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II. Issues and Approaches

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THE NATURE OF A CORRECT THEORY OF PROOF AND ITS VALUE

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> There are few employments of life in which it is not sometimes advantageous to pause for a short time, and reflect upon the nature of the end proposed. --Boole

This is the second of a series of three articles dealing with application of linguistics and logic to the study of mathematical reasoning, especially in the setting of a concern for improvement of mathematical education. The present article presupposes the previous one. Herein we develop our ideas of the purposes of a theory of proof and the criterion of success to be applied to such theories. In addition we speculate at length concerning the specific kinds of uses to which a successful theory of proof may be put vis-a-vis improvement of various aspects of mathematical education. The final article will deal with the construction of such a theory.

1. PROOFS AND RULES OF INFERENCE

As we have been using the word above, a proof is an articulation of deductive reasoning from premises to conclusion. Thus, when a mathematician writes a proof he is primarily interested in communicating his reasoning to others. He is explaining to others his reasoning that if the premises are true, the conclusion must also be true. Secondarily, he is recording a mental process/event--viz., the particular process of reasoning from those particular premises to that particular conclusion during a particular time interval.

Regularity in Proofs. If we consider proofs that we have written or if we survey the proofs found in the literature of mathematics we find many repetitions of simple patterns. This is a clue to the fact that the writing of proofs is a rule-governed activity.[16] However, if we recall our experiences we will notice that in writing proofs we do not think of ourselves as following rules. It is only after the fact that we see the patterns and postulate the existence of the rules to account for the regularity. This situation is analogous to the situation involving writing of sentences. After seeing many examples of sentences, we notice repeating patterns and postulate the existence of rules to account for

[16] The nature of rule-governed activity is treated in several articles in this book.

the regularity. Sentences are constructed according to rules but we are not conscious of following rules in writing sentences. The same with proofs.[17]

When you write a proof you are generally doing (or redoing) the reasoning that you are expressing in the proof. Moreover, when you are reasoning in a particular branch of mathematics (e.g., geometry or arithmetic) you are generally thinking about the subject matter of that branch--although, as Hilbert, Boole and others point out, if your reasoning is correct, the subject matter is irrelevant and the reasoning would apply equally well to any other subject matter.[18] The point that I am making is that when you are writing a proof you are too busy to think of any rules even if you knew which ones to think of. This is exactly analogous to speech: when you utter a sentence you are generally thinking about what the sentence is about and thus are too busy to bother with rules. Indeed, for example, as you begin to learn a foreign language in a classroom situation, as long as you have to think of the rules you generally make rather dull conversation because you are too busy to give much thought to what you are talking about. Thus, carrying this over to reasoning, if you knew the rules explicitly and actually thought of them while you reasoned you would likely not get very far in your mathematics.

Rules of Inference. Let us use the term "rule of inference" to refer to the rules according to which proofs are constructed. The rules of inference are rules for constructing proofs in the same way that the rules in a sentential grammar are rules for constructing sentences. Because of our hypothesis that the discourse level, which includes the proofs, must have kernel/compound structure there will be two types of rules: initial string rules asserting that certain strings are proofs ab initio and production rules which build up compound proofs from simpler ones. As a result of my own experience in formulation of rules of inference it seems that each production rule can be written in the following form: if such-and-such is a proof then the result of adding so-and-so to the end of it

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[17]The question of the reality of rules of either sort is in many respects analogous to the question of the reality of language structure briefly mentioned above in Section 3 of the first article in this series.

