What is Dialectical Philosophy of Mathematics?[1]

The late Imre Lakatos once hoped to found a school of dialectical philosophy of mathematics.[2] The aim of this paper is to ask what that might possibly mean. However, Lakatos' own philosophy comes dressed in Popperian clothes. Mathematics, in his work, is driven along by counterexamples to conjectured theorems. Admittedly he swiftly moves from strictly logical counterexamples to 'heuristic' ones.[3] A 'heuristic' counterexample is an object or phenomenon which, while not strictly inconsistent with the theorem in hand, nevertheless indicates some shortcoming in it (such as a lack of generality or explanatory power). Perhaps Lakatos intended this transition from logical to heuristic counterexamples as a sort of bridge from a simple Popperian picture of progress in mathematics to a more richly dialectical conception. Here though he suffers from the difficulty that even Popper did not take a 'Popperian' view of mathematics. Rhetorical subtleties aside, Lakatos is associated with the superficially exciting claim that mathematics suffers refutations and revolutions akin to those found in the history of the physical sciences. To extend and develop his work is (it seems) to search the history of mathematics for refutations, or at least for items that might by some stretch of language count as 'falsifiers'. Refutations turn out to be rare in general and almost non-existent in twentieth-century mathematics. Of course, more sympathetic readings find a richer, more Hungarian Lakatos hiding beneath the Popperian gloss.[4] However, exploring these readings raises the danger that the task of getting Lakatos right may displace that of developing a philosophical understanding of mathematics itself.

Some scholars look to Lakatos' work on the physical sciences to guide us in extending and developing his philosophy of mathematics. This, though, suffers from a related drawback. His 'methodology of scientific research programmes' shares the ambition common to all forms of demarcationism. This aim was to articulate a universal standard for the evaluation of scientific work, valid at all times and in all places. If we once admit this goal, then we inevitably find ourselves side-tracked into an argument about the very possibility of such an ahistorical definition of 'good science'. That debate absorbed the energies of many philosophers of science. There is no obvious benefit in revisiting it in the philosophy of mathematics. Moreover, a demarcationist 'methodology' of science is, necessarily, topic-neutral. It must be able to decide the scientific status of *any* sort of enquiry. Its criteria must apply to Newton and Marx, to Freud and Einstein. I shall argue below that such highly general methodological models are at best blunt instruments for understanding patterns of thought proper to mathematics.

None of this is to deny that Lakatos is a hugely important figure in the philosophy of mathematics. But his importance does not lie in his fallibilism (which turns out to be quite innocuous once the Popperian paint is peeled off). His significance lies, rather, in having turned philosophical attention to what one might call the 'inner life' of mathematics. For most of the twentieth century epistemologists of mathematics have assumed a 'deductivist' model of mathematical argument. On this view the sole point of a mathematical proof is to derive the desired theorem from explicitly stated premises. The philosophy of mathematics has, consequently, been dominated by deductive logic. More to the point, it has been dominated by

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[1] This paper was first given as talk for the Centre for Philosophical Studies at King's College (London) 20th October 1999, and later read at the University of Hertfordshire. I am grateful for some penetrating questions on those occasions, and to Richard Ashcroft, David Corfield, Lewis Griffin, Mary Leng, Yehuda Rav and some anonymous referees, who were each kind enough to read a draft.

[2] In a letter to Marx Wartofsky, now in the LSE Lakatos archive (folder 12.1, item 12).

[3] [1976] p. 83.

[4] See Larvor [1998].

formal logic—that is, logic in which the content of an argument plays no role in the inference, which is carried entirely by the argument form. Philosophical programmes that concentrate on formal logic inevitably lose touch with the content of the arguments they set out to understand. That sort of work has its philosophical merits, and formal logic is of course a worthwhile branch of mathematics in its own right.[5] Nevertheless the mainstream English-speaking tradition leaves interesting philosophical questions unasked. Lakatos' unfulfilled wish enjoins us to find dialectical ways of asking them without reproducing his shortcomings.

The Dialectical Philosopher

Suppose we accept, for the sake of argument, that the sole point of a mathematical proof is to derive a theorem from freely accepted premises by deductively valid steps. Suppose further (which is not the case) that there is no controversy about the logical system into which mathematical proofs are to be formalised. We may still reasonably enquire how the premises for proofs are chosen. Why should mathematicians explore the deductive closure of *this* set of axioms rather than *that*?

