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A Renaissance of Empiricism in the Recent Philosophy of Mathematics*

by IMRE LAKATOS

INTRODUCTION

According to logical empiricist orthodoxy, while science is *a posteriori*, contentful and (at least in principle) fallible, mathematics is *a priori*, tautologous and infallible.[1] It may therefore come as a surprise for the historian of ideas to find statements by some of the best contemporary experts in foundational studies that seem to herald a renaissance of Mill's radical assimilation of mathematics to science. In the next section I present a rather long list of such statements. I then go on (in section 2) to explain

* This paper developed out of some remarks made in the course of a discussion at a conference in 1965. This discussion was initiated by a paper of Professor Kálmar's on an empirical approach to mathematics. (The whole discussion is published in Lakatos (*ed.*) [1967]).

The paper was originally accepted for publication in this *Journal* in 1967. Certain changes were suggested, however, and whilst making these Lakatos decided the paper needed further modifications. Only some of these had been made at the time of his death in February 1974.

Lakatos's general epistemological position changed considerably in the period 1967–74 (see particularly his [1970]). No doubt this would have induced substantial changes in this paper, especially in Sections 2, 4 and 5.

In preparing the paper for the press we have introduced a few minor changes in style and presentation. We have also added a few footnotes (indicated by a '*').

JOHN WORRALL and GREGORY CURRIE

[1] This empiricist position (and one of its central difficulties) is very clearly described by Ayer in his [1936]:

> . . . Whereas a scientific generalisation is readily admitted to be fallible, the truths of mathematics and logic appear to everyone to be necessary and certain. But if empiricism is correct no proposition which has a factual content can be necessary or certain. Accordingly the empiricist must deal with the truths of logic and mathematics in one of the two following ways: he must say either that they are not necessary truths, in which case he must account for the universal conviction that they are; or he must say that they have no factual content, and then he must explain how a proposition which is empty of all factual content can be true and useful and surprising. (pp. 72–3)

the motivation and rationale of these statements. I then argue (in section 3) for what I call the 'quasi-empirical' nature of mathematics, as a whole. This presents a problem—namely what kind of statements may play the role of potential falsifiers in mathematics. I investigate this problem in section 4. Finally, in section 5, I examine briefly periods of stagnation in the growth of 'quasi-empirical' theories.

1 EMPIRICISM AND INDUCTION: THE NEW VOGUE IN MATHEMATICAL PHILOSOPHY?

Russell was probably the first modern logician to claim that the evidence for mathematics and logic may be 'inductive'. He, who in 1901 had claimed that the 'edifice of mathematical truths stands unshakable and inexpugnable to all the weapons of doubting cynicism',[1] in 1924 thought that logic (and mathematics) is exactly like Maxwell's equations of electrodynamics: both 'are believed because of the observed truth of certain of their logical consequences.'[2]

Fraenkel claimed in 1927 that:

> the intuitive or logical self-evidence of the principles chosen as axioms [of set theory] naturally plays a certain but not decisive role; some axioms receive their full weight rather from the self-evidence of the consequences which could not be derived without them.[3]

And he compared the situation of set theory in 1927 with the situation of the infinitesimal calculus in the eighteenth century, recalling d'Alembert's '*Allez en avant, et la foi vous viendra*'.[4]

Carnap, who at the 1930 conference in Königsberg still thought that 'any uncertainty in the foundations of the "most certain of all the sciences" is extremely disconcerting',[5] had decided by 1958 that there is an analogy—if only a distant one—between physics and mathematics: 'the impossibilty of absolute certainty'.[6]

Curry drew similar conclusions in 1963:

> The search for absolute certainty was evidently a principal motivation for both Brouwer and Hilbert. But does mathematics need absolute certainty for its justification? In particular, why do we need to be sure that a theory is consistent,

[1] Russell [1901], p. 57

[2] Russell [1924], pp. 325–6. He obviously hesitated between the view that one can put up with this state of affairs (and work out some sort of inductive logic for the *Principia*), and the view that one has to go on with the search for self-evident axioms. In the Introduction to the second edition of the *Principia*, he says that one *cannot* rest content with an axiom that has mere inductive evidence (p. xiv), while on p. 59 he devotes a little chapter to the 'Reasons for Accepting the Axiom of Reducibility' (although still not giving up the hope of deducing it from some self-evident truth).

[3] Fraenkel [1927], p. 61.

[4] Fraenkel, *op. cit.*, p. 61.

[5] Carnap [1931], p. 31. English translation in Benacerraf and Putnam (*eds.*) [1964].

[6] Carnap [1958], p. 240.

or that it can be derived by an absolutely certain intuition of pure time, before we use it? In no other science do we make such demands. In physics all theorems are hypothetical; we adopt a theory so long as it makes useful predictions and modify or discard it as soon as it does not. This is what has happened to mathematical theories in the past, where the discovery of contradictions had led to modifications in the mathematical doctrines accepted up to the time of that discovery. Why should we not do the same in the future? Using formalistic conceptions to explain what a theory is, we accept a theory as long as it is useful, satisfies such conditions of naturalness and simplicity as are reasonable at that time, and is not known to lead us into error. We must keep our theories under surveillance to see that these conditions are fulfilled and to get all the presumptive evidence of adequacy that we can. The Gödel theorem suggests that this is all we can do; an empirical philosophy of science suggests it is all we should do.[1]

To quote Quine:

We may more reasonably view set theory, and mathematics generally, in much the way in which we view theoretical portions of the natural sciences themselves; as comprising truths or hypotheses which are to be vindicated less by the pure light of reason than by the indirect systematic contribution which they make to the organizing of empirical data in the natural sciences.[2]

And later he said:

To say that mathematics in general has been reduced to logic hints at some new firming up of mathematics at its foundations. This is misleading. Set theory is less settled and more conjectural than the classical mathematical superstructure than can be founded upon it.[3]

Rosser too belongs to the new fallibilist camp:

According to a theorem of Gödel . . . if a system of logic is adequate for even a reasonable facsimile of present-day mathematics, then there can be no adequate assurance that it is free from contradiction. Failure to derive the known paradoxes is very negative assurance at best and may merely indicate lack of skill on our part . . .[4]

Church, in 1939 thought that: 'there is no convincing basis for a belief in the consistency either of Russell's or of Zermelo's system, even as probable'.[5]

