Better to call a Mathematician a Pluralist than a Formalist.

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In this paper we try to convert the mathematician who calls himself, or herself, "a formalist" to a position we call "methodological pluralism". We show how the actual practice of mathematics fits methodological pluralism better than formalism while preserving the attractive aspects of formalism of freedom and creativity. We extend the freedom and creativity to include not only interpretation or application - as formalist does - but to also include presentation of a theory and methodology of proof.

In the first section, we give a characterization of formalism. We draw our characterisation from Detlefsen by allegiance to his careful work on Hilbert and formalist philosophy. In the second section, we quote some mathematicians who explain to us why they think that formalism is attractive. In the third section, we analyze three cases of actual mathematical practice where the characterisation of the philosophical position called "formalism" does not fit the practice. In the final section we shall propose our alternative view: "methodological pluralism", which preserves some of the insights of formalism, but provides a fuller account, which better fits mathematical practice.

Before writing anything more, we should add a note about the term "pluralism": methodological pluralism is part of a larger, more general, pluralism, which is currently being developed as a position in the philosophy of mathematics in its own right.[1] Having said that, henceforth, in this paper, we abbreviate "methodological pluralism" by "pluralism".

§1 Characterisation of Formalism

We begin with Delefsen's characterisation of formalism. We end the section with a general description of formalism.

1.1 Detlefsen's Characterisation of Formalism

Formalism is a philosophy of mathematics, which was developed in the late nineteenth century and at the beginning of the twentieth. To be more precise, it is not simply one philosophy.[2] Following Detlefsen's careful characterization of formalism,[3] we can think of

[1] For example see Friend (2011)
[2] Hilbert's relationship to formalism will be discussed later.
[3] See Detlefsen (2005), pp. 236-237.

formalism as a family of positions, each member of which has all of five characteristics.[4] The difference between positions is found in the interpretations of the characterizations of the relative weighting of the characteristics.

(1) The first, is that geometry no longer sets the standard for rigor.[5] Instead, the standard is set by arithmetic. This follows from a conception of rigor which is particular to formalism. For the formalist, rigor follows from an act of abstraction away from intuition instead of intuition beeing embedded in a theory, and informing our conception of our rigor (which is what we often find in pre-Hilbert and Tarski presentations of geometry).

Amongst the formalists, there are different standards and conceptions of rigor. For Hilbert, rigor consisted in using a precise step-wise or finitistic proof method based on the finite number of “strokes on a line”.[6] Other formalists use different bases for rigor. For example, they might use a particular syntactic system of proofs.[7] The conception of rigor places a methodological constraint on the notion of “best practice”.

(2) The second characteristic is the formalist "rejection of the classical conception of mathematical proof and knowledge. From Aristotle on down, proof and knowledge were conceived on a *genetic* model".[8]

(3) The formalist rejects also "the traditional 'presentist' conception of rigor which saw it as consisting primarily in the keeping of an object continuously before the visual immagination or intuition of the prover during the course of a proof".[9]

(2)-(3) The second and the third characteristics represent the fact that formalists reject the idea that mathematical proof should be based on a “genetic” model of proof, because they do not believe that we have knowledge of mathematical truths (or just mathematical contents), through having knowledge of their origins and causes (although this might, of course, be useful for some learning purposes).

As a result, formalists draw a strong distinction between the learning of mathematics and the content of mathematics. The content has an ideal presentation. This may be manifested by means of "properly rigorous" proofs. Or, the presentation might not be in terms of proofs but rather that we should present theories axiomatically, as required by Bourbaki. That is, the formalist does not believe that either: the mathematician’s displaying of proofs

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[4] A note of caution: while we often gloss Hilbert as a formalist, we follow Hallett’s caution against calling Hilbert a formalist.[4] It was Brouwer who first called him a formalist! Nevertheless, as we shall see, Hilbert’s position is one in the family of formalist philosophies. (Hallett, 1995) p.45.

[5] Aristotle thought of geometry as setting the standard for rigor, since Euclid had presented geometry axiomatically. It was not until we discovered the independence of the parallel postulate, and that we could give an axiomatic presentation of arithmetic (Dedekind and Peano 1888 - 1889), that it was conceivable that arithmetic should take over the position of setting the standard.

[6] See, for instance Hilbert (1923), which represents his mature work on proof theory.

[7] See, for instance, Troelstra and Schwichtenberg (2000).

[8] Shapiro (2005), p. 236-237.

[9] Sahprio (2005), p, 237.

or the content of mathematics, should be tied to the origins of a problem or to the history of a method.

Before the mid nineteen century, mathematics was past from master to student, and was inextricable from the history of the development of that part of mathematics. Mathematics was an art. It was not presented abstractly as *fait accompli*. This is the historical genetic conception of mathematics. But there is another genetic conception, and that is the conceptual genetic conception.

Russell and Brouwer had a conceptual genetic conception. In their Principa Mathematica, Russell and Whitehead stated that all mathematics should be built within logic. They had the idea that logic is the genesis or "cause" of mathematics[10]. Brouwer had quite a different view of the causes, or genesis, of mathematics. For Brouwer, the only possible foundation for mathematics is intuition: "to exist in mathematics means: to be constructed by intuition."[11]

The formalist who adopts characteristic (3) disagrees with the historical genetic conception and with the conceptual genetic conception. As a result, for such a formalist, proofs should contain no "historical" or "story telling" element, or tracing back to something more basic, such as logic. Proofs are purely formal exercises.

