THE PURPOSES OF LOGICAL FORMALIZATION (1)

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I hope that the title of my lecture has not misled you into expecting too much. As you know I have long been an advocate of a formalist philosophy of mathematics, and have devoted some time to refining the notion of formal system which is central in that philosophy. In the course of doing this, I have had occasion to reflect on the question of what formalization achieves in the progress of human thought and of mathematics in particular. This is a large question; larger than I can treat in all its aspects; I shall only make a few remarks which have been suggested by my reflections and lead to conclusions somewhat different from those currently accepted. The remarks are taken from the current version of a section of the same title in the proposed second volume of [CLg] (2).

It is not necessary to go into detail as to what a formal system is. If you think of it, as you probably do, as the construction of some sort of formalized language, that will be adequate for the present discussion. The important points are as follows. We deal with statements which we derive from explicitly stated assumptions by explicitly stated and readily verifiable rules. Alongside these formal statements we consider other statements which we understand independently of the formal system in terms of our prior, or at least extrinsic, experience. These other statements I have called *contensive* statements (this term is an analogue of the German word 'inhaltlich', for which we have

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(2) For explanation of the references see the Bibliography at the end of the paper. For earlier discussion of a related theme see [RLS].

no exact counterpart in English). We suppose that we have a method of establishing a correspondence between the formal statements and certain contensive statements, constituting the development of some subject matter to which the formalization is being applied. Taking this for granted I wish to address myself to the problem of what we accomplish, or aim to accomplish, by such an activity. I shall have particularly in mind the formalization of logic with reference to mathematics.

A naive view of the relation between a formal system and its application is that the latter comes first; and that from the contensive theory, fully grown and developed, the formal system is obtained by some process of abstraction — even, according to some, a mere mechanical translation into some formalized language. If that were the case all that formalization could accomplish would be a consolidation of the gains already achieved in the contensive theory. Thus formalization accomplishes nothing essentially new. Such a position seems to be defended by some philosophers.

We can answer this objection as follows. In the first place the consolidation may be very worthwhile — it may at least show that some considerations which arise in the contensive theory are irrelevant, make certain relationships stand out with greater clarity, etc., all of which may lead to progress somewhere else. Also one avoids certain ambiguities of ordinary language in the contensive theory; this point is well known and I shall not comment on it further. But a more important point is that the translation into a formalized language does not exhaust the possibilities of formalization. It may happen that the contensive notions which correspond to the formal primitives do not appear on the surface; they have to be found by analysis, and in this analysis formalization can make a substantial contribution.

An illustration of this is found in the discovery of combinators (3). The contensive domain under investigation here was

(3) The example which follows is an account of an experience which I had as a graduate student in 1926-27. The original discoverer of combinators was Schönfinkel; I presume, but do not know, that he had a similar experience.

the somewhat complex process of substitution which was taken in the *Principia Mathematica* as a primitive process. It was found that this could be formalized in a relatively simple formal system in which the atoms were the primitive combinators. This result was not obtained by translating into a formalized language statements already known in the contensive domain. To be sure, after the formalization is completed, we can interpret it in the contensive domain. Such an interpretation developed during the process and helped to guide the study; but it was not there to begin with. This example illustrates that formalization can be creative. It is probably typical of most of the fundamental systems of logic where we do not have a model given beforehand.

Thus formalization plays a role analogous to that of theory construction in natural science; indeed, since theory construction may be regarded as a kind of partial formalization, it may be considered an extension and refinement of such theory construction. This is not the place to go into the role of theory construction in science. Extensive books have been written on that subject and I do not intend to add to them. Philosophers of science rather generally admit its importance, and stress the fact that science progresses by an interaction of theory and factual observation or experiment. Not only does theory consolidate the gains made by experiment, but it often leads the way and suggests what experiments should be performed; notable examples are electromagnetic radiation (Maxwell 1873, Hughes 1879, Hertz 1888, Marconi 1897) and the discovery of the outer planets, Neptune and Pluto. The situation with respect to formalization is analogous.