Gödel in 1944 stressed that under the influence of modern criticism of its foundations, mathematics has already lost a good deal of its 'absolute certainty' and that in the future, by the appearance of further axioms of set theory, it will be increasingly fallible.[6]

In 1947, developing this idea, he explained that for some such new axiom,

even in case it had no intrinsic necessity at all, a (probable) decision about its truth is possible also in another way, namely, inductively by studying its 'success',

[1] Curry [1963], p. 16. See also his [1951], p. 61.
[2] Quine [1958], p. 4.
[3] Quine [1965], p. 125.
[4] Rosser [1953], p. 207.
[5] Church [1939].
[6] Gödel [1944], p. 213.

that is, its fruitfulness in consequences demonstrable without the new axiom, whose proofs by means of the new axiom, however, are considerably simpler and easier to discover, and make it possible to condense into one proof many different proofs. The axioms for the system of real numbers, rejected by the intuitionists, have in this sense been verified to some extent owing to the fact that analytical number theory frequently allows us to prove number theoretical theorems which can subsequently be verified by elementary methods. A much higher degree of verification than that, however, is conceivable. There might exist axioms so abundant in their verifiable consequences, shedding so much light upon a whole discipline, and furnishing such powerful methods for solving given problems (and even solving them, as far as that is possible, in a constructivistic way) that quite irrespective of their intrinsic necessity they would have to be assumed at least in the same sense as any well established physical theory.[1]

Also, he is reported to have said a few years later that:

the role of the alleged 'foundations' is rather comparable to the function discharged, in physical theory, by explanatory hypotheses . . . The so-called logical or set-theoretical 'foundation' for number-theory or of any other well established mathematical theory, is explanatory, rather than really foundational, exactly as in physics where the actual function of axioms is to *explain* the phenomena described by the theorems of this system rather than to provide a genuine 'foundation' for such theorems.[2]

Weyl says that non-intuitionistic mathematics can be tested, but not proved:

No Hilbert will be able to assure us of consistency forever; we must be content if a simple axiomatic system of mathematics has met the test of our elaborate mathematical experiments so far . . . A truly realistic mathematics should be conceived, in line with physics, as a branch of the theoretical construction of the one real world, and should adopt the same sober and cautious attitude toward hypothetic extensions of its foundations as is exhibited by physics.[3]

Von Neumann, in 1947, concluded that

After all, classical mathematics, even though one could never again be absolutely certain of its reliabilty, . . . stood on at least as sound a foundation as, for example, the existence of the electron. Hence, if one was willing to accept the sciences, one might as well accept the classical system of mathematics.[4]

Bernays argues very similarly: It is of course surprising and puzzling that the more content and power mathematical methods have, the less is their self-evidence. But 'this will not be so surprising if we consider that there are similar conditions in theoretical physics'.[5]

According to Mostowski mathematics is just one of the natural sciences:

[1] Gödel [1947], p. 521. The word 'probable' was inserted in the reprinted version, Gödel [1964], p. 265.

[2] Mehlberg [1962], p. 86.

[3] Weyl [1949], p. 235.

[4] Neumann [1947], pp. 189–90

[5] Bernays [1939], p. 83.

[Gödel's] and other negative results confirm the assertion of materialistic philosophy that mathematics is in the last resort a natural science, that its notions and methods are rooted in experience and that attempts at establishing the foundations of mathematics without taking into account its originating in the natural sciences are bound to fail.[1]

And Kalmar agrees: '... the consistency of most of our formal systems is an empirical fact; ... Why do we not confess that mathematics, like other sciences, is ultimately based upon, and has to be tested in, practice?'[2]

These statements describe a genuine revolutionary turn in the philosophy of mathematics. Some describe their individual *volte-face* in dramatic terms. Russell, in his autobiography, says: 'The splendid certainty which I had always hoped to find in mathematics was lost in a bewildering maze ...'.[3] Von Neumann writes: 'I know myself how humiliatingly easily my own views regarding the absolute mathematical truth changed ... and how they changed three times in succession!'[4] Weyl, recognising before Gödel that classical mathematics was *unrescuably* fallible, refers to this state of affairs as 'hard fact'.[5]

We could go on quoting; but surely this is enough to show that mathematical empiricism and inductivism (not only as regards the *origin* or *method*, but also as regards the *justification*, of mathematics) is more alive and widespread than many seem to think. But what is the background and what is the *rationale* of this new empiricist-inductivist mood? Can one give it a sharp, *criticisable* formulation?

2 QUASI-EMPIRICAL VERSUS EUCLIDEAN THEORIES

Classical epistemology has for two thousand years modelled its ideal of a theory, whether scientific or mathematical, on its conception of Euclidean geometry. The ideal theory is a deductive system with an indubitable truth-injection at the top (a finite conjuction of axioms)—so that truth, flowing down from the top through the safe truth-preserving channels of valid inferences, inundates the whole system.

It was a major shock for over-optimistic rationalism that science—in spite of immense efforts—could not be organised in such Euclidean theories. Scientific theories turned out to be organised in deductive systems where the *crucial* truth-value injection was *at the bottom*—at a special set of theorems. But *truth* does not flow upwards. The important logical flow in such *quasi-empirical theories* is not the transmission of truth

[1] Mostowski [1955], p. 42.
[2] Kalmár [1967], pp. 192–3.
[3] Russell [1959], p. 212. For further details about Russell's turn, *cf.* my [1962].
[4] Neumann [1947], p. 190.
[5] Weyl [1928], p. 87.

but rather the retransmission of *falsity*—from special theorems at the bottom ('basic statements') up towards the set of axioms.[1]

Perhaps the best way to characterise quasi-empirical, as opposed to Euclidean theories, is this. Let us call those sentences of a deductive system in which some truth values are initially injected, 'basic statements', and the subset of basic statements which receive the particular value true, 'true basic statements'. Then a system is Euclidean if it is the *deductive closure* of those of its basic statements which are assumed to be true. Otherwise it is quasi-empirical.

An important feature of both Euclidean and quasi-empirical systems is the set of particular (usually unwritten) conventions regulating truth value injections in the basic statements.

A Euclidean theory may be claimed to be true; a quasi-empirical theory—at best—to be well-corroborated, but always conjectural. Also, in a Euclidean theory the true basic statements at the 'top' of the deductive system (usually called 'axioms') *prove*, as it were, the rest of the system; in a quasi-empirical theory the (true) basic statements are *explained* by the rest of the system.

