amount A of activity which establishes these same stipulations, provides the reasons of them, derives this corpus of statements according to them, and produces other sorts of items like problems, conjectures, methods, research programs.

By speaking of amount of activity, I mean the (intellectual and practical) activity of a number of actual men and women performed in well-determined times and spaces. This is what is also often called 'mathematical practice'. A large part of this activity consists in writing treatises, papers, letters, or personal notes, and in teaching or giving talks.

The immediate results of this activity are discourses, mainly written discourses or texts, possibly supplemented by diagrams or other sorts of pictures, and these discourses contain different sorts of phrases. Some of them provide the relevant stipulations. Some are intended to give reasons for them or at least are such as to manifest these reasons in some way. Some are the statements of the corpus S.

This is the material that the historian and the philosopher (which are possibly the same person) are confronted with. To provide the subject matter of philosophy of mathematics, that is, a large enough system of appropriate and faithful descriptions of different fragments of real all-time mathematics, they have to extract from this quite disorganised material a system of distinct theories. These means that they have to reconstruct different, and possibly generatively connected, quadruples (S, R, S, A).

To avoid any misunderstanding, it is important to remark that a theory is generally not that which is exposed in a single treatise, however innovative, large and complete this treatise would be. A treatise can of course be intended to expound a theory, but, in general, a theory results from a quite larger corpus of materials. A large part of the job of the historian, perhaps the most difficult one, should consist in identifying this corpus and extracting from it just a single theory.

Another important remark is the following. As I understand it, a theory is not a closed system. Both, a net of stipulations and a system of reasons for them can be extended or restricted and this is often what happens actually.

But more importantly, even if a net of stipulations is given and is taken as being fixed, the theory that includes this net of stipulations is not supposed to include the totality of statements that can be derived according to them. The corpus S of statements that this theory includes is not supposed to be, so to speak, the derivative closure of these stipulations. It is rather the corpus of statements that have been actually derived according to these stipulations.

Moreover the same net of stipulations can admit different reasons, either with respect to the same aim, or to different aims, and vice versa, the same system of reasons can lead to different stipulations. On the other hand, in the generality of cases, a same corpus of statements can be derived in different ways starting from the same stipulations, and these stipulations can lead to different problems, conjectures, methods and programs.

It follows that though connected to each other, the four components of a theory are not supposed to be such that one of them can be determined if the three other are fixed.

This make the establishment of the conditions of identity for theories quite difficult. I will not consider this difficult problem here. I simply remark that the identity of a certain theory does not generally result only from the application of some appropriate general conditions, but results also (and even overall) from particular and of course questionable decisions taken by the historian during his reconstructive work.

Another difficult problem that I do not want to consider here is the problem of the connections that different theories have to each other in the generative system they are supposed to compose. I simply remark that these connections largely depend on the systems of reasons and the amount of activity included in these theories.

## 5

The problem I would like to deal with here is that of the domain of objects that is related to a certain theory.

According to my description, a theory does not include a domain of objects. Still, I maintain that any theory determines a domain of objects: the objects of this theory, as I shall say for short.

This is not the same as saying that different theories necessarily have different objects. The link between a theory and a domain of objects is certainly an application, but not necessarily an injective one, I argue.

The main reason for this is that a theory can explicitly be intended to result from a particular sort of dealing with the objects of some other theory. For example, the theory of real functions is explicitly intended to deal with real numbers, that are, or at least can be regarded as, the objects of another theory.

But, apart from this, the main problem is that of understanding the way a theory determines the domain of its objects.

I know at least four contemporary construals of the notion of mathematical object that seem to me to fit, at least partially, with my notion of mathematical theory, so as to allow one to say that a theory determines a domain of objects.

5.1

The first is Shapiro's conception of mathematical objects as places in structures: claim ($O.vi$), in my previous list.

According to Shapiro, 'a structure is the abstract form of a system' and a system is 'a collection of objects with certain relations'[3]. This could appear to be circular, and would be, in fact, if the objects that form a system were necessarily mathematical objects, that is, places in structures, and there were be no other way to get a structure than making abstraction from some of the systems it is the form of.

But both these claims are false, according to Shapiro. Let us consider them.

Shapiro's structuralism is supposed to be a philosophy of modern mathematics, and philosophy of mathematics is understood by him as an interpretative enterprise: a way to account for real mathematics. Hence, claim ($O.vi$) does not hold for him for real all-times mathematics, and there is room to manta-in that a form of mathematics, where mathematical objects were not places in structures, existed. Thus, there is room to argue that a structure, whose places are mathematical objects, is the abstract form of a mathematical system whose objects are not places in structures, though they are mathematical. To make an example, we could maintain that modern, or Peano arithmetic is about the structure of progression, and this is the abstract form of the system of pre-modern arithmetic, which is a mathematical system whose objects are not places in structures.

But, according to Shapiro, a structure can also be obtained in another way, that is, 'through a direct description of it'[4]. The best and, by far, the most usual way to provide such a description is by means of a system of axioms understood as implicit definitions. Hence, to consider the same example, there is no need to rely on Euclid's arithmetic to get the structure of progression. This could merely be understood as the structure described by Peano's axioms (presumably at the second order).

But also in this second case, a structure would continue to be a form of a system, and the possibility of a system that has this form and whose objects are not places in structure seems to be admissible: Euclid's arithmetic would provide an example of this sort of system.

