Andrei Rodin

On Categorical Integration

Contents: page:

1. Epistemic Integration and Differentiation

Even most basic epistemic acts like memorising involve an operation of "bringing together" (events, memories, perceptions, etc.), which I shall call (epistemic) *integration*. Integration can be achieved through putting integrated elements into certain relation (for example, the relation of causality) or in some other way. Different ways of epistemic integration I shall call *integration modes*. In what follows I will describe several integration modes. The principle aim of this paper is to introduce and explicate one such mode associated with the mathematical category theory. In what follows I shall compare the categorical integration with more convenient epistemic integration modes. The present introductory generalities play an auxiliary role in this paper, so I shall say only few things about the general notion of epistemic integration.

Like its mathematical counterpart the epistemic integration is associated with the differentiation. By the epistemic differentiation I mean any operation of distinction. In many cases epistemic integration and differentiation are so tightly associated with each other that they cannot be distinguished but abstractly, not as separately performed acts. This observation makes me stay apart from popular questions of priority concerning integration and differentiation. Given a part-whole relation, for example, I will not ask whether the whole or rather its parts are "given first", or whether one should think about part/whole relation in terms of differentiation of the whole into parts or rather in terms of integration of the parts into the whole[1]. Although in what follows I shall talk mostly about integration modes I assume that much of what I'm going to say can be equally spelled out in terms of differentiation[2].

---

[1] Such priority questions have been discussed throughout the history of philosophy from Parmenid and Plato who insisted on the ultimate priority of the identity over the difference (and hence of integration over differentiation) to recent writers trying to reverse this relation to the opposite. What is reason to expect this pair of basic concepts to be ordered?

[2] The reader might notice that what I call here epistemological differentiation and integration correspond to what is traditionally called analysis and synthesis. I'm agree in principle but of course to make the correspodence precise one would need to specify some particular account of analysis and synthesis for such accounts are many. A change of terminology helps me to refresh this old issue.

A special question about epistemic integration is this: whether or not the integration should be pushed up to the last limit in order to achieve an ultimate unification of all human knowledge? The usual scepticism about theories of everything and universal laws applicable indiscriminately across disciplinary barriers is, in my opinion, quite justified because theories, laws and similar integrating devices, which work well at relatively large scales of epistemic integration like that of scientific discipline, fail to do so at larger scales, and so one can hardly expect a theory of everything to be a good theory. However this healthy scepticism hardly allows one to avoid easily the issue of ultimate epistemic integration for in some form it suggests itself anyway. People link even distant branches of knowledge through a folk metaphysics assuming the realms of physical and mental, organic and non-organic, historical and natural, etc. So a real question, in my view, is not about the choice between ultimate integration and no ultimate integration but between integrating strategies. One strategy is to delimit the reach of science by certain scale of integration , and allow things to integrate themselves at upper scales spontaneously (perhaps leaving a hope to replace this spontaneous integration by scientific one in the indefinite future). Another option is to try scientific integration strategies at the higher scales of epistemic organisation already now. Strategically I choose the second option. Tactically I prefer not to talk about ultimate integration anymore: how far any given integrating strategy reaches is a matter of test but not of prior decision. True, any integration strategy has its limits of applicability, which should be determined and respected. However this doesn't mean that at certain point one should give up science and replace it by folk metaphysics, religious dogmas or anything else.

2. Centred Integration Modes

The following list of integration modes and the informal treatments of these modes I'm giving below are no way supposed to be exhaustive. The purpose of this section is to place the categorical integration among and contrast it against other integration modes, rather than study all the integration modes systematically. I call these modes "centred" because all of them achieve the integrating effect through attachment of certain elements ("integranda") to some kind of centre, or "whole", albeit in very different ways. In the section **3** I shall show that categorification doesn't work like this but attaches the integrada to each other directly.

2.1 Generalisation

Generalisation is involved in any situation when one has a "general case" (think about proposition *animals are mortal*) and a number of corresponding "special cases" and "examples"(think about propositions *humans are mortal*, *Socrates is mortal*, etc.) So the generalisation works as a centred integration mode: it attaches all the special cases and examples to the general case.

Noticeably Socrates is an individual human but not a special case of human. Nevertheless the generalisation treats special and individual cases similarly: the general concept *human* "brings together" Socrates, Plato, you reader and all other people like it does it with *man* and *woman*. Thus as an integration mode the generalisation is associated with two different differentiation modes: *specification* and *individuation* (exemplification).

A logical mechanism of generalisation is *predication*: the property of being human shared by all people is truly predicated (that is, "truly said about" any human individual) [3]. So it is this common property H which plays the role of integrating centre here. Notice that in order to achieve its integrating effect one and the same H should be in a way present "in" or "at" or

[3] There is a difference between Aristotle's and modern understanding of predication: in Aristotle's view "is animal" predicates on "human" while in modern view this is not the case (but "is animal" and "is human" both predicate on Socrat). I assume the modern view but the difference is apparently not important for my argument.

“over” every human being at any time, and so “hook” all these people together. Whether or not to think realistically about this sheaf HS of hooked people is an ontological matter. Plato, for example, famously stipulated for this end the Ideal Human (the idea of human, or ideal humanness) in which all the individual humans “partakes”. The following simplification suggests itself: why not think of these people as forming a *class*, namely class HC of all human beings? Whatever might be an ontic decision about properties the commonsensical notion of class (or set or collection) has obviously a stronger intuitive appeal.

However sheaf HS doesn’t reduce to class HC or at least not straightforwardly. Talking about all human beings I simple-mindedly (as any practical person would do) mean all *living* humans. So class HC is variable: people die and are born. But HS hooks more people than HC contains in any given time since HS reaches all the past and future generations of humans too. One might try to repair this by making all the past, present, and future people into one big class, of course. But this is certainly not what one usually means by “all human beings”.

Let me recall some traditional logical terminology needed for what follows. Class HC is called *extension* of predicate H. The “meaning” of predicate H (“humanness”) is called its *intension*. In 4.5 I consider the old issue of intension and extension from a new categorical perspective. Notice that the notion of class (set, collection) may be thought of independently of generalisation and predication: given few things we can make a class out of them even if they don’t share common properties. So we can consider *setting* as an independent integration mode which might be associated or be not associated with generalisation: I consider it in 2.6 and recall often after this. The same is true about *sheaving* which I consider in 2.7. We shall see that setting and sheaving both allow for non-trivial mathematical treatment.

Generalisation is built in very deeply in natural languages (through predication) and theoretical thinking of all kinds. Think about the pair “general notion – example” ubiquitous in mathematics (at least since Euclid’s times), natural sciences, and philosophy. The obvious and perhaps even trivial character of this observation makes it only more important to analyse the role of generalisation in mathematics and sciences critically. We shall see that the categorification gives reason to re-assess the epistemic role of generalisation profoundly (see 6.1)

2.2 Co-generalisation

By co-generalisation I mean not a differentiation mode associated with generalisation but another integration mode. Just like a common property brings together individuals which happen to have this property an individual brings together properties which it happens to have. This integration mode is quite traditional in spite of the new name. The co-generalisation can be illustrated with Aristotle's notion of substance which is a thing with multiple and variable properties like an individual human (noticeably it allows for integration of incompatible properties).

Although the co-generalisation seems to be just as basic as the generalisation for some reason the latter is usually regarded more respectfully in science: scientists tend to generalise, that is, to subsume more specific and individual cases under least possible number of generalities rather than to attach more generalities to least possible number of individuals. Apparently this trend is grounded on a common assumption according to which it is up to the scientist to bring about and to cancel generalities but not to bring about and to cancel individuals, so the scientist finds himself in a position to adjust generalities to individuals but not in a position to adjust individuals to generalities. A writer of literature fiction usually finds himself in the opposite position: he has a unlimited freedom to play with (fictious) individuals but unlike scientist usually takes generalities for granted (or he can play with both). That is why writers of fiction use more co-generalisation than generalisation: fictious stories usually involve a relatively small number of characters but a lot of circumstances. Just think how boring would be a novel where the same story is repeated many times and only characters exchange!

The domain of application of co-generalisation is not limited by fiction: think about history (as a discipline). The history of France, roughly speaking, comprises everything one can truly say about the past of this country. France is a real but not fictional country although it is humanly construed, in particular through the work of its historians. But this no way turns the history into fiction.

Perhaps we should also rethink the role of co-generalisation in sciences. I can see no reason why science should improve only on its generalisations always taking pre-scientific individuations (like “this body”, “this organism” etc.) for granted. The idea that to bring about and to cancel individuals is rather divine than human work is certainly wrong as far as we are talking about the modern experimental science. The problem of individuation becomes explicit in the particle physics (French & Crause 2004), and in my view this case calls for changing the prevailing old-fashioned attitude to the issue of individuation in science.

Co-generalisation already plays an important role in disciplines like geosciences often regarded as theoretically inferior with respect to fundamental biology and physics. Like in history in geosciences we have a unique fixed object of study (the Earth), and possibilities of generalisation are limited. To see a potential theoretic importance of co-generalisation think also about cosmology (which became a science only quite recently): do we need indeed to stipulate parallel universes to give a sound scientific account of ours?

Like generalisation the co-generalisation may be associated either with setting or with sheaving, however to think of co-generalisation in terms of sheaving seems to be more convenient, particularly when among integrated properties there are incompatible ones.

2.3 Relation and co-relation

Russell is responsible to the view according to which relations should be thought of on equal footing with properties: formally properties are one-place predicates and relations are many-place predicates. There is however a sense in which properties play a special role in Russellian account too (like in Aristotelian): n-placed predicate (relation) is equivalent (in a sense which can be made precise) to one-place predicate (property) ranging over (ordered) n-tuples of individuals. If we accept Russell's analysis or relation we should distinguish the following two steps through which relation works as an integration mode: first it makes individuals into n-tuples (ordered pairs, triples, etc.) and second it brings together n-tuples of individuals standing in given relation. The discussion on predication in 2.1 applies to the second step. The discussion on setting in 2.6 applies to the first step (but doesn't cover the issue of ordering).

However there is an intuition about relations left aside by this analysis. Think about *John loves Mary*. It is natural to think of John's love to Mary as a particular thing, and in the older philosophical language it would be appropriate to qualify this thing as relation. (Example *John is taller than Mary* apparently doesn't support such intuition. But even in such case one might save the intuition by arguing in the instrumentalist vein, that to be compared John and Mary need either to be brought into direct physical contact, or to be measured by the same measurement rod, etc.) The Russellian analysis of relations is not incompatible with stipulation of John's love to Mary as a particular but it doesn't imply anything like this. So the problem is partly terminological: we might use indeed the term "relation" as Russell suggests and find a different term for "relations as particulars". We shall see that the category theory suggests terms "morphism" and "functor" for it. I shall follow this convention until 6.3 where I give it up (for the reason which I shall explain).

By co-relation I mean an integration mode linking different relations through common relata. What has been said above about co-generalisation in the literature fiction, history and science applies to co-relation: a normal novel has more than just one character, and relations between the characters is often what the whole story is about, etc.

I qualify relation and co-relation as centred integration modes meaning the Russellian analysis of relations as predicates reducing them to generalisation and setting (plus ordering). With the co-notions it works similarly. Remark however the idea of "particular relation" hints to a non-centred integration: John's love attaches him to Mary directly without any assistance of the couple $\langle$ John, Mary $\rangle$. (The couple is brought about through the love but not exists in advance and at certain point brings the love about.) We shall see in **3** how this works with categories, and in 6.3 I shall return to relations.

2.4 Formalisation and co-formalisation

It is not clear for me whether formalisation should be considered as separate integration mode or as a symbolic device used for relation and generalisation (as integration modes) but anyway it is worth attention in the present context. Consider formula P: X+Y=Y+X with the convention according to which X,Y stand for natural numbers, and + and = have their usual meanings. Substituting for X,Y different natural numbers we get equalities 1+1=1+1, 1+2=2+1, etc. So the integration works: an infinite number of equalities are "compressed" in P (given the convention). A further compression can be achieved through making P range over

various two-placed predicates (binary relation) and rewriting the above formula as P(X,Y). So we get into the second level of formalisation and now P(X,Y) may stand not only for X+Y=Y+X but also for X+Y=XY.

The trick of formalisation may be viewed as modelling of Plato's Forms by symbolic means. Think about X+Y=Y+X as a common *form* of expressions 1+1=1+1, 1+2=2+1, 2+3=3+2, …This means, as usual, that meanings of "+" and "=" are fixed while X,Y are variables. Notice that the operation of substitution has a diagrammatic aspect: we identify two tokens of X, two tokens of Y, and keep the diagram (scheme) …+…=…+… invariant through the substitution. If we now think about the scheme as a symbolic counterpart of Platonic form then Plato's controversial notion of "partaking" of a thing in its form (idea) gets represented by the mundane notion of substitution.

Clear examples of formalisation in the above sense are found already in Aristotle, in particular in his syllogistic. He formulates his "perfect syllogism" like this: if all A are B, and all B are C, then all A are C given the convention according to which A,B, C are substituted by *humans*, *mortal* and the like. Introducing this notation Aristotle likely had in mind the mathematical notation of his time but it is not clear whether Greek mathematicians ever used letters in a similar way. When Euclid denotes a triangle ABC this can be more naturally understood as a proper name rather than logical variable. But not exactly: when ABC is used in a proof of some general proposition about certain kind of triangles ABC plays a role of variable: the proof justifies the general proposition because ABC may stand for any triangle of given kind. This shows that the notion of substitution and the distinction between variables and constants is not trivial issues to be taken for granted. (More about this in 4.3.)

Since the beginning of 20^{th} century formalisation is widely used in philosophy and particularly in Analytic philosophy bringing about disciplines like Formal Ontology and Formal Epistemology. What is most questionable in this development is the tendency to equate non-formal with vague and imprecise, and formal with clear and distinct.

Formalisation is prima facie a generalisation device. In addition it is a symbolic device which functions through substitution and essentially depends on its properties. Surely formalisation can be helpful in philosophy just like in science and mathematics. However I think that it is a more pertinent task for today's philosophy to understand what is formalisation, how it works, and which purpose it may serve than to formalise itself and stuff around.

A category (in the sense of category theory) is a powerful formal device: the notion of category captures a common form shared by sets, topological spaces, vector spaces, groups and many other kinds of mathematical objects. But as we shall see the category theory at the same time shows limits of and provides an alternative to generalisation and formalisation as integration modes.

By *co-formalisation* I mean the following. Consider a number of different formalisations of the "same intuition". It might be the case that these formalisations share a core (formal) structure. Then a formalist will argue that the formal method reveals nuances which the intuition is incapable to grasp, so what intuitively looks like one concept is in fact a family of different concepts. But it might also happen that these different formalisations are done be very different formal means and share nothing in common except the common intuition. Then it is a non-trivial task to formulate precise translation rules from one formalism to another and explain in which sense different formal constructions are "intuitively the same". Anyway the fact that different formalisations can be linked through common intuition allows for considering the co-formalisation as an integration mode. I develop this idea and give an example in 6.4.

2.5 Mereological summing

Now I come to integration modes which have a stronger intuitive appeal than ones considered above. By the mereological summing I mean an integration mode in virtue of which parts make together a whole (which they are parts of). If terms "part" and "whole" are understood enough liberally then the whole issue of epistemic integration reduces to a form of mereological summing: it is all about making a whole from given parts. Now I would like to be more specific about parts and wholes; I return to the liberal interpretation in 6.5.

A way to specify notions of part and whole is to consider a *relation* between a whole and its parts and study its formal properties generalising upon relevant examples. Then an immediate observation will be that this relation is a partial order.

A systematic formal treatment of mereology has been first suggested in 1916 by Lesn'ievsky as a foundation of mathematics alternative to the set-theoretic foundation. This attempt was unsuccessful in the sense that mathematicians were not interested. This is hardly surprising since Lesn'ievsky's project has been motivated logically and metaphysically but only very indirectly by the contemporary mathematical research. This situation persists until today: although the formal mereology has been revived and widely popularised in the philosophical community about two decades ago it remains a metaphysical business mostly detached from the contemporary mathematics and science.

There is an interesting aspect of mereology which seems to resist formalisation. It doesn't seem pointless to argue that a living organism is a whole of its parts in a different and stronger sense than an inanimate aggregate like a set of chairs or a stature. I'm not going to justify or refute this view but mention it because it gives me a good opportunity to say something about applicability of mathematics outside of its own domain. For at least some proponents of the informal organic mereology would say that such mereology cannot be mathematical in principle because mathematics is incapable to grasp the essence of what is organic. Such an argument one finds in Aristotle's *Metaphysics* (1036b) where the author argues that a hand makes part of body only when the hand is alive and functions properly, and that these features cannot be preserved in a mathematical model.

It is instructive to compare this Aristotle's view with Descartes' view on animals as mechanical automata. It might seem that the apparent absurdity of the Descartes' view gives reason to think that Aristotle might be right. However Descartes' point, as I can understand it, is subtler. Descartes consciously prefers a mathematical model which is "clear and distinct" to a vague discourse about "organic whole" even if this mathematical model clashes dramatically with what we usually think and feel about organisms. A bad mathematical biology in Descartes' eyes is better than good non-mathematical biology.

Myself I'm rather in Descartes' camp, and one specific reason why I think its cause is not hopeless is that the two philosophers speak about very different mathematics (which is hardly surprising given the historical distance). Descartes' mathematics unlike Aristotle's allows for motion. This makes Descartes' mechanisms better models of living organisms than static figures Aristotle had in mind. Further mathematical progress might provide biologists with much better tools for doing biology mathematically than presently available. There is special reason for considering the category theory for it. For this mathematical theory widely exploits the intuitions of organicicity and naturality, as we shall see, and thus clearly demonstrates that Aristotle was wrong thinking that such intuitions are alien in mathematics.

In 6.5 I shall explain why I don't follow the rule here and don't introduce a notion of co-mereology: it appears to be the same thing as mereology.

2.6 Setting

The intuition behind the notion of set is similar to that behind the notion of whole, or perhaps is even more basic. For a set is virtually nothing over and above its elements, or at least such is the idea. Things are allowed to make a set without any further requirement that they share a property, stand in advance in a certain relation, etc. To make a set one needs nothing but its elements.

Sets can be elements of sets – this doesn't seem to be counterintuitive. But then we need to take an important decision: either to think about everything as a set, or to stipulate atoms, i.e.

things which may be elements of sets but which are not themselves sets. Another logical possibility which has a weaker intuitive appeal but appears to be more important in mathematics is to reserve a kind of sets which cannot be elements of other sets. According to the standard terminology such things are called *proper classes* but not sets. Notice that the introduction of proper classes restricts the promiscuous character of setting, so given certain things you might be unable to make a set out of them. Like above I will not follow this terminology and speak about sets and classes indiscriminately. (This helps for my purpose of having a fresh look at the issue.)

In 2.1 I have already mentioned a link between setting and predication through the notion of extension. This link between is made more explicit in modern logic through the notion of *semantics* which is a set (class) or a system of sets whose elements serve as values of logical variables. Because of set-theoretical difficulties philosophers often avoid to speak about sets and even classes talking about semantics and use the word "domain" instead; they also often say that a variable "ranges over" given domain meaning that the variable takes there its values as explained above. In the following discussion I assume that basic features of this standard construction of logic are known to the reader.

Like the notion of whole the notion of set seems to be hopelessly general and giving no hint of how precisely works the integration mode in question. However the history (and particularly the recent history) of the notion of set is strikingly different from that of the notion of whole. Unlike mereology set theory was readily accepted by mathematical community, became a field of active mathematical research , and the notion of set until today is widely used in nearly all branches of mathematics.

2.6.1 Historical remarks on and about set theory (I)

The creator of this theory Georg Cantor was originally motivated not by the idea to develop a mathematical account of the general metaphysical notion of set but by the idea to generalise the concept of natural number to the effect of allowing for infinite (or *transfinite* as Cantor himself called them) numbers. Cantor wanted infinite numbers to account for infinite trigonometric series (Cantor 1872) and their geometrical representations on the real line.
His general notions of set given in his 1883b and 1895 (which are not the same) both generalise upon his work on point sets (1879-1880-1882-1883a,c –1884a,b - 1885) which explicitly involve a topological context: Cantor considers points on given real line and given Euclidean n-dimensional space.
As it has been noticed by Hume and likely was already known about the time when people first learned counting two finite sets A, B have the same number of elements if and only if there is a one-one correspondence between their elements. Cantor's idea was to extend this construction to the infinite case, and use it for definition of infinite numbers.
Similar attempts had been made long before Cantor but they always brought results considered as paradoxical. Notice first that the infinite case unlike the finite case it is impossible to specify a one-one correspondence between elements of sets simply through listing coupled elements. However one can provide a rule for such coupling. Consider for example this rule: each natural number n is coupled with number 2n. So one gets an one-to-one correspondence between set N= $\langle 1,2,3,.. \rangle$ of natural numbers and set E=$\langle 2,4,6,.. \rangle$ of even numbers. Now if one extends usual properties of finite sets to this case one gets two incompatible conclusions: (i) there are just as many natural numbers as even numbers, that is, N is just as big as E and (ii) N is twice as big as E. So the extension doesn't work.
Traditionally results of this kind were viewed as additional evidences in favour of a more general point made by Aristotle and repeated by many philosophers after him: it is wrong to

think about "all" natural numbers because what is meant by saying that natural numbers are infinitely many is exactly that they form nothing like finished collection.

Cantor made this: he extended to the infinite case the property of finite sets that (i) the one-to-one correspondence between elements of two given sets implies that the two sets have the same number of elements but not the property that (ii) the one-to-one correspondence between elements of given set A and elements of given subset (part) Sb of A implies that Sb=A. In particular he suggested to accept that there are just as many natural numbers as even numbers. In Cantor's view this also blocked the general Aristotle's argument against infinite sets which according to Cantor depends on hidden assumption (ii). What remained crucial for making sense of the idea of infinite number was to show that such numbers don't collapse into one. For this end Cantor envisaged an argument showing that natural numbers cannot be brought into one-to-one correspondence with (all) points of given line, or in other words, that the points cannot be enumerated. So Cantor escaped from what seemed to be a dead end since Aristotle's times. He also showed that the points are in one-to-one correspondence with set P of all subsets of N, and proved a general theorem saying that the set PM of all subsets of given infinite set M is not in one-to-one correspondence with M but is (in a reasonable sense) bigger.

The case of points on given line helped Cantor, I suppose, to persuade the mathematical community that the idea that some infinities are "more infinite" than others was not absurd: points of given line provided a visible example of how an infinity bigger than that of natural numbers looks like. A possibility to account for geometrical continuum in terms of (sets of) points was something that Aristotle also forcibly objected arguing ad absurdum. However it looked like Cantor found a reasonable way to lift the old Aristotle's ban, and when Hausdorff in (1914) and others successfully used Cantor's approach for treating geometrical continuity in terms of its points (point-based general topology) a big part of the community was convinced.

In his works of 1879-1885 works Cantor widely uses philosophical and historical arguments in order to justify the notion of infinite set against the existing tradition and also puts forward some strong metaphysical hypotheses ad hoc aiming at application of his set theory outside pure mathematics. For example, in (1885) he makes a guess that while the elements of the usual corporal matter are countable the elements of the ethereal matter are not. However only in his (1970) Cantor changes his philosophical defence for offence making the following bold claim: the pure mathematics *is* the general set theory; geometry (the theory of point sets), the theory of functions, the mathematical physics, and all the natural sciences including chemistry and biology are application of the general set theory[4]. Leaving natural sciences and ether aside one may observe that Cantor's belief that the pure mathematics is basically set theory is still shared by many philosophers (albeit only by few mathematicians) today. Although reasons for the belief might be very different in different cases I think it is important to analyse the historical context in which Cantor first came to this idea for understanding the present state of affairs.

Cantor didn't elaborate his view but as far as I can understand he hoped to reduce qualitative differences like that between the corporal and ethereal matter or that between organic and non-organic matter to quantitative differences between infinite numbers (cardinalities). A reason why Cantor believed that this could work was most probably what he saw as the successful application of this method to the distinction between continuity and discreteness

[4] The paper was written in 1884 as a reply to Tannery (1884) who stressed the metaphysical significance of Cantor's work. Cantor submitted the paper to Acta Mathematica but it was rejected by the editor. However some 86 years later thanks to Grattan–Guinness (see his 1970) the older decision was revised and the paper finally published in the same journal.

allowing for accounting of the geometrical continuum in terms of its points. To see the importance of the issue we need to look farther back into the history.

According to the traditional view dating back to Plato, Aristotle, and Euclid (as far as we can reconstruct Euclid's philosophical assumptions from the structure of his "Elements" – see my 2003) the subject matter of mathematics involves a fundamental ontological distinction between numbers and magnitudes reflected through the disciplinary boundary between arithmetic and geometry. Pointing to the incommensurability of diagonal of given square with its side is a standard argument used by ancient authors in order to demonstrate the distinction. This discovery made by Pythagoras and/or his followers in the context of efforts aiming at reduction of "the All to numbers" showed to early Greek thinkers that geometrical magnitudes resisted the reduction, and this fact was taken by following generations of Greek mathematicians and philosophers with all the philosophical seriousness. To the epoque of Cantor and Hausdorff the traditional distinction between numbers and magnitudes had been already profoundly shaken by Analytic geometry and Calculus. This development started in early modern times by Fermat, Descartes and others didn't aim at anything like the revival of the Pythagorean reductionist program. But it nevertheless had exactly this effect to the end of 19^{th} century: it is amazing to see how the idea of arithmetisation of mathematics became again a serious mathematical issue some 2500 years after Pythagoras.

A part of the new arithmetisation was the theory (or more precisely theories) of the real number which gave a precise mathematical sense to the pre-theoretical view that all lengths can be uniformly represented by numbers, and so bridged the gap between arithmetic and geometry. Cantor's theory not only allowed for a version of theory of the real number but also suggested a reduction of numbers and magnitude to sets: arithmetic could be viewed as a theory of finite and countable sets and traditional geometry as a theory of sets of higher cardinality. Given Cantor's notion of infinite set one could easily dream to make mathematics into a universal formal science about abstract "things", that is, about everything, leaving technical problems for a later study, as did Cantor himself in his rejected paper.

