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ON PROOFS IN MATHEMATICS

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Abstract: In his book Chateaubriand points out some differences between the mathematical and the formal notions of proof. I argue here that the contrast between both cannot be exaggerated, and that the latter fails to represent essential aspects of the former. I also sketch a view of the nature of mathematics that can accommodate one particular feature of mathematical proofs the formal notion, by its very nature, cannot: their freedom.

Keywords: Chateaubriand. Formal proofs. Mathematical proofs. Mathematical structuralism.

SOBRE DEMONSTRAÇÕES EM MATEMÁTICA

Resumo: Em seu livro, Chateaubriand aponta algumas diferenças entre a noção formal e a noção matemática de demonstração. Eu argumento que o contraste entre ambas não pode ser maior, e que aquela é incapaz de capturar alguns aspectos essenciais desta. Eu apresento também um esboço de uma teoria sobre a natureza da matemática capaz de acomodar um aspecto particular das demonstrações matemáticas que a noção formal, pela sua própria natureza, não pode: a liberdade que por direto cabe àquelas.

Palavras chave: Chateaubriand. Demonstrações formais. Demonstrações matemáticas. Estruturalismo matemático.

Three chapters of Chateaubriand's book are dedicated to proofs and proving; understandably, in a book so intimately concerned with logical matters. But the importance of the concept of proof in logic only reflects its relevance in *mathematics*. So, it is natural to ask whether the logical conception of proof corresponds faithfully to its mathematical counterpart (after all, the formalization of mathematics has always been one of the major *raisons d'être* of formal logic). Has logic had with respect to the mathematical notion of proof, the same success it had with respect to that of algorithm, which it managed to define in precise terms?

Chateaubriand seems not so sure. He thinks that two defining features of formal proofs – that they are finite objects that must be algorithmically checkable – do not and need not in general belong to proofs as they occur in "real life"; I agree with him, but I want to radicalize his criticism. I claim that formal proofs, which as a rule must be confined to well-determined formal systems, cannot capture the "exploratory" character of mathematical proofs, which are often not circumscribed to (linguistic, conceptual) contexts determined a priori. I will try here to substantiate this claim – and also sketch a view of the nature of mathematics that conforms better to what I think is the true nature of mathematical proofs.

The question I want to ask is this: are proofs understood as syntactic manipulations of symbols according to prescribed rules in the context of formal-logical systems in any way a reasonable model, idealization or desideratum for proofs actually written by mathematicians? Philosophical analyses of the notion of proof are scarce, as Chateaubriand correctly notices, but analyses of the *mathematical* conception of proof are even more so; I take this chance to offer my views on the subject.

I claim that between mathematical and formal proofs there are more, and more important differences than a few determinate features such as finiteness and algorithmic checkability, differences dramatic enough so as to not only disqualify the latter as passable idealizations of the former but make the formal notion of proof a distortion of what goes on in mathematics under the same name.

Possibly *the* major difference is this: whereas formal proofs *presuppose* a formal context, a formal system – language, rules, and axioms – that must *already* be in place *before* proofs within the system are devised (for the context frames and imposes constraints on proofs) mathematical proofs often create their own context and are not *a priori* constrained either by a language or by a previously designed proof apparatus. We cannot *begin* to write a formal proof without having already at hand a completely determined formal system, whereas mathematical proofs often go hand in hand with the constitution of mathematical theories and concepts.

In mathematics, proving is a *free* enterprise that often accomplishes more than guaranteeing truth and producing knowledge (the logical and epistemological roles of proofs). It can *also* clarify concepts (often creating new ones), build connections between different branches of mathematics, change the meaning of mathematical statements and induce new discoveries, among many others a careful analysis of this practice would reveal. *But in order to do these things mathematical proofs cannot be confined to a proof apparatus fixed beforehand*.

Brouwer was right in believing that formalization plays no role in mathematical *practice* and that we cannot predetermine mathematical proof techniques. In fact, Brouwer's views on proofs are the most faithful account of the true character of mathematical proofs we can find in the traditional philosophical literature. His foundational goals (not mentioning mystical prejudices), however, impose unreasonable restrictions on some well-established mathematical methods. I think that Brouwer's major mistake was to conjoin a peculiar interpretation of mathematical existence – or a theory of meaning, if you like – with the belief that mathematical theories are *contentual* – that is, theories of *determinate* mathematical domains of *objects*. I claim, against him and most philosophies of mathematics, that mathematics does not care about objects at all and that s*tructures*, that is, "empty" forms that do not exist independently "in themselves" – and often do not exist at all – are what mathematics is really about[1]. I will say more about this soon.