It might be argued that this question has no philosophical significance. *Mathematicians are free to choose any consistent set of axioms they please. Since consistency is the only constraint, the choice must be a matter of subjective preference and is therefore philosophically uninteresting*. However, mathematicians do not settle on sets of axioms just because they like the look of them. They do not feel themselves to be constrained by consistency alone. When they evaluate a piece of research they are not satisfied with the information that all the theorems have been validly deduced from consistent premises. Some work is judged important, some trivial, some promising, some not, and so forth. These judgments are sometimes contested and then there may be an exchange of reason-giving. Evangelists for category theory, for example, see in it other virtues in addition to consistency. Reasoned evaluation is not arbitrary, even if it has a subjective element to it. It is of course possible to re-describe these choices in entirely sociological or psychological terms, but to do that is to give up philosophy in favour of social science.[6] It is to abandon the inside-phenomenological stance for the outside-observer position.

We can now begin to characterise dialectical philosophy of mathematics. The task is a little delicate because the word 'dialectical' has a long history and has acquired some notoriety. First, the dialectical philosopher of mathematics adopts what I have just called the 'inside-phenomenological stance'. Do not let the word 'phenomenological' mislead you. This is **not** a study of what it feels like to do mathematics. The phenomenologist takes up a point of view and studies its logical constitution as it were 'from the inside'. But we are not concerned with the individual mathematician. We are interested in the 'point of view' belonging to mathematics itself. This way of speaking is of course analogical. Mathematics has no subjectivity in the proper sense. It feels neither joy nor pain. Nevertheless, the analogy is not mysterious. We can say that the theory of projectile motion is 'blind' to the ethical difference between a distress flare and an assassin's bullet (though theor*ists* are not). There is no mystery in the remark that analysis was 'conflicted' over the rival versions of the early calculus (though individual mathematicians clearly supported one over the other). Thus the sense of 'phenomenological' in play here is something like that in Hegel's *Phenomenology of Spirit*.[7]

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[5] An anonymous referee observed that "Formal logic as an *object* of mathematical study should not be confused with the deductive logic used *in* maths. The are not necessarily the same." This is true and, indeed, part of the point.

[6] David Bloor's 'strong programme' in the sociology of knowledge is the most obvious instance, though the point applies to any version of naturalised epistemology that does not take seriously the problem of normativity.

[7] Gian-Carlo Rota [1997] finds a similar philosophical orientation in Husserl (p. 184).

The point of the inside-phenomenological stance is to insist that changes in the body of mathematics normally take place for mathematical reasons. The dialectical philosopher assumes the rationality and integrity of mathematical inquiry just as the humanist philosopher assumes the rationality and integrity of human subjects ('integrity' here means the stability and coherence that allow us to speak of the *character* of this person or that sub-field). These assumptions are, of course, qualified by the influence of historical and environmental factors—neither people nor fields of mathematical research can entirely escape the conditions under which they grow up. Statistics, for example, could not develop until there were organisations with an interest in gathering large bodies of standardised data. Similarly, it is hard to see how the study of strange attractors could have preceded the computer age. Moreover the rationality and integrity assumptions are defeasible in particular cases. Human beings sometimes suffer delusions, or dementia. Analogously, mathematical development may be distorted by ideological interference, stymied by academic rivalries or halted by the fall of empires. Nevertheless, the default position is that the direction of mathematical development and the response of mathematics to external stimuli are both best explained by factors proper to mathematics itself.

To switch vocabulary for a moment, the dialectical philosopher of mathematics insists on the possibility of 'internal history'. Now, the distinction between internal and external history cannot be made with the clarity that Lakatos hoped for. Aside from the defeasibility of internalist assumptions, the fact is that much historical material could fit equally well into either internalist or externalist stories. For example, "Soon after Wiles gave his proof, an error was spotted and he had to withdraw it." This sentence could be found equally well in an analysis of the grid/group structure of the mathematical community or in a rationally-reconstructed case-study. However, dialectical philosophy does not require a sharp distinction between internal and external factors because it does not share the logical positivist and Popperian horror of 'psychologism'. In Popper this determination to insulate science from the human foibles of scientists produced the absurd notion of 'knowledge without a knowing subject'. Dialectical philosophy, in contrast, typically recognises that human minds, however fallible, are the only available vehicles for the greater rationality of science. All that the dialectical philosopher need insist on is that the general direction of a historical development is best explained by an analysis of the concepts governing that development. This assumption may be implausible in military, political or economic history, but it is in just this respect that science and mathematics differ from most other human activities.