The conclusion I draw from these considerations is that Shapiro's construal of the notion of mathematical object is not rich or general enough to be included among the fundamental notions of philosophy of mathematics, provided that the aim of philosophy of mathematics be that of accounting for some relevant features of real all-times mathematics, as I have assumed from the very beginning.

But can it be included among the fundamental notions of a philosophy of mathematics that would aim to account for some relevant features of modern mathematics, or, more generally, of a certain form of mathematics?

*3 Cf. S. Shapiro, Philosophy of Mathematics: Structure and Ontology, Oxford Univ. Press, 1997, pp. 74 and 73.*
*4 Cf. ibid. p. 74.*

I guess that the answer should be positive. I think, indeed, that insofar as a structure is described by a system of axioms that work as implicit definitions, and it is merely understood as that which is described by such a system, it may be easily associated with a theory in my sense of this term.

This theory would be composed of: a net of stipulations including the axioms plus all the necessary instructions for deducing from them statements of an appropriate language according to the rules of an appropriate logic; a system of reasons to justify the choice of these axioms, this language and this logic; the corpus of statements that have been actually derived by these axioms according to these stipulations; the amount of activity that led to establish these same stipulations, to provide these reasons, to deduce these statements, to formulate problems, to advance conjectures, to elaborate methods of proof, and outline research programs.

The statements derived from the axioms would include singular constants and quantifiers whose range should be understood, according to the precepts of Shapiro's structuralism, as being restricted to a domain of items that are supposed to satisfy the axioms themselves, that is to be appropriate for the axioms to hold.

This domain would then be the domain of the objects of the theory and these objects would be the items included in it. These objects would have some properties (mainly relational ones). These properties would include those that are assigned to all of them or to some of them by the quantified statements that occur among the axioms and are included in the corpus S.

Some of these objects would moreover be denoted by the singular constants that occur in the axioms and in the statements included in S. These should then be understood as particular objects of the theory whose properties include those that are assigned to them by these axioms and statements.

The properties that are so assigned to the objects of the theories would not be all the properties that they have, however. The reason is simply that, in my picture, the corpus S does not coincide with the deductive closure of the axioms. Thus the objects of the theory can have properties that have to be discovered, and to prove a theorem about them is just to discover one or more of these properties.

So it should not be difficult to define a technical notion of internal existence and a technical notion of internal truth for objects of a theory like this, and to explain in what sense we would have knowledge about them.

I do not consider this matter here. I would rather make a marginal remark on this matter.

These technical notions of internal existence and truth could be supplemented by other notions of external existence and truth. This would bepossible insofar as the system of axioms is understood as a description of a structure that is not directly identified with the form of a previous system (being rather identified merely with the structure described by these axioms), but is intended to be the form of a previous system. This is the same as supposing that the system of reasons justifying the stipulations of the theory include arguments for showing that these stipulations are the right ones to be made in order to describe the form of a previous system.

In this case, a statement of the theory could be said to be externally true, if it can be interpreted in a language capable of describing this previous system, and so interpreted it holds for this system. On the other hand, an object of the theory could be said to exist externally if this system includes an object that corresponds to it according to the same interpretation.

Insofar as the previous system would be a system of abstract mathematical objects, they would be, in turn, the objects of another mathematical theory and these notions of external truth and existence would work as bridges between two mathematical theories, one of which is supposed to be, in some sense, more fundamental, but perhaps less rigourous, than the other.

The relations of these theories would then imitate, in some way, the relations that hold between a physical theory and the external word that this theory is supposed to account for. Hence, the possibility of introducing these notions of external existence and truth could be intended as an argument for showing that mathematical objects so understood are structurally analogous to the objects of empirical sciences, though not to the empirical objects themselves.

5.2

Shapiro's construal of the notion of mathematical object seems thus to fit quite well with my understanding of mathematical theories, but it only applies to a limited family of mathematical theories and seems even to require that other sorts of mathematical theories pre-existed.

Among these theories there are those whose stipulations and derived statements form a formal theory in the usual sense of this term. But it seems to me that Shapiro's construal also apply to other sorts of theories that we could generally call 'informal'.

What is relevant for this construal to apply is that the stipulations of the theory be capable of determining at once the domain of its objects. This is what J. M. Salanskis considers the distinctive feature of that what he calls 'correlative objectivity' (note that the term 'objectivity' is here used to mean a form of determination of a domain of object and not, as is usual in philosophical technical English, a mode of judgement)[5].

In Salanskis' picture, this sort of objectivity is contrasted with 'constructive objectivity': claim ($O.vii$), in my previous list. The fact that correlative objectivity is not the only possible form of mathematical objectivity fits very well with the fact that Shapiro's construal of the notion of mathematical object only applies to a limited family of mathematical theories. Moreover, Salanskis insists on the fact that this is not only an historical contingence, and that constructive objectivity is rather, as it were, a condition of possibility of correlative objectivity. This also seems to fit quite well with my previous considerations, though I do not think that Salanskis' reasons for including constructive objectivity in correlative objectivity be similar to Shapiro's reason for distinguishing between structures and systems of objects.

But, this possible discrepancy apart, that which is more relevant for my purpose is whether Salanskis' notion of constructive objectivity is appropriate to account for the large part of real all-time mathematics that does not fit with Shapiro's account.