Noticeably algebra in the eyes of Cantor and of most participants of foundational debates in mathematics since the late 19th century and at least until 1930 (when Van-der-Warden's (1930) appeared) was not a mathematical discipline like arithmetic and geometry but rather a technique of manipulation with numbers (also useful in geometry). This fact certainly cannot be explained by a mere saying that until 1930 algebra didn't yet prove to be important or that before this date its epistemological significance was not yet recognised. As early as in the late 17th century Descartes and his followers stressed the mathematical and epistemological importance of algebra and method against the traditional substantialism in mathematics. This Cartesian view was anything but marginal in the early modern European mathematics while the revival of the old-fashioned idea of arithmetisation of mathematics didn't occur before the second half of the 19th century.

An official pretext (if not a profound historical reason) for it was the need of logical clarification and foundation of calculus and other mathematical disciplines. An early example of such logical clarification and foundation is the "$\varepsilon - \delta$ language" for calculus invented by Weierstrass around 1870 and known today to any mathematical student. This clarification involved a good deal of restoration of ancient standards of rigor[5] and sweeping out early modern ideas like Newton's concept of fluxia (a variable understood as changing

---

[5] Compare, for example, Xth book of *Elements* where Euclid treats irrationalities or Archimedes' version of the integral calculus. I wonder couldn't be here cases of real influence of ancient authors on mathematicians of 19 century: noticeably the " age of rigor " in mathematics coincides with the time when a great philological work on edition and publishing of original Greek mathematical texts was done in Germany by Heiberg and others. I have no historical evidences supporting this hypothesis.

mathematical entity) or Leibniz's infinitesimals as merely heuristic. I think that today the whole development of conservative regimentation of mathematics in the second half 19th century needs a re-assessment. It is too evident that without the early modern conceptual revision of mathematics tightly connected with the contemporary development physics which produced the bulk of the modern science most of mathematics as we know it today couldn't be developed. Isn't it then too reckless to disqualify the early modern innovations as merely heuristic and return to an older conceptual scheme adapting it for new purposes? True, this adaptation helped to preserve the continuity between the old and the new mathematics (which didn't happen in physics), and proved the viability of the old scheme. So I don't want to say that foundational achievements of mathematics of 19th and 20th centuries are useless; the $\varepsilon - \delta$ language is very useful indeed. What needs to be revised is the disqualification of early modern mathematical ideas. I shall not make this in the present paper but the reader will see that certain aspects of categorical mathematics rehabilitate some of such ideas like the notion of variable understood as changing mathematical object (see Lawvere 1976).

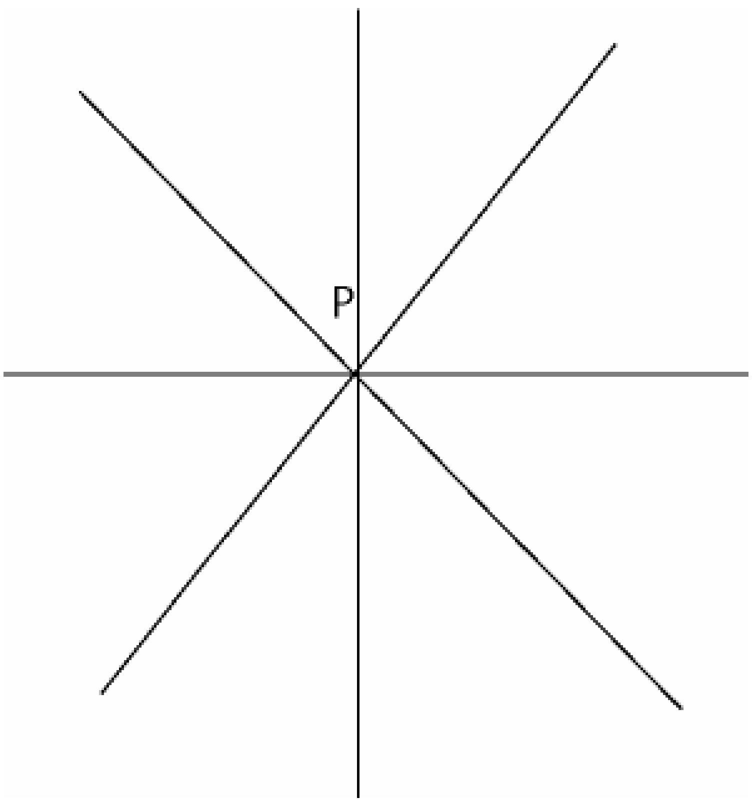
Without taking into consideration this historical context it is difficult to understand Cantor's own claim of foundational importance of his theory (which he never justified), the wide consensus (achieved quickly and lasting until today at most philosophical departments) that *if* set theory (Cantorian or other) is a good theory *then* it is good foundation of mathematics, and moreover that set-theoretic difficulties are foundational difficulties. Recall that nothing like working set-theoretic mathematical foundations didn't appear before Bourbaki (before 1950-ies), so the whole issue remained highly speculative. (Bourbaki's first volume immediately revealed weak points of the project now treated with the category theory - see 2.8.2.)

The general character of Cantor's notion of set as a "collection of well-distinguished objects of thought" explains why Cantor's set theory was important for logic. At least since the late 19th century sets (under the name of "classes") were involved into logical studies, in particular in works of Boole, Schroeder and Russell. For Russell (in 1903) it seemed obvious that sets, classes, collection, etc. are different names of the same thing. The identification of mathematical sets and logical classes was not only somewhat natural but also very desirable: if, for example, every geometrical object could be indeed viewed as a set (class) of points then given a system of logic operating on abstract sets one might hope to solve with it geometrical problems, or at least reformulate known solutions with a logical rigor and rule out logically dubious mathematical arguments. Finite sets didn't provide such possibilities. A logical regimentation of mathematics was likely the last thing Cantor could think of as a possible application of his theory. For during all his career he urged the conceptual autonomy of mathematics defending his set theory against straightforward logical and philosophical objections. But ironically what resulted from his fight when it proved successful gave philosophers more powerful an instrument of logical regimentation of mathematics than they ever had before. In 4.5 I shall give a more specific reason why Cantor's infinite sets appeared to be so important for logic in spite of the fact that prima facie it was a purely mathematical invention.

When Russell discovered his famous paradox (the notion of class of all classes which are not their own elements is contradictory) this threatened Cantor's set theory in eyes of many including Russell himself. (Moreover so since Cantor applied an argument ad absurdum involving a very similar contradiction in order to distinguish between "different infinities".) So the Cantorian paradise (to use Hilbert's famous word) was troubled but it was too good to be given up for such a specific reason. At that point a logico-metaphysical study of the abstract notion of set supposed to provide Cantor's set theory with a solid logical ground, came into the play. Since these efforts qualify as *foundational* I shall continue the story in 2.8.

2.7 Sheaving (co-setting)

I have already mentioned sheaving informally in 2.1 and 2.2. Now I'm going to explain its mathematical notion. Term "sheaf" was first introduced into mathematics to denote a figure like this:



This figure helps to challenge the set-theoretical atomism according to which every geometrical figure, in particular a straight line, *is* a set of points: the sheaf suggests to regard a point (P) as a set of straight lines in its turn. Since a point can be represented as a set of lines just like a line can be represented as a set of points there is no reason to view such representations as reductions: lines remain to be lines, points remain to be points and things of one kind represent things of the other kind. Admittedly this duality between straight lines and points is limited: all Euclidean geometrical objects can be naturally thought of as sets of points but not as sets of lines. So points have a stronger representation capacity. But this doesn't imply that every geometrical object *is* a set of points ...

The above sheaf of lines is, of course, a set of lines. But let's see if this picture can be interpreted differently. For if it cannot be interpreted differently a sheaf would be nothing but a special kind of set or a set-based construction, and this is not what I meant talking about sheaving before. I start with a philosophical argument (of a phenomenological sort) challenging Cantor's assumption that any "collection of objects of thought" is a set (Cantor 1895) and after this return to mathematics.

Think about a series of events. Is it a set? It depends on how one thinks about it. When one recollects a series of events or perceives them in the real time the events are not given all at once but pass one after another. What I want to stress is not that the events form a linear order or another structure but that they are linked "disjunctively" rather than "conjunctively" in this sense: at one time one recollects or perceives one or few but not necessarily all events of given series (no matter what "one time" exactly means here). It is tempting to suppose that in the memory all events (or their mental representations) get stored in the conjunctive way forming a set like a collection of photos. But let's avoid any such additional hypotheses: at the phenomenological surface recollection works just like perception. Such a "disjunctive series" of events is a sheaf but *not* a set: all the events are linked through the perceiving or recollecting subject disjunctively . Since recollected events are objects of thought the recollection provides an evidence that objects of thought may be brought into a whole without making anything like set.

Let's now see how the sheaf pictured above helps to illustrate the spatio-temporal sheaving. Think of the sheaf as a system of tunnels in a rock rather than a construction in the outer space (the lines should get some width for it), and explore the sheaf from the inside but not from the outside as usual. (More about the idea intrinsic geometry in 5.2.2). The geometry of a single tunnel is Euclidean (1D), so finding yourself in any tunnel of the sheaf you observe nothing unusual. The only unusual element of the construction is point P because it allows for switching between different spaces (lines), and so links them disjunctively. P makes the lines into a whole but it is a very different kind of whole than a set of lines on given plane or a set of points on given line. For a set of lines is a kind of thing *in which* each individual line is found. But the sheaf of lines is something different (from the inner perspective): lines are nowhere (they are themselves full-blooded spaces, forget about the rock) and *in* each line there is a special element which it shares with any other line.

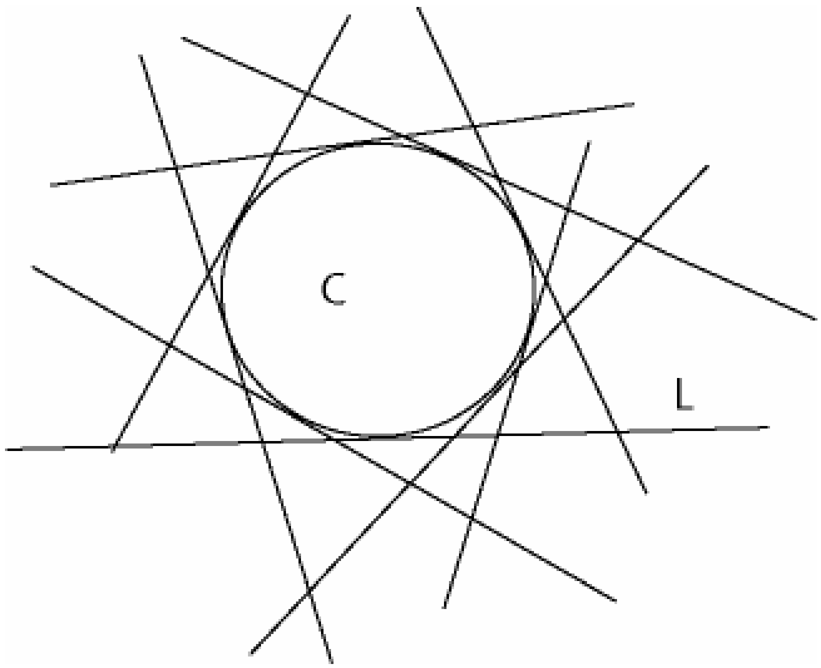
Cantor assumed that the representation of point of Euclidean n-space by a n-tuple of real numbers cancels the topology makes the notion of point set non-temporal and non-spatial (Cantor 1883a). I can see no reason to take this assumption for granted, and I believe that the spatial geometrical residuum has never been swept out from his set concept. Whether or not it is appropriate to call a sheaf "collection" is not important; what is important is that Cantor missed sheaving and wrongly assumed that examples of collections of points in an Euclidean space give a general idea of how things are made into a whole.

The term "sheaf" in the modern mathematics usually refers not to the above ancient construction but to a more advanced one which involves the spatio-temporal intuition even more explicitly. I shall call the latter notion of sheaf "modestly modern" because in 5.1 I'm going to present another update. To get the modestly modern notion of sheaf from the above sheaf of lines replace point P by topological space T and the lines by abstract sets, so that each open set O of T (each neighbourhood of each point of T) will correspond to particular set S (which might carry an additional structure like that of group or ring). Functions (morphisms) f sending opens to sets should satisfy two "gluing conditions" which amount to respecting two principle features of topology T: inclusions of its opens (to other opens) and coverings of its opens (by other opens). This construction may be viewed as a "continuously variable function" or "continuously variable set": one thinks of O as a continuously variable domain (continuous in the sense of topology T), and of S=f(O) as continuously variable value. Importantly these variations don't reduce to the choice of particular values from given list as when one talks about a family or a set of functions. For T is not a bare set of opens but a topology, and so its opens make part of topological structure and should be thought of as geometrical "places" but not just members of a set or list. Intuitively T may be thought of as a space-time: being asked about the value of f one asks to specify "where" and "when". (To repeat: this is not a simple case of indexing but a case of "advanced indexing" which for better shouldn't be called indexing at all. For topological space is not just a set of points or opens. It may be construed from both kind of sets but only through adding a structure, namely a topological structure.)

The duality between lines and points transforms in the latter construction into that between opens and points, and becomes non-trivial. If T is construed as usual as set PT of points provided with topological structure then one may consider usual set-theoretic functions s (pointwisely defined) from O to some fixed set R, (often the set of real numbers), and take S to be the set of such functions s possibly restricted by certain condition like differentiability. (Warning: not confuse functions s and f: the former sends points of O to elements of S while the latter sends O as a whole to S as a whole.) So one gets a "sheaf of functions" which is a standard example. However this is in fact not necessary in order to respect the gluing conditions since they are formulated in terms of inclusions and coverings of the opens without mentioning its points. This observation allows for regarding set-valued sheaf Sh (without additional structures) as just a more explicit construction of topological space: one considers abstract opens O (not sets but just "things") partially ordered in a suitable way (namely forming a *frame*) and regards set $S \leftarrow O{:}f$ as set of points of O. So one gets a construction comprising abstract functions (morphisms) satisfying certain algebraic conditions (inclusions of opens are morphisms too), one the one hand, and (sets of) points, on the other hand. The question “How much of topology can be done *pointlessly*, that is, without explicit mentioning of elements of S?” doesn’t have a simple answer (except “much”), so one can always think about duality between the two aspect of any given sheaf but not always in a precise mathematical sense. Precise mathematical dualities can be construed out of it (Stone dualities) but it is not the only interesting thing to do. For a mathematician looking for new constructions rather than wide generalisations it is perhaps more interesting to explore topological constructions which cannot be made in principle without application of pointless methods: they show that pointless topology brings indeed something new except new generalities (Johnstone 1983).

In 4.2-4.4 I’ll show how this duality in a more general form re-appears in topos theory, and in 5.1 I’ll make the link between sheaves and toposes explicit.

To see clearer the link between the two notions of sheaf given in this section (the ancient and the modestly modern) consider this example. I shall introduce it again through replacement of elements of the ancient sheaf. Replace point P by circle C and make the lines L to be tangent lines to the circle at its every point. So we get a sheaf in the modestly modern sense: to every open (open interval) U on C corresponds a set of lines tangent to points of U.



Although the example is trivial (because the tangent lines are in one-to-one correspondence with points of the circle) it shows correctly how look really interesting examples like the sheaf of tangent vector spaces over given differentiable manifold ("fibration").

Unlike the ancient case the "disjunctive link" between the lines in the latter example gets a certain structure, namely that of (topology of) the circle. Now if we think of our lines as "temporal stages" or "instant photos" of one and the same line L rotating around the circle then the character of this motion can be described in terms of (geometry of) C without appealing to the enveloping space (plane).

2.7.1 Open problems: structured points, stronger relativity of space, and co-setting.

In the ancient case we can also think about rotation of the line around fixed point P but then the character of the rotation will depend on the Euclidean properties of the enveloping plane and be not intrinsic. However the ancient notion of sheaf perhaps might be more useful than it seems at the first glance because it suggests this possibility: in order to internalise the situation we may reasonably associate with point P a rotation group or a topological structure. Such "structured point" would perhaps better serve for the generalised sheaving I'm trying to define. Notice that as a base of sheaving topological space T plays not the usual role of container (as any geometrical space normally does) but the unusual role of hook. It should be taken into consideration that the notion of space in the modern geometry is relativised in the following sense: one and the same thing like a circle can be viewed both externally as an object in a space and internally as a space in which other objects (like opens O) may live. (I shall talk about this relativisation giving more details in 5.2.3.) However this relativity doesn't clarify the situation completely. Officially only the intrinsic geometry (topology) of T (circle C) counts for sheaving but notice that the sheaved elements (lines L) don't live really in T (on the circle). Allowing myself a metaphor, I suggest that for sheaving we need a notion of space "turned inside out" or "everted" space having no inside but only outside. A point is just such a thing but the problem is that it has no internal structure, so turning a point inside out one gets nothing. Hence the idea of structured point (already appeared in the algebraic geometry for different reason (Cartier 2001). But perhaps it is only another metaphor for a concept which is wanted here.

Another possible solution of the problem could be a further relativisation of the notion of space, namely the relativisation of the usual distinction between extrinsic and intrinsic geometrical properties. Perhaps we should refuse from the idea of "purely intrinsic" geometry independent from any external framework and at the same time regard "extrinsic properties" of A not as essentially determined by its external framework F but as relational properties (in the sense of relations between A and F). In the given example this amounts to regarding geometry of the sheaf as determined by two kinds of relations: internal relations (say, between the lines and the circle) and external relations with Euclidean plane and other things (so the sheaf is not viewed as a self-sufficient construction anymore). In such a relational framework (relational in the double sense indicated above) the individuation of a sheaf (or any other object) may be viewed as making a boundary between internal and external relations. If we accept this we may also reasonably assume that the distinction is itself relational, that is, that it makes no sense unless both distinguished parties are present (like in the case of distinction between right and left or between big and small but not between black and white). Then objects with no external or no internal structure are banned (so this suggestion is different from the former). The first part of the ban is not particularly problematic: the idea of space as universal container which can be viewed only from the inside but not from the outside plays no longer any role in mathematics (although it continues to play a role in physics: the space-time of General relativity is not Newtonian absolute space but nevertheless it officially cannot be observed from the outside or at least not in a physical sense). However the idea of point as a primitive structureless object ruled out by the second part of the ban remains a cornerstone of mathematics just like it was in the ancient times. Hence the idea of structured point again . Another open problem concerns logical aspects of the notion of sheaf and its relations with the notion of set and set theory. Mathematical and logical means known to the date allow to devoid a sheaf from an embedding geometrical space but not from the setting. There is no mathematical means needed to justify the claim that lines in the above examples of sheaves don't form sets, or to specify in which sense they do, and in which sense they don't. (The fact that lines L in the second given example form a set even follows from the modestly modern definition of sheaf: C as a whole is open and maps to the set of all lines sheaved over it.) Thus the informal notion of sheaving as co-setting, that is, as an alternative integration mode, is not supported by the modestly modern notion of sheaf considered here. So a better account is wanted. Even if the spatio-temporal intuitions I relied upon in my informal arguments are misleading something should be done about this anyway because a systematic clash between common intuitions about important mathematical constructions and official formal accounts of these constructions is hardly healthy.

The categorical version of the notion of sheaf presented in 5.1 certainly sheds a new light on these problems but in my understanding doesn't resolve them.

2.8 Foundation

Foundation is an integration mode which works with higher scales of epistemic structures like a theory or a whole scientific discipline. The idea of foundation is roughly this. An epistemic structure T (theory, discipline) is partially ordered, and this partial order has bottom element usually called "foundations" in a narrow sense (while in a wider sense foundation is an integration mode); I shall call this bottom element "foundation kernel" to avoid confusions. A foundation kernel may comprise a set of principles, which in its turn can be structured in a similar way. For example, Euclid's *Elements* is a foundational kernel of all the ancient mathematics including topics like conic sections and optics. But *Elements* themselves have a similar internal structure: here we find postulates and axioms on which following theorems depend.

The structure of foundation in the very general sense just mentioned may be also attributed to any narrative – fictional or historical. The order of words in writing or speaking is total (and of course it has a bottom, that is, a starting point) but the order of meanings is only partial: at any stage a narrative may refer to some features presented earlier but not necessarily to all of them. This shows that foundation is a very basic integration mode organising our speech, writing, time perception, and memory (the global structure of anyone's personal memory has a natural starting point). However the idea of foundation as it is used in theoretical sciences, and in particular in mathematics, usually requires more:

(i) It often makes a sharper distinction between foundation kernel and the rest of the founded body of knowledge. The idea is this: given foundation kernel F and certain recursive (repeatable) procedure P (or a number of such procedures) to generate the whole T through application of P to F. Two principle examples of P are logical deduction and constructive principles like substitution. In the case of formal axiomatic systems discussed in what follows both aforementioned types of generic procedures are in the play. Given a set of axioms the rest of the theory is obtained through deduction. But on the syntactic level formulae are obtained from chosen basic symbols and specified construction rules, which allow for generation of all possible formulae from the basic alphabet.

(ii) Foundation is often used for justification purposes, so F justifies the rest of T through P. Then F itself must be justified independently by different means.

(iii) Foundation may be used jointly with generalisation. Then F involves only most general issues while P has a specification function. Examples are logical and ontological foundations: logic is viewed as a general account of scientific reasoning, and ontology is viewed as a general account of what there is.

What I want to stress is the fact that when one drops some or all conditions (i-iii) one still has an integration mode (whether one will call it foundation or not). For (i) notice that even in the case of Euclid's *Elements* the distinction between foundational kernel and the rest of the theory is not sharp – in one sense one may count the whole Euclid's work as a kernel (this is the intended meaning of the title), and in another sense only Postulates and Axioms. Most of contemporary introductory mathematical and scientific textbooks are similar in this respect. The most important class of examples of foundational structure dropping condition (ii) is provided by empirical sciences: here we might have a well-distinguishable foundational kernel (think of Newtonian mechanics) but the justification of T comes (at least partly) not from inside the kernel but from particular experiments and observations. If the foundational kernel is a set of primitive elements, and the rest of T is obtained through constructive procedures the foundation will have nothing to do with generalisation, so (iii) is dropped. Finally recall the example of simple narrative structure where (i-iii) are all dropped – it still works as an integration mode!

My general conclusion is that unlike justification and integration foundation is not a necessary epistemic requirement – whether foundation is understood in a stronger sense, which assumes (i-iii) or in weaker sense of the partial order with bottom. Foundation is nothing but a specific integration mode associated with specific communication and education practices. Foundation may be very useful indeed: studying one and the same book like Euclid's *Elements* many people may get prepared for doing different things and still well communicate with each other. However with de-centralisation of education and information flows this is no longer the only reasonable strategy. It is not evident that foundation (even in the weak sense) which fits well traditional printing technologies helps to organise electronic information flows equally well. In any event, I can see no reason to regard the idea of knowledge without foundation as an heresy, and can see a need for alternative higher-scale integration modes. A non-founded knowledge should be definitely dissociated from not justified and not integrated knowledge. Thinking about alternatives to foundation I mean rather stronger than weaker integration principles. I shall say more about this in 6.3.

<u>2.8.1 Foundationalism versus Anti-Foundationalism</u>

Few words about this issue widely debated by philosophers of mathematics during the last decade. I definitely reject the foundationalism as the idea according to which certain foundations of mathematics exempted from any possible revision may and should provide a secure framework of doing mathematics from the moment when this foundations is assumed to the indefinite future. My principle reason for the rejection of the foundationalism is not a frustrating feeling that no suitable candidate for the role of eternal foundation is available but an observation that our scientific and mathematical theories are generally *more* endurable than our buildings just because unlike buildings theories can survive even radical revisions of their foundations. So the idea to make mathematics more secure through making it more building-like is, in my view, completely wrong. Think about Pythagorean theorem for example. Proposition 1.47 of Euclid's *Elements* is purely geometrical and says roughly this: if one constructs squares on each side of given right triangle then the biggest square is composable from pieces of the two smaller squares. A contemporary elementary geometry textbook says this: let's x, y, z be lengths of sides of right triangle; then $x^2+y^2=z^2$. This is one and the same Pythagorean theorem but foundations assumed in the two cases are very different, in particular the latter proposition needs the notion of real number while the former doesn't involve numbers at all (except the natural number 3 used in the definition of triangle). Even taking for granted that the set theory provides good foundations for today's mathematics it would be absurd to think of today's mathematics as started sometime in 20th century but not some 2500 years earlier. The analogy between a building and a theory is not pointless but shouldn't be taken too straightforwardly. So I'm certainly agree that foundations should be without foundationalism, as Shapiro (1991) puts it. But unlike Shapire I don't think that foundation as an integration mode is epistemologically indispensable. Moreover I think that foundation is insufficient for integration of knowledge at higher levels of its organisation. For example, foundation doesn't help us to explain in which sense Euclid's Proposition 1.47 and proposition $x^2 + y^2 = z^2$ from today's textbook are two different forms of the same Pythagorean theorem, and so doesn't allow for integration of mathematics through its history. The case of Pythagorean theorem seems to be simple, so one can easily explain the equivalence of its two versions. However in other cases the task might be not easy at all. I think that following generations of mathematicians would be more grateful to us for a tool helpful for securing the continuity of mathematics through conceptual and foundational changes rather than for an offer of allegedly secure foundations which they will likely reject very soon as outdated. In 6.5 I show how the category theory can be used as such a tool.

2.8.2 Historical remarks on and about set theory (II: continued from 2.6.1)

Let's return to sets. After Cantor's set theory was troubled by Russell paradox and other logico-semantical difficulties a solution was sought in development of formal theory of sets based on a system of formalised logic. Such a theory aimed at providing a foundation for mathematics and perhaps also for empirical sciences. So we got ZFC (Zermelo-Fraenkel set theory with Choice) and other similar proposals. ZFC until today is viewed as an (or even *the*) official foundation of mathematics at most philosophical departments in spite of the fact that it plays very little or no role in the actual mathematical research outside the set theory itself (and definitely plays no role in empirical sciences) .