Notice that the object of study is mathematical *development* rather than truth or validity. Human rationality reveals itself in speech and action. It is not a tenseless state that can be inspected without reference to time and change. Similarly, the dialectical philosopher of mathematics seeks rationality and integrity in the development of mathematics. Does the mathematical community reach the right choices for the right reasons? Remember we noticed that even on a deductivist picture of mathematical argument it is necessary to choose the axioms. Real mathematicians have to choose problems, techniques and proof-strategies. The mathematical community has to decide when to treat a result as proven and worthy of celebration. These choices (to reiterate) are neither arbitrary nor 'simply subjective'. They are, however, time-bound. I choose a problem now in the belief that it will be fruitful in the future. The community declares a theorem proven, confident that it will not have to withdraw that status later. When a result is judged to be significant, it is deemed to have staying power.

Clearly the dialectical philosophy of mathematics sketched here owes something to Hegel. This debt is compatible with the spirit of Lakatos, who named Hegel as one of the three 'ideological sources' of the Ph.D. thesis that eventually became *Proofs and Refutations*.[8]

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[8] See Larvor [1999] for more on the relation between Lakatos and Hegel.

Nevertheless one may reasonably worry that the present project might inherit some of Hegel's shortcomings. Does it require actually-existing contradictions? What of the unity of opposites? The transformation of quantity into quality? The famous negation of the negation? We must of course endeavour to learn what we can from Hegel, but his view suffers from such severe defects that we should not be tempted to adopt it wholesale. Let us see what can be salvaged.

What is correct in Hegel is the thought that dialectic principally concerns concepts rather than propositions. Formal logic requires terms to keep the same meaning from the beginning of an argument to the end. Otherwise the argument falls into the fallacy of equivocation. (Remember that a formal argument is one in which the non-logical content plays no role in the inference. This indifference to content is only possible so long as the non-logical terms are fixed.) In a dialectical argument, however, the meanings of the central terms develop as the argument uncovers defects in the primitive concepts with which the question in hand was first posed. 'Justice' means something quite different at the end of Plato's *Republic* than it did at the beginning. This distinction between the formal logic of propositions and the dialectical logic of concepts is obscure in Hegel for two reasons. First, in his day the only formal logic was the categorical syllogistic. Therefore it seemed that all logic deals with relations between categories; in formal logic the categories are fixed, whereas in dialectical logic they are permitted to develop. Second, a typical Hegelian concept is so rich and internally complex that perhaps 'conception' would be a better translation of Hegel's *Begriff* than the more modest 'concept' (some translations side-step this ambiguity by rendering *Begriff* as 'notion'). Nevertheless, Hegel understood that formal and dialectical logic are strictly incompatible. What is fallacious in the former is the very point of the latter. Indeed, his determination to keep them apart is the source of his undoing. He attempted to produce a pure dialectic in which each stage in the development of a concept gives rise to its successor without the concept-term ever appearing in a sequence of propositions. The famous slogans (the *unity of opposites*, the *transformation of quantity into quality*, and the *negation of the negation*) are Hegel's attempt to explain how to get from concept to concept (or from concept-stage to concept-stage) without invoking any formal logic.[9]

Hegel's thought here is entirely reasonable—formal and dialectical logics have different aims and incompatible standards of rigour, so we ought not to mix them up. However, concepts only come to life when they are used in propositions. Hypotheses—not concepts—are vulnerable to counterexamples. Theories may be faulted for a lack of scope or explanatory capacity, but concepts (strictly speaking) may not. A concept remains inert if it is not put to work in a conjecture, theory, theorem-candidate or proof. Where the dialectical transitions in Hegel are obscure and unconvincing, it is I think for this reason, that he tries to get from concept to concept without the terms in question figuring in propositions. This leads him into the category-error of asking whether a given concept is true. Sometimes Hegel's dialectical stories do make sense, but in most of these cases the 'concepts' in play are so rich that they are more like models or theories. Such 'conceptions' are normally conceptions *of* something specific, and may therefore be assessed for accuracy. In such cases Hegel evades the charge of category confusion. On this point Lakatos has the advantage over Hegel. In *Proofs and Refutations* the dialectical development of concepts is structured and motivated by the effort to prove a theorem. The formal logic of propositions and the dialectical logic of