[18]The formal nature of reasoning was clearly presupposed if not explicitly recognized even by Aristotle. This is shown in my as yet unpublished article "A Mathematical Model of Aristotle's Syllogistic." It was explicitly recognized probably as early as 1851 by Boole (pp. 235ff). Hilbert's remarks quoted by Reid (pp. 57ff) show that he also was well aware of this fact very early in his career. However, despite the long history of this idea and despite widely published warnings by prominent mathematicians concerning misconstruals (e.g.,Poincaré, pp. 5ff) it has nevertheless been taken to imply that reasoning itself consists in a mindless application of computational techniques. The important point to realize in connection with present purposes is that,although subject matter or content is irrelevant to soundness of reasoning in the sense that sound reasoning about one subject when reinterpreted correctly is equally sound when applied to another,it is still the case that reasoning divorced from all subject matter rarely, if ever, occurs in practice. Even Hilbert's heralded formal treatment of geometry was, by Hilbert's own admission (p. 3), a codification of the fundamental facts of our spatial intuition. Indeed, were Hilbert's proofs not understood in this way they would scarcely be understandable.

is also a proof.[19] This implies that each production-type rule of inference has the effect of lengthening an already existent proof.

Since proofs frequently begin with assumptions laid down without proof, we may suppose that one initial string rule says that any finite list of sentences may be written down to start a proof provided that each such sentence is clearly marked as an assumption. Thus we might state the premise rule as follows: any finite list of sentences of the form 'Assume p'(for p a sentence) is a proof. Examples of production-type rules of inference are easy to think of. The rule of detachment (or modus ponens) can be stated: Any proof containing both p and 'if p then q' may be lengthened by adding q onto the end. Many other rules will come to mind.

Knowledge of Rules of Inference. It is important to distinguish a stronger and a weaker sense in which one may know a rule of inference. Let us say that a person has weak knowledge of a rule of inference if he reasons in accord with that rule. Thus weak knowledge of a rule of inference is a non-self-conscious kind of knowledge. All mathematicians and most people, I imagine, have weak knowledge of quite a few rules of inference although few people are self-conscious about the rules according to which they reason. On the other hand, let us say that a person has strong knowledge of a rule of inference if he can explain the details of the rule, point-out places where it is used, etc. Strong knowledge of a rule of inference is a very self-conscious kind of knowledge. Mathematicians generally have weak knowledge of many rules of inference and strong knowledge of very few. A logician who is poor at reasoning may have strong knowledge of many rules of inference and weak knowledge of very few, although most logicians, it seems, have weak knowledge and strong knowledge of many rules of inference.

The same distinction carries over to knowledge of rules of sentence construction. All speakers of English have weak knowledge of many sentential rules whereas only linguists can be expected to have strong knowledge of more than a few such rules. Linguists make it their business to have strong knowledge of rules of sentence construction whereas other speakers are content to be able to use the rules, i.e., to have weak knowledge of the rules.

Naturally, it is not to be expected that everyone has even weak knowledge of all rules of inference. Certainly the high school freshman could not be expected to know all of the rules of inference used by the professional mathematician. In a sense, knowing a rule of inference involves an understanding of a type of logical connection. Of course, as people acquainted with mathematical education, we have all had the discouraging experience of seeing a student mimic a teacher's pattern of reasoning without understanding it. In such cases, I believe, we will always be able to ascertain that the student has not learned the rule, but only the superficial aspects of a few applications of it. Nevertheless, I must acknowledge the theoretical possibility of a student who knows how to use an impressibly large class of rules without understanding any of them. Such a student could verify a correct proof of a conclusion from some assumptions

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19 For purely heuristic reasons we are using the term "proof" in such a way that a partial proof (initial segment) is counted as a proof. Thus, a finished proof will be a "proof" which satisfies certain additional conditions. This issue will be dealt with in the third article.

without believing that the conclusion actually followed from them--i.e., he would not be willing to risk anything to defend the thesis that, if the assumptions were true, then the conclusion would necessarily also be true. (Cf. fn. 20 below.)

