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## Toward a Hermeneutic Categorical Mathematics

or

why Category theory does not support mathematical structuralism

### 1. Mathematical Interpretation

#### a) *Hermeneutics of the Pythagorean theorem*

In his (1883) and a number of later writings W. Dilthey urges the autonomy of humanities (Geisteswissenschaften)<sup>1</sup> from natural sciences (Naturwissenschaften). Dilthey's principle argument is methodological: objecting against attempts of Comte and Mill to extend scientific methods to moral, political and other humanities issues Dilthey purports to constitute the autonomy of humanities by providing them with a proper methodology independent from that of natural sciences. An important role in the Dilthey's methodology of humanities plays his notion of *hermeneutic understanding*, viz. an understanding achieved through an interpretation, which Dilthey distinguishes from understanding achieved through a scientific explanation. As it has been noticed already by Husserl (1954) and recently stressed by Brown (1991), Crease (1997) and Salanskis (1991) hermeneutic issues are in fact not less important in natural sciences and mathematics than in humanities. A straightforward evidence of the relevance of understanding through interpretation in mathematics comes from the usual school practice: what counts as a genuine understanding (as opposed to mechanical memorising) of a given mathematical fact by a pupil is his or her capacity to formulate and prove it in his or her "own words" and apply it in a new unexpected situation. Obviously in a research environment the variability of forms of expressions of mathematical contents is even higher.

How this variability allows for a stable translatable mathematical content (meaning) and how precisely this phenomenon can be described? I think that this question has been so far very little studied. The question is not really specific for mathematics and can be easily reformulated as a problem of general theory of meaning. However the case of mathematical meaning is particularly interesting because in this case we have a better chance to solve the

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<sup>1</sup> The term "Geisteswissenschaften" has been earlier suggested by Gernon, the first German translator of Mill's *System of Logic* (1843), as translation of Mill's "Moral Sciences".

problem by rigor mathematical methods. As far as the problem is solved inside mathematics one can think about application of the obtained mathematical solution elsewhere<sup>2</sup>.

It might be argued that a mathematical content shouldn't be confused neither with cognitive and social activities through which this content is "proceeded", nor with various symbolic and linguistic *forms* in which this content is expressed and communicated. It might be further argued that once a mathematical content is considered on its own rights the hermeneutic issues become irrelevant. In the next paragraph I shall show that the latter claim is false since there are important issues, which can be described both as "purely mathematical" and hermeneutic. In the present paragraph I shall show that the former claim is problematic even if not plainly false since a mathematical content cannot, generally speaking, be easily separated from its form of expression in a way making the form of expression mathematically irrelevant.

Think about the Pythagorean theorem. As formulated in (Lang&Murrow 1997, p.95) the theorem says this:

(LM) Let  $XYZ$  be a right triangle with legs of lengths  $x$  and  $y$ , and hypotenuse of length  $z$ . Then  $x^2 + y^2 = z^2$ .

(Doneddu 1965, p.209) under the title *Pythagorean theorem* states the following (my translation from French):

(D) Two non-zero vectors  $x$  and  $y$  are orthogonal if and only if  $(y-x)^2 = y^2 + x^2$ .

Finally the famous proposition 47 of Book 1 of Euclid's *Elements* states this (hereafter I quote Euclid by Heath's translation (1926):

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<sup>2</sup> Brown certainly goes too far when he says:

"Any notion of a correct universal meaning does not arise within hermeneutic understanding. The way in which an expression is seen and used is always in a state of flux, being modified as the life experience of the individual affects the contexts in which it is seen as being appropriate."

Perhaps some reservations about the idea of "universal meaning" should be indeed made (as also suggested by my analysis of the Pythagorean theorem below in the main text) however the very fact that the "flux" that Brown talks about involves amazingly stable structures is just too obvious. This fact is not incompatible with Brown's dynamic approach (which I think is basically correct) but shows a non-trivial character of the mathematical conceptual dynamics. I would like also to notice that even if this dynamic approach is not metaphysically neutral it doesn't imply the radical social constructivism, to which Brown adheres in his (1994), but squares equally well with broadly empiricist views on mathematics.

(E) In right-angled triangles the square on the side subtending the right angle is equal to the squares on the sides containing the right angle.

Are (LM), (D) and (E) different forms of the same theorem? An attentive reader will immediately answer in negative pointing to the fact that (D) comprises the proposition usually called the *converse* of the Pythagorean theorem. This Donnedu's terminological decision I leave without commenting but ask the same question exchanging (D) for its *only if* part (which I denote (D')) for further references). Now a plausible answer is *Yes, of course!* However obvious might be the answer, particularly in a mathematician's eyes, let's make some basic hermeneutics about (LM), (D'), (E) and read these propositions carefully before making any decision. Obviously each of the three propositions can be correctly interpreted only within a larger theory. Fortunately all the three books, from which I took the quotations, are elementary textbooks and so require no or very little previous mathematical knowledge. So in each of the three cases it is clear what is the corresponding larger theory. To simplify my task I shall skip almost everything concerning *proofs* of the theorems and discuss only their statements.

Lang&Murrow in their book for beginners provide a fairly minimalist conceptual basis for their version of the Pythagorean theorem: before learning the theorem a student is supposed only to habituate him- or herself to basic geometrical constructions like triangles and learn the notion of *length* of a given straight segment. The latter notion reduces in the Lang&Murrow's book to the notion of *distance* between two given points. Lang&Murrow introduce the notion of distance through informally stated axioms of metric space and occasionally mention that distances are *numbers* one reads off from a graduated ruler. What a smart kid should think given two different rulers one of which is graduated in inches and the other in centimetres? The unwillingness of the authors to elaborate on this point is understandable since they commit themselves to keeping the Pythagorean Secret (not to be confused with the Pythagorean theorem) out of the reach of their students. I mean the incommensurability problem. Following the teaching strategy, which the legend attributes to Pythagoras himself, Lang&Murrow like most of authors of today's elementary mathematical textbooks reserve the truth about incommensurability for those of older students who choose to study mathematics at an advanced level and are capable not only to see the problem but also treat it by a modern remedy.

(Donnedu 1965) shows how the remedy may look like. This textbook applies Bourbaki's "architectonic" principles: it starts with making up a Boolean set-theoretic framework, then

develops on this basis a theory of real numbers, and only after that comes to geometrical issues construing the Euclidean space as a vector space over the field of the reals. Formula  $(y-x)^2 = y^2 + x^2$  requires an accurate interpretation: the minus sign on the left denotes the subtraction of *vectors* while the plus sign on the right denotes the sum of real numbers (so the two signs do *not* denote here reciprocal operations as usual); the square on both sides is understood in the sense of the scalar product of vectors. We see that the price of the rigor is quite high: the Donnedu's version of Pythagorean theorem requires from the pupil a much more serious preparatory work, and after all the theorem doesn't explicitly refer to any triangle at all<sup>3</sup>! Euclid's classical presentation of the Pythagorean theorem depends, of course, on principles laid out in the beginning of the first book of the *Elements*. An extended historical comment on this theorem wouldn't be appropriate here, so I shall stress only one point often presenting a difficulty for a modern reader. (E) says that the bigger square equals to the smaller squares. How to understand this? An interpretation that immediately comes to mind is this: the *area* of the bigger square equals the *sum of the areas* of the smaller squares. But this is certainly *not* what Euclid says. Euclid speaks here about equality of figures, not about equality of their areas. In spite of the significant differences between Euclid's and Hilbert's axiomatic methods stressed in what follows, it is safe to think about the relevant notion of equality as formally introduced through Axioms of the first Book of the *Elements*. (I follow Heath saying this. I shall argue in what follows that the principle difference between Euclid's and Hilbert's methods concerns Euclid's Postulates but not the Axioms<sup>4</sup>). The Axioms give this: (i) equality is transitive (Axiom 1; symmetry of this relation is granted by the linguistic form, in which the Axioms are expressed); (ii) congruent figures are equal (Axiom 4); (iii) figures, which can be composed out of equal figures or complemented to equal figures, are equal (Axioms 2,3). Clearly Euclid's equality is a binary relation, so when he says in 1.47 that one square equals two other squares it is puzzling. The solution of the puzzle is this: consider the union of the two smaller squares as one relatum, and the bigger square as the other relatum. Remark that (iii) applies to the case of topologically disconnected figures like the union of two squares. So using Axioms 1-4 and a number of preceding theorems Euclid proves his version of the Pythagorean theorem. The equality of areas, of course, implies the Euclidean equality just

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<sup>3</sup> This is a reason why mathematical textbooks like Donnedu's written in 60-70-ies in order to apply the Bourbaki's standard (or some its mild version) in the school practice are today only rarely in use. See (Kline 1974).

explained. The converse, however, doesn't always hold. It doesn't hold, for example, for circles: given two smaller circles such that the sum of their areas is equal to the area of a bigger circle the bigger circle is obviously not equal in the Euclidean sense to the two smaller circles. A less trivial mathematical fact is that polyhedra of the equal volume are, generally speaking, not equal in the Euclid's sense either.

This short exposition of (LM), (D') and (E) is by far sufficient for claiming the obvious: although there is a sense in which all the three propositions express the "same theorem", the differences between them are quite significant from a mathematical viewpoint. So the claim that the three propositions say "essentially the same thing" cannot and shouldn't be taken as obvious. One who seeks to sweep the issue of interpretation out of mathematics might probably take now a different strategy and claim that, say, only (D') represents the Pythagorean theorem in its correct form while (E) is hopelessly outdated and (LM) is a simplified account for kids. Then it may be argued that the problem of how to translate between (LM), (D') and (E) belongs to the history of mathematics and to the theory of mathematical teaching but not to the pure mathematics. Perhaps a sufficient purification of the meaning of "mathematics" can make this view tenable. But I don't think that such purification would be reasonable. The fact that certain mathematical facts like the Pythagorean theorem survive during millennia through very different conceptual settings and reappear in new forms seems me very significant. A formal analysis fails to account for this long-term stability of mathematical concepts just like it fails to account for the fast conceptual dynamics of a mathematical classroom. (LM), (D') and (E) are called by the same name of the Pythagorean theorem not only for historical reason. There is a genuine *mathematical* reason for this. However it would lead me too far if I elaborate here further on mathematical contents of different versions of the Pythagorean theorem. Instead I shall try to answer this general hermeneutic question: How the claim that given mathematical propositions A, B "say the same thing" can be possibly justified?

Consider the notion of logical equivalence first. May one generally look at logically equivalent mathematical propositions A,B (i.e. propositions which imply each other) as different expressions of the same mathematical fact? Obviously not. Equivalent mathematical propositions may "mean" very different things, so the equivalence of two given mathematical propositions may be quite non-obvious. For example in the traditional Euclidean setting the

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<sup>4</sup> Euclid calls these propositions not "axioms" but "common notions". The term "axiom" is used by Aristotle; in particular Aristotle qualifies Euclid's "common notions" as axioms. So this identification is apparently unproblematic.

theorem saying that the sum of internal angles of any triangle is equal to two right angles is equivalent to the Fifth Postulate (“Axiom of Parallels”). But this cannot be seen immediately without a proof: the two propositions *mean* different things. So the logical equivalence is not sufficient for the purpose. But is it necessary? Given that A, B are “different forms of the same theorem” does it follow that A, B are logically equivalent (in symbols  $A \Leftrightarrow B$ )? This might sound like a reasonable requirement but looking at our example of the Pythagorean theorem one would wish to relax it. For given A, B one cannot assert and moreover prove  $A \Leftrightarrow B$  unless A, B belong to the same theory. But our (LM), (D’), (E) all belong to different theories! Let’s see whether such a common background theory C can be acquired. Recall that (LM), (D’), (E) as they stand cannot be separated from their mother theories without changing their meanings. So the wanted background theory C should incorporate not only the three propositions but also the three corresponding theories or at least relevant parts of these theories. Some *bricolage* of this sort can be certainly made up (usual school geometry textbooks provide many such examples) but one cannot seriously believe that a combination of a Bourbaki-style set-theoretic framework with Euclid’s *Elements* could give a viable mathematical theory. Instead of combining theories treating differently the “same subject” (in a sense that we are still looking to define) it is much more reasonable to try to *interpret* theories in each other’s term. So let’s leave the notion of logical equivalence aside and consider instead how mathematical propositions belonging to different independent theories may *translate* to each other.

