MATHEMATICAL EXPERIMENTS AND MATHEMATICAL PICTURES

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1. INTRODUCTION

In a previous paper 'What, if anything, is an experiment in mathematics?' [1995], I undertook an investigation of the question whether there is any such thing as an experiment (say, in the naive sense of the physical sciences) in mathematics. In fact, what I did was to examine what mathematicians themselves have reported as genuine mathematical experiments. In summary, these were my conclusions:

- (C1) Two types of experiments are mentioned in the literature. One sees a computation (preferably using sophisticated machinery) as an experiment, the other sees 'real-world' experiments as possible candidates;
- (C2) I rejected the idea of a computation as an experiment for the simple fact that to any computation there corresponds an arithmetical equation and that equation can be proved in a suitable part or fragment of arithmetic;
- (C3) I also rejected the idea of a real-world experiment because either the result is mathematically useless or, if the experiment produces a different result, it is not the mathematics that is questioned but the underlying physical theory (see further for an example, viz., Plateau's problem);
- (C4) Therefore, there are no such things as experiments in mathematics.

Although (C4) seems to be a firmly stated and bold conclusion, it is subject to the initial proviso, namely that I am reflecting here on the notion of mathematical experiment *as used by the mathematicians themselves*. Rather trivially, this means that I have only dealt with what one might call the standard picture of mathematics as seen, more implicitly than explicitly, by the average mathematician. I will not try to spell out this picture[1]–that would no doubt make a fascinating piece of sociology of mathematics, see, e.g., Fischer, Restivo and Van Bendegem [1993]–but I am quite convinced that it would have the following ingredients:

1. Evidence for this standard picture can be found in my [1993] wherefrom these characteristics are drawn.

(M1) Doing mathematics is to a large extent a self-justified activity;
(M2) Mathematicians all implicitly or explicitly share the idea of the existence of a unique mathematical universe;
(M3) There is (almost) complete agreement on the preferred method to obtain the description of the universe: the method of (more or less formalized) proofs;
(M4) They share 'the general accessibility belief': If, for any mathematical statement S, there is a proof p of S, then it can, in principle, be found or constructed by any mathematician;
(M5) They share 'the general control belief': Any proof can be checked by any mathematician such that he or she can be certain that the proof is either correct or faulty;
(M6) How a proof is to be found is mostly a matter of some kind of innate capabilities the mathematician is supposed to have. Terms such as 'gifted', 'talented', 'having the right sort of intuition' are used to describe these somewhat mystical powers.

Thus, the general conclusion of that paper could be formulated thus: the notion of a mathematical experiment does not make sense within the standard picture of mathematics. This statement leads quite naturally to the following question: Are there alternative pictures of mathematics such that the notion of a mathematical experiment *does* make sense within it? Of course, this is a philosophical problem of first order and it would therefore require a book-length study to formulate an answer to it. My aim in these few pages is rather modest: to bring together some arguments that support the idea of genuine mathematical experiments notwithstanding all the evidence to the contrary if we look at current mathematical practice. To be more specific, I will claim that:

(1) All mathematical problems when traced back in the history of mathematics eventually reduce to a practical real-world problem. Although this does not imply an empiricist view of mathematics, it comes rather close. I will refer to this view as the *chronologically empirical view*, and
(2) The fact that the standard picture does not view mathematics as empirical at all is, above all, due to, what I would like to call, the *a priori obsession*. It has led to a sharp division in the scientific enterprise, enabling mathematics to acquire its own very special status.

2. What is empirical about abstract mathematics?

Consider the following passage:

> An elliptic curve over Q is said to be modular if it has a finite covering by a modular curve of the form $X_0(N)$. Any such elliptic curve has the property that its Hasse-Weil zeta function has an analytic continuation and satisfies a functional equation of the standard type.

What on earth is empirical about this? Are we not talking about the most abstract object possible? It would be folly to say: 'No, we are not', hence the reasonableness of the question. My claim is that the if we retrace the steps that have led to the above passage, then somewhere we will stumble on really concrete problems involving real objects. A few examples may serve as illustration, though what is really required of course are general arguments as to why any problem, no matter its level of abstractness, must eventually reduce to a practical problem.