(4) The fourth characteristic of formalism *safeguards* the idea that proof is a purely formal exercise. The characterisitic is "its advocacy of a nonrepresentational role for language in mathematical reasoning".[12] "Non-representational" means, here, "not tied to (or held responsible to) an interpretation". The idea is to move away, not only from intuition, but also from subjective interpretations. Mathematics should not be thought of as an art, passed on a tradition of transmission from teacher to student, where this is necessarily geographically and linguistically parochial, but as an international, objective science. For Detlefsen, (4) is "perhaps [the] most distinctive component of the formalist framework".[13]

Like (2), (4) turns into a methodological constraint when it is taken to prescribe the proper presentation of a mathematical theory. The methodological constraints turn out to be quite liberal. And this hails the fifth and final point: the creative component of formalist positions.

(5) Mathematicians have a freedom to create and work with different reasoning tools in order to get genuine knowledge. Pared of all content, the constraint consists only in the

[10] In Russell and Whitehead (1910) they enforce the notion of genetic method of construction for mathematical entities already presented in Russell's *Principles of Mathematics*.

[11] Brouwer (1975), p. 96.

[12] Detlefsen (2005), p. 237.

[13] Detlefsen (2005), p. 237.

insistence that mathematics should be consistent, and consistency should be the sole guarantor of existence. The rest is free play.[14]

In historical and philosophical formalism we can find these five characteristics[15]. To better fit present day conception of formalism we do not hold the formalist to the first characteristic. In particular, present day mathematicians hold to (5) as the marked advantage of formalism over other philosophies of mathematics.

1.2 General Description of Formalism

The mathematicians who call themselves formalists tell us that they feel that, as formalists, that they prize the freedom and creativity formalism brings to mathematics. They are free to interpret the symbols as they choose – give them any interpretation which fits the formal constraints of rigor (characteristic (4)). For Hilbert, and our formalist, mathematics is only to be thought of as the collection of all mathematical theorems ever given in history, where the theorems are generated in axiomatic theories, and that is all.[16] Theories do not possess specific contents. Rather, they, together, form a web connecting sentences to sentences and theories to theories. The nodes and web and are divorced from the original concepts which motivated formal theories as Russell or Brouwer believed, with their more genetic conception of mathematics.[17] Ultimately, the objects of a formal mathematical theory are uninterpreted, they are simply structures which we can fill with arbitrary objects or concepts. As Hilbert said: "One must be able to say at all times — instead of points, lines, and planes — tables, chairs, and beer mugs."[18] Hilbert's re-construction, or re-conceptualisation, of mathematics is based on whole axiomatic theories rather than single concepts or entities. In this respect what is important are not specific mathematical objects, but rather their inter-relations and the structures they make.

For a strong formalist, these structures are *all there is* to the *meaning* of mathematics. It is this severing of the umbilical cord linking mathematicians to the genesis and ontology of the theory that gives the mathematician so much freedom. In its mature guise, formalism is

[14] This formalist proposal was controversial. Brower had strong objections to consistency playing any important role in mathematics: "the question whether a certain language is consistent, is not only unimportant in itself, it is also not a test for mathematical existence." (Brouwer 1975 p. 101.)

[15] Detlefsen (2005), p. 237.

[16] Of course, there was a lot of mathematics in the past which was not axiomatised. But Hilbert's idea was that, in principle, it could all be axiomatised, as Boubaki tried to implement.

[17] This refers to Detlefsen's third characteristic about the genesis of a formal theory. Hilbert would agree that we might well **start** with some concepts, but at some point the formal representation has to take over and leave behind the original motivation. Otherwise we are guilty of the genetic fallacy.

[18] Hilbert made his famous statement at the Berlin railway station. Many authors use this example. A reference can be found, for example in Shapiro (1997), p. 157.

developed into a structuralist philosophy, which resonates with aspects of Hilbert's axiomatic program.[19]

Under the strong formalist the objects of study of mathematics are quite ethereal, since they are places in a structure. This is *all there is* to mathematical ontology for *ante rem* structuralists and formalists. Application, or the filling of these places is quite a different matter. The places can be filled with more common objects, mathematical objects or never be filled at all. In other words, when mathematics is applied, objects from outside mathematics are supplied to occupy the object places in an already existing mathematical structure. Applications are an afterthought. This is not to say that a field of application cannot, or never, stimulates the development of mathematical theories. The point is not historical or psychological, but conceptual. Particular applications might stimulate mathematicians to develop, or discover a mathematical structure, but the structure is conceptually prior, not epistemically prior. That is, the developed mathematical structures are free-standing. They do not depend (except maybe historically) on applications. Putting the claim another way around: should an application which stimulated the development of a mathematical structure turn out not to fit the structure very well any more, the structure remains as a pure mathematical theory. Mathematicians will then look to change the theory, or modify the theory to fit the application better. The original theory is not thrown out, just no longer applied. It joins the cannon of pure mathematics. *Grosso modo*, for the formalist, justification for a mathematical theory comes, not from ontology application or genesis, but from elsewhere: from consistency of the theory, from rigor, precision and demonstrability. That is all.