Even though a given person may not know all of the rules of inference (as the skills of mathematical reasoning evolve new rules may come into use), it is most likely the case that most normal high school freshmen know several of the simpler rules. Moreover, it is my view that some of the more complex rules are learned by developing skill in the use of the simpler rules and, then, seeing how steps may be skipped. This is certainly not to suggest the obviously wrong conclusion that "quantum jumps" do not occur. For example, it was probably not until the late 19th century that mathematicians began using the choice rule ⌜infer (Ef)(x)Rxf(x) from (x)(Ey)Rxy⌝ and it is difficult to see how this rule could be broken down into a deduction using significantly simpler rules. Indeed, "quantum jumps" must have occurred--otherwise we would have no rules at all.

The opinion concerning acquisition of knowledge of some of the more complex rules means that after a student has gone through a certain fixed pattern of detailed reasoning several times he may develop a feel for the upshot of the pattern and begin to omit the details in future proofs--thus, in effect, gaining weak knowledge of a more complex rule. We may imagine that the professional mathematician, after years of experience in deductive reasoning, has developed weak knowledge of very complex rules well beyond the comprehension of beginning students. From this point of view, it is natural to expect that as mathematical reasoning becomes increasingly sophisticated, more and more complex rules of inference will evolve.

If we wish we may even speculate that the mathematics student has two kinds of "vocabularies" of rules--an active vocabulary that he can actually use in doing proofs and a passive vocabulary of rules which he can "follow" but not use. This sort of hypothesis may partially account for inability of students to recreate reasoning that they have followed in class.

Correctness of Rules of Inference. We may wonder about correctness and incorrectness of rules of inference--is it conceivable that a small group of persons or even a whole society writes proofs according to incorrect rules? Indeed, suppose that everyone wrote proofs according to a certain rule, would not the universal acceptance of a rule make it correct? On a certain level, these are very easy questions once we recall that a proof is designed to show that a certain conclusion follows from certain premises. If a conclusion follows from some premises then it is impossible that the premises are true and the conclusion false. Thus *if* a system of rules could be used to prove a false sentence from a set of true sentences *then* certainly at least one of the rules is incorrect or, in the terminology of logic, *unsound*. Thus, it is *possible* that a small group or even a whole society writes proofs according to incorrect rules. (It is possible but I have never seen it happen--although I have seen people make mistakes in proofs.) Moreover, concerning this second question we can say that the universal acceptance of a rule of inference would not make it sound.[20]

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[20] It is instructive as well as amusing to imagine a "country" in which the system of reasoning devised by Copi (1954) were adopted as

Incidentally, it follows from what has been said above that if a certain society writes proofs incorrectly then possibly someone could discover that fact--however, if a society writes proofs correctly then there seems to be no way of finding out for sure that it does.

Parenthetically, I might add here that if I were an Intuitionist, I would have said that I had seen examples of the use of unsound rules. The Intuitionist, e.g., Heyting (1956), would say that most mathematicians use unsound rules and that much of the literature of mathematics contains incorrect proofs. In particular, Intuitionists regard one of the forms of indirect proof as unsound. Let us consider this in a little more detail. The kind of indirect (or reductio ad absurdum) reasoning involved in the standard proof of the irrationality of $\sqrt{2}$ from the axioms of arithmetic proceeds, after the (tacit) assumption of the axioms, by assuming that $\sqrt{2} = n/m$ for some integers n and m and deducing a contradiction.[21] This sort of reasoning is regarded as sound by the Intuitionists because what the Intuitionist means by "not p" is that the assumption of p leads to a contradiction. However the Intuitionist does not regard as sound the other reductio rule which allows one to prove p from some assumptions by assuming "not - p" and deriving a contradiction. For him this would only prove "not-not-p" from original assumptions. "Not-not-p" means that it is absurd to assume that p is absurd and, for the Intuitionist, this does not in turn mean that p itself is true. This view leads to the rejection of one rule of double negation (any proof containing "not-not-p" may be lengthened by adding p), and to the rejection of the rule of excluded middle (any proof may be lengthened by adding "p or not-p").