The most important attempt to match foundational claims of set theory with existing mathematical practice has been undertaken in 50th by a group of French mathematicians under the nickname of Nicolas Bourbaki. Bourbaki's project aimed at filling the gap between common mathematical practice and the model of formalised mathematical discourse suggested by logicians and set theorists. This was a matter of compromise: although Bourbaki didn't achieve (and perhaps didn't mean to achieve) a formalisation of mathematics to the extent of ZFC or similar formal theories, and from the point of view of logical purists continued to do mathematics "naively", in eyes of a major part of mathematical community Bourbaki's mathematics was exceedingly formal. Bourbaki's project which lasted some 40 years was, in my opinion, the most important foundational project in mathematics of 20th century. It had an immense impact on mathematical education, and today Bourbaki-style definitions and proofs became common. But Bourbaki also made it clear that bold claims according to which mathematics is "basically" set theory which even today are not unusual in philosophical departments are not justified. Bourbaki made Cantor's intuition real and reconstructed nearly the whole of mathematics as being "about sets". However this didn't allow for reduction of mathematics to set theory – if by "set theory" one means anything like ZFC and by "reduction" anything like (reversed) logical deduction. The fact that basic definitions and principle theorems in , say, group theory or functional analysis can be recasted in Bourbakist set-theoretic terms doesn't imply that these theorems are deducible in ZFC. From the logical point of view a part of the problem is that the concepts involved appear to be of higher logical order so the deductive capacities of first-order theories like ZFC are not sufficient for managing them. To switch to higher-order logics (as does Shapiro 1991) remains an option but this begs the question as we shall shortly see.

The development just described concerned mathematical but not logical notion of set although in ZFC like elsewhere the two are, of course, entwined. A fundamental (to mean "foundational") difficulty of all axiomatic set theories is this: these theories supposed to provide a logically sound (mathematical) notion of set use another (logical) notion of set as a part (semantics) of their logic. They not only provide setting with a logical framework but also use setting for logical purposes.

The intrinsic link between setting and logic suggests that the two should be developed together from the outset. However in the ZFC and its likes a different strategy is taken: two notions of set, logical and mathematical are kept apart. What precisely happens is one of the following options, or some combination of these:

(1) The logical notion of set required for semantic purposes is left informal and highly sensitive to philosophical attitudes and ontological commitments. Frege, Russell and their followers would consider "everything that there is" as the only domain where all logical variables range; some others after Boole would make logical variables range over domains

chosen occasionally in applications, other people would refer to intuition, common sense or mathematical practice which allegedly makes the notion of logical set (or domain or class) self-evident at least in an appropriate pragmatic sense.

(2) In constructive approaches logical and/or mathematical sets are supposed not to be given in advance but brought about through the corresponding axiomatic theory itself. In most cases constructive approaches fall under (1): we have an axiomatic theory such that its semantic is only informally interpreted in terms of construction and generation rather than existence. Such are constructive and intuitionistic set theories: they differ from their classical counterparts by the kind of logic (and its semantics) but the principle scheme remains the same. However most radical constructive approaches like one in (Hilbert 1904) don't fall under (1): here only particular symbols and well-formed strings made of these symbols are allowed as values of logical variables, and so theories becomes purely formal and not interpreted in the usual sense at all.

(3) The notion of logical set (semantics) is treated formally (axiomatically) in the same way as the notion of mathematical set. In particular sets in the sense of ZFC can be taken as logical sets for another theory or for ZFC itself (which is not a good idea, technically speaking). This starts a regress since a logical set so construed requires another (meta-)logical set. I shall call this regress *semantical* for further references. There are again few possible attitudes toward this regress:

(3.1) One might push the semantical regress forward in the hope that it will end up with some sort of self-evident semantic like in case (1) ;

(3.2) One might stop regress deliberately at certain point for pragmatic reasons.

(3.3) One might take a relativistic stance after Skolem, assuming that a formal axiomatic set theory provides not a notion of set per se but rather relation between two notions of set: one assumed for semantic purposes and the other developed through given theory.
(3.4) One might try to make the regress into a circle, making a notion of set obtained axiomatically to provide semantic of this very theory.

Foundationalism falls under (1) and (3.1) but some version of (1), particularly those in which semantics is allowed to be flexible and dependent on current mathematical practice, can be hardly qualified as foundationalist. (3.2) is Shapiro's "Foundations without foundationalism". I'm agree with Shapiro that foundationalist dream to reach a bedrock of self-evidence through formalisation (possibly involving a finite number of formal $(\text{meta})^n$ – theories) proved hopeless and should be given up. However I don't find appealing Shapiro's stoic defence of building on the sand either, although it has some morally attractive features. To my mind Shapiro's view smuggles too much of frustrated foundationalist desires the author is trying to fight.
Shapiro gives up foundationalist ambitions to explain to mathematicians what they are "really" doing, and to teach them what they should do, and replaces this by a modest task of modelling existing mathematical practice with formal logical means. I 'm agree that philosophers and mathematicians involved into research on foundations are not in a position to prescribe epistemic norms and standards to the rest of mathematical community. But I believe nevertheless that philosophy and foundations of mathematics should contribute in a way to what the rest of the mathematical community is doing. For otherwise philosophy and foundation of mathematics becomes a marginal activity without a clear purpose. So I 'm not agree with Shapiro's idea that foundations must only reflect or model actual mathematical practice without being involved in it. Why to model mathematics if it is fine by itself?

Historically it is not the case that all foundational (if not foundational"ist") revisionary proposal failed in the real mathematics. Although Hilbert's most radical proposals did fail indeed his axiomatic method as presented in his "Foundations of geometry" became standard in mathematics. Bourbaki's set-theoretic foundations, however compromised against formal standards of logicians, serve in mathematical education and research until today and allow for a continuous replacement by new categorical means. I cannot see any reason why today the situation should be very different. Rejecting foundationalism with Shapiro I nevertheless believe that foundations, if any, should influence and suggest revisions of actual mathematical practice but not only model, reflect, and revise themselves according to current mathematical practice. In other words foundations (if any) must effectively integrate actual knowledge and serve for educational purposes.

Skolem's relativism (3.3) in my view is a serious and challenging option. I don't understand why Shapiro bluntly calls it absurd. We know from physics how fruitful relativistic arguments might be for science, and we know how important was relativisation of the notion of geometrical space achieved in the geometry of 19th century (see 5.2.3 below). Why thing should be different in logic, and moreover in mathematical logic? A conservative instinct to conceive relativisation of convenient concepts as a threat to rationality is profoundly misleading, in my view. Rationality is all about relativity and relativisation. To develop a formal relativistic account of sets a priori seems me more rational an option than to appeal to intuition, common practice, ultimate metaphysical structure of reality and the like. True, any new turn of relativisation requires a revision and reconstruction of existing models of rationality but this is how rationality extends and develops but not how it corrupts.

The problem of Skolem 's relativism, however, is that it has been hardly clearly spelled out by Skolem himself. He apparently meant (3.3) referring in (1958) to the theorem known today by his and Loewenheim's name, which shows explicitly that the notion of set developed through

ZFC and its likes is sensitive to the background logical set, namely to its cardinality. (Models of ZFC corresponding to logical sets of different cardinalities are non-isomorphic; the very fact that ZFC with all its infinite cardinalities can be modelled through a logical set of minimal infinite cardinality is often regarded as a paradox) The Skolem-Loewenheim theorem (I speak collectively about all of them) is hardly surprising if one thinks about it without philosophical prejudgements and utopist foundationalist projects in mind. However in the same paper Skolem speaks about relativity of mathematical concepts in a different sense, meaning a kind of if-then-ism with respect to basic mathematical notions revealed by their formalisation (one has just as many different notions of set as axiomatic theories of sets). He also makes here remarks in the vein of Hilbert's finitism mentioned in (2) above. So Skolem's relativism is in fact a mixture of many different things, but anyway, in my view, it is interesting to develop these ideas rather than reject them outright. In 2.7.1 I have already made a relativistic proposal, and in 6.3 I shall show how category theory contributes to the relativistic trend in mathematics (I shall argue that the if-then-ism indeed should and can be avoided).

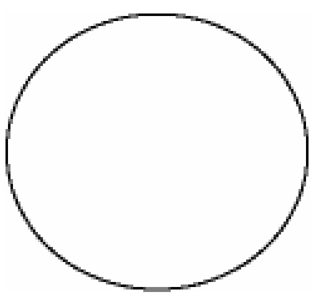
(3.4) is usually seen as illegitimate on the ground that a circle so obtained will be vicious. Even if ZFC is used to provide semantics for ZFC, one needs to distinguish between these two copies of ZFC as well as between an object-language in which one formulates a theory and a meta-language in which one discuss semantic of this theory. I am not sure myself that the horror of circularity must be always respected. Recall that Hilbert's axiomatic method from a traditional point of view (stressed by Frege in his discussion with Hilbert, see Frege (1971)) involves a circularity: one state axioms about things which are not yet defined, and only after finding a model satisfying the axioms one shows what these things could be (but only in a rare case of categorical theory - what these things definitely are). Early practitioners of the modern axiomatic method including Fraenkel and Hilbert didn't separate an object language from a meta-language as rigidly as most logicians do this today, and treated "external axioms" of model-theoretic character on equal footing with axioms of the theory in questions. Prominent examples are Hilbert's completeness axiom in his (1899) according to which (in modern terms) his model of Euclidean geometry cannot be possibly extended, and Fraenkel's restriction axiom according to which his model of set theory cannot be possibly restricted. Interestingly Carnap who is responsible for the distinction between object-language and meta-language defended such external axioms from their critics and didn't apply the distinction between language and meta-language for this case but used a second-order logic instead (Carnap&Bachmann 1963). This lets me think that (3.4) shouldn't be rejected outright on a general ground. In **4** we shall see how this problem reappears in a category-theoretic framework.

Constructivist approach (2) have been first matched with the formal axiomatic method by Heyting through his formal version of the intuitionistic logic in (1956). Nevertheless the original tension between the constructivist and the formal axiomatic ways of reasoning persists until today. Remind that in Hilbert 1899 definitions play only an auxiliary role and are dispensable. In constructivist approaches, even if they are developed in a Hilbert-style formal axiomatic framework, definitions strike back; as Macintyre (2003) predicts "one can easily imagine the subject being called Definibility Theory in the near future". Today's revival of mathematical constructivism (by which I mean a trend in mathematics and logic rather than an external philosophical position concerning the nature of mathematical objects) is closely associated with the category theory and categorification, as we shall see.

3. Grouping and Categorification

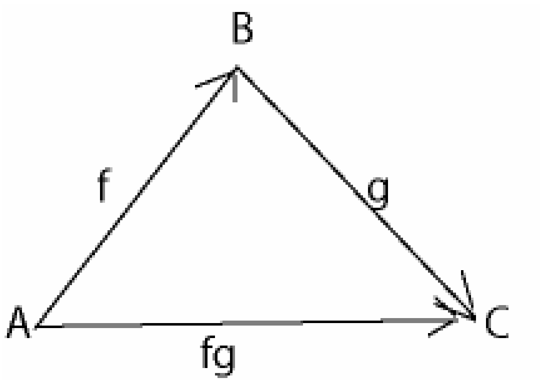
The integration modes considered above are all centred which means that they work like mereological summing: integrated elements are kept together by some sort of “whole” or “centre”. (The term “centred” is motivated by the case of sheaving.) The only partial exception was the relation integration mode apparently allowing for linking integrated elements to each other directly. But as I have already noticed this feature of relation remains unaccounted by the usual analysis of relation as predicate. In the case of categorification we also have an analogue of whole (a category) which makes it natural to speak about categorification as an integration mode. However as we shall now see a category is construed not through attachment of its elements to the category but through attachment of its elements directly to each other. This differs categorification strikingly from the above integration modes, and as I believe extends the importance of category theory beyond pure mathematics. Grouping is an integration mode which I consider as “pre-categorification” for explanatory purposes although it is important for its own and not necessary for categorification. In a sense grouping is centred and in a different sense it is not as we shall shortly see. This makes grouping a natural link between centred integration modes and categorification.

Consider a circle.

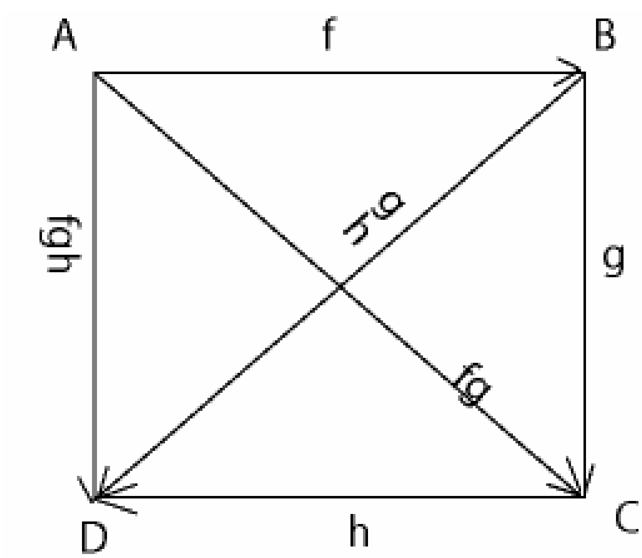


A usual way to do this is to go through the pair generalisation/exemplification: the term “circle” invokes the general notion of circle (with or without the help of definition) which gets exemplified by drawing on a paper or only in the imagination. After doing this use setting and think about the class (set) of “all” individual circles. A further integration of individual circles may be achieved as follows. Take your class of (all) circles and consider *transformations* turning circles into each other. These transformations include displacements and scalings (changes of circles' sizes). Marking some points on the circles you can also include a third kind of transformations: rotation of given circle along itself permuting marked points but leaving the circle as it is and where it is. Finally provide each circle with a fake *identity* transformation which doesn't transform anything but leave any given circle and any marked point on it as and where they are. Notice that we talk here about particular transformations of circles but not about global transformations of the whole class of circles. Given two circles there are many such transformations turning one into the other, and given one circle there many different rotations of the circle along itself but only one identity transformation. If the target circle of certain transformation is also the source circle of another transformation the two transformations are composable: transformation $f:A\to B$ composes with transformation $g:B\to C$ bringing transformation $fg:A\to C$. The composition of transformation has the associativity property: $(fg)h=f(gh)$. (But the commutativity in the general case fails: gf even doesn't exist if A and C are different.)

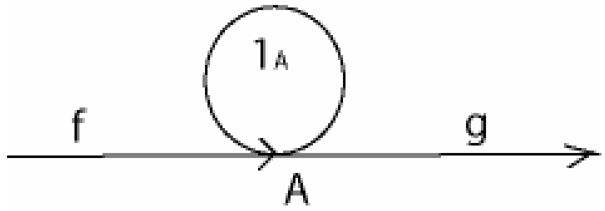
What I just described is an example of *category.* The general notion is introduced as follows. One considers a class OB of *objects* and another class MR of *morphisms*, and specifies for any morphism two objects (which may coincide) called its *domain* and its *co-domain*. Morphism f with domain A and co-domain B is written $f:A\to B$ and intuitively thought of as a transformation of A into B like in the case of circles. If co-domain B of morphism $f:A\to B$ coincides with domain B of morphism $g:B\to C$ the two morphism are *composable* which means that there exist morphism $fg:A\to C$ uniquely determined by f and g:



The composition of morphisms is associative: (fg)h=f(gh)= fgh.

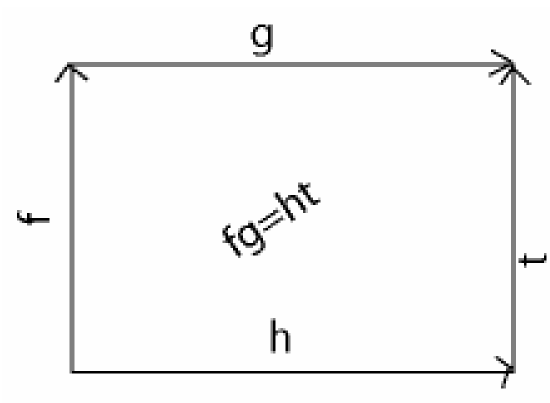


Each object A is provided with identity morphism 1_A satisfying the following conditions: for any “incoming” morphism f, and any “outgoing” morphism g we have $f1_A$=f and 1_Ag=g:



Since the latter definition implies that an identity morphism is uniquely determined by its object identities may formally replace objects, so objects in the notion of category are in fact redundant. I continue nevertheless to speak about objects for convenience as it is usually done in mathematics. To take the replacement of objects by their identity morphisms with a philosophical seriousness is a challenging task but I shall not pursue it in this paper. (See my (forthcoming) where I informally discuss identity in categories.)

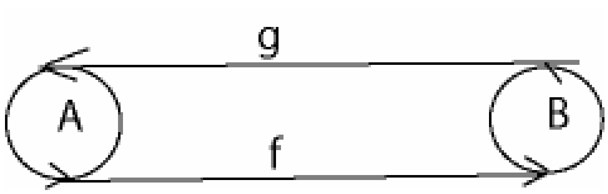
Equations like h=fg or fg=ht (where f,g,h,t are morphisms and the composition sign is omitted as above) are diagrammatically represented in the category theory by *commutative diagrams*. For fg=ht the diagram is this:



and by saying that it is commutative one means that fg=ht holds indeed. The fact that given categorical diagram is commutative may be also spelled out in this way: whatever path you take on the diagram following the arrows you get the same result. (But beware that the result here is not a destination like object B but a morphism resulting from the composition of all the pieces of given path). Following (Lawvere 1963/2004) I write the composition in the direct geometric order to the contrary of the algebraic habit to right it in the reversed order like in formula g(f(x)) for the composition of two functions.

Let's return to our circles. Observe that transformations of circles described above have an additional strong property left outside the general notion of category just introduced: they are reversible. Informally it is clear what this means: given transformation f of circle A into circle B or into itself one may always consider reverse transformation g "returning everything at place", and moreover such that f appears to be reverse with respect to g in the same sense (check that neither part of this conjunction implies the other).

<u>Definition 1</u>: morphism $f:A\rightarrow B$ is reversible or isomorphism (iso for short) iff there exist morphism $g:B\rightarrow A$ such that $fg=1_A$ and $gf=1_B$.



If such g exists it is unique (check).

As we shall immediately see this additional property of reversibility allows us to describe the construction with the older notion of *group*. Recall that group is a set of things f,g,… provided with a binary operation fg=h and containing a special element 1 such that for any f f1=f1, and moreover (reversibility) that for any f there exist g (called reverse of f) such that fg=gf=1. Thinking about elements of given group as morphisms and about the group operation as composition of morphisms we see that any group is a special case of category having only one object (one identity) and such that all its morphisms are reversible. How can this help when circles in our example are many?

This is how. Instead of thinking of a plane full of circles of different sizes think about just one circle CrS moving around the plane and changing its size in all possible ways. The fusion of all individual circles into one implies fusion of some transformations too. All identity transformations get fused into one identity transformation 1. Rotations and scalings (changing of size) of different individual circles should be fused in a nice way without allowing them to collapse into one: think about different circles undergoing the "same" transformation and then apply this transformation to CrS. (I leave it to the reader to specify what the "same transformation" should exactly mean.) It doesn't work equally well with motions because the motion in empty plane hardly makes sense or at least a mathematical sense: how we could distinguish between different positions of CrS if the plane is empty? There are two possible solutions. A "local" solution is to attempt (like in the case of self-transformations) to define what is the "same" motion of different circles, and then to apply such generalised motions to CrS. (Say, "two steps ahead" may be thought of as the same motion no matter who, where, and when goes two steps ahead.) The idea to treat motions as self-transformation clashes with usual intuitions about changes and motions but speaking formally this works: all the transformations of all our circles are described through the group of transformations of CrS. (A worry remains whether we didn't loose something about motions of individual circles. This worry is not ungrounded as I shall shortly show but as far as we are working on Euclidean plane we don't in fact loose anything.) The other solution suggested by Klein in his Erlangen Program (1872) is more radical and involves a revision of an earlier step: instead of the fusion of circles and their transformations consider the transformations of the circles as (induced by) self-transformations of the plane (with all possible objects of different kinds found on the plane). Then the group of transformations of circles my be specified as a subgroup of transformations of the plane.

The integration mode of *grouping* just described gives me an opportunity to stress the impact of metaphysical schemes and associated intuitions to mathematics. We have started with a common conceptual scheme which involved (i) the general notion or *form* of circle CrF, (ii) examples of circle CrE and (iii) *class* CrC of all such examples on given plane. Then we have supplemented spatial intuitions associated with the notion of class with temporal intuitions of change and motion associated with notion of *substance* regarding all the CrE of class CrC as "temporary states" (think of photos) of its substance CrS. Interestingly this conceptual operation involves a spatial intuition of higher order: I mean the usual way of thinking of general forms as located *over and above* its instantiations, and of substances (as the term suggests) as located *under and beneath* its states and attributes. One might think that this intuitive and metaphysical business has little if anything to do with or at least can be strictly separated from the mathematical construction. However it doesn't seem to be the case. The notion of class of circles doesn't imply anything like its group structure. So the temporal intuitions make a great mathematical job indeed in this case. It is not easy to sweep them out even at the final stage: how to talk about geometrical transformations without mentioning that they are transformations and without saying what they are transformations of? This can be done, of course, through some sort of formal construction but it would be against the common opinion of mathematicians themselves to call only such purified formal constructions mathematical.

As we see to shift from a group-theoretic to a categorical description of our example we need only to allow our circles to be many skipping their dubious fusion. A category where all morphisms are reversible, i.e. are isomorphisms, like in the given case is called *groupoid.* It is indeed usual in the category theory to identify isomorphic objects but in some geometrical contexts like ours it might be not a good idea. For identifying all our circles we loose the sense of the geometrical space they are in, and applying transformations only to the space as a whole we loose their local features which matters any time when local and global properties of the space (unlike the Euclidean case) are not the same.

This is why I regard grouping as “half-centred” integration mode and as an intermediate (albeit formally redundant) step to categorification: like in the case of category we think here about transformations but only as self-transformations of the same object.

3.1 Endurance and Perdurance

Notions of groupoid and category seem to be so simple and natural that one may wonder why they have not been formulated earlier in the history. One hidden difficulty might be of metaphysical character. Notice that the traditional spatio-temporal intuitions associated with the notions of class and group don’t really apply to the notion of category smoothly.

Common intuition and traditional metaphysics suggest two ways for thinking about processes:

(i) one makes no essential difference between space and time, and treats temporal stages of changing objects on equal footing with their spatial parts;

(ii) one relies on the notion of changing substance; this allows for multiple spatially distinct changing substances.

Taken ontologically seriously (i) and (ii) become basic claims of two rival metaphysical views concerning processes: (i) pendurantism, and (ii) endurantism (Lewis 2000). Proponents of each view argue that only one of (i)-(ii) is correct. Pendurantists often refer to space-time of Relativity saying that a rigid distinction between space and time apparently implied by the view of their opponents is a scientific anachronism. Endurantists find themselves mostly in a defending position appealing to philosophical tradition, natural language, logic, and common sense rather than science. I shall not take sides in this metaphysical dispute but remark the following. First, the idea of changing substance and related intuitions continue to play an important role in contemporary mathematics and science, in particularly through the group theory (which is particularly important for the particle physics). Second, remark that in a category (given the intuitive interpretation of this notion given above) the two allegedly incompatible ways to think about changes are mixed up. Recall our group and groupoid of circles. The group is an endurantist construction: we have one circle-substance moving around the plane and changing its size. The groupoid seems to be a perdurantist construction: the circle-substance is replaced by a class of circles which can be thought of as "momentary states" of the substance. But in fact it is not: recall that circles of the groupoid transform also to themselves (identities and rotations).

Thus the notion of category gives reason to think that the endurantism versus pendurantism controversy might be not a deep controversy after all, and the two ways of thinking about processes can be combined within a categorical framework. The usual interpretation of categorical morphisms as transformations doesn't fit indeed usual spatio-temporal intuitions (or at least it doesn't fit the endurantism versus pendurantism dilemma) but transforms them in a non-trivial way. In my view making this fact explicit and developing new intuitions is a better strategy than trying to sweep spatio-temporal intuitions out of the category theory like it has been done in the set theory since Cantor.

3.2 Sets as a category: ZFC and SFM

Another example of category is category S of sets where objects are sets and morphisms are functions. Using Bourbaki-style constructions of mathematical objects like groups or topological spaces as sets with a structure one may define various categories of such structured sets specifying set-theoretic functions between their base sets in a suitable way, namely making functions preserve or "respect" corresponding structures like in the case of homomorphisms of groups (given groups G,G' f:G→G' is homomorphism iff ab=c in G then f(a)f(b)=f(c) in G') and continuous transformations of topological spaces. Further categorical constructions can be made through taking elements of earlier constructed categories or the categories themselves as objects of new categories. Morphisms between categories are called *functors* and the general definition of functor requires nothing but respect of categorical structures which amounts to certain equational (commutativity) conditions. A simple example is forgetful functor from (the category of) groups to (the category of) their underlying sets. This example shows that the talk of "respecting" structures better describes the property in question than "preservation" of structures.

All these categories we shall call *concrete* in the following sense: they are made of objects and morphisms given in advance (sets and functions, groups and homomorphisms, etc.); usually this means that the objects and the morphisms are defined set-theoretically. However one may look at the situation differently: to begin with the general abstract notion of category explained in the previous section (involving only abstract objects and abstract composable morphisms between objects satisfying simple conditions mentioned above), and then impose certain algebraic (usually equational) conditions on morphisms and their compositions to the effect that the resulting category has needed properties. Categories obtainable in such a way I shall call *abstract* (definitions of concrete and abstract categories given in this section are preliminary, see 4.4 below for further details). In particular an abstract theory of categories may be developed through considering the category CC of "all" categories (see 6.6 below about it). A similar approach can be applied to the notion of set: one looks for an abstract category SC which is like S. To see how categorical equational conditions may replace usual set-theoretic definitions recall the categorical definition of the notion of isomorphism (Def. 1 above) and observe that in S it coincides with the usual notion of isomorphism as one-to-one correspondence between elements of two sets. In order to specify sets by categorical means one needs to find specific categorical properties of S and assume them in SC. Even if one will not be in a position to prove a meta-theorem showing that SC and S is the same thing (thinking of S as constructed on the basis of ZFC or another axiomatic theory) but nevertheless SC will reasonably (and hopefully better than ZFC) resemble S as this category is used in the mathematical practice then S=SC could be taken as a definition.