Example 1: Fermat's Last Theorem.[2] The above quote is the opening paragraph of a paper entitled 'Modular elliptic curves and Fermat's Last Theorem'; its author, the now famous mathematician Andrew Wiles. To the average professional mathematician this paper is extremely hard to read, even harder to understand. It reaches one of those rare summits of abstraction in mathematics so much so that it is claimed that no more than ten mathematicians throughout the Western world are capable of appreciating and evaluating the work done by Wiles (and Taylor).

Nevertheless, the origin of the problem is to be found in an algebra book of a Greek mathematician. Although a problem such as 'Find a, b and c such that $a^2 + b^2 = c^2$' might seem an abstract question about numbers, in fact, as we all know, it can be connected to right-angled triangles. There are many reasons to believe that the solution $3^2 + 4^2 = 5^2$ was found in an empirical way. There does not seem to be a horribly large distance from the task 'Find a, b and c...' to the problem 'Find *all* a, b and c...', the latter question being at first answerable in a crude fashion–there are a finite number of them or there is no upper limit to the number of solutions–in a later stage answerable in a more detailed fashion. But squares of numbers are related to squares (what else?), third powers are related to cubes,... is it not unavoidable to extend the question and ask whether the equation $a^3 + b^3 = c^3$ has solutions or not?[3]

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2. Details of Fermat's Last Theorem and references can be found in my [1987]. The last result mentioned there was Mordell's conjecture. What is therefore lacking, are the results of Andrew Wiles published as Wiles [1995] and Taylor and Wiles [1995]. The former is the main work, the latter is a gap-filling addendum to the former.

3. On top of that, this equation is clearly linked to one of the oldest geometrical problems around: the duplication of the cube. Take a equal to b, and the equation reduces to $2.a^3 = c^3$. An integer solution of this equation would imply in a straightforward way a solution to the duplication problem.

Before you know, the general question has been asked: what about solutions of $a^n + b^n = c^n$? Once thus generalized, one can permit oneself to 'forget' that fourth powers do not seem to correspond to anything geometric, to 'forget' that if the exponent n equals a number larger than the numbers of electrons in the universe, the numbers in fact no longer correspond to no matter what. If on top of that, the methods one has selected through the ages to handle this type of problem, support the process of forgetting, then it becomes almost unavoidable to generalize from a problem in elementary number theory to complex numbers (after all, $x^3 + y^3 = (x + y)(x^2 + xy + y^2)$, but that does not seem to work for $x^4 + y^4$, unless you accept that $(x^2 + iy^2)(x^2 - iy^2)$ is a good solution). But, sadly enough, complex numbers do not behave as your standard numbers, e.g., prime decomposition is not unique,[4] thus ideal complex numbers and their divisors come into play. The latter one can sort in classes, one can calculate (a) class number(s) and that leads one to Bernoulli numbers that connect with the exponential function, more precisely, $x/(e^x - 1) = \sum_n B_n . x^n/n!$, where B_n is the n-th Bernoulli number. Before one is aware of it, theorems such as the following appear:

> Let p be a given prime greater than 2 and let g(l) and h(l) be nonzero cyclotomic integers built up from a pth root of unity $l \neq 1$. Then g(l) divides h(l) if and only if every prime divisor which divides g(l) also divides h(l) with multiplicity at least as great.

Should I continue with elliptic curves? If $c \neq 0$, then it follows from $a^n + b^n = c^n$, that $(a/c)^n + (b/c)^n = 1$. Write A for a/c and B for b/c, thus $A^n + B^n = 1$ describes a curve over the reals and what we look for, are rational solutions. Then why not generalize to arbitrary algebraic number fields K (of which R, the set of reals, is a special case, with its special subset Q, the set of rationals)? And there you have Mordell's conjecture:

> Let K be an algebraic number field and let C be a nonsingular projective curve over K, with genus $g \geq 2$. Then the set of points of C which are K-rational is necessarily finite.

Gerd Faltings proved this conjecture. But finite is not zero. For that we need elliptic modular semistable curves. Then, finally, the answer is: no solutions.