For a formalist, models, or interpretations, can only be used to prove consistency, not origin.[20] We might think that the formalist would allow models or interpretations to show the intended interpretation. But the original intended interpretation is a sort of story one tells about how we came about thinking of a set of axioms. Once the axioms are developed, and the formal system is sufficiently defined to "run on its own" we are free to completely disregard the intended interpretation. The "origins" story, or the "intended interpretations" are not part of pure mathematics, only, a regrettable part of the heuristics.

To summarize the formalist position, recall that is a family of positions. The attractive features are the freedom and creativity granted by the divorce from the genesis of theories, from any sense of independent ontology and from intended interpretations. How one then

[19] The idea of a structure fully determining ontology is echoed, for example, in Shapiro's ante rem structuralism. As Shapiro says: "On my view, mathematical objects are places in structures, and these structures exist independently of any non mathematical systems that exemplify them. To characterize such structures, I borrowed the term ante rem from metaphysics". Shapiro (2008), p. 286. Though very similar to formalism, Shapiro's ante rem structuralism is an independent position in philosophy of mathematics.

[20] In Hilbert (1902) Hilbert maintains that "the concept 'provable' is to be understood relative to the underlying axioms-system. This relativism is natural and necessary".

grounds a theory, or prevents it from reaching too-heady heights is what distinguishes the different positions.

§2 The Voices of Formalism

Many mathematicians today call themselves "formalists". A large number of working mathematicians have endorsed formalism, sometimes explicitely, sometimes implicitly, as manifested though their presentations of mathematics.
Bourbaki, for example, affirms that

> From the axiomatic point of view, mathematics appears... as a storehouse of abstract forms - the mathematical structures; and it so happens - without our knowing why - that certain aspects of empirical reality fit themselves into these forms, as if through a kind of preadaption. Of course, it can not be denied that the most of these forms had originally a very definite intuitive content; but, it is exactly by deliberately throwing out this content, that it has been possible to give these forms all the power which they were capable of displaying and to prepare them for new interpretations and for the development of their full power.
> It is only in this sense of the word "form" that one can call the axiomatic method a "formalism".[21]

We diagnose that the two main reasons for mathematicians endorsing formalism are that:

(1) Formalism allows mathematicians to practice their freedom of invention within a rigorous framework. As Detlefsen remarks: "the mathematician, qua mathematician, has a freedom to create instruments of reasoning that promise to further her epistemic goals."[22]
This free epistemic pursuit is undertaken in a framework of rigor. The standards, or characterization of rigor, however, must not be thought of as stipulated in advance. The history of mathematics, shows us that the standards of rigor have changed over time.

(2) Formalism seems to avoid traditional, genetic, intuition-based or interpretation-based philosophical commitment to ontology, existence or absolute truth.
This second reason is a reaction against traditional foundationalism in mathematics, as seen in realism, logicism and constructivism.

> Many working mathematicians (though by no means all) are suspicious of logicians' [and philosophers'] apparent attempt to take over their subject by

[21] Bourbaki (1950), p. 231.
[22] Detlefsen (2005), p. 237.

> stressing its foundations. …[Moreover,] I have been persuaded by Edwin Coleman that foundationalism in mathematics should be regarded with considerable suspicion; or at least that proper 'foundations', …would be much more complex and semiotical than twentieth century mathematical logic has attempted. In which case it would be arguable whether 'foundations' is an appropriate term.[23]

Other mathematicians hold a schizophrenic position between traditional realism and formalism. The schizophrenia is described by Moschovakis:

> Nevertheless, most attempts to turn these strong [realist] feelings into a coherent foundation of mathematics invariably lead to vague discussions of 'existence of abstract notions' which are quite repugnant to a mathematician. Contrast this with the relative ease with which formalism can be explained in a precise, elegant and self-consistent manner and you will have the main reason why most mathematicians claim to be formalists (when pressed) while they spend their working hours behaving as if they were completely unabashed realists.[24]

To whom H. C. Dales replies with a reversed schizophrenia that

> It seems to me that most mathematicians really are formalists for all the days of the week. It is of course very useful when seeking proofs within the formal system to have a 'realistic picture' in one's mind, and so it is temporarily convenient, during the week, to be a realist, but it is the realism that the mathematician does not really believe in.[25]

Not all mathematicians feel the need to have a realist picture or to pay lip service to realism. For example, Abraham Robinson held an unabashed formalist position. He wrote about the foundation of mathematics that:

> My position concerning the foundations of mathematics is based on the following two main points or principles:
> i) infinite totalities do not exist in any sense of the word (i.e., either really or ideally). More precisely, any mention, or purported mention, of infinite totalities is, literally, meaningless.
> ii) Nevertheless, we should continue the business of Mathematics "as usual", i.e., we should act as if infinite totalities really existed.[26]

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[23] Mortensen (1995), p. 4.
[24] Moschovakis (1990), p. 320.
[25] Dales (1998), p. 185.
[26] Robinson (1965), p.232.