I shall not attempt to say just what are the features in theory construction which are responsible for this role in scientific progress. But certainly one important factor is this: theoretical reasoning is a more easily checked process than experimental verification. Other factors certainly exist: the disclosure of analogies between otherwise apparently unrelated notions, suggestion of new lines of inquiry, predictive power, etc. But for the moment I shall overlook these.

The theories considered in science generally are deductive theories in the sense that their statements are deduced from certain initial ones. However in the place of explicitly formulated rules, inferences in proofs are supposed to be made by logic. The same thing is true of many of the theories considered in mathematics. The necessity of greater explicitness arose when formalization began to be applied to logic itself. Let us now consider some of the factors which apply under that more special application.

If the subject matter is logic, then the idea that we derive the theorems by means of logic is circular. In order to refute the charge that in operating the system we use the very logic which we are formulating, we have to pass to full formalization, where the inferences are made by effectively verifiable rules. In agreement with the analogy with above mentioned property of theory construction, the rules must be such that the drawing of inferences by them is a more objective process than drawing them contensively. Indeed for really fundamental investigations we strive for the utmost in simplicity and objectivity. The effectiveness in the notion 'effectively verifiable' must be so understood.

Research in the last thirty years has shown that we can define a concept of effectiveness in relatively objective terms. Indeed, a number of different definitions of effectiveness have been proposed, differing essentially in their contensive aspects; nevertheless these definitions have all turned out to be mutually equivalent. Among the more important of these formulations (4) (considered as definitions) are Herbrand-Gödel-Kleene recursiveness (in combination with Gödel enumeration), Turing computability, computability by a Markov algorithm, and representability in combinatory logic (also in combination with Gödel enumeration). In the terminology of the first of these the formal statements, axioms, and rules must be recursive, whereas the class of (elementary) theorems is recursively enumerable. I shall use the term *recursive effectiveness* for effectiveness in this sense.

However, recursive effectiveness is not all that we want in a formal system of logic. A recursive process can be so complex

(4) For other examples see KLEENE [IMM] pp. 317 ff.

that it would take an electronic calculator, operating in microseconds, a thousand years to carry it out. A formalization in which the inferences had such a character would hardly accomplish any of its objectives. Recursive effectiveness is thus a necessary condition for a logical formalization; and this fact may be important in connection with proving the impossibility of doing this or that. But further conditions of simplicity and objectivity are required. What they are is a little vague; but the question is important. The rules for fundamental systems of logic are, in fact, of a very simple character.

A formalization of logic in this strict sense was, in a way, forced on us by the paradoxes. These arguments seemed impeccable from the standpoint of the older logic; they showed that our logical intuitions, as they existed at that time, were not reliable. The paradoxes, in fact, involved situations which the older logic did not take account of. That logic had certain limitations which had not been formulated; indeed no one made any attempt to formulate them until the paradoxes were discovered. Thus these intuitions are not suitable as an ultimate criterion of rigorous proof. We are therefore forced to supplant these intuitions by something more precise; and formalization seems the natural way of doing this.

These facts have important consequences for mathematics. Although the formalization of logic has interested philosophical logicians since antiquity, it did not interest mathematicians for two reasons; first because the logic of the logicians was inadequate for mathematics; secondly because mathematicians believed they possessed infallible logical intuitions, and a formalization of logic was therefore superfluous. In the nineteenth century, however, doubts about these intuitions began to be expressed; the criticism of Kronecker, for example, showed that what was intuitively self-evident to many mathematicians was by no means so to certain others. This was a blow, — although not always felt as such — to the infallibility of the logical intuitions which many mathematicians claimed they possessed. The discovery of the paradoxes was a more shattering blow. A general reform of these logical intuitions was called for. This is true in spite of the fact that these arguments now appear to many persons as explainable fallacies; there is still no generally accepted view as to the nature of the explanation.