Whether a deductive system is Euclidean or quasi-empirical is decided by the pattern of truth value flow in the system. The system is Euclidean if the characteristic flow is the transmission of truth from the set of axioms 'downwards' to the rest of the system—logic here is an *organon of proof;* it is quasi-empirical if the characteristic flow is retransmission of falsity from the false basic statements 'upwards' towards the 'hypothesis'—logic here is an *organon of criticism.*[2] But this demarcation between patterns of truth value flow is independent of the particular conventions that regulate the original truth value injection into the basic statements. For instance *a theory which is quasi-empirical in my sense may be either empirical or non-empirical in the usual sense:* it is empirical only if its basic theorems are spatio-temporally singular basic statements whose truth values are decided by the time-honoured but unwritten code of the experimental scientist.[3] (We may speak, even more generally, of Euclidean versus quasi-empirical theories independently of *what* flows in the logical channels: certain or fallible truth and falsehood, probability and improbability, moral desirability or undesirability, *etc. It is the how of the flow that is decisive.*)

The methodology of a science is heavily dependent on whether it aims at a Euclidean or at a quasi-empirical ideal. The basic rule in a science which adopts the former aim is to search for self-evident axioms—

[1] For an exposition of the story see my [1962]. The concept and term 'basic statement' is due to Karl R. Popper; see his [1934], ch. v.

[2] *Cf.* Popper [1963], p. 64.

[3] For a discussion *cf.* my [1971].

Euclidean methodology is puritanical, antispeculative. The basic rule of the latter is to search for bold, imaginative hypotheses with high explanatory and 'heuristic' power,[1] indeed, it advocates a proliferation of alternative hypotheses to be weeded out by severe criticism—quasi-empirical methodology is uninhibitedly speculative.[2]

The development of Euclidean theory consists of three stages: first the naive prescientific stage of trial and error which constitutes the prehistory of the subject; this is followed by the foundational period which reorganises the discipline, trims the obscure borders, establishes the deductive structure of the safe kernel; all that is then left is the solution of problems inside the system, mainly constructing proofs or disproofs of interesting conjectures. (The discovery of a decision method for theoremhood may abolish this stage altogether and put an end to the development.)

The development of a quasi-empirical theory is very different. It starts with problems followed by daring solutions, then by severe tests, refutations. The vehicle of progress is bold speculations, criticism, controversy between rival theories, problem-shifts. Attention is always focused on the obscure borders. The slogans are growth and permanent revolution, not foundations and accumulation of eternal truths.

The main pattern of Euclidean criticism is suspicion: Do the proofs really prove? Are the methods used too strong and therefore fallible? The main pattern of quasi-empirical criticism is proliferation of theories and refutation.

3 MATHEMATICS IS QUASI-EMPIRICAL

By the turn of this century mathematics, 'the paradigm of certainty and truth', seemed to be the last real stronghold of orthodox Euclideans. But there were certainly some flaws in the Euclidean organisation even of mathematics, and these flaws caused considerable unrest. Thus the central problem of all foundational schools was: 'to establish once and for all the certitude of mathematical methods'.[3] However, foundational studies unexpectedly led to the conclusion that a Euclidean reorganisation of mathematics as a whole may be impossible; that at least the richest mathematical theories were, like scientific theories, quasi-empirical. Euclideanism suffered a defeat in its very stronghold.

The two major attempts at a perfect Euclidean reorganisation of classical

[1] For the latter concept *cf.* Lakatos [1970].

[2] The elaboration of empirical methodology—which of course is the paradigm of quasi-empirical methodology—is due to Karl Popper.

[3] Hilbert [1925], p. 35.

mathematics—logicism and formalism[1]—are well known, but a brief account of them from this point of view may be helpful.

(*a*) *The Frege–Russell approach* aimed to deduce all mathematical truths —with the help of ingenious definitions—from indubitably true logical axioms. It turned out that some of the logical (or rather set theoretical) axioms were not only not indubitably true but not even consistent. It turned out that the sophisticated second (and further) generations of logical (or set-theoretical) axioms—devised to avoid the known paradoxes—even if true, were not indubitably true (and not even indubitably consistent), and that the crucial evidence for them was that classical mathematics might be *explained*—but certainly not *proved*—by them.

Most mathematicians working on comprehensive '*grandes logiques*' are well aware of this. We have already referred to Russell, Fraenkel, Quine and Rosser. Their 'empiricist' turn is in fact a quasi-empiricist one: they realised (independently even of Gödel's results) that the *Principia Mathematica* and the strong set-theories, like Quine's *New Foundations* and *Mathematical Logic*, are all quasi-empirical.

Workers in this field are conscious of the method they follow: daring conjectures, proliferation of hypotheses, severe tests, refutations. Church's account of an interesting theory based on a restricted form of the law of excluded middle (later shown to be inconsistent by Kleene and Rosser[2]) outlines the quasi-empirical method:

> Whether the system of logic which results from our postulates is adequate for the development of mathematics, and whether it is wholly free from contradiction, are questions which we cannot now answer except by conjecture. Our proposal is to seek at least an empirical answer to these questions by carrying out in some detail a derivation of the consequences of our postulates, and it is hoped either that the system will turn out to satisfy the conditions of adequacy and freedom from contradiction or that it can be made to do so by modifications or additions.[3]

Quine characterised the crucial part of his *Mathematical Logic* as a 'daring structure . . . added at the constructor's peril'.[4] Soon it was shown

[1] Intuitionism is omitted: it never aimed at a reorganisation but at a truncation of classical mathematics. (**Editors' Note:* Not all the theorems of intuitionist mathematics are theorems of classical mathematics. In this sense, Lakatos is wrong to describe intuitionism as simply a 'truncation' of classical mathematics. Nevertheless, an important point remains. While Russell's logicism and Hilbert's formalism each regarded its task as the justification of the whole of classical mathematics, Brouwer's intuitionism was willing to jettison large parts of classical mathematics which do not meet its standards of justification.)

[2] Kleene and Rosser [1935].

[3] Church [1932], p. 348.