2. THEORIES OF PROOF

By a theory of proof for English, say, I mean a discourse grammar (1) which is intended to describe some or all of the proofs expressible in English and (2) whose rules are intended to be rules of inference known by persons who express their reasoning in English. If we are given such a theory, we may want to inquire concerning its correctness and its comprehensiveness. It would be natural to call it correct if each of its rules were used by some speakers of English. (There are, of course, other possibilities but this one will suffice in this context.) Furthermore, it would be natural to call it comprehensive if every rule used by any speaker of English were included among its rules. Of course, the correctness and the comprehensiveness of a given theory of proof would be relative to a given time in order to leave open both the possibility of "old" rules being abandoned and also the possibility of "new" rules being "devised."

The hope of ever getting a correct and comprehensive theory of proof is dim. But it is certainly possible to contribute toward such a theory. This would be done first by considering one's own reasoning and trying to formulate the rules implicit therein. The next step would be to survey the mathematical literature in an attempt to find correct proofs that are not constructible by means of one's own rules and which, therefore, may be presumed to be constructed according to "new" rules. After some of these were formulated the continuation of the project would involve

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"official reasoning." Parry (1965) has discovered several invalid "arguments" whose respective conclusions are deducible from their respective premise sets in Copi's system.

[21] Cauman (1966) gives an interesting discussion of this proof.

The notion of comprehensiveness contrasts in two ways ...

Compr looks to methods whereas ccl looks to results. 2nd, compr looks to impl, ccl to logical truth. But both render the system as formalized to the system in the preformal stage.

getting other workers to formulate their own rules and to help in the survey of the literature. It is hard to imagine how one could ever determine whether a particular theory were comprehensive and, of course, if a theory were comprehensive relative to a fixed time it may very well not be comprehensive relative to a later time.

To many readers, the above will sound at least utopian if not far-fetched. It may very well be utopian but, given the Chomsky-Harris idea of trying to develop a sentential grammar of English, the above can easily be seen as an application of the same core idea to a part of the totality of English discourses. Thus, the idea of a comprehensive discourse grammar for all of English is even more utopian. Now, as for being far-fetched, I would simply reply that it is no more far-fetched than the ideal of a comprehensive sentential grammar of English, and a considerable body of researchers are developing this today.

As soon as one seriously considers the project of working toward a correct and comprehensive theory of proof in English, he is quickly faced with a crucial consideration. Since a discourse grammar takes as a starting point a sentential grammar, and since a sentential grammar for English does not exist in anything like a complete form, it becomes clear that the project cannot be begun in a systematic fashion. This objection is well-taken but fortunately a reasonable substitute for a sentential grammar is available at least for the part of English used in mathematical proofs. As a result of centuries of logical analysis of mathematical discourse we now have formally defined symbolic languages which are sufficiently rich so that all of mathematical discourse can be symbolically stated.[22] Thus, we may choose a formal language into which to translate proofs and use the grammar of this formal language as the sentential grammar needed for the theory of proof. Taking this path our resultant theory of proof will necessarily be an idealization of an actual theory of proof in the same sense that, say, a formal language for arithmetic is an idealization of the part of English used in discourse about arithmetic. If it so happened that a group of mathematicians actually used a formal language in their investigations and they wrote their proofs in the formal language then we could investigate the body of proofs as such without translating and without regarding ourselves as developing an idealization. (Cf. Church (1956), pp. 2, 3, 47, fn. 108).

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[22] Current symbolic languages can express all mathematical statements only in the sense that to each mathematical statement there corresponds a symbolic sentence having the same truth conditions. This is not to say that for every mathematical statement there corresponds an equivalent symbolic sentence which makes the same statement in the same way. For example, "No even number is odd" would be glossed as '$\sim\exists x(Ex \& Ox)$' because in none of the current languages do we find a "nothing quantifier." Moreover, the phrase "a, b, and c are distinct objects," which occurs repeatedly in mathematics, must be glossed in current languages by a tortured construction involving a conjunction of three inequalities. Problems of this sort, once noticed, are easily solved. Indeed, Lewis and Langford (pp. 306ff, 335ff) have solved the above two problems. However, all such problems must be solved before a comprehensive theory of proof can be constructed. The reason is that the variety of regular reasoning possible in a language depends on the linguistic devices available.