According to Salanskis, 'constructive objectivity is the objectivity of those objects that we expect to "construct" through a recursive clause.' Moreover, 'a recursive clause consists in giving a set of primitive objects and a list of constructive rules providing instructions for

*5 Cf. J. M. Salanskis, "Platonisme et philosophie des mathematiques", in M. Panza and J. M. Salanskis, L'objectivité mathematiques. Platonisme et structures formelles, Masson, Paris, 1995, pp. 179-212; J. M. Salanskis, Philosophie des mathematiques, forthcoming.*

constructing a new object based on a number of objects that are supposed to have been already constructed'[6].

A recursive clause can thus be understood as a stipulation and be part of the net of stipulations that are included in a mathematical theory, according to my understanding of this notion. If so, this net of stipulations should also include appropriate instructions for deriving statements attributing properties to the objects so constructed. It seems thus quite easy to associate with a certain constructive clause, or, in Salanskis' parlance, with an example of constructive objectivity, a theory in my sense of this term.

Still, things seem to me not to be so simple as one could think at first glance. Two possibilities seem to me to be open.

We can firstly understand the term 'recursive' in Salanskis' account in a quite strict sense, that is, as it is usually understood in logic and computer science today. This seems to be Salanskis' understanding. Under this understanding, the set of primitive objects relative to a recursive clause is a finite small set of objects, identified extensionally. And as also the constructive rules are finite in number, the result is a infinite numerable domain of objects that can be understood as the constructive closure of the primitive objects under the constructive rules.

If all this is the case, there is no difficulty in associating with a certain constructive clause a theory in my sense of this term. And, it is also easy to understand how this theory determines is domain of objects. These would be appropriate compositions of elementary signs, or what these compositions stand for, if a referential attitude is admitted.

In the first case, properties would be assigned to these objects by statements belonging to a language that does not include these signs but speaks of them, and would depend on the way these signs compose, as in a contentual theory in Hilbert's sense. In the second case, properties would be assigned to these objects by statements belonging to the same language that includes them, and would depend on the way these statements are generated, according to the stipulations of the theory.

In both cases, any object would be individually introduced and would thus be associated with a well defined condition of identity, and the totality of them would have an intrinsic modal nature. It would be the totality of objects that could be constructed trough the recursive clause if it were indefinitely applied. Still, the understanding of quantified statements including quantifiers whose range is given by the domain of these objects would present no difficulty.

The simplicity of this situation depends on a fundamental circumstance: the net of stipulations included in the theory explicitly determines the potential totality of the objects of this theory and provides the tools to actually exhibit or denote any number of them though appropriate signs or singular constants.

But this simplicity has quite a high price, I think. This price is that, if Salanskis' notion of constructive objectivity is so understood, the theories that are associated with it do not provide the complement of those that are associated with structures in Shapiro's sense, relatively to real all-time mathematics. To take an example, there would be no room to understood Euclid's geometry as a theory associated with a recursive clause, and then as an example of constructive objectivity.

*6 Cf. J. M. Salanskis, Philosophie des mathematiques, cit., pp. 53-54 of the typescript.*

If one included Euclid's geometry among theories depending on a recursive clause, one should understand the term 'recursive' in a quite special way. And this would produce a considerably more complex situation.

The example of Euclid's plane geometry seems to me paradigmatic. Let's consider it.

Its stipulations include explicit definitions. But however these are understood, they are certainly not able to define objects, since they do not provide conditions of identity for them. They rather define sorts of objects, that is, they provide conditions of application of concepts, without ensuring that these are sortal concepts.

The conditions of identity of the objects that fall under these concepts are provided separately. In my view, they are provided by that which one could call the 'rules of givenness' of these objects.

These are a particular sort of constructive rules that establish that an object can be given if some other objects are given. Euclid's first three postulates provide three such rules. But these rules work only if they are associated with a practice of working on diagrams, and the primitive objects are not identified with a finite number of objects extensionally identified, but with some objects of a certain sort.

In my interpretation, these objects are segments and the rules of givenness teach how to give plane geometric objects starting by finite numbers of independent segments. The simplest example is the way an equilateral triangle is given, starting with any segment. This is done by a double application of two rules; that related to postulate 3, and that related to postulate 1.

What is relevant for my purpose is that this segment is in no way specified, once and for all, as that particular segment, among all the segments that Euclid's theory is about. It is no more than any given segment, and it is identified just insofar as it is actually given, that is, represented by an appropriate diagram that is actually drawn. Then, even the triangle that is constructed based on it is just the triangle represented by the diagram that is actually drawn based on this first diagram and following the rules of givenness.

But, if so, what about the totality of segments and equilateral triangles that could be given, by tracing other diagrams on the same support and in the same time, or by tracing other diagrams on other supports and times? How is this totality defined? How are its single elements told apart? Which are their conditions of identity?

I do not think these questions admit a faithful and plausible answer within Euclid's plane geometry. The domain of this theory is not determined at once, indeed it is never totally determined, actually or even potentially. The kinds of objects included in this theory are defined by appropriate stipulations. Other stipulations indicate how to provide objects of these kinds. Finally, other stipulations again give instructions for ascribing properties to given objects. But strictly speaking, no property can be assigned in Euclid's plane geometry to all the objects of a certain sort, for example to all triangles. Universal statements in this theory have another, not quantified, or perhaps intrinsically modal, form. They assert, for example, that if a triangle is given, and it is thus identified as a particular object, then it has certainly some properties, or, if you prefer, that a triangle that has not these properties cannot be given.