The purpose of this section is not to realise this project but to compare it with the usual idea of formal axiomatic theory of set. For references I choose ZFC on the formal side and Sets for Mathematics (SFM for short) by Lawvere and Rosebrugh (2003) on the categorical side. Let me start with what the categorical and the formal axiomatic treatments of sets share in common: in both cases sets are thought of as abstract "things" and then an account is given of how these things relate to each other. In both cases the idea is that the "behaviour" or "mutual disposition" of sets with respect to each other determines what these things *are*. I stress this point because this feature is often pointed as specific for the abstract algebraic and categorical approaches as if it were alien for the formal axiomatic method. As far as I can see this feature is fully present in theories like ZFC or even in Hilbert's seminal work (1899) where primitive geometrical objects are taken to be just "things" of two different types. The fact that ZFC uses membership $\in$ (the relation between an element of set and the set it is element of) as primitive doesn't change, in my view, this general point because $\in$ is first of all a formal relation between abstract sets, and its interpretation is a different matter. So the real difference between the two approaches is neater. One might feel that the term "behaviour" better suits the categorical approach while "mutual disposition" is more appropriate in the case of formal axiomatic. This is hardly surprising given that the notion of category involves the notion of operation (composition) while formal relations used in axiomatic system might have different informal meanings or have no fixed informal meaning at all. From the formal point of view this is not an important difference. However SFM is not a formal theory in the sense of ZFC. This crucial point I shall analyse later.

Another thing ZFC and SFM share in common is a foundational difficulty: both use some notion of set before they start to account for it, and neither of the two theories suggest a natural remedy which could be used after the theory in question is done. In the case of ZFC I mean the notion of logical set (class) assumed in the underlying logic of this theory (see 2.8.2 above), and in the case of SFM the notion of class involved into the definition of abstract category given above: one needs for it a class of objects and a class of morphisms between each pair of objects. In the construction of category of categories (CC) the latter problem is treated and perhaps resolved so this feature of SFM shouldn't be generalised upon all the category theory. I postpone the discussion on this foundational problem until 6.6.

Now let's see the difference. ZFC takes membership $\in$ as the only basic (primitive in the logical sense) set-theoretic relation. Why? Note that the question has two parts.

The first part is more obvious: Why $\in$ but not another relation? Another relation which suggests itself as basic set-theoretic relation is the sub-set (parthood) relation $\subseteq$. However this doesn't work. For $\subseteq$ can be easily defined in terms of $\in$ ($A \subseteq B$ iff for any X $X \in A$ implies $X \in B$) but not the other way round. (The categorical account of sets presented below will make it clear why no set theory can be developed through $\subseteq$ uniquely.) So the answer is basically this: because it works. This answer doesn't imply, of course, that nothing else can work better.

The second part of the question is less obvious, more general, and in the given context more important: Why the choice between $\in$, $\subseteq$ or anything like that? Well, this concerns the very idea of axiomatic method in the form used in ZFC. The idea is roughly this (leaving model-theoretic issues aside): to assume a system of formalised logic (as if it were independent from setting – which is obviously not the case), choose primitive terms in minimal possible number (for the reason of parsimony), and on this basis to lay out a list of axioms and definitions. Another requirement of informal and pragmatic nature says that the resulting formal theory should reasonably comply with its informal prototype. It is not unlikely that ZFC is the best or nearly the best possible theory satisfying these requirements. Nevertheless, as I have already argued, it complies with current mathematical practice (outside the set theory itself) too poorly, so one may think of improvements at the expense of general principles of building formal theories. Even if the choice of $\in$ as basic set-theoretic primitive is wholly justified in the narrow sense mentioned above a working mathematician may still argue that the role of $\in$ in ZFC and similar theories is overtly exaggerated while other features of sets, which he sees as more essential, are neglected. Then the discussion comes down to philosophical and epistemological issues: the supporter of ZFC will say that the way this theory is built is the only way to make a theory appropriate in the given case, and so the mathematician has no other choice but between ZFC and its improved versions. The mathematician's normal reaction will be that of passive resistance: simply not use ZFC in his work. If he is not particularly interested in philosophical issues he will accept the claim of foundational importance of ZFC, then forget about it and do different things (like most Soviet scientists did with official Marxist foundations of their sciences). Perhaps this is a caricaturised picture of what indeed happens in mathematics but not too much so.

Hence "Sets for Mathematics". In order to account for "behaviour" or "disposition" (I deliberately mix up the metaphors in order to suppress their suggestive power and reveal what is behind them) SFM uses not relations like $\in$ or $\subseteq$ but functions $f{:}A \rightarrow B$. Are functions relations? The question in the given context is tricky. If we think of functions and relations set-theoretically ("pointwisely") then f is indeed a binary relation. But it is relation between *elements* of A and B, not between A and B themselves. Unlike membership $\in$ function $\rightarrow$ is not a relation in the sense of formal logic. It is a particular mathematical object best viewed as comprising its domain and co-domain and "something in between". As I have already mentioned objects A,B as things of a different sort than morphisms are easily dispensable and can be replaced by their identity morphisms. Then we get a network of functions including identity functions linked through compositions. This step apparently takes us beyond the idea of linking sets through functions in order to learn what the linked elements are. But whether this step is made or not (which mathematically prima facie doesn't matter, or at least doesn't matter at the level of elementary constructions we are talking about) it is clear that saying that both ZFC and SFM "relate sets to each other" in different ways one should be aware that the notions of relation involved in the two cases are very different. In order to respect the standard mathematical and logical terminology in the case of SFM we should speak about morphisms instead of relations pointing to the fact that this is not the same thing. (The usual logical notion of relation can be reconstructed categorically but this is a different matter which I leave aside.) However the traditional philosophical notion of relation seems to be relevant to the categorical notion of morphism, and it would be wrong to overlook this for terminological reason. I shall return to this issue in 6.3

Let me demonstrate a categorical recasting of basic set-theoretic notions with two examples which are both important for what follows: I shall present the categorical version of the notion of subset (called *subobject*) and the categorical version of the set-theoretic membership.

The idea is to identify subsets of given set A with isomorphism classes of injective functions to A ("mappings into"), and specify the notion of injective function (the categorical notion of isomorphism we have already introduced). Recall that function is called injective or "into" when different elements of the source (domain) set are mapped to different elements of the target (co-domain) set. Here is the categorical definition (we are working in abstract category C):

Definition 2: morphism f is monomorphism (mono for short) iff

(*) for all morphisms g,h (given gf and hf exist) gf=hf implies g=h,

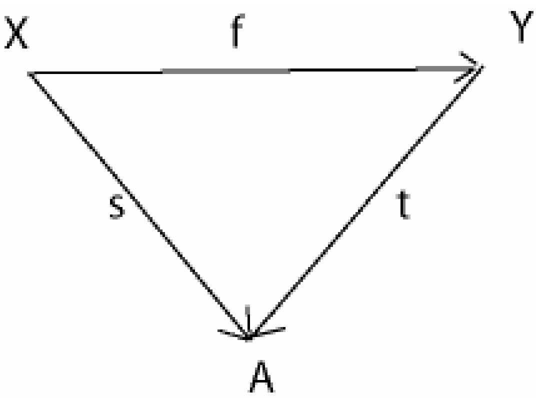
or in other words iff f is right-cancellable. It is easily checked that when morphisms mentioned in the above definition are functions between sets (understood pointwisely as usual) monos in the sense of Def.2 are indeed injective functions.

Definition 3: subobject Sb of object A is a monomorphism to A;

Subobject Sb of A so defined doesn't look yet as a subset because Sb (as any other morphism) has not only its co-domain A but also its domain X, while the notion of subset of given set A doesn't involve any other set. That is why in order to interpret the notion of subobject set-theoretically one needs to think of it "up to isomorphism", which amounts, speaking set-theoretically, to identification of injective functions coinciding on their images.

The fact that in the category theory the difference between isomorphic object is often ignored is remarkable. It is often used as an evidence that category-theoretic mathematics is "structuralist" meaning that it doesn't care about concrete objects but studies abstract structures. In 4.4 below I will argue that this is true for abstract algebra but not for the category theory. Anyway there exist to the date no official theory justifying such "loose" understanding of identity (up to isomorphism) in the category theory, and I think that such a theory is indeed wanted (see my forthcoming paper).

Recall that in order to talk about the isomorphism of subobjects such isomorphisms must be explicitly constructed: we need isos as morphisms but not the isomorphism as logical relation. Notice that by Def.3 subobjects are morphisms but not objects of C, and hence isomorphic objects in C cannot be subobjects. However subobjects of given object A can be made into a (new) category Sb(A) as follows. Objects of Sb(A) are subobjects $s:X\to A$, $t:Y\to A$, ...(note that s,t are morphisms in C), and morphisms of Sb(A) are morphisms $f:X\to Y$ of C such that ft=s (all these morphisms are also morphisms in C), or to put it diagrammatically such that the triangle



commutes. (*) implies that there is at most one morphism between X and Y (in either direction) satisfying fs=t. For obvious reason a category with this property is called partial order. If in Sb(A) both morphisms $f:X\to Y$ and $g:Y\to X$ exist then they are mutually reverse, in symbols $fg=1_X$ and $gf=1_Y$, (since there is also no more than one morphism $X\to X$ which is identity 1_X), and hence s and t are isomorphic (Def.1) and can be viewed as the same.
Remark the constructive character of the notion of subobject. Given a set one is often inclined to think non-constructively that all its subsets just come with it. Given an object in C one, generally speaking, knows nothing about its subobjects, so they should be always specified explicitly.
A surprising feature of the categorical notion of subobject, viewed as a recasted notion of subset, is that the notion of element of set, which seems to be indispensable in the usual definition of subset (A is subset of B iff all elements of A are elements of B) is dispensed with in the categorical version of this notion. So our subobjects, generally speaking, are "pointless", that is have no elements. Let's make the elements. It is usual in set-based

mathematics to talk about functions in terms of elements; now we shall see how it can be done the other way round.

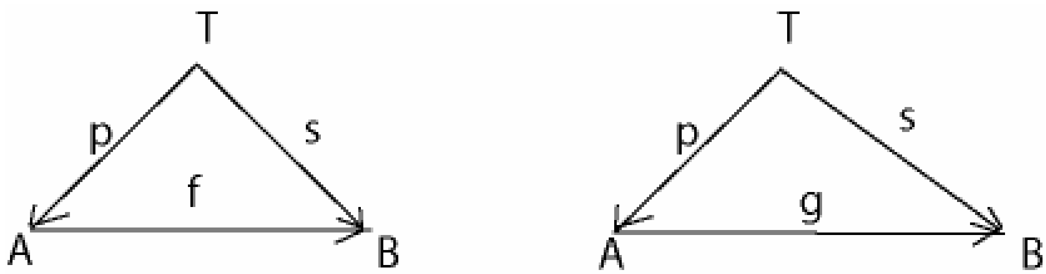
Observe that given a singleton set 1 (set with exactly one element) and element P of set A there is exactly one function p sending the unique element of 1 to P, and so P and p may be identified. Thus elements of A are identified with functions $p:1 \to A$ which "pick up the elements". This construction depends of the choice of 1 but since all singletons are isomorphic we can regard them as one and the same thing up to isomorphism. Now to define singleton 1 categorically observe that for any set A there is exactly one function $f: A \to 1$:

<u>Definition 4:</u> Object 1 of abstract category C is called *terminal* if for any object A of C there is unique morphism $f: A \to 1$.

Applying the latter condition to 1 itself we see that 1 has no morphisms to itself except identity. So given two terminal objects 1 and 1' there is exactly two morphisms in each direction, and their compositions brings identities. So the terminal object, if it exists in C, is unique up to isomorphism (see Def.1).

<u>Definition 5:</u> Morphism $p:1 \to A$ where 1 is terminal object is called point or global element of A.

Now we are ready to formulate an important categorical property which holds for sets but not for every category with the terminal object. In the category of sets S two morphisms (functions) are equal iff they coincide on all their common points (elements of domain and co-domain): this allows for the usual pointwise definition of function. Morphisms f and g (with the same domain and co-domain) coincide on all their common points iff the left triangle commutes iff the right triangle commutes (p, s refer to the same points on both diagrams).



In particular this property holds for subobjects in S, that is, for subsets. This is not the case in any category: morphisms, and in particular subobjects may coincide on all their points if any but be different. This is not the matter of weakness of the general notion of category. Such "palpable" concrete categories as the category of groups (with group-theoretic homomorphisms as morphisms) or that of topological spaces (with continuous transformations as morphisms) don't have the specific property of sets just mentioned.

Let me stress an important conceptual change caused by the categorical recasting of the set-theoretic notions of subset and element. A basic (naive) set-theoretic construction, which is particularly important in logic and point-based topology, has three floors: basic elements p at the ground floor, set U of these elements at the top second floor, and subsets Sb of U at the middle first floor. In logical semantics p are individuals, U is the domain of discourse, and subsets Sb are extensions of (one-placed) predicates. In the point-based topology p are points, U is the underlying set of given topological space (the set of all its points) and Sb are open and closed subsets (by specifying which subsets of U are open one makes U into a topological space). The categorical recasting turns this hierarchy upside down. Global elements are points p. They are global in the sense that they are morphisms from the terminal object, and the definition of terminal object involves ("quantification over" as a logician would say) all the objects of given category (see Defs 4-5). Subobjects Sb of U are found at the intermediate level since they involve not only their co-domain U but also “neighbour” objects as their domains. (The relevant notion of neighbourhood is made explicit in the notion of generalised Grothendieck topology, as we shall see in 5.1). Finally U as a particular object is found at the bottom of the inversed hierarchy. This reversal is not achieved through a mere renaming as if in the category theory one would call by “points”, etc. some different things than in the set-theoretic topology. In fact the old hierarchy remains untouched. For there is a clear sense in which subobjects are “in” its object, and points are “in” its object and its subobjects. Observe that points are trivially subobjects (Defs 2,4,5), and that in Sb(A) subobjects are not in points, so categorical points are “partless” as they should be. The identity of A is also trivially subobject (part), and the biggest part of A is A itself as it should be. So category theory not just turns things upside down but provides a new order of things preserving the usual one. This new picture explains (if not formally proves) the failure of basing a set theory solely on parthood $\subseteq$ and the success of basing it on membership $\in$: the former relation unlike the latter reflects only a local feature of setting.

Set-theoretic notions of Cartesian product of sets, disjoint sum, powerset, as well as more complicated set-theoretic constructions like fibred product, allow for the categorical recasting along the similar vein. This work brings some easy but powerful generalisations like the categorical notions of limit and co-limit, which can be also done in a set-theoretic framework but for the price of significant lost of simplicity and “naturality”. These categorical notions apply not only to sets but also to many other concrete categories. This makes the category theory a powerful tool quite useful in many domains of mathematics.

Let’s return to ZFC. Except specific set-theoretic relation $\in$ it uses logical connectives like conjunction &. As I have already mentioned in 6.8 the logical part of ZFC is indeed not independent of set-theoretic issues but ZFC keeps them apart making a sharp difference between sets (classes) used in logical semantics and the mathematical sets which it accounts for through $\in$. In SFM we find no similar distinction, and no external means like the first-order logic of ZFC. The most evident explanation of this fact is that unlike ZFC SFM is not a formal theory, and that in order to formalise SFM one have to make something similar. So the reader may argue that the idea to treat ZFC and SFM on equal footing is wrong since ZFC is a formal theory built for foundational purposes while SFM is an introductory account of the category of sets (also useful an elementary introduction to the category theory) which doesn't aim to anything similar. Things can be safely viewed in this way indeed but in fact the ambition of SFM and the categorical mathematics in general goes much further: to demonstrate that an external logical framework like one used in ZFC is in fact not needed for a strict mathematical accounting of sets and other categories but that logic can be instead recovered within the category theory itself and then applied in the categorical context.

Before I consider the issue of categorical logic I would like to conclude this section by the following remark. What categorification as an integration mode competes with is not setting but formal logical foundation. So there is no big philosophical issue of "sets versus categories": categories don't replace sets in the older foundational framework and don't allow one who used to think that mathematics is basically the set theory to change his mind and think instead that mathematics is basically the category theory. In the category theory the category of sets continues to play a distinguished role in spite of the fact that in many cases older set-theoretic reductions become unnecessary and exceedingly restrictive. More important from the epistemological point of view is that the category theory challenges logicism and formalism suggesting a different way of conceptual building and conceptual management.

4. Categorical logic

The simplest – and at the same time the "most categorical" way of doing logic in categories is to identify logic with an algebraic structure in an abstract category with needed properties. Such a straightforward algebraic approach to logic dates back to Boole who described in his (1854) "laws of thought" in algebraic terms (through an algebraic structure known today by his name) making little or no difference between logical implication and set-theoretic inclusion, or between syntax and semantics. Given basic "universal" set U consider its subsets and usual Boolean operations on the subsets: union, intersection, inclusion, complement (with respect to U). Then replacing the subsets by true propositions (intuitively: by propositions true "about" elements of corresponding subsets and only them), and the set-theoretic operation by logical disjunction, conjunction, implication, and negation correspondingly, you get a perfect match: the two sets of operations has the same formal structure; since we talk about operations here it is appropriate to qualify the structure as algebraic. Or to put it more accurately, *if* you match propositional logic with sets as indicated above *then* you get both reasonable logic and reasonable setting, and so may consider your setting as a visualisation of your logic. Any user of Venn diagrams knows how much this can be helpful. By the way this hints to the fact that not only pure algebra but also geometry is involved into the resulting synthesis (see 5.1 below).

4.1 Boole

I cannot here consider Boole's philosophy of logic and mathematics in details (see Boole 1997 for it) but it is roughly this. Algebra, and particularly symbolic algebra, is a mathematical and symbolic tool helpful for management of mental acts which are the substance of logical reasoning. So within the scope of algebraic logic algebra and logic are indistinguishable although this doesn't exclude non-logical issues in algebra and nonalgebraic issues in logic (both outside the algebraic logic). Boole regarded his algebraic logic as an application of mathematics assuming that "the ultimate laws of thought are mathematical in their form" (Boole 1854, p. 407), and tending to base logic on mathematical principles rather then the other way round. Unlike Frege and Russell Boole understood universal set U as chosen occasionally in any given application but not as fixed once and forever. In this sense Boole's logic is universal but "local"; importantly this localness appears only in the philosophical interpretation and practical application but not in the symbolic machinery. We shall see that in the categorical setting the notion of localness becomes mathematical and makes part of the machinery.

Some reservations concerning standard presentations of Boolean logic are in order. Boole never spoke himself about operanda of his algebra as propositions but interpreted them either as classes or as mental acts of selecting elements of corresponding classes from universal set U. (The latter mental acts should be distinguished from mental acts corresponding to Boolean operation themselves.) Nevertheless it doesn't seem impossible to translate Boole's philosophy into modern terms: he would likely say that formal logical connectives and their semantic set-theoretic counterparts represent different aspects (discursive and intuitive) of the same fundamental algebraic "laws of thought".

Such a perfect harmony between logical reasoning and mathematical intuition might be very attractive for some people and look suspicious for some others. But in any event it is appropriate to ask: why we need anything like formal logical semantics if things are so good and simple, and the problem of semantical regress doesn't even appear? The answer is, of course, that the Boolean framework is poor and insufficient for logic needed in mathematics and elsewhere. Boole definitely had predication in mind developing his algebraic logic but achieved only a propositional, not predicate logic. So one may reasonably argue that the conceptual simplicity of Boole's approach is nothing but an oversimplification leading to a technical failure.

The good news for Boole's project is that the category theory allows for putting it far forward through resolving technical problems just mentioned without giving up Boole's idea of straightforward algebraisation (and geometrisation – recall Venn diagrams) of logic. I don't want to say that Boole's philosophy of logic is necessary and sufficient for philosophical analysis of categorical logic but I think that it is a good starting point for it. It seems me evident that logico-semantic concepts developed in works of Frege, Hilbert, Peano, Russell, and Carnap are not sufficient for this purpose, and that to develop a new appropriate conception of logic we need also to look further back into the history.

4.2 Topos

Let me now briefly explain how logic is done in categories. The principle categorical algebraic structure used for logical purposes is the partial order of subobjects described in 3.2 above. When our category C is set-like (in the sense that it allows for categorically recasted version of set-theoretical constructions like product, etc.) one could expect that the partial order of its subobjects will be Boolean. However this is not the case: although the resulting partial order appears to be logical (that is, it presents an algebraic structure interpretable as a logic) it corresponds (generally speaking) to Intuitionistic but not Boolean logic. This fact is remarkable (one gets an Intuitionistic logic naturally but not as a formal representation of Intuitionistic credo) and it reflects the constructive character of the notion of subobject already stressed.

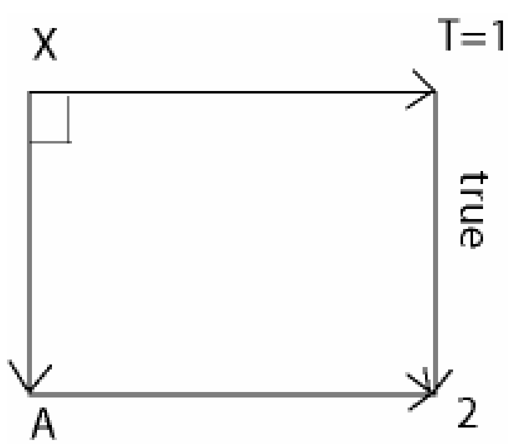
Notice that interpreting the algebra of subobjects in logical terms we not only get a categorical counterpart of the algebra of classes but also settle the issue of universal class (universal set). Consider the concrete category S for simplicity. *Each* non-empty set in S has a logical structure (which in S is Boolean), and in this sense serves as "universal set" with respect to its subsets. Noticeably no distinction between "mathematical" and "logical" set is involved here: each "mathematical" set serves also as "logical". So the local character of Boolean logic becomes here a feature of mathematical construction but not only of its interpretation. Since our sets are not independent but make a category this localness of logic doesn't cause splitting of logic into multiple separate copies. The categorical logic so construed has global aspects too, as we shall now see.

Local logical structures (of the form Sb(A)) brought together in C (an abstract logical category) can be viewed as making part of one many-sorted logic (each object A of C is interpreted as a particular sort), which operates on many domains at once. This is certainly an advantage as far as our logic is supposed to comply with patterns of common speech and reasoning where distinctions between different sorts of things (persons and numbers, for example) are usually kept quite rigidly (so it is difficult to imagine a context where "all things" would imply "all people and all numbers"). Functors Sub(A)→Sub(B) (for some objects A,B of C) can be regarded as substitutions of variables of sort B by variables of sort A, and this allows for a purely algebraic introduction of logical quantifiers ("all" and "some") as (adjoint) functors going into the opposite direction (Kock&Reyes 1977; Lawvere 1969). If now one associates with C an "internal language" that is, a sorted first-order language L with "default" semantics provided by C as hinted above, one gets a full-blooded first-order logic which may be sound, complete and have other good properties as far as C has needed categorical properties (Makkai&Reyes 1977). From the "radically categorical" point of view (and with the accord to Boole's approach) the introduction of internal language is redundant since the ("informal language" of) category theory itself represents logic better than L, so L is nothing but a symbolic convention.

Formally speaking C can be viewed as a basis of unusual semantic for L. However this semantic is unusual not only because it is not set-theoretical but also because it involves an unusually tight connection between syntax and semantics which blurs the whole distinction. As it will become clear from 4.5 below C is not semantic at all if by semantic one means extensional semantic.

Notice for example that in Tarskian semantics quantifiers are not interpreted as particular sets while in L they are interpreted as particular morphisms (functors). Again the view that categories simply replace sets for certain purposes appears to be wrong. Another specific feature of L making the distinction between syntax and semantics not even dispensable but rather irrelevant is the internalisation of truth-values which in the case of categorical logic can be recovered directly in C but not as usual on Platonic heavens or as mere symbols on the paper. For saying that truth-values make part of semantics is strange if not absurd.

Let's see how it works in S. Here truth-values True, False are identified with elements (points) of the pair 2. (I say *the* pair since up to isomorphism there is only one such thing in S.) The truth-values are not simply attributed to propositions by special functions but make part of a categorical construction called the *classifier of subobjects*, which, if exists, is also unique up to isomorphism. Given set A, point P of A, and subset X of A the classifier "says" whether it is true or false that P is in X:



(This commutative square is supposed to have an additional property of being a *pullback* which I leave here without explanation.)

Now logical connectives in S can be defined globally (for the whole category but not separately for each object) as morphisms 2x2→2 where x denotes Cartesian product as usual. The notion of classifier of subobjects is lifted to abstract category C (that is, defined categorically); if C is set-like (Cartesian closed) and has a subobject classifier (which implies that it is set-like in a stronger sense) then it is called *topos.* In particular S is topos. (Lawvere 1975, MacLane 1975; Fourman 1977; Bunge 1985).

4.3 Internal language, internal interpretation, and internal topos

The unusually tight connection between a topos and its internal language (or internal logic) rises a concern: don't we loose here the very notion of logic? For any system of logic makes a distinction between terms allowing different interpretations and logical form invariant under interpretations. Think about formula (**) A & B meaning the conjunction of two propositions as usual. The usual reading of (**) applies this convention: meanings of A,B vary while & always stands for conjunction. Moreover A, B may vary their meanings at least in two different ways: (i) A,B may vary their meanings (values) *within* each interpretation of (**), and (ii) A, B may keep their meanings fixed within each interpretation of (**) but change their meanings with the change of interpretation. By interpretation of (**)" I mean a finite or infinite set of propositions P1, P2, ….. When A,B are variable in the sense (i) they range over this list; if A,B are variable in the sense (ii) they are identified with certain propositions from the list. Variables in the sense (i) are usually called logical variables while variables in the sense (ii) are called non-logical constants. Things like & which don't vary with interpretations are called logical constants.

To make a physical analogy think of (**) as a boat moving around a sea. There are movable elements on the boat like sailors and steady elements like the dock. (Say, A maybe logical variable and B non-logical constant.) But since the dock with other parts of the boat moves with respect to the sea the sense of "steady" is relational. We have here a hierarchically built complex reference system which has 3 levels: the sea in the state of absolute rest, the boat which serves as a moving reference system, and elements of the boat some of which move while some others are steady with respect to the boat. Similarly in logic: we have a logical form invariant through interpretations, various interpretations, and finally constants and variables in each interpretation.