Remark that a bare claim that, say, (LM) translates to (E) and/or vice versa doesn’t explain anything and can provide no additional support for the idea that (LM) and (E) express the same theorem. But how the claim that A translates to B (in symbols  $A \rightarrow B$ ) can be justified at all? Let’s consider a linguistic analogy. What kind of justification except an appeal to authority can be provided for the claim that the Latin phrase *cogito ergo sum* translates into the English phrase *I think therefore I am*? In order to apply this linguistic example to our mathematical problem we need to imagine a person who has a good command both in Latin and English but doesn’t know how exactly to translate between the two languages. This situation is less unusual than it might seem: to have a good command in languages is necessary but certainly not sufficient for being a good translator. Similarly one may well understand both Euclid and Doneddu and have a strong feeling that sometimes the two authors touch upon the same subject matter but be nevertheless unable to make any reasonable translation between the two accounts. In order to avoid such schizophrenic situations let’s think of two persons instead of one: an Englishman who doesn’t understand

Latin and a Roman who doesn't understand English. The Englishman is trying to translate the Roman's saying *cogito ergo sum*. The Roman's role is this: when he hears a meaningful response from the Englishman he continues the dialog, else he remains silent. The question of what exactly the Roman counts as meaningful is, of course, crucial for this language game: let the Roman be liberal about this but asking for something more than merely syntactic correctness. The Englishman is aware about the basic linguistic convention just mentioned, so he can always check with the Roman whether Latin expressions he's trying to construct are meaningful or not.

Now suppose that by a miracle (or memory of his university courses) the Englishman guesses correctly the English translation of *cogito ergo sum*. Moreover he guesses correctly the following detailed translation:

*cogito* → *I think*

*ergo* → *therefore*

*sum* → *I am*.

At this point an experimental check of the guess becomes possible. The Englishman observes that the phrase *I think therefore I am* allows for an rearrangement of its elements bringing another meaningful English phrase: *I am therefore I think*. The reason why this transformation is possible is in fact quite profound: both English phrases have the same logico-grammatical structure, which allows for transformation of *X therefore Y* into *Y therefore X* (this transformation is obviously not truth-preserving and not meaning-preserving but it preserves meaningfulness). The Englishman hypothesises that Latin has the same structural property and tries *sum ergo cogito*. It works and so the Englishman gets from the Roman a new linguistic material for continuing the game. If the Englishman makes enough correct hypotheses the game may turn into a genuine conversation in Latin. Remark however that the following erroneous tentative translation

*cogito ergo sum* → *I am therefore I think*

specified as

*cogito* → *I am*

*ergo* → *therefore*

*sum* → *I think*

passes through the same check. The error may be revealed if an Englishman's attempt to construct another Latin phrase using *sum* for *I think* or *cogito* for *I am* produces a sheer nonsense.

The moral of this example is this. To construct (not just *stipulate*) a translation between two mathematical propositions  $A, B$  belonging to corresponding theories  $T_A$  and  $T_B$  one has to look both inside and outside the given propositions. Looking inside  $A$  and  $B$  one distinguishes elements of both and specifies which translates to which. This, generally speaking, can be done in many different ways. Outside  $A$  and  $B$  one looks for other propositions  $A', B'$  built out of the same elements and translates  $A', B'$  elementwise. This may rule out certain hypothetical translations like in the above linguistic example. Remark that meaningfulness in any given language can be defined differently (more or less liberally). In mathematics too one may make different requirements concerning what a correct translation is supposed to “preserve”. Obviously any translation of mathematical propositions should preserve truth-values or at least it should always translate true propositions into true propositions. (This requirement doesn’t allow for replacement of translation by logical implication.) Extending in this way the domain of translation one might ultimately get one or few global translations  $T_A \rightarrow T_B$ . One may also encounter a situation when no extension of the domain of translation is possible. In this latter case there is no other solution but consider the translation  $A \rightarrow B$  as primitive. Generally, a given translation  $A \rightarrow B$  is extendable up to translation  $T_A' \rightarrow T_B'$  between certain fragments of theories  $T_A$  and  $T_B$ . The reader can easily see that this is the case for any reasonable translation between (LM), (D’) and (E). The replacement of Euclidean squares by squares of real numbers generalises upon the case of rectangles in the obvious way. This allows for a uniform translation of Euclidean propositions of the type of theorems of the Book 2 of *Elements* but hardly for more than that. Although this extension of the domain of translation is very limited it justifies the claim that the translation rule in question is meaningful.

Unless one figures out a space of possible translations between (LM), (D’) and (E) and their corresponding theories (which can be called a *hermeneutic space*) the question whether these propositions tell us the same thing or different things can be hardly reasonably answered. In fact to describe such a space seems to be more important than to give a yes-no answer to the above question, which in any event is a matter of convention. (Obviously the mere existence of translations between propositions  $A, B$  is not sufficient for considering these propositions as identical: one should rather require the existence of *reversible* translations of a certain kind.) I hope that I have shown that even in elementary cases like that of the Pythagorean theorem the issue of translation between different mathematical setting is not quite trivial. For a more involved example of the same type think about Bourbaki’s seminal work (1939 -). Today one can hardly seriously claim that Bourbaki revealed to the rest of mathematical community *the*

way in which one must do mathematics. He rather showed *a* way of doing mathematics, which is good for certain purposes and not good for certain other purposes. It seems that we still lack is a precise mathematical account of what the “bourbakization” of mathematics exactly amounts to: how a given mathematical theory translates from its original form to its bourbakized form. The same applies to *categorification* of mathematics discussed in what follows, and to any other past or future project of reconstruction of the whole of mathematics on a new conceptual basis. Although the categorification can be rightly viewed as one project of the kind among many others I shall show in what follows that it also provides a natural framework for treating the meta-problem just posed.

To conclude this paragraph let me stress what I see as the principle epistemological impact of the hermeneutic approach. A theory (mathematical or not) can be thought of as an umbrella embracing a plurality of concepts, facts, etc. and making these things into one structured whole – and built accordingly. Alternatively a theory can be thought of and built as a network of coherent translations between different viewpoints<sup>5</sup>, individual experiences, cognitive and linguistic activities, etc. (recall the mathematical classroom). The hermeneutic approach corresponds to the latter option. We shall see that the hermeneutic approach to theory-building squares well with the ongoing project of categorification of mathematics.

#### *b) Imaginary geometry of a hilly terrain*

Hermeneutic issues about mathematics discussed in the previous paragraph involved a historical and educational dimension. Now I shall show how the notion of interpretation gets involved in mathematics in a more abstract manner and becomes “purely mathematical”. The term “interpretation” appears in the title of Beltrami’s paper of 1869 *Saggio di interpretazione della geometria non-euclidea* which in eyes of many people first showed that the non-Euclidean geometry was something “real”. However the history of interpretation as a mathematical concept starts not with this paper but sometime in 1820-ies (if one makes a reasonable distinction between the history and the corresponding prehistory). Interestingly (but perhaps not so surprisingly) interpretation became a genuine mathematical issue during the same period of time (and mostly in the same part of the Europe) when Schleiermacher, Dilthey and their followers stressed the role of interpretation in humanities. This emergence of the “interpretative mathematics” in 19-th century is, in my view, crucially important for an

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<sup>5</sup> Think about relativistic theories in physics beginning with Classical Mechanics.

adequate understanding of history of mathematics of 20-th century and of the present situation in this discipline. So let me shortly recall the history.

As it is well known Lobachevsky discovered the non-Euclidean geometry presently called by his name through “playing with axioms”, namely through the replacement of Euclid’s Fifth Postulate (“Axiom of Parallels”) by its negation. Like his Ancient and Modern predecessors Lobachevsky hoped to get a contradiction and hence a proof of the Postulate. However like Bolyai and few other people working on the problem around the same time Lobachevsky at certain point changed his attitude and came to the conviction that he explored a new vast territory rather than approached the desired dead end. He called this new geometry *Imaginary* (Lobachevsky 1837) because of the speculative character of his enterprise and probably as a precaution: if his theory would turn after all to be contradictory he would win anyway getting the wanted proof. Nevertheless Lobachevsky seriously considered a possibility of using astronomical observations for checking whether the physical space is Euclidean or not. Moreover he rightly noticed that the Euclidean hypothesis can be possibly falsified but not definitely verified through observations because of limited accuracy of astronomical measurements. Lobachevsky’s epistemological stance with respect to this question fits very well a broadly positivist view on relationships between science and mathematics according to which mathematics is a domain of a pure speculation while application of mathematics in sciences and technology is an empirical matter.

While Lobachevsky made his discoveries following a traditional line of research Gauss got a totally different insight on the problem. Although Gauss’ name hardly needs an additional promotion in the history of mathematics, I claim that his role in the discovery of non-Euclidean geometries is often misinterpreted and underestimated (like in Bonola 1908). After reading Bolyai’s paper (1832) Gauss claimed that he found for himself nothing new in it, and this claim is at least partly confirmed by existing evidences. He made then a controversial remark that “to prise it (the Bolyai’s paper – *AR*) would mean to prise himself” (Bonola 1908). Historians often explain this Gauss’ reluctance by personal and sociological reasons or by his alleged epistemological conservatism. I think that Gauss’ cautious attitude to Bolyai’s and Lobachevsky’s results was quite justified and so it doesn’t need any non-mathematical explanation. Gauss didn’t share Lobachevsky’s notion of geometry as a speculation but considered it as an empirical science<sup>6</sup>. For this reason he was very sceptical about the whole

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<sup>6</sup> “...wenn die Zahl bloss unsers Geistes Product ist, der Raum auch ausser unsern Geiste eine Realitaet hat, der wir a priori ihre Gesetze nicht vollstaendig verschreiben koennen.” Gauss’ letter to Bessel 9 April 1830; *Werke*,

line of research that led to Lobachevsky's discoveries (known at the time as the "theory of parallels"). This Gauss' attitude had nothing to do with conservatism: on the contrary, in Gauss' eyes the theory of parallels was too *traditional* and missed really new ideas<sup>7</sup>. From the today's historical distance it is easy to argue that Gauss was perfectly right, and that his insights clearly spelled out by Riemann in 1854 played more important role in the change of views on space it time occurred in the 19-20-th century than the whole story about the Fifth Postulate. However one had to be a mathematician of the rang of H. Weyl to see this clearly already in 1918:

"The question of the validity of the "fifth postulate", on which historical development started its attack on Euclid, seems to us nowadays to be a somewhat accidental point of departure. The knowledge that was necessary to take us beyond the Euclidean view was, in our opinion, revealed by Riemann."<sup>8</sup>

Given that Riemann's concept of manifold provides the mathematical basis of the today's best theory (or theories) of space and time, and so replaces in this role Euclidean space of Classical mechanics, Weyl's point is hardly disputable. That Riemann's geometrical works are directly based on Gauss' is not disputable either. What makes it difficult for a part of historians to appreciate Gauss' and Riemann's contribution is apparently the fact that the mathematical work of these people doesn't fit the popular story about liberalisation of mathematical thought from its alleged stickiness to everyday spatial experience by Lobachevsky<sup>9</sup>. Riemanean geometry just like Euclidean geometry about two and a half millennia earlier has been first sketched on the ground by Gauss and only after that worked out in a more abstract form by Riemann and correctly applied by Einstein to Heavens. What triggered this development was a new attentive look at the space we live in rather than a mere play of imagination or an abstract mathematical speculation.

In 1818-1832 Gauss was busy with what geometry used to be in its early age and later got a different name of *geodesy*. He started with the obvious observation that the hilly terrain of Hanover was not an Euclidean plane. He also saw that that the current physical hypothesis

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v. 8, p. 201. The context makes it clear that Gauss makes here no difference between geometrical and physical space.

<sup>7</sup> "In der Theorie der Parallellinien sind wir jetzt noch nicht weiter als Euklid war. Diess ist die partie honteuse de Mathematik die frueh oder spaet eine ganz andere Gestalt bekommen muss." *Werke* v.8, p. 166. This is written in 1813, that is, before Bolyai's and Lobachevsky's works were published. But I believe that Gauss didn't find the new *Gestalt* he was looking for neither in Bolyai nor in Lobachevsky.

<sup>8</sup> Weyl 1952, pp. 92

according to which the Kingdom of Hanover together with its mother planet float in the infinite Euclidean space was not particularly helpful for geodesic purposes. So he looked for different geometrical models. This led him to the theory of curved surfaces that he presented in his *Disquisitiones generales circa superficies curva* published in 1827. One may speculate that if Gauss like today's cartographers would have a properly equipped satellite in his disposal he wouldn't make his geometrical discoveries. There are firm evidences that Gauss saw connections between non-Euclidean (or *anti-Euclidean* as he himself called it) geometry obtained through playing with axioms and the geometry of curved surfaces he was working on. I believe that Gauss rightly guessed that the latter leads to a more fundamental generalisation of the notion of space than the former.