Along these lines Nelson gives a passionate “apology for formalism”:

> What we devote our lives to is seeking for proofs; if a proof follows the formal rules, it is correct; if it does not, it is not a proof and is worthless unless it suggests a way to find a proof. No other field of human endeavor has maintained such a consensus over such a vast extent of space and time.
> […]
> Formalism denies the relevance of truth to mathematics. But, one might object, mathematics works – the evidence is all around us. Does this not imply that there is truth in mathematics? Not in the slightest.
> […]
> In mathematics, reality lies in the symbolic expressions themselves, not in any abstract entities they are thought to denote. The symbol ∃ is simply a backwards E. If we conclude that a certain entity exists just because we have derived in a certain formal system a formula beginning with ∃, we do so at our peril. The dwelling place of meaning is syntax; semantics is the home of illusion.[27]

In other words, formalism fits very well with many mathematician’s reported perceptions of present day mathematical practice. Confirming this, in the article (REFERENCE) Serre's speaks also of Bourbaki:

> Q: What has been the influence of Bourbaki on mathematics?
> A: A very good one.

Now that we have read some testimonies from mathematicians, we should turn to the practice of mathematics. The mathematicians we quoted describe themselves as formalists – with some realist leanings, but maybe their behavior tells another story.

§3 How Formalist Mathematics Should Look

In the next section, we shall look at some test cases, but before we should give some idea as to how we are to judge them. What should the practice of mathematics look like, according to the formalist? Let us give two different views: (I) Hilbert-style formalism emphasizeds consistency, reliability and fruitfulness. (II) The Bourbaki conception requires an axiomatic presentation of mathematical theories. The inferential process (proof procedure) involved should be very strict, since there is nothing else to safeguard against nonsense. Proofs should be thorough. Each step should rigorously follow from the previous

[27] Nelson (1997), p. 3.

steps. A proof, for Hilbert takes place within a theory.[28] For Hilbert and Bourbaki the theory contains axioms and rules of inference. The rules account for each step in a proof. A rigorous proof, then, consists in stating some axioms, using only allowed rules of inference of the formal system of proof of that theory, and coming to a conclusion. Natural deduction proofs from logic are perfect examples.[29] We deviate from a Hilbert or Bourbaki proof when we:

(1) fail to specify which theory we are working in,
(2) import foreign axioms
(3) use rules of inference not in the proof theory
(4) fail to completely formalize our proofs (or fail to show that we could do this in principle), or
(5) leave unexplained gaps in our reasoning.

The importance of sticking to the strict methodology is that if we have proved the theory to be consistent (or equi-consistent with another theory) then, by following the proof theory – the given methodology - we ensure continued consistency. Losing consistency is a real danger, because, as we know, rival[30] formal mathematical theories contradict each other. Do mathematicians follow the precepts of the formalist? We look at three cases.

§4: Three Test Cases

4.1: The classification of Finite Simple Groups.

The first case concerns "big projects". In these, mathematicians divide the main goal into different sub-goals each of which is again divided into other sub-goals, or "cells". This cell-structure allows mathematicians (and computers) to work almost in parallel. Each cell works on specific problems (that are not always directly connected with the main goal, but that are necessary for its success). The success of the project depends on the success of the work of the cells. "Success" consists in finding the solution to a problem, such as classifying mathematical theories. Roughly speaking the task of people working in a cell is to prove theorems. Because "achieving the goal" is important, the mathematicians and

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[28] In Hilbert (1996) Hilbert claims that "the development of mathematical science as a whole takes place in two ways that constantly alternate: on the one hand we drive new provable formulae from the axioms by formal inferences; on the other, we adjoin new axioms and prove their consistency by contentual inference".
29 This is not the only standard of rigor. For a structuralist as Shapiro, for example, it is enough that the proof be accepted, or acceptable, by working model theorists and set theorists.
[30] We use here the word "rival" in the sense Beall and Restall use it in their *Logical Pluralism*. See Bell and Restall (2006).

computers working in a cell avail themselves of whatever it takes to prove the theorem of that cell.

An actual example is the case of the classification of finite simple groups. This mathematical endeavour started more than a century ago and ended in 1983. It has been a collective work, resulting in thousands of pages in books, articles and manuscripts written by many different mathematicians. The collection of work is essentially (especially if we are thinking as a formalist) one long proof, of one long theorem. The classification is a type of theorem, or "solution". The "proof" is fragmented into many sub-proofs. It is a collection of a very large number of different proofs made with different techniques on different topics. The general proof is "unsurveyable by a single human being".[31]

Is there a problem with mixing methodologies? There is some controversy concerning the classification of the so-called "quasi-thin" groups. The work of the mathematician, and Abel prize winner, Jean-Pierre Serre showed how this could be regarded as a gap in the larger proof of "the theorem".[32] The gap is due to the length and the structural complexity of quasi-thin groups. The criticism of Serre addresses the fact that the dishomogeneity of the general proof for finite simple groups does not allow us to prevent *further* gaps arising that have not yet been discovered and fixed.

Note that this is not a side issue. The classification of quasi-thin groups is a key step for main goal of the classification of finite simple groups. Quasi-thin groups were announced to be classified in the early 80's by Geoff Mason but in fact they were not. In Mason's proof, critical gaps were left, due to the "proliferation" of unexpected groups. Only in 2004 Michael Ashbacher and Stephen Smith give a complete proof in two volumes, running to more than 1200 pages.

The problem with the proof of the classification of finite simple groups is that the proof itself is so huge and impenetrable, made by so many different "pieces" that, by quoting the words Daniel Gorestein (the mathematician who had the idea of the classification and who oversaw the project till his death)

"it will gradually become lost to the living world of mathematics, buried deep within the dusty pages of forgotten journals".

