

Abstract Mathematical Tools and Machines for Mathematics†

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One of the characteristic features of twentieth-century mathematics is the emergence of abstract mathematical tools, instruments and machines for use in mathematics. Unfortunately, this feature has been totally ignored, even denied, by philosophers. To wit:

A crucial part of the practice of empirical science is constructing means of access to (many of) the objects that constitute the subject matter of that science. Certainly this is true of theoretical objects such as subatomic particles, black holes, genes, and so on. However analogous theoretical work in higher set theory may seem to be to theoretical work in the empirical sciences, this disanalogy remains. Empirical scientists attempt to interact with most of the theoretical objects they deal with, and it is almost never a trivial matter to do so. *Scientific theory and engineering know-how are invariably engaged in such attempts*, which are often ambitious and expensive. *Nothing like this seems to be involved in mathematics.* (Azzouni [1994], p. 5) [Emphasis mine.]

Our main purpose here is to show that there *are* tools and machines in mathematics and that mathematical ‘theory and engineering know-how are invariably engaged in... attempts’ to know better the theoretical objects they deal with. Furthermore, we believe that this simple fact has important philosophical implications and even offers interesting philosophical challenges. If all we achieve is to attract attention to the issues involved, we will have attained our goal.

Abstract Tools and Machines: Setting the Stage

Mathematicians (almost) *literally* talk about instruments. They use this expression in its primary sense: the instruments are certain constructions

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applied to certain 'objects' in a given context in order to obtain useful information. The analogy with instruments in the empirical or factual sciences is surprisingly close. It is a trivial fact that contemporary science depends heavily on experimentation, which in turn depends heavily on instrumentation. Science and technology join efforts in their cognitive enterprise. In a way, mathematicians are nowadays in a similar position, at least in some fields: there is now a vast body of mathematical instruments which are used as tools to demonstrate certain results. Just as our evaluation of the acceptability of a result in the empirical sciences depends crucially on the instruments used and on our knowledge of the way they work and how they were used, so too in the case of mathematics, a result will be seen as a breakthrough if the community (of mathematicians) is convinced that they understand the tools used and that they can verify that they were used properly. Thus, mathematical progress also largely depends on 'conceptual technological' innovations.¹

Here is how a Fields Medalist presents the situation in an informal discussion:

I think one has to be quite specific, and say that there are two different attitudes that a mathematician has towards mathematics. And one of them is, of course, he has to create tools, exactly as Watson and Crick had to have the electronic microscope, in order to observe things. So a mathematician has to create tools, and he does a work of creation at this point. On the other hand, until a mathematical tool has been used to unveil a part of this mathematical reality that I am talking about, it is not really accepted by the mathematical community. It is considered, but it is not really accepted until it has been successfully used. This is something which is labelled a 'breakthrough', and we understand what that is. So I think it is a mistake to talk only about creation. I think there are the two aspects, and these two aspects are fairly clearly separate when talking about physics, or when talking about biology. However, in mathematics, they are more mixed together, and are more difficult to separate, yet they are present. (Connes [1992], p. 99)²

There are many issues in this fascinating passage. Let us consider them individually.

1. What are the tools created by the mathematicians Connes is referring to? Here is a preliminary list of obvious candidates: spectral sequences, homology theories, cohomology theories, homotopy theories, K -theories, sheaves, schemes,³ representation theory and character theory, commuta-

¹ Even though I will emphasize the similarities between the two cases, I should immediately point out that my goal is *not* to show that mathematics proceeds like the natural sciences. In fact, it does not. I *do* believe that the analogy is very fruitful in this case, but it is at bottom only that: an analogy.

² As was pointed out to me by Colin McLarty, in fact Watson and Crick used X-ray crystallography, not an electron microscope.

³ This is a fascinating case: Weil's conjectures, proved by Deligne in the early 70's

tive algebra, graph theory in group theory, classical geometry in algebra, especially field theory, model theory in algebra, in combinatorics, and in analysis. Before we go any further, two important comments have to be made about this enumeration.

a. The tools in the list do not *merely* refer to interactions between disciplines, e.g., analysis and number theory. The claim is not that results from one theory can sometimes be used in a different theory and thus are 'tools'. This would be a rather trivial claim which would not be of much interest. The presence of tools becomes obvious only when we look at *how* a discipline is related to another.

b. It is far from clear that even the above somewhat restricted list is *uniform*, in the sense that the theories mentioned constitute tools in the same way, e.g., compare homology theory in algebra to model theory in algebra. It is not clear that mathematicians and logicians would put them on the same footing.

2. '...until a mathematical tool has been used to unveil a part of this mathematical reality that I am talking about, it is not really accepted by the mathematical community. It is considered, but it is not really accepted until it has been successfully used.' There are at least three elements here, one ontological and three epistemological.

a. The ontological claim is that mathematics is divided into two distinct *ontological* parts:⁴ 'real' mathematics and the mathematical instruments which were invented to study or to understand this 'reality' better. Just as the natural sciences divide the (material) world into (at least) two fundamentally different categories of things, namely artefacts, i.e., constructed instruments, and natural kinds, things which are 'given' or exist independently of us, so too mathematics is divided into two similarly distinct categories of entities, namely artefacts, i.e., instruments which were constructed and therefore possess properties which reflect some of our limitations, and 'natural kinds', entities which presumably exist as such independently of us.

and Mordell's conjectures, proved by Faltings in the 80's, were proved with the help of scheme-theoretic tools. Again, this is a case where a tool, or some would prefer to say a 'language', allows an extremely powerful unification, a phenomenon which is not without ontological implications.

⁴ This is not without reminding us of Hilbert's distinction between real propositions and proofs and ideal propositions and proofs. It is therefore tempting to say that the tools we are referring to are similar or even particular cases of Hilbert's ideal elements. However, the tools we are referring to here are more on the side of the ideal elements Hilbert was referring to as an analogy with the ideal elements he was introducing, even though there are noticeable differences also. We will not enter the debate as to whether Hilbert was an instrumentalist or not, or whether even these ideal elements were just as real as the 'real' elements. See Detlefsen [1986] and Hallett [1989] for different interpretations on these issues.

Epistemologically, we have the following facts:

- b. Before a tool is accepted by the community it has to be 'successful', which can mean various things, for instance, the tool does something which was impossible before or greatly simplifies previous results;
- c. A tool has to be used in a specific way, and thus comes with certain norms;
- d. Mathematics is divided into two distinct *epistemological* parts: roughly speaking, just as in the natural sciences there are theories about instruments and their use and theories about natural kinds and their properties, so too in mathematics there are theories about instruments and their use and theories about natural kinds and their properties.

Connes does not carefully distinguish the ontological element from the epistemological ones in his claim. We believe they should be, for, in a sense, they are largely independent of one another. Indeed, the ontological separation could well be inadequate and the epistemological separation could nevertheless be correct. This should be particularly clear if we consider the weight these distinctions carry in the context of the natural sciences. It should be obvious that no matter what ontological status we attribute to entities in general in this context, artefacts are just as real or unreal as natural kinds. Indeed, it is crucial that they be related ontologically, that they share some ontological 'stuff' and that the properties of the artefacts be 'derived' from the properties of the natural kinds. The only difference here is that artefacts would not exist without us, whereas natural kinds presumably would, or at least could, come up 'naturally'.