If I'm right, some objects of a similar theory can be denoted by singular constants or represented by appropriate symbols, and can thus be understood as what these constants or symbols stand for. But the domain of these objects cannot be identified with the range of the quantifiers occurring in appropriate statements.

It follows that neither Shapiro's identification of mathematical objects as places in structure, nor Salanskis' identification of some of them with objects whose objectivity is constructive – provided that this last notion is understood in the usual sense of 'recursive' – applies to the objects of a theory like Euclid's plane geometry.

Thus the question is how to define these objects in a compact and general way.

5.3

One possibility could be to rely on Linsky and Zalta's construal of the notion of mathematical object[7]. This depends on a more general construal of the notion of abstract object that results from Zalta's 'Object Theory', which is in fact a metaphysical axiomatic system[8].

This system uses the usual language of modal higher order logic supplemented by a new mode of predication called 'encoding', formally expressed by formulas like 'αF', to be read as 'α encodes F'. This is taken to be a well-formed formula insofar as 'α' is a term for objects and is true only if the object α is abstract, that is, only if 'A!α' holds, where 'A!' is a predicative constant of the appropriate level (depending, of course on the level of α) defined in terms of the primitive predicate [(to be a) concrete object]. If this predicate is denoted by 'E!', the definition is the following:

$A!\alpha =_{df} \neg \Diamond E!x.$

An abstract object is thus defined as an object that is not possibly concrete.

This definition, as well as any other definition and formula I shall consider, admits a typed version that can treat objects of higher types. For simplicity, I shall omit type specifications.

The previous definition is, of course, only the background of the story. This begins with a schema of comprehension axioms:

$\exists\, x\, (A!\ \wedge \forall F\, (xF \Leftrightarrow \varnothing)),$ (1)

where ∅ is any formula in which $x$ is not free. This schema ensures that for any closed formula ∅, there exists an abstract object that encodes exactly the property that satisfies ∅.

This schema goes together with another schema of axioms providing the (Leibnizian) condition of identity for abstracta:

$(A! \wedge A!y) \Rightarrow ((x = y) \Leftrightarrow \forall F\, (xF \Leftrightarrow yF))$ (2)

It follows that for any closed formula ∅, there is one and only one abstract object that encodes exactly the property that satisfies ∅. To arrive at a way of denoting this object, it is enough to replace in this schema the existential quantifier with the ι-operator, which yields the following schema for definite descriptions:

$\iota x\, (A! \wedge \forall F\, (xF \Leftrightarrow \varnothing))$ (3)

---

*7 B. Linsky and E. N. Zalta, "Naturalized Platonism versus Platonized Naturalism", The Journal of Philosophy, 92, 1995, pp. 525-555; E. N. Zalta, "Neo-Logicism? An Ontological Reduction of Mathematics to metaphysics", Erkenntnis, 53, 2000, pp. 219-265.*

*8 E. N. Zalta, Abstract Objects: An Introduction to Axiomatic Metaphysics, Reidel, Dordrecht, 1983. For a presentation of the basic ideas and ingredients of this theory, to be used for dealing with mathematical objects, see also E. N. Zalta, "Natural Numbers and Natural Cardinals as Abstract Objects: A Partial Reconstruction of Frege's Grundgesetze in Object Theory", Journal of Philosophical Logic, 28, 1999, pp. 619-660.*

To say it informally, an abstract object is thus understood as the objectual correlate of a bunch of properties, and, for any bunch of properties, there is one and only one abstract object. Moreover, it is enough to generalize the schema (1) to higher orders to get object-properties or object-relations, understood as objectual correlates of a bunch of conditions on properties and relations.

The suggestion made by Linsky and Zalta is thus a formal way to implement quite an old idea: namely that objects result from objectivation of properties, or rather conditions on properties. The use of the usual resources of philosophical logic makes it possible to formulate this idea so as to associate it with appropriate restrictions for avoiding inconsistency (which amount, in fact, to a restriction on higher-order comprehension schema for predicates).

But this is not the same as restricting the domain of abstracta. The distinction between usual predication, or exemplification, and encoding, allows Linsky and Zalta's universe of abstract objects to include objects of any sort, and even the round square, or the set of the sets that do not belong to themselves. These are simply objects that encode properties that are exemplified by no object.

Thus, for Linsky and Zalta abstracta exist whenever there are properties, and regardless of whether these properties are exemplified or not. This is what the schema (1) ensures. But not all abstracta are mathematical, of course. For an abstract object to be mathematical, there has to be a mathematical theory T, and certain properties have to be exemplified in T by this object.

If so, this object belongs to T, namely it is the abstract object that encodes exactly the properties that it exemplifies in T: claim ($O.viii$) in my previous list.

The problem is thus how to understand what does it means that certain properties are exemplified by an abstract object in a certain mathematical theory. Insofar as the problem is that of distinguishing mathematical objects among other sorts of abstracta, it would be natural to begin by explaining what a mathematical theory is. This is not exactly what Linsky and Zalta do, however.

They begin by 'extending the notion of an object encoding a property to that of an object encoding a proposition', which is done 'by treating propositions as 0-place properties'[9].