Of course, this sketchy oversimplified slightly mystifying story does not constitute full proof of my claim, but it does show to a certain extent how 'naturally' abstraction occurs. This holds even stronger for the second example. After all, one might object that problems such as Fermat's Last Theorem are in a way still about very concrete things. All of the above could have been

4. A simple example: 5 can be written as 1.5 (so it would be a prime), but it can be equally written as (2 - i).(2 + i), where i is the imaginary unit (so it is not a prime).

summarized by saying that the problem is about powers of integers or rationals and that seems pretty concrete: a square as a sum of squares, a cubic as a sum of cubics... But what about the 'truly abstract': set theory? Actually, it is precisely set theory that inspired me to formulate the bold claim I am presenting here. As a matter of fact, the development of set theory started out from a very practical problem.

Example 2: Cantor's transfinite set theory.[5] Start with the heat equation well-known in physics: $\Delta T = (c\rho/\lambda).\partial T/\partial t$, where $\Delta T = \partial^2 T/\partial x^2 + \partial^2 T/\partial y^2 + \partial^2 T/\partial z^2$. This is a partial differential equation. What does a general solution look like? Fourier gave an almost complete answer: a series of trigonometric functions, i.e., sines and cosines. Surely there will be no discussion about the concrete nature of sines and cosines. But having a solution is one thing, establishing that it is unique, another. In one of his first papers, Cantor gave a solution to that problem: under certain conditions, the Fourier representation of a given function is unique. However, the conditions had to be satisfied for *all* points in the domain of the function. Could this condition be weakened? Surely, if the number of exceptions is finite, this can be no problem at all. Could there be an infinity of them? Yes, if they are distributed in certain ways, namely such that they form a convergent series, such that there is an accumulation point. Cantor shows that this idea works for one accumulation point, easily extending the result to a finite number of accumulation points, less easily extending it to an accumulation point of accumulations points, and there you have the beginning of a transfinite number hierarchy. Add Dedekind to it, who was looking for a neat formulation of the real numbers and transfinite set theory is born and the most important question has been asked right from the start: how many real numbers are there? This problem required the ingenious diagonal method, but the answer was startling enough: a lot more than the (cardinal) number of natural numbers (or fractions or algebraic numbers, for that matter). How could one avoid asking the next question: what in between? And so we arrive at the (in)famous continuum hypothesis (CH), problem number one of Hilbert's list of 23 main research topics for this century.[6]

Example 3: It is no doubt a rather trivial statement that natural numbers are closely linked to numerical practice. Thus 1, 2,... are firmly rooted in the empirical world. But then so are prime numbers. It does not require a lot of

5. The over-reduced story told here has one source mainly: Dauben [1979].

6. It is not difficult at all to show that all 23 research topics are easily reducible to concrete problems. Actually, one of them, problem 6, asks precisely for a 'Mathematische Behandlung der Axiome der Physik'. In as much as Hilbert's list can be seen as representative for present-day mathematical research, this is a rather direct way to support my claim. However, I will not do this here and I prefer, instead, to focus on more general arguments. For an overview and discussion, see Alexandrov [1971].

mathematical insight to think of numbers in terms of geometrical arrangements and then to ask questions about part and whole. But once there are prime numbers, it seems natural to ask 'How many?'. This leads straight away to what mathematicians themselves call one of the most beautiful theorems of mathematics: the proof of the infinitude of prime numbers. But then realizing that there is a very nice connection between on the one hand the harmonic series $\sum 1/n$, for $n = 1, 2, 3,...$ and the infinite product $\prod 1/(1-1/p)$, where p runs over all primes. To be precise, the connection is that both series are identical.[7] Thus the infinitude of primes leads straightaway to the infinitude of the harmonic series. It is not a great step away to look for the generalization of the harmonic series, i.e., $\sum 1/n^k$, for $n = 1, 2, 3,...$ and k a given exponent. Write this as a function of k, say $Z(k)$. Then the above says that $Z(1)$ = infinity. One of the beautiful results in mathematics is that $Z(2) = \sum 1/n^2 = \pi^2/6$. But why stop at the natural numbers as domain of the function Z. Why not generalize to complex numbers? This leads to $Z(s) = \sum 1/n^s$, where s is a complex number. The Riemann zeta-function is born. In fact, one of the most famous (still) open problems in mathematics is the question: What are the zeros of $Z(s)$?

As said before, these few examples do not constitute a general argument. But, it seems obvious that if such abstract cases have an empirical origin, then surely so for more 'mundane' mathematical problems. However, one might object that all this shows is that mathematical problems have (perhaps) empirical roots. But then mathematics is not only about problems. Is not the most interesting activity of mathematicians the task to search for proofs. And proofs clearly do not have empirical origins. Let me argue briefly to the contrary.