This analogy strikes me as an old-dated: few centuries after Descartes and Galileo we still use in logic a conception of variation which is rather Aristotelian and assumes the notion of absolute rest. Notice that the Aristotelian framework allows for certain relativity: the dock is steady with respect to the boat but movable with respect to the sea. However the Aristotelian relativity of motion unlike the Galilean relativity requires the absolute rest (but not absolute motion), and doesn't allow for skipping through levels of the hierarchy; for example, it doesn't allow for a sailor to be at rest with respect to the sea when he moves along the moving boat (even if he moves with the same speed in the opposite direction). Moreover the Aristotelian relativity remains stable under re-attributions of the absolute rest (so the rest can be thought of as absolute in an operational but not necessarily ultimate metaphysical sense): the hypothesis that the sea moves with the boat and the sailors with the globe in an outer space doesn't immediately brakes this conceptual scheme down but effects the re-attribution of the absolute rest to immovable stars or ether, or the like. Similarly the remark that conjunction & means different things in different systems of logic doesn't change the situation profoundly. The fact that in the categorical logic the distinction between constants and variables become flexible lets me hope that a stronger logical relativity can be achieved in a categorical framework.

Let me now touch upon a more specific aspect of the problem. The worry about the notion of internal language is this: since its semantics (if it is indeed a semantics) is rigidly fixed we apparently loose at least one level of variation, namely the semantic variation. This would made irrelevant the model theory and in particular the model-theoretic notion of logicicity as invariance under the change of interpretation (already mentioned). However the worry is ungrounded. Take topos S for simplicity and compare it with the primitive model-theoretic setting where abstract universal set U is interpreted many times differently as a set of people, chairs, points and whatnot. Since S is the category of "all" sets all possible (local) interpretations of U are already in S: each non-empty set A of S has a logical structure Sub(A) and all these local structures are integrated in S into one global topos structure. So in S the notion of interpretation is not swept out but on the contrary is *internalised*, that is, included into the mathematical construction of logic. Global aspects of logic which in the standard approach are either left informal to philosophy and practice or formally accounted by a separate theory (meta-theory) in the categorical setting make part of the same mathematical construction of topos. I stress this point because what is usually stressed is only the fact that classical logical and mathematical notions like that of subset or truth get *localised* in categorical contexts (Bell 1986). They do get localised indeed but they do so against new global categorical structures like that of topos.

An important reservation is here in order. It is certainly weird to think that, say, the set of my friends is an object of S because S contains all sets. This implies two things. First that S should be thought of as the category of all *abstract* sets rather than of all sets. The notion of abstract set should be then properly specified. Then the set of friends may be thought of as a functor from friends to abstract sets but not as a particular set. This idea doesn't fit well the distinction between abstract and concrete categories made earlier (for then we regarded S as a basic example of concrete category) but it certainly makes sense in the context of Cantor's work (see Lawvere 1994). Second, this shows that other logical categories than S, in particular specific toposes related to given subject-matter, should be wider applied. This reservation doesn't cancel the main point concerning the internalisation of the model-theoretic notion of interpretation in a topos. In the next section we shall see how this internalised interpretation is made more explicit through the notion of functorial semantic

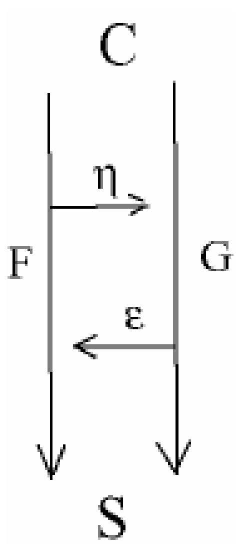
Although in the categorical context there is no rigid distinctions between a theory, its underlying logic, and a meta-theory taking care about semantics of the underlying logic a similar hierarchical construction can be easily made. Given topos T with internal language L, one can use this language in order to construct another topos T' (with its own language L') *inside* T. For this end one spells out in L axioms for T', makes some object O of T represent the class of objects of T', another object M of T – to represent morphisms of T', etc. (MacLarty 1992). Things are arranged here in a simpler way than in the standard model-theoretic setting. For the "mega-topos" T (with its language L) plays both the role of "underlying logic" and the role of meta-theory with respect to T' and L'.

Now let me now touch upon the following question which from the foundational perspective seems to be crucial: whether internal language L of topos T is sufficient for constructing (or defining) T itself? The positive answer to this question could mean getting out of the semantic regress: we would have a logic taking care about its own semantics! I cannot discuss technical issues (a theory of definibility is needed for a sound answer) but it me seems that the issue of logical circularity looses its usual sense and appeal in the given context. A well-adjusted "first-order" internal language L of topos T is sound with respect to T in the sense that each deducible formula in L corresponds to (describes) certain commutative diagram in T. Completeness is a more involved issue. If one chooses T to be an abstract topos having no other properties except logical L and T are essentially indistinguishable (L may be viewed as a particular form of symbolic presentation of T), completeness becomes trivial, and the question of definibility of T by means of L comes to its starting point. If otherwise T is assumed to have other properties except logical - it might have also geometrical properties like in the case of Grothendieck topos considered below - we get a non-trivial interplay between geometry and logic interesting both mathematically and philosophically (see 5.1 below). Non-trivial completeness results are still provable (Makkai, Reyes 1977). This completeness can indeed be understood as "complete description of T in terms of L" but the notion of description cannot any longer be taken for granted and must be thought of as a functor with required properties from T to a "logically transparent" topos with L as internal language. (So categorical completeness theorems claim existence of such functors with good properties). Although it remains unclear how such results amount to definibility (and what exactly one should mean by definibility here) one thing seems to be certain: whether we talk about more transparent logical categories or more opaque geometrical ones the orthodox distinction between intuitive "naive" and rigor "formal" mathematics doesn't apply to this situation. It is absurd to think that supplying given topos T with an internal language L one automatically jumps from "naive mathematics" to the "formal level of rigor". For the usual "naive" way of working with T already involves a good deal of formalisation which however is well supported by intuition through categorical diagrams. The linear (or mostly linear) writing used in L has its own advantages and can be indeed very helpful. But there is no gap here between "naive" and "formal" mathematics invented by logicists.

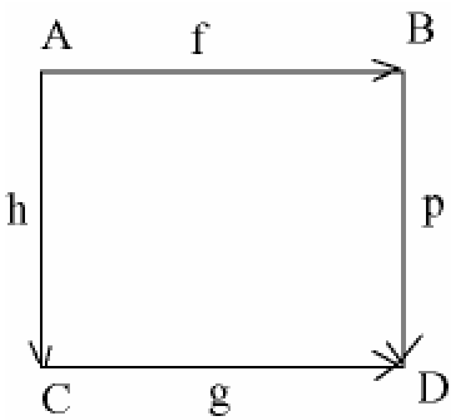
However the distinction between the "abstract form" and the "intuitive content" continues to play a role the topos-theoretic context. This distinction rooted in the traditional Platonic metaphysics is internalised in a topos along with the notions of logical system (internal language) and of its interpretation. To see this we should stop to think of topos as a description of sets in terms of functions, and take different toposes into consideration.

4.4 Functorial semantics. Abstract and concrete categories. Structuralism

Consider functors G,F:C→S, where C, S stand for an abstract category (not necessary a topos) and the category of sets correspondingly as before. We take these functors as objects of new category conventionally denoted S^C and called *functor category* with *natural transformations* as morphisms: a natural transformation transforms a functor into another functor respecting their structures in the usual algebraic sense. There are quite few things to respect here: one needs to descend through functors down to morphisms and objects of the "first level" to spell out the corresponding equational conditions! The term "natural transformation" emphasises this fact. To make S^C better visible I picture it as below:



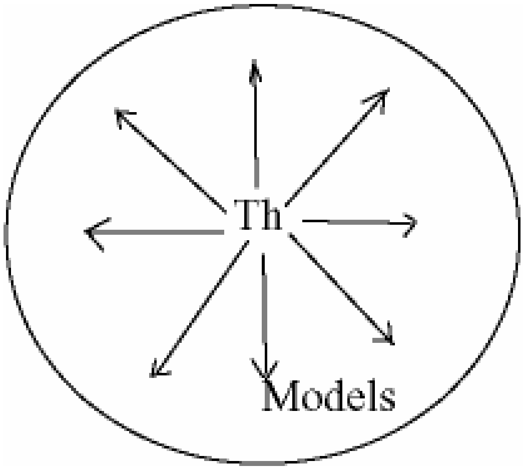
If C is *small* category, that is, its objects and morphisms form sets (but not proper classes) then S^C is topos (Johnstone 1977). The property of S to be a topos survives when one makes out of S some other concrete categories like category S^2 where objects are pairs of sets and morphisms are pairs of functions, or category $S(\rightarrow)$ where objects are functions $f:A\rightarrow B$ and morphisms are pairs of functions h,p such that squares



commute. In the two last examples S is provided with abstract structures, namely with that of pairing, and "functionality". S^C generalises upon such examples to the effect that by structure one understands any small abstract category. To see that this is indeed a generalisation upon the above examples think of pairs of sets as functors from two-element set 2 to S picking up from S pairs of its objects. S^C involves both a concrete (S) and an abstract (C) category, and so internalises the distinction: abstract "form" C is variously "incorporated" through functors G,F in "matter" S. Observe that in all the above examples structures (2, $\rightarrow$ and C) are borrowed from S. So they are "abstract" not only in the sense of being nothing concrete but also in the sense that they are detached elements of something concrete, namely of S.

Following Lawvere (1963) we can interpret S^C in terms of model theory: C will represent theory Th (C needs not to be a topos but must have required logical properties), functors F,G (all or some of them dependently on Th) will be models of Th, and S^C (or its subcategory M) will be the category of these models. This construction makes the internalisation of the notion of interpretation in topos explained in 4.3 explicit.

Thus the usual distinction between abstract form and concrete intuitive content is recovered in the categorical setting. However it is recovered not quite in a standard form. Let's see the difference. In the standard axiomatic setting all models of Th are, generally speaking, linked to each other (integrated) only through Th. To make this visible I show this fact by a diagram:



To put it in epistemological terms introduced in **2** the standard (minimal) integration of models applies only (i) formalisation (Th is formal in a sense in which its models are not) and (ii) foundation (formal Th is an axiomatic foundation of "naive" constructions M). The above reservation "generally speaking" is essential because certain horizontal links between different models (of the same and of different theories) also play an important role in the standard axiomatic method, and particularly in the Tarskian model theory. A horizontal relation between models taken into account form the very beginning of axiomatic method is isomorphism: when all models of given theory are isomorphic they are usually not distinguished, so one may say that the theory completely describes its unique model (see 6.4). Such horizontal links between models were made explicit and progressively important in the model theory, in particular, through Tarski's convention according to which all models are set-theoretic constructions. However these important developments haven't changed the above basic picture which doesn't require horizontal links between models and makes them a secondary issue.

Lawvere's "functorial semantics" changes this basic picture indeed. In the new arrangement models of given theory Th form category $M \subset S^{Th}$, that is, they are linked to each other directly by default. Moreover under certain conditions Th itself may be considered as a subcategory of the category of its models, and under different conditions as a "generic model" (Lowvere 2004). Moreover when M has logical properties the horizontal links between models support a logical structure. Recall that in the standard Tarskian setting logic comes from the other side, so to speak: it "underlies" Th, that is, integrates Th and other theories based on the same logic (through generalisation).

Thus the traditional metaphysical arrangement which involves a concrete background (matter) and an abstract form has different specific features in the standard formal axiomatic approach and in its categorical version. In the standard approach models are subsumed under its common "form" (formal theory Th which they are models of), which itself is subsumed under another formal theory having status of "logic" for Th. Logical pluralism according to which logics are many troubles this hierarchical organisation but doesn't change it significantly because it doesn't provide any alternative integration scheme. But the categorification does this, even if it is not motivated by the logical pluralism. In a category of functorial models logic emerges from links between its parts but is not brought from higher spheres through a number of intermediate offices. Applying the historical optics of classical philosophy we may say that while the standard axiomatic approach developed in the beginning of 20th century almost perfectly reproduces Platonic scheme in which things are subsumed under invariant forms (the very term "model" is perfectly Platonic), the categorical approach makes an Aristotelian revision of Platonism: forms loose their superior status and become either abstractions (dependent on what they are abstractions from, that is, concrete models) or models of special sorts; the notion of matter (background) becomes at least as much important as that of form, and the essence (I think about Aristotle's sense of the term) is located in the middle.

Taking the Aristotelian revision seriously, one should accept that the zeal for a "purely algebraic" (in the sense of the Van-der-Warden-style abstract algebra) description of sets and other mathematical objects by categorical means is a Platonic reflex which should be given up. The category (and particularly topos) theory is rooted in geometry at least as deeply as in algebra and in spite of early suspicions of being "abstract nonsense" reinforces the role of geometrical intuition (or "intelligible matter" by Proclus' word) in the most profound mathematical issues. "Dematerialization" of mathematics (and other branches of knowledge) in the vein of abstract algebra shouldn't be confused with the categorification.

There is an important nuance here which I want to make clear. The fact that things like sets, groups, vector spaces, topological spaces and many others form categories, or in other words, that the notion of category is enough general and formal to subsume all these mathematical structures, is what makes the categorification possible but it is not how it works. The specific feature of categorification as an integration mode is its capacity to link integrated elements directly without reduction to common ground, common form or anything else. So the categorification doesn't endanger concrete mathematics and other branches of concrete knowledge but on the contrary makes them more significant.

For this reason the often repeated claim according to which the category theory provides a strong support for mathematical and more generally epistemological *structuralism* (which is, roughly, the view according to which only abstract structures but not concrete objects can be accounted for scientifically and mathematically) seems me misleading if not plainly wrong. This claim is apparently based on an erroneous identification of categorification with the project of abstract algebra. One argument allegedly supporting the claim refers to the aforementioned fact that in the category theory identity is usually understood up to isomorphism, that is, that isomorphic objects are not distinguished. The claim is misleading because it assumes that there are things called structures, which are captured from somewhat unobservable plurality of concrete objects by isomorphisms and become the subject matter of science and mathematics. This picture perhaps fits certain aspects of mathematics but not the category theory which studies not abstract structures defined up to isomorphism but isomorphisms themselves as well as morphisms of different sorts. Given morphism $f:A\rightarrow B$ there is no way to prescribe to A and B a common structure (in the usual sense) unless f is an isomorphism. Moreover, as we have seen in the beginning of **3**, if one starts with the notion of group as a prototype of that of category then in order to turn the former to the later one needs two steps: first, *not* to identify isomorphic objects and second, to take into consideration other morphisms than isomorphisms. True, in most cases objects of a category are "taken up to isomorphism" although groupoids provide an important counter-example. Any abstract category can be seen as a structure defined up to an isomorphism. But the category theory involves not only abstract categories. And the reason for it is not that we cannot avoid "concrete examples" and do only an abstract algebra. Even if we can do so the concreteness strikes back at a higher scale of conceptual organisation. Consider for example functor category S^{Gr} with functors from groups to sets as objects and natural transformations of these functors as morphisms. No matter how abstractly we think about groups and sets there is a sense in which S^{Gr} is a concrete category just like S and Gr themselves. For the talk about isomorphic copies of S, Gr or S^{Gr} doesn't make much sense since all sets, groups, their morphisms, and all functors between the two categories are supposed to be already there. (Gelfand and Manin in their (2003, p. 70) call the isomorphism of categories "a useless notion".) This doesn't rule out non-identity categorical equivalences (reversible functors), and doesn't forbid us to "copy" concrete categories just like we do it with numbers. What exactly is going on with identity in categories is, in my view, an open question. But in any event the usual assumption that higher-order categorical constructions (in terms of its complexity and its "size") always lead to higher level of abstraction is just wrong because it doesn't take into account the fact that the concreteness strikes back. Notice that this return of concreteness is a very good thing because it makes categorical notions easier manageable.

For the recent discussion on the mathematical structuralism and categories see (Awodey 1996, 2004), (Halle 1996), (Hellman 2003), (Landry&Marquis 2005), (MacLane 1996), (MacLarty 2004, 2005). Landry&Marquis in their (2005) summarise the discussion as follows:

“The first, and probably most important, common element present in all the previous developments, and shared by all category theorists and categorical logicians, is the assumption that by adopting a top-down approach to analysing mathematical concepts the “shared structure” between abstract mathematical systems can be accounted for in terms of the morphisms between them.” (p.19).

What is the “shared structure”? Consider this description given in Halle (1996):

“An abstract-structure is just what is left when, beginning with a model-structure, we abstract away from all that is inessential, leaving behind only what is common to all other model-structures *isomorphic* to it.” (p.125, italic mine).

What is important for my argument is not the difference between free-standing abstract structures and structures “embodied” into models which Halle is making here but the fact that both kinds of structures are thought of through the notion of isomorphism. This kind of things Weyl had in mind saying in his (1949) that

“A science can determine its domain of investigation up to an isomorphic mapping. In particular it remains quite indifferent as to the “essence” of its objects.” (p.25-26)

Compare also the quotation from Hilbert’s letter in 6.4 below.

Now Landry&Marquis tell us this:

“For example, in the type of category called Top, we present the shared topological structure of any abstract kind of structured system be taking “objects” as abstract kinds of topological spaces and "morphisms" as abstract kinds of continuous mappings independently of what these abstract kinds are kinds of." (pp.35-36)

The problem is that the "shared topological structure" just mentioned cannot be a structure in the sense of Halle (1996) simply because objects of category Top, that is, abstract topological spaces, generally speaking, are not isomorphic. Landy&Marquis might have a different notion of structure in mind but they don't explain this.

Awodey in (1996) and (2004) notices the difference and distinguishes between structures and "types of structures", so only the former is defined up to isomorphism while the latter is defined "up to" arbitrary morphism. However in the conclusion to his (1996) he writes:

"The subject matter of pure mathematics is invariant form, not a universe of mathematical objects consisting of logical atoms. This trivialisation points to what may ultimately be an insight into the nature of mathematics" (p. 235)

In my view this trivialisation misses a crucial point, namely the fact that categorical morphisms, generally speaking, are non-reversible. For usually an "invariant form" is thought of as invariant under a reversible transformation like a motion. One might think of a generalisation of the notions of form and structure to the effect of including non-reversible transformation but I think that it would be more appropriate to use a different terminology in order to avoid a possible confusion.

Awodey and Landry&Marquis point to the general character of the notion of category as an argument supporting the thesis that the mathematical structuralism is a natural philosophical position vis-à-vis the categorical mathematics. Indeed defining category Top top-down one says nothing about the "nature" or "content" of its objects (topological spaces), and so is free to consider as topological spaces very different constructions if their morphisms have needed properties. This make it natural to think of Top as an abstract form shared by all such constructions (or more precisely – by all concrete topological categories) and invariant under the reversible substitution of objects and morphisms of one concrete topological category to objects and morphisms of another like in the case of substitution of letter X by letter Y. But categories are more complicated things than letters and the reversibility of such transformations except trivial cases fails. It is more appropriate to think the relation between abstract category Top and its concretisations (like the category TOP of topological spaces constructed bottom-up set-theoretically) not naively in terms of external substitution but through the internalisation of the abstract/concrete relation within a higher order categorical construction like functorial semantics. As this latter example clearly shows one, generally speaking, cannot expect models (concretisations) to be isomorphic, and moreover one has no reason to pick up a single model (up to isomorphism) as "standard" and regard other models as pathological. So it is misleading to think of given abstract category as a "shared form" of corresponding concrete categories or at least if the "shared form" is understood as usual up to isomorphism. I shall return to this point in 6.4.

I strongly suppose that the difference between abstract and concrete concepts in mathematics (and perhaps also elsewhere) has hardly any sense at all but relational. If you think about an abstract category C as a geometrical construction made of arrows and provided with the simple algebraic rules involved into the definition of category there is no reason to consider C as anything abstract. To conceive C as abstract one needs to recall that sets, groups, etc. all make categories. Of course the fact that C may subsume so many things is important and useful. However it might be equally useful to forget this (this shouldn't be a problem for philosopher) and think of C as it stands by itself, as a simple algebro-geometrical construction. Abstract thinking is the capacity to detach a concept ignoring its links to other concepts. It is useful only in combination with a capacity to the opposite, namely to conceptual linkage. It is more appropriate, in my view, to think of conceptual universe as a field providing a lot of material for such double activity rather than one grandiose ongoing construction where every concept has a certain place, and so is either abstract or concrete in an absolute sense. Saying this I certainly don't mean that occasional conceptual construction should replace serious science. On the contrary, I suppose that it is vital for today's science to integrate itself at higher levels of organisation (that is, globally) differently than the rigid Platonic hierarchical scheme suggests. The purpose of this is to make science not only more flexible and funny but also more stable and more endurable.

I conclude this section with some open problems:

(i) That C is abstract and S is concrete is usually taken for granted. But how to define abstractness and concreteness categorically? One reason to think of C in S^C as abstract with respect to S I have already mentioned. Another obvious point is this: concrete categories are constructed in the bottom-up order while abstract ones are constructed (or perhaps one should say in this case *defined*) in the opposite top-down order. Remarkably S^C allows for both way of conceptual building: one has both an abstract and a concrete category here and may pursue further constructions and definitions moving in both directions toward the middle. (ii) A more specific question about the background. Leaving aside specific mathematical issues concerning infinity one may say that the idea behind the notion of set or class is that of "bare things without structure": a set is nothing over and above its elements (except the fact that these elements for some reason are brought together into a set). This is the principle reason to regard sets as "background", that is, as a kind of mathematical "matter" on and of which (most or all) mathematical structures are built. Bourbaki's great work made this metaphysical intuition into a mathematical reality. But in the categorical context the idea of sets as "bare" and unstructured can hardly be justified: the category of sets is a topos, and have other highly specific properties. In fact this intuition of set as a background is far from being straightforward, and apparently it is based on the assumption that the structure involved into the notion of set which allows for differentiation between abstract elements without identifying them is a minimal structure without which no mathematics is possible. (Given a pair of abstract elements one is supposed to be able to detect that the elements are different, so it is indeed a pair but not a singleton. Then one can choose one element and leave out the other. Since there are two different elements one may conclude that there are two possible choices here. But given two such choices one cannot say whether the same or different elements were chosen in the two cases unless the pair is ordered.) A more traditional metaphysical idea of background or matter is that of boiling magma beautifully pictured by Plato in Timaeos 50. Plato thought that things of this kind are far out of the scope of mathematics and science. This was correct with respect to his contemporary mathematics but is no longer correct today when a lot of mathematical work is done for modelling of chaotic phenomena. An example of "chaotic functor" is given in (Lawvere 1994). Why not to follow a philosophical hint and try chaotic or other "highly dynamic" categories as a background? (iii) Toposes as epistemic frameworks differ from formal axiomatic structures as I have already shown. But at the same time the two kinds of frameworks share a common metaphysical scheme involving matter and form (concrete background and abstract theory). Why this ancient metaphysical (and epistemic) scheme is so persistent, and do we really need to respect it any longer? Do we really need to reconstruct it in categories? Instead of Aristotelian-like conceptual space with its fixed ups and downs a stronger form of conceptual relativity could be assumed and developed. Galilean relativity could be a tentative guiding principle. How to realise it in logic?

4.5 Extension and Intension

Now I'm going to check the categorical logic (I say *the* categorical logic meaning the general categorical framework presented above but not any particular logical category which are many) against the traditional logico-philosophical issue of extension and intension. I shall speak about properties (one-place predicates) for simplicity; this discussion may be generalised to the case of relations as usual. Recall from 2.1 that "intension of property P" refers to what P means and "extension" to the class of "all" individuals sharing this property. Already Plato in his *Sophist* seems to note that there is a functional relation between them: a well-formed predicate (intension) like "featherless biped" picks up certain class H of individuals however the same class H is equally picked up by different predicates like "human" or "rational being". It is not surprising that traditionally in logic predicates were thought of first of all in terms intensions or even identified with their intensions while extension was seen as a function from predicates to "things". What is this universal domain of things exactly remains a controversy. Should it comprise only things existing in the presence or include also past things existing no longer and possible future things which are only about to exist (and perhaps may fail to do so)? What about counterfactual things?; etc. A way to tackle the problem is to relativise the notion of extension of given predicate P to chosen universal class U provided that for any individual X from U P(X) either is true or not. Then the extension of P in U is defined as subclass Sb of U whose members are those individuals X from U for which P(X) is true. This remark played an important role in the history of axiomatic theory bringing Zermelo's subset axiom schema which says that given U and P satisfying the aforementioned semantic condition such class Sb exists. Such relativisation and restriction of the notion of extension prima facie gives another reason in favour of the traditional attitude according to which predicates are essentially intensions but not extensions. However today's student of logic if she is not particularly interested in the history and philosophy might never hear about intensions. What happened in logic in the beginning of 20^{th} century and what still effects logical curricula today Quine called the "flight from intension" (Quine 1960). How did it happen?

It seems clear that this conceptual change in logic is connected to mathematisation of logic started in 19^{th} century by Boole and others. Although Boole himself didn't develop a system of mathematical predicate logic he gave the idea how to use classes for it which was taken by Russell, Tarski and others. So the logical extension was accounted for mathematically. It would be perhaps wrong to say that no attempt to mathematise intension have been made in the early days of mathematical logic. Frege's *Begriffshrift* (1879) may be viewed as such an attempt if this work can been qualified indeed as mathematical. But Frege's project was not successful, so it looked like the only way to do logic mathematically was to do it extensionally.

A concrete example of the fly from intension is Russell's modification (in his 1903) of Frege's definition of the notion of natural number given in (Frege 1884). Frege remains in the traditional framework and accurately distinguishes between "Begriff" (concept) and its "Umfang" (extension). Following Cantor Frege attempts to define numbers through one-to-one correspondences between sets. Frege's final definition is this: "the number which belongs to concept F is the extension of the concept "equinumeral (gleichzahlich) to the concept F"" (p. 80[e]) where "G is equinumeral to F" means that extensions of predicates G,F are isomorphic (as classes). Russell sweeps out Frege's talk about concepts from this definition as redundant and defines natural numbers as isomorphism classes of sets. For Frege the distinction between concepts (intensions) and their extensions remains fundamental and doesn't allow him to make this obvious step.

