Oswaldo Chateaubriand*

Proof and Proving[1]

There is a certain confusion that underlies my discussion in "Proof and Logical Deduction" that is now necessary to clear up. It is the confusion, mentioned there in p. 81, between "proofs" in the idealized sense in which they are usually characterized in theories of proof, in logic and philosophy, and the actual "proofs" that we use in proving things to ourselves and to each other. Let's, for the moment, refer to the latter as 'proofs' (and, for emphasis, also as 'provings') and to the former as 'idealized proofs' — eventually I will drop this terminology, however, using 'proof' for both and adding qualifications only when necessary.[2]

Provings are an aspect of our activities. We use provings even to establish trivial details of our day to day routines. The mathematician uses much more sophisticated provings because this is essential to his understanding of mathematical reality. What Hardy gracefully describes (PLD pp. 86-87) are provings, not idealized proofs.

Strictly speaking, idealized proofs are not the mathematician's concern; so much so that it was within logic and philosophy, and not within mathematics, that theories of proof have been developed. Even when a mathematician says that he is interested in a proof, it is not the proof as such — i.e., as an object of investigation in itself — that interests him. When he is proving something to someone he wants to make a point. That's why Hardy says that when the other person sees it, then "the proof is finished". When he is proving something to himself, he wants to see. That's why he follows the peaks.

---

* Departamento de Filosofia da PUC-RIO.

1 This paper is a follow-up to my earlier paper "Proof and Logical Deduction"—to which I will refer as 'PLD'. They are both part of the second volume of my book *Logical Forms* and in the revision of the manuscript will be blended together into a single chapter.

2 I haven't been able to find a good terminology for the distinction I want to make. Idealized proofs, in the sense that I am using the term, may be quite concrete and actual—as they are used in elementary logic texts, for example—but they involve an idealized conception of what a proof should be like.

What often happens, however, is that the mathematical proof can serve as a model for other proofs, and in this sense contains more information than the theorem proved. It may also contain more information in the more straightforward sense, emphasized by constructivists, that it spells out in some detail ideas that are only formulated synthetically in the statement of the theorem. And even when the mathematician sees the truth of a new theorem through a proof, its connections to previous knowledge and, especially, its consequences in relation to old and new knowledge may not be too clear, and the relationships contained in the proof can be an initial foothold. But that's essentially what interests him about the proof over and above the theorem proved; the additional information.

Aside from this, the only serious interest that a mathematician has in proofs is aesthetic. He is also interested in the theorem proved for aesthetic reasons, of course. And this is no mere syntactic interest that derives from clever symbolic manipulation; the content, the meaning, the depth, is quite essential. The proof, and theorem, is beautiful because it reveals an interesting pattern; it is striking because it puts things together in a totally unexpected way; it is simplicity itself, yet it is so deep. That's the stuff of which poetry and painting and music are made.[3] The beauty of a theorem is also tied up to its truth;

---

3 Here are some snippets from Hardy *A Mathematician's Apology* (pp. 84-85, 89-90):

> A mathematician, like a painter or a poet, is a maker of patterns. If his patterns are more permanent than theirs, it is because they are made with *ideas*. ... The mathematician's patterns, like the painter's and the poet's, must be *beautiful*; the ideas, like the colours or the words, must fit together in a harmonious way. Beauty is the first test: there is no permanent place in the world for ugly mathematics. ... The best mathematics is *serious* as well as beautiful... The 'seriousness' of a mathematical theorem lies ... in the *significance* of the mathematical ideas which it connects. ... [A] mathematical idea is 'significant' if it can be connected, in a natural and illuminating way, with a large complex of other mathematical ideas. ... The seriousness of a theorem, of course, does not *lie in* its consequences, which are merely the *evidence* for its seriousness. ... The beauty of a mathematical theorem *depends* a great deal on its seriousness ...

Most mathematicians do not say such things in print, but I think that they generally have a very strong aesthetic feeling for mathematics and would empathize with Hardy's statements. And we saw (PLD p. 93 n. 10) that Brouwer basically agrees with Hardy's point—"the fullest constructional beauty is the *introspective beauty of mathematics*". (See also the more extensive discussion of beauty in pp. 1238-1239 of "Consciousness, Philosophy, and Mathematics".) Brouwer concludes his paper by saying that "intuitionistic mathematics is inner architecture" (p. 1249).
In the recently published volume of Gödel's correspondence we find the following remarks addressed to Paul Cohen (p. 378):

> Let me repeat that it is really a delight to read your proof of the ind[ependence] of the cont[inuum] hyp[othesis]. I think that in all essential respects you have given the best possible proof & this does not happen frequently. Reading your proof had a similarly pleasant effect on me as seeing a really good play.

sometimes it is thought to be true just because it is too beautiful or imaginative to be otherwise.[4] Of course, a proof (or a theorem) may be unusual, and be interesting as such, but unless it has some deeper meaning it is merely a curiosity to be placed in an anthology of mathematical recreations.