So far as mathematics is concerned, it is worth noticing that a similar situation arose earlier. In the eighteenth century mathematicians reasoned with their intuitions and discovered much mathematics that is useful even today. But their methods were unsound by present standards; it is now known that their methods can lead to contradictions. It is customary to say that the eighteenth century mathematicians were sloppy; but it is not clear — my historical information is not sufficient to answer such questions — that they were any less sure of the infallibility of their conclusions than the nineteenth century mathematicians were of theirs. It is also probable that the contradictions deducible by eighteenth century methods were instrumental in bringing about the arithmetization of analysis in the nineteenth century. The effect of the latter movement was to supplant the intuitions of the eighteenth century by new and more sophisticated ones; for although beginners find it necessary to put in all details of an ε δ proof, the seasoned mathematician knows instinctively when this can be done, and this feeling amounts to an intuition.

If we allow this analogy to be our guide, we can draw certain conclusions from it. In the first place the supposedly infallible logical intuitions of the nineteenth century were not actually so; it is necessary to refine our intuitions of set, function, proposition, etc., just as in the nineteenth century analysts found it necessary to refine the earlier intuitions of continuity and limit process. We can do this by introducing a higher degree of formalization. Some progress toward such a refinement has already been made; indeed, although no general agreement on the explanation of the paradoxes has yet been reached, mathematicians are not as worried about them as they once were. In the second place our current intuitions are the result of a process of growth or evolution; what reason is there to suppose that this process will not continue ? Moreover, infallible intuitions are not necessary for the healthy development of mathematics. Even though the methods of the eighteenth century mathematicians were unsound, yet they attained results which still stand; and the paradoxes do not endanger the solid achievements of nineteenth century arithmetization. The foundations of mathematics are not like those of a building, which may collapse if its foundations fail; but they are rather more like the roots of a tree, which grow as the tree grows, and in due proportion. Thus we can conceive of mathematics as a science which grows, as other sciences do, as much by reformation of its fundamental structure as by extension of its gross size.

The avoidance of the paradoxes is thus an objective of a reformulation of logic. If it were the only objective, a formulation with a convincing consistency proof would be all we require. However there are difficulties about this. The results of Gödel show that a finitary consistency proof for a system of real mathematical interest is impossible. Nevertheless some modern logicians have amassed impressive, but of course nonfinitary, evidence for the consistency of, e.g., classical mathematical analysis. But it would be a mistake to suppose that such a consistency proof is all we require. In the first place there are important and useful systems which it does not cover; for example those needed in categorical algebra ([5]); and indeed, since we are constantly using new methods, it is likely that we shall always have uses for systems whose consistency is unknown. In the second place, except for the obvious fact that a system with a known inconsistency is useless until it is modified, consistency is irrelevant from a pragmatic standpoint; we do not hesitate to use a system whose consistency is surmised on empirical or intuitive considerations. Mathematics has no more need of a priori consistency proofs than it does of a priori intuitions. In the third place the proofs are so complex — some of them use ordinals of the third number class — as to leave doubt as to whether the systems are really more secure than they were before.

An alternative to such attempts is to proceed boldly to experiments with systems whose consistency is only plausible. This is what we have done in the past; the attempt being sometimes apparently successful, as in the case of abstract set theory, sometimes not, as in certain cases of Frege, Church, and Quine. Since

([5]) See MacLane [CAR].

we are dealing with concepts for which we have no intuitions, we must proceed by trial and error. The aim should be, not to run away from the paradoxes, but to meet them head on and understand them. We are studying the boundary between the sound and the unsound; and any approximation to that boundary, from either side, is a step in advance. Thus we increase our knowledge by studying very weak systems, whose consistency can be investigated by finitary means; by the derivation of new contradictions; and by proposing systems for serious study which are based on heuristic and tentative considerations, without regarding the whole effort as wasted if a contradiction should develop later. By such means formalization of logic can deepen our understanding of inference in much the same way that theory deepens our understanding of science. This understanding is a worthwhile aim in itself (even though not everyone may be interested in it); this explains the efforts in that direction before the discovery of the paradoxes.