Let $p$ be any proposition. Linsky and Zalta propose to associate to it the 0-place property [(to be) such that p]. This must not be confused with the monadic property [(to be an) $x$ such that p($x$)], where 'p($x$)' is a proposition involving an individual variable $x$. Linsky and Zalta's 0-place property p(to be) such that pq is the property that $x$ has if and only if it is the case that p, where $p$ does not depend on $x$, that is, it is the property that any object has if and only if it the case that p.

This is made clear by using a notation involving a vacuously bounded variable: the 0-place property [(to be) such that p] is thus denoted by '[λy p]'.

On this basis, Linsky and Zalta suggest that an object encodes a proposition $p$ insofar as it encodes the property [λy p], and 'identify a mathematical theory T with the abstract object that encodes just the propositions asserted by T'[10]. It follows that a mathematical theory T is the abstract object that encodes every and only every property [λy p] that is such that $p$ is asserted by T, i. e.:

*9 Cf. B. Linski and E. N. Zalta, op. cit., p. 538.*
*10 Cf. ibid., p. 539.*

$T = \iota x\,(A! \wedge \forall F\,(xF \Leftrightarrow \exists p\,(F = [\lambda y\ p] \wedge t \text{ asserts } p)))$ ,

which supposes, of course, that there is a mathematical theory T, and merely provides a 'theoretical description of T[11]'.

But what is meant when we say that a proposition *p* is asserted by T?

The answer is implicitly given by Zalta's definition of a mathematical theory in terms of two primitive notions: that of purely mathematical proposition and that of authorship[12]. Using the monadic and diadic predicative constants 'Math' and 'A', respectively applied to a proposition and to a pair of individuals, the definition runs as follows:

$MathTh\,(x) =_{df} \forall F\,(xF \Leftrightarrow \exists p\,(Math(p) \wedge F = [\lambda y\ p])) \wedge \exists y\,(E!(y) \wedge A(y, x)).$

From this, using a result concerned with encoding and the 'logic of descriptions', Zalta proves that:

$MathTh\,(T) \Rightarrow T = \iota x\,(A! \wedge \forall F\,(xF \Leftrightarrow \exists p\,(F = [\lambda y\ p] \wedge t[\lambda y\ p]))).$

It follows that for Zalta, to say that T asserts *p* is the same as saying that T encodes the 0-place property p(to be) such that pq, which Linsky and Zalta also express by saying that *p* is true in T in force of the following definition:

$T \models p =_{df} t\,[\lambda y\ p]$ (4)

To this, Linsky and Zalta add a 'rule of closure' that ensures that a logical consequence of a set of propositions is true in a theory T if these last proposition are true in T. Encoding is not closed under logical consequence, so this rule does not follow from (4) and Linsky and Zalta see fit to introduce it as a separate principle.

Using these notions, Linsky and Zalta suggest a construal of the notion of mathematical object.

Suppose that '$\kappa_T$' is a term of the language of T such that, in this language, it is possible to say that $\kappa_T$ exemplifies certain properties, that is, to write a statement like '$F\kappa_T$'. Linsky and Zalta suggest the following definition:

$\kappa_T =_{df} \iota x\,(A!x \wedge \forall F\,(xF \Leftrightarrow T \models F\kappa_t)),$ (5)

where $\kappa_T$ is just a mathematical object of the theory t. This object is thus, as I was saying, the abstract object that encodes exactly the properties that it exemplifies in T.

By replacement we have that:

a mathematical object $\kappa_T$ of a theory T is the abstract object that encodes exactly the properties F such that $T[\lambda y\ F\kappa_T]$

a mathematical object $\kappa_T$ of a theory T is the abstract object that encodes exactly the properties F such that T encodes the proposition $F\kappa_T$.

11 *Cf. E. N. Zalta, "Neo-Logicism? An Ontological Reduction of Mathematics to metaphysics", cit., p.232.*
12 *Cf. ibid., pp. 230-231.*

From this construal of the notion of mathematical object, it does not follow that there are mathematical objects. This rather depends on the existence and features of mathematical theories themselves. This construal is only used to describe mathematical objects that are already supposed to exist and have certain properties.

To understand this point, take any property N that is usually supposed to be exemplified by more than one object, for example the property of being a triangle or that of being a natural number. According to (1) and (2), there will be a unique object

$$\iota x\ (A!\ \wedge\ \forall F\ (xF \Leftrightarrow \forall y\ (Fy \Leftrightarrow Ny))) \qquad (6)$$

that encodes this property. Let n be this object. It could be called 'the N'. It is then clear that n encodes N and no other property that is not extensionally equivalent to N.

According to (2) and (5), for n to be a mathematical object, it has to be the case that

$$\forall F\ (nF \Leftrightarrow T \models Fn),$$

for some T, and, as it is the case that nN, this entails that $T \models Nn$, for these T.

It follows that for n to be a mathematical object, there has to be a mathematical theory T where n is N, and where n is F only if F is extensionally equivalent to N. Then the triangle or the natural number are mathematical objects only if there is a mathematical theory in which they are a triangle and a natural number and have only the properties that are extensionally equivalent to these last properties.

Thus, even if–according to Linsky and Zalta's construal of the notion of mathematical object–there is one and only one abstract object for any property, it is a mathematical theory that decides whether, for a certain property N that is exemplified in it, there is or is not the object that is just the N.

This fits quite well with the idea that the notion of mathematical object is a fundamental notion of philosophy of mathematics in my sense, that is, an interpretative and not a normative notion.