A proof, in the sense in which logicians normally use this word, is an idealized extrapolation from ordinary proofs. This idealization involves representing the proof as having a certain structure — which may not be at all like the actual one — which fits into a general theory of proof structures. Except for very simple proofs, this idealized structure (conceived linguistically, mathematically, or mentally) can only be described and not strictly produced, since it is often much too long and complex. Moreover it tends to lose a lot of the meaning, insight and cleverness. (These may be present in finding the structure, of course, but this is other meaning, insight and cleverness.) Sometimes it loses all of that altogether. It is this loss that encourages the syntactic view of proofs. There seems to be nothing left but symbolic transformations that have to be algorithmically checkable. And even so this is an exaggeration. Textbook proofs are sometimes influenced by conceptions of what a proof should be like, i.e., by a theory of idealized proofs, but even so the writers try to preserve as much of the meaning, insight and cleverness as they can. When a textbook goes too far in its decomposition of the proofs, it tends to lose its better audience[5].

---

4 Referring to the letter that he got out of the blue from Ramanujan, then an unknown Indian clerk, containing the statements of many theorems without proofs, Hardy recalls (*Ramanujan*, p. 9):

> ... but (1.10)-(1.12) defeated me completely; I had never seen anything in the least like them before. A single look at them is enough to show that they could only be written down by a mathematician of the highest class. They must be true because, if they were not true, no one would have had the imagination to invent them.

5 Good examples of this are books on set theory written by logicians who want to make explicit the logical structure of the proofs —for example, Suppes' *Axiomatic Set Theory*. But the best way to learn set theory, and understand what is going on, is through an informal presentation of the subject where the proofs follow the usual mathematical standards that emphasize seeing the results clearly rather than making sure that each step follows from previous steps by some rule of inference of a simple sort. This does not exclude that for certain purposes —philosophical, metamathematical or other—it may be important to present the proofs in accordance with the cannons of the idealized theory of proofs. Suppes' book, for example, served as a basis for a computer course on axiomatic set theory at Stanford—see Suppes *University-Level Computer Assisted Instruction at Stanford: 1968-1980*.

As a philosophical example we can consider Frege's project of showing that arithmetic is analytic. For this purpose it is essential that the proof that the basic arithmetical principles derive from general logical principles by means of purely logical inferences be carried out in a way that leaves no room for doubt. That's why Frege insists on an explicitly verifiable formulation of all the principles and inferences. And even if it follows that for every arithmetical theorem there is a purely logical "gapless" proof, it doesn't follow that such proofs are an *analysis* of the

The distinction between proofs and idealized proofs is quite similar to other distinctions one can make. It is a distinction between a systematic representation of some phenomena and the phenomena themselves. Proofs are part of the activity of mathematics; they serve to establish results and to get people to see the lay of the land. The devices one uses in proofs depend a lot on the audience and on the purpose of the proofs, but they are generally much more varied than what gets encapsulated in the idealized proofs of a theory of proofs. This contextual dependence of proofs involves also a temporal aspect. This is no news to an intuitionist since intuitionistic proofs, as mental constructions, are obviously temporal; and Brouwer has carried this over to the idealized proofs that involve the creating subject. Given the basic assumptions of intuitionistic mathematics this is entirely natural, and it is surprising that people have been taken aback by Brouwer's coherence.[6]

---

mathematical proofs. The process of discovery *and* justification in mathematics may well be "synthetic" even if the theorems can be shown to be analytic in Frege's sense that there are purely logical deductions of them from general logical principles. In section 90 of *Grundlagen* Frege remarks (pp. 102-103):

> I do not claim to have made the analytic character of arithmetical propositions more than probable, because it can still always be doubted whether they are deducible solely from purely logical laws, or whether some other type of premiss is not involved at some point in their proof. This misgiving will not be completely allayed even by the indications I have given of the proof of some of the propositions; it can only be removed by producing a chain of deductions with no link missing, such that no step in it is taken which does not conform to some one of a small number of principles of inference recognized as purely logical. To this day, scarcely one single proof has ever been conducted on these lines; the mathematician rests content if every transition to a fresh judgment is self-evidently correct, without enquiring into the nature of this self-evidence, whether it is logical or intuitive. A single such step is often really a whole compendium, equivalent to several simple inferences, and into it there can still creep along with these some element from intuition. In proofs as we know them, progress is by jumps, which is why the variety of types of inference in mathematics appears to be so excessively rich; for the bigger the jump, the more diverse are the combinations it can represent of simple inferences with axioms derived from intuition. Often, nevertheless, the correctness of such a transition is immediately self-evident to us, without our ever becoming conscious of the subordinate steps condensed within it; whereupon, since it does not obviously conform to any of the recognized types of logical inference, we are prepared to accept its self-evidence forthwith as intuitive, and the conclusion itself as a synthetic truth—and this even when it obviously holds good of much more than merely what can be intuited.