It should be just as obvious that artefacts and natural kinds differ nonetheless considerably from the epistemological point of view. The *main* property of an artefact is its function, and this depends partly on us. Thus, in the context of the natural sciences, the distinction between artefacts and natural kinds seems at first ontologically pointless but epistemologically crucial.

We will now proceed as follows. We will first take a rather quick look at what we take to be a generic case of the tools above, namely *K*-theory. Our main objective here is to show that it is a tool and try to understand its nature. We will afterwards compare the case of *K*-theory with a different type of tool, namely notational systems. We hope that this will indicate, among other things, how different tools can be in mathematics.

Before we take a close look at our generic tool it is useful to make some remarks about tools in general, so that we can see why it is legitimate to call these theories tools and how they differ from tools in the natural sciences and other tools in mathematics.

Tools in General and in Mathematics: Some Remarks

What is a tool in mathematics for mathematics? What is a tool in general? Let us start by listing what we take to be the most fundamental properties of material tools.

- i) tools are *built* or at the very least, '*prepared*'; what this means is that tools are not given as such; either they are made up or we take something given and modify it so that it can serve as a tool;
- ii) tools act *on* something or are acted upon by something; so we have to have first a collection of objects which we want to investigate; whether these objects or the collection of objects is '*real*' or not is undecided at this stage;
- iii) tools '*interact*' with the objects under study; this '*interaction*' can transform the given objects or it can leave them more or less as they are, *i.e.*, some are invasive, others are non-invasive;
- iv) this '*interaction*' is usually *planned* to some extent and it usually reveals a property of the object or phenomenon under study, or its absence;
- v) hence, tools have a *function*; they are used with a certain goal in mind, either to construct something or to repair something or to make a diagnosis;
- vi) in order to know whether this goal has been achieved, a *norm* has to be applied, telling us whether the tool was efficient or not.

Clearly, to talk about mathematical tools is to make a metaphor: literally speaking, a tool is a physical or concrete object created for the execution of some task. More often than not, it has to be fixed, repaired, tinkered with. None of our examples of mathematical tools are physical objects and none '*breaks down*' or suffers as a result of power failure. Hence, even though our mathematical tools can be inappropriate, none can be said to be dysfunctional.⁵

Thus, the tools we are considering here are abstract objects and procedures. But, perhaps surprisingly, this is the *main* difference between our class of mathematical tools and concrete tools. As we will try to show, they possess all the other properties in the above list, even though some of them have to be somewhat modified, *e.g.*, clause ii). This similarity with concrete tools will in fact help us to distinguish these tools from other mathematical tools, even though we cannot claim that we can provide in this way a satisfactory classification of tools.

However, in order to guide us through the following steps, we should im-

⁵ Nowadays, only computers, given their role in mathematics, can literally be called tools for mathematicians and again in a somewhat different sense. For computers do not interact with mathematical objects, whatever that could mean, but with data or inputs representing mathematical objects or properties. Computers, however, *do* transform these data and *do* have a goal. What we still lack in this case is a *norm* of evaluation, so that we can decide what type of tool we are dealing with; *e.g.*, do we really have a proof of the four-color conjecture?

mediately point out that most of the tools we have in mind here should be thought of as *machines* rather than as tools.⁶ As we will try to show in the next section, this is particularly clear—to mention just a few examples—in the case of spectral sequences, homology and cohomology theories, homotopy theory, K -theory, and sheaves and schemes. It would be particularly interesting, as we plan to do in a forthcoming paper, to compare the latter with, say, character theory, which plays such an important role in contemporary mathematics. Furthermore, many of these machines really ‘take off’ when they are used together, e.g., sheaf theory with homological algebra, K -theory with cohomology. It is for these machines that Connes’s analogy with the electron microscope is especially appealing. Let us now take a brief look at a particularly telling case of these machines, namely K -theory.

A Prototypical Tool: K -theory

In orthodox algebraic topology one breaks a space down into atomic parts (cohomology or homotopy) and then tries to see how the atoms were put together (operations or k -invariants). Now many of the spaces that turn up in practice, e.g., homogeneous spaces, have such a regular structure that it is a pity to break them right down. Instead, like the biochemist, we should look around for large standard molecules out of which these spaces are built. It would seem that the linear groups provide such standard molecules and that this, in a philosophical sense, explains the success of K in homotopy theory. (Atiyah 1962 [1988], p. 294)

The functor denoted by ‘ K ’, which stand for the German word *Klasse*, was created in the fifties by A. Grothendieck to prove a generalized version of the Riemann-Roch theorem in algebraic geometry. (See, e.g., Dieudonné [1989], p. 599.) His construction was quickly adapted to the topological context by Atiyah and Hirzebruch who then created, almost in a single stroke, topological K -theory. From then on this theory has been developed, generalized and applied in various ways. Very roughly, K -theory stipulates how to associate certain abelian groups (and sometimes rings), denoted by K^i or K_i for some i , to a given class of objects, i.e., topological spaces or rings, and ways of studying these groups. Let us now try to see how this is done and why it is so useful. We will present in steps what K -theory is and then look at some specific applications.

The first thing to consider is what is called the Grothendieck group of an abelian semigroup. Given an abelian semigroup M , for instance the natural numbers \mathbb{N} with addition, its Grothendieck group $F(M)$ can be constructed in various equivalent ways. Here is one standard construction. Take the product $M \times M$ and then form the quotient under the equivalence relation

$$(m, n) \approx (m', n') \Leftrightarrow \exists p \text{ such that } m + n' + p = n + m' + p.$$

⁶ ‘One might say that Algebra is a machine for solving certain types of problem but that abstract Algebra is a machine for making machines’ (Atiyah 1977 [1987], p. 269).

This quotient yields the Grothendieck group $F(M)$ of M . There is a natural semigroup homomorphism ϕ from the abelian semigroup M to the Grothendieck group $F(M)$.

It is easy to verify that when $M = \mathbb{N}$, the natural numbers, then $F(M) \cong \mathbb{Z}$, the ring of integers, and this is indeed a familiar way to construct the integers out of the natural numbers, except that in the case of \mathbb{N} , there is no need for a p in the definition of the equivalence relation because \mathbb{N} satisfies the cancellation law. In general, the semigroup is not embedded in the constructed group. The latter happens only when the cancellation law already holds in the semigroup. Indeed, consider the abelian semigroup (\mathbb{Z}, \times) , consisting of the integers under multiplication. In this case, $F(\mathbb{Z}) \cong 0$, the trivial group with one element.⁷

The foregoing construction is functorial and associates to any abelian semigroup a commutative group ‘in the best possible way’. This means that for any group G and homomorphism $\theta : M \rightarrow G$ there is a unique homomorphism $\psi : F(M) \rightarrow G$ such that $\psi\phi = \theta$.