But, the formal machinery apart, what are the essential conclusions of Linsky and Zalta's analysis of the notion of mathematical object?

Linsky and Zalta present some of them in the following ways (1995, p. 25):

[. . . ] there is no distinguished 'model-theoretic' perspective to tell us what are the 'objects of' a theory T. [. . . ] the objects of a theory are the ones described by its *de re* claims, for these attribute properties to objects. Note that the statement '$\exists x Px$' counts as a *de re* claim about a property P, but that it doesn't count as a *de re* claim about mathematical individuals. From $T \models \exists x Px$, we can validly infer $\exists F\ (T \models \exists x Fx)$, but we can't validly infer $\exists x\ (T \models Px)$[13].

Knowledge of particular abstract objects doesn't require any causal connection to them, but we know them on a one-to-one basis because *de re* knowledge of abstracta is by description. All one has to do to become acquainted *de re* with an abstract object is to understand its descriptive, defining condition, for the properties that an abstract object encodes are precisely those expressed by their defining conditions. So our cognitive faculty for acquiring knowledge of abstracta is simply the one we use to understand the comprehension principle[14].

---

*13 Ibid., pp. 233. I have omitted type specfications in Linsky and Zalta's formulas.*
*14 B. Linsky and E. N. Zalta, op. cit., p. 547.*

It thus seems that, according to Linsky and Zalta, mathematical theories are corpora of statements (or propositions) closed under logical consequence and their objects are nothing but those that are named by individual constants of any type that appear in these corpora. Each of these statements in which individual constants appear is moreover understood as a *de re* claim about the objects of this theory that these constants denote, and these objects are taken to exist within the theory.

Moreover, such a *de re* claim is the expression of *de re* knowledge, since it is assumed that the relevant individual constants have been introduced by appropriate descriptions, or are appropriate descriptions like '$\iota x$ (φ)', and 'de re knowledge of abstracta is by description'.

If I understand well, this means that the statement

's knows that T $\models$ Pα'

has to be analysed as

'of the object α of T, s knows that it is P in T',

or perhaps as

'in T, s knows of α that it is P'.

5.4

When applied to particular axiomatised theories, Linsky and Zalta's analysis leads to conclusions that, formal and linguistic subtleties apart, can be compared with the conclusions that Shapiro's structuralism leads to. The slogan `a mathematical object is a place in a structure' becomes, 'a mathematical object is the abstract object that encodes the properties that the theorems of the theory it belongs to assign to it'.

The explicative power of the notion of structure is here replaced by the notion of encoding. Whereas the former is implicitly defined by Shapiro's theory of structures, the latter is implicitly defined by a system of metaphysical axioms that apply to any sort of abstract objects.

The result is that mathematical theories replace structures, but they are understood as kinds of stories, whose peculiarity depends on the fact that the statements that compose them are mathematical. But as the predicate Math is primitive, this is in no way an explication.

Nor is the fact that the objects of a mathematical theory encode exactly the properties they exemplify in it an explication, since in this way the notion of mathematical object depends on that of a mathematical theory.

To get back to conclusions similar to those of Shapiro's structuralism, we have thus to limit ourselves to axiomatic mathematical theories and admit that we already know how they are constituted.

The advantage of Linsky and Zalta's account is that it can easily be extended to mathematical theories that are neither axiomatic, nor founded on a recursive clause understood in the usual way.

But if the term 'theory' is used here in my sense rather than in Linsky and Zalta's or, more generally, in the usual sense that identifies a mathematical theory with a corpus of statements, some relevant specifications have to be made.

The main one is that the relevant corpus of statements should not be considered closed under any relation of consequence or derivation. This suggests a rejection of Linsky and Zalta's rule of closure for mathematical theories.

But if this is done, does it remain plausible to speak of truth? If the notion of truth in a theory is defined as Linsky and Zalta suggest, in terms of encoding, that is, as a purely technical notion, this question is merely terminological. One could argue, however, that it would be better not to speak of truth in a theory, which could be easily done by unpacking 'T $\models$ p' as 'T[λy p]', that is, by simply abandoning definition (4).

Still, we have also seen that in Linsky and Zalta's language, the statement 'T[λy p]' is a formal version of 'T asserts p'. Would it not then be possible, then, to avoid encoding and merely admit that mathematical objects are what singular terms occurring in the statements belonging to an appropriate corpus of statements stand for?

This results by replacing 'appropriate true statement (or [. . . ] appropriate statement that may warrantedly be claimed to be true)' with 'the statements belonging to an appropriate corpus of statements' in claim (O.i$x$), that derives, in turn, from a relativisation to a mathematical theory of the neo-logicist understanding of Frege's context principle as applied to the case of natural numbers[15].

The general idea behind this replacement is that, when mathematical reference and nowledge are concerned, the neo-logicist understanding of Frege's context principle can be conserved by replacing the requirement of truth with some other suitable requirement of appropriateness for the relevant corpus of statements.

Of course the neo-logicist understanding of Frege's context principle is radically modified through such a replacement, and, supposing that the new requirement has no other special virtues of logicity, this goes together with the abandonment of any neo-logicist foundational perspective. But this is not a worry for me, since such a perspective is completely foreign to my proposal.

The relevant question is diffeerent: what new requirement has to be adopted?