The key Gauss' idea that allowed for this generalisation was the idea of *intrinsic geometry* of a given surface. Abbott's popular *Flatland* (first edition 1884) explains the idea but oversimplifies the general situation: really interesting things happen when one consider living on a curved surface rather than in the Flatland. (For a better example consider living on a sphere like our globe.) Abbott seems to suggest to the reader the following moral: just like 3D creatures like ourselves are in a position to observe things going on a plane from a "higher viewpoint" and perform tasks impossible on the plane (like escaping from a plane prison) a creature living in a space of 4 or more dimensions would find herself in a similar position with respect to us ordinary humans. Abbott might believe that this higher viewpoint could be achieved through doing mathematics. But this moral is not justified mathematically. However fascinating the idea of 4D space might be the intuition that rising of dimension allows for solving problems in lower dimensions is quite misleading. Given a geometrical problem on a plane switching to 3D space is rarely helpful<sup>10</sup>. The idea of "intrinsic viewpoint" obtained through *lowering* the dimension is much more profound, and as a matter of fact it played a far more important role in mathematics of late 19-th and the whole of 20-th century.

However naive might sound the story about life on a surface it is indeed profound both philosophically and mathematically. Philosophically because it amounts to a non-trivial relativisation of the notion of space. One may conceive of space either as a container of spatial (geometrical) objects or as a network of relations between such objects. These two possibilities reflect the Modern dilemma between the "absolute" and the "relational" theories of space. The two possibilities seem to be mutually exclusive but they are not: a space in the

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<sup>9</sup> Bonola 1955, Toepell 1986

sense of container can be identified with an object standing in a particular relation to other objects. Given, say, a sphere  $S$  in space  $E$  one may think of  $S$  as a space containing certain other objects: points, circles, spirals, spherical triangles, etc. This gives a relational theory of space compatible with the idea of space as container. Remark that this theory makes relational the very distinction between a space and an object in a space. Aristotle made a similar move when he defined a *place*  $P$  of a given body  $A$  as the internal surface of another body  $B$ , namely of the “smallest container” of  $A$ .

Let me now show that this move is mathematically non-trivial. What is life on a sphere from an *intrinsic* mathematical viewpoint? First of all, we don't have the usual notion of sphere, in particular, its centre doesn't belong to our space and so “doesn't exist” for us. However some basic features of the sphere can be detected without leaving the surface: for example, the fact that moving “straight ahead” (this latter notion also needs to be specially defined) one returns to the starting point. Gauss' principle achievement in his theory of surfaces was the distinction between these two kinds of properties, viz. intrinsic and extrinsic: the latter depend on the ambient 3D space, the former do not. The possibility of purely intrinsic description of a surface leads to a generalisation of the Euclidean notion of space, that is, to non-Euclidean geometries: only in the special case when the given surface is flat its intrinsic geometry is Euclidean. Riemann in (1854) sketched this new notion of space and called the new concept by the term *manifold* occasionally used before by Gauss. A Riemannian manifold is a  $n$ -dimensional analogue of a curved surface seen intrinsically. The talk of a *curved space*, which became colloquial after Einstein, refers to the concept of Riemannian manifold.

Let's now return to Beltrami. This man like Gauss started his research in geometry with geodesy. He knew Gauss' results in this domain and tried to elaborate on them. Beltrami's geometrical discoveries originated from the classical cartographic problem: How to make a plane map of a curved surface? More specifically Beltrami asked the following question: How to map a curved surface onto a plane in such a way that the mapping is one-to-one on points and *geodesic* lines on the surface go to straight lines on the plane? (A *geodesic* is a line that marks the shortest path between its close points; geodesics on a plane are straight lines. So the notion of geodesic generalises upon that of a straight line for the case of curved surfaces. The notion of geodesic is *intrinsic*: distances of paths between points of a surface don't depend on how the surface is embedded into a space.) Here is first important result

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<sup>10</sup> An interesting example is given by conic sections. This issue has been first treated in Antiquity as 3-dimensional. But a more satisfactory theory of conics found in any standard textbook is 2-dimensional. The issue becomes clearer through lowering but not rising of dimension.

obtained by Beltrami: such a mapping is not possible unless the *curvature* of the surface in question is constant. (The *curvature* is a basic *local* intrinsic property of a given surface, which shows how much the surface is curved around a given point; the concept is due to Gauss.) Hence Beltrami's interest to surfaces of constant curvature (a sphere is an obvious non-trivial example) – the issue which had been already studied before Beltrami by another Gauss' follower Minding<sup>11</sup>. In 1866 Beltrami read (Lobachevsky 1840) in French translation recently published by Beltrami's long term collaborator Houel. This led Beltrami to his main discovery presented in his *Saggio* of 1868: mapping geodesics of a surface of constant *negative* curvature (which Beltrami called a *pseudo-sphere*) to straight lines of a plane one gets the Lobachevskian “imaginary” but not the “real” Euclidean geometry. Beltrami's immediate interpretation of this result was this: Lobachevsky's “imaginary plane” is *in fact* a pseudo-sphere!

From an epistemological viewpoint the situation looked curious. Consider this analogy. John studies ants and makes some unusual hypothesis about these animals. This hypothesis has consequences, which are even more unusual but cannot be definitely ruled out by available observations. Given the lack of a decisive evidence John's hypothesis is commonly viewed as a clever speculation and doesn't attract much attention. But then an unexpected event happens. John's colleague Peter discovers that John's theory perfectly describes the life of cockroaches. So Peter publishes a paper where he claims that John's theory is fine but the author mistook the animals.

Remark that the cartographic problem of mapping curved surfaces onto a plane already involves a mathematical notion of *interpretation*: figures on a surface are *interpreted* as some other figures on a plane and the other way round. In a more general way this issue is treated in the *projective* geometry, which is another important source of *interpretative* or *hermeneutic* mathematics emerged in 19-th century. However Beltrami's *Saggio* put the problem of interpretation in mathematics onto a new level: the issue was no longer *only* about interpretation of particular geometrical objects in terms of their images but about interpretation of a *theory*, viz. of Lobachevsky's *imaginary geometry*, in terms of another theory, viz. Euclidean geometry supplemented by Gauss' theory of curved surfaces. The

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<sup>11</sup> A thorough historical account of Beltrami's research and all needed references can be found in Boi&Gacardi&Tazzioli (1998). I follow it in the present paper. Noticeably Beltrami in his published worked and particularly in his correspondence is very explicit about his sources and motivations, so I don't think that there is any room for historical controversies here.

closest historical analogy of this new situations mentioned by Beltrami himself<sup>12</sup> is the interpretation of arithmetic of complex numbers in terms of the Euclidean planimetry. Beltrami's *Saggio* was impressive but had two serious flaws. An attentive reader might detect the first one through my informal description of Beltrami's work. What does it mean that through mapping geodesics of a pseudosphere to straight lines of a plane one gets Lobachevskian geometry? If the plane is Euclidean this is a sheer contradiction. If it is Lobachevskian this begs the question (I mean the question of interpretation of the Lobachevskian geometry as Beltrami poses it in *Saggio*). The only remaining possibility is to consider the given plane as *absolute* in Bolyai's sense (that is, as a plane such that all the axioms of Euclidean geometry except the Fifth Postulate hold for it), and then look what additional constraints are imposed by the chosen mapping. For the obvious reason the absolute geometry doesn't allow one to work with infinite straight lines but it allows for doing certain things with their finite segments. So Beltrami could map only a *finite piece* of a pseudosphere onto a finite piece of the absolute plane and observe that this mapping made the piece of the absolute plane into a piece of the Lobachevskian plane. Thus the Beltrami's claim that the Lobachevsky's plane geometry was "in fact" the intrinsic geometry of a pseudosphere was not wholly justified. This fact has been first stressed by Helmholtz in 1870 and then by Klein in 1871. I can hardly believe that Beltrami didn't see the flaw earlier. I guess he rather hoped that the problem was minor and solvable. Hilbert in (1901) showed that it was not. The other flaw of the *Saggio* is stressed by Beltrami's himself in the end of this work: the suggested model of Lobachevskian planimetry doesn't generalise to the 3D case. For that reason Beltrami at certain point called the Lobachevskian stereometry a "geometrical hallucination"<sup>13</sup>. The need of very different treatment of 2D and 3D cases made Beltrami to suspect that something went wrong. He changed his views completely during the same year 1868 after reading Riemann's *Habilitaetsvortrag*, which became accessible to him thanks to its publication by Dedekind. Soon after the publication of the *Saggio* Beltrami published another paper (1868-69) where he treated the Lobachevskian plane and the Lobachevskian space on equal footing using Riemann's notion of manifold: the plane and the space are both manifolds of constant negative curvature that differ only by the number of dimensions. Remark that the notion of manifold allowed Beltrami for playing down the issue of mathematical interpretation. For given this notion one may argue that the "correct" description of a geometrical space is the intrinsic one (on the contrary to what Abbott

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<sup>12</sup> letter to Houel of 18 Nov. 1868

<sup>13</sup> Letter to Houel of 18 Nov. 1868

apparently believed). This view remains today standard, particularly among physicists. In what follows I shall challenge this standard view arguing that the issue of interpretation cannot be reduced in geometry through taking a “purely intrinsic viewpoint”.

After Beltrami’s *Saggio* the idea that objects of a given theory can be modelled by some other objects of another theory became a common place in mathematics. Klein and Poincaré soon came up with new models of Lobachevskian plane: both are found in (Klein 1893)<sup>14</sup>. Unlike the Beltrami’s model these new models represented the whole of Lobachevskian plane and didn’t use the notions of intrinsic geometry and of Riemannian manifold: they are built as Euclidean constructions with certain additional analytic devices.

The development of non-Euclidean geometries was not the only factor, which made this new freedom of mathematical interpretation possible. Another source of this freedom was the projective geometry. The history of this latter geometrical discipline is closely connected to the history of non-Euclidean geometry outlined above. I shall only briefly mention the history of the duality principle. As it has been noticed by Poncelet in 1822 given a theorem of projective geometry one may get another theorem formally exchanging words “points” for “straight lines” and “straight lines” for “points”. Gergonne called this phenomenon *duality* and Steiner made the duality into foundations of projective geometry (Kline 1972, pp. 845-46). A moral that Hilbert later drew from the duality principle was this: intuitive pictures one associates with terms “line” and “point” are not essential; what counts is only abstract relations between these things determined by the given theory as a whole.

The freedom of mathematical interpretation achieved in geometry to the end of the 19-th century is quite remarkable. Earlier people believed that mathematics in general and geometry in particular had its specific subject matter as any other science. Traditionally the subject matter of geometry was defined as *magnitude* or *figure*. This helped to distinguish geometry from arithmetics studied the subject of *number*. The figure and the number were thought of as two specific kinds of *quantity*. Euclid in his *Elements* develops an arithmetical theory of proportions and a geometrical theory of proportions as two independent theories in spite of their striking similarity. Proclus in his *Commentarium* explicitly rejects the opinion of Eratosthenes according to which this similarity should be taken seriously. Applications of algebraic methods in geometry by Fermat, Descartes and their followers, which pathed the way to Analytic Geometry and Calculus, put this traditional understanding of the subject

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<sup>14</sup> In fact the core of Klein’s model representing Lobachevskian straight lines by segments of Euclidean straight lines has been already used by Beltrami in *Saggio* as an auxiliary construction: Klein added the needed metrical function.

matter of mathematics into question. Descartes developed a radically new philosophy of mathematics, which wholly justified the aforementioned Eratosthenes' view. However the new Cartesian understanding of mathematics also relied on the notion of primitive spatial intuition, which was supposed to provide a "material" for further mathematical constructions. So the Modern mathematics remained compatible with the traditional epistemic scheme in which certain primitive objects and basic truths about these objects are taken for granted while more complex objects and propositions are treated in terms of the primitive ones. That is why the discovered possibility of exchanging in a geometrical theory points for straight lines or straight lines for curve lines looked anyway striking – certainly more striking than the possibility of representation of points by tuples of numbers. Notice that the possibility of representation of *numbers* by *figures* had been well known already in Antiquity. (In arithmetical books of the *Elements* Euclid represents numbers by straight segments.) Platonic metaphysics (as it is reconstructed by Proclus in the *Commentarium*) explains the possibility of representation of numbers by figures through a backward ontological reduction of figures to numbers. But this reduction leaves the difference between the two kinds of mathematical objects epistemologically fundamental – just like the difference between mathematical objects and their material "images".