Moreover, the use of the symbolic language may in the end be seen as a distinct advantage as it may enable the theory to transcend English and provide a theory of proof for other languages as well.[23] However, one should not overlook the possibility that the idiosyncrasies of the various languages will also make themselves known on the discourse level and, in particular, in the proofs expressible in the various languages. This is not to suggest that a conclusion may be provable from certain premises in one language but not in another, though this may be true. Our suggestion was that even if exactly the "same" conclusions are provable from the "same" premises in two different languages it may turn out that there are <u>means</u> of doing it in one language not available to the other. Both of these hypotheses are likely--and perhaps interesting to investigate.

3. THE VALUE OF A THEORY OF PROOF

Before we can consider the possible value of a theory of proof, we should try to determine specifications for a theory which could actually be developed. Otherwise, our speculations would be too hypothetical to be very interesting.

In the first place we postulate the existence of a **managably small set of** simple rules of inference which must be known in order, for example, to be able to prove the main theorems of plane geometry and arithmetic. It is immaterial whether these rules, which we will call <u>the basic rules</u>, are redundant. [A set of, say, three rules is redundant if everything that can be proved using all three can also be proved using only two.] We can easily imagine that the basic rules can be discovered. It is my opinion that the basic rules could be discovered and formulated within a short time by several logicians working with several high school mathematics teachers--provided that the mathematics teachers (1) had been in the habit of making up new proofs and encouraging their students to make up new proofs and (2) had been developing geometry in different ways from year to year. In other words, the mathematics teachers working on the project must have some wide experience to refer to in these matters. What I have in mind as a model is the situation wherein several linguists work with several native informants in developing a sentential grammar of an exotic language.

In order to discuss the value (utility) of a theory of proof then let us imagine that we have the basic rules neatly formulated. Now, when we are asking about the value of this theory of proof what we are really concerned with is the possible answers to the following question: how could a mathematical educator use this theory to improve mathematical education?

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[23] Instead of regarding symbolic languages as idealizations of natural languages some linguists and logicians prefer to distinguish "the logical form" of a sentence from its "grammatical form" <u>and</u> to regard symbolization of a sentence as an attempt to express its logical form. From this point of view a discourse grammar based on a symbolic language would generate the logical forms of discourses (or discourse deep structures). Grammatical forms or surface structures of sentences and discourses are thought of as obtained from their logical forms or deep structures by means of encoding functions called transformations (cf. Keenan, 1969).

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A theory of proof which included the basic rules would provide strong (self-conscious) knowledge of the rules of inference commonly used in elementary mathematics. It seems to me that there are four areas within mathematical education in which such knowledge would be of use, viz., in teaching, in testing and guidance counseling, in curriculum design, and in attempts to understand the psychology of mathematical learning.

Teaching. One important part of a mathematical education is learning to reason deductively and developing skill at it. There may be much more to learning to reason than merely acquiring knowledge and skill in the use of the rules--but certainly these are part of it. Imagine a teacher who has knowledge of the rules in both the weak and the strong senses, i.e., he not only knew how to use them, but he also could refer to them explicitly, formulate them, etc. Such a teacher would be in a very advantageous position vis-a-vis trying to teach mathematical reasoning. Firstly, he would be better able to detect ignorance of specific rules. Now, when a teacher sees a student having difficulty with a proof he is left to his own ad hoc devices concerning diagnosis of the difficulty. Secondly, he would be able to be much more clear in his own writing of proofs because he could be self-consciously critical of his own proofs. Thirdly, he would have a guide in choosing exercises and examples. When the class is having difficulty seeing a proof which involved a complicated application of a rule, the teacher would be able to choose another theorem which involves a simpler application of the same rule, and then, in presenting it to the class he could point out that the reasoning in the complicated case is similar to the reasoning in the simple case. All three of these points hinge on the advantage that an articulate teacher has over one who is merely expert in the subject matter. Consider, for example, the excellent tennis player who is not articulate about what is involved in playing tennis. In trying to teach a beginner to play tennis, the expert player is reduced to showing. If he sees the student doing something wrong he cannot say exactly what is wrong. Even in showing the student what the motions are like, the teacher will not know what to exaggerate and he will not be able to distinguish his own idiosyncrasies from what is essential about tennis. Finally, he will be poor at developing drills, etc.