6 Kreisel started working out Brouwer's theory of the creating subject in "Informal Rigour and Completeness Proofs". (For further discussion see Troelstra *Principles of Intuitionism*, pp. 95-107.) I quote Kreisel's description of his axioms (pp. 159-160):

The basic notion is

$$\Sigma \vdash_m A$$

> the (thinking) subject Σ has evidence for asserting A at stage m. The parameter m will be particularly important for statements A about free choice sequences α, for which, at stage m, only the values $\alpha(0), \ldots, \alpha(m\text{-}1)$ are given.

In fact, the question of the temporality of proofs is a very interesting issue that bears on my discussion in PLD. Consider, for example, Zermelo's proof of the well-ordering theorem.[7] A new principle of proof was recognized here: the axiom of choice. It raised essentially the issue of the infinity of proofs that I discussed in my paper and it led to a tremendous debate. For many years mathematicians were careful to point out when and where their proofs depended on the axiom of choice. Is there any doubt that the overwhelming majority of mathematicians now recognize this as a legitimate method of proof? What happened to change so many minds? Wasn't it the recognition of its truth? I.e., that as a method of proof it is truth preserving? Evidently, this did not happen by a magical process of conversion, but through the exploration of the relations between the axiom of choice and other mathematical principles and theorems, old and new, and through the discussion and clarification of the many issues involved.[8]

Naturally, the distinction between idealized proofs and the actual proofs we use is not very precise; especially since our conceptions of what a proof should be influence our practice of proving. But I think that we can see better

---

(i) $\Sigma \vdash_m A$ is decidable for each given Σ, m, A;

(ii) $A \rightarrow \forall\Sigma\neg\neg\exists m(\Sigma \vdash_m A)$ and $\forall\Sigma\ [\exists m(\Sigma \vdash_m A) \rightarrow A]$
(universality of mathematics).

The first axiom states the decidability of proofs that we mentioned (PLD, p. 84) in connection with Myhill's argument against the intuitionistic version of Markov's principle. The first axiom in (ii) says that if A is true (in the intuitionistic sense), then the absurdity (contradictoriness) of the creating subject S proving A at some stage m is absurd. This means that there cannot be intuitionistic truths that are *in principle* unprovable by the creating subject even if *in practice* he doesn't prove them. And the second axiom in (ii) says that proof is intuitionistic truth-preserving — the creating subject can only prove what is true. I shall comment further on these axioms below.

7 E. Zermelo "Proof that Every Set Can Be Well-Ordered". In later years Zermelo developed a thoroughly infinitistic position with respect to logic and proof. See Moore "Beyond First-Order Logic: The Historical Interplay between Mathematical Logic and Set Theory".

8 See Moore *Zermelo's Axiom of Choice* for a thorough discussion. The initial issues raised in the exchange of letters between Borel, Hadamard, Baire and Lebesgue — translated in Appendix 1 of Moore's book — had to do with the infinite number of choices, the question of dependent and independent choices, the purely existential character of the choices, the very existence of the sets from which the choices were made, etc. Later these were joined by many other issues, such as the so-called Banach-Tarski paradox. Moore concludes his book with the remark (p. 310):

> The plaintive aside of Dana Scott, quoted at the beginning of the Epilogue, echoes the qualms of many mathematicians past and present: The Axiom of Choice is surely necessary, but if only there were some way to make it self evident as well....

My impression is, as I said above, that mathematicians who are not influenced by philosophical considerations think that the axiom is true; just as true as any important mathematical basic principle. Moreover, I quite agree with Gödel's remark—quoted together with Scott's by Moore—that it is a true axiom also in the sense of expressing an *essential* characteristic of the extensional notion of set—see Gödel "Russell's Mathematical Logic", p. 230. In this sense, the in these terms where exactly lies Enderton's fallacy about giving, and also van Dantzig's fallacy about admitting fictitious idealizations.

Enderton supposes that because the actual proofs we use are finite affairs, this imposes a restriction of finiteness on the idealized representations of these proofs; the idealized proofs. (This is also Hilbert's idea.) This is like supposing that because ordinary motions are not strictly continuous in the mathematical sense, they should not be represented mathematically as continuous motions. Moreover, he projects the idealized proofs into the world and thinks of them as being given in a *literal step by step sense*, and takes this to be the fundamental content of the *givings*. But, on the one hand, this is not what the actual givings are like; and, on the other hand, we can actually give even the idealized proofs by describing them appropriately. They can be given in exactly the same sense in which we can give any other mathematical objects, finite or not. Therefore, no evidence has been produced, of any kind, that the finiteness of the actual givings should be considered an essential constraint upon the structure of the idealized proofs.