Again, the fact that inferences are objectively verifiable means that they are independent of metaphysical presuppositions. This makes it possible to formalize different sorts of such presuppositions and the intuitions connected with them. We have already seen that mathematicians may have different sorts of intuitions. Some mathematicians are willing to work with the axiom of choice, others not; a few insist on strict constructibility or compatibility with some philosophical position, while others are more liberal. It is not essential that all mathematicians agree as to what inference systems they use. But in developing an intuition to guide his reasoning, a mathematician should be aware of what assumptions of this nature are involved. Formalization makes it possible to consider different such systems and compare them with one another.

At this point it is necessary to be clear about some things which are not objectives of the formalization of logic; neither are they implied by the formalist conception of mathematics.

In the first place, it is not a goal of formalization to lay out a treatment of mathematics, or of any considerable portion of it, as a sequence of formal theorems. This became impractical even with the *Principia Mathematica*; if the analysis of the rules is pushed further until they have the simple character which they have in combinatory logic, it becomes out of the question. Rather the situation is that the rules form the basis of an ultimate criterion of rigor. By means of epitheorems (or, if you prefer, metatheorems) we can derive rules which can be used in the working development with full awareness of what lies back of them. One does not need the ultimate criterion any more than one needs to use primary standards every time one makes a physical measurement.

Another point is that formalization does not attempt to construct a system which embraces the whole of mathematics, nor the whole of logic either. The famous incompleteness theorems of Gödel show that this is impossible. These theorems were indeed very disturbing to those who sought such a unique system. But if one regards mathematics as the science of formal structure as such, and not of any particular formal structure, the theorems do not exclude a treatment of logic by formal, i.e., mathematical methods.

Again it is not a goal of logical formalization to abolish logical intuitions. The working mathematician reasons by his intuitions; let us hope that he will always do so. But it is one thing to suppose that these intuitions are ultimate; and something else to say that, although they are a reliable working tool, they are capable of analysis and improvement. The result of formalization will be a refinement of intuitions. This refinement may even be such that the working mathematician can reason correctly, i.e. in a way which admits of formalization, without being conscious of formal structure as such.

Finally formalization does not exclude philosophical criticism. The derivability of a statement in a formal system is a matter of verifiable fact which philosophical speculation has to take into account. But the significance of that fact, the relation of the system as a whole to our knowledge of the subject matter, and similar questions can be the subject of philosophical criticism. Such criticism may make suggestions which lead to further progress.

Formalization thus shows that intuitive deduction can be theoretically replaced by derivation according to precise and objective rules. This gives an effectively verifiable basis for such deduction; and this fact increases our understanding. But it by no means follows that the replacement should always be actually carried out. We do not abandon our intuitions but reinforce them. Moreover formalization provides a way of studying different kinds of intuitions and comparing them.

BIBLIOGRAPHY

Items are referred to by author's name and three-letter title abbreviations in brackets. These are explained below. When no author's name is given (or implied) the author is Curry or Curry and Feys.

[CLg]: *Combinatory Logic* vol. 1. North Holland Publ. Co., Amsterdam, 1958, xvi + 417 pp.

[RLS]: The Relation of Logic to Science. Presented at Brussels, September 5, 1962. Published in: *Information and Prediction in Science,* edited by S. Dockx and P. Bernays, Academic Press, 1965, pp. 79-96.

KLEENE, S.C. [IMM]: *Introduction to Metamathematics.* North Holland Publishing Co. and P. Noordhof N.V., x + 550 pp., 1952.

MACLANE, Saunders, [CAL]: Categorical Algebra. *Bulletin of the American Math. Society,* vol. 71 (Jan. 1965), pp. 40-106.