This is contrary to the methodological idea of proof the formalist has in mind, since this quasi-empirical incapability of handling the proof makes the proof itself not rigorous (the situation is similar to the case of computer-based proofs). This is a strong reason why the classification project does not fall neither in the Hilbert-style formalis, nor in the Bourbaki version of it.

What is surprising, if we remember conception (I) of formalism from the last section, on rigor of proof and working within a declared theory, is that these gaps seem to have brought no discredit to the results of "the theorem". This is a typical example of a work of

[31] Otte (1990), p. 61.

[32] See, for instance, Raussen and Skau (2003)

mathematics which hardly fits into a strict formalist framework. The resulting classification is hardly a demonstration in the formalist's sense.

We might think that this is because the "demonstration" is unsurveyable. But this is not the major problem, since what "surveyable", "finitist" and "demonstrable" mean can be flexibly interpreted to fit this case. The material is, after all, gathered in a two-volume work. What is more damning is that we see examples of mathematicians deviating from a strict axiomatic system. This enough to discredit the project from the Bourbaki conception.[33]

We claimed above that many mathematicians today consider themselves to be formalists. If those working on the classification of finite groups also consider themeselves to be formalists then, at the very least, they are not strong formalists, at worst they do not follow their own tenets. They exercise their freedom and creativity in an unconstrained manner. Mathematicians almost never give what a formalist would count as a proof. This is not just a question of shortening proofs to make them more perspicuous, but, here, the mathematicians use "illegitimate" techniques in proof. In our test case (and in many other instances as well) we can find what we shall call "deviant" proofs. These are "proofs" where mathematicians use steps which deviate from the rigorous set of rules, methodologies and axioms agreed to in advance. Of course, shortcuts can be useful to speed up a proof without any danger of inconsistency, or we can change our minds about the methodology or proof theory we are using. But, strictly speaking, a straight shortcut can be proved as a lemma, and therefore, the steps could be filled in upon request. Here we are interested in something else. Deviant shortcuts or detours can help to circumvent an impasse which *could not* be overridden with the standard steps agreed upon in advance.

There is a rebuttal against the argument. One might think that the argument has mis-fired. After all, the classification of all finite simple groups is hardly "a theorem". It is a classification. The project is not to prove a theorem, but to give a meta-level result. It was never meant to be "carried out within formal theory", with axioms and rules of inference given in advance. Thus, we have relied on a metaphor for our argument to show that mathematicians are not really formalists, but the metaphor does not carry.

Here is our counter-argument to the rebuttal. It is correct to say that we have stretched the metaphor in saying that "the classification is the theorem" – which we are trying to prove. It is also correct that no umbrella formal theory was agreed upon "in advance" for "proving the theorem". However, even if the work is being done at the meta-level, this should not entail that all standards are dropped. The classification does require careful definitions, it does require proofs – that a particular group or class of groups falls under a particular classification. These proofs – even if they are carried out at the meta-level, are still proofs, which are checked for correctness and so on. The results of these proofs are gathered

[33] Of course the Bourbaki program was to take existing theories and re-present them as axiomatic theories, and maybe this could be done. Nevertheless this has not been the practice in this case.

together as a unified result – about one subject: finite simple groups. If we are to disregard the genesis of the idea of a finite simple group, and still think of these as forming one concept, which can be classified, then we had better be using the same means for proof of our classification! Thus, the metaphor does hold sufficiently for us to make our argument. If this is not fully convincing, then it is up to us to find a better example. There are several "big' mathematical projects being carried out today, and they show the features we are interested in – a lack of adherence to one "method" of proof, and therefore run the risk of inconsistency in methodologies. The conclusion we wish to draw from the example is that the formalist theory is too strict in respect of proof and meta-theory. The obverse of the strictness is oversimplification. The phenomenon of "big projects" requires a more subtle treatment to explain the acceptance, by mathematicians, of the results of such projects.

As we see from the above example, the mathematical practice displays pluralism in methodology, which runs directly against the formalist conception. This is for the very good reason that pluralism in methodology might generate an inconsistency. The pluralist agrees that this is a possible danger in importing different methodologies. In light of these concerns, the pluralist believes that we should have a sense of ideal, formalistically acceptable proof within a mathematical theory; that deviation should be flagged and carefully scrutinized; but not that it should be banned. The scrutiny urged by the pluralist is systematic.

First we declare our allowed methodologies, theories, proof procedures and so on. Call the latter "the methodology agreed upon in advance". If we find gaps, we prove (using the methodology agreed upon in advance) that they can be filled with the methodology agreed upon in advance. If we are importing foreign methodologies, axioms, rules etc., then we need to determine whether adding these to the methodology agreed upon in advance will create an inconsistent theory. There is no danger when the foreign and native methodologies all belong to a larger theory. In this case, we ought to have agreed to the larger theory in advance, and we simply change what we declare as our "allowed methodologies, theories and proof procedures". But this is not always possible, as we shall see in the next two examples.