Idealization is essential to science. It is only by getting out of the world of phenomena and by not restricting ourselves to "literal" representations of them that we can get any kind of theory about reality. What else are mathematics, biology, geology, economics, logic and philosophy; to name but a few?[9] Unless we idealize and represent the phenomena in some way, we

---

independence of the axiom of choice from the other axioms of set theory is neither upsetting nor surprising.

Concerning the issue of the recognition of truth, Gödel remarks in a different connection ("What is Cantor's Continuum Problem?", p. 265):

> ... even disregarding the intrinsic necessity of some new axiom, and even in case it has no intrinsic necessity at all, a probable decision about its truth is possible also in another way, namely, inductively by studying its "success". Success here means fruitfulness in consequences, in particular in "verifiable" consequences, i.e., consequences demonstrable without the new axiom, whose proofs with the help of the new axiom, however, are considerably simpler and easier to discover, and make it possible to contract into one proof many different proofs. ... There might exist axioms so abundant in their verifiable consequences, shedding so much light upon a whole field, and yielding such powerful methods for solving problems ... that no matter whether or not they are intrinsically necessary, they would have to be accepted at least in the same sense as any well-established physical theory.

Scott and Moore presumably agree with some of this ("surely necessary") for the axiom of choice, but not with its intrinsic necessity.

9 I am using 'phenomena' in the very general sense of aspects of reality. It may be an abuse of language to talk of mathematical, or logical, or philosophical, phenomena, literally interpreted, but that's not the point. From the fact that every science involves idealized representations of the aspects of reality with which it deals, nothing follows about the reality, or lack of reality, of the objects postulated by that science. I am always amused and amazed when I read the standard

won't get even a very primitive theory.[10] It doesn't follow, however, that this idealized representation must be *justified* by showing how the *actual* phenomena *could* have been like that. That's what's wrong with the argument that *we could in principle* produce sequences of any finite length, or that we could imagine beings that could do that, and van Dantzig is quite right to take it apart. But it doesn't follow from this that idealized representation of the phenomena is *itself* inadmissible. That's van Dantzig's fallacy concerning Brouwer.[11] From the point of view of the subject, as Brouwer sees it, experience presents itself as indefinitely extendable, so there is nothing absurd in representing it by means of infinite structures. And it is not only not absurd, but necessary.

It would seem, therefore, that when we discuss a theory of proof (and of logical deduction) we are dealing with idealized structures that have a certain relation to provings — and the theorems themselves are idealized structures

---

argument — believe it or not — that since mathematics is a human creation (or idealization), then it has no objects. Here is a short version by Bridgman in *The Logic of Modern Physics*, p. 60:

> It is the merest truism, evident at once to unsophisticated observation, that mathematics is a human invention.

It is a truism indeed; and what follows from it? Nothing! Nothing that wouldn't follow about any other science. They are *all* human inventions. But Bridgman and others want to conclude from this that the objects of mathematics don't exist; that there are no specifically mathematical aspects of reality. Now, in the case of proofs, at least in the classical conception, we are not dealing with mathematical phenomena but with human thought and communication—that's why Hardy talks about "gas". Since mathematical structures are universal however, it is not surprising that the idealized representation of the proofs can be formulated as a mathematical theory about mathematical objects.

10 Ordinary language already contains a large degree of idealization, as one can see by considering how unrelated languages represent the same phenomena. In *Language, Thought, and Reality* ("Languages and Logic"), Whorf gives several examples illustrated by figures to reflect better the analysis of phenomena.

11 Before introducing the axioms for the creating subject Kreisel remarks (*Op. Cit.*, p. 159):

> In Brouwer's own philosophy (or: analysis) of mathematics, theorems are supposed to be about mental acts of a thinking subject; more precisely, of a *correctly* thinking subject. ... Superficial examination may suggest that the restriction to *correctly* thinking subjects makes the notion of: thinking subject, wholly empty. That this is not so is shown by (c) below: one of the main purposes of the analysis is to restrict the notion of thinking subject so as to eliminate *accidental* psychological elements, yet to exploit essential ones.

It is important to emphasize, however, that this is an idealized representation, for the purposes of theory, and not an idealization of how we are (or would like to be). I, therefore, disagree when Troelstra says (*Op.Cit.*, p. 95):

> The central idea is that of an idealized mathematician (consistent with the subjectivistic viewpoint of intuitionism, we may think of ourselves; or even better, to obtain the required idealization, we may think of ourselves as we should like to be), who performs his mathematical activities in a certain order (you may think of the order given by time).