What gave Russell the courage to sweep out intension from Frege's definition was Cantor's set theory. Recall that before Cantor not only the class of "all points" but also the class of "all points of given segment of straight line" was illegitimate just like any other infinite class. Cantor's approval of infinite sets gave logicians a freedom of talk about extensions which they never had before. This changed the attitude: intension was no longer viewed as a cornerstone of logic but started to be viewed as a difficulty like in the case of "intensional contexts".

Logical semantics in Tarski's sense is an extensional structure. What is not semantical in Tarski's notion of logic is its syntax (leaving pragmatic issues apart) which has nothing to do with intension either. Thus the issue of intension, that is, of what words *mean* has been indeed left outside the pure logic to linguistic and philosophy. One might think that this fits the requirement that the pure logic must be formal but this is plainly wrong. This requirement doesn't effect the choice between extension and intension. There is no reason why the pure logic should care more about the planet Venus than about Morning star or Evening star. There might be some philosophical and also technical but not formal logical reasons to fly from intension. I shall not discuss here the philosophical reasons but technical ones are obvious: mathematics developed in the first half of 20th century allowed indeed for a mathematical theory of logical extension but not for a mathematical theory of intension and meaning. However the categorical logic not only makes this possible but also makes this real.

Consider a logical category C and interpret its subobjects as predicates as usual. Only when C is the category of sets S the subobjects (and their algebra) are completely determined by their global elements (points) and so the logic is fully extensional. But definitely S is not the only logical categories. In the general case the situation is just like in the traditional logic: predicates might have or not have extensions (sets of their points) but anyway the logic of predicates is not determined by their extensions: two different predicates may coincide on all their points, that is, have the same extension. In 5.1 below I shall show how functions from predicates to their extensions are made explicit in a sheaf construction. So if one accepts the categorical conceptualisation of predicates as subobjects and sticks to the usual way of thinking of extensions as sets one gets the traditional situation when extensions are determined by predicates but not the other way round.

What is yet wanted to make this link with the traditional logic clear and apply the categorical logic broader is an interface between the categorical logic and reasoning in natural languages which would allow for a categorical representation of usual logical examples. Internal languages are interfaces of this sort but developed for a different purpose. Perhaps some kind of diagrammatic *Begriffshrift* could be indeed helpful.

5. Categorical geometry

We have already seen that the notion of category bridges logic with mathematics trough algebraisation in the spirit of Boole's pioneering works. Now I'm going to show that categorical logico-algebraic constructions and particularly the notion of topos also have a geometrical sense and originate from geometry historically. The geometrical face of the categorical logic enables one to think of "logical space" not as a metaphor (sticking to Fregean dogma that logical matters are timeless, spaceless and has nothing to do with intuition) bat as a true geometrical space or at least something very similar. This close link between logic and geometry is not something absolutely new in the history of these two disciplines. A telling fact is that the early logical vocabulary is mostly borrowed from geometry (in many cases through rhetoric): think of "figure of syllogism" for example. Euclid's *Elements* for centuries served as a model of logical but not only mathematical rigor; in 20th century a similar role played Hilbert's "Foundations of Geometry". However after the century of logicism and Fregean dogmatism this re-established link is indeed refreshing.

In this section I shall present two examples of geometrical toposes, and then come back to the history of "heroic age" (by Boyer's word) of geometry (19th century) and consider it from a categorical perspective. This historical essay will also serve me for understanding the genealogy of Hilbert's axiomatic method.

5.1 Sheaves and Grothendieck topos

An attentive reader could already notice that the discussion on sheaves in 2.7 and the discussion on toposes in 4.2-4.4 had certain things in common. Now I'm going to make this link explicit. Recall the "modestly modern" notion of sheaf given in 2.7 above. Consider two set-valued sheaves F,G over common topological space T. Morphisms M: F→G between the sheaves send sets (values) of F to values of G and commute with inclusions of opens of T. Then the gluing of coverings is preserved automatically. It is easy to see that morphisms M also form a sheaf (exercise). Now if we take "all" sheaves on given topological space T with "all" morphisms between the sheaves we get topos Sh in which T plays role of the true-value object. If we now think of T as "space-time" as suggested in 2.7 then the internal language of Sh become a "spatio-temporal" logic associated with T: T has true-value "true" (as any topos) meaning here "always and everywhere true", "false" (never and nowhere true"), and intermediate values corresponding to each open ("spatio-temporal region") O of T ("locally true in O").

The fact that sheaves form a topos is remarkable for few reasons. First, it shows that the notion of topos is definitely not a reformulated notion of set. Topos S of sets is a "limit case" of Sh corresponding to the minimal discrete topological space 2 (2 has two elements both of which are clopen, that is open and closed; one is included into the other but not the other way round). If sheaves are viewed as (continuously) variable sets (see 2.7) then the usual sets correspond to the limit case of constant. (Notice that the "Galilean relativity" is not achieved here, so we should speak about variable and constant sets in the absolute Aristotelian sense.) Second, it shows the geometrical face of the notion of topos. For as I have already mentioned a sheaf may be viewed as a more explicit construction of topological space. This is moreover true about topos Sh. To see what the shift from the notion of sheaf over topological space to the notion of topos of all such sheaves over the given space precisely amounts to recall the example of circles given in the beginning of **3**. From the classical point of view it might seem that the shift from an individual sheaf to topos of sheaves as well as the shift from an individual circle to groupoid of circles amounts to nothing. For one individual circle perfectly represents its general notion, and there is no need to multiply examples. But recall that taking "all" circles into consideration allows one to consider their transformations, and this brings about a new important structure. Similarly with sheaves: the resulting topos Sh is a new structure indeed and at the same time it is a very natural extension of the notion of sheaf, and hence of the notion of topological space. Cum granis salis we can think of topos Sh as "implicitly contained" in the notion of topological space. This is indeed how the notion of topos emerged historically, and where the term comes from: research in topology in 1960ies led Grothendieck to this notion but in a more general form, which I'll present shortly. A further categorisation which immediately suggests itself is a category of all toposes of sheaves (with different bases). I will not consider the latter construction here because of its higher complexity.

Grothendieck topos combines properties of geometrical topos Sh and "logical" topos S^C presented in 4.4. Formally speaking, it is a generalisation of the former and specification of the latter. (Ancient operations of generalisation and specification still work unproblematically!)

Recall the notion pointless topology from 2.7: opens are thought of as abstract "things" standing in a partial order relation satisfying conditions implied by the usual axioms for topological space. The partial order relation is thought of as inclusion (part/whole relation). Recall the "second gluing condition" for sheaves which concerns the covering structure. In the point-based (set theoretic) topology the notion of covering is treated in terms of unions and intersection of opens which are lifted smoothly into the pointless setting. Grothendieck's idea was to treat coverings independently from inclusions, and so allow other categories than partial orders to get a cover. *Grothendieck topology* (warning: it is not a particular topological structure but a generalised notion of topology) is defined on an abstract category C (which needs not to be a partial order) through specification of families of incoming morphisms for every object of C; these families of morphisms satisfy conditions implied by the usual notion of covering but not requiring C to be partial order (see Makkai&Reyes 1977). Philosophically the notion of Grothendieck topology is interesting because it shows clearer than standard accounts the borderline between topology and mereology. From the Grothendieck's perspective the standard (point-based or pointless) topology is viewed as *mereotopology* (to use the term invented by philosophers), that is, as a combined account of mereological and the (generalised) topological structure.

Category C equipped with Grothendieck topology is called a *site*. Now sheaf on given site C can be defined as functor $Sh:C\rightarrow S$ (where S is the category of sets) respecting topology of C. This second upgrade is useful even without the first, that is, in the case when C is frame (partial order) Fr of opens: instead of taking opens to sets one by one as it has been is done in 2.7 Fr is taken to (the category of) sets at once by functor Sh which can be then identified with the sheaf. Importantly functor Sh is *contravariant*, that is, it inverts directions of morphisms (inclusions) in C. If the assumed notion of functor requires preservation of directions of morphisms (i.e. corresponds to that of *covariant* functor) then Sh is written as $C^{op}\rightarrow S$ ($Fr^{op}\rightarrow S$) where C^{op} (Fr^{op}) denotes a category obtained from C (Fr) through reversal of its morphism and called *opposite* to C (Fr). The reversal of morphisms is needed because of the duality between points and opens mentioned in 2.7: if an open is thought of as union of its points then a point can be dually thought of as intersection of its opens; when unions and intersections are both defined in terms of inclusions the inversion of the inclusions exchanges unions for intersections and the other way round. Although this duality is not always "perfect" in the sense that it is not always possible to reconstruct opens through their points and points through their opens the reversal is required in any event. (In the Boolean case the duality is perfect and is known under the name of "De Morgan laws".)

(In a standard set-theoretic framework based on membership what I just said is erroneous albeit its intuitive appeal because the above explanation of duality requires identification of point P with singleton $\langle P \rangle$. However this problem doesn't arise in the categorical setting we are working in here. This is another argument for doing geometry categorically.)

Grothendieck topos is topos of all sheaves $Sh:C^{op} \rightarrow S$ on given site C, that is, the functor category $S^{C(op)}$. So Grothendieck topos may be viewed as S^C properly specified. (It is not exactly true because S^C as presented in 4.4 requires C to be small while a site in given Grothendieck topos requires only a weaker condition of local smallness but I skip this detail now, although it might be relevant for understanding of the relationships between logical and geometrical faces of topos.) Such specification has not only geometrical but also logical sense, and in some case the two senses become hardly distinguishable.

Consider the following fact which is plainly tautological but still sounds surprising. Take C to be frame Fr, so $S^{Fr(op)}$ is a topos of sheaves Sh in the usual ("modestly modern") geometrical sense (but written categorically). Fr represents (or *is*) "geometric propositional logic" GPL in the same sense in which Boolean algebra represents (or *is*) classical propositional logic CPL. GPL may be interpreted as "logic of finite observation": it doesn't allow for infinitary conjunctions (one cannot check all swans in order to claim that all of them are white) but does allow for infinitary disjunctions (if one finds a black swan one may conclude that the claim that all swans are white is false) (Vickers 1989). So there is little doubt that it is indeed a logic and quite useful (albeit weak) one. Geometrical properties of Sh correspond to (or *are*) logical properties of GPL. Let U,V be opens in Fr and S(U), S(V) be sets to which U,V correspond in Sh. We think of S(U), S(V) geometrically as sets of points of U,V, and logically as their "extensions". U<V implies $S(U) \subseteq S(V)$ by the definition of sheaf ("inclusions of

opens are respected”). This proves (this *is*) soundness of GPL. The converse implication doesn’t hold generally but if it does one says that sheaf Sh “has enough points” and calls frame Fr “spatial”. The spatiality of Fr is (equivalent to) completeness of GPL. So the geometrical property of Sh to be space-like (to have enough points) is (equivalent to) its logical completeness.

When things come to the first- and higher-order logic one needs to take into consideration the whole topos $S^{Fr(op)}$ but not an isolated sheaf as in the latter example, and the situation becomes far less trivial – and mathematically far more interesting. But there is no reason to think that the close link between geometry and logic just demonstrated brakes up abruptly at certain point. Methodologically it is perhaps not the best strategy to force the identification of geometrical and logical properties like in GPL but the idea that the two kinds of properties are properties of one and the same thing (topos) remains a fruitful guiding principle.

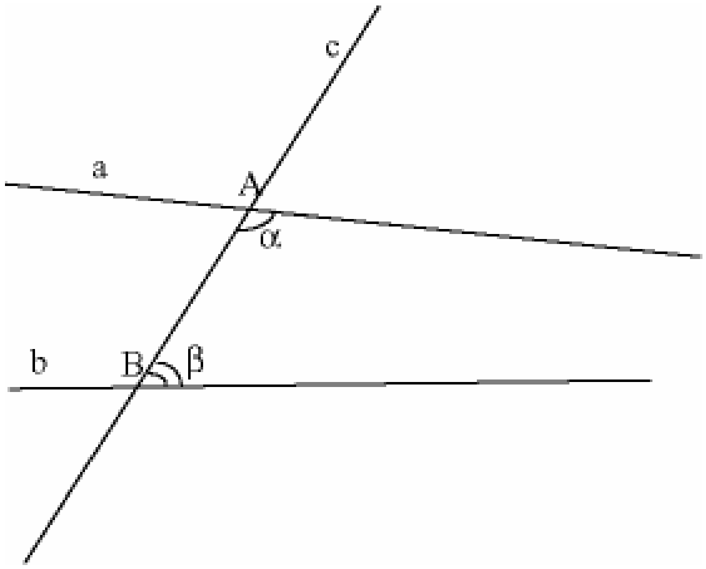
In the following section I approach the issue of categorical geometry from a different side considering its early pre-history. The reader will see how the “geometrisation of logical space” made explicit in the topos theory roots in the geometry of 19th century.

5.2 Differentiable manifolds and categorical integration of non-Euclidean geometries

Non-Euclidean geometries were not discovered by a genius of a single person but were developed through works of several generations of mathematicians (the number of generations depends on how one specifies the starting point of this development). What is perhaps less obvious this discovery didn’t result from one progressive intellectual development but from a synthesis of two different and even opposite developments.

5.2.1 Bolyai and Lobachevsky

Consider two straight lines a,b intersected by third straight line c. For further references I shall call this kind of construction a *linear triple*.



If $\alpha+\beta = \pi$ then a,b are parallel (aIIb), that is, they do not intersect. This fact can be proved on the basis of principles of Euclidean geometry but without 5th postulate, that is – to use an outdated term used by Bolyai (1832) - on the basis of "absolute geometry" (AG). The 5th postulate says that the converse also holds, or in the contrapositional form used by Euclid – that if $\alpha+\beta < \pi$ then a, b intersect. (The case $\alpha+\beta > \pi$ is straightforwardly reduced to the case $\alpha+\beta < \pi$.)

The problem of 5th postulate is that it has a poor or no intuitive appeal. Concerns about the lack of an intuitive support for the 5th postulate were expressed already in ancient times. Today we are in a position to say that the intuition didn't deceive Greek mathematicians and rightly pointed to the element of Euclid's system, which could be possibly varied without destroying geometry as such. One may wonder why this postulate had been adopted at all, and why the Lobachevsky geometry had not been discovered already in ancient times.

A standard answer to this question is the following. Greeks were too attached to everyday spatial experience an didn't think about geometry more abstractly as we do it today after Lobachevsky and Hilbert. What made the discovery of non-Euclidean geometry possible is a liberation of geometry from the everyday spatial intuition. In my view this argument is plainly wrong in spite of the fact that the story about the liberation of geometry from the intuition has certainly some truth in it (this is how Lobachevsky understood his own work after all). For, as it had been noticed by Greek mathematicians long before Lobachevsky, the everyday spatial intuition neither supports nor gives a clear evidence against the 5th postulate but leaves it problematic. This is how the whole issue around the "problem of parallels" emerged (already in ancient times).

There is a simple logical reason why Greeks took the Euclidean but not non-Euclidean option, which makes it unnecessary to suppose that they were prejudged against the non-Euclidean geometry in one way or another. The reason is that AG is essentially incomplete in the following sense: given a linear triple defined through angles α, β and the segment AB it doesn't give an answer to the question (Q) whether straight lines a,b intersect or not. Given the primitive character of the construction of linear triple it is not surprising that the question is not isolated and cannot be simply left open for future study. With the 5th postulate one gets a necessary and sufficient condition for aIIb, so for any given triple Q gets a yes-no answer. But the mere negation of 5th postulate taken by Lobachevsky as an axiom of his alternative geometry doesn't produce the same effect, as we shall immediately see. So the assumption of 5th postulate as a provisionally hypothesis remains the only reasonable option. Indeed, understanding the 5th postulate as universally quantified over all possible linear triples we get as its negation existensional proposition EL, which claims the existence of triple L such as $\alpha+\beta<\pi$ and a,b parallel (not intersecting). Let it be our working hypothesis that EL doesn't contradict AG, and let's see what kind of theory if any we can get by adding the former to the later. Fix a triple L with $\alpha+\beta<\pi$, boldly stipulate that a,b are parallel in this case, and pursue further constructions and related propositions on the basis of this stipulation and AG. Let G_L stand for the bulk of constructions and related propositions obtainable in this way. (Given a non-trivial logical structure of reasoning in the elementary geometry I prefer here the informal term "bulk" replacing "deductive closure".) Since all linear triples with $\alpha+\beta<\pi$ are "alike", one might expect that G_L doesn't depend on the choice of L, so we would have here a general theory alternative to Euclidean geometry. However it does as immediate examples show: to see this take a linear triple, consider continuous changes of its parameters α, β, AB, and decide few times differently when a,b are parallel and when they are not. So the mere negation of 5th postulate prima facie doesn't bring about anything deserving the name of geometry even if one doesn't get any contradiction with it.

Let me now describe the progress in the theory of parallels achieved to the 1830 (the year of publication of Lobachevsky 1830) in the following informal terms. Think about "envelope space" $\Sigma\, G_L$ as formed by "bringing together" all bulks G_L corresponding to all possible choices of L. Since $\Sigma\, G_L$ includes incompatible G_L it should be thought rather as a set of logical possibilities rather than a geometrical space. It has been shown that $\Sigma\, G_L$ can be sliced in a nicer way than into G_L-s. Namely, the choice of Euclidean triple L may be replaced by the choice of finite segment S such that $\Sigma\, G_S = \Sigma\, G_L$, where G_S are bulks like G_L but with a different underlying construction (replacing an Euclidean triple) completely determined by the choice of S. Slices G_S are nicer than crude pieces G_L in the following sense: (unlike G_L) G_S are all disjoint and incompatible, so $G_S = G_{S'}$ iff S=S', where the former equality sign stand for a reasonable equivalence between different G_S and the latter equality sign stands for the congruence. This means that for any linear triple with parameters α, β, AB any G_S (unlike G_L) provides a yes-no answer to the question whether a,b intersect or not.

Although ΣG_S is better structured than ΣG_L it is still a bulk of incompatible theories but not a theory. Any particular G_S neither can be called a theory in the usual sense because it depends on a particular construction, which involves segment S. But since G_S are all alike (isomorphic) they can be also regarded as equivalent *up to* the choice of L. From *this* point of view the choice of L is not essential. What is essential is only the fact that L exists and is fixed. This gives reason to call *the* Lobachevskian geometry what prima facie looks like a family of incompatible geometrical constructions and related propositions.

The linear parameter S (which maybe defined in many different ways) has been discovered by Lambert (1728-1777) and called the *absolute unit of length* (Bonola 1908). The interplay between the "relative" and the "absolute" involved here presents a piece of dialectics which must please a Hegelian. As things look from the traditional perspective the "unit" S is chosen arbitrarily from a continuum of available options. But from a viewpoint from the inside G_S S looks as fixed and unchangeable. This is not too surprising after all but only shows that what looks like absolute from a local viewpoint gets relativised if considered from a broader viewpoint. (Notice that here the broader viewpoint corresponds to the traditional Euclidean view.) What is more surprising is that the notion of the absolute unit of length - which appears here as "absolute" in a very modest and quite relational sense of the word - may be tentatively strengthened along the following vein. In *any* G_S every length is related to (corresponding) S, i.e. is expressed in the form rS, where r is a real number. So it is arguably pointless to say that in *different* G_S the expression "r absolute units" refers to *different* lengths but more reasonable not distinguish between copies of G_S saying that in *the* Lobachevsky geometry r always refers to the same length. In particular, if in a linear triple the length AB is given as rS then given α, β one is always in a position to say whether a,b intersect or not. So unlike what we have in the Euclidean geometry, in the Lobachevskian geometry we don't need to specify a unit of length because we have an "absolute" one. This, of course, explains Lambert's term.

Lobachevsky like many mathematicians before him tried to prove 5^{th} postulate as a theorem of AG through reduction ad absurdum, that is, by drawing consequences from the negation of 5^{th} Postulate in the hope of a contradiction. Consequences of this hypothesis were unusual but not plainly absurd. Lobachevsky was not the first who changed the attitude and started to regard such consequences not as merely pathological but as belonging to a different mathematical theory than Euclidean geometry, but he made it more systematically than his predecessors. *The* geometrical theory in which 5^{th} postulate was false Lobachevsky called "Imaginary" geometry. As the term clearly indicates Lobachevsky was motivated by the idea of detachment of geometrical imagination from the direct linkage with the usual spatial experience. He assumed as a methodological principle that mathematical theory could be sound even being purely "imaginary", i.e. corresponding to nothing in the physical reality and in the everyday experience. He assumed further that whether or not a mathematical theory fits a physical reality is not mathematical but physical question to be answered on the basis of experience (experiment and/or observation). This allowed him to take a pluralistic stance and suppose that in addition to geometry corresponding to the physical space there could be another geometry corresponding to nothing physical. He didn't take for granted the view according to which the physical space was Euclidean but considered astronomical observations in order to verify or falsify this hypothesis. In fact he realised that given the limited accuracy of observation 5^{th} postulate can be possibly falsified but cannot be possibly strictly verified by empirical methods. Lobachevsky's epistemological views combining the autonomy of mathematical reasoning with a clearly empirical approach to questions concerning physical reality fit very well today's scientific standard.

The story of liberation of geometrical intuition from the everyday experience by Lobachevsky and other founders of Non-Euclidean geometry is well known (Bonola 1908). I assume that this is a true story but I want to argue that this is a very incomplete story which if taken alone provides a distorted vision of the history and conceptual structure of the modern geometry. Let me now show what I think is the other side of the coin.

5.2.2 Gauss and Riemann

When in 1932 Gauss' friend Wolfgang Bolyai sent him a paper of his son Janos (Johann in German version of the name) containing results similar to those obtained by Lobachevsky a couple of years earlier Gauss reacted in a somewhat controversial way. He claimed that Janos' paper contained nothing he didn't already know some 30 years earlier, and that he wasn't ready to praise Janos' work because, as Gauss put this, "to praise it , would mean to praise himself". His reaction to Lobachevsky's results to which he got access only in 1840ies was more positive but this didn't encourage Gauss to publish his version of non-Euclidean geometry either. It is too easy to explain Gauss' dissatisfaction with Lobachevsky-style results by sociological reasons but I suggest to take it seriously, and to look for a true mathematical reason behind it.

The true mathematical reason behind Gauss' dissatisfaction by Boliay's work is, in my view, that he liked neither Lobachevsky's idea of splitting geometry into a number of incompatible theories[6], nor the idea of incomplete "absolute" geometry suggested by Boliay (and known

[6] Before Lobachevsky this line had been already taken by Schweikart (in a letter to Gauss of 1818) who called LG "astralishe Groessenlehre" (which can be perhaps better translated as "astrometry" rather than "astral geometry"). Unlike Lobachevsky Schwekart didn't make any significant progress in LG but clearly claimed its independence from EG and guessed that while EG worked well in application at human scales (in particular in the kind of

earlier to himself). Boliay's and Lobachevsky's approaches are very similar, and the fact that the former talked about "absolute geometry" while the latter defended his geometrical pluralism doesn't matter mathematically at all and doesn't matter much methodologically either. What is common in Boliay's and Lobachevsky's approaches is this: the idea of geometrical space is supplemented with (and in a sense subsumed under) that of logical space (of possibilities) in which different geometrical spaces – or at least geometrical constructions viewed before as incompatible - may peacefully co-exist. This peaceful co-existence of incompatible constructions didn't satisfy Gauss, as I suppose. Whatever is a philosophical attitude of today's historian toward the issue it is not correct to claim that the later development proved Gauss wrong. For Gauss' work indeed led to the discovery of new unifying framework for EG and LG, so today like in Euclid's time we have one science of geometry with its multiple branches but not a number of independent geometries as Lobachevsky suggested. This new unifying framework had been not been completely achieved by Gauss himself but his famous student Riemann did this in his *Habilitationsvortrag* (1854) based on Gauss' ideas. Before I explain what it is let me just name it "geometry of differentiable manifolds" (GDM).

To stress the importance of Gauss and Riemann against that of Bolyai and Lobachevsky might sound like knocking into the open door. Today's best account of physical space and time (Einstein's General relativity and its updates) is based on GDM. So it is GDM but not AG nor LG which after all replaced EG in physics. What I want to stress indeed is not personal contributions but contributions of different methodological strategies to what is commonly called the "discovery of non-Euclidean geometries". Assuming that the story about the diversification of geometry into "geometries" through "liberation from experience" by

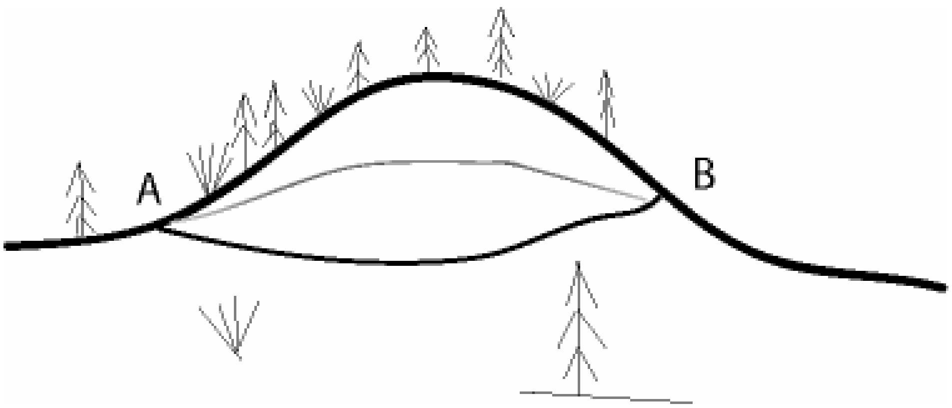
---

measurements which had given the geometry its name) at the astronomic scale things might stand differently (hence the term "astrometry"). See (Bueler 1981).

Lobachevsky and others is by and large correct, I want to show the importance of new integration of geometry achieved by Gauss and Riemann.