[9] "One difficulty which should be avoided comes from mixing up the speculative with the ratiocinative methods, … As a matter of fact, non-speculative thinking also has its valid rights which are disregarded in the speculative [i.e. dialectical] way of stating a proposition." (*Phenomenology of Spirit* §§64-65). Hegel goes on to argue that the nature of language requires us to express dialectical thoughts in propositional form. This is unfortunate, he claims, because the rigidity of grammar obscures the fluid dynamism of dialectical thinking. (*Op. cit.* §66).

concepts are both necessary if a field is to develop (Lakatos calls them language-statics and language-dynamics respectively[10]). They are in tension, since formal logic is conceptually conservative while dialectical logic is innovative, but this tension is proper to any intellectual discipline.[11] We have already noted that Lakatos' view is excessively Popperian, and that the patterns in *Proofs and Refutations* are not readily found in twentieth-century mathematics. Nevertheless on this point—the necessary inter-penetration of formal and dialectical logics—he is correct.

So far, then, our dialectical philosopher tracks the development of mathematics from the inside in the sense described above. He or she is especially interested in the emergence of new concepts. This in turn leads to a concern with the evaluative discourse of mathematicians. We hope to tease out the grounds on which a body of research is said to be deep, fruitful, promising, trivial or sterile. We do this with no specific dialectical model in hand. The ambition is to describe the rationale of mathematical research as we find it, rather than to press it into some pre-formed mould. The dialectical philosopher is, in this sense, a methodological anarchist.

The final point in this description of the dialectical philosopher of mathematics is as follows: he or she has nothing to say about the ultimate ontological status of mathematics or its objects. Whether we adopt fictionalism; or embrace a kind of emergentism in which mathematics produces itself out of the activities of mathematicians; or whether we think of progress as ever-closer approximation to a pre-existing Platonic reality, makes no difference to our study of the inner logic of mathematical development. The dialectical stories turn out the same regardless of any ontological commitment. Of course, *mathematicians* may hold metaphysical views which affect the development of the discipline, but this is part of what the dialectical philosopher hopes to understand. Since the dialectical stories are not altered by anterior ontological constraints, the dialectical philosopher has no criteria for choosing between ontological doctrines. This is part of the motive for introducing the term 'phenomenology'. In its own way the phenomenological tradition is as sceptical as empiricism about the possibility of 'ultimate' metaphysical knowledge. It is possible, in phenomenology, to do 'relative metaphysics' i.e. to compare the presuppositions of one area of human life with another. Mathematics is a tool for physical science as well as a study in its own right. We can compare the metaphysical features of empirical and mathematical objects, since this contrast occurs within human experience. What we cannot do, as phenomenologists, is to make claims about the world beyond our experience (reading the word 'experience' more widely than is usual in the empiricist tradition).[12] Hence, the programme here outlined may attract those who still feel that there is something illegitimate about ontological questions but who cannot accept the empiricist diagnosis. It will, for the same reason, repel those who believe that ontological questions are indispensable to philosophy.

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[10] [1976] p. 93.

[11] "According to traditional static rationality you have to make the first choice. Science teaches you to make the second" (1976 p. 93). There is in this something of Feyerabend's mission to protect anarchic science from the dead hand of philosophical rationalism. In Kuhn the opposition between conceptual innovation and conservation is realised socially as the 'essential tension': scientists are reluctant to upset the dominant paradigm, but they can if they must.

[12] A phenomenologist may allow that there might be other philosophical perspectives from which ontological questions are both important and tractable. The opposite is arguable: in a recent book (*Platonism and Anti-Platonism in Mathematics*, New York: Oxford University Press, 1998) Mark Balaguer argues that Platonism and fictionalism are equally well-supported, and that there is, therefore, no fact of the matter regarding the existence of mathematical objects.

Cases

So far we have a programmatic description of the dialectical philosopher of mathematics. But does this animal exist in nature? At best it is bound to be rare because, first, a dialectical philosopher of mathematics must be a philosopher. In particular he or she must understand the history of philosophy well enough to see how the programme described here differs from other philosophical approaches to mathematics. Otherwise, one could not steer one's own research, and would have little to contribute that could not come from a thoughtful mathematician.[13] In addition, one must master some serious mathematics, especially if one proposes to work on twentieth century material (though it is not necessary to have done mathematical research oneself). Moreover, one would hope to exercise some discretion in selecting cases for study. *Proofs and Refutations* is often criticised on the grounds that the Descartes-Euler conjecture is a singular case. No other theorem, it is said, developed in quite that way. We need not now explore whether this charge against Lakatos is justified. The point is that in order to avoid this criticism one must choose cases that are in some way representative. That requires a broad overview of mathematics and its recent history.