I claim that even the notion of proof is, to a certain extent, empirical in its origin. I rely here mainly on the work done by Teun Koetsier in his [1991]. Without going into the details of the Lakatosian framework that he has taken over and adapted to his particular needs, the rough outline of the model that he presents consists of three levels:

(a) On the micro-level, individual mathematicians prove theorems, formulate conjectures, check proofs or theorems, search for counter-examples to disprove a statement, and so on.

(b) This micro-level activity presupposes of course that a mathematician already knows, implicitly or explicitly, what are inter-

7. The proof is not difficult at all. Just remember that $1/(1 - 1/p)$, where p is a prime, can be expressed as an infinite series, namely, $1 + (1/p) + (1/p)^2 + ... + (1/p)^n + ...$ Thus each factor in the infinite product $\prod 1/(1 - 1/p)$ can be replaced by this sum. If we multiply out all these sums, each term in the resulting sum will look like this: $(1/p_1)^{n1}.(1/p_2)^{n2}. ...(1/p_k)^{nk}$, or $1/p_1^{n1}.p_2^{n2}. ...p_k^{nk}$. This expression corresponds to the prime decomposition of some natural number n, thus it is of the form $1/n$. Take the sum of all the terms and you get the harmonic series $\sum 1/n$. What remains to be shown is the uniqueness. The key is the fact that the prime decomposition is unique.

esting mathematical problems (in a particular field), that she knows what proof methods are likely to work for a particular problem, that she knows what did or did not work in the past. Koetsier calls this kind of knowledge a research project (RP) to be situated on an intermediate level.

(c) Finally, on the macro-level, RPs are structured by research traditions (RTs): 'A mathematical research tradition is a group research activity, historically identifiable (in a certain period), characterized by common general assumptions (in the form of, e.g., definitions and axioms) about the entities that are being studied in a particular fundamental mathematical domain, and it involves assumptions about the appropriate methods to prove properties of those entities' (Koetsier, [1991], p. 151).

The most important thing to note is that proof techniques are part and parcel of a research tradition and, therefore, part and parcel of certain specific research projects. Hence as traditions and projects change and develop, so do potentially proof methods. One of the examples given by Koetsier shows precisely such a change, where the empirical origin of the notion of proof becomes evident. In Greek mathematics, Koetsier distinguishes two traditions that he calls, chronologically, the *Demonstrative Tradition* (DT) and the *Euclidian Tradition* (ET). A major point of difference between DT and ET is the fact that ET introduces the notion of proof as standard method for establishing mathematical truths. Koetsier claims that the proof method of DT is *non-deductive*. It is based on a form of 'Anschauung'. The best example to illustrate this is the 'proof' of $(n+1)^2 = n^2 + 2.n + 1$, in Pythagorean fashion. Thus, to show that $4^2 = (3 + 1)^2 = 3^2 + 2.3 + 1$, it is sufficient to look at these two drawings:

Of course, if this is to count as a convincing method, we must assume that a particular case can be 'seen' as an arbitrary case. That is, I am supposed not only to grasp this figure (or rather its meaning) but also all other cases similar to it. Granted that sense can be made of 'proof by looking',[8] then it is obvious

8. The expression 'proof by looking' is actually an entry in Wells [1991]. I quote: 'Many simple arithmetical facts can be proved 'at sight', by examining a suitable figure (p. 198). If Koetsier is right, one might just as well leave out the 'simple', for he claims that, according to Becker, there is a 'proof by looking' of this arithmetical fact: any number of the form $2^n.(1 + 2 + 2^2 + ... + 2^n)$ such that $p = 1 + 2 + 2^2 + ... + 2^n$ is a prime, is perfect.

that proofs too have their origin in an activity that is not purely 'mental', but involves a clearly visual act. All this being said, the transition from DT to ET is a major one indeed. There does not seem to be a gradual transition. The introduction, to name but the most important change, of the *reductio ad absurdum* is a real break. If one is to prove A and to start from not-A, then it is not clear at all how one is to visualize not-A, as A is supposed to be the case in all circumstances.[9] But that is the problem to be addressed in the next paragraph: how did mathematics not just grow away from its empirical 'roots', but managed to acquire this special status of total abstractness?