Testing and Guidance Counseling. It seems to me that a student's ability in deductive reasoning is an important index of his mathematical aptitude, his ability to learn mathematics. This means that a student who is skilled in understanding and producing mathematical proofs will be much more likely to benefit from mathematics courses than one who does not have such skills. It is obvious that a man who has a characterization of what he wants to test is in a better position to design a test than a man who does not have such a characterization. A theory of proof is a characterization of the abstract structure underlying reasoning ability and it should provide a very useful framework for designing tests of reasoning ability. At the very least a theory of proof would provide a better knowledge of what is being measured in tests of reasoning ability and, therefore, also in mathematical aptitude tests.

In order to get an idea of how such tests may be helpful in guidance counseling we must speculate concerning the kinds of things that might be discovered by use of the tests. For example, one might be able to show experimentally that unless a student had acquired weak knowledge of the basic rules by a certain age the chances of his ever being competent in mathematics are very slim. This would enable counselors to advise students concerning careers in mathematics and related areas. Moreover, it is not unreasonable to suppose that normal mathematical development could be characterized in terms of the number and kind of rules learned at various ages (or at various testable stages). This would permit objective identification of unusually able and unusually backward students, again leading to more efficient and more scientific counseling. The professional mathematical educator can certainly conceive of other applications in this vein.

Curriculum Design. One of the aims of curriculum design is to trace a sequence of topics in mathematics which parallels the optimal development of the student's interests and abilities. The reason for this is the desire to give the student the maximum benefit from his formal educational experience. The idea is that the student is best educated by presenting to him at each stage in his education those concepts and proofs which he is best able to respond to. It is absurd either to present things which are too trivial or to present things that are beyond the student's ability. It seems to me then that a characterization of the development of mathematical skill in terms of the number and kind of rules acquired at various ages would provide a valuable framework for use in the design of an efficient curriculum. It would at least permit the knowledge of what would be very difficult and what would be very easy, as far as reasoning is concerned, and this, in turn, would permit more rational choices among alternative theorems to be presented or between alternative developments of a particular topic.

In addition, one can easily imagine a battery of specific remedial programs each designed to teach a specific rule or cluster of rules. Such remedial programs used in conjunction with the diagnostic tests mentioned above might very well form a formidable weapon in trying to overcome inadequate preparation.

In the discussion of knowledge of rules of inference we suggested that complex rules are sometimes learned through experience with simpler ones. If this turns out to be true then the details of the interrelation of knowledge of complex and simple rules will be very important in the choice of alternative developments of a subject as well as in the design of drills and so on.

Finally, we return to the hypothesis of active and passive vocabularies of rules. The truth of this hypothesis would lend additional justification to the suggestions of Professor J. J. LeTourneau (personal communication) to the effect that there should be two separate but parallel mathematics programs--one aimed at developing skill and concrete experience in creating theorems and proofs, the other aimed at acquainting the student with the body of existent mathematical knowledge. Naturally, a theory of the active vocabulary would be applied in the former, whereas the latter would use the passive theory.