The above remarks show that in spite of its striking new features revealed in the geometry of the 19-th century the notion of mathematical interpretation was not completely new. One may even argue that it was known in mathematics since its early history. For centuries mathematicians used to *substitute* some mathematical objects for some other objects and some symbols for some other symbols looking for structures invariant through such substitutions. This is what the whole discipline of *algebra* is about: algebraic *variables* take different values leaving the *form* of a given algebraic expression invariant. In physics this leads to the fundamental distinction between physical *laws* represented by algebraic equations and *initial conditions*, which provide numerical inputs to these equations. It may be further argued that the same idea of invariance through substitution is fundamental for the very notion of mathematical object: for example, a *circle* (a mathematical object) may be thought of as an invariant of any series of exchanges of material objects (drawings and the like), which are told to *represent* the mathematical circle. This view has been thoroughly spelled out by Plato who in fact suggested a more elaborated theory according to which mathematical objects in their turn represent (are *images of*) things of yet another sort he called *ideas*.

Let me now stress the principle point I'm trying to make in this paper: although the older notion of *substitution* (and the related notion of invariance through substitution), on the one

hand, and the notion of *interpretation* as it emerged in the geometry of the late 19-th century, on the other hand, have indeed much in common the latter does *not* reduce to the former (albeit the former indeed reduces to the later). This fact has *not* been fully recognised in the end of 19-th – the beginning of 20-th century, and the new issues about interpretation were treated along the traditional pattern of substitution. My task it to explore possibilities left out by this development.

## 2. Foundations

### 2.1 Elements *and* Grundlagen

We have seen that the non-Euclidean geometry emerged in 19-th century had two well distinguishable sources. The first is the traditional line of research aiming at proving the Fifth Postulate through drawing a contradiction from its negation. This line of research started in Antiquity and resulted into Bolyai and Lobachevsky's works. The second line starts with Gauss' geodesic work and leads to the notions of intrinsic geometry and Riemanean manifold. Beltrami brought the two lines together showing that Lobachevskian spaces *are* Riemanean manifolds of a particular sort. However the question of *foundations* of the new geometry remained open. Obviously neither Euclid's *Elements* in its original version nor the generalised version of Euclid's system proposed by Bolyai (his *absolute geometry*) could serve this purpose. Taken seriously the problem of foundations of geometry in the end of 19-th century would have to account not only for the Riemanean geometry in its full generality but also (at least) for projective geometry and topology. Klein (1893) made a substantial progress toward a theoretical unification of geometry developing various links between these disciplines but he didn't produce anything like a replacement of Euclid's *Elements*. Hilbert's *Grundlagen* first published in 1899 partly meets this challenge.

I say "partly" because in this work Hilbert accounts only for a very limited part of his contemporary geometry. Basically the *Grundlagen* shows how the Euclidean geometry looks like from a new viewpoint and how it connects to the Lobachevskian geometry and some other geometries based on the Euclidean geometry. The *Grundlagen* continues the traditional Euclidean-Lobachevskian line and doesn't touch upon the Riemanean viewpoint<sup>15</sup>. This work became highly influential because of its *method*, viz. Hilbert's *axiomatic method*, not because of its content. Hilbert believed that using this method one might build appropriate foundations

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<sup>15</sup> For a historical account of origins of Hilbert's *Grundlagen* see (Toepell 1986). Hilbert certainly understood himself that his *Grundlagen* was rather a demonstration of a method rather than a working foundations of

of the whole of mathematics and of other sciences. As it is usually happens with projects aiming at reform of the whole system of human knowledge Hilbert's project of axiomatisation of mathematics and sciences brought controversial results. On the one hand, nothing like an effective global axiomatisation of mathematics, and moreover of natural sciences, has been ever achieved. On the other hand, Hilbert's *Grundlagen* remains a paradigm of a "reasonably formal" (against later "more formal" approaches) axiomatic system in eyes of the majority of working mathematicians. A today's student of mathematics may easily think – and read in many textbooks - that the axiomatic method as it is presented in the *Grundlagen* is just a more rigor version of the method first used by Euclid in his *Elements*. In this paragraph I shall try to show that this view is wrong, and that in fact Hilbert's axiomatic method is a specific response to the specific situation in geometry of the end of 19-th century described in the previous section of this paper. After that I shall argue that this response can be no longer seen as adequate and needs a replacement. But let me first to compare few first pages of the *Elements* and the *Grundlagen* in order to show that the latter work is not just an elaborated version of the former.

Euclid starts with giving basic definitions while Hilbert assumes primitive notions of point, straight line and plane without trying to define them. According to Hilbert the only reasonable answer to the questions "What is point?", "What is straight line?" and "What is plane?" can be given by pointing to *places* of these concepts in a conceptual network (conceptual *structure*) determined by an appropriate system of axioms. The same in Hilbert's view applies to primitive geometrical relations like that of congruence. The latter feature might be more difficult to grasp for one unfamiliar with the formal method; I'll give some more details shortly.

Another obvious difference between the *Elements* and the *Grundlagen* is this: after giving basic definitions and before coming to axioms<sup>16</sup> Euclid list five *Postulates* while in Hilbert's work there is no such things at all. Certainly the *Grundlagen* is not the first introductory text in geometry written after *Elements* without making use of postulates (as principles of a different sort than axioms). Nevertheless this is an essential feature of the *Grundlagen*, which certainly needs to be taken into account. Consider the first three Postulates of the *Elements*:

1. to draw a straight line from any point to any point

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geometry. In his (1902), included as an Appendix in later editions of the *Grundlagen* he made an attempt to apply his axiomatic method for building a geometrical framework in Klein's "Erlangen" style.

<sup>16</sup> See footnote 4.

2. to produce a finite straight line continuously in a straight line
3. to describe a circle with any centre and distance

Remark that the Postulates 1-3 are *not* propositions about geometrical objects but descriptions of certain *operations* performed with geometrical objects. But obviously the Postulates say that these operations are *feasible*. So one may tentatively paraphrase the Postulates by the following *existential propositions*:

- 1'. For any two (different) points there exist a (finite) straight line joining them.
- 2'. For any finite straight line there exists an infinite straight line to which the finite straight line belongs.
- 3'. For any point and a finite straight line with one end at this point there exists a circle such that the given point is its centre and the given finite straight line is its radius.

Reformulated in this way the Postulates turn into propositions and may be called *axioms*. This move seems to be innocent but it is not. For this move allows one to think of the *Elements* as a system of propositions derived from a set of basic propositions called axioms. The *Grundlagen* is indeed (or at least supposed to be) such a system but the *Elements* is not. The *Elements* is a system of *constructions* generated by a set of elementary constructions described by the Postulates 1-3<sup>17</sup> (that is, constructions by the ruler and the compasses) *and* a system of propositions associated with these constructions. Proclus in his *Commentarium* analyses the distinction between the two aspects of the theory of the *Elements* in terms of the Platonic ontological distinction between Becoming and Being: geometrical objects are treated by Euclid both *qua* constructed (generated) and *qua* pre-existing entities. But one doesn't need to buy the Platonic metaphysics to recognise the distinction. Whether the theory of the *Elements* can be *interpreted* as a system of propositions is a different question. The *Grundlagen* shows how this can be reasonably done. Whether such modification allows for a higher standard of rigor is again a different question. In any event it is clear that the *Grundlagen* is not just a more rigor version of the *Elements* but a different mathematical theory based on different ideas about geometry and mathematics in general.

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<sup>17</sup> The Postulates 4-5 unlike Postulates 1-3 *are* propositions. The fact that Postulates 4-5 are essentially different from the Postulates 1-3 has been noticed by many commentators. Heath suggests that the Postulates 4-5 might be a later addition. In any event it is quite clear why Postulates 4-5 are not listed among the Axioms: unlike the Axioms they are not universally valid but involve specific geometric constructions. Euclid or a later editor of the *Elements* could invent for the Postulates 4-5 a special rubric of "additional hypotheses" or the like.

Hilbert's ideas behind his *Grundlagen* can be hardly correctly understood outside the historical context sketched in the previous section. The plurality of models of Lobachevskian geometry (which in 1890-ies already included Klein's and Poincaré's models), the phenomenon duality in projective geometry and similar *hermeneutic* mathematical phenomena known to the date, discredited in Hilbert's eyes the traditional view according to which primitive geometrical objects like points and straight lines and basic truths about these objects should be taken for granted<sup>18</sup>. In the *Grundlagen* Hilbert suggested a new understanding of the subject matter of geometry (and mathematics in general), which I am now going to explain.

The subject matter of the traditional mathematics can be defined through distinguishing specific properties of material objects (called *mathematical*) like the *shape* and ruling out non-mathematical properties like the *colour*. A mathematician is allowed to use material objects in his or her work, and even make some mathematical use of non-mathematical properties (think about the problem of four colours) but he or she should never confuse these material objects with the proper subject matter of a mathematical study. The new kind of mathematics invented by Hilbert makes a further step in the same direction: it rules out *all* non-relational properties as irrelevant and allows into its proper subject-matter only *bare things* and *bare relations* between these things. In practice a mathematician may think about these new mathematical things in the usual way, call them by usual names and use usual helpful drawings. But one is also free to use some unusual names and images for it. This is a matter of personal taste or, perhaps, of a research skill. In any event names and images don't count in the final result: a ready-made mathematical theory must not depend on traditional mathematical notions and on the intuitions associated with these notions (to let alone names and pictures) just like it must not depend on the colour of inks used for writing it down. So Hilbert assumes that the *same* mathematical theory can be interpreted through traditional constructions in different ways just like it can be written down by different inks.

The *Grundlagen* provide a tentative theory (in fact few different theories) with desired unusual properties. Such theories are commonly called *formal*. Let me shortly recall how the formal theory of the *Grundlagen* works. Hilbert assumes tree types of primitive *things* and tree types of primitive *relations* between these things. These things and relations are thought of as *variables* (or empty *places*), which can be differently interpreted (*filled up*) with certain

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<sup>18</sup> Frege defended this traditional view in his polemics with Hilbert following the first publication of the *Grundlagen* in 1899. But since Frege argued on general philosophical and logical grounds and didn't touch

traditional content and so get a meaning. The intended interpretation (one which is particularly *helpful* for grasping the theory) is this: the three types of things are *points*, *straight lines* and *planes* while relations are *incidence* (the relation which holds, say, between a line and a point *on this line*), *congruence* and *betweenness* (for points incident to the same line). Then Hilbert stipulates certain propositions about these relations between these things as axioms. These axioms are *formal* in the sense that they don't assume any fixed interpretation. But under the *intended* interpretation they turn into axioms of Euclidean geometry (or rather of a *version* of Euclidean geometry). The formal axioms imply some other formal propositions, which Hilbert calls *theorems*. Under the intended interpretation these formal theorems turn into theorems of Euclidean geometry. Hilbert assumes that since the inferences don't depend on any particular interpretation of the axioms they are valid in *any* appropriate interpretation.

To proceed in this way carefully one needs, of course, to specify logical means and rules about interpretation and check that everything works properly. Hilbert didn't make this in the *Grundlagen* but did make such attempts in his later works (Hilbert&Bernays 1934); other people in different occasions made significant contributions into this project. I cannot and don't need for my present purpose revise here this later part of the story, which is rather well known<sup>19</sup>.

As a matter of fact *interpretations* of formal theories play in the *Grundlagen* a more important role than that of helpful intuitive images. As far as a particular interpretation is taken as unproblematic it may provide an important information about its corresponding formal theory. Consider a formal system of axioms corresponding to Euclidean plane geometry (i.e. having the usual Euclidean plane among its models) and then exchange the counterpart of Euclid's Fifth Postulate for its negation. Before Bolyai and Lobachevsky people believed that the obtained system is contradictory, Bolyai and Lobachevsky decided differently. (These people worked, of course, not with the *formal* axiomatic system itself but with one of its models.) How one can definitely decide whether the new set of axioms is consistent or not? The notion of formal system allows this. The fact that there exists an Euclidean construction, which is a model of Lobachevskian geometry, implies that *if* Euclidean geometry is consistent *then* Lobachevskian geometry is consistent too. This in its turn implies that the Fifth Postulate is *independent* from the rest of Euclidean axioms, that is, it cannot be neither proved nor refuted

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upon new hermeneutic mathematical matters Frege's arguments were not convincing for Hilbert. See (Frege 1971).

on the basis of the other axioms. So the *Grundlagen* gave a precise solution (or at least spelled out more clearly an earlier obtained solution) of the old problem.