3. The a priori obsession of (abstract) mathematics

If the claim made in the previous paragraph is correct, then, not taking into account any other circumstances, two possibilities (roughly) remain:

(a) Either mathematics remained 'faithful' to its empirical roots and developed as such, or

(b) Mathematics drifted away from its roots to develop into a quite different activity up to the point of denying its roots.

What if other circumstances are taken into account? Is it then not possible that, since (b) is what we have actually seen happening in the history of mathematics, one arrives at the conclusion that (a) is not a real possibility, for the simple reason that it is quite impossible? Hence, even accepting the idea of the empirical roots, this does not help to turn mathematics into an empirical science. After all, if I happen to see a cloud formation in the sky on a rainy day and I happen to notice that this formation seems to spell '$p \rightarrow p$', it does not make sense to call the statement '$p \rightarrow p$' an empirical truth rather than a logical truth on the basis of the fact that it occurs in nature.

Thus, the first thing I would like to show is that (a) does represent a genuine possibility. Of course, as mathematics did not develop along the lines of (a), what follows is necessarily a thought experiment.

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9. To give an example: suppose I want to prove that there is no solution of the equation $4.x^2 + 2.x - 1 = 0$ in integers. I reason as follows: suppose there is a solution n. Then $4.n^2 + 2.n$ is always even, whether n is even or odd. But then $4.n^2 + 2.n - 1$ is always odd and hence never equal to 0. There seems to be no way to visualize the supposition, as $4.x^2 + 2.x - 1 = 0$ does not in fact have solutions in the integers. Of course, a way out, is to avoid *reductio* altogether. Hence the importance from this point of view of the work of constructivist mathematicians. For this specific case, it is sufficient to remark that $b^2 - 4.ac = 4 + 4.4 = 20$ is not a perfect square.

10. A more exact formulation would be to say: the level that is put forward as the ideal height to reach. In 'real' mathematics, this level is not always attained and a different story is told on the stage and behind the screens. This metaphor is the basis of a rather provocative paper of Reuben Hersh, namely his [1991].

Suppose then that mathematics developed in such a way that the standards of proof never reached the level they did today.[10] To be more specific, the introduction of an explicit formal language that seems to govern the entire reasoning process did not take place. What I have in mind is the sort of proof one would find in mathematical texts up to the 17th and 18th century. Proofs of this type need not be absolutely convincing. There is room for doubt (as Lakatos has shown quite explicitly). One might be tempted to accept the proof but there is no necessary need to do so. In such circumstances, it is clear that something additional is required. It cannot be something *in* mathematics, it has to be something *outside*. But then given its empirical roots, why not try out an experiment? Thus, a fellow mathematician gives me a shoddy proof of Goldbach's conjecture. After reading it through as carefully as I can, I am not convinced at all of its correctness, so what I do is to try out a couple of tests. These come out positive. This does not convince me that the proof is (after all) correct, but it surely helps to strengthen my faith in its correctness.

Am I not contradicting myself here? Did I not, in the introduction reject the idea of a computation as an experiment for the simple fact that to any computation there corresponds an arithmetical equation and that equation can be proved in a suitable part or fragment of arithmetic, i.e., my claim (C2)? Within the standard picture, this is indeed so. But, within this empirically oriented picture, a computation can be interpreted as a genuine experiment. To make clear the distinction, let me have a second look at one of the examples I gave in my [1995]:

> I can remember reading years ago that the probability of two positive integers, chosen at random, being relatively prime is $6/\pi^2$. It seems that one R. Chartres, in about 1904, tested this mathematical result experimentally by having each of fifty students write down at random five pairs of positive integers. Out of the 250 pairs thus obtained, he found 154 pairs were relatively prime, giving a probability of 154/250. Calling this $6/x^2$, he found $x = 3.12$, while $\pi = 3.14159...$ (Honsberger [1970: 3])

I commented that 'the result $\pi = 3.12$ does not force us to reconsider the value of π. Thus, although such experiments are perhaps possible, they are completely uninteresting and hence of no importance for mathematics as such'. But suppose now that, due to the different standards of proof, the result is not all sure, namely, that the probability of two positive integers, chosen at random, being relatively prime is $6/\pi^2$. The experiment is done and a result comes out that says, e.g., $\pi = 1.5$. Then surely I have good reasons to doubt the proof. Given the standard picture, I will undoubtedly question the way the experiment is performed and invoke the 'hazards' of probabilistic reasonings.