[4] Quine [1941*a*], p. 122. Some critics of Quine may say that it is only he who has made a 'daring' structure out of the natural simplicity of mathematics. But surely the Cantorian paradise is a 'bold theoretical construction, and as such the very opposite of analytical self-evidence' (Weyl [1947], p. 64). Also *cf.* the Weyl quotation in section 2.

by Rosser to be inconsistent and Quine then himself described his earlier characterisation as one that had 'a prophetic ring'.[1]

One can never refute Euclideanism: even if forced to postulate highly sophisticated axioms, one can always stick to one's hopes of deriving them from some deeper layer of self-evident foundations.[2] There have been considerable and partly successful efforts to simplify Russell's *Principia* and similar logicistic systems. But while the results were mathematically interesting and important they could not retrieve the lost philosophical position. The *grandes logiques* cannot be proved true—nor even consistent; they can only be proved false—or even inconsistent.

(*b*) While the Frege–Russell approach aimed to turn mathematics into a unified classical Euclidean theory the *Hilbert approach* offered a radically new modification of the Euclidean programme, exciting both from the mathematical and the philosophical points of view.

Hilbertians claimed that classical analysis contains an absolutely true Euclidean kernel. But along side this there are 'ideal elements' and 'ideal statements' which, though indispensable for the deductive-heuristic machinery, are not absolutely true (in fact they are neither true nor false). But if the whole theory, containing both the concrete-*inhaltlich* and the ideal statements can be proved consistent in a Euclidean metamathematics,[3] the entire classical analysis would be saved.* That is, analysis *is* a quasi-empirical theory[4] but the Euclidean consistency proof will see to it that it should have no falsifiers. The sophistication of Cantorian speculation is to be safeguarded not by deeper-seated Euclidean axioms *in*

[1] Quine [1941*b*], p. 163. By the way, the most interesting feature of Rosser's paper is the search for ways of testing the consistency of *ML*. Rosser shows that 'if one can prove *201 from the remaining axioms, then the remaining axioms are inconsistent' (Rosser [1941], p. 97).

[2] Also, one can choose to cut down a quasi-empirical theory to its Euclidean kernel (that is the essential aspect of the intuitionist programme).

[3] Originally the metatheory was not to be axiomatised but was to consist of simple, protofinitary thought-experiments. In Bologna (1928) von Neumann even criticised Tarski for axiomatising it. (The generalisation of the concept of 'Euclidean theory' to informal, unaxiomatised theories does not constitute any difficulty.)

* *Editors' Note:* Lakatos is, perhaps, wrong to think that Hilbert's philosophy, at least as here presented, can be subsumed easily under Euclideanism. Metamathematics is an informal unaxiomatised theory and such theories do not have the required deductive structure to be candidates for Euclidean status. Informal theories can obviously be axiomatised, but one of Hilbert's central claims was that there was no need for this in the case of metamathematics (*cf.* footnote 2 *above*). Each principle assumed in a metamathematical proof was to be so obviously true as not to be in need of justification (or, rather, to be immediately justified by the so-called 'global intuition').

[4] To quote Weyl again:

> . . . whatever the ultimate value of Hilbert's program, his bold enterprise can claim one merit: it has disclosed to us the highly complicated and ticklish logical structure of mathematics, its maze of back-connections, which result in circles of which it cannot be gathered at a first glance whether they might not lead to blatant contradictions. (*op. cit.*, p. 61)

the theory itself—Russell has already failed in this venture—but by an austere Euclidean *metatheory*.

Eventually, Hilbertians defined the set of statements whose truth values could be regarded as directly given (the set of finitistically true statements) so clearly that their programme could be refuted.[1] The refutation was provided by Gödel's theorem which implied the impossibility of a finitary consistency proof for formalised arithmetic. The reaction of formalists is well summed up by Curry:

> This circumstance has led to a difference of opinion among modern formalists, or rather, it strengthened a difference of opinion which already existed. Some think that the consistency of mathematics cannot be established on *a priori* grounds alone and that mathematics must be justified in some other way. Others maintain that there are forms of reasoning which are *a priori* and constructive in a wider sense and that in terms of these the Hilbert program can be carried out.[2]

That is, either metamathematics was to be recognised as a quasi-empirical theory or the concept of finitary or *a priori* had to be stretched. Hilbert chose the latter option. According to him the class of *a priori* methods was now to include, for example, transfinite induction up to ϵ_0, used in Gentzen's proof of the consistency of arithmetic.

But not everybody was happy about this extension. Kálmar, who applied Gentzen's proof to the Hilbert–Bernays system, never believed that his proof was Euclidean. According to Kleene: 'To what extent the Gentzen proof can be accepted as securing classical number theory . . . is . . . a matter for individual judgment, depending on how ready one is to accept induction up to ϵ_0 as a finitary method.'[3] Or, to quote Tarski:

> . . . there seems to be a tendency among mathematical logicians to overemphasize the importance of consistency problems, and the philosophical value of the results so far in this direction seems somewhat dubious. Gentzen's proof of the consistency of arithmetic is undoubtedly a very interesting metamathematical result which may prove very stimulating and fruitful. I cannot say, however, that the consistency of arithmetic is now much more evident to me (at any rate, perhaps to use the terminology of the differential calculus, more evident than by epsilon) than it was before the proof was given. To clarify a little my reactions: let G be a formalism just adequate for formalizing Gentzen's proof, and let A be the formalism of arithmetic. It is interesting that the consistency of A can be proved in G; it would perhaps be equally interesting if it should turn out that the consistency of G can be proved in A.[4]

However, even those who find transfinite induction up to ϵ_0 infallible would not be happy to go on stretching the concept of infallibility so as to

[1] Herbrand [1930], p. 248. It took three decades to arrive at this definition.
[2] Curry [1963], p. 11.
[3] Kleene [1952], p. 479.
[4] Tarski [1954], p. 19.

accommodate consistency proofs of stronger theories. In this sense 'the real test of proof-theory will be the proof of the consistency of *analysis*',[1] and this has still to be seen.