4.2 Renormalisation.

Another problematic case is the mathematical procedure of renormalization. It is a procedure used in physics, especially in quantum electrodynamics, to eliminate infinite quantities during certain types of calculations. In these physical theories integrals represent observable physical quantities, which often diverge towards some specific limits. To avoid these divergences being infinite – and therefore uncalculable – renormalization is used as a mathematical reformulation of the theory, which allows us to eliminate these divergences. Basically, the procedure cuts off the divergences at a calculated number and this allows us to obtain finite (rational) values. This move has a strong practical justification, since the mathematical divergences do not seem to affect the physical results – accordance with the measured data and predictions. Nonetheless, there is a problem with the explanatory power of the procedure. The particular cut-off points decided upon by the physicist are mathematically *ad hoc*, and so, unjustified. But the situation is worse than this. Renormalisation leads to conceptual and mathematical inconsistency.

For example, strictly speaking, in renormalizing, we subtract infinities from infinites in order to get a particular finite result. This is mathematically inconsistent. However, this is the mathematical process at the base of renormalization. The procedure is deviant, for a formalist, because there is a back and forth play between physics and mathematical calculation. We might say that renormalisation is a process that launders the inconsistency of the results from mathematics with the magic soap of application provided by physics. The physics “tells us” to fix the pure mathematical results so that we never deal with infinite values.

Although the use of renormalization is standard especially in Quantum Field Theory, the criticisms of its intrinsic mathematical inconsistency remain. The physicists themselves are also uncomfortable with renormalization. Lewis Ryder summarizes this way:

> "In the Quantum Theory, these [classical] divergences do not disappear; on the contrary, they appear to get worse. And despite the comparative success of renormalisation theory the feeling remains that there ought to be a more satisfactory way of doing things".

The "unsatisfaction" is about the continuous back and forth between the mathematical theory used to renormalize the divergencies and the physical theory where those divergencies appear. As Feynaman puts it:

> "The shell game that we play ... is technically called 'renormalization'. But no matter how clever the word, it is still what I would call a dippy process! Having to resort to such hocus-pocus has prevented us from proving that the theory of quantum electrodynamics is mathematically self-consistent. It's surprising that the theory still hasn't been proved self-consistent one way or the other by now; I suspect that renormalization is not mathematically legitimate".

So the practice of renormalization does not fall under Hilbert style formalism. By the very nature of the two layers structure of renormalization, we can say that it does not fall under Bourbaki's axiomatic version of formalism.

Renormalization contrasts with the rigid constraints the formalist account imposes! Here the supposed formalist allows the application, or the interpretation, to shift the mathematical results. Freedom and creativity abound, but adherence to formalist constraints are lacking. What we see is either very dubious scientific practice, or a pluralist methodology.

The pluralist insists on vigilance and care in these situations. It is quite right that the real world, to which we apply the mathematical theory sets parameters on the possible mathematical theories we can effectively use. But reality only sets parameters. For this reason we should be very careful in trusting our particular mathematical theory to completely and closely model reality. For this reason, we have to be particularly vigilant when letting the mathematics predict physical outcomes when these are not verified by the application, indeed the application would have us modify the mathematics in a mathematically *ad hoc* way. That is, the mathematical theory is not perturbed by the presence of infinite values. It is the application that "tells us" that infinite values are impossible, and so we modify the mathematical results. These adjustments to the original mathematics is done systematically: whenever a "physical" value is calculated to be infinite replace the value with an appropriate finite number. This sort of "adjustment" is symptomatic of the systemic and pervasive nature of problems in physics of dealing with infinite and infinitesimal quantities. It is a problem that permeates mathematical applications. We temporarily accept the gap between perfect mathematics and measured physical quantities, but we also work to identify and overcome the problem. Here the problem is not with inconsistency within the physical world, or within the pure mathematical theory. Rather, the "inconsistency" has to do with a mismatch between the pure mathematical theory and what we metaphysically suppose, and what we can measure of, the physical reality. Here, the methodological pluralist will not caution against inconsistency, but will endorse more careful use of mathematics, and seek a mathematical justification for what looks *ad hoc*.

4.3 Lobachevsky's model for indefinite integrals

Another example, which is less modern, provides us with a similar case. The Russian mathematician Lobachevsky in dealing with the problem of finding the exact solutions for indefinite integrals, had the idea to apply to the calculus his imaginary (non Euclidean, hyperbolic) geometry. The method of Lobachesvky mirrored the usual technique of using geometry as a model for this kind of operation. However he used a hyperbolic model instead of a Euclidian model. In his "Application of the imaginary geometry to some integrals", he applied what we call today "Hyperbolic trigonometry" to calculate complex integrals. These are the only geometric equivalents of indefinite integrals. This was possible because

> … the limiting surface sides and angles of triangles hold the same relations as in the usual geometry.[34]

It is therefore possible "to develop the Hyperbolic trigonometry on the basis of the usual (Euclidean) trigonometry"[35], and this can, for example, be used to solve certain integrals

[34] Lobachevsky (1914), vol III.

[35] Andrei Rodin, "Did Lobachevsky have a model of his 'Imaginary geometry'?", unpublished manuscript.

"which earlier were not given any geometrical sense"[36]. That is, Lobachevsky recognized how we can "translate" from one world to another, and then

> As far as we are (sic!) found the equations which represent relations between sides and angles of triangle (sic!) [...] Geometry turns into Analytics, where calculations are necessarily coherent and one cannot discover anything what is not already present in the basic equations. It is then impossible to arrive at contradiction, which would oblige us to refute first principles, unless this contradiction is hidden in those basic equations themselves. But one observes that the replacement of sides a, b, c by √-1, √-1, √-1 turn these equations into equations of Spherical Trigonometry. Since relations between lines in the Usual and Spherical geometry are always the same, the new geometry and Trigonometry will be always in accordance with each other.[37]

In this case, Lobachevsky explains how he avoids inconsistency (or where to find inconsistency if it is there). Thus, the mixing of methods does not necessarily lead to inconsistency. This is just one danger.