In short, what I claim is that, although perhaps the very same set of actions is performed, their weight and their role in the mathematical undertaking is interpreted in an entirely different way. The problem is not on the level of activities taking place or not, but on their interpretation(s).[11]

Another possible objection is that perhaps for some (if not very few) mathematical theorems or theories we can design a sort of experiment, but for the most (if not major) part mathematics is so abstract that it really stretches the imagination beyond limits to believe that there is something empirical about it. Here I can rely on an over-used if not over-rated argument, namely the Löwenheim-Skolem result. Any theory, however complex, must have among its models countable models. Hence, finite fragments of these countable models can always be implemented in the 'real world'. If it is a reasonable assumption, as I believe it is, that a finite part of a countable model can be relevant to the whole model, then, in this sense, experiments will always be possible, and will be able to play a decisive role in certain cases.

But, one might remark, are these models not very often of the 'weird' kind? It is then reasonable to talk about an experiment? Should we not expect a direct or obvious link between what the mathematical entities and the real-world entities? No, we should not. I could easily argue that in, say, the physical sciences, such direct links are often missing–just think of elementary particle physics–but let me present a simple example that shows quite obviously that there need not be a direct or obvious connection between an experiment and a mathematical problem.

Example: What mathematical statement is connected to the following experiment? Take a square piece of land, its side being 99 meters. Make a grid, side 1 meter, and plant a tree on the intersections, thus 10.000 trees are planted. Position yourself at one of the corners of the land. Then there will always be directions such that no tree will block your view.[12]

Summarizing, as far as (a) is concerned, I am deeply convinced that this sketchy presentation could be reformulated into a coherent picture that would present mathematics as an empirical enterprise much as it is done in physics. Therefore, if (a) is possible, why did (b) 'make it'?

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11. A second example I gave was Plateau's problem that deals with surfaces of minimal tension given certain boundaries. I wrote in my [1995]: 'suppose that a soap film turned out to have a completely unexpected geometrical form, then this would be devastating for the underlying physical theory but not necessarily for the underlying mathematics'. In fact, reality has contradicted me. Mathematicians Hu, Kahng and Robins found soap films that did not fit the mathematical solutions. No new physics was discovered, but the mathematical apparatus was adapted to take into account the thickness of the film. And all was well again. In this case, the experiment did have an effect on the mathematics (although not in the strong sense that a proof was found to be mistaken), in contrast to my claim.

12. Suppose that the corner where you are standing is the origin (0,0) of an imaginary coordinate system. Each tree has integer coordinates (x,y) where $0 \leq x,y \leq 99$. Take the line of sight that corresponds to the equation $y = \sqrt{2}.x$. If there was a tree on this line, then there would be a n and a m, such that $m = \sqrt{2}.n$, or $\sqrt{2} = m/n$. But that is impossible. Hence, no tree stands on this line (though, because of its thickness, it can partially overlap this line). Actually, there are an infinity of clear lines of vision, namely all lines corresponding to $y = \sqrt{p}.x$, where p is a prime. I leave open the question whether this could count as an experimental check of the infinitude of primes.

The standard answer would most probably run along the following lines: mathematics, although perhaps empirical in its origin, soon found out that its true domain of study are abstract objects. The knowledge acquired about abstract objects stands quite apart from the knowledge one acquires about the real world. Roughly we have *a priori* knowledge on the one hand, *a posteriori* knowledge on the other hand. No wonder then that mathematics stands apart. *A priori* knowledge is (often) considered to be necessary knowledge, hence the certainty that mathematics provides.

Now ask yourself whether the history of Western philosophy could be told without any reference to abstract objects, numbers, geometric elements, divine entities, God, to name but the most important ones. Clearly not. Thus, the conclusion seems almost trivial: of course, mathematics had to develop along the lines of (b). The whole cultural setting was such that any other development was almost excluded. Actually, a very strong case can be made for this point of view. Just one tale-telling example. The quote that follows has been taken from a letter addressed to Grace Chisholm Young, dated June 20, 1908:[13]

> I have never proceeded from any 'Genus supremum' of the actual infinite. Quite the contrary, I have rigorously proven that there is absolutely no 'Genus supremum' of the actual infinite. What surpasses all that is finite and transfinite is no 'Genus'; it is the single, completely individual unity in which everything is included, which includes the 'Absolute,' incomprehensible to human understanding. This is the 'Actus Purissimus' which by many is called 'God'.