Would we really like to be like that?

that have a certain relation to statings. The constraints that we impose on such a theory must derive in part from an account of the phenomena (the provings), and in part from the character of the idealized proof structures. Some of the latter may be short enough and clear enough to actually correspond to the structure of actual proofs. But, in practice, we don't think about things in this way. Our conception of idealized proof influences our conception of actual proof and we see the latter as short for the former. This means that these two different aspects get mixed up into one in which idealized proofs are produced and given; in full, or in abbreviated form, or by description. The appeals to intuition, knowledge and insight in the form of pictures, gestures, drawings of all sorts, etc. are considered to be irrelevant filler. More than that, in fact; no matter how helpful they may be in practice, they are considered to be dangerous and potentially misleading elements that are not part of the "real" proof.[12] But the actual proofs do involve all these devices, and in many cases they are quite essential to see clearly what is going on. In fact, even when we study a proof that is presented in a more standardized form, in a textbook or a paper, we provide these visual aids ourselves in order to understand the proof—and we often say that we are "reconstructing" the proof. (This reconstruction involves also other aspects of a proof, such as steps that are considered to be sufficiently clear for us to fill in as we deem necessary, and is part of our training for giving and for taking in proofs.)

In any case, proofs are usually represented as syntactic structures consisting of steps that are made up of sentences that are also thought of as idealized syntactic structures. In the general case, neither the proofs nor the sentences are intended for practical use, but a theory of proofs must involve descriptions of them that we can use and pass around in our theoretical discussions. Since this is done in the world of phenomena, van Dantzig's limitations apply. We are dealing with the strongest kind of effectiveness and finiteness; it is even more than feasible because it must be practical. A description with a billion words may be feasible, but it certainly isn't practical. So, these descriptions cannot contain any idealization — we must be able to actually give them to

12 The argument for this is basically the following: Some pictures (drawings, etc.) are misleading and lead to fallacious proofs; therefore, all pictures (drawings, etc.) should be avoided in proofs. Everybody is aware, of course, that also statements can be misleading and lead to fallacious proofs, and there are lots of examples, but the feeling is that checking should be easier for statements. I doubt that this is true in practice, but in principle it leads to the demand for algorithmic checkability, which would be much harder, if not impossible, to formulate for pictures, drawings, etc.

people in a very literal sense. This doesn't mean, of course, that the structures themselves have to obey restrictions of effectiveness and finiteness, although there may be other reasons for imposing such restrictions upon them. But, when we go on to consider these theories of proof, or of logical deduction, as subjects of inquiry and study the connections between the ideal structures, their descriptions, and the provings, we may idealize again and describe the descriptions as being merely feasible or, perhaps, even recursive — or something in between.

And how should the steps be organized in an idealized representation of proofs? I argued in PLD that the structures needn't be finite chains. It is generally recognized that a reasonable mathematical representation of proofs is provided by well-founded trees. The foundations are the premises, the nodes are the statements, and the connection between the totality of nodes that converge without intermediaries to a lower node, are the steps. How long can these trees be? As long as we want. How wide? As wide as we want. But we have to be able to describe them effectively in some sense.

It seems to me also that everything that I said about proofs concerning finiteness and effectiveness applies equally well, *mutatis mutandis*, to the nodes; the idealized sentences (say) that represent the statings. Thus the nodes can be of any length and structure — since nothing forces statings to be represented linearly — but they must also be effectively describable.

Now we come to a psychological constraint. As everyone has been saying all along, provings must be convincing — really convincing, not idealized convincing. But I disagree with Church that they must carry final conviction in any reasonable epistemological sense. I don't even think that they must carry final conviction in the practical sense that we are certain but we recognize that we might change our mind in the future. Do we really have to be certain? Is it the same certainty that we have that we are reading this right now? How certain are we? If we graded our students by certainty, they'd all flunk. Does this mean that they haven't taken in the proofs? Some haven't, no doubt; but all? So I would settle on simple conviction as the psychological constraint, acknowledging that for different kinds of proofs we may fiddle around with the degree of this conviction.

In fact, it is not even clear to me that deductive conviction is necessarily stronger than inductive conviction, which is never final. I would argue for this by appealing to Newcomb's problem.[13] The problem is set in terms of

13 See Nozick "Newcomb's Problem and Two Principles of Choice".

some being (an extraterrestrial or whatever) who puts you in the following situation. There are two boxes; one containing one thousand dollars, and the other either containing one million dollars or containing nothing. You have two choices: you can pick the contents of both boxes (choice 1), or you can pick the contents of the second box only (choice 2). Whether or not the second box contains the million dollars depends on a prediction that the being makes about you personally. If he (she, it) predicts that you will take the content of both boxes (prediction 1), then he doesn't put the million dollars in the second box. If he predicts that you will take only the contents of the second box (prediction 2), then he will put the million dollars in it. (Nozick adds the qualification that if the being predicts that you will make your choice by means of a random decision — e.g., by flipping a coin — then he doesn't put the million dollars in the second box.) Suppose now that it is your turn to play. After centuries of playing —lots of people want a million dollars, or even a thousand — you have inductive conviction in the predictions of the being; they were never wrong. What choice do you make?