There remained however the following important issue to be sorted out. Suppose a formal system  $S$  of (uninterpreted) axioms has two different models  $A, B$ . The formalist viewpoint outlined above suggests that differences between  $A$  and  $B$  are superficial and mathematically irrelevant like the difference of colour of two drawn circles. But suppose that now the system  $S$  is extended by some additional axioms, and that  $A$  is a model of the extended system  $S'$  but  $B$  is not. Since the difference between  $A$  and  $B$  is now grasped by the formal method a formalist must recognise the difference between  $A$  and  $B$  as essential. So in order to be consistent a formalist needs a criterion of the “essential sameness” of models independent of their corresponding theories. Using such a criterion one may distinguish the case when all possible interpretations of a given formal theory are “essentially the same” from the case when models are essentially different. In the former case a given theory is called *categorical*<sup>20</sup>. The criterion of “essential identity” of models adopted by Hilbert is *isomorphism*. Here is how he explains his axiomatic method to Frege (cit. by Frege 1971, p.13)

“You say that my concepts, e.g. “point”, “between”, are not unequivocally fixed. ... But surely it is self-evident that every theory is merely a framework or schema of concepts together with their necessary relations to one another, and that basic elements can be construed as one pleases. If I think of my points as some system or other of things, e.g. the system of love, of law, or of chimney sweeps ... and then conceive of all my axioms as relations between these things, then my theorems, e.g. the Pythagorean one, will hold of these things as well. In other words, each and every theory can always be applied to infinitely many systems of basic elements. For one merely has to apply a univocal and reversible one-to-one transformation and stipulate that the axioms for the transformed things be correspondingly similar. Indeed, this is frequently applied, for example in the principle of duality, etc. ...”

Hilbert’s doesn’t say in this passage explicitly that *all* models of a given theory should be always transformable into each other by reversible transformations, i.e. be *isomorphic*, but most certainly he has this in mind. Indeed in the context of formal mathematics the

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<sup>19</sup> See Henkin&Suppes&Tarski (1959). By the later standard the *Grundlagen* look as a rather *informal* work. The historical relativity of the term *formal* is obvious and hardly requires any special explanation.

<sup>20</sup> The term is not Hilbert’s; it is introduced by O. Veblen in 1904.

categoricity looks like a desired property. In the next paragraph I shall analyse and then challenge this view. Now let's see how the categoricity of a formal axiomatic system can be secured (if it can).

Consider a finite plane geometry  $FG$  with  $n$  points  $p_1, \dots, p_n$ ,  $m$  straight lines  $l_1, \dots, l_m$  and  $k$  basic relations  $R_1, \dots, R_k$  (possibly of different arities) between them (for real examples of this sort see (Dembowski 1968)). Axioms of  $FG$  can be formulated in such a way that all  $p_1, \dots, p_n$ ,  $l_1, \dots, l_m$ ,  $R_1, \dots, R_k$  are mentioned explicitly. The system is obviously categorical. For let variables  $p_1, \dots, p_n$ ,  $l_1, \dots, l_m$ ,  $R_1, \dots, R_k$  in a model  $\mathbf{M}$  of  $FG$  take values  $p_1, \dots, p_n$ ,  $l_1, \dots, l_m$ ,  $R_1, \dots, R_k$ , and in model  $\mathbf{M}'$  values  $p'_1, \dots, p'_n$ ,  $l'_1, \dots, l'_m$ ,  $R'_1, \dots, R'_k$ . There is the obvious isomorphism between the two models.  $\mathbf{M}'$  can be obtained directly from  $\mathbf{M}$  through the substitution of  $p'_1, \dots, p'_n$ ,  $l'_1, \dots, l'_m$ ,  $R'_1, \dots, R'_k$  for  $p_1, \dots, p_n$ ,  $l_1, \dots, l_m$ ,  $R_1, \dots, R_k$  correspondingly.  $\mathbf{M}$  can be obtained from  $\mathbf{M}'$  by the reverse substitution: this substitution like any other is *reversible*. If a theory is supposed to allow for an infinite number of primitive objects (and/or types of primitive objects and/or primitive relations) as Euclidean geometry (where however the number of primitive types and primitive relations is finite) then one cannot proceed as just described (at least if one wants to get a finite list of axioms). This doesn't rule out the possibility to get a categorical formal system but makes it obviously more problematic.

In the first edition of the *Grundlagen* Hilbert didn't see the problem and took the categoricity of his proposed formal theory for granted. In later editions he used an additional Completeness Axiom granting this property. The axiom says that the "system of things" described by the rest of the axioms is *maximal* in the sense that any extension of this system by some additional *things* of any type is impossible (such an extended system will be not a model of the theory). Hilbert relies here on the intuition that, say, any given straight line can be "filled in" with its points "completely" without leaving any "free space". In fact the Completeness Axiom as stated above can be derived as a theorem from the latter particular case, so in the third and later editions of the *Grundlagen* this later particular statement is given the title of the Completeness Axiom. Obviously this last axiom of the *Grundlagen* is of very particular character because unlike others it refers to possible models. (In today's terms the Completeness Axiom of the *Grundlagen* can be characterised as a proposition of *second order*.) This axiom stipulating the categoricity of the *Grundlagen* by a fiat is hardly compatible with the idea of purely formal "uninterpreted" mathematics since it brings the issue of interpretation into the formal theory itself.

The difficulty appeared to be typical. The finite substitutional pattern of theory  $FG$  doesn't apply universally. Popular formal theories like Zermelo-Fraenkel Set theory (ZF) and Peano Arithmetic (PA) are *not* categorical. Categoricity of these theories can be forced by second-order axioms. Some people like Shapiro (1991) take this option seriously. The majority look at non-categoricity as an inevitable evil one should learn to live with. Non-standard models of ZF and PA are usually viewed as mathematical curiosities in a way similar to which non-Euclidean geometries used to be viewed in 19-th century. Some philosophers try to be helpful and suggest how to rule out non-standard models on epistemological and ontological grounds. Few people would be ready to give up the formal axiomatic method because of the non-categoricity problem. Let me however approach the question from a different end and ask this question: What is so particularly good about categorical theories?

## 2.2 Categorical theories and functorial models

Remind where the requirement of categoricity comes from. The *best precision* with which a formal theory can possibly describe its models is *up to isomorphism*. A theory, which meets this standard, is called categorical. People who oppose the formal method often argue that this precision is *insufficient*, and that in mathematics one needs “concrete” intuitive objects and constructions of the kind provided by Postulates of the *Elements*. A formalist needs such concrete intuitive objects as well - for otherwise he loses the very distinction between a formal theory and its interpretations (models). A radical version of formalism which consistently sweeps the issue of interpretation outside mathematics dialectically turns to its opposite: a mathematical work reduces to a “play of symbols”, that is, to a kind of drawing. According to a more moderate version of formalism one should distinguish between the *intuitive mathematics*, which remains indispensable in mathematical discovery (including concept-building and theorem-proving), and the *formal mathematics*, which is equally indispensable for making mathematical concepts and theorems into consistent systematic theories, checking proofs, and communicating mathematical results to public. Even if the formal method is imprecise in the sense that it fails to distinguish between “individual constructions” (isomorphic models) it arguably prevents any uncontrolled wild behaviour of these individual construction in the public domain, and so provides mathematics with best stable patterns available in this discipline. Let me however consider the question of “precision” of the formal method from a different viewpoint.

In my view, the fact that the formal method fails to distinguish between isomorphic models of a given theory is indeed problematic. However the problem as I see it is *not* that this method

doesn't distinguish between *individual* constructions (whatever this might mean) but that it doesn't distinguish between isomorphic constructions of different *types*. From a formal point of view a two-dimensional Riemannian manifold of constant negative curvature and the peculiar Euclidean construction known as Klein's model of Lobachevskian geometry are treated on equal footing. However it is clear that the two models are essentially different: the Riemannian manifold is "natural" (or "canonical") while Klein's model is "artificial". The difference is *not* psychological or pragmatic. It concerns the fact that Riemann's notion of manifold is a generalised notion of space embracing the notion of Lobachevskian plane as a special case while the Klein's model of this plane doesn't involve any such generalisation. The formal method fails to recognise this essential difference.

My other objection to the formal method goes in the opposite direction: there is a sense in which this method is *too restrictive* to complete the task it has been designed for. I'm not going now to defend the freedom of mathematical imagination and mathematicians' right to communicate their intuitions to public. The argument is purely mathematical. Hilbert's problem, which led him to the formal method was this: How to formulate a mathematical theory leaving its interpretation free ("up to interpretation")? Hilbert's response: such a theory **T** must be *formal*, which means that its primitive terms (objects and relations) are *variables* taking their semantic values ("meanings") through interpretations; given such interpretation (model) **M** one obtains another model **M'** of the same theory through a one-to-one substitution (exchange) of primitive terms. If **T** is categorical then the substitution of terms allows one to obtain *all* models of this theory from any given model.

Designing the notion of formal theory Hilbert apparently aimed at a categorical theory and so considered *reversible* transformations between models (one-to-one substitutions of terms) as the *only* kind of interpretation he had to cope with. But the notion of interpretation as it has emerged in geometry of the 19-th century does *not* reduce to such reversible interpretations (isomorphisms). Interpretations are, generally speaking, *non-reversible*. So Hilbert's formal method didn't completely meet the challenge. The lack of a categoricity of workable formal systems is, in my view, a clear symptom of the problem. Let me now spell out this crucial argument more precisely.

Consider Beltrami's model of Lobachevskian geometry. In modern terms the principle claim of Beltrami's *Saggio* is formulated as follows: a 2-dimensional Riemannian manifold of constant negative curvature (Lobachevskian plane) is embeddable into 3-dimensional Euclidean space (which is another Riemannian manifold). As it stands the claim is false: in order to get a true statement one needs to replace "embeddable" by "locally embeddable". But

let's now ignore this detail and consider the notion of embedding. Certainly Beltrami's embedding is an interpretation (remind the full title of his paper): the notion of Lobachevskian plane is *interpreted* through this embedding in terms of the "usual" Euclidean space. But the embedding is *not* reversible: one cannot embed the Euclidean space into the Lobachevskian plane. An embedding is a sort of transformation called *monomorphism*. Carving a pseudosphere out of its ambient space one may show indeed that the pseudosphere is isomorphic (in fact only locally isomorphic) to the Lobachevskian plane. On this basis one may think about other isomorphic models like Klein's model. However this reasoning is misleading: in the given context a pseudosphere cannot be carved out from the Euclidean space and considered as a self-standing object. For if the pseudosphere is indeed carved out from its ambient space and considered as a self-standing space (manifold) it ceases to be Euclidean. This brings indeed a better presentation of Lobachevskian plane (suggested by Beltrami in his *Teoria*) but the whole point about interpretation of Lobachevskian geometry in Euclidean terms gets lost. This example shows that the issue of interpretation in mathematics as it has emerged in geometry of the 19-th century doesn't reduce to the old idea of *substitution* of different values for a variable, which apparently has led Hilbert to his formal axiomatic method. For such substitutions are always *reversible*<sup>21</sup> while geometrical interpretations, as we have just seen, are *not*. So Hilbert's formal method didn't meet indeed the *hermeneutic challenge* of his contemporary mathematics in its full generality. Let us now see how this challenge can be met.