Gauss never made his epistemological views explicit but unlike Lobachevsky he never tend to detach mathematics from experience and everyday practice. On the contrary his main results have been achieved through bringing geometry *back* to the immediate contact with the same kind of experience from which this science emerged more than two thousand years before. I mean Gauss' work in the vast project of surveying the Kingdom of Hanover during crucial (for the history of geometry) years 1818-1832. New methods in the geodesy – which is a new name for what geometry used to be in the pre-theoretical epoch – has been as much important for the "geometrical revolution" of 19th century as Lobachevsky's freedom of mathematical imagination and Schweikart's insight that Aristotle might after all have a point arguing that things at astronomical and human scales are essentially different. The principle theoretic improvement in the geodetic surveying made by Gauss is this: instead of thinking of given territory as a piece of Euclidean plane one should think of it as a smooth surface. The latter hypothesis is more realistic (moreover if the territory is hilly like the Hanover region) but just like the former it involves a strong idealisation. To provide evidences that no piece of Earth surface is perfectly smooth is just as easy - and in most contexts just as pointless - as to provide evidences that no such piece is perfectly plane. The new basic geodesic hypothesis needed a theory of smooth surfaces which Gauss developed in his (1827). Gauss' key idea in this prominent paper is that of *intrinsic* geometry of surface.

To get the idea you need to find yourself in a hilly surroundings and forget everything that you have learnt in the school about spherical Earth flying in the infinite Euclidean space. Then if you do geometry not only with drawings on A4 paper but also with measuring lengths about tens meters on the ground you will immediately discover that the space you are in is not Euclidean. In particular, you'll find that between two sufficiently distant points A and B there could be few different well-distinguishable paths, all of which are shortest possible:



Such "return back to things" (*epoché*) is necessary but, of course, not sufficient for doing new mathematics: after the removal of the old conceptual scheme a new one must be installed. This goes through new simplifying hypothesis the most important of which in given case is this: when A and B are sufficiently close then the shortest path between them is still unique, and the Euclidean geometry remains valid (differentiability). So one conceives a geometrical space comprising points (just like EG), continuous and smooth (locally Euclidean) paths between points (called geodesics), and everything constructable out of this by earlier known methods. Riemann's notion of smooth manifold is an elaborated and generalised version of such a space.

It is remarkable to observe that the mathematical part of our best physical theory of space/time, namely, of General Relativity, originates from the advanced surveying work just like spatial concepts of earlier physics originate from the surveying practice of our ancient ancestors. Just as before today's best physics generalises upon conceptual schemes developed through earthy human practice. We could expect that a new look at the surface we live on, particularly one dropping the differentiability hypothesis in a non-trivial way, will be equally important for the future physics.

5.2.3 Integrating power of interpretation

It is obvious that the notion of differentiable manifold made geometry again into one science but it is much less obvious how and why. True, GDM generalises upon EG and LG but so does also AG without any interesting integrating effect. In fact GDM involves much more "geometries" than AG because any manifold can be viewed as a geometrical space with its own "geometry"[7]. The very fact that all these things fall under the same notion of manifold explains nothing: a mere generalisation is not sufficient to bring together pieces of geometry split by Bolyai and Lobachevsky. To see where the integrating power of GDM comes from let's come back to Gauss' idea of intrinsic geometry.

Here is the trick. After you have forgotten that you are doing geometry on a curved surface located somewhere in Euclidean space (*epoché*) and learnt to live in a non-Euclidean space autonomously, remember the enveloping Euclidean space again (*epistrophé*). So you get two different perspectives on your geometry: internal and external (Euclidean). Now you may

---

[7] In 1-dimensional case there are only two essentially different (intrinsic) geometries: that of continuous infinite line (without endpoints and self-intersections) and that of circle. In 2-dimensional case there are 3 infinite series of geometries: these of spheres with n handles attached (otherwise called n-tori; n = 0,1,2,…); of projective planes with n handles attached, and of Klein bottles with n handles attached. For 3-dimensional manifolds the classification is far more complicated but if the prove of Poincaré's conjecture proposed by Grisha Perelman in 2003 is correct, the problem is finally settled in this case too. For dimensions >3 classification (in the usual sense and by anything like usual means) is provably impossible but the possibility of new brakethrough results in this field, of course, shouldn't be ruled out.

look at "smooth paths" in your space in two different ways: as primitive objects ("lines") of a unusual geometry and as curves in the usual Euclidean space. This allowed Beltrami (1868) to achieve what he thought was a complete reduction of LG to EG. Beltrami found (constructed) in the Euclidean 3D space a surface called pseudosphere with the following property: geodesics on the pseudo-sphere behave just like straight lines in LG. Beltrami claimed boldly that Boliay and Lobachevsky "in fact" discovered nothing but geometrical properties of the pseudosphere – which is an Euclidean object - without acknowledging this. Noticeably Gauss himself never took Beltrami's conservative line in spite of the fact that he himself studied only surfaces embeddable into Euclidean space. Instead Gauss readily praised Riemann's suggestion to define a manifold from the outset intrinsically. There is indeed a pure mathematical reason not to take the Euclidean reduction suggested by Beltrami seriously: most Riemann manifolds are not embeddable into Euclidean space (even higher-dimensional). So the internal and external perspectives are not equivalent: there are manifolds which are invisible from the external Euclidean perspective at all.

Although the Euclidean reduction suggested by Beltrami doesn't work for all manifolds (and in fact it doesn't work smoothly even in the case studied by Beltrami himself[8]) the possibility to interpret some "geometries" in terms of some others is remarkable. Reichenbach (1928) compared it with translation between two spoken languages. A native speaker of language A would start to learn language B by translating B to A but later he would speak in B without

[8] The problem noticed by Hilbert and Hemholz immediately after Beltrami published his result is that Beltrami's pseudo-sphere has singularities, that is, particular points or particular curves (dependently on given type of pseudo-sphere) where the differentiability, and hence, Beltrami's interpretation of LG fails. The problem might look technical but in fact it is profound : as Hilbert proved in 1901 there exist no Euclidean surface with intrinsic LG and without singularities.

any help of A. A native speaker of B learning A would make the same but the other way round. So, Reichenbach argues, interpretation of LG in EG suggested by Beltrami plays no distinguished role, and what is indeed important is the relativisation of geometrical notions achieved through translation between different "geometries".

Imagine that each "geometry" is translated into each other by Beltrami's method whereas this is possible. Then if there are enough available translations the split universe of geometries gets integrated into one conceptual whole. The notion of manifold allows for a strict definition of such translation as smooth transformation. (Smooth transformations are more general than these used by Beltrami because they are not necessarily embedding). This is how the notion of manifold helps to integrate geometry.

The notion of category allows for a rigor account of the obtained global construction: it is the (concrete) category of differentiable manifolds (as objects) with smooth transformations as morphisms. That is why it is justified, in my view, to regard the integration of geometry achieved by Riemann as a case of categorification occurred long before the category theory itself appeared at the scene.

Notice that the double (external and internal) perspective on given manifold (think about the interpretation of LG on Euclidean pseudo-sphere) relativises the distinction between a geometrical space (with its "geometry") and a geometrical object in given geometrical space. This distinction hardly appears in mathematics before modern times: Euclid talks about geometrical objects like squares and circles but never about geometrical space as a whole. Now we come back to a similar view except we can also think of some geometrical objects as "places" (Greek "topos") for some others. The idea of absolute geometrical space (no matter there is one such thing or many) is left to historians together with its physical counterpart.

6. Categorification against centred integration modes

The reader could notice that the list of integration modes given in part **2** is partly motivated by the category theory. Many comparisons between these modes and categorification have been already made throughout this paper. Now I shall make such comparisons more systematically trying not to repeat myself.

6.1 Categorification versus generalisation and setting: localisation

In the classical Greek philosophy one finds two generalising concepts the difference between which is often left unclear: species and genus. Both are predicated similarly (Socrates is human, Socrates is philosopher) and this is perhaps a reason why in the later tradition (as exemplified by Porfirius) the distinction between genus and species was understood relationally: a species G was called genus with respect to another species S if S is sub-species of G. However there are many hints giving reason to think that more than this is involved here, in particular Aristotle's ban of free shifting between genera, and his claim that "being" is not a genus and so cannot be split into species (so large genera like "motion" and "rest" shouldn't be viewed as species of being).

Perhaps genera can be better understood as logical types. Then interpreting types as objects of a logical category we get a picture which seems to be by and large in accord with what Plato says us in the end of his *Sophist*: dialectics (categorical structure) of genera determines the logic of species.

Leaving historical speculations aside we can say that the categorical construction of logic leaves to generalisation a modest and local role of bringing together sub-objects of each particular object. The principle integrating effect of categorification is achieved through composition of morphisms but not through generalisation, and the latter clearly doesn't reduce to the former.

A similar remark concerns setting. The principle idea of setting is that of free integration for everybody: given two things A,B one gets set $\langle A,B \rangle$ immediately (pairing). But the free setting requires extensionality, and the price of extensionality appears to be quite high (perhaps unacceptable). The distinction between set A and singleton $\langle A \rangle$ revealed by the logical analysis shows that the naive understanding of extensionality according to which a set is "nothing but its elements" is wrong, and that the issue of integration cannot be simply avoided in this way.

Categorification suggests an alternative way of integration instead of trying to avoid the issue. It works roughly like this: given things A,B construct morphism(s) $A \rightarrow B$ and/or $B \rightarrow A$. Here we have nothing over and above A,B like brackets $\langle , \rangle$ (which are supposed to be innocent but in fact are not) but instead we get something in between. This makes categorification more difficult than setting (the morphisms are not given for free but should be constructed, and it might not work) but also more realistic if the think about setting and categorification in practical and physical terms. Thinking of A,B as physical objects one may look for a reasonable physical interpretation of morphisms $f{:}A \rightarrow B$ as interactions (or the other way round look for a reasonable categorical interpretation of known physical interactions between A,B). The standard set theory suggests no such possibility, and this makes the standard notion of set an abstract logico -metaphysical matter detached from the physical reality and practice.

6.2 Categorification versus co-generalisation and sheaving: decentralisation

Recall that I called co-generalisation an integration mode in virtue of which different properties, "stories", and attributes of other kinds are "hooked together" by common subject matter. Sheaving is a suitable mathematical representation of this idea. We have seen that the notion of Grothendieck's topos allows for doing logic with sheaves, and that such logic is "spatiotemporal". So sheaving and categorification work well together, and, of course, my term “co-generalisation” (like other co-terms) is categorically-motivated albeit it refers to rather traditional notion.

Notice however that co-generalisation and sheaving are centred integration modes but categorification is not. The categorification of the notion of sheaf involves a hidden conflict between the centred character of sheaving and non-centred character of categorification. Recall the three different notions of sheaf mentioned in this paper: the ancient example of sheaf of straight lines, the “modestly modern” notion of sheaf over topological space – see 2.7, and the categorical notion of sheaf over site – see 5.1. We observe that the “centralisation” of sheaf is first weakened and then disappears: first the centre (point) becomes a topological space, and second the topological space becomes a site and the sheaf turns into functor $C \rightarrow S$ from site to sets or another background category. Notice that logical categories, and particularly toposes have distinguished objects like terminal object or subobject classifier. So the categorical “decentralisation” doesn't imply that its elements become all “equal” like elements of a set.

6.3 Categorification versus relation and co-relation: relativisation

As I have already stressed categorical morphisms are not relations in the sense in which this term is used in the modern logic and philosophy which respects the modern logic. However the fact that this term was used differently in the past, and is still used differently today (by authors who don't like the new regimentation or simply don't care about it) is difficult to ignore in a philosophical analysis of the category theory. For much of what philosophers have written about relations becomes relevant when one thinks of their relations as our morphisms. Let me for this reason talk now about relations in a broad traditional sense.

Few times in the paper I have mentioned relativisation as an epistemic operation. Speaking very roughly relativisation takes place when given object A one discovers that its properties (again in a broad sense) not only depend of its environment (this is normally allowed before) but cannot be even reasonably thought of without this environment. A standard example (for which the restricted logical notion of relation will do) is the property of being big. So relativisation like foundation is an integration mode working at higher scales of organisation of knowledge.

A scary side of relativisation may be demonstrated by Lobachevsky's example: before Lobachevsky's discoveries people had a science of geometry which provided certain non-trivial truths about its subject-matter (like one saying that the sum of internal angles of any triangle equals to two right angles), and then it appeared to be that a large class of geometrical truths is conditional or perhaps even conventional in the sense that the truths depend one one's free choice of axioms. So there is no longer true geometry and geometrical truth. To take the "if-then" line remains an option but this doesn't look like a satisfactory replacement. Whether geometrical objects are a human handcraft or divine creation we want truths about *them* but not about certain implications.

I'm agree that such a relativisation is bad. But I think the reason why it is bad is that it is poor and incomplete. In 5.2 I showed that Gauss and Riemann relativised the old geometry not only much further but also much better making the if-thenist attitude unnecessary. That geometrical and other scientific truths depend on assumed hypotheses people knew long before Lobachevsky, so it could hardly be a particularly bad news. That geometrical axioms could be no longer viewed as true about physical space (even modulo mathematical idealisation and physical approximation) because they now could be chosen in different incompatible ways was indeed, I suppose, a bad news for many. I must confess I understand these people and with some reservations share their attitude. But the point about the physical space was only a consequence of the main bad news about splitting geometry into incompatible parts only weakly connected through the if-then device. When the if-then thing remains the only working integration device in a discipline the situation is worrying indeed. The main achievement of Lobachevsky is not the relativisation of geometry but revealing its complexity. A satisfactory relativisation of geometry allowing for a new effective integration of this discipline was achieved by other people. As I have already mentioned it involved far more profound relativisation of geometrical concepts than Lobachevsky thought of, such as the relativisation of the notions of geometrical object and geometrical space. I think it would be quite unfair with respect to people like Galileo and Einstein to reserve the term "relativisation" for the trivial if-then relativisation.

Thinking about morphisms as relations in the broad traditional sense we can say that the mathematical notion of category easily allows for "relation of relations". In fact even the pre-categorical notion of structure-preserving transformation (homomorphism) can be regarded this way (if the structure in question is understood as relational structure). Co-relation in the sense of 2.3 is a special case of "relation of relations", namely a centred case. This makes the co-relation useful for translating traditional centred conceptual schemes into the new categorical context. Recall how one gets topos of sheaves from a topological space: first, one relates opens of the space to sets (possibly with a structure respecting relations between the opens) and so gets a sheaf, second, one relates different sheaves over the same space, and so gets a topos. If we allow for talking about morphisms as relations a topos of sheaves represents the general notion of co-relation in the sense of 2.3.

6.4 Categorification versus formalisation: irreversibility

The historical perspective given in 5.2 allows for regarding the idea of formal axiomatic method developed by Pasch (1882) and Hilbert (1899) as a link between the two lines of development in the geometry of 19th century discussed above: Lobachevsky's line of logical construction of mathematical theories, and Gauss-Riemann's line aiming at crossinterpretations of different theories. The formalist solution bridging the two lines is this: to construct mathematical theories logically making them invariant under possible interpretations. Hilbert:

"… it is certainly obvious that every theory is only a scaffolding or schema of concepts together with their necessary relations to one another, and that the basic elements can be thought of in any way one likes. … One only needs to apply a *reversible* one-one transformation and lay it down that the axioms shall be correspondingly the same for all transformed things. " (Letter to Frege from 29 December 1899, see Frege 1980; italic mine)

From the categorical viewpoint it is clear in which sense the formalist solution is limited: the invariance under interpretation (understood as "identity up to isomorphism of interpretations") leaves non-reversible interpretations including embeddings aside. If the invariance under interpretation (that is, categoricity in the old model-theoretic sense) is not achieved this from the formalist viewpoint looks like a failure or at least a difficulty. Let informal theoretic construction G be the intended interpretation (model) of formal theory F; in other words F is a tentative formalisation of G. Suppose that in addition to G F has another model G'. If G and G' are isomorphic then a formalist says (and I am agree) that it is essentially one and the same thing best (at least for a special formalist purpose) represented by F because unlike G and G' F doesn't involve any irrelevant content. However if G and G' are not isomorphic the situation is viewed by the formalist like this: since F doesn't distinguish between the two theories it fails to capture any of them, and so F is only an incomplete description of G. A good deal of theoretical work done in the formalist framework aims at ruling out non-standard (non-isomorphic to intended) models. As I have already mentioned in 2.8.2 it appears to be, generally speaking, impossible. From the categorical viewpoint the very purpose of ruling out non-intended models looses its significance. The failure of this enterprise demonstrates the obvious fact that morphisms between mathematical constructions (including mathematical theories) cannot be reduced to isomorphisms. What the category theory suggests is to take morphisms of all kinds into consideration instead of fighting for isomorphisms at any price.

To explain his view Hilbert famously argued that if points and straight lines in Euclidean geometry were exchanged for beermugs and tables but the relations between these things were kept (formally) the same we would still have Euclidean geometry albeit in a different appearance. Such transformation is obviously reversible because one may exchange the beermugs back to points and the tables back to lines. An issue widely discussed throughout the 20th century was whether or not Euclidean geometry or another mathematical theory indeed survives this kind of radical re-interpretation, and whether or not non-relation properties of points, beermugs, and other things count theoretically. People arguing against formalism in favour of mathematical intuition would say that they do, and hence the beermug geometry wouldn't be the same – in any appropriate sense of "same" - as Euclidean geometry of lines and points. Structuralists would say that Hilbert is right. Let's take the structuralist side and suppose that Hilbert is right indeed. There still remains another question which in my view is more importnt: whether or not given theory can survive (in some reasonable sense) also a non-reversible (non-isomorphic) interpretation? Cannot it be the case that G, G' are not isomorphic but nevertheless "essentially the same"?

Let me give a linguistic argument pointing to such a possibility. Let's change Hilbert's dummy example to make it more trivial but at the same time more realistic. Suppose that given Euclidean geometry we don't exchange points to beermugs really but only call them differently, so what everybody calls a point we shall call a beermug. Obviously this leaves Euclidean geometry as it is. Such a change of terminology is safe and reversible. The point is that what mathematician usually mean by "changing the terminology" is neither safe nor reversible but often quite meaningful and effective.

I shall first demonstrate this first for natural languages and return to mathematics later. Consider translation $t:A\rightarrow B$ between two languages which translates text a in A into text b in B. Consider also a backward translation $t':B\rightarrow A'$ which translates b into text a' in A. Given the common translation practice one cannot expect to get a'=a, or at least not literally. Thus t is not an isomorphism. However since translation is supposed to "preserve meaning" one might expect that a' and a have the same meaning (are semantically equivalent), in symbols $a'\cong a$. If the semantical equivalence is indeed an equivalence one can split a language (thinking of it as a set of all meaningful expressions) into equivalence classes making translation reversible "up to semantical equivalence". This would automatically imply a semantical equivalence between (equivalence classes of expressions in) different languages, so one would get "pure meanings" invariant under translation. Theoretically this doesn't seem absurd and may be even suggested as a definition of meaning. But practically it is absurd: only very basic meanings available in natural languages (and taken *literally*) like the meaning of the word "mother" in English might allow for such a universal factorisation but it definitely doesn't work generally. Check any good dictionary to see this clearly.

One might suggest that the non-reversible character of translation is a specific feature of natural languages, and that in science and mathematics things are or at least *should* be different. For expressions in natural languages are often vague while scientific and mathematical expressions should be clear. I shall not talk about natural sciences but the case of mathematics clearly shows that the non-reversibility of translation (interpretation) doesn't imply vagueness, and that the restriction of mathematical interpretations to reversible interpretations cannot be justified. When mathematicians conventionally speak about reformulation of theories into "set-theoretic terms", "categorical terms" and the like, there is no slightest reason to suppose that such transformations are reversible. Since this kind of talk can be disqualified as informal and hence imprecise let me give a precise example. Think again about arithmetical models of geometrical theories used by Hilbert in (1899) in order to demonstrate his method: they are *embedding* (monos) into arithmetic but not isos. The reversibility can be forced in this case only through restriction of arithmetic to the arithmetical models in question. But this is an artificial operation which doesn't have any other purpose except saving the idea of formalisation. This restriction picks up an arithmetical construction which doesn't make sense outside the discipline: to make geometry with numbers one needs to learn something about numbers before. This example shouldn't make the reader think that all essential morphisms are monos.

The fact that translation and interpretation in natural languages and in mathematics is, generally speaking, irreversible is so obvious that I cannot believe that it could be simply missed by people started formalist projects in mathematics and philosophy. But I think that they assumed a wrong choice between formalisation and the free play of metaphors and associations, which is good for poetry and useful in the everyday life but dangerous for science. Although the notion of category is itself a formal notion it makes it clear that the choice is wrong: irreversibility of interpretation doesn't make it chaotic and uncontrolled, and structures can be well respected without reversibility.

Thus the category theory suggests the following alternative to the formal axiomatic method in geometry and elsewhere: theoretical objects in question are made into a category, the category is linked functorially to other available categories, and logical issues are treated in terms of logical properties of this categorical construction. Whether or not toposes should be viewed as key constructions in this approach is an open question.

6.5 Categorification versus co-formalisation and mereological summing: intuition

Recall that in 2.4 I called co-formalisation the following operation: different foramlisations of the same intuitive notion are gathered around (now we can also say – sheaved over) the notion they are formalisations of. Thinking of Grothendieck topos as a categorical reconstruction of formalisation one may reconstruct co-formalisation as a topos category T↓B over a common base B taking functors C→B as objects and functors C→C' (making resulting triangles commutative) as morphisms. Think of C,C' as given formalisations of B (given formal theories), and of B as "unknown" representing the intuition underlying its different formalisations C,C': the idea is to specify B as strongly as possible requiring that co-formalisation category T→B keeps reasonably good properties. Let me leave details for mathematicians (perhaps it may be better done otherwise than just suggested) and explain an epistemological significance of co-formalisation. I believe that as an epistemic operation (like formalisation) it makes sense even without application of the mathematical machinery. Mathematicians and (at list some) philosophers like examples, and many of these people would agree that examples are sometimes more interesting than general concepts they exemplify. Recall that exemplification is a differentiation mode associated with generalisation. Categorification doesn't cancel the generalisation but limits its reach replacing it at the global level. Co-formalisation is a categorical analogue of exemplification which also works globally.

Consider a square. It exemplifies many differing mathematical concepts: a regular polygon, a particular group, etc. Collecting such concepts we get a kind of sheaf structure. One might think it is basically a sheaf of properties of the square, so we don't get anything unusual here. However this sheaf has this non-trivial feature. The usual way to study a mathematical concept is first to define it (through a traditional definition or Hilbert-style axiomatic), and then prove certain facts about it, that is, to make a theory. But giving the example I mean something more involved: the same square Q is treated by multiple theories T,T',..which are formally independent (each may use its own definition of the square) but nevertheless linked through the square Q. Generalising and formalising strategies would require to make all T,T' into one theory but the co-formalisation suggests a different way of integration.

Like a topological space square Q effects "gluings" between T,T',..through their mutual interpretations: one may start with the notion of square as Euclidean polygon, and then explain how it represents a group but one may also do it the other way round providing a group-theoretic account of Euclidean geometry in Klein's vein. Reversibility is not assumed here: the two interpretations are not symmetric and don't reduce both accounts to anything like common formal theory. Since we don't chose any particular theoretical account of our square it would be more appropriate to call it *intuition* rather than concept. The notion of intuition thus introduced better suits to the notion of intuition given by Descartes than the formalist understanding of intuition as vague, irresponsible and at the same time creative kind of thinking.

Formalisation reveals the important fact that different theories might have common form. But the fact that one theory might have different forms is not less important and not less interesting mathematically. The standard argument against formalisation appealing to intrinsic properties of models ("Euclidean geometry is about points but not beermugs") is misleading because, as the above construction shows, there is nothing particularly intrinsic about intuition. Intuitions involves just as "many things" as forms and have the same relational character: intuitions make no sense without their multiple forms just like forms make no sense without multiple intuitions associated with them. It is not only that forms without intuitions are empty while intuitions without forms are blind but rather that without each other forms and intuitions have no sense at all.

At the first glance the part/whole relation didn't play any significant role in the present paper. Categorically this relation is usually interpreted as inclusion of subobjects. When one works in the category of sets the partial order is Boolean and self-dual. This explains why no interesting notion of mereological co-summing easily comes to mind: it is the same thing. In the general case the order is Heyting, and its dual is not. This is the matter of "imperfect duality" between pointless and point-based topologies, and between intension and extension discussed above.

Nevertheless it is obvious that the issue of epistemic integration I have chosen as a general framework in this paper is profoundly linked to the part/whole issue. I consider the old problem about how multiple things manage to make it into one whole and suggest setting, sheaving, categorification and other "integration modes" as possible solutions. Perhaps it is indeed possible to avoid thinking of objects of category as elements of set or class but it is still hardly possible to avoid thinking of the objects as "parts" of its category in some irresponsible sense.

A formal-minded philosopher could object that such a broad intuitive understanding of the notion of whole confuses many different relations, so the notions of part and whole should be strictly defined through a formal mereology. The defender of intuition will remark that the intuitive notion of whole continues to play in mathematics and science an import role while formal mereology appears to be mathematically trivial and serves nothing.

I suggest the following way out of this strife: co-formalise the part/whole thing instead of formalising it. For this end think about the "broad intuitive sense" as "broad" not like in the case of "broad generalisation" but in the dual sense. (It is also helpful to change the spatial metaphor for "deep" or something else.) Then consider multiple conceptualisations of the part/whole intuition, for example these which I call integration modes (or choose the list in a different way appropriate for your purpose) and look how they translate into each other.

6.6 Categorification versus foundation: axiomatic method

Euclid's *Elements* are often viewed as the first application of axiomatic method in mathematics albeit in an imperfect form different from today's. Certainly Hilbert had Euclid's *Elements* in mind writing his (1899), and the genealogical line between these two great mathematical texts can be indeed traced throughout all the historical distance separating them. However the view that Euclid's method is nothing but an imperfect version of Hilbert's is wrong at least for this reason: Euclid uses two kind of principles, namely axioms and postulates, where Hilbert uses only axioms. As a historian rightly observes "Modern mathematics see no essential difference between an axiom and a postulate" (Boyer, p. 116) and use the term "axiom" indiscriminately. I think that this is an error to be corrected, and that the error is not only historical but also theoretical.

Taking seriously Euclid's wording (unfortunately not preserved in standard translations) one discovers the obvious: his postulates are not true propositions about geometrical objects or anything else but requirements. For example, the first postulate requires "to draw straight line from any point to any point". It is tempting to interpret this requirement as an existential proposition claiming the existence of such a line but this, in my view, is a misinterpretation. What is meant here is that the required operation is feasible. So in addition to axioms (primitive true propositions) Euclid assumes a set of primitive operations. The claim that the operations reduce to existential propositions is a very strong claim, which in my own view is wrong, and which in any event is far from being anything obvious and innocent .