Gödel's and Tarski's incompleteness results however reduce the chances of the final success of Hilbert's programme still further. For if *extant* arithmetic cannot be proved by the original Hilbertian standards, the gradual, consistent (and indeed, ω-consistent) augmentation of theories containing arithmetic by further axioms can only be reached by still more fallible methods. That is, the future development of arithmetic will increase its fallibility. Gödel himself has pointed this out in his paper on Russell's mathematical logic:

> [Russell] compares the axioms of logic and mathematics with the laws of nature and logical evidence with sense perception, so that the axioms need not necessarily be evident in themselves, but rather their justification lies (exactly as in physics) in the fact that they make it possible for these 'sense perceptions' to be deduced; which of course would not exclude that they also have a kind of intrinsic plausibility similar to that in physics. I think that (provided 'evidence' is understood in a sufficiently strict sense) this view has been largely justified by subsequent developments, and it is to be expected that it will be still more so in the future. It has turned out that (under the assumption that modern mathematics is consistent) the solution of certain arithmetical problems requires the use of assumptions essentially transcending arithmetic, *i.e.*, the domain of the kind of elementary indisputable evidence that may be most fittingly compared with sense perception. Furthermore it seems likely that for deciding certain questions of abstract set theory and even for certain related questions of the theory of real numbers new axioms based on some hitherto unknown idea will be necessary. Perhaps also the apparently unsurmountable difficulties which some other mathematical problems have been presenting for many years are due to the fact that the necessary axioms have not yet been found. Of course, under these circumstances mathematics may lose a good deal of its 'absolute certainty'; but, under the influence of the modern criticism of the foundations, this has already happened to a large extent. There is some resemblance between this conception of Russell and Hilbert's 'supplementing the data of mathematical intuition' by such axioms as, e.g., the law of excluded middle which are not given by intuition according to Hilbert's view; the borderline however between data and assumptions would seem to lie in different places according to whether we follow Hilbert or Russell.[2]

Quine says that in the field of *grande logique* construction 'at the latest, the truism idea received its deathblow from Gödel's incompleteness theorem. Gödel's incompleteness theorem can be made to show that we can never approach *completeness* of elementhood axioms without approaching contradiction . . .'[3]

[1] Bernays and Hilbert [1939], p. vii.
[2] Gödel [1944], p. 213.
[3] Quine [1941*a*], p. 127.

There are many possible ways of augmenting systems including arithmetic. One is through adding strong, arithmetically testable, axioms of infinity to *grande logiques*.[1] Another is through constructing strong ordinal logics.[2] A third one is to allow non-constructive rules of inference.[3] A fourth one is the model-theoretic approach.[4] But all of them are fallible, not less fallible—and not less quasi-empirical—than the ordinary classical mathematics which was so much in want of foundations. This recognition —that not only the *grandes logiques*, but also mathematics is quasi-empirical—is reflected in the 'empiricist' statements by Gödel, von Neumann, Kalmar, Weyl and others.

(It should however be pointed out that some people believe that some of the principles used in these different methods are *a priori* and they were arrived at by 'reflection'. For instance, Gödel's empiricism is qualified by the hope that set theoretical principles may be found which are *a priori* true. He claims that Mahlo's 'axioms show clearly, not only that the axiomatic system of set theory as used today is incomplete, but also that it can be supplemented without arbitrariness by new axioms which only unfold the content of the concept of set explained above'.[5] (Gödel, however, does not seem to be very sure of the *a priori* characterisability of the concept of set, as is evident from his already quoted quasi-empiricist remarks and also from his hesitation in his [1938], where he says that the axiom of constructibility 'seems to give a natural completion of the axioms of set theory, in so far as it determines the vague notion of an arbitrary infinite set in a definite way'.[6]) Weyl actually made fun of Gödel's over-optimistic stretching of the possibilities of *a priori* knowledge:

> Gödel, with his basic trust in transcendental logic, likes to think that our logical optics is only slightly out of focus and hopes that after some minor correction of it we shall see ***sharp***, and then everybody will agree that we see ***right***. But he who does not share this trust will be disturbed by the high degree of arbitrariness involved in a system like *Z*, or even in Hilbert's system. How much more convincing and closer to facts are the heuristic arguments and the subsequent systematic constructions in Einstein's general relativity theory, or the Heisenberg

[1] Such strong axioms were formulated by Mahlo, Tarski and Levy. As to the arithmetical testability of these axioms:

> It can be proved that these axioms also have consequences far outside the domain of very great transfinite numbers, which is their immediate subject matter: each of them, under the assumption of its consistency, can be shown to increase the number of decidable propositions even in the field of Diophantine equations. (Gödel [1947]. p. 520)

[2] This line of research was initiated by Turing ([1939]) and developed by Feferman ([1968]).

[3] *Cf. e.g.* Rosser [1937]; Tarski [1939]; Kleene [1943].

[4] *Cf.* Kemeny [1958], p. 164.

[5] Gödel [1964], p. 264 (*Cf.* Gödel [1947], p. 520).

[6] Gödel [1938], p. 557.

–Schrödinger quantum mechanics. A truly realistic mathematics should be conceived, in line with physics, as a branch of the theoretical construction of the one real world, and should adopt the same sober and cautious attitude towards hypothetic extensions of its foundations as is exhibited by physics.[1]

Kreisel, however, extols this sort of aprioristic reflection by which, he claims, one gains set theoretical axioms, and 'right' definitions, and calls anti-apriorism an 'antiphilosophic attitude' and the idea of progress by trial and error empirically false.[2] What is more, in his reply to Bar–Hillel, he wants to extend this method to science, thereby re-discovering Aristotelian essentialism. He adds: 'If I were really convinced that reflection is extra-ordinary or illusory I should certainly not choose philosophy as a profession; or, having chosen it, I'd get out fast'.[3] In his comment on Mostowski's paper he tries to play down Gödel's hesitation as out of date.[4] But just as Gödel immediately refers to inductive evidence, Kreisel refers (in the Reply) to the 'limitations' of the heuristic of reflection. (So, after all, 'reflection', 'explication' *is* fallible.)

4 THE POTENTIAL FALSIFIERS OF MATHEMATICS

If mathematics and science are both quasi-empirical, the crucial difference between them, if any, must be in the nature of their 'basic statements', or 'potential falsifiers'. The 'nature' of a quasi-empirical theory is decided by the nature of the truth-value injections into its potential falsifiers.[5] Now nobody will claim that mathematics is empirical in the sense that its potential falsifiers are singular spatio-temporal statements. But then what is the nature of mathematics? Or, what is the nature of the potential falsifiers of mathematical theories?[6] The very question would have been an insult in the years of intellectual honeymoon of Russell or Hilbert. After all, the *Principia* or the *Grundlagen der Mathematik* were meant to put an end—once and for all—to counterexamples and refutations in mathematics. Even now the question still raises some eyebrows.