The motivation behind this construction is eminently practical, for ‘semigroups without cancellation are usually very hard to handle; yet in many cases their Grothendieck groups are fairly tractable’ (Rosenberg [1994], p. 5). Indeed, K -theory is based on this passage from semigroups to groups and is so useful because some problems become tractable.

Let us now move to elementary topological K -theory and see how and when the above construction comes in. We start with a category of compact spaces with continuous functions between them. These spaces are our object of study in this case.⁸ They are what is given to us, the ‘reality’ we are trying to understand. Our goal is to obtain some information about these spaces in a uniform manner, so that we can classify them in some way, or have a better understanding of their structure. This can be done in many ways, and in fact constitutes a large portion of algebraic topology. One tries to associate algebraic structures to spaces in a uniform way. In the case of K -theory, the first step associates to each space X and continuous function f a commutative semigroup and semigroup homomorphism in a uniform manner so that the latter ‘captures’ some of the interesting properties of these spaces. In other words, we ‘translate’ topological properties into algebraic properties. Once this is accomplished, we apply the above functor to the commutative semigroups to obtain groups, denoted by $K(X)$ for a

⁷ This is in fact a general phenomenon: it suffices that the monoid has an element ∞ such that for any other $m \in M$, $m + \infty = \infty$. In the case of the multiplicative monoid of integers, this element is 0.

⁸ These spaces are worthy of interest, for the set of solutions in projective space of an equation or a set of equations in many variables constitute such a space. It turns out that some of the qualitative properties of this space reflect important properties of the equations themselves, in particular of their solutions.

given X , which represent in an algebraic manner and in a context where we can handle these representations 'effectively',—i.e., we are doing linear algebra—some of the topological properties of the spaces. As we will see, it is this information that allows us to obtain various important results about the structure of these spaces. This is basically the same strategy which is used in algebraic K -theory, even though in this case the construction of the higher K -groups is not as direct as in the topological case.⁹ In algebraic K -theory, one starts with the category of rings and ring homomorphisms, then associates to each ring a commutative semigroup and to each ring homomorphism a semigroup homomorphism, then applies the above functor to end up in the category of abelian groups and group homomorphisms, and thus obtains information about some of the structural properties of these rings. Let us now try to make this process more specific, again in the case of topological K -theory.

We are given, say, the category of compact spaces and continuous maps between them. The first step in the construction is to associate in a uniform manner an abelian semigroup to each space. This is done with the help of vector bundles.¹⁰ Roughly speaking, a vector bundle over a space X is a continuous family of vector spaces parametrized by the points of X . It is formally defined as follows. Let X be a topological space.

First, a *family of vector spaces* over X is a topological space E together with:

- i) a continuous surjective map $p : E \rightarrow X$;
- ii) a real vector space structure of finite dimension in each $E_x = p^{-1}(x)$, $x \in X$ compatible with the topology on E_x induced from E .

Given two families of vector spaces over X , $p : E \rightarrow X$ and $q : F \rightarrow X$, a morphism $\phi : E \rightarrow F$ is a continuous map such that

- i) $q\phi = p$;
- ii) for each $x \in X$, $\phi : E_x \rightarrow F_x$ is a linear map of vector spaces.

Whenever ϕ is bijective and ϕ^{-1} is continuous, ϕ is said to be an isomorphism.

Let V be a finite-dimensional real vector space. Then $E = X \times V$ with $p : E \rightarrow X$, the projection onto the first factor, is a family of vector spaces over X , called the product family with fiber V . If F is a family which is isomorphic to a product family, then F is said to be *trivial*.

⁹ It is interesting to note some of the differences existing in the development of topological K -theory and algebraic K -theory. In the topological context, all the higher K -groups were defined early on by Atiyah and Hirzebruch [1961]—see Atiyah [1988]—but the road was considerably steeper for the higher algebraic K -groups. It took almost 10 years before Quillen finally convinced everyone that he had found the right definition. See Quillen [1974]. We should also mention that there are some differences between real K -theory and complex K -theory, the latter being somewhat easier to handle in general.

¹⁰ We should here emphasize that vector bundles were not introduced for that purpose. They were introduced as early as 1935 by Whitney in the context of differential geometry.

Let Y be a subspace of X and $p : E \rightarrow X$ a family of vector spaces over X . Then the map $p^{-1}(Y) \rightarrow Y$, called the *restriction* of E to Y and denoted by $E|_Y$, is a family of vector spaces over Y .

A *vector bundle with base X* is a family of vector spaces over X such that each $x \in X$ possesses a neighborhood U such that $E|_U$ is trivial. Thus, a vector bundle over X is a family of vector spaces which is locally trivial.

Here are two standard examples. First the trivial bundle $p : X \times \mathbb{R}^n \rightarrow X$, where p is the projection on the first factor. For the second example, let X be the 2-sphere $S^2 = \{x \in \mathbb{R}^{2+1} : \|x\| = 1\}$, $\|\cdot\|$ being the usual norm. For every point x of S^2 we choose E_x to be the real vector space orthogonal to x . Then $E = \cup E_x$, the union being disjoint, is naturally a subspace of $S^2 \times \mathbb{R}^3$, and together with the induced topology and the appropriate projection is the tangent bundle of the sphere. The latter example easily generalizes to any dimension n . Note that the tangent bundle of the sphere of dimension greater than or equal to 2 is *not* a trivial family. If it were, then there could be an everywhere non-zero vector field on the sphere, contradicting the infamous 'hairy-ball' theorem.

It is easy to verify that vector bundles over X form a category. Moreover, and this is now the crucial fact, it is possible to define standard algebraic operations on vector bundles. The most important for our purpose is the so-called *Whitney sum*: if $p : E \rightarrow X$ and $q : F \rightarrow X$ are two vector bundles over X , their Whitney sum is defined by

$$E \oplus F = \{(x, y) : x \in E, y \in F, p(x) = q(y)\}.$$

We can 'picture' this sum in the following way: over any point x of X , the vector space $(E \oplus F)_x$ combines a copy of E_x with one of F_x 'orthogonal' to it. This sum satisfies the following properties: $E \oplus (F \oplus G) \cong (E \oplus F) \oplus G$ and $E \oplus F \cong F \oplus E$.

The set of *isomorphism classes* of the vector bundles over a space X , denoted by $\text{Vect}(X)$, is an abelian semigroup. One can therefore apply the above functor and obtain an abelian group, called the K -theory $K(X)$, or, for reasons we will mention in a short while, $K^0(X)$, of the space X . Thus, roughly speaking (and very roughly, for it is not quite true), the computation of $K^0(X)$ amounts to the computation of the isomorphism classes of vector bundles over X . This constitutes an important item of information about X . For instance, and this is the simplest case, for a one-point space P it is easy to show that $K^0(P)$ is isomorphic to the integers.¹¹ Indeed, a vector bundle over P is merely a finite-dimensional vector space, and so

¹¹ For K_0 , the algebraic construction runs in parallel. Given a ring R , one constructs the appropriate semigroup by taking the isomorphism classes of finitely generated projective modules over R and then applies the functor K . It is easy to see that in this case, when the ring R is in fact a field, then the finitely generated projective modules over it are simply the finitely generated vector spaces over R and the only isomorphism

is determined up to isomorphism by its dimension. Thus, the dimensions are the isomorphism invariants of $\text{Vect}(P)$ and hence $\text{Vect}(P) \cong \mathbb{N}$ and $K(P) \cong \mathbb{Z}$.