The author: Georg Cantor. I do realize that to produce one example might be extremely biased, but it does show that the connection is possible. And did not Hilbert talk about a 'paradise' when he referred to Cantor's transfinite number hierarchy and did not Gordan say 'Das ist nicht Mathematik, das ist Theologie', referring to an existence proof written by Hilbert?

All this does not mean that your average mathematician is a full-scale Platonist or essentialist. The *a priori* 'obsession' belongs to the tacit background knowledge. There is no need to affirm it explicitly, since it has already permeated the whole of the mathematical world. Are not mathematical truths discovered (instead of constructed)? Or, more basically, the fact that there are mathematical *truths* (instead of 'bare' statements)? Or the basic faculty of mathematical intuition to gain this *a priori* knowledge (instead of the basic faculty of investigating (imaginary) fictions or scenarios)?

In fact, what is wrong with a fictionalist account? There are no such things as abstract objects, but what we do have are helpful fictions. Remarkably enough, this position is usually rejected by invoking the importance of being

13. See Dauben [1979: 290].

consistent. If, say, numbers are fictions, then there is no difference between the number five and Sherlock Holmes. Both exist in fictional worlds and help us to achieve a better understanding of this real world. But then, do we not want a difference between the number five and Sherlock Holmes? Suppose we do. What does differentiate them? Many argue along the following lines. If we were to find out that Sherlock Holmes does actually have inconsistent properties, then this would not mean the end of Holmes. If in one story he is six feet tall and in another one five feet something, this does not reduce Holmes to the realm of *impossible* fictional objects. But if the number five did have such inconsistent properties, this would mean the end of the number five. Herein lies the difference.[14]

However, the distinction is, I believe, not solid. Three short remarks to defend my case:

(a) If in one and the same story Sherlock Holmes would appear both as a man and a woman (biologically that is, not disguised as), then that story has a deep problem that will puzzle any reader. In other words, our generosity to keep fictional literary characters alive, is not without bounds;

(b) At present we do not even know and, if metamathematical results are invoked, we will very likely never know whether mathematics is consistent or not (such that the proof or argument is convincing). Hence, as a *criterion* for a distinction this is not very helpful;

(c) We have at present such a thing as inconsistent mathematics.[15] Unless one is to reject this as not being proper mathematics, the whole consistency issue is besides the point. If I insist on using the term 'obsession', it is precisely for the tenacity with which defenders of the existence of abstract objects, hold on to their position.

Summarizing, that (b) became the dominant view seems perfectly understandable if one takes a view that situates mathematics as part of the culture wherein it occurs. I am defending the position that the *a priori* idea was injected into mathematics from the outside rather than as a result from or a by-product of the mathematical activity itself. But this is just saying that to do mathematics (or whatever it is you are doing) is not the same thing as to solve your epistemological and ontological problems about mathematics (or whatever it is you are doing).

14. This is my summary of the position defended by Katz in his [1995], especially footnote 2.
15. The very first book on this topic is now available: Chris Mortensen's [1995]. In terms of papers, discussion notes and the like, the issue has been around for quite some time. For historical background, see Priest, Routley and Norman [1989].

4. Conclusion

As I said in the beginning, the aim of this paper is quite modest. First, I have tried to gather some material to show that other pictures besides the standard picture are possible. Secondly, at least one of them leaves room for a genuine idea of mathematical experiments. Furthermore, that specific picture is definitely not the standard picture. Thus, the inevitable conclusion: if you are eager to talk about mathematical experiments in the full sense of the word, it is time to reconsider your philosophical options, because Platonism-*cum*-experiments will not do the job. Although I focused here on a fictionalist account (thereby expressing my personal preference), this still leaves room for a realist account, but then this will have to be a realism *à la* Maddy, as the only world still available to get the realism working, has to be the 'real' one. But that is (according to my preference) another story.

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