But comprehensive axiomatic set theories and systems of metamathematics, can be, and indeed have been, refuted. Let us first take comprehensive axiomatic set theories. Of course, they have *potential logical falsifiers:* statements of the form $p \,\&\, \neg p$. But are there other falsifiers? The potential falsifiers of science, roughly speaking, express the 'hard facts'. But is there anything analogous to 'hard facts' in mathematics? If we accept the view that a formal axiomatic theory implicitly defines its subject-matter, then there would be no mathematical falsifiers except the logical ones.

[1] Weyl, *op. cit.*, p. 235. [2] Kreisel [1967*a*], p. 140. [3] Kreisel [1967*b*], p. 178.
[4] Kreisel [1967*c*], pp. 97–8. [5] See *above*, p. 206.
[6] It is hoped that this Popperian formulation of the age-old question will shed new light on some questions in the philosophy of mathematics.

But if we insist that a formal theory should be the formalisation of some informal theory, then a formal theory may be said to be 'refuted' if one of its theorems is negated by the corresponding theorem of the informal theory. One could call such an informal theorem a *heuristic falsifier* of the formal theory.[1]

Not all formal mathematical theories are in equal danger of heuristic refutation in a given period. For instance, *elementary group theory* is scarcely in any danger: in this case the original informal theories have been so radically replaced by the axiomatic theory that heuristic refutations seem to be inconceivable.

Set theory is a subtler question. Some argue that after the total destruction of naive set theory by *logical* falsifiers one cannot speak any more of set-theoretical facts: one cannot speak of an *intended* interpretation of set theory any more. But even some of those who dismiss set-theoretical intuition may still agree that axiomatic set theories perform the task of being the dominant, unifying theory of mathematics in which all available mathematical facts (*i.e.* some specified subset of informal theorems) have to be explained. But then one can criticise a set theory in two ways: its axioms may be tested for consistency and its definitions may be tested for the 'correctness' of their translation of branches of mathematics like arithmetic. For instance, we may some day face a situation where some machine churns out a formal proof in a formal set theory of a formula whose intended meaning is that there exists a non-Goldbachian even number. At the same time a number theorist might prove informally that all even numbers are Goldbachian. If his proof can be formalised within our system of set-theory, then our theory will be inconsistent. But if it cannot be thus formalised, the formal set theory will not have been shown to be inconsistent, but only to be a *false* theory of arithmetic (while still being possibly a true theory of some mathematical structure that is not isomorphic with arithmetic). Then we may call the informally proved Goldbach theorem a *heuristic falsifier*, or more specifically, an *arithmetical falsifier* of our formal set theory.[2] The formal theory is false in respect of

[1] It would be interesting to investigate how far the demarcation between logical and heuristic falsifiers corresponds to Curry's demarcation between mathematical truth and 'quasi-truth' (or 'acceptability'). *Cf.* his [1951], especially ch. xi. Curry calls his philosophy 'formalist' as opposed to '*inhaltich*' or 'contensive' philosophies, like Platonism or intuitionism (Curry [1965], p. 80). However, besides his philosophy of formal structure, he has a philosophy of acceptability—but surely one cannot explain the growth of formal mathematics without acceptability considerations, so Curry offers an '*inhaltlich*' philosophy after all.

[2] The expression 'ω-consistency', is as Quine pointed out (Quine [1953] p. 117.), misleading. A demonstration of the 'ω-inconsistency' of a system of arithmetic would in fact be a *heuristic* falsification of it. Ironically, the historical origin of the misnomer was that the phenomenon was used by Gödel and Tarski precisely to divorce truth ('ω-consistency') from consistency.

the informal *explanandum* that it had set out to explain; we have to replace it by a better one. First we may try piecemeal improvements. It may have been only the definition of 'natural number' that went wrong and then the definition could be 'adjusted' to each heuristic falsifier. The axiomatic system itself (with its formation and transformation rules) would become useless as an explanation of arithmetic only if it was altogether 'numerically insegregative',[1] *i.e.* if it turned out that no finite sequence of adjustments of the definition eliminates *all* heuristic falsifiers.

Now the problem arises: *what class of informal theorems should be accepted as arithmetical falsifiers of a formal theory containing arithmetic?*

Hilbert would have accepted only finite numerical equations (without quantifiers) as falsifiers of formal arithmetic. But he could easily show that *all* true finite numerical equations are provable in his system. From this it followed that his system was complete with regard to true basic statements, therefore, if a theorem in it could be proved false by an arithmetical falsifier, the system was also inconsistent, for the formal version of the falsifier was already a theorem of the system. Hilbert's reduction of falsifiers to logical falsifiers (and thereby the reduction of truth to consistency) was achieved by a very narrow ('finitary') definition of arithmetical basic statements.

Gödel's informal proof of the truth of the Gödelian undecidable sentence posed the following problem: is the *Principia* or Hilbert's formalised arithmetic—on the assumption that each is consistent—true or false if we adjoin to it the negation of the Gödel sentence? According to Hilbert the question should have been meaningless, for Hilbert was an instrumentalist with regard to arithmetic outside the finitary kernel and would not have seen any difference between systems of arithmetic with the Gödel sentence or with its negation as long as they both equally implied the true basic statements (to which, by the way, his implicit meaning-and-truth-definition was restricted). Gödel proposed[2] to extend the range of (meaningful and true) basic statements from finitary numerical equations also to statements with quantifiers and the range of proofs to establish the truth of basic statements from 'finitary' proofs to a wider class of intuitionistic methods. It was this methodological proposal that divorced truth from consistency and introduced a new pattern of conjectures and refutations based on arithmetical falsifiability: it allowed for daring speculative theories with very strong, rich axioms while criticising them from the outside by informal theories with weak, parsimonious axioms. *Intuitionism is here used not for providing foundations but for*

[1] See Quine, *loc. cit.*, p. 118.
[2] See his intervention in 1930 in Königsberg; recorded in Gödel [1931].

providing falsifiers, not for discouraging but for encouraging and criticising speculation!