When Beltrami read the *Habilitaetsvortrag* he identified (in his *Teoria*) the Lobachevskian plane with 2-dimensional Riemannian manifold of constant negative curvature and the Lobachevskian space with 3-dimensional manifold of the same type. He repeated in the *Teoria* the point made in the *Saggio*: the former notion is interpretable in Euclidean terms while the latter is not. However in the new context this remark lost the significance it had in the *Saggio*. For as far as the notion of Riemannian manifold is taken seriously one doesn't need any longer to look for an Euclidean model of Lobachevskian plane in order to claim that this plane is "real". Does this make unimportant the whole issue of geometrical interpretation? I don't think so. The notion of Riemannian manifold doesn't work by a magic. One cannot do

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<sup>21</sup> I shall not provide here a detailed analysis of the notion of substitution but it is obvious that the reversibility is a basic feature of this transformation: the substitution of A at the place of B implies the possibility to substitute B at the place of A.

anything with Riemannian manifolds without considering interpretations commonly known under the name of *maps*. A basic fact about Riemannian manifolds making part of the usual definition of the concept is that any such manifold is locally embeddable into the Euclidean space of the same dimension (differentiability). So the notion of Euclidean space remains fundamental for the notion of Riemannian manifold and is certainly *more* than just a particular case of the latter notion. Remark that interpretations (maps, transformations) of Riemannian manifolds are indispensable when these things are thought of as *geometrical objects*. One might object that manifolds are *spaces* but not objects. But recall that Gauss' idea of intrinsic geometry allows for a relativisation of this distinction: a manifold A is called an object with respect to manifold B and manifold B is called a space with respect of manifold A iff A embeds into B. So in order to look at a given manifold either as a space or as an object one needs embeddings anyway. In fact there is a "natural" notion of map between Riemannian manifolds which is more general than embedding and isomorphism. It is a straightforward generalisation of the notion of isomorphism of the manifolds obtained through giving up the reversibility condition. Maps between Riemannian manifolds are *differentiable transformations*<sup>22</sup>. Considering *all* Riemannian manifolds together with *all* maps ("mutual interpretations") between them we get a kind of "super-space" in which the manifolds live<sup>23</sup>. Let us denote this super-space **RM** and see how it looks like. Is it something like a usual (Euclidean) space? Not really. While the Euclidean space may be viewed as a *container* of all its points, straight lines, circles and other figures **RM** is a *network* of manifolds. (Remind that things like points and straight lines are manifolds on their own rights.) In spite of this difference the analogy can be carried out in more precisely. I mean the fact that the notion of Euclidean space can be accounted for in terms of *transformations* of Euclidean objects (Klein 1872). This works for other geometrical spaces like the Lobachevskian space. The principle difference between the notion of geometrical space so construed and **RM** is this: in the former case all the transformations in question are reversible (think about motions, affine transformations, homeomorphisms, etc.) and so form *groups*, while in the latter case they are not. So giving up the reversibility of geometrical transformations causes indeed a fundamental change of the usual concept of space.

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<sup>22</sup> Like in the case of isomorphisms of Riemannian manifolds (often called *diffeomorphisms*) the condition of differentiability of these maps can be specified in different ways: one may demand either that the maps are differentiable only once, twice, or an infinite number of times.

<sup>23</sup> I use the word "super-space" *not* as a technical term here. A mathematical reader shouldn't think about super-symmetries and super-strings.

A network of mutually transformable objects like **RM** is called a *category*. The notion of category introduced by Eilenberg and MacLane in (1945) is very weak: one requires only an operation of *composition* of transformations between the objects, the associativity of this composition and the existence of an identity transformation for each object. Transformations of objects are called in the Category theory *morphisms*. The aforementioned requirements are met not only by Riemannian manifolds but by any mathematical concept coming with appropriate notions of object and transformation (between different objects falling under the same concept). For standard examples think about sets and functions, groups and group homomorphism, topological spaces and continuous transformations, etc. **RM** is the category of Riemannian manifolds, which has such manifolds as objects and differentiable maps as morphisms.

The notion of category can be thought of as a very general *form* of mathematical concepts. This seems to be correct but in fact is quite *misleading*. It is misleading because the notion of category is *more general* than the notion of form. Objects are told to have the same form when they are isomorphic, that is, when they are mutually transformable by *reversible* transformations. For example all circles (say, on Euclidean plane) are isomorphic – they transform into each other by motions and scale transformation, which are all reversible. So in the category of circles all morphisms are isomorphisms: such categories are called *groupoids*. Given a groupoid of circles one may *identify* all its objects and call the obtained unique object *Circle*. The groupoid then reduces to a *group*. The Circle can be then thought of as the common *form* of all circles. But in a more general situation when objects of a category transform into each other irreversibly such reduction is impossible. So the notion of category allows one to explain how the notion of form comes about. The converse is not the case. So it is misleading to call consider a category as a kind of form. I shall develop this fundamental point in the next section. Now let's see how the notion of category allows for generalisation of Hilbert's formal axiomatic method.

Instead of looking for a formal *categorical* theory bringing about a group of isomorphic models we shall now look for a theory bringing about a *category* of models. I warn the reader that this older usage of the term *categorical* has nothing to do with the Category theory. In what follows I shall use the expression *categorical theory* in a different sense meaning a theory built by category-theoretic means unless it will be specified otherwise. Unfortunately I cannot avoid using in this paper the term *categorical* in the two different senses.

Think about our category **RM** or Riemannian manifolds. How it can be possibly specified? It may be argued that whatever might be the specification it cannot possibly provide any better

precision than up to isomorphism. However this is not correct. For there is a sense in which **RM** is *unique* and cannot have isomorphic copies. This follows from its description as the category of *all* Riemannian manifolds. Whatever *all* might exactly mean here if taken seriously this term doesn't allow for acquiring additional "copies" of objects and morphisms of **RM**: all the copies are already there! The only category isomorphic to **RM** is **RM** itself. Identity is the only isomorphism available in this situation<sup>24</sup>. Concerning the meaning of *all* I don't think that one should think about large proper classes and be afraid of set-theoretic paradoxes here. I guess that the notion of *class* could be avoided in this context although this is not allowed by the official definition of the notion of category. For **RM** is nothing but *the concept* of Riemannian manifold construed in a particular way. One doesn't need to have "all manifolds" as full-fledged individuals in order to conceive of **RM** (Rodin 2005). The concept of Riemannian manifold like many other important mathematical concepts (set, group, topological space, finally the concept of category itself) is determined "up to an arbitrary morphism" but not up to isomorphism. This is another way of saying that manifolds, sets, groups, topological spaces and categories themselves form categories. Only in simplest cases all morphisms reduce to isomorphisms and corresponding categories reduce to groupoids and groups. This latter pattern, which underlies the whole idea of formal method, cannot and shouldn't be applied everywhere in mathematics, and moreover everywhere in science. It should be stressed that in a *different* sense **RM** is *not* unique (so the expression "the category of Riemannian manifolds" needs a careful interpretation). For the concept of Riemannian manifold like any other mathematical concept can be specified in many different ways. This terminological problem has nothing to do with the point just made concerning the impossibility of "isomorphic copying" of **RM**. Perhaps the following convention is reasonable: the isomorphic copying is impossible when "all" is mentioned in the title of a given category. So one might speak about *a* category of *all* Riemannian manifolds pointing to a specific version of the general concept. Otherwise isomorphic categories can be, of course, easily constructed, although this doesn't make much mathematical sense<sup>25</sup>. For this later reason a different convention might be equally reasonable: isomorphic copies of categories are not allowed but by a special permission.

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<sup>24</sup> In order to make a historical justice it must be noted that the notion of category I use here goes beyond one introduced in (Eilenberg&MacLane 1945). For the authors explicitly say that "Such examples as the category of all sets or the category of all groups are illegitimate" for the usual set-theoretic reason. So the authors assume that objects of a category form a set. Nowadays such categories are called *small* while categories such that their objects form proper classes are called *large*. The idea of categorical reconstruction of mathematical concepts presented below in the main text has been first put forward by Lawvere in his thesis of 1963.

<sup>25</sup> Gelfand&Manin (2003, p. 70) call the isomorphism of categories "a useless notion".

The easiest way to conceive **RM** is to take its objects and morphisms, i.e. manifolds and differentiable transformations, as ready-made, i.e. defined by traditional methods, and attach the latter to the former. Categories so construed are sometime called *concrete*; when people speak about the category of Riemannian manifolds, the category of groups, etc. they usually mean concrete categories. Let's denote the concrete category of Riemannian manifolds **RMC**. Although **RMC** is not a self-standing conceptual construction it has specific *categorical properties* that may be studied. Such properties are expressed in the form of *equations* saying that compositions of certain morphisms equal to compositions of certain other morphisms and that all the morphisms needed to satisfy these equations exist. Graphically these equations are represented as *diagrams*. When specific categorical properties of **RMC** are identified one may try to *define RM* in terms of these categorical properties just like in the *Grundlagen* Euclidean space is defined in terms of its *formal properties*. This goes as follows: one takes an abstract category **RMA**, stipulate that **RMA** has the same categorical properties as **RMC**, and tentatively identifies **RMA** with **RMC**. Since all the categorical properties in question just like all the formal properties of Euclidean space cannot be simply listed (because they are too many) one needs a theoretical structure. So one may tentatively stipulate certain properties of **RMA** as axioms and try to infer from them others. This looks quite like the standard axiomatic method but it works more like in the *Elements* rather than in the *Grundlagen* given a number of basic categorical constructions one makes new constructions respecting certain rules. A version of **RMA** construed in this way can be found in McLarty (1992), where the reader can also find further references.

Before continuing to explicate the idea of categorical reconstruction let me briefly touch upon this general question: What does it mean that a given mathematical concept is adequately reconstructed? In a fixed mathematical framework it may be shown that certain properties  $P$  and  $Q$  of a given object  $O$  of certain type  $T$  are equivalent in the sense that  $O$  has the property  $P$  if and only if it has the property  $Q$ . For example all sides of a given triangle are equal if and only if all its angles are equal. This allows for two obvious alternative definitions of the notion of regular triangle. However when people talk about “formal reconstruction”, “set-theoretic reconstruction”, “categorical reconstruction” the situation is quite different. In such cases there is no indisputable criterion of whether a given reconstruction is correct or not. The situation is similar to that with different version of the Pythagorean theorem: one may talk about *translation* of mathematical content through different general frameworks but not logical equivalence. When Hilbert claimed that his axioms characterise completely the Euclidean geometry he, of course, went far beyond what people at the time normally assumed.

That his formal method was incapable to distinguish between a point and a beer mug (to use Hilbert's famous saying) was not a problem for Hilbert himself but his drinking companion might have a different opinion. Hilbert could provide very serious arguments in favour of his view but he couldn't provide anything like a *mathematical proof* for it. In the case of a categorical reconstruction the situation is similar: in order to justify the claim that **RMA** is **RM** one needs to show that abstract objects and morphisms of **RMA** have all basic properties of Riemannian manifolds and differentiable transformations construed by traditional methods (today this means: by set-theoretic methods). However there is a space for different decisions concerning questions like this: Which properties of traditional constructions are basic and so should be preserved within the new framework? What this preservation exactly amounts to? etc.

Although the question What is a good conceptual reconstruction? has no simple answer it is easy to see where the categorical method works better than the formal method. First, as I have already argued, the categorical method allows one to avoid the problem of isomorphic copying. Second, unlike the formal method the categorical method allows for distinguishing between "canonical models" and different kinds of "external models" of a given theory. The former are objects of a category "targeted" by the given theory (like **RMA**) while the latter are further constructions involving non-identity morphisms of the target category and components of functors from the target category to different categories. (Functor is a morphism between different categories. In the abstract Category theory objects of any category are treated as categories just like in the abstract Set theory elements of sets are treated as sets. So the distinction between morphisms and functors, which seems to be very palpable in the context of concrete categories, becomes redundant as far as the categorical approach is used consistently. I retain the distinction following the common usage.) In a categorical framework one is *not* obliged to treat a Riemannian 2-manifold of constant negative curvature and Klein's model of the Lobachevskian geometry on equal footing. For this 2-manifold is a particular object  $O$  of **RM** while the Klein's model is a rather specific construction in **RM**, which involves different objects of this category: bounded and unbounded lines (1-manifolds), the Euclidean plane and the Euclidean disk. Although the two models have the same formal properties they have different categorical properties. So in order to justify the claim that points, pairs of real numbers and beer mugs are quite different things after all one doesn't need to refer to a primordial intuition: one may instead consider a categorical framework, which is large enough to see that these things behave differently. The principle epistemological argument in favour of the categorical viewpoint against the formal

one is this: the isomorphic constructions in question cannot be “carved out” from their conceptual environments and made into self-standing formal theories. We have seen that a similar holistic epistemological argument underlies the formal method itself. However in a larger context the argument turns against the formal method.

From a formal viewpoint the first of the aforementioned features of the categorical approach – that it allegedly allows to capture a category *precisely* but not just up to isomorphism – looks perhaps the most suspicious. The explanation of this apparent mystery is simple: the notion of category provides a *constructive* framework for doing mathematics (as opposed to a *formal* framework). In this sense categorical mathematics better fits the model of the *Elements* than that of the *Grundlagen*. For example, this basic principle of Category theory can be better, in my view, spelled out as a *postulate* rather than as an axiom:

for morphisms  $f:A \rightarrow B$  and  $g:B \rightarrow C$  to construct morphism  $fg:A \rightarrow C$ .