Again, this last example suggests that the semigroup can be embedded in the group. For in the case of a one-point space, two vector bundles, E and F , will be equal in $K(P)$ if and only if they are isomorphic. The general case is somewhat different, and this is why $K^0(X)$ does not quite reflect the isomorphism classes of vector bundles over X . In fact, it reflects the relation of being stably equivalent, which we now briefly describe. Let us denote by T_n the trivial bundle of rank n over X , where the rank of a trivial bundle is simply the dimension of the fibers of the bundle. Two bundles E and F over X are said to be *stably equivalent* if and only if $E \oplus T_n \cong F \oplus T_n$ for some n . This means that two bundles might not be isomorphic but can become so when an adequate degree of freedom is 'added' to them. For instance, let E be the tangent bundle of the sphere S^2 and let $F = T_2$. We have already noted above that E is not isomorphic to F . But, as one can easily check, $E \oplus T_1 \cong T_2 \oplus T_1$, that is, $E \oplus (S^2 \times \mathbb{R}^1) \cong S^2 \times \mathbb{R}^3$. This example can again be easily generalized to an arbitrary dimension p . Whenever two vector bundles are stably equivalent they are equal in $K^0(X)$. Thus, E and F above will be identified in $K^0(S^p)$. This is again a concrete case in which the semigroup is not embedded in its Grothendieck group.

It is also important to note that these functors are not 'fine-grained', in the sense that $K^0(X) \cong K^0(Y)$ when the spaces X and Y are homotopy equivalent.¹² Thus, when some K -groups of two spaces are not isomorphic, the spaces are not homotopy equivalent and, *a fortiori*, not homeomorphic. Here is an easy illustration: the circle S^1 is not a contractible space. For, vector bundles over a one-point space are all trivial. If the circle were contractible, and thus homotopy equivalent to the one-point space, all *its* vector bundles would be trivial. But the Moebius band over the circle is a

invariant of the modules are the dimensions, and thus the semigroup is the set of natural numbers \mathbb{N} and the group completion the integers \mathbb{Z} . It is interesting to note that only *finitely* generated projective modules are considered. The reason is that when arbitrary projective modules are considered, then the group completion becomes trivial, *i.e.*, any valuable information is lost. (See Rosenberg [1994] for a detailed argument.)

¹² Two spaces X and Y are said to have the same homotopy type if there is an homotopy equivalence between them, *i.e.*, if there are continuous maps $f : X \rightarrow Y$ and $g : Y \rightarrow X$ such that the composites fg and gf are homotopic to the identities, id_X and id_Y respectively. This means that the two spaces can be 'deformed' into each other continuously, *i.e.*, by stretching, contracting, smashing points together but without tearing. Notice that two homeomorphic spaces will have the same homotopy type but that homotopy equivalent spaces need not be homeomorphic, *e.g.*, a contractible space is homotopy equivalent to a one-point space but need not be homeomorphic to it. It follows from this and from the observation that $K(P) \cong \mathbb{Z}$ that for any contractible space X , $K^0(X) \cong \mathbb{Z}$.

non-trivial vector bundle and thus the K -groups are not isomorphic.

One key aspect of the construction is that it is functorial: K is a contravariant functor from the category of topological spaces to the category of abelian groups. Furthermore, two homotopic maps $f_0, f_1 : X \rightarrow Y$ are sent to the same homomorphism $K^0(Y) \rightarrow K^0(X)$.

These are just the first preliminary steps. Some results have to be proved for K -theory really to get off the ground. In particular, it is when the higher groups K^n , for n an integer, and the connecting homomorphisms have been defined that K -theory becomes a generalized cohomology theory, that is, it satisfies all the axioms of Eilenberg and Steenrod except one, the dimension axiom, and can be used as such.¹³

What matters here is how K -theory is *used* in practice. As M. Atiyah put it, K -theory is a way of 'codifying qualitative information in algebraic form' (quoted by Booss & Bleeker [1985], p. 218) and whereas 'the usual algebraic topology destroys the structures [of manifolds] too much... , K -theory ('comparable to molecular biology' (Atiyah)), searches for the essential macromolecules which make up the manifold' (Booss & Bleeker [1985], p. 245). Basically, as we have already mentioned, the groups constructed via K -theory are sometimes easier to compute, which in turn makes possible simpler definitions and proofs. Here we will just mention two important applications of K -theory. One of these applications is the simplification of the proof of Adams's theorem on Hopf invariants, which is used to prove that the dimension of a finite-dimensional real-division algebra is 1, 2, 4, or 8. (These are the reals, the complex numbers, the quaternions, and the octonions or Cayley numbers, respectively.) The other application is to the Atiyah-Singer index theorem which, like all index theorems, exhibits deep relations between analysis and topology.

In connection with the first of these applications we note that Adams's theorem on Hopf invariants was first proved by means of cohomology theory, and then the proof was simplified with the help of K -theory. The theorem states: let n be an even integer and let $f : S^{2n-1} \rightarrow S^n$ be a continuous map with Hopf invariant an odd number. Then $n = 2, 4$ and 8 . (See Atiyah [1967], Husemoller [1966], p. 201, Mahammed *et. al.* [1980], p. 36, or Karoubi [1978], p. 272.)

It may be useful to recall that a real algebra is a real vector space V with a 'multiplication' $V \times V \rightarrow V$ such that $(\alpha x + \beta y)z = \alpha(xz) + \beta(yz)$ and $x(\alpha y + \beta z) = \alpha(xy) + \beta(xz)$ hold for all α, β in \mathbb{R} and all x, y, z in V , and a real *division* algebra V is a non-trivial real algebra such that x, y in V with $xy = 0$ implies either $x = 0$ or $y = 0$.

Now we come to the Atiyah-Singer index theorem. As we have already mentioned, index theorems connect in an unexpected and very fruitful way

¹³ See Rotman [1988], p. 231, for a presentation of the axioms.

questions related to the existence of systems of differential equations, *i.e.*, questions of analysis, with questions connected with the topology of a certain space (and its K -theory). And here, as Atiyah has said ' K -theory is just the right tool to study the general index problem' (1973 [1988b], p. 492). Following this work of Atiyah, many index theorems have been proved. Each of these theorems has provided an important bridge between a certain sector of analysis and a certain sector of topology with important applications in physics. (See, for instance, Blaine, Lawson & Michelson [1989] and Connes [1990].)