It is surprising how far constructive and even finite falsifiers can go in testing comprehensive set theories. Strong axioms of infinity for instance are testable in the field of Diophantine equations.[1]

But comprehensive axiomatic set theories do not have only arithmetical falsifiers. They may be refuted by theorems—or axioms—of naive set theory. For instance Specker 'refuted' Quine's *New Foundations* by proving in it that the ordinals are not well-ordered by '$\leqslant$' and that the axiom of choice must be given up.[2] Now *is* this 'refutation' of the *New Foundations*, even a heuristic refutation? Should the well-ordering theorem of shattered naive set theory overrule Quine's system? Even if, with Gödel and Kreisel, we consider naive set theory as re-established by Zermelo's correction,[3] we could admit the well-ordering theorem and the axiom of choice as a heuristic falsifier only if we again extend the class of (intuitionistic) heuristic falsifiers to (almost?) *any* theorem in corrected naive set theory. (We may call the former the class of *strong heuristic falsifiers* and the latter the class of *weak heuristic falsifiers*.) But this would surely be irrational: at best we have to consider them as two rival theories (*strictly* speaking *no* heuristic falsifier can be more than a rival hypothesis). After all nothing prevents us from forgetting about naive sets and focusing our attention on the new unintended model of *New Foundations*![4]

Indeed, we can go even further. For instance, if it turned out that all strong set-theoretical systems are arithmetically false, we may modify our arithmetic—the new, non-standard arithmetic may possibly serve the empirical sciences just as well. Rosser and Wang, who—three years before Specker's result—showed that in no model of *New Foundations* does '$\leqslant$' well-order both finite cardinals and infinite ordinals as long as we stick to the intended interpretation of '$\leqslant$', discuss this possibility:

One may well question whether a formal logic which is known to have no standard model is a suitable framework for mathematical reasoning. The proof of the pudding is in the eating. For topics in the usual range of classical mathematical analysis, the reasoning procedures of Quine's *New Foundations* are as close to the accepted classical reasoning procedures as for any system known to us. However, in certain regions, notably when dealing with extremely large ordinals, the reasoning procedures of Quine's *New Foundations* reflect the absence of a standard model, and appear strange to the classically minded mathematician. However, since the theory of ordinals is suspect when applied

[1] See *above*, p. 212, fn. 1
[2] Specker [1953]; also *cf.* Quine [1963], p. 294 ff.
[3] *Cf.* Gödel [1947], p. 518 and Kreisel [1967].
[4] For philosophers of science after Popper it should anyway be a commonplace that *explanans* and *explanandum* may be rival hypotheses.

to very large ordinals, it is hardly a serious defect in a logic if it makes this fact apparent.

We suspect that the idea that a logic must have a standard model if it is to be acceptable as a framework for mathematical reasoning is merely a vestige of the old idea that there is such a thing as absolute mathematical truth. Certainly the requirements on a standard model are that it reflect certain classically conceived notions of the structure of equality, integers, ordinals, sets, *etc.* Perhaps these classically conceived notions are incompatible with the procedures of a strong mathematical system, in which case a formal logic for the strong mathematical system could not have a standard model.[1]

This of course amounts to the claim that the only real falsifiers are logical ones. But other mathematicians, Gödel for example, would surely reject the *New Foundations* on Specker's refutation: for him the axiom of choice and the well-ordering of ordinals are self-evident truths.[2]

No doubt the problem of basic statements in mathematics will attract increasing attention with the further development of comprehensive set theories. Recent work indicates that some very abstract axioms may soon be found testable in most unexpected branches of classical mathematics: *e.g.* Tarski's axiom of inaccessible ordinals in algebraic topology.[3] The continuum hypothesis also will provide a testing ground: the accumulation of further intuitive evidence against the continuum hypothesis may lead to the rejection of strong set theories which imply it.[2] Gödel [1964] enumerates quite a few implausible consequences of the continuum hypothesis: a crucial task of his new Euclidean programme is to provide a self-evident set theory from which its negation is derivable.[4]

If one regards comprehensive set theories—and mathematical theories in general—as quasi-empirical theories, a host of new and interesting problems arise. Until now the main demarcation has been between the

[1] Rosser and Wang [1950], p. 115.

[2] In his original paper [1947], Gödel says that the axiom of choice is exactly as evident as the other axioms 'in the present state of our knowledge' (p. 516). In the 1964 reprint (Gödel [1964]) this has been replaced by 'from almost every possible point of view' (p. 259 n. 2). He proposed, after some hesitation, a further extension of the range of set-theoretical basic statements that in fact amounted to a new Euclidean programme—but immediately proposed a quasi-empirical alternative in the case of failure. (See especially the supplement to his [1964].)

[3] *Cf.* Myhill [1960], p. 464.

[4] Kreisel criticises Gödel (Kreisel [1967*a*]) for not discussing his turn from proposing the constructibility axiom as a completion of set theory in 1938 to surreptitiously withdrawing it in 1947. One would think the reason for the turn is obvious: in the meantime he must have studied the work done on the consequences of the continuum hypothesis (mainly by Lusin and Sierpinski) and must have come to the conclusion that a set theory in which the hypothesis is deducible (like the one he suggested in 1938) is false. It may be interesting to note that according to Lusin a simple proposition in the theory of analytic sets which Sierpinski showed to be incompatible with the continuum hypothesis is 'indubitably true'—indeed he puts forward an impressive argument (Lusin [1935] and Sierpinski [1935]).

proved and the unproved (and the provable and unprovable); radical justificationists ('Positivists') equated this demarcation with the demarcation between meaningful and meaningless. But now there will be a new demarcation problem: ***the problem of demarcation between testable and untestable (metaphysical) mathematical theories with regard to a given set of basic statements.*** Certainly one of the surprises of set theory was the fact that theories about sets of very high cardinality are testable in respect to a relatively modest kernel of basic statements (and thus have arithmetical content).[1] Such a criterion will be interesting and informative—but it would be unfortunate if some people should want to use it again as a meaning criterion as happened in the philosophy of science.