The associativity law of the composition in its usual form is an *axiom* (a proposition)

$$(fg)h = f(gh)$$

but in a higher category theory it is replaced by this *postulate*:

for morphisms  $(fg)h$  and  $f(gh)$  to construct morphism  $a: (fg)h \rightarrow f(gh)$ .

(This morphism  $a$  is called *associator* and it is normally required to be reversible. Remark that  $a$  is a morphism between morphisms. Such morphisms are called 2-morphisms.)

Thus doing mathematics categorically one works with particular constructions but not general forms of constructions. Mutual *interpretations* between given categorical constructions are further categorical constructions: morphisms, functors or some more involved constructions made out of them. So the *hermeneutic challenge* of geometry of the end of 19-th century is met by the Category theory in a far more radical way than by the Hilbert’s formal method: *morphisms*, which are *elementary interpretations*, are made into building blocks of mathematical constructions. Remark that the notion of *object* of a category is in fact redundant: identity morphisms are sufficient to make the categorical machinery work properly.

Another important difference between the formal and the categorical approaches concerns the place and role of *logic* in resulting theories. I shall touch upon this question only briefly here. The formal approach fits well the traditional view on logic (dating back to Aristotle) as the most general form of reasoning, or more precisely, of the *correct* reasoning (in particular the correct mathematical reasoning). Moreover this traditional view on logic seems to be essential for the very idea of formal axiomatic method for epistemological reason: if the whole of mathematics is rewritten in the form of formal theories then *the* logic is necessary for

making these theories into a whole. (One may argue that a pure logic is not sufficient for unification of mathematics but this argument is not important in the present discussion.) Ironically the formal approach made a genuine revolution in logic, which brought about a vast plurality of alternative logical systems (some of which are combinable while some other are not) and so made untenable the traditional view on logic as *the* most general form of reasoning (which one?). Philosophers argued in 20-th century a lot in order to distinguish on metaphysical, epistemological, pragmatic or different grounds a particular system of formalised logic, which might be viewed as *the* core logic replacing the traditional Aristotelian logic. Many including Russell and Quine argued that the Classical predicate calculus should be viewed in this way. Other philosophers (Beall 2000) defended pluralist views on logic without suggesting any alternative mechanism of unification of knowledge (apparently assuming that such unification is not really needed). I shall not go into this debate but remark that the hermeneutic approach, which I advocate here, allows for unification (or perhaps better to say *integration*) of mathematics without a notion of universal logic. The hermeneutic integration works through mutual interpretations of mathematical theories but not through a logical conceptual umbrella. Category theory makes the notion of mathematical interpretation effective and precise.

A categorical reconstruction of mathematical concepts unlike a formal reconstruction doesn't start with logic. It starts with the general notion of category, which is much *weaker* than any reasonable system of logic. A logicist may object that the notion of category found in standard textbooks is *informal*, so in order to define this notion rigorously one should start with a logical calculus anyway, and then make up a formal category theory. Axioms for such formal theory can be written down indeed (Lawvere 1966). I shall not object this logicist assumption directly but show how things look like *if* one assumes the notion of category as primitively given. In this case a notion of logic can be recaptured through a further construction: there is a way to associate a formal logical calculus with an abstract category with appropriate properties (Bunge 1984, Makkai & Reyes 1977). This gives the notion of *internal logic* of a category. Not surprisingly the internal logic of the category **S** of *sets* is Classical logic. Only slightly relaxing requirements making an abstract category into **S** one obtains the notion of *topos* and *Intuitionistic* logic associated with type of categories. So instead of taking logic for granted and developing on this basis a mathematical theory one proceeds the other way round: takes for granted a certain category, say, **RM** (which may be viewed either as **RMC** or **RMA**) and then ask what kind of logic if any is internal for this category.

The logicist may again object that even if the general notion of category is taken for granted one needs a certain mechanism allowing for different specifications of this general notion bringing about **RMA**, **S** or any other particular category. As far as such specifications are made through informal axioms (or *postulates* as suggested above) natural language gets essentially involved, and it becomes quite unclear what kind of conceptual resources are in the play. This logicist challenge is easier to meet. Let me describe the categorical device of *functorial semantic* providing a reasonable solution of the problem (it has been first introduced in Lawvere (1963)). We shall see that this device allows for making a closer analogy between the formal and the categorical methods, and that at the same time the different technique changes the formalist pattern profoundly.

The idea of functorial semantic looks much like a formalist idea: one starts with a theory made up on the basis of a logical system and then makes up its models. The logical system in question is a “logical” category, which can be viewed as just another symbolic convention concerning the internal logic of this category. Then this logical category is strengthened up to a certain theory **T**, which for a similar reason can be viewed as a formal theory. So far there is no essential difference between the categorical and the standard formal settings. The difference appears when one considers models of **T**. Functorial models are functors from **T** to another “base” category, which is usually is taken to be **S** but may be something else. I skip reasons by which one may distinguish between functors  $\mathbf{T} \rightarrow \mathbf{S}$ , which are models of **T**, and functors of the same form, which are not. Not surprisingly models so construed, generally, are *not* isomorphic. However the categorical setting doesn’t make it reasonable to try to force the isomorphism (i.e. *categoricity* of **T** in the old sense). For the wanted “precision” of **T** can be obtained differently. The categorical setting straightforwardly allows for considering the category **M** of models of **T** (having functors of the form  $\mathbf{T} \rightarrow \mathbf{S}$  as its objects and transformations between these functors called *natural* transformations as morphisms). So instead of getting a group of isomorphic models one gets a category of models. Although the functorial models are defined “only up to morphism” but not up to isomorphism **M** comprises all of them at once. Although the word “all” should be obviously taken with a pinch of salt **M** is a perfectly manageable construction. In addition the theory **T** itself becomes to look differently: **T** may be treated as its own model and (as identity functor) be included into the category **M**. So one gets a category of models where one particular model *generates* all the others (i.e. generates the whole category). A theory becomes a *generic model*. This again brings one back to the pattern of the *Elements* where few basic constructions generate the rest.

### 3. Formalisation and Categorification

We have seen that taking into consideration non-reversible transformations between mathematical objects and treating these transformations on equal footing with isomorphisms has quite dramatic consequences for mathematics. Category theory is the general theory of non-reversible transformations (morphisms). Starting with very weak general assumptions about morphisms this theory develops into a reach mathematical discipline, which allows to think seriously about the possibility of categorical reconstruction of the whole of mathematics. In this section I shall try to outline the new notion of mathematics brought about by this development.

By a *network of interpretations* I shall understand any instant of *collective* cognitive and symbolic human activity. Such a network has spatial and temporal characteristic, as well as more specific characteristic, which I shall discuss shortly. Now I consider two epistemic procedure with such a network, namely formalisation and categorification.

A *formalisation* of a given network of interpretations amounts to extraction of its reversible fragments (if any). Such fragments are told to have invariant *forms*, which one may treat then as self-standing abstract entities. So one may forget about mathematical classrooms, the history and the geography of mathematics, mathematical models in physics and other sciences, and do “pure mathematics”. Remark that the result of this procedure depends on the choice of isomorphisms: there are could be different available options.

*Categorification* suggests itself in the same situation as a more general and more flexible epistemic procedure. It amounts to accounting for interpretations of a given network as categorical morphisms and looking for categorical properties making this network manageable. A basic property of this kind is *coherence*, which can be specified in many different ways. The most basic notion of coherence is given by the usual categorical notion of functor.

The assumption according to which all collective human cognitive activities turn around the same conceptual forms implies a *kind* of coherency. Whether this assumption describes adequately how people actually think is less important: as far as foundational epistemic issues are concerned one looks for a definition of *reasonable* thinking rather than tries to describe how people actually think. I guess that many opt for the formal method because they believe that it provides the *only* way of making a collective cognitive activity coherent and reasonable. However this assumption is wrong as I have already shown.

Cutting a given network of interpretations into isomorphism classes and identifying each such class with a particular *form* one gets the following two problems. The first problem is how to bring the obtained *different* forms back together. As far as I can see the only way to make this without using other means is to organise the network as a *hierarchy* of forms. This can be done through considering isomorphisms of different kinds (different equivalence relations) some of which are more general and some more specific than some others. Then one may get a single universal form on the top of the hierarchy and a number of more specific forms making its body. This is the traditional model of organisation of knowledge (and of society). It might work but generally it doesn't: the epistemic requirement according to which any *reasonable* network of interpretations must allow for such hierarchical organisation is unreasonable itself. Instead of implementing this traditional hierarchical structure modern adepts of the formal method try to persuade themselves and others that the very idea of unification of knowledge at higher scales is misleading (and politically dangerous), so one should learn to live in the world of small disconnected formal patterns. Remark that treating the given network of interpretations as a category one may grasp its global structure without stipulating anything like a "universal form" of the whole thing.

The second problems related to the first is that things of quite *different types* appear to be isomorphic, that is, to have the same form. The problem can be partly treated by the above recommendation of keeping isomorphisms specific and isomorphism classes small. However this doesn't work when the formal method is applied to mathematical theories. Doing geometry *formally* after Hilbert one cannot avoid the confusion of points with beer mugs. This is a joke but the identification of points with tuples of numbers is not. In this sense an isomorphism is a very imprecise map. As I have already argued from a categorical viewpoint the obvious difference between points and numbers is not the matter of a primordial intuition. The shortest way to spell out the difference is to say that points and numbers belong to different categories. The difference between these categories can be made clear even if following Hilbert's advice one forgets how an individual point and an individual number look like and observes how these things *behave* with respect to their likes. In a limited domain these things indeed may behave similarly but looking at their larger conceptual environments one may also see the difference. The formal method doesn't allow for such a wider look except the whole of mathematics is organised hierarchically (which is not a realistic assumption).

Although Hilbert's formal approach to geometry looked like a radical proposal it perfectly fits the traditional Platonic notion of mathematics as a science of *form*. Let's see more precisely

what was new and what was traditional in Hilbert's proposal. A wider historical look shows that a really new feature in the *Grundlagen* was the notion of mathematical *interpretation* (although it was not a Hilbert's own invention) but not his notion of formal theory itself. The traditional geometry (and mathematics in general) can be also said to be formal albeit not exactly in the same sense. For example, a circle conceived traditionally is a form of shared by many material objects. By the analogy with Hilbert's formal method one might call such material objects *interpretations* of the mathematical circle (Plato would call them *images*).

What makes the principle difference between interpretations of a formal theory in the Hilbert's sense and material images of usual geometrical objects is the fact that in the former case the interpretation is an internal mathematical matter while in the latter case it is not. The issue of mathematical interpretation entered mathematics before the *Grundlagen* had been published (with the publication of the *Saggio* the latest). Hilbert applied the traditional Platonic schema in the new situation and got a result, which looked very unusual. Thinking about interpretations of formal theories Hilbert had in mind reversible substitutions of primitive terms and relations in one model by their counterparts from another model. So he got the notion of formal theory. However, as we have already seen, this doesn't work in fact: mutual interpretations of models don't reduce to substitutions except simple cases.