The formalist overshoots when he insists that all proofs use only one methodology. The pluralist in methodology allows for together inconsistent theories to be used together, provided we are careful to avoid inconsistency, and this is exactly what Lobachevsky does. As Kagan puts it:

> He [Lobachevsky] considered the given integral as a value of length of a certain curve in a hyperbolic plane, as the area of a certain figure in a plane or any other surface, as the volume or mass of a certain solid, and since these were metrical values in hyperbolic space, the consideration he deduced on the basis of imaginary geometry indicated how to find the value of the considered integral. And when this value was found it was frequently possible to find also analytical ways which led to the same goal. The congruency of the results obtained Lobachevsky regarded as confirmation of the correctness of hyperbolic geometry.[38]

Nevertheless, the formalist criteria have been violated. Moreover, the deviance smacks of circularity: to use a new tool to produce "correct results" which themselves are to be regarded as a proof of the correctness of the tool. Nonetheless, by using this method we do actually find the "right results" confirmed by congruence. This recalls the renormalization case, but this time instead of using the physical world to correct or normalize the mathematics, Lobachesvky used a hyperbolic model.

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[36] Andrei Rodin, "Did Lobachevsky have a model of his 'Imaginary geometry'?", unpublished manuscript.

[37] Lobachevsky (1914), p. 34.

[38] Kagan (1957), p. 59.

This seems to confirm the fact that the mathematical practice of proof does not rigidly follow the rules of the logical system supposed to underlie it (proof theory, in our case). As John Corcoran points out, in mathematical practice it is common to find

> sentences beginning with "for purposes of reasoning suppose that". Here suppositions other than axioms are being introduced not as main premises but merely to begin a subsidiary deduction.[...] The myth that a proof is simply a sequence of (declarative) formulas has its usefulness but truth cannot be claimed for it.[39]

The supposition does not just introduce an idealization, but might introduce something quite foreign to the theory. Therefore, following these examples we maintain that mathematicians are not as formalist as they declare, and it seems that the actual mathematical practice is closer to pluralism than to formalism.

§5 The Problem: Diagnosis and Solution

So what has gone wrong? The reasons the mathematicians were attracted to formalism were that it (1) allows for the creativity and freedom of mathematicians and (2) avoids heavy foundational philosophical disputes about ontology and correct universal methodology. These attractive ideas are what led to deviance. But the deviance is deviant to the formalist, and also the realist.

We propose keeping these two attractive ideas in mind and present pluralism as a neat philosophical replacement of formalism. Since it does not encounter the problems we saw with formalism, the adoption of pluralism is a net gain.
Whereas the formalist allows freedom only in interpretation, the pluralist also allows freedom in methodology. However, the pluralist advocates vigilance when “foreign” elements are introduced.
When a pluralism in methodology is being practiced, the pluralist makes two recommendations:
(1) Know what counts as a strict proof within a mathematical theory (axiomatisation is fine as a starting point), but axiomatisation is just about being explicit as to one’s justification, it is not to provide an ultimate justification. This is important because the gaps in an otherwise strict proof will signal deviation. The pluralist does allow deviance in proof. The trick is that we have to know when we are being deviant and when we are not. As explained in the last section, the vigilance can be made quite systematic, it is not just a vague call to caution.

[39] Corcoran (1973), p. 32.

(2) We should bear in mind that when revising, correcting or being critical of a result we should look first to the deviant steps. Then, as with Lobachevsky, we might be able to show that there is no inconsistency.

However, the analysis of the pluralist does not stop here. For example, we might need to re-evaluate the original theory. For example, if our mathematical model of some part of physics (the application of mathematics to "reality") predicted a certain outcome, and we found that the outcome was not what was predicted by the mathematics, then we look first for an error in calculation, second, we look at any deviant elements in the making of the faulty prediction, but thirdly, we might look at revising the whole theory – making a new one, adding axioms, adding rules of inference, modifying or eliminating existing ones, etc. Or, "one may give a perfectly coherent account" provided we only allow limited kinds of information between incompatible theories or methodologies.[40] That is, Priest and Brown give us a model for working with together incompatible theories each of which is internally consistent. While Priest and Brown were interested in incompatible scientific theories (such as quantum and classical mechanics) we can imagine extending this sort of model for cases of using incompatible proof theories, such as in the three cases we examined. We leave this for future work. Such future work is where the pluralist goes beyond the formalist. This was the recommendation made in the second test case.

§6 Conclusion

Methodological pluralism better describes mathematicians' practice than does formalism. We do not need methodological rigidity (to adhere to a particular proof theory) to guarantee consistency when we can use "reality", physical theory or another mathematical theory such as hyperbolic geometry, to sanction the methodological deviance in proof.[41] But when we use deviant methods, we continue our search for mathematical justification, holding the temporary "reasult" (deviantly obtained) in abeyance.