Formal proofs require formalization, and here is an example of how it works. Suppose we want to formalize arithmetic so as to have a context in which to carry formal proofs. The first thing we do is to set a group of axioms (we try to make them as complete as possible) which "capture" our "intuitive" grasp of the domain of numbers (or the concept that governs this domain). *No other* extra-logical truth can play any role in formal proofs in this system. Nothing is further removed from what actually happens in the business of proving arithmetical truths. In real life there is *no a priori* restriction on where to look for help: the arithmetic of complex numbers or complex analysis – as is often the case – algebra,

[1] We could, following Chihara, say that mathematical theories are *structural descriptions* of existing or *possibly* existing domains of *real* objects, but this approach gets artificially entangled with modal notions that are perfectly avoidable – provided one is willing to give up certain empiricist prejudices. Mathematical theories can of course, in some cases, describe structural properties of actually existing domains, but not always (in particular not when the domain of the theory is infinite). In general mathematical domains (objectual domains described by mathematical theories) are purely formal (or structural) – i.e. determined exclusively as to form or structure, not content (what explains why mathematical truths are invariably invariant under isomorphisms, that is, structure preserving transformations) – existing only as correlates of their theories (i.e. mathematical domains have *only* the properties their theories attribute to them). The *objective* existence of mathematical domains is parasitic on the objectivity of their theories, a feature even purely logical fictions can have.

algebraic geometry, or even hitherto uncreated theories[2]. One of the standard *mathematical* techniques of proof in number theory is to search for structures where to immerse the domain of numbers (or a re-conceptualized version of it) in such a way that arithmetical operations and relations appear as restrictions to the numerical domain of (all or some of) the structuring operations and relations of the domain in which the original number system is immersed; by so doing we may be able to prove truths in this larger domain whose reducts to the original arithmetical domain are the arithmetical facts we wanted to prove[3]. Nothing of this sort is possible once we immobilized a theory in the straightjacket of a formal system.

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[2] For example, the fundamental theorem of algebra has many different proofs (Gauss, the first to prove it, gave five of them in his doctoral dissertation). Albeit being a theorem of the *algebra* of complex numbers, and having a few algebraic proofs, most of its proofs are *analytic* (for instance, as a direct consequence of Liouville's theorem in complex analysis) or involve *topological* notions (like continuity, for example). This phenomenon is too common in mathematics, but it can raise some uncomfortable questions, for instance: how can analytic and topological notions have any relevance for the arithmetic of complex numbers, *the concept of which makes no appeal to them*? If we formalized complex arithmetic in order to carry out formal proofs, we would not be able to use extra-arithmetical notions to prove the fundamental theorem of algebra, a very un-mathematical restriction.

[3] For instance, we can easily prove infinitely many non-evident trigonometric identities among *real* numbers by means of De Moivre's formula for *complex* number exponentiation. It is not difficult to understand how this is possible. Since mathematical truths are structural (see below) there may be structural relations among mathematical entities of some sort whose "natural" place is a different, more complex structural context, which we usually associate with mathematical entities of a different sort. This helps to explain why reinterpreting mathematical entities of some type as entities of another type (for instance, real numbers as particular complex numbers) is so often enlightening in mathematics.

As a rule, mathematicians do not work within the limits of pre-designed systems, domains or structures. So, the logical notion of proof cannot be a regulative ideal for mathematical proofs; not only because it imposes some unrealistic requirements on proofs (such as finiteness and mechanical checkability), but because it fails to capture the *essence* of what mathematical proving is all about.

Besides showing *that* something is true, a proof in mathematics must ideally show *why* it is true. Aristotle had already, long ago, called our attention to the fact that proofs must be *explicative* (whenever possible). I think that a way, maybe the only way of fulfilling this requirement is to find the context where the proved statement follows "naturally" from its premises, a "natural" proof, as we may call it. A clear sign that establishing truth is not the *only* role of mathematical proofs is the fact that mathematicians are *always* interested in new proofs of already well established theorems. The reason may be that they are never sure that they found the "best", i.e. the "really" explicative, simplest, most natural proof; in short, the context where the particular truth under consideration "naturally" belongs. (The ideal context, of course, is that where the statement to be proved needs no proof and is "intuitively" clear. Mathematics is always striving for obviousness.)[4] This important aspect of the mathematical proving activity is dramatically minimized, if not altogether eliminated, when proving is confined to predetermined formal systems (where the interest seems to lie mainly in *canonical* proofs).