The index is a simple property of an operator. It can be defined very easily, but its computation is a different matter. The importance of the index is that, on the one hand, it is an invariant property of an operator (a fact that can turn out to be important for its computation) and on the other hand the index of a family of operators 'dissects' the space of these operators into connected components. To see what all this means, we have to introduce the context of functional analysis.

Let H be a separable Hilbert space. Let $T : H \rightarrow H$ be a bounded linear operator. T is called a *Fredholm* operator if $\ker T = \{u \in H : Tu = 0\}$ and $\text{coker } T = H/\text{im}(T)$ are finite-dimensional. Now, for any such operator, the index of T is defined by

$$\text{index } T := \dim \ker T - \dim \text{coker } T.$$

The definition is simple enough. The index captures a 'rough' property of an operator. Literally speaking, it measures the 'relative "size" ' of the space of solutions of an operator, the kernel of the operator, with respect to the space 'uncovered' by this operator, the cokernel of the operator. Given a specific operator T , the computation of the index can be very cumbersome: one has to investigate the space of solutions of the operator, *i.e.*, solve certain differential equations or, more frequently in this case integral equations, and compute the non-zero values of the operator. One way to simplify these matters is to rely on the invariance of the index.

The index is invariant in the following sense: for an important class of operators K , called 'compact' in the literature, it can be shown that $\text{index}(T + K) = \text{index } T$ whenever T is Fredholm. This means that the index of an operator T with unknown eigenvalues can sometimes be computed by calculating the index of $T + K$, for K a compact operator, which is sometimes feasible.

The index can itself be seen as a map with domain the space F of Fredholm operators defined on a Hilbert space H , with the 'natural' topology, and codomain the integers, *e.g.*, $\text{index} : F \rightarrow \mathbb{Z}$. As Dieudonné has shown, the index is locally constant, which is equivalent to saying that it is constant on connected components of the space F .

Now that we have introduced the space F of Fredholm operators, it is natural to investigate its properties as a topological space. As usual, one way to do this is to explore the images of known topological spaces X in F . The image of a continuous map $T : X \rightarrow F$, from a compact space X , is a (continuous) family of Fredholm operators in a Hilbert space H . Whenever X is connected, Dieudonné's result implies that $\text{index } T(x) = \text{index } T(x')$ for all $x, x' \in X$. It is then possible to define vector bundles, called $\text{Ker } T$ and $\text{Coker } T$, over X , in a natural way. Thus:

$$\text{Ker } T := \cup_{x \in X} \{x\} \times \ker T(x),$$

and similarly for the $\text{Coker } T$. Hence $\text{Ker } T$ and $\text{Coker } T$ belong to the category of vector bundles over X , and we can consider their isomorphism class $[\text{Ker } T]$ and $[\text{Coker } T]$. It is then possible to define the index bundle of T by:

$$\text{index } T := [\text{Ker } T] - [\text{Coker } T],$$

where this difference is now taken in $K(X)$.

When the space X is a point, then the index bundle is the usual index. It is thus a proper generalization of the classical index, and is usually called the analytical index. It can take more specific forms whenever the class of operators considered is specified, for instance the so-called Wiener-Hopf operators or, more generally, elliptic operators of various kinds. Using K -theory, Atiyah and Singer have found a different, purely topological, definition of the index, called the topological index of an operator. This definition is what is referred to in the literature as the 'Atiyah-Singer index formula'. The index theorem then shows that the two definitions agree.

***K*-theory as an Instrument**

Let us first try to capture abstractly the previous construction. It can be separated into two phases: the preparation or the set up and the 'transformation' proper. Steps 1 and 2 below constitute the preparation and steps 3 and 4 the transformation.

Step 1: We start with a given object or structure, a type of topological space or ring. We associate to this given object an object of a different type, say a vector bundle or a finitely generated projective module. These latter objects depend on the given one in a systematic fashion.

Step 2: We move to the collection of all these objects of the new type, together with the morphisms between them. This gives us a category. We then move to the isomorphism classes and thus get a new object, namely a semigroup. This latter object has been constructed in a systematic fashion from the given object. It is now this constructed object that we will 'project' or 'transform' into a different object which we can manipulate or understand better.

Step 3: We now define a whole family of functors, the K^i or K_i s, which are the 'projection' of our set up into the analyzable 'screen'. This is the first part of the 'experimentation' proper. The important fact is that the K^i or K_i s are systematically related to one another by the connecting homomorphisms.

Step 4: The projections themselves are (abelian) groups, that is objects which we know well and can usually manipulate easily. It is at this stage that many results about K -theory become relevant and useful, since they tell us how to 'read' the 'data'.

We can now easily see in what sense K -theory can be thought of as an instrument. We are in a position at least to show that the properties of the instruments we have listed above can be attributed to it, modulo the fact that it is not a concrete object.

(i) As we have said, K -theory (of schemes) was *invented* by Grothendieck; he was quickly followed by Atiyah and Hirzebruch who developed topological K -theory; but, these historical facts aside, it should be clear from the presentation above that K -theory is more or less an assemblage of different operations; it is a systematic *construction*;

(ii) The construction has a starting point. Outputs given by the machine are constructed from something and the latter can be viewed as being what the tool 'acts' upon. In the topological case, it is a category of topological spaces, in the algebraic case, it is a category of rings, in the algebraic-geometry case, it is a category of schemes; *etc.* Notice though, that from an abstract point of view, K -theory can be used as soon as a certain type of category is available; indeed in applications, K is applied to a given category which is usually constructed from another category, e.g., the category of isomorphism classes of vector bundles on a space;

(iii) Strictly speaking, we cannot say that K -theory 'interacts' with these objects, certainly not causally; however, it should be clear that these objects are used and in a way *transformed* or *translated* into different objects. As Atiyah said, the K in a way 'decomposes' the given objects into some of their components and it codifies in an algebraic form some of the properties of the objects. An interesting aspect of the situation here is that the causal chain is replaced by a sequence of operations which certainly has an algorithmic flavor. However, the operations involved are not elementary and the algorithm looks more like a series of 'macro' operations in a programming language than a Turing machine;

(iv) This is obvious: the construction, being a systematic construction, is planned and carried out in a certain order;

(v) It clearly has a function, in fact it could be said that it has numerous functions, depending on how one looks at it. For it could be said that its first and foremost function is to allow more elementary proofs of important results, e.g., Bott's periodicity. But as we have indicated, its function, as

a tool, is rather to reveal important *properties* of the objects studied, and *only* these properties;

(vi) This is an interesting and difficult question: the presence of norms. Here a close study of the development of algebraic K -theory, in particular of higher-order algebraic K -theory, would be revealing. Here are some of the norms we have in mind (some have already been mentioned by Connes in the quotation).

1. Clearly, mathematicians can come up with all kinds of constructions, as any scientist can; only those which are useful in one way or another will be admitted. This raises the question of the characterization of the *epistemic utility* of a tool.