Apart from its formal metaphysical tricks, Linsky and Zalta's account suggests to adopt the requirement of appurtenance to a mathematical theory.

By relying on the notion of mathematical theory that I have previously introduced, this could be done as follows.

Let T = (S, R, S, A) be a mathematical theory. Let us say that a statement is an objectually relevant statement of T if and only if it is a statement of S, or it is a statement belonging to S that has the same form of some statements of S, that is, it has the same form as a statement that can be derived according to the stipulations included in S.

One could then argue that mathematical objects are what singular terms occurring in objectually relevant statements of some theory T stand for.

Once the corpus of the objectually relevant statements of a theory T is established, a technical notion of truth in this theory can of course be defined and truth can be reinserted in

*15 Cf. B. Hale and C. Wright, "Benacerraf's Dilemma Revisited", European Journal for Philosophy, 10, 2002, p. 115. Cf. also: C. Wright, Frege's Conception of Numbers as Objects, Aberdeen Univ. Press, Aberdeen, 1983, p. 14, and many other passages occurring in several papers, most of which are collected in B. Hale and C. Wright, The Reason's Proper Study. Essays Toward a Neo-Fregean Philosophy of Mathematics, Clarendon Press, Oxford, 2001.*

the analysis. But, once again, this would be no more than internal truth, which could be contrasted with external truth, defined by generalising the definition that applies to structures.

The problem of establishing the corpus of the objectually relevant statements of a theory T would of course be far from simple. But it is not a general philosophical problem. It depends on particular historical analyses and reconstructions. This is also the case for the more general problem of establishing the four components of a theory T, and thus the domain of its objects.

Hence, the difficulty of these problems is not a good reason for rejecting the previous construal of the notion of mathematical object.

There is another difficulty: if mathematical objects are described in this way, and the corpus S is not closed under an appropriate relation of consequence, the domain of mathematical objects of a theory is limited by our finite linguistic resources.

According to the previous perspective, a mathematical object is something that is named or individually described, and all eternity is not enough for even a countably infinite set of names and individual descriptions to be made available. Should we conclude that there is only a finite number of mathematical objects?

This would be a rather odd conclusion. I see two possible solutions that avoid it.

I am not really proposing the first one. It would consist in supplementing the corpus S of statements included in any theory T = (S, R, S, A) with, as it were, a potential extension obtained by closing it according to a suitable relation of consequence. Though the new corpus of statements $S_0$ so obtained would not be, strictly speaking, part of the theory, one could then admit that the objectually relevant statements of T include all the statements of $S_0$.

The resulting situation would be similar, mutatis mutandis, to those attached both to Linsky and Zalta's and to neo-logicist construals of the notion of mathematical objects. A mathematical object would be something that is, as it were, potentially named or individually described.

The second solution is the one that I propose. It consists in distinguishing between two essentially diffeerent sorts of mathematical theories. The first sort includes axiomatic theories whose axioms provide an implicit definition of a domain of objects and theories founded on a recursive clause understood in the usual way. The second sort includes non axiomatic theories (or axiomatic theories where axioms do not provide implicit definitions), as Euclid's plane geometry, where objects are (more or less) explicitly defined in general, then introduced or given individually through appropriate procedures that include a designation of them through appropriate names or individual descriptions.

For theories of the first kind, one could assume that the domain of the objects of a theory T is simply what is implicitly defined by the axioms of T or potentially established by its recursive clause, then add that some of 30 these objects are what singular terms, occurring in the objectually relevant statements of T, stand for. Under my understanding of this construal, the objects of these theories could also be identified with places in structures, provided the notion of structure be appropriately adapted. They form, in any case, a genuine domain of quantification.

For theories of the second kind, the notion of a domain of objects has to be understood differently. The domain of objects of such a theory is not a range of quantification, that is, a set of well distinguished elements, some of which are named or described individually, whereas others are only supposed to exist. It is rather the domain of application of, so to speak, a partially sortal concept: a concept characterised by a (more or less) well defined condition of application, but such that the conditions of identities of the items falling under it depend on the possibility of a particular individuation, by means of a name, an individual description, a diagram, etc.

In this last case, the domain of objects of a theory certainly includes objects that singular terms, occurring in the objectually relevant statements of this theory, stand for. But this inclusion does not comply with the settheoretic notion of inclusion. These objects are not picked out from a set in which they are already supposed to be distinguished. Like points on a line, they are distinguished only when they are picked out, and thus named, described, represented, etc.

One could then say that, taken individually, any object of such a theory is the item that a singular term, occurring in the objectually relevant statements of T, stands for. But taken in their totality, the objects of such a theory would simply be the X's, where X is an appropriately defined concept.

To say that a similar theory deals with objects is moreover not the same as asserting that some abstract objects exist and that they are objects of this theory. It rather means that the procedures that are authorised by the stipulations of this theory are apt to generate objects and identify them in the context of a particular argument or proof, by distinguishing them from any other object considered in this same argument or proof.

The objects of such a theory are then the items that a singular term, occurring in the objectually relevant statements of it, stands for only in the context of a single argument or proof. The only form of universality which can be attained in this way would thus depend on the stability of procedures authorised by the stipulations of the theory.

## 6

A last remark before nishing. Suppose that α is an objects of a theory T of the first sort, or an object of a theory T of the second sort identified in the context of a single argument or proof. Are Linsky and Zalta right in arguing that a statement that assigns a property to it in T has always to be understood as a *de re* claim about α, and that it is the expression of *de re* knowledge?