But there is more. Once a formal system is fixed any statement in the system has its meaning determined by it (Hilbert says something of this sort, I think, when he says that the axioms of a formal system are implicit definitions). In interpreted axiomatic

[4] The proofs of, say, Fermat's last theorem or the four-color theorem were not the final words on them, but the beginning of a renewed effort to find new, simpler, more intuitive, more *enlightening* proofs.

systems the axioms are explanations or clarifications of meaning; proofs in the system add nothing. In mathematical practice the opposite happens; with each *new* proof, differently contextualized, the statement being proved *acquires a new meaning*. A classical example of this is given by Lakatos analysis of Fermat's (or Fermat-Descartes') theorem on simple polyhedra. Its proof shows that what the theorem *really* states is a topological, not metrical property of space.

It cannot in general be *a priori* eliminated the possibility of finding new proofs of well-established theorems in completely different contexts, giving them new shades of meaning. One can go as far as to say that the meaning of a mathematical statement is not determined once and for all, but is open to new readings as new proofs of the statement are found (Wittgenstein *may* have said something analogous – we are never sure of what he *really* said). This also explains why mathematicians always welcome new proofs of already well-known results. New proofs enrich the theorem and are certainly not only curious, but superfluous exercises.

Of course, no one has ever said (that I know) that mathematical proofs, *as far as mathematical interests are concerned*, are somehow deficient and must be substituted by formal proofs whenever possible. But formal proofs are often considered what mathematical proofs should look like if mathematicians were uncharacteristically careful and meticulous. However, the design of formal systems – which in general strives for *categoricity* (the characterization of a single formal structure, that is, a single class of structurally identical, isomorphic domains) or *completeness* with respect to *intended* models – goes in the *opposite* direction of the interests of mathematics, which as a rule lie in the interplay of structures, systems and languages.

Summarizing, I think formal proofs are far from being reasonable models or ideals for real mathematical proofs, not only because some of their defining features do not correspond to features of mathematical proofs, but because the design of formal systems and formal proofs do not capture the *essential* aspects of the *structural* analysis of mathematical domains that characterizes mathematical proofs. Real mathematical proofs open up roads to the future, whereas formal versions of them, for the most part, are only ruminations over the past. Trying to understand the mathematical role of proofs by studying formal proofs is like trying to do ornithology by studying stuffed birds.

Now that we are no longer interested in foundational studies (for we no longer think that mathematics needs a foundation), I do not see much significance in the formal analysis of mathematical proofs, at least as far as the philosophy of *mathematics* is concerned. I believe that we can learn more about the nature of mathematics by studying mathematical proofs as they actually happen than by dissecting them within the artificial boundaries of formal systems.

A moment of reflection on the aspects of mathematical proofs I have brought up is enough to suggest a view on the nature of mathematics that, surprisingly, is not as well established as it should. If mathematics were, as some people insist, a science of particular abstract *objects* (like numbers or sets, for example) either independently existing, as Platonists believe, or created in the mathematical activity itself, as some mathematical idealists claim, then it is really surprising that the theory of objects of a sort (e.g. complex numbers and functions of complex variables) has anything to say about objects of a different sort (e.g. positive integers). In ontological sciences, like zoology or astrophysics, which are concerned with *objects* of determinate types, animals or heavenly bodies in our examples, nothing of the sort happens (provided, of course, there are no relevant connections among objects of different domains); we do not expect the theory of star evolution to say anything about felines (if we are not astrologists, of course). But, as already observed, this phenomenon is overwhelmingly common in mathematics.

Since complex numbers are not an answer to the question "how many?", how can they be relevant for the theory of the positive integers, which are *nothing but* an answer to this question? The solution to the puzzle that offers itself naturally is the following: no matter the concept that is at the basis of a mathematical theory (and on whose meaning the truth of the axioms of the theory is grounded), all mathematics cares about are structural relations among the objects that fall under it. There is a simple proof of this claim: no mathematical theory can singularize a unique model; it can, at best, single out only a *class* of *isomorphic* models, i.e. *structurally identical* objectual domains. In other words, mathematical theories can at best singularize structures (or, if we wanted to emphasize their linguistic nature, theories – if consistent – can only provide logically articulated systems of structural descriptions of structurally identical domains), never well-determined singular objectual domains. Therefore, objects are not what mathematical theories are really about.