Detlefsen [1986] has given a list of properties that a tool must have to be accepted. It is interesting to try to apply these properties in our context, for it turns out that they are not very revealing here. Of course, Detlefsen was trying to generalize from the properties of Hilbert's program, or rather the properties that Hilbert's ideal mathematics had to have to be considered a valuable tool, and thus came up with properties fitted to this tool; however they do not generalize to the tools we are discussing.¹⁴

The two basic properties he presents are efficiency and acuity. Here is what he has to say about them:

At its heart lies an analysis of the notion of epistemic utility which sees the epistemic utility of an instrument as being composed of two distinct ingredients: namely *efficiency* and *acuity*.

What we are here calling efficiency has to do with how rapidly or easily a device D generates conclusions about a given subject-matter. D 's efficiency as a guide to truths concerning a subject-matter S will be determined by how many conclusions regarding S are generated by D per unit time and/or effort. And we will say that device D_1 is more efficient than device D_2 as a guide to truths concerning S just in case D_1 generates more conclusions regarding S per unit time/effort than D_2 does.

On the other hand, what we are terming acuity is itself to be thought of as an amalgam of two elements: *perspicacity* and *reliability*. We shall say of an instrument D that it is a *perspicacious* guide to the truth regarding S just in case it succeeds in generating as outputs a significant body of the truths

¹⁴ Detlefsen's properties do not apply to some claims Hilbert made before he came up with his program. 'In perhaps most cases when we fail to find the answer to a question, the failure is caused by unsolved or insufficiently solved simpler and easier problems. Thus all depends on finding the easier problem and solving it with *tools that are as perfect as possible* and with notions that are capable of generalization' (D. Hilbert [1900]; quoted by Booss & Blecker [1985], p. 218). Or again: 'In answer let me point out how thoroughly, by the very nature of the mathematical sciences, any true progress brings with it the discovery or more incisive tools and simpler methods which at the same time facilitate the understanding of earlier theories and eliminate older more awkward developments. By acquiring these sharper tools and simpler methods the individual researcher succeeds more easily in orienting himself in the different branches of mathematics. In no other science is this possible to the same degree' (*ibid.*, p. 103.)

regarding S . And we shall consider an instrument D a *reliable* guide to the truths concerning S to the extent that the only conclusions regarding S that it generates are truths. Finally, an epistemic instrument is *acute* to the extent that it is perspicacious and reliable. (Detlefsen [1986], pp. 28–29).

It should be clear that it is not very revealing to use efficiency as characterized by Detlefsen, at least for K -theory. For, by using K -theory we can sometimes prove some results more simply, in the sense that the proofs are considerably shorter than those using other means. But there is a price to pay, which some might consider quite high, *i.e.*, it takes quite a long time and effort to master the tools and understand them. However, this is not what is considered to be the advantage of K -theory as a tool. It is not that *all* proofs using K -theory are considerably shorter and easier to find than proofs relying on different tools. But before we say more about what makes it so valuable, let us turn to the other two properties discussed by Detlefsen.

Perspicuousness does not fare much better: again, it is too quantitative. K -theory does allow proof of a substantial body of truths regarding topology, algebra or arithmetic, if only because it is very general. But this is too 'rough'. We need, on the one hand, a more qualitative measure: K -theory allows proof of 'important' results. Of course, this in turn can mean many different things. On the other hand, Detlefsen's property only applies to instruments used in proofs. As we have seen, K -theory is also used to *define* invariants which are then used in proofs.

Finally, reliability works for logical tools, but not for the type of tools we are discussing. It does not make much sense to say that K -theory 'generates truths and only truths' about a subject matter. It is not that K -theory could generate falsities, even when properly used, but we can certainly think of cases where it would be irrelevant or pointless.¹⁵

Consequently, a different way to characterize the epistemic utility of the tools used by mathematicians has to be found. We take as our guide some properties of concrete tools and adapt them to the present context. We submit *faithfulness*, *handiness*, *power*, and *robustness*, characterized thus:

a. We say that a tool is *faithful* whenever it 'fits' the objects it is supposed to act upon in the right way. In the case of a concrete tool, *e.g.*, a screwdriver, it amounts to a certain correspondence between the object acted upon and the tool. In the case of topological K -theory, this amounts roughly to the fact that the K -functors are homotopy invariant. This tells us how 'well' they fit. Here, we can certainly compare, for instance, K -theory with other cohomology theories and discuss their faithfulness relative to certain objects.

b. We say that a tool is *handy* if it simplifies a task in one way or another.

¹⁵ I, for one, would be surprised to find out that the K -theory of Boolean rings has important applications in logic.

The task here could be a proof, a computation, as in the Riemann-Roch theorem or the definition of an object. Again, this is trivial for concrete tools and we can find concrete indicators of simplicity in terms of effort, energy, etc. It is not so easy for mathematical tools. It remains to be seen whether logical complexity can be of any help here. *Understanding* might be more relevant.¹⁶

c. We say that a tool is *powerful* whenever it helps to solve an important problem and by doing so can be adapted to many different fields and thus allows a surprising unification of mathematics. (This is related to Detlefsen's perpicuousness.)

d. Finally, we say that a tool is *robust* whenever it allows vast generalizations, that is, it remains useful when we try to enlarge the context of its original employment. It may even sometimes suggest how to generalize certain results.

This is only a tentative list. We cannot claim that it is complete or exhaustive, nor are we sure that it is adequate. More work has to be done before we can be sure of this.

2. There has to be a norm for the proper application of an accepted tool; one has to be able to tell when a tool has been used properly, for a given proof might be unacceptable whenever a tool has not been used adequately at one point or another in it (one can fix something with a tool by trial and error, but the whole thing might fall down or be unsafe). This norm is crucial for it is only when we know that the tool has been used properly that we can adopt a certain epistemological attitude towards a result proved with its help. Compare this with the norm accompanying the use of, say first-order, logic in proofs: then we can verify whether each step is an application of one of the given rules or if it is simply a given hypothesis of the problem.

In the case of K -theory, some of the implicit norms seem to be these:

- (i) the different K^i 's are *functors*;
- (ii) the different K^i 's are related to one another in a systematic fashion, *i.e.*, we have to be able to define a connecting morphism between them;
- (iii) the different K^i 's are related to other functors in a systematic fashion, *i.e.*, the fundamental groups, more generally the homotopy groups, the general linear groups of the spaces, standard cohomology groups, etc.

Again it is far from clear that this is the correct list, even partial, of the norms at work. There are probably others, *e.g.*, K^0 presupposes an additive category. In a sense, the purpose of the theory surrounding a given tool

¹⁶ 'Since Adams's original proof involved the development of the subtle Adams spectral sequence relating cohomology to stable homotopy and a deep study of secondary cohomology operations (Adams's final paper in *Annals of Mathematics* occupied 80 pages!), it is a signal triumph of K -theory to produce a proof which only requires a knowledge of certain very natural *primary* operations in K -theory (Hilton [1971], p. 2).

is to make these norms explicit so that one learns how and when to apply the tool. In their turn, these norms guarantee a form of objectivity, a form of *methodological* objectivity.¹⁷ It might be useful to think about concrete tools and try to transfer whatever can be extracted from them to the mathematical context. Moreover, it is hard to see at this stage how these norms can be generalized to all the tools in the family, unless it is precisely these norms which can serve as a basis of a proper classification of tools. But this is sheer conjecture.