The pluralist likes the fact that under formalism, the symbols and their manipulation is ideally quite rigid, since this forces us to be explicit about our deviance from the norm, and can then guide further research. In other words, rather than hold mathematicians to a rigid standard of rigor, we use the standard of rigor to make us aware of deviance. This is simply practical.

The pluralist is also in agreement with the formalist over the issues of absolute ontology, truth or correctness. These are relative terms – relative to the systems being used at the moment. So the pluralist is free from the metaphysical baggage of ontology, truth and uniqueness of solution.

[40] Taken from Priest (2011) See also, Brown and Priest (2004).

[41] Pluralist does not insist on consistency, since, e. g., she endorses paraconsistent theories. However, she endorses and encourages cross-checking with other theories.

REFERENCES

Bell, J. C., Restall, G. Logical Pluralism, Clarendon Press, Oxford, 2006.

Bourbaki, N. "The Architecture of Mathematics", American Matheamtical Monthly, 57, 1950, pp. 221-232.

Brouwer, L. E.J, Collected Works. Vol. 1. Philosophy and Foundations of Mathematics. A. Heyting (ed.), North-Holland/American Elsevier, 1975. Vol. 2. Ge-ometry, Analysis, Topology and Mechanics, H. Freudenthal (ed.), North-Holland/ American Elsevier, 1976.

Brown, B., and Priest, G., “Chunk and permeate I: The infinitesimal calculus”, Journal of Philosophical Logic, 33, 2004, pp. 379-388.

Corcoran, J., “Gaps between logical theories and mathematical practice”, in Bunge, M. (ed) The methodological unity of science, Reidel, Dordrecht, 1973, pp. 23-50.

Dales, H. C., “The mathematician as a formalist”, In Dales, H. C. and Olivieri, G., (eds) Truth in Mathematics, Clarendon Press, Oxford, 1998.

Detlefsen, M. “Formalism”, in Shapiro, S. (ed), The Oxford Handbook of Philosophy of Mathematics and Logic, Oxford University Press, Oxford, 2005, pp. 236-317.

Frege. G. Philosophical and Mathematical Correspondance, Gabriel, G. et al. (eds), The University of Chicago Press, Chicago, 1980.

Friend, M. “Pluralism and “Bad” Mathematical Theories: Challenging our Prejudices”. In Paraconsistency: Logic and Applications, proceedings to the WCP4 conference: Fourth World Congress on Paraconsistency, Springer, forthcoming.

Hallett, M. “Hilbert and logic”, in Marion, M. and Cohen, R (eds.), Québec Studies in the Philosophy of Science, Part 1: Logic, Mathematics, Physics and the History of Science, (Boston Studies in the Philosophy of Science, Volume 177), Kluwer Publishing Co., Dordrecht, 1995, pp. 135-187.

Hilbert, D. “Die Grundlagen der Mathematik”, Engl. Tr. “The Foundations of Mathematics”, in Van Heijenoort, J., From Frege to Gödel: A Source Book in Mathematical Logic, 1879-1931, Harvard University Press, cambridge, MA, 1967.

Hilbert, D, “Mathematical Problems” an address to the International Congress of Mathematicians at Paris (1900), En. Tr. in Bulletin of the American Mathematical Society, 8, 1902, pp. 437-479.

Hilbert, D. “The Logical Foundations of Mathematics”, 1923, in W.B. Edwald (ed.) From Kant to Hilbert: A Source Book in the Foundations of Mathematics. vol. 2, Clarendon Press, 1996, pp.1134-48

Hilbert, D. and Bernays, P. Grundlagen der Mathematik, Springer, Berlin, 1968-70.

Kagan, V. F., N. Lobachevsky and his contribution to science, Foreign Languages Pub. House, Moscow, 1957.

Lobachevsky, N. I., Geometrical researches on the theory of parallels, Open Court Publishing Co., Chicago, 1914.

Mortensen, C. Inconsistent Mathematics, Kluwer Academic Publishers, Dordrecht, 1995.

Moschovakis, Y. N., Descriptive Set theory, AMS, 1980.

Nelson, E. "Confessioni d'un matematico apostata", Nuova Civiltà del le Macchine, 1997, pp. 243-247. Published online in English http://www.math.princeton.edu/nelson/papers.html.

Otte, M., "Intuition and Formalism in Mathematical Proof ", Interchange, 21, 1, 1990, pp. 59-64.

Priest, G., review of Azzouni, J., *Talking about logic*, Philosophia Mathematica, 19-3, 2011, p. 362.

Raussen, M. and Skau, C., "Interview with Jean-Pierre Serre", European Mathematical Society Newsletter, 9, 2003, pp. 18-20.

Robinson, A. "Formalism 64", in Logic, Methodology and Pilosophy of Science (Proc. 1964 Internat. Congress), North-Holland, Amsterdam, 1965, pp. 228-246.

Shapiro, S. Philosophy of Mathematics. Structure and Ontology. Oxford University Press, New York, 1997.

Shapiro, S. "Identity, Indiscernibility, and Ante Rem Structuralism: The Tale of I and - I", Philosophia Mathematica 16, (3), 2008, pp. 285-309.

Troelstra, A. and Schwichtenberg, H. Basic Proof Theory, Cambridge University Press, Cambridge, 2000.

Whitehead, A. N., and Russell, B. Principia Mathematica. Vol. 1, Cambridge University Press. Cambridge, 1910.