Another problem is that testability in mathematics rests on the slippery concept of heuristic falsifier. A heuristic falsifier after all is a falsifier only in a Pickwickian sense: it does not falsify the hypothesis, it only suggests a falsification—and suggestions can be ignored. It is only a rival hypothesis. But this does not separate mathematics as sharply from physics as one may think. Popperian basic statements too are only hypotheses after all. ***The crucial role of heuristic refutations is to shift problems to more important ones***, to stimulate the development of theoretical frameworks with more content. One can show of most classical refutations in the history of science and mathematics that they are heuristic falsifications. The battle between rival mathematical theories is most frequently decided also by their relative explanatory power.[2]

Let us finally turn to the question: ***what is the 'nature' of mathematics***, that is, on what basis are truth values injected into its potential falsifiers? This question can be in part reduced to the question: What is the nature of *informal* theories, that is, what is the nature of the potential falsifiers of *informal* theories? Are we going to arrive, tracing back problem-shifts through informal mathematical theories to empirical theories, so that mathematics will turn out in the end to be ***indirectly empirical***, thus justifying Weyl's, von Neumann's and—in a certain sense—Mostowski's and Kalmar's position? Or is ***construction*** the only source of truth to be injected into a mathematical basic statement? Or ***platonistic intuition***? Or ***convention?*** The answer will scarcely be a monolithic one. Careful historico-critical case-studies will probably lead to a sophisticated and composite solution. But whatever the solution may be, the naive school concepts of static rationality like ***apriori-aposteriori***, ***analytic-synthetic*** will only hinder its emergence. These notions were devised by classical epistemology to

[1] The term 'content' is here used in a Popperian sense: the 'arithmetical content' is the set of arithmetical falsifiers.

[2] *Cf.* Lakatos [1977]. **Editors' Note:* This study in rival research programmes in the theory of the continuum will be published in a volume of collected essays.

classify Euclidean certain knowledge—for the problem-shifts in the growth of quasi-empirical knowledge they offer no guidance.*

5 PERIODS OF STAGNATION IN THE GROWTH OF QUASI-EMPIRICAL THEORIES

This history of quasi-empirical theories is a history of daring speculations and dramatic refutations. But new theories and spectacular refutations (whether logical or heuristic) do not happen every day in the life of quasi-empirical theories, whether scientific or mathematical. There are occasional long *stagnating periods* when a single theory dominates the scene without having rivals or acknowledged refutations. Such periods make many forget about the criticisability of the basic assumptions. Theories, which looked counterintuitive or even perverted when first proposed, assume authority. Strange methodological delusions spread: some imagine that the axioms themselves start glittering in the light of Euclidean certainty, others imagine that the deductive channels of elementary logic have the power to retransmit truth (or probability) 'inductively' from the basic statements to the extant axioms.

The classical example of an abnormal period in the life of a quasi-empirical theory is the long domination of Newton's mechanics and theory of gravitation. The theory's paradoxical and implausible character put Newton himself into despair: but after a century of corroboration Kant thought it was self-evident. Whewell made the more sophisticated claim that it had been solidified by 'progressive intuition',[1] while Mill thought it was inductively proved.

Thus we may name these two delusions 'the Kant–Whewell delusion', and the 'inductivist delusion'. The first reverts to a form of Euclideanism;

* *Editors' note:* Since this paper was written a good deal of further work has been done on testing proposed set theoretical axioms, like the continuum hypothesis and strong axioms of infinity. (A good survey is to be found in Fraenkel, Bar Hillel and Levy [1973]. See also Shoenfield [1971] for the axiom of measurable cardinals.) Levy and Solovay's work ([1967]) indicates that large cardinal axioms will not decide the continuum problem. As another line of attack, alternatives to the continuum hypothesis have been formulated and tested. An example is 'Martin's axiom', which is a consequence of the continuum hypothesis, but consistent with its negation (see Martin and Solovay [1970] and Solovay and Tennenbaum [1971]). Of the six consequences of the Continuum Hypothesis which Gödel regarded as highly implausible, three follow also from Martin's Axiom. But Martin and Solovay take a different attitude to that taken by Gödel. They have, they say, 'virtually no intuitions' about the truth or falsity of these three consequences.

Lakatos does not, by the way, distinguish between two different types of mathematical consequences of axioms of this kind. Many of these consequences, for example about constructible real numbers, despite being about 'ordinary' mathematical objects are of interest only to set theoreticians. Few such consequences will be of a kind which are testable against informally proved mathematical theorems.

[1] *E.g.* Whewell [1860], especially ch. xxix.

the second establishes a new—inductivist—ideal of deductive theory where the channels of deduction can also carry truth (or some quasi-truth like probability) upwards, from the basic statements to the axioms.

The main danger of both delusions lies in their methodological effect: both trade the challenge and adventure of working in the atmosphere of permanent criticism of quasi-empirical theories for the torpor and sloth of a Euclidean or inductivist theory, where axioms are more or less established, where criticism and rival theories are discouraged.[1]

The gravest danger then in modern philosophy of mathematics is that those who recognise the fallibility and therefore science-likeness of mathematics, turn for analogies to a wrong image of science. The twin delusions of 'progressive intuition' and of induction can be discovered anew in the works of contemporary philosophers of mathematics.[2] These philosophers pay careful attention to the degrees of fallibility, to methods which are *a priori* to some degree, and even to degrees of rational belief. But scarcely anybody has studied the possibilities of refutations in mathematics.[3] In particular, nobody has studied the problem of how much of the Popperian conceptual framework of the logic of discovery in the empirical sciences is applicable to the logic of discovery in the quasi-empirical sciences in general and in mathematics in particular. *How can one take fallibilism seriously without taking the possibility of refutations seriously?* One should not pay lip-service to fallibilism: 'To a philosopher there can be nothing which is absolutely self-evident' and then go on to state: 'But in practice there are, of course, many things which can be called self-evident ... each method of research presupposes certain results as self-evident.'[4] Such *soft fallibilism* divorces fallibilism from criticism and shows how deeply ingrained the Euclidean tradition is in mathematical philosophy. It will take more than the paradoxes and Gödel's results to prompt philosphers to take the empirical aspects of mathematics seriously, and to elaborate a philosophy of critical fallibilism, which takes inspiration not from the so-called foundations but from the *growth* of mathematical knowledge.

[1] *Cf.* Kuhn, especially his [1963].

[2] The main protagonists of Whewellian progressive intuition in mathematics are Bernays, Gödel, and Kreisel (see *above*, pp. 212–13). Gödel also provides an inductivist criterion of truth, should progressive (or as Carnap would call it 'guided') intuition fail: an axiomatic set theory is true if it is richly verified in informal mathematics or physics. 'The simplest case of an application of the criterion under discussion arises when some set-theoretical axiom has number-theoretical consequences verifiable by computation up to any given integer' (Supplement to Gödel [1964], p. 272).

[3] Kalmar—with his criticism of Church's thesis—is a notable exception (see Kalmar [1959]).

[4] Bernays [1965], p. 127

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