Be that as it may, there are mathematical theories which are tools or instruments in the sense that their objects satisfy the properties of instruments in general and, moreover, they satisfy these properties in a *specific* way. To see this, we can compare them with one other type of tool: notational systems.¹⁸

Notational systems can be considered as tools. Notice, however, that this is not unproblematic. Indeed, a strict formalist would certainly not consider them as such: quite the contrary. For her, notational systems are what mathematics is about and not something useful to help us understand and do mathematics. But we do believe that they are first and foremost tools, instruments of thought and communication.¹⁹ Does a notational system satisfy the six properties in our list?

(i) It certainly satisfies the first property: they are undoubtedly constructions.

(ii) This is the crucial difference. Notational systems do not act on objects, at least not in the sense that we use this expression for concrete instruments and not in the same way that *K*-theory could be said to depend directly on a given category of objects. A notational system is a tool inasmuch as it is related to something else, inasmuch as it stands for something else in a systematic fashion. However, this 'standing for' is radically different from what the *K*-groups stand for. Whereas the *K*-groups are *built up from*

¹⁷ This might be the *only* form of objectivity in mathematics. See Bunge [1974], p. 169, for a characterization of methodological objectivity in mathematics.

¹⁸ Another interesting case study is the axiomatic method, which seems to have caused great pain towards the end of the nineteenth century and at the beginning of the twentieth, even to Russell.

¹⁹ It is clear that Frege first thought of his *Begriffsschrift* as a tool (he even compares it to a microscope!): 'This ideography, likewise, is a device invented for certain scientific purposes, and we must not condemn it because it is not suited to others. If it answers to these purposes in some degree, one should not mind the fact that there are no new truths in my work. I would console myself on this point with the realization that a development of method, too, furthers science. Bacon, after all, thought it better to invent a means by which everything could easily be discovered than to discover particular truths...' (Frege 1879, 1967, p. 6). In his *Foundations of Arithmetic*, Frege states that 'it is possible, of course, to operate with figures mechanically, just as it is possible to speak like a parrot: but that hardly deserves the name of thought. It only becomes possible at all after the mathematical notation has, as a result of genuine thought, been so developed that it does the thinking for us, so to speak' (Frege 1883 [1980], p. iv).

the objects studied and thus depend directly on these objects, a notational system does not have to depend on the objects it denotes. We do not devise a notational system for arithmetic by manipulating the numbers themselves in a systematic fashion, whatever that could mean.

(iii) Similarly, the K -groups can be seen to encode genuine properties of the objects whereas it is much harder to claim that symbols of a notational system encode genuine properties of the denoted objects. There is usually very little in the symbols themselves which encodes a property of the denoted object whereas a K -group has to be seen as the algebraic encoding of a specific property of a given object.

However, we now know that a notational system, rather a *deductive* system, can capture within its own structure part of the structure of the operations definable on the objects studied, as for instance in the case of a positional system for number theory. Thus, notational systems may capture *as a whole* properties of the objects studied *as a whole*. This is after all a large part of the interest in and the usefulness of mathematical logic. Of course, in doing so, we shift from the point of view of notational systems as tools in their specific applications, *e.g.*, computations, to notational systems as tools inasmuch as they capture, as representations, fundamental properties of the things they talk about. This shift is more drastic than first appears. For by doing it, we consider the notational (deductive) system as an *abstract* system, whereas as a computational tool, it is rather some of its *concrete* properties, that is the manipulation of symbols, which make it an interesting tool.²⁰ When we consider a notational system as a whole, we treat it as an abstract structure which captures or encodes properties of something else, as the K -groups do. The main point here is that there is no comparable shift for K -theory. For instance, it does not make any sense to introduce a type-token distinction in K -theory whereas it makes perfect sense when we deal with a notational system.

(iv, v) As to the planning and the function, there is no doubt that the situation is again different. As we have seen, the fundamental purpose of K -theory is to allow the definition of invariants which are then useful in various ways in proofs. The point of contact here is definability theory: a notational system certainly must allow certain definitions, but it is far from clear that this is its sole and principal function.

(vi) Clearly, a notational system comes equipped with rules and thus with norms of application which are in general purely local. As we have already mentioned, these norms are different from the norms inherent in K -theory.

²⁰ Think of the different notational systems for derivatives, *e.g.*, Newton's versus Leibniz's.

Notational systems are tools inasmuch as they allow an automatic coordination of thought, but of the sensory-motor type, that is of the direct input-output mode. We believe that this is particularly clear in the acquisition of the basic arithmetical operations, including the division algorithm. Thus, very quickly, most of the notational operations are carried out 'without thinking'. Hence a notational system is considered to be a good tool whenever it takes over some of the thinking required in the solution of problems. Another good example of this is the graphical notational system used in category theory, for some proofs are obvious when one looks at the proper diagram.

Thus, notational systems, although they are tools, are not tools in the same way as the machinery we are talking about. As we have just indicated, the major and crucial differences occur at points (ii) and (iii).

There is one further element worth mentioning. As soon as we talk about tools and instruments we let the knowing subject and his capacities enter the picture.²¹ For a tool is always used by someone with certain capacities and its design and success also depend on these capacities. For instance, K -theory makes the computation of certain invariants easier than other cohomology theories, and this is because it allows us to ignore irrelevant information and we find it easier to compute them. Similarly, crucial features of notational systems are consequences of our cognitive make up. But again, there are important differences between the two cases. It is reasonable to claim that the reason why the K -groups are easier to compute is because they are, as groups, 'coarser' than other cohomology groups. (Recall Atiyah's claim above.) It is as if we used an optical microscope instead of, say, an electron microscope. Thus, it is not because the K -groups fit our cognitive apparatus better than other groups, but because they encode the properties differently. I submit that in the case of notational systems, in their concrete applications and not as abstract structures, their use as tools depends more heavily on the way they fit our cognitive needs and constraints. Frege's notation for logic, as a concrete notational scheme, is too far from our standard notational practice to be insightful.

Conclusion

We hope that we have convinced the reader that it makes perfect sense to say that contemporary mathematics contains abstract tools and machines.

²¹ 'As beings suffering such limitations [e.g., limited amounts of time and energy to expend in the pursuit of epistemic goals], we are naturally concerned that our methods of epistemic acquisition be maximally efficient; i.e., that no alternative methods available to us yield a higher return on the expenditure of our limited epistemic resources than the ones we have adopted. The Hilbertian defense of the ideal methods of classical mathematics, as we have presented it, is a proposal in this general spirit' (Detlefsen [1986], p. 83). However, different tools might reveal different aspects of a problem, each one leading to different and revealing generalizations for instance.