Psychology. It is already clear enough that a theory of proof would provide a fruitful source of ideas for hypotheses and experiments in the psychology of mathematical learning. Moreover, one might wish to consider a more comprehensive theory of proof as an idealized description of the more-or-less behavioral aspects of the psychological processes of reasoning. We have already pointed out that the written (or spoken) proof is our only access to another person's reasoning processes. The written proof is a permanent record of the reasoning and, moreover, it is a "trace" of the behavioral aspect of the reasoning. The rules of inference in accordance with which the proofs are written are thus more-or-less behavioral "norms." Given all this, it is easy to speculate that a theory of proof could lead to a psychological theory of deductive reasoning--perhaps analogous to the way that Kepler's Laws describing the orbits of planets lead to a kinetic theory explaining the orbits in terms of the effects of forces.

Finally, on the subject of applications of a theory of proof, I would like to suggest that the quality of writing of mathematics texts could be greatly improved if the writers would take the trouble to learn the rules of inference used by their prospective audiences. A mature mathematician must learn how to reason in a fashion understandable to a freshman if he wants freshmen to learn the mathematics (and not just memorize). Frequently, the mature mathematician encounters (in teaching) theorems which he sees "immediately" and he finds himself at a loss as to what to say to prove them. If he knew the rules of inference used by his class then he would know exactly what to say. If mathematics texts (and mathematics teaching) are improved in this way then one can expect that capable but non-genius students will be more able both to appreciate the beauty of mathematics and also to keep from "getting turned-off by the chicken scratching." Quite possibly all this could lead to the kind of improvement in the field of mathematics that we have seen after the rediscovery of the axiomatic method. In the axiomatic method we find the ideal of the deductive/definitional organization of branches of mathematics: a theory of proof provides a partial answer to the question of what deduction is.

Following all of these hopeful speculations I want to emphasize two negative points. In the first place, none of the above applications will be easily or mechanically achieved despite the fact that much of the groundwork is done. A tremendous amount of very detailed creative thought, dialogue and experimentation is needed. There is even cause to wonder whether there is a natural place to begin. And, there are pitfalls, one of which is the gap between the precision and simplicity of the symbolic languages, on the one hand, and the vagueness, ambiguity and complexity of natural language on the other. Anyone seriously desiring to pursue any of the above applications must become extremely sensitive to the nuances of normal English--and very few mathematicians have the patience for this. A pilot experiment in deductive reasoning recently conducted in a Philadelphia school ended distressingly because the subjects were diverted by too many linguistic red herrings in the test questions. Something can be perfectly clear in the symbolic language and perfectly confusing when translated mechanically into English.

Paradoxically, the second negative point issues from the exhilarating feeling of power and self-confidence that a mathematically competent person derives from learning to be articulate about what he is good at, i.e., from learning a clearly presented and apparently comprehensive theory of proof. Such a person naturally wants to teach the theory to his students--but if the students are not yet good at reasoning they cannot appreciate the significance of what they are learning. They may learn the rules and they may learn how to follow the rules. The disaster is that they come to believe that mathematical reasoning is nothing but following rules. As we pointed out in the beginning of this article, if a person has his mind occupied with the rules then the chances are slim that he will have any attention left for the subject matter or for the deeper parts of reasoning. If a person learns the rules as external rules (as prescriptions) and not as descriptions of what he already does (or would do naturally), the result is stultifying. If pressure is

cf Pascal

put on a student to accept a rule self-consciously before he knows the rule non-self-consciously (i.e., if a rule is imposed on a student), he will either rebel or lose his intellectual integrity, or adopt the view that it's all a silly game. Another equally undesirable but less disastrous effect of teaching an uncomprehensive theory of proof even to students who can appreciate it derives from the fact that they may reason according to rules not in the theory. In this case, the students will tend not to use the rules absent from this theory thus weakening their powers of reasoning. The upshot is that they will be poorer at reasoning after learning the theory than they were before learning it.[24]

[24] Dr. Albert Hammond, late professor of philosophy at Johns Hopkins University, reported to the author in a personal communication the results of tests administered to logic students before and after his course. The tests involved making elementary inferences from material presented in the form of imaginary newspaper articles and narrations of fictional events. His report was to the effect that almost every subject was significantly worse at elementary reasoning after the course.