There are two basic arguments. The first says that you should make choice 1 because, whatever prediction the being made, the money either is already there or it is not (and this is actually verifiable); so, at least you get the thousand dollars, and lose nothing by your choice. The second argument says that you should make choice 2 because everyone who made it before you got a million dollars, and everyone who didn't make it got only a thousand. So, what do you do?[14]

I haven't followed the literature on Newcomb's problem, but Nozick considers two kinds of solution. The first consists in the appeal to principles of choice that seem to justify choice 1 — although he ultimately leaves it open whether or not they do in fact justify it. The second consists in justifying

---

14 This is what Nozick says at the end of the introduction (p. 117):

> I should add that I have put this problem to a large number of people, both friends and students in class. To almost everyone it is perfectly clear and obvious what should be done. The difficulty is that these people seem to divide almost evenly on the problem, with large numbers thinking that the opposing half is just being silly.

And then he adds

> Given two such compelling arguments, it will not do to rest content with one's belief that one knows what to do. Nor will it do to just repeat one of the arguments, loudly and slowly. One must also disarm the opposing argument; explain away its force while showing it due respect.

I remember once, in the early seventies, telling this problem to G.H. von Wright, who had never heard of it. He made choice 2 with argument 2, and no matter how hard I tried he just couldn't see any plausibility in argument 1. Finally he acknowledged it, but wouldn't be convinced.

choice 2 through something like backwards causation, or direct inspection of the future — your choice is the grounds for the being to make his prediction.

In psychological and logical terms, what the problem does is to set deductive conviction against inductive conviction. But we assume that deduction is more trustworthy than induction. After all we are all tired of knowing that induction doesn't always work. (Remember Russell's chicken that was fed every day until, one day, got its neck rung.) In fact, those who rationalize choice 2 in terms of backwards causation are giving in to this deductive compulsion, because once we have backwards causation, then we have a deductive argument rather than an inductive one. (Thus, for his final analysis Nozick decides to rule out this possibility.) But those who have inductive conviction, on the basis of the unfailing success so far, are willing to trust the oracle even without any idea as to how it works. In fact, the assumption that one can solve the problem by a lengthier deductive explanation in terms of principles of choice that justify choice 1, seems to be met by the fact that those who have inductive conviction are likely to continue making choice 2.[15]

Although I am completely convinced by the deductive argument — barring, with Nozick, backwards causation — I know that I would make choice 2, and continue making it no matter how many principles of choice are thrown at me, for the simple reason that a million dollars makes a difference to me whereas one thousand doesn't, and I have overwhelming evidence that by making choice 2 I will get a million dollars. One may consider this a gambling principle — and my reactions vary if I start fiddling around with the relative amounts — but, in any case, it shows that even if we have deductive conviction it is not *final* when it comes to putting our (?) money where our mouth is. It seems to me also that there is a real possibility that the reason for the split is that both groups are right; that the arguments are *equally* good. They may not be equally good in the *same way*, but there may be more than one way for an argument to be good.

Now we come to an ontological constraint; proofs must be truth-preserving. This is part of the essence of proof, and it is this ontological constraint that makes proof an epistemological notion. The transcendental notion of truth,

---

15 In p. 135 Nozick says:

> I believe that one should take what is in both boxes. I fear that the considerations I have adduced thus far will not convince those proponents of taking only what is in the second box. Furthermore I suspect that an adequate solution to this problem will go much deeper than I have yet gone or shall go in this paper.

For further discussion by Nozick (and references to the literature) see *The Nature of Rationality*.

determined by reality, makes proof a normative notion and not merely a psychological one. And this is epistemology. Proofs must carry conviction, but justifiably so.

And how must proofs carry conviction and exhibit the truth of the conclusion as conditioned upon the truth of the premises. Ramanujan saw many of his true conclusions, and yet he often didn't have proofs.[16] There is another constraint on proofs; they must consist of steps that are (generally) agreed to be legitimate.[17] This is a social constraint, and it is what Enderton and Church were talking about when they defended algorithmic checkability. But they obviously went too far. What is important is that a proof be decomposable into steps each of which is recognized as a legitimate principle of proof. Again, this is no mere psychological requirement because it would seem that each such step must be truth-preserving in its own right, or at least contribute in some way to the truth-preservation of the whole structure. So, it is an epistemological decomposition.

But how elementary must these steps be? If we are dealing with machines, then they must be algorithmically checkable. If we are dealing with normally educated people, however little they may know about logic and mathematics, then they must be reasonably simple steps. Maybe this implies algorithmic checkability, as Church argues, maybe not. If we are dealing with professionals, then things get pretty hairy. But even if we are dealing with Hardys and Littlewoods and Ramanujans, there still must be some such structure. Not to make oneself understood, but in order to understand. There are peaks that can be reached only through other peaks.[18]

One of the problems with Church's argument is that it uses the wrong generalization. He tries to obtain universality by considering all human beings,

---

16 Still commenting on Ramanujan (see note 4) Hardy says (quoted by Newman in "Srinivasa Ramanujan", p. 374):

> His ideas as to what constituted a mathematical proof were of the most shadowy description. All his results, new or old, right or wrong, had been arrived at by a process of mingled argument, intuition, and induction, of which he was entirely unable to give any coherent account.