Once this is established, the number of issues that one has to address becomes daunting. We would like to conclude by emphasizing only three of them.

Firstly, the mathematical universe is not as uniform as is usually thought. If we admit that there are instruments and machines within this universe, then we grant the fact that there are mathematical 'natural kinds' and mathematical 'artefacts'. We are then faced with the problem of providing criteria for the natural kinds or for the artefacts. This seems to me to be a particularly difficult problem for realists. But it also raises the question of the adequacy of the various instrumentalist positions in mathematics. We will come back to these issues in a forthcoming paper.

Secondly, a general classification scheme for mathematical tools and machines should be provided. Concrete analysis of specific tools can only go so far. A general classification scheme would be extremely useful for the epistemology of mathematics. It is far from clear how this should be tackled. A logical approach might be a good start if only one knew where to start. It is rather stunning to see that, at least to my knowledge, we don't even have the proper logical tools to effect a logical analysis of, say, algebraic topology. All the logical tools we have at our disposal are devised for a theory and not for the interaction of theories.

Finally, and this is closely related to the previous point, it is clear that the tools and machines we have been considering here possess an algorithmic dimension. One wonders whether a proper logical analysis *à la* Turing could not be provided for some of them, thus revealing important aspects of 'the technique of our thinking'?²² Are there any *logical* reasons underlying the success of these tools? Some progress has been made very recently on this question. Indeed, Sergeraert's work [1994] establishes the computability

²² This expression comes from a famous passage in Hilbert's paper on the foundations of mathematics. The whole passage runs as follows:

For this formula game is carried out according to certain definite rules, in which the *technique of our thinking* is expressed. These rules form a closed system that can be discovered and definitively stated. The fundamental idea of my proof theory is none other than to describe the activity of our understanding, to make a protocol of the rules according to which our thinking actually proceeds. Thinking, it so happens, parallels speaking and writing: we form statements and place them one behind another. If any totality of observations and phenomena deserves to be made the object of a serious and thorough investigation, it is this one. . . [Hilbert 1928, 1967, p. 475].

In a way, the tools we have exhibited make this thinking, which parallels speaking and writing (in Hilbert's words), less obvious: for the tools, even though they certainly help our thinking, do so by organizing some objects and their properties in a particularly transparent way, so that this organization guides our thinking. It might very well be that Hilbert's proof theory comes closer to describing our activity of justification and misses large parts of our activity of understanding. For there are very often many different proofs of one and the same result. Many mathematicians feel that often one of these proofs provides 'the real reason' for a result (see Mac Lane [1986]), or 'gets to the heart of the problem', as Atiyah is supposed to have said about his new proof with R. Bott of the periodicity theorem.

of many groups of algebraic topology and even opens the door to a new analysis of some K -groups. This work seems to capture the algorithmic aspect of these constructions and thus establishes an explicit link with real machines. But this will have to be examined elsewhere.

References

- ATIYAH, M. [1967]: *K-theory*. New York: Benjamin Press.
- _____, [1987]: *Collected Works*. Volume 1: Early Papers & General Papers. Oxford: Oxford University Press.
- _____, [1988a]: *Collected Works*. Volume 2: K -theory. Oxford: Oxford University Press.
- _____, [1988b]: *Collected Works*. Volume 3: Index Theory: 1. Oxford: Oxford University Press.
- AZZOUNI, J. [1994]: *Metaphysical Myths, Mathematical Practice*. Cambridge: Cambridge University Press.
- BASS, H. [1974]: *Introduction to Some Methods of Algebraic K-Theory*. Providence, Rhode Island: American Mathematical Society.
- BLAINE LAWSON, JR., H., and M. L. MICHELSON [1989]: *Spin Geometry*. Princeton: Princeton University Press.
- BOOSS, B., and D. D. BLEECKER [1985]: *Topology and Analysis*. New York: Springer-Verlag.
- BUNGE, M. [1974]: *Treatise in Basic Philosophy*. Volume 2. Dordrecht: Reidel.
- CONNES, A. [1990]: *Géométrie non commutative*. Paris: InterEditions.
- _____, [1992]: 'Round table discussion', in C. Casacuberta and M. Castellet (eds.), *Mathematical Research Today and Tomorrow*. LNM 1525. New York: Springer-Verlag, pp. 88–108.
- DETLEFSEN, M. [1986]: *Hilbert's Program*. Dordrecht: Reidel.
- FREGE, G. [1879], [1967]: *Begriffsschrift*, in J. van Heijenoort (ed.), *From Frege to Gödel*. Cambridge, Mass.: Harvard University Press, pp. 1–82.
- _____, [1883], [1980]: *The Foundations of Arithmetic*. Translated by J. L. Austin. Evanston: Northwestern University Press.
- HALLETT, M. [1989]: 'Physicalism, reductionism & Hilbert', in A. D. Irvine (ed.), *Physicalism in Mathematics*. Dordrecht: Kluwer, pp. 182–256.
- HILBERT, D. [1928], [1967]: 'Die Grundlagen der Mathematik', *Abh. math. Sem. Hamburg*, 6, 65–85, in J. van Heijenoort (ed.), *From Frege to Gödel*. Cambridge, Mass.: Harvard University Press, pp. 464–479.
- HILTON, P. [1971]: *General Cohomology and K-Theories*. Cambridge: Cambridge University Press.
- HUSEMOLLER, D. [1966]: *Fibre Bundles*. New York: Springer-Verlag.
- KAROUBI, M. [1971]: *K-Théorie*. Montréal: Les Presses de l'Université de Montréal.
- _____, [1978]: *K-Theory*. New York: Springer-Verlag.
- MAC LANE, S. [1985]: *Mathematics: Form and Function*. New York: Springer-Verlag.
- MAHAMMED, N., et. al. [1980]: *Some Applications of Topological K-Theory*. Mathematics Studies 45. Amsterdam: North-Holland.

- QUILLEN, D. [1974]: *Higher Algebraic K-Theory I*. LNM 341. New York: Springer-Verlag, pp. 85–147.
- ROSENBERG, S. [1994]: *Algebraic K-theory and Its Applications*. New York: Springer-Verlag.
- ROTMAN, J. J. [1988]: *An Introduction to Algebraic Topology*. New York: Springer-Verlag.
- SERGERAERT, F. [1994]: ‘The computability problem in algebraic topology’, *Advances in Mathematics* **104**, 1–29.
- WALL, C. T. C. [1965]: ‘Finiteness conditions for CW-complexes’, *Annals of Mathematics* **81**, 56–69.

ABSTRACT. In this paper, we try to establish that some mathematical theories, like K -theory, homology, cohomology, homotopy theories, spectral sequences, modern Galois theory (in its various applications), representation theory and character theory, etc., should be thought of as (abstract) machines in the same way that there are (concrete) machines in the natural sciences. If this is correct, then many epistemological and ontological issues in the philosophy of mathematics are seen in a different light. We concentrate on one problem which immediately follows the recognition of the particular status of these theories: the demarcation problem between ‘natural kinds’ and ‘artefacts’.