I think they are not. I agree that if there is something like *de re* knowledge of abstracta, then this is by description, however the general notion of an abstract object is understood. But is there such a sort of knowledge?

Consider the statements 's knows that in T it is the case that Pα', and analyse it as 'of the object α of T, s knows that it is P in T'. The object α of T is something that a description or a name stands for. But in order to have a *de re* knowledge of it, it is certainly not enough to know this name or this description.

It is best to proceed slowly. The statement 's knows that in T it is the case that Pα' is certainly not a statement of T, and even if T is not a formal theory, it should be possible to admit that it is a statement of a language L that is not the (or a) language of T. 'α' is then a singular constant of L but it is not a name or a description that refers to α in T. Let us suppose 'α' and '$\iota x\ (\alpha_x)$' to be instead, respectively, a name and a description of α in T. And suppose that s understands the language(s) of T, and is perfectly familiar with this name and this description. Suppose also that s is acting (thinking, calculating, arguing, etc.) in T–that is, s is performing part of the amount A of activity of T, and is perfectly aware of it.

We can thus eliminate the prefix 'in T' and admit merely that s knows that Pα or that P$\iota x$ ($\alpha_x$) (for the sake of simplicity, suppose that 'P' denotes the same property in L and in the (relevant) language of T). The question is thus whether it is admissible to argue that s knows of α or $\iota x$ ($\alpha_x$) that it is P: if it is so, the knowledge is *de re*, otherwise is *de dicto*.

α is that which 'α' or '$\iota x$ ($\alpha_x$)' stand for in the statements 'Pα' or 'P$\iota x$ ($\alpha_x$)' of T. But this does not clarify what it is. It is simply a way to argue that the terms 'α' or '$\iota x$ ($\alpha_x$)' of the (relevant) language of T refer, and thus, that the statements 'Pα' or 'P$\iota x$ ($\alpha_x$)' of T can be analysed as claims about an object, and so that mathematical activity and knowledge can be understood as activity on objects and knowledge about objects.

But is this knowledge a knowledge of objects, that is, is it *de re*?ظ

According to Linsky and Zalta:

$\alpha = \iota x\, (A! \wedge \forall F\, (xF \Leftrightarrow T \models F\alpha))$ :

Let us assume that this is the right way to describe a. Still, this description is available in t, and it is quite implausible that s, though perfectly able to understand Linsky and Zalta's comprehension principle, has clear knowledge of what makes that $\forall F\, (xF \Leftrightarrow T \models F\alpha)$: this would require s to have clear knowledge of all the properties that a has and does not have in T!

Thus, it is certainly not because s knows Linsky and Zalta's construal, agrees with it, and understands their comprehension principle that knows α. Linsky and Zalta's argument seems thus to be simply ineffectual.

But this is not the same as arguing that mathematical knowledge cannot be *de re*.

In order to know of something that it has a certain property (assuming this is different from knowing that this something has this property), it is necessary to have some form of acquaintance with this object that does not depend on its having this property. Thus, I argue that in order to have *de re* knowledge of abstracta, it should be necessary to have some form of acquaintance with them that does not depend on their having at least some of their properties.

Let us suppose this is so for the abstract object a, and that P is a property like these and that α is P. Then we could have a form of acquaintance with α that does not depend of its having P. We could then be brought to know that α is P, and this would be, I claim, genuine *de re* knowledge about α.

The question is thus whether it is possible to have a form of acquaintance with mathematical objects that does not depend on their having at least some of their properties, and whether s has this form of acquaintance with α.

It seems to me that the answer to the former question is 'yes', and the answer to the latter is 'it depends on the role that the terms that denote α in T play in this theory'.

I have quite a simple argument in favour of the first answer. If such a form of acquaintance with mathematical objects were impossible, it would also be impossible to have a clear criterion to decide whether a mathematical problem of the form 'which are the objects that are so and so?' has been solved. But mathematical activity is certainly concerned with such problems. Take for instance: 'look for a root of the equation $x^2 + 1 = 0$', which asks in fact for the determination of the objects that satisfy this equation. Moreover, mathematicians are normally able to recognise without any doubt that such a problem, when advanced in an appropriate theory, has been solved, if indeed it has been, and this means that they are able to identify an object they are acquainted with independently of its being a root of such an equation.

The answer to the second question thus depends on whether ‘α’ or ‘$\iota x$ ($\alpha_x$)’ are expressions of such a form of acquaintance. If they are, then s knows of α or $\iota x$ ($\alpha_x$) that it is P, and otherwise s merely knows that α or $\iota x$ ($\alpha_x$) is P. Of course this cannot be decided in general. It depends on ‘α’ or ‘$\iota x$ ($\alpha_x$)’ and T, and namely on the nature of the activity that is attached to T.

It seems to me that it is just because *de re* mathematical knowledge is possible but mathematical knowledge is not necessarily *de re*, that the notion of mathematical object is crucial to an understanding of the nature of mathematical knowledge.

But if this is so, and I’m right in my account, then it is mathematical activity, or, if you prefer, mathematical practice which decide whether mathematical knowledge about certain objects is *de re* or *de dicto*. Then the question of the nature of mathematical knowledge is not purely an abstract epistemological question: it is a question about mathematical activity, that is, in the final analysis, about history of mathematics.