And he quotes a similar remark by Littlewood (*Ramanujan*, pp. 11-12):

> ... the clear cut idea of what is *meant* by a proof, nowadays so familiar as to be taken for granted, he perhaps did not possess at all; if a significant piece of reasoning occurred somewhere, and the total mixture of evidence and intuition gave him certainty, he looked no further.

17 Even solipsists must postulate this. It is part of Brouwer's reasoning in his proof of the bar theorem, and it also lies behind the intuitionistic postulate that one must be able to recognize a proof when one sees one. (It is not clear, however, that Brouwer was a solipsist.)

18 See the quotation from Hardy in note 15 (pp. 93-94) of PLD.

even some fictitious humans who cannot think at all except for comparing strings of symbols. But what should be done is to consider classes (or groups) of human beings. Even minorities of one. What is a proof for a professional mathematician may not be a proof for an undergraduate, and what is a proof for an undergraduate may not be a proof for someone who can only compare strings of symbols, but it doesn't follow that only proofs that would *in principle* satisfy the latter are proofs. In fact, from the point of view of the undergraduate or of the professional these things may be unintelligible gibberish that can only be recognized as having anything to do with proving by relating them in an intelligible way to "real" proofs.

Although these constraints must be examined in more detail I would like to conclude my present discussion with a few remarks concerning some aspects of certain standard presentations of logical deduction.

It is interesting that even in logic texts that adhere to the syntactic view of proofs there are often rules of inference that involve description. These are usually derived rules of inference. In Mates' book, for example, we have a rule TH for sentential logic that says that one can appeal to previously proved theorems in one's proofs.[19] And in many logic texts, including Mates', when we get to deduction in first order logic we have a rule T that allows one to use any tautological consequences in one's proofs.[20] Students are often puzzled about rule T; they feel that it undercuts all the careful and painful work that they did to understand proofs in sentential logic. It mixes them up — and they are right.[21]

Rule TH is a concession to mathematical practice. It basically corresponds to the 'it's well-known's, 'by the lemma's, 'by so-and-so's theorem's, 'by a routine argument's, etc. Since, however, one is working within the self-imposed limitations of the syntactic view, one says that these proofs that use rule TH aren't really proofs; they are, rather, instructions for getting a proof —Mates says (p. 102) that "TH is only a device for abbreviating proofs." So, they are descriptions of proofs. Moreover, given the demand on algorithmic checkability, it is essential that, in principle, applications of this rule be reducible to algorithmically checkable transformations. Mates says that "it

19 *Elementary Logic*, p. 101.
20 *Ibid*, p. 112.
21 In fact, student proofs often involve long sequences of consecutive applications of rule T breaking the proof into natural steps (or previously learned steps). And when one points out that they can skip all of that and do it in *one* step — because it is algorithmically verifiable and, hence, *not essential to the proof* — they feel rather lost.

adds convenience, but not strength to our deductive system." But, in any case, one can see how this kind of rule begins to take into account some real features of provings.

On the face of it, rule T seems to be, like rule TH, a concession to mathematical practice — one can appeal to previously established tautologies and tautological consequences. That is part of the standard motivation for it.[22] But, in fact, what it seems to do is to discard all that makes a proof a proof in favor of an oracle. No wonder that students are puzzled.

What is the justification for rule T? Since we have an algorithmic method for determining whether or not a sentence is a tautological consequence of a finite set of sentences, the truth-table method, then, *in principle*, all applications of rule T are algorithmically checkable. There is an interesting reversal here, however. The appeal to algorithmic checkability started out as a means to ensure that one could check whether a proof was carried out in accordance with accepted principles of proof. It was meant as a constraint on proofs as justifications. That is already questionable, as I have argued, but for the sake of the argument let it pass. Now it turns out that algorithmic checkability is considered *sufficient* to legitimize a principle of proof.

But, one could argue, rule T not only conforms to the essential feature of checkability but it is also clearly truth-preserving; i.e., it satisfies the ontological constraint. That is not enough, however, because the ontological constraint on proofs has only a limiting function; by placing this constraint we give up some of our autonomy and bow to reality. It is at a different level than the other constraints, which are partly designed to implement it, and even if we can show that in principle we can ascertain that reality is thus and so, it doesn't follow that we have an acceptable epistemological principle.

The problem with rule T is that it doesn't bring about understanding. According to rule T any tautology has a one step proof and any tautological consequence of finitely many previous steps has a proof of one additional step, independently of whether or not the calculation has been carried out or can be carried out. And even if we were to carry out the computation, this would not ensure understanding.[23] Helping us understand why something is

---

22 Mates remarks (p. 115) that "[i]t is obvious that a large repertoire of tautologies is indispensable for virtuosity in the application of this rule."