Now, from this perspective it is not difficult to understand why mathematical theories can so easily communicate with each other. Unlike objectual domains, which can be immersed one in the other only by means of re-conceptualization (objects of one type "seen" as objects of a different type), structures can naturally be extended or immersed in more complex structures, and some structural relations, which can easily be *stated* in the language of one structure (which I call the context of enunciation), may require a more complex structural milieu (the context of *proof*) in order to be adequately treated and eventually established as a fact (of the larger structure, but also of the narrower, to the extent that structural properties can reflect down on substructures and partial structures).

This makes it clear why proving in mathematics requires the search for the convenient context of proof, which so often does not coincide with the context of enunciation; and why the formal model of proof is so inadequate, confining as it does the search for a proof invariably to the context of enunciation. Of course, to look for proofs within the same structural context in which the theorems are enunciated is also desirable, for it is, among other things, if we succeed, a sign of strength of the context in question. But to find many, and as many as possible, *different* proofs, in as far apart as possible contexts is even better, for this is essentially what mathematics is all about, a second level study of structures and their mutual relations (Bourbaki says something more or less to the same effect, but Husserl said it first, at least with respect to what he considered purely formal theories). How far from canonical formal proofs we are!

The fact enunciated in a theorem is indifferent to the particular nature of the objects involved, even when the enunciation is apparently referring to them (for the nominal, relational and conceptual terms of the assertion, after all, denote objects, relations and concepts of one particular domain); mathematical theorems express structural properties *only*. This is all we have to see in order to understand how proving in mathematics works and why the formal model of proof is inadequate vis-à-vis the real life of mathematics.

It is, as always, a matter of dispute among metaphysicians the ontological character of structures. Are they Platonic entities (*ante rem* realism) or simply Aristotelian ones (*in re* realism)? I.e., do they exist independently of the domains they in-form, or are they only aspects of them? Maybe the best approach is to consider the term "structure" only as a way of speaking and give reality only to structural descriptions, which are nothing but assertions of a language. But, of course, since we assume that different descriptions can describe the *same thing*, there is *something* they describe. There are, however, ways of giving this entity a sort of existence, like that of cultural artifacts (Mahler's eighth symphony, for instance), that escapes both Plato's and Aristotle's models. But it is wise not to enter into this here.

One of the most interesting things about the structural approach to mathematics that I have been sketching here is that it can offer a *uniform* treatment to the problem of the application of mathematics in the empirical sciences and mathematics itself. (We often hear philosophers referring to the former, but almost never to the latter.) The more familiar brand of structuralism in the philosophy of mathematics (Shapiro's, Resnik's) tends to explain the applicability of mathematics to natural sciences in terms of the idea of *filling* of an empty structure (as if Nature offered a domain *already in-formed* by a given structure). The problem is that never or almost never Nature is so generous. What happens is that *Nature* (or our *sensibility* or *understanding*) in general offers very *poor structures* we *chose* to *imbed* in richer *mathematical structures* (all italicized words are important). That is, applying mathematics to Nature is just like applying it to itself, everything boils down to finding adequate structures of immersion. Physicists in general are sympathetic to this idea that we can *choose* the mathematical theory (or, equivalently, structure) that is adequate to deal with particular physical phenomena. Famous examples are Poincaré, Weyl, Heisenberg and Bridgman. A particular mathematical theory is not in general inevitable, in mathematics or in science.

This is a good point to stop; I believe I made my view clear that the formal notion of proof, more than inadequate for imposing unnecessary restrictions on proofs, is a distortion of the true character and goals of proofs as they occur in mathematics. Formal proofs result in general from formal-logical analyses of pre-existing proofs carried out in fixed formal systems (as a rule proofs are already available and formal systems are chosen so as to permit their logical analysis). Chateaubriand thinks (and I agree with him) that some of the effectiveness conditions imposed on formal systems (algorithmic checkability, and more strongly, finiteness) distorts the correct logical analysis of proofs (they, for instance, misrepresent the ways in which proofs can secure truth).

I argued here that the formal analysis of proofs (and the formal model of proof) more than offering a distorted picture of the logic of proofs, fails to capture the dynamics involved in the structural analysis of mathematical domains that constitutes the characteristic feature of mathematical proofs. The picture of mathematics I also sketched here goes naturally, I think, with the account of mathematical proof I offered.

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