Proclus (1873) mentions ancient mathematicians (Speusippes and Amphinomes) who like Hilbert (and quite few European mathematicians before Hilbert) tended to reduction of postulates to axioms, as well as other constructively-minded mathematicians (Menaechmus and his school) who tried reduction in the opposite direction. Proclus himself doesn't engages himself into such reductionist projects but nevertheless he philosophically sympathises the first group of people since his Platonic view implies the superiority of *being* (which he associate with axioms for the simple reason that axioms tell us how things *are*) over *becoming* (which he associates with postulates which tell us what can be *done*). However neither Proclus nor Plato himself ever go so far as to straightforward reduction of the mathematical becoming to the mathematical being. A specific reason why such reduction looks unlikely for a Platonic is that Platonism (I mean the Platonism based on Plato's philosophy but not the synonymous ontological doctrine in the contemporary philosophy of mathematics) places mathematics between the ideal (that is, real for a Platonic) being and material becoming. Since being and becoming are mixed up in mathematics anyway the distinction between being and becoming *within* mathematics only reflects the same distinction in the basic ontology, and so to challenge this distinction in mathematics would mean to challenge the basic ontology. The category theory, if taken seriously, tends to revision of the formal axiomatic method suggested by Hilbert to the effect of re-establishing the Euclidean balance between constructive ("concrete") and propositional ("abstract") aspects of mathematics (see 4.4). Hilbert himself would never claim that his formal mathematics replaces usual intuitive informal mathematics. He rather thought in the Kantian vein that the former without the latter is empty while the latter without the former is blind. So I don't suspect him to be a simplistic Platonist, who thought that he could reduce all the mathematics to *forms* for free. But no matter how one decides which kind of mathematics – formal or informal - is more important or decides that both are important the formal axiomatic method takes the two kinds of mathematics apart. The model theory started by Tarski can be viewed as the first step of bringing the two parts of mathematics back together, and the categorical mathematics as a further step into the same direction.

The question "Can a categorical foundations replace standard set-theoretic foundations of mathematics?" doesn't have a straightforward answer not only because things are not yet settled to the date but also because the categorification is not neutral with respect to the idea of foundations, so it would be misleading to talk about set-theoretic and categorical foundations taking the notion of foundation for granted. As I have already argued the categorification suggests an alternative to the idea of formal axiomatic method (to put it in two words the alternative is that of functorial linkage), and hence challenges the idea of formal axiomatic foundations. This point I shall not discuss any longer. But what about foundations of different kinds?

The functorial linkage is compatible with the general idea of foundations as integration of theoretical constructions through linking them to a single centre (foundational kernel) but it doesn't provide any additional support to this idea, and moreover shows that a higher-scale integration can be achieved differently, and hence foundation might be not needed.

However if the notion of category indeed works throughout the mathematics and perhaps beyond and allows for integrating it into one whole doesn't this make the category theory a new centre, a new foundation? I think this remains an open question, and the answer depends on how the category theory is applied to the rest of mathematics. It is illuminating to consider this question together with a foundational problem of the category theory itself which I have mentioned in 3.2: to define an abstract notion of category one needs to speak about its morphisms, and it is difficult to see how speaking about morphisms one can avoid speaking about sets or classes of morphisms. (There is also more specific reason why this talk is important: it allows for the distinction between big, small and locally small categories which appears to be essential.) I shall consider briefly two different approaches to this difficulty, and we shall see that these approaches imply different views on the question of foundational role of the category theory with respect to the rest of mathematics.

One is (Lawvere 1966) suggestion of category of categories CC as foundations; see also 3.3 above and (MacLarty 1991). The idea is to construct CC as abstract Cartesian closed "meta-category" not taking care about foundational difficulties including one just mentioned, then pick up its object C and take care about these difficulties in C using logical and other resources of CC. In particular, objects of C are its points in CC. The internal logic of CC allows for specifying C in different ways. In particular it may be specified as the abstract category of sets SC also mentioned in 3.3. CC plays here both roles of "meta-theory" and of "logic" with respect to C like in the case of internal categories considered in 4.3.

This approach rises the following question. CC and all it objects like SC are abstract in the sense of 3.3. Should SC be formally distinguished from the concrete category of sets S as its abstract counterpart? If the answer is "yes" one may indeed regard SC as an abstract counterpart of S similarly to how a set in the sense of ZFC is regarded as an abstract counterpart of a "naive" set. Then CC may be viewed as foundations of mathematics in the same formalist (albeit no longer logicist) sense in which ZFC is viewed as a foundations of naive set theory and all the naive mathematics. If the answer is "no" there are again two option: the distinction between abstract and concrete categories can be either (i) internalised in CC or (ii) simply dispensed with. The internalisation (i) can be achieved if one thinks of S and other concrete categories as objects of CC, and of abstract constructions in CC as descriptions of these concrete categories. In this case CC becomes a common conceptual framework rather than foundation in the usual sense. Remarkably CC can be then no longer viewed as an abstract category. For supplying CC with concrete categories like S, Top (the category of topological spaces) and the like one is making a bottom-up construction: first makes known mathematical concepts into categories, second constructs functors between these categories, and finally studies global properties of the obtained fragment of CC. However in Lawvere (1966) CC is treated as an abstract category constructed rather top-down. Perhaps the internalisation of the concrete/abstract distinction can been also achieved in CC through some form of relativisation of this distinction. But in any event such internalisation seems to be incompatible with the view on CC as plainly abstract, and with the idea to use CC as a purely top-down foundations (see 4.4). (ii) remains an option however at least Lawvere's works reinforce the role of the concrete/abstract distinction in mathematics rather than show how to avoid it.

A different approach to the same foundational difficulty (how to speak about morphisms without using the notion of set or class) is taken in (Benabou 1985). Benabou starts with functor $C \rightarrow S$ asking which properties of sets are indeed needed in order to make sense of abstract category C. He observes that what is needed is an analogue of indexing allowing to speak about families of morphisms. This leads Benabou to the notion of fibred category (first introduced in (Grothendieck 1963)) which is a category were objects are functors $C \rightarrow S$ respecting the needed property, and morphisms $C \rightarrow C'$ make the resulting triangles commute. Fibrations can be similarly defined over different bases than S. The notion of fibred category is not supposed to provide a foundations but it shows how a foundational difficulty can be tackled without a conceptual closure like CC (and perhaps without replacing of older foundations by a new ones). Remark that in Lawvere's CC approach categories are thought of as objects (of CC) or identity functors while taking Benabou's approach seriously one should rather regard a (non-identity) functor like $C \rightarrow S$ as what a category essentially is.

The idea of CC as foundations is apparently that of a global structure allowing for description of working mathematical concepts in terms of its internal relations (I use the term "relation" liberally again). This feature is common for CC foundations and the formal axiomatic foundations. The fibration approach is interesting from the foundational point of view because it tends to the relativisation of the internal/external distinction similar to that suggested in 2.7.1: the internal structure of functor $C \rightarrow S$ makes it self-sustainable without turning it into a conceptual closure.

In later work 2003 Lawvere defends the idea of foundations in practical and educational rather than logical sense , and hence of foundations in the sense of Euclid's *Elements* or SFM rather than ZFC or Hilbert's *Grundlagen* . He describes the purpose of foundations and the role of axiomatic method as follows: "Foundations is derived from application by unification and concentration, in other words, by the *axiomatic method.* Applications are guided by foundations which have been learned through education". Although Lawvere doesn't distinguish between postulates and axioms like Euclid the axiomatic method in his broad sense certainly allows (and indeed involves) basic constructions and basic operations as well as basic truths about these things. Foundations so understood is a generic but no longer closure construction like CC.

I'm agree that foundations in the sense of Lawvere 2003 remain important, and I can see that integration of knowledge by itself doesn't accomplish the concentration function stressed by Lawvere. The need of concentration for educational purposes gives indeed good reason to take centred integration modes seriously. However I would like to remark that the structure of education is itself a non-trivial issue. The old situation when one Euclid's book could serve as *the* mathematical foundations for generations is certainly left in the past. Sheaving gives a hint how a more advance educational structure may look like: think of "sheaf of applications" and its base as foundations serving as a guide for continuous shifting between the applications.

7. Conclusion: hermeneutic mathematisation versus logical formalisation

Throughout the history mathematics and philosophy were inspired by each other: think, for example about late Plato's account of Forms as numbers or Descartes' notion of method tightly connected to his own work in the algebraisation of geometry, or Brouwer's intuitionism. There are two kind of danger in this kind of work which I tried to avoid: of merely metaphorical use of mathematical terminology, and of replacing philosophical work by popularisation of mathematical ideas. I didn't intend to write a paper in the philosophy of mathematics which would explain the nature of mathematical objects and the place of mathematics among other disciplines. However in this Conclusion I would like to say a few words on relationships between philosophy, mathematics and logic.

A reason behind the interest of the philosophy of 20th century, and particularly Analytic philosophy, in formal logic was the need to distinguish philosophy as a science from philosophically laden literature fiction, political discourse and other philosophically-related activities. This was needed at least for the following two purposes: (i) to create an international scientific standard making possible the philosophical discussion between philosophers with different cultural and political backgrounds, and (ii) to bridge the gap between philosophy, on the one hand, and natural sciences and mathematics, on the other hand, appeared in 19th century, particularly in the German Idealism. The use of formal logical calculi in philosophy was supposed to serve both these purposes.

Such formalisation doesn't sweep the informal argumentation out of philosophy but helps to fix the most important points of given philosophical discourse. Like physics or mathematics philosophy gets a formal kernel invariant under translations between and paraphrasing within natural languages and independent of individual expressive styles of particular philosophers. It should be noticed that the formalisation doesn't diminish but on the contrary reinforces the role of individual style in any particular occasion just like the fact that all piano players use the same 12 tones makes individual qualities of each player only more important..

I recognise the importance of both aforementioned purposes but I don't agree about the means. Today we can, and I believe, also should make it differently. Let's consider purpose (ii) first. As it has been often noticed both by proponents and opponents of formal methods in philosophy a historical predecessor of the formal philosophy of 20th century is the Scholasticism. A sound historical account of how the Scholastic tradition which survives until today in Catholic philosophy and theology influenced the Analytic philosophy of 20th century is yet wanted but examples of people like Brentano make it clear that the proximity between the two intellectual movements is more than a matter of an anachronistic speculation. I'm not prejudged against the Scholasticism but I take quite seriously the fact that Modern philosophy and science emerged in 17th century through a sharp divergence with the Scholasticism. Points of the divergence are many but I would like to stress only one: while Scholasticism relied on the Aristotelian logic as the universal formal framework the new science used mathematics. Historically the latter approach comes down to the Platonic tradition revived in Europe during the Renaissance.

In the Analytic philosophy of 20th century the Aristotelian logic was exchanged by another kind of logic closer linked with mathematics. However this didn't change the principle according to which logic and metaphysics provide an universal framework for sciences. Attempts to apply this approach to the contemporary science straightforwardly made in the logical positivism were given up in 1930ies and after this Analytic logico-philosophical studies dealt mostly with everyday language rather than science and mathematics. During last few decades a lot of significant efforts have been made for bridging the Analytic philosophy with natural sciences. But there were no similar efforts made with respect to the contemporary mathematics.

As far as mathematical matters are concerned the Analytic philosophy seems to be happy with its own contribution to mathematics, namely ZFC and alternative logical foundations of mathematics. However significant this joint work of philosophers and mathematicians might be the general point about the conflict between the philosophical logicism, on the one hand, and modern mathematics and science, on the other hand, applies to ZFC as well. Remark that to the contrary to what Cantor envisaged for the future of the set theory ZFC doesn't have and apparently cannot have any application in empirical sciences. As I have already said, it plays no other role in the rest of mathematics but that of its official foundation.

The problem of formal logical approaches to the contemporary science and mathematics is not merely a technical problem. I suppose that this problem originates in the divergence between logic and metaphysics, one the one hand, and science and mathematics, on the other hand, occurred in the early modern times. This doesn't mean, of course, that logic and metaphysics should be simply given up and replaced by science and mathematics. Already Leibniz made a very valuable attempt to reconcile the conflicting parties, and further attempts in this direction perhaps could be interesting. However I see no reason to revise the view on the emergence of the modern science in the 17th century as a breakthrough in the Western intellectual tradition, and to restore the old-fashioned logicism in a new appearance. The modern science not only shows no sign of declination, but through technology it became an indispensable element of today's global civilisation. Another breakthrough in science which occurred in the beginning of 20th century reinforced but not delimited such principle features of the modern science as relativisation and mathematisation of physical concepts. This gives reason to think that if the pre-modern logical and metaphysical tradition should be preserved in some form it should be transformed according to modern scientific principles rather than these principles be dispensed with in favour of the pre-modern views. Speaking in 4.3 about the notion of logical variable assumed in the contemporary logic I have shown in which sense this notion is Aristotelian rather than Galilean. So there remains a lot of work to be done to allow the modern logic to deserve its name.

Thus logical formalisation of philosophy doesn't achieve purpose (ii). It makes philosophy look like a science but this philosophical science remains quite detached from modern mathematised sciences. A further mathematisation of logic, in particular through the categorification, is a reasonable modernising strategy. However mathematics should be also wider involved in philosophical studies directly. I don't mean to turn philosophy into mathematics or mathematics into philosophy but to make a bridge between the two disciplines like one I have tried to make in this paper. This better serves for the purpose (ii) but also serves for the purpose (i) since as a matter of fact mathematics translates through cultural and political barriers like logic and probably better.

There is a more specific point about (i) which I would like to stress here. The idea to provide philosophers with a standard formal language with certain criteria of soundness allows indeed for making an international and intercultural philosophical community a reality. However it is not unique solution and, as I believe, it is a poor solution. A better albeit much more costly solution is to integrate different philosophical traditions through mutual interpretations. A formal reason why the latter solution is better is that like in the cases of translations between natural languages or functions between sets there is no slightest reason to expect that philosophical interpretations are reversible, and hence can be harmlessly dispensed with. An informal reason is that philosophers like any other people usually don't like to give up their local discourses in favour of what they often regard as just another local discourse which attempts to make itself into global one without respecting others.

The hermeneutic integration strategy is not new in philosophy, of course. But the importance of the notion of interpretation in mathematics recognised only relatively recently, and the power of the category theory as a formal means of interpretation show indeed something new, namely that the traditional notion of philosophical interpretation doesn't fall that far from mathematics and modern mathematised sciences as the earlier hermeneutic tradition assumed. This gives me reason to believe that the hermeneutic mathematisation is a more promising strategy in philosophy than logical formalisation turning philosophy into the new Scholasticism. This concluding remarks don't replace a wider discussion on philosophical logicism and formalism, and on the Scholastic heritage, but they challenge the dominant view and show an alternative.

Literature:

1. Aristotle, *Metaphysics*
2. Awodey, S., 1996, *Structure in Mathematics and Logic: A Categorical Perspective*, in: Philosophia Mathematica (3), N4, pp. 209-237
3. Awodey, S., 2004, *An Answer to Hellman's Question: Does Category Theory Provide a Framework for Mathematical Structuralism?*, in: Philosophia Mathematica (3), N12, pp. 54-64
4. Bell, J., 1986, *From Absolute to Local Mathematics*, in: Synthese 69 (3), pp. 409-426
5. Beltrami, E., 1869, *Saggio di interpetrazione della geometria non-euclidea*, in: Giornale di Matematiche, v.6, pp. 284-312; republished in: Opere Matematiche t.1 Milano: Ulrico Hoepli 1902, pp. 374-405
6. Benabou, J., 1985, *Fibred Categories and the foundations of naive category theory*, in: The Journal of Symbolic Logic, 50, pp. 10-37
7. Bonola, R., 1908, *Die nichteuklidische Geometrie*, Teubner
8. Boole, G., 1854, *An Investigation of the Laws of Thought on which are Founded the Mathematical Theories of Logic and Probabilities* MacMillan, Cambridge and Walton Maberly, London
9. Boole, G., 1997, *Selected Manuscripts on Logic and its Philosophy* (Eds. Grattan-Guiness I. and Bornet G.), Birkhaeuser (Science networks; vol.20)
10. Boyer, C.B., 1968, *A history of mathematics* John Wiley & Sons
11. Bueler, W.K., 1981, *Gauss: a biographical study* Berlin, NY: Springer-Verlag
12. Bunge, M., , 1984, *Toposes in Logic and Logic in Toposes*, in: Topoi, 3, N1, pp.13-22

13. Cantor, G., , 1872, *Ueber die Ausdehnung eines Satzes aus der Theorie der trigonometrischen Reichen*; in: Math. Ann., Bd.5, S.123-132

14. Cantor, G., 1879-1880-1882-1883 (a)- 1884 (a), *Ueber unendliche lineare Punktmannigfaltigkeiten*, N1: Math. Ann., Bd.15, S. 1-7; N2: Math. Ann., Bd.17, S. 355-358; N3: Math. Ann., Bd.20, S. 113-121; N4: Math. Ann., Bd.21, S. 51-58; N6: Math. Ann., Bd.23, S. 453-488

15. Cantor, G., 1883 (b), *Grundlagen einer allgemeinen Mannigfaltigkeitslehere: Ein mathematisch-philosophischer Versuch in der Lehre de Unendlichen*, Teubner

16. Cantor, G., 1883 (c), *Sur divers théorémes de la théorie des ensembles de points situés dns un espace continu à n dimension. Première communication*, in: Acta math., vol.2, pp.409-414

17. Cantor, G., 1884 (b), *De la puissance des ensembles parfaits de points*, in: Acta math., vol.4, pp.381-392

18. Cantor, G., 1885, *Ueber verschiedene Theoreme aus der Theorie der Punktmengen in einem n-fach ausgedehnten stetigen Raume G_n. Zweite Mitteilung*, in: Acta math., vol.7, pp.105-124

19. Cantor, G., 1895-1897, *Beitraege zur Begruendung der transfiniten Mengenlehre*, in: Math. Ann., Bd.46, S. 481-512; Bd.49, S. 207-246

20. Cantor, G., *Principien einer Theorie der Ordungstypen*, in: Acta math., 1970, vol. 124, pp.83-101 (in Grattan-Guinness 1970).

21. Carnap, R. and Bachmann, F., 1963, *Ueber Extremalaxiome*, in: Erkenntnis 6, pp. 166-188

22. Cartier, P., 2001, *A Mad Day's Work: From Grothendieck to Connes and Kontsevich. The evolution of concepts of space and symmetry*, in: Bulletin of the American Mathematical Society; v.38 N4, pp.389-408

23. Euclid, *Elements*

24. Fourman, M., 1977, *The Logic of Topoi*, in: :Barwise J. (ed.) The Handbook of Mathematical Logic , North-Holland

25. Frege, G., 1884, *Die Grundlagen der Arithmetik* , Breslau

26. Frege, G., 1879, *Begriffssrift*, Halle: Verlag von Louis Nebert

27. Frege, G., 1971, *On the Foundations of Geometry and Formal Theories of Arithmetic*, E.H.W. Kluge (ed.) New Haven, London; Yale University Press

28. Frege, G., 1980, *Philosophical and Mathematical Correspondence*, Oxford: Basil Blackwell

29. French, S. and Crause, D., 2004, *Identity in Physics: A Historical, Philosophical, and Formal Analysis* , Leeds &Florianopolis (unpublished manuscript)

30. Gauss, C.F., 1827, *Disquisitiones generales circa superficies curvas* , Goettingen

31. Gelfand ,S.I., Manin, Yu.I., 2003, *Methods of Homological Algebra* , Springer-Verlag Berlin-Heidelberg (Second edition)

32. Grattan-Guinness, I., 1970, *An unpublished paper of Georg Cantor*: *Principien einer Theorie der Ordungstypen* , in: Acta math., vol. 124, pp.65-107

33. Grothendieck, A., 1971, *Catégories fibrée et descente*, (*SGA 1*, *exposé 6*): Lecture Notes in Mathematics, 224, Springer-Verlag, Berlin-Heidelberg

34. Grothendieck, A., Artin, M. and Verdier, J.-L., 1977, *Théorie des topos et cohomologie étale de schémas* (*SGA 4 v.1*): Lecture Notes in Mathematics, 224, Springer-Verlag, Berlin-Heidelberg

35. Hausdorff, F., 1914, *Grundzuege der Mengenlehre*, Leipzig: Veit

36. Halle, B., 1996, *Structuralism's Unpaid Epistemological Debts*, in: Philosophia Mathematica (3), N4, pp. 124-143

37. Hellman, G., 2003, *Does Category Theory Provide a Framework for Mathematical Structuralism?* , in: Philosophia Mathematica (3), N11, pp. 129-157

38. Hilbert, D., 1899, *Grundlagen der Geometrie*, Leipzig, Teubner

39. Hilbert, D., 1904, *Ueber die Grundlagen der Logik und Arithmetik*, in: Congress of Mathematics, Heidelberg, pp.174-185

40. Hyting, A., 1956, *Intuitionism. An Introduction.* Amsterdam: North-Holland

41. Johnstone, P., 1977, *Topos Theory*, LMS Mathematical Monographs 10, Academic Press

42. Johnstone, P., 1983,*The Point of Pointless Topology*, Bulletin of American Mathematical Society 8(1), pp. 41-53

43. Klein, F., 1872, *Vergleichende Betrachtungen ueber neuere geometrische Forschungen*, Erlangen

44. Kock, A., Reyes, G., 1977, *Doctrines in Categorical Logic*, in: Barwise J. (ed.) The Handbook of Mathematical Logic , North-Holland

45. Landry, E. and Marquis, J.-P., 2005, *Categories in Context: Historical, Foundational, and Philosophical*, in: Philosophia Mathematica (3), N13, pp. 1-43

46. Lawvere, F.W., 1963, *Functorial Semantics of Algebraic Theories*, Ph.D. Thesis (Columbia University); reprinted with Author's Comments by Buffalo Workshop Press POB 171, 2004

47. Lawvere, F.W., 1966, *The Category of Categories as a Foundation of Mathematics*, La Jolla Conference on Categorical Algebra, Springer-Verlag, pp.1-20

48. Lawvere, F.W., 1969, *Adjointness in Foundations*, in: Dialectica 23, N 3/4

49. Lawvere, F.W., 1975, *Continuously variable sets: algebraic geometry = geometric logic*, in: Logic Colloquium 73, Bristol, pp.135-156

50. Lawvere, F.W., 1976, *Variable Quantities and Variable Structures in Topoi*, in: Algebra, Topology, and Category Theory, NY: Academic Press, pp. 101-131

51. Lawvere, F.W., 1994, *Cohesive Toposes and Cantor's "lauter Einsen"*, in: Philosophia Mathematica, vol.2, pp.5-15

52. Lawvere, F.W., 2003, *Foundations and Applications: Axiomatization and Education*, in: The Bulletin of Symbolic Logic vol.9 N2 (June) pp. 213-224

53. Lawvere, F.W. and Rosebrugh, R., 2003, *Sets for Mathematics*, Cambridge University Press

54. Lewis, D., 2000, *On plurality of worlds* , Blackwell

55. Lobatcheffky, N.I., 1837, *Géometrie Imaginaire*, in: Journal fuer die reine und angewandte Mathematik N17

56. Macintyre, A., 2003, *Model Theory: Geometrical and Set-Theoretic Aspects and Prospects*, in: The Bulletin of Symbolic Logic vol.9 N2 (June); pp. 197-212

57. MacLane, S., 1975, *Sets, Topoi, and Internal Logic in Categories*, in: Logic Colloquium 73, Bristol, pp.119-134

58. MacLane, S., 1996, *Structure in Mathematics. Mathematical Structuralism*, in: Philosophia Mathematica (3), N4, pp. 174-183

59. MacLarty, C., 1991, *Axiomatizing a Category of Categories*, in: The Journal of Symbolic Logic vol.56, N4 (December)

60. MacLarty, C., 1992, *Elementary Categories, Elementary Toposes*, Oxford University Press

61. MacLarty, C., 2004, *Exploring Categorical Structuralism*, in: Philosophia Mathematica (3), N12, pp.37-53

62. MacLarty, C., 2005, *Learning from Questions on Categorical Foundations*, in: Philosophia Mathematica (3), N13, pp. 44-60

63. Makkai, M. and Reyes E., 1977, *First Order Categorical Logic*, Springer-Verlag: Berlin, Heidelberg, NewYork (Lecture Notes in Mathematics 611)

64. Pash, M., 1882, *Vorlesungen ueber neuere Geometrie*, Teubner-Verlag

65. Porphirius, *Isagoge*

66. Plato, *Timaeos*

67. Proclus, D., 1873, *Procli Diadochi in primum Euclidis elementorum librum Commentarioum* ed. Friedlein, Lepzig

68. Quine, W.V.O., 1960, *Word and Object*, MIT Press

69. Reichenbach, H., 1928, *Philosophie der Raum-Zeit-Lehre* Berlin: Walter de Gruyter

70. Riemann, B., 1854, *Ueber die Hypothesen, welche der Geometrie zu Grunde liegen (Habilitationsvortrag)*, reprinted in *Gesammelte Mathematische Werke* (Narasimhan, ed.), Berlin, Heidelberg, NewYork: Springer-Verlag, 1990

71. Rodin, A., forthcoming, *Identity and Categorification* , Philosophia Scientiae

72. Rodin, A., 2003, *Математика Евклида в свете философии Платона и Аристотеля* Москва: Наука

73. Russell, B., 1903, *The Principles of Mathematics*, London: Geogre Allen & Unwin Ltd.

74. Shapiro, S., 1991, *Foundations without foundationalism* Oxford: Clarendon Press

75. Skolem, T., 1958, *Une relativisation des notions mathematiques fondamentales*, in: Colloques Internationaux du CNRS, pp.13-18

76. Tannery, J., *Revue*, Bulletin des sciences mathématiques et astronomiques (2)8, 1884, pt. 2, pp.162-171

77. Vickers, S., 1989, *Topology via Logic*, Cambridge University Press (Cambridge Tracts in Theoretical Computer Science 5)

78. Warden, Van der, 1930, *Modern Algebra* Berlin: Springer-Verlag

79. Weyl, H., 1949, *Philosophy of Mathematics and Natural Science*, Princeton: Princeton University Press