23 This is well illustrated by Searle's Chinese room experiment. He imagines himself to be locked in a room with an English program which, by means of purely algorithmic manipulations, allows him to produce Chinese outputs in response to Chinese inputs—and to him, Chinese is Chinese ("meaningless squiggles"). No matter how good the program, and no matter how indistinguishable

true is an essential feature of proofs, for we not only want to know *that* a theorem is true but also *why* it is true.

References

Bridgman, P.W. *The Logic of Modern Physics*. New York, N.Y.: Macmillan, 1960.

Brouwer, L.E.J. "Consciousness, Philosophy, and Mathematics". *Proceedings of the Tenth International Congress of Philosophy*. Amsterdam: North-Holland, 1949.

Chateaubriand, O. "Proof and Logical Deduction". In E.H. Hauesler and L.C. Pereira (Eds.) *Pratica: Proofs, Types and Categories*. Rio de Janeiro: PUC-Rio, 1999.

Chateaubriand, O. *Logical Forms. Part I: Truth and Description*. Campinas: Unicamp, 2001.

Frege, G. *The Foundations of Arithmetic: A Logico-Mathematical Inquiry into the Concept of Number* (1884). Oxford: Blackwell, 1950.

Gödel, K. "Russell's Mathematical Logic" (1944). In P. Benacerraf and H. Putnam (Eds.) *Philosophy of Mathematics: Selected Readings*. Englewood Cliffs, N.J.: Prentice-Hall, 1964.

Gödel, K. "What is Cantor's Continuum Problem?" (1947). In P. Benacerraf and H. Putnam (Eds.) *Philosophy of Mathematics: Selected Readings*. Englewood Cliffs, N.J.: Prentice-Hall, 1964.

---

the answers may be from those of a Chinese speaker, Searle argues that it doesn't account for knowing Chinese. And he concludes ("Minds, Brains, and Programs", p. 418):

> Well, what is it that I have in the case of the English sentences that I do not have in the case of the Chinese sentences? The obvious answer is that I know what the former mean, while I haven't the faintest idea what the latter mean.

The case of rule T is very similar. Even for a feasible not overwhelmingly long truth-table, the difference between that application of rule T and a proof is like the difference between Chinese and English for Searle. Evidently, Searle's argument is part of a different discussion, relating to artificial intelligence and the computational view of mind, which does not concern me at this point. But, independently of what it shows about mind, it does show something about proof. An *idiot savant* who could process a proof procedure in his head without understanding what he was doing wouldn't be proving things—as the words well imply.

Evidently, there are some important questions that arise here in connection with computer assisted proofs—e.g., the proof of the four color theorem—but this very interesting issue is beyond the scope of my present discussion.

Gödel, K. *Collected Works. Volume IV.* Oxford: Clarendon Press, 2003.

Hardy, G.H. *Ramanujan*. Cambridge: Cambridge, 1940.

Hardy, G.H. *A Mathematician's Apology* (1940). Cambridge: Cambridge, 1969.

Kreisel, G. "Informal Rigour and Completeness Proofs". In I. Lakatos (Ed.) *Problems in the Philosophy of Mathematics*. Amsterdam: North-Holland, 1967.

Mates, B. *Elementary Logic* (1965). Second edition. New York, N.Y.: Oxford, 1972.

Moore, G.H. "Beyond First-Order Logic: The Historical Interplay between Mathematical Logic and Axiomatic Set Theory" *History and Philosophy of Logic*, 1980.

Moore, G.H. *Zermelo's Axiom of Choice: Its Origins, Development, and Influence.* New York, N.Y.: Springer, 1982.

Newman, J.R. "Srinivasa Ramanujan". In J.R. Newman (Ed.) *The World of Mathematics*, vol. 1. New York, N.Y.: Simon and Schuster, 1956.

Nozick, R. "Newcomb's Problem and Two Principles of Choice". In N. Rescher et al. (Eds.) *Essays in Honnor of C.G. Hempel*. Dordrecht: Reidel, 1969.

Nozick, R. *The Nature of Rationality.* Princeton, N.J.: Princeton, 1993.

Searle, J. «Minds, Brains, and Programs». *The Behavioral and Brain Sciences*, 1980.

Suppes, P. *Axiomatic Set Theory*. Princeton, N.J.: Van Nostrand, 1960.

Suppes, P. *University-Level Computer Assisted Instruction at Stanford: 1968-1980.* Stanford, Ca.: 1981.

Troelstra, A.S. *Principles of Intuitionism*. Berlin: Springer, 1969.

Whorf, B. *Language, Thought, and Reality.* Cambridge, Mass.: MIT, 1956.

Zermelo, E. "Proof that Every Set Can Be Well-Ordered" (1904). In J. van Heijenoort (ed.) *From Frege to Gödel.* Cambridge, Mass.: Harvard, 1967.