To see how basic is the assumption of reversibility of mathematical transformations (operations) consider this question: Is the operation of addition  $7+5=12$  reversible or not? The question can be understood in different senses and, correspondingly, given different answers. The operation (+5) can be cancelled (reversed) by this subtraction:  $12-5=7$ . But given that 12 is as a sum of two natural numbers it is not possible to specify these numbers uniquely:  $7+5=10+2$ . In this latter sense the operation (in fact a different operation) is irreversible. But yet in a different (perhaps not "properly mathematical") sense the latter operation is nevertheless reversible: when 7 and 5 are summed up and bring 12 about the summands don't perish but survive on the left side of the equality:  $7+5=12$ . In *this* latter sense any mathematical operation and any categorical morphism  $A \rightarrow B$  is reversible. This fact suggests to think of the notions of mathematical operation and transformation as mere metaphors describing particular *relations* between mathematical objects. In this latter view when 7 and 5 are summed up "nothing happens" indeed: the story of emergence of 12 out of 7 and 5 is just a way to say that the three numbers stand in a particular ternary relation. I shall not discuss this Platonic view systematically but remark that it ceases to be plausible as far as mathematics is considered as human activity going on in space and time. A machine performing the operation  $7+5=12$  may keep or not keep the summands in its memory after the

operation is done. The same is true for humans. One may argue after Plato that these facts have nothing do with numbers in themselves but I think that a more challenging task is to reconstruct the number in themselves from the relevant conceptual dynamics empirically observed in mathematical classrooms and elsewhere. If during a mathematical reasoning one forgets where he or she has started from this certainly disqualifies the reasoning. So the reversibility is certainly required in this case. This basic feature of mathematical reasoning is made explicit in the *Elements* where each proposition is repeated twice: immediately before and immediately after its proof (but before *Q.E.D.*)

Granting this basic reversibility of mathematical reasoning one might argue that mathematics is ultimately formal while non-reversible mathematical transformations are superfluous structures construed on this formal basis. However the argument doesn't go through as far as a larger-scale conceptual dynamics is taken into the consideration. The larger-scale dynamics shows that the reversibility of mathematical reasoning is not in fact so fundamental as it seems. For at larger temporal (and perhaps also spatial) scales mathematics is obviously non-reversible. When the Pythagorean is taught in school a teacher may reasonably look for reversibility of interpretations of this theorem given by different pupils, which shows that all the pupils learnt indeed the same thing. (Remarkably not *any* kind of isomorphism is desired in this situation. The mere phonetic isomorphism will not do: when all the pupils in a class utter the statement of the theorem and its proof in exactly the same words the teacher would suspect that none of them in fact *understands* it.) But one cannot reasonably apply the same standard thinking about historical development of mathematics. Greek mathematics and the contemporary mathematics are not isomorphic. One might extract a common *form* of the Pythagorean theorem invariantly preserved throughout the history by considering certain local isomorphisms between ancient and modern theories. However impressive and important such long-preserved mathematical identities might be they don't tell us much about how mathematics subsists and develops. Non-reversible interpretations of ancient theories in modern terms (and perhaps some backward interpretations as well) tell us much more. Quasi-eternal formal concepts like *the* Pythagorean theorem (or, say, *natural number*) can be best understood as epiphenomena of continuous non-reversible conceptual mathematical dynamics. As the example of the Pythagorean theorem clearly shows transformations involved into this dynamics don't reduce neither to isomorphisms (which is obvious for otherwise mathematics couldn't develop) nor to *monomorphisms* (embeddings) of older contents into new contents. For mathematics like any other science not only acquires new contents but also constantly revises its older contents and throws some of them away. The

cumulative model of development of science and mathematics is oversimplified even if it allows for occasional “revolutions” (Kuhn 1962). In a categorical framework such oversimplified assumptions no longer look as “natural”. A categorical analysis makes it clear that to keep a certain branch of science (mathematical or not) at the same fixed point of its development is not a trivial task, as anybody involved into the educational business certainly knows. In fact this task is hardly realisable at all since conceptual change is a very basic feature of science and mathematics. Without new research science and mathematics quite rapidly corrupt but not just cease to develop.

Thus the conceptual dynamics of mathematics is, generally, non-reversible. The reversibility is an important but strictly *local* feature of this dynamics. So the view that non-reversible transformations and the Category theory studying such transformations (morphisms) are construed on the top of a reversible formal basis is ungrounded. This implies that the usual view on mathematics as *formal* science is very limited and should be given up. This equally applies to *logic* as a part of mathematics. Instead mathematics (and in particular logic) should be thought as a science of *interpretation*, that is, as a categorical hermeneutics.

This new vision of mathematics brings a new notion of meaning. Meaning is usually thought of as an invariant of paraphrasing within a given language and of translations between different languages. However this notion makes sense only when the paraphrases and translations are reversible. Otherwise there is no invariant, or at least not in the usual sense. That paraphrases in and translations between natural languages are, generally, not reversible can be demonstrated by simple linguistic examples. So the usual notion of meaning doesn't go through. Hence the notion of meaning as a kind of substance transferred from a speaker to another speaker should be given up. What makes a linguistic communication meaningful is its *coherence*, which doesn't require reversibility and can be specified by category-theoretic means. I leave this issue for another study.

#### 4. Conclusion: Mathematical Structuralism.

In the philosophy of mathematics of the 20-th century structuralism has been first associated with Bourbaki's fundamental *Les Eléments des mathématiques* aiming at reconstruction of mathematics in set-theoretic terms. *Les Eléments* starts with a version of Set theory, and then purports to represent further mathematical concepts as “sets equipped with structures”. I shall not analyse here the general definition of structure given in (Bourbaki 1939 - , v.1, ch.4) but give only this simple example. Take a set  $G$  and associate with any ordered pair of its elements a third element of the same set. This is how Bourbaki defines a *binary operation* on

$G$ . Then given the needed properties of the operation  $G$  turns into a *group*.  $G$  can be called in the given context a “set equipped with a group structure”. The idea is that a “structure” is put on the top of a “bare set”. Bourbaki’s definition of structure is general enough to allow for similar constructions of topological spaces, rings, modules and a wide spectrum of other mathematical concepts. Crucially concepts so construed are workable, that is, allow for carrying out proofs. A large part of mathematical research in the second part of the 20-th century has been made in this general framework.

A reader familiar with the Category theory can easily recognise basic categorical constructions among Bourbaki’s generic “structures” like the *terminal* and the *initial* structure. This suggests to question the informal metaphysics of *Les Eléments* according to which all mathematical constructions are made of the same set-theoretical “matter” and distinguished by their specific structures. The reason for the questioning is that the Category theory seems to be capable to account for structures without making any use of the “matter”. Moreover it allows for treating the set-theoretic “matter” as a particular kind of structure. So the dualistic metaphysics of structure and matter becomes redundant: the Category theory allows one to dispense with the “matter” in favour of pure structures. Hence the thesis that the Category theory supports the mathematical structuralism (Awodey 2004). Structuralism says, roughly, that only structures count. Here is an official definition (Hellman, forthcoming):

“Structuralism is a view about the subject matter of mathematics according to which what matters are structural relationships in abstraction from the intrinsic nature of related objects.”

Hellman quite rightly, in my view, traces the history of mathematical structuralism back to Hilbert. In the new language the basic idea of the *Grundlagen* can be spelled out in this way: only *structures* described by formal theories are essential while “instantiations” of these structures (models of the formal theories) are less important and at least in some contexts can be dispensed with.

This short explanation of the mathematical structuralism (see also Awodey 1996, MacLane 1996) is sufficient for showing that the Category theory in fact does *not* support this view. Here is the core argument. The notion of *structure* is defined by Bourbaki *up to isomorphism*. This basic property of the notion of structure survives in *any* of its versions. So the notion of structure is a version of the more general notion of *form* (which I *define* as an invariant through an isomorphism). Category theory makes it clear that the old Platonic notion of mathematics as a study of forms (and in particular of *structures*) is limited and suggests a

more general notion of mathematics as a study of *categories*. Categories are, generally, *not* forms (structures) but forms (structures) are indeed simple categories, which, generally, are *not* sustainable outside their wider categorical environments. Traditional formal mathematics, and in particular *structuralist* mathematics, is nothing but a very specific case of categorical mathematics. So Category theory does not support mathematical structuralism.

To clarify the argument let me distinguish between two different features of mathematical structuralism: epistemological and ontological *holism* about mathematical matters, on the one hand, and the “exchangism” about models (the idea that one model of a given formal theory can be exchanged for another one), on the other hand. In most discussions about structuralism the two issues are tightly intertwined although they are very different. The *Grundlagen* is an explicitly holistic theory: Hilbert stresses the fact that geometrical notions including primitive ones have no relevant meaning outside their corresponding theories. According to Hilbert the relevant meaning of these notions is their *formal* meaning, so points, etc. have to be thought of as “places in a structure” which can be either “filled in” with one’s favourite intuitive content or perhaps just left “free”. The two claims are very closely interrelated in Hilbert’s account. But remark that the former doesn’t imply the latter. While holism can be reasonably argued for in the traditional Euclidean setting (the Euclidean notion of point arguably has no meaning outside its corresponding theory just like the Hilbert’s) the exchangism about models is a new specific feature of the *Grundlagen*. The holism and the exchangism together imply a broadly structuralist view: the “intrinsic nature” of mathematical objects (if any) doesn’t matter; only structures of relations between the objects are mathematically relevant. I assume that the mathematical holism alone does *not* imply structuralism. Let’s now see what happens with holism and “exchangism” in the categorical approach.

Categorical approach certainly pushes mathematical holism further forward. While the formal approach suggests to work with a chosen favourite model of a given formal theory (keeping in mind that this model can be exchanged for another isomorphic model) the categorical approach suggests to work with a *category* of models instead of picking up just one. What happens with the exchangism then? As far as one works with *all* models of a given theory at once the exchange is no longer required. Moreover as far as the exchange of models is thought of as an isomorphism (substitution of terms) this notion cannot account for a non-trivial category of models. A given category A can be often *interpreted in* another category B. I mean that given categories A, B one may consider a functor  $F:A \rightarrow B$  or a category of such functors. In this sense the talk of “all models” shouldn’t be understood too straightforwardly: any “category of all models” in principle allows for further constructions, which in some

different sense could be counted as new models. But anyway since  $F$  is, generally, non-reversible (not an isomorphism) it cannot be seen as providing an “instantiation” of a given abstract structure. Moreover, as I have already explained, the case when  $F$  is an isomorphism can be reasonably ignored by a special convention: although the notion of isomorphism of categories is not contradictory it is “useless” as Gelfand & Manin put this.

It might be nevertheless argued that the categorical approach pushes holism and exchangeism further forward accordingly. While the formal approach allows one to dispense with the “intrinsic nature” of points, straight lines, etc. the categorical approach seemingly allows to dispense with the “intrinsic nature” of geometrical spaces themselves considering them as “abstract objects” of a properly specified category. So one may argue in Hilbert’s vein that in a categorical context it becomes irrelevant *what* is “taken” as geometrical space as far as things called spaces form a category with required properties. This view is misleading for the following principle reason: the idea that one is in a position to “take something for” a geometrical space, and then exchange this something for something else, doesn’t apply when the categorical approach is taken seriously. For “taking something for” is a reversible substitution. What one can do in a categorical mathematics is this: construct various categories of “spaces” and interpret them in each other and in different categories. Such interpretations are generally irreversible.

What makes people to look for different “instantiations” of categories and think about “non-instantiated” categories as “abstract structures” is apparently this residual form of *substantialism*: even if “individual substances” do not matter in mathematics they are supposed to be around ready to “take” suitable “places” in a structure; the structure itself is on this account a kind of second-order substance allowing for exchange of its contents. The spatial intuition of “place” involved in this metaphor is not innocent since it supports the assumption of the possibility of the reversible exchange of the occupants of the abstract “places”. However in a categorical context this residual substantialism and the associated spatial intuition of reversible motion (behind the notion of place) are, generally, irrelevant. Taking the notion of morphism seriously one should in fact reconsider the colloquial distinction between abstract and concrete categories explained above. In this colloquial distinction “abstract” means “formal”. If I am right that categorification and formalisation are quite different things this distinction is misleading. A distinction between abstract and concrete mathematical concepts can be reasonably made in a categorical context in a different

way<sup>26</sup>. I shall not consider this issue here but remark that doing mathematics categorically it is hardly helpful to consider “abstract morphisms” as abstract. It is more appropriate to develop a new intuition supporting this primitive categorical notion. Notice that a common intuition about *irreversible transformation* involves the notion of *time*; this shows that in a categorical mathematics usual spatial intuitions can be reinforced by spatio-temporal intuitions.

As far as one takes the categorical approach seriously the notion of “internal nature” of a mathematical object should be equally reconsidered. It is natural in a categorical context to call “internal properties” of a given object properties of morphisms *into* this object. Given a category  $C$  and a chosen object  $O$  of this category such morphisms form another category called *slice category*  $C/O$ . So the notion of category provides a perfect instrument of “looking inside” its objects.

As far as the mathematical structuralism is seen against the traditional mathematical essentialism according to which mathematics studies particular things like numbers and figures (given to us through an intellectual intuition or abstraction from experience or in a different way) then Category theory indeed supports the structuralist side. However Category theory doesn't support the residual essentialism inherent in mathematical structuralism. The categorical approach indeed pushes mathematical structuralism further forward. It broadens the structuralist “exchangism” allowing for non-reversible interpretations. At this point the structuralism ceases to be itself. One might argue instead that the Category theory suggests a new generalised notion of structuralism. I don't agree. I think that to call the new framework “structuralist” would be inappropriate and misleading. For unless a given morphism (functor)  $F:A \rightarrow B$  is reversible there is no way to stipulate anything like “structure” with respect to which  $A, B$  can be thought of as its “instantiations” or “models”. A categorical mathematics is not about structures invariant through exchanges of contents but about translations between different contents.

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<sup>26</sup> As proposed by Lawvere in his lectures during the conference “Ramifications of Category theory”, Florence 2003.

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