A few figures from the recent history of philosophy embody some of the features I have described, such as Cavaillès, Hadamard or Pólya. However, to be forward-looking I have selected three living philosophers to illustrate the general case.[14] None of these has, to my knowledge, used the terms 'dialectical' or 'phenomenological' to describe themselves (but none objected to an earlier draft of this paper).

Yehuda Rav: Why do we prove theorems?

In the spring of 1999 Yehuda Rav, a mathematician at the University of Paris, published an article entitled 'Why do we prove theorems?' (*Philosophia Mathematica* vol. 7 part 1 Feb. 1999 pp. 5-41). He proposes a thought-experiment. Suppose that a machine called 'Pythiagora' were built that answers mathematical questions instantaneously and accurately (this fantasy obviously requires us to ignore some well-known results in formal logic). Instead of having to wait for human mathematicians to settle the Goldbach conjecture, we can just ask Pythiagora. Immediately the answer will flash back: 'true' or 'false' accordingly. What we do not get is a proof. If the function of proofs is solely epistemic, then we are no worse off, since we know (somehow) that Pythiagora never makes a mistake. The task of Rav's article is to argue by examples that proofs do a lot more than merely to confer certainty on theorems. His examples are richly diverse and mostly drawn from the twentieth century.

Rav's first cases restate a thought familiar from Pólya and Lakatos: failed proofs can lead to unforeseen theorems. Work on the Goldbach Conjecture led to a host of results in number theory and related fields (Rav gives a list on pp. 7-8). Of course, we could have got all this extra information from Pythiagora. However, many of the theorems discovered in the course of trying to decide the Goldbach Conjecture give answers to questions that no-one is likely to have thought of otherwise. Rav offers the Continuum Hypothesis as further illustration of this point. The Continuum Hypothesis is, like the Goldbach Conjecture, undecided so far.[15] Unlike the Goldbach Conjecture, which seems to be something of a logical

[13] Normally when we ask what a philosopher of *activity X* has to contribute that could not come from a thoughtful practitioner of *X*, the answer cites the special logical sensitivity that results from philosophical training. The philosopher is said to have an analytical acumen not given to others, and certainly not to practitioners of *X*. However plausible this answer may be in the case of education or ethics, it clearly will not wash for mathematics.

[14] There are others e.g. Paul Ernest, Paulo Mancosu and Jean-Pierre Marquis.

[15] In fact the cases are not quite parallel. The continuum hypothesis is known to be independent of the usual axioms of set theory, whereas nobody expects the Goldbach Conjecture to be independent of the axioms of arithmetic. In fact some mathematicians and philosophers (such as Hartry Field) believe the continuum hypothesis to be (not just undecided but) 'undecidable' in something like the same sense as the parallel postulate

dead end, the Continuum Hypothesis has all sorts of interesting consequences. What is more it is at the heart of set theory and intimately connected to topology. Critics often complain that Lakatos' study of the Descartes-Euler conjecture cannot be extended to more recent mathematics because Lakatos chose a case with an atypically high reliance on geometrical intuition. These criticisms cannot touch the Continuum Hypothesis. Here again, Rav gives a list of results arising from the as-yet-unsuccessful efforts to resolve the matter one way or the other (pp. 9-10).

Thus far, Rav restates and elaborates the case against deductivism. He goes on to argue (by giving instances) that very little mature modern mathematics could be formally axiomatised, even in principle. Thus he opposes the common view that mathematicians would be able to present their proofs as derivations from explicitly-stated axioms within a specified formal system, but for the length of the resulting papers. Mathematicians could not express their proofs in a formal logical system even if they wished to because mathematical arguments are not merely formal (in the sense explained above of indifference to non-logical content). That (argues Rav, p. 13) is why mathematical papers are so hard to read even when one has studied logic. The inferences appeal to features of the non-logical content, which is why one has to understand so much background material in order to grasp a mathematical argument. If the inferences of mathematical proofs were carried entirely by topic-neutral logic then one could check their validity easily (though without knowing what had been proved).

The case is worth making because deductivism is alive and well even among mathematicians. In the recent 'math wars' over the nature of proof the conservative tendency sometimes articulated their account of mathematical rigour in deductivist terms.[16] However, Rav's treatment of the Goldbach Conjecture and the Continuum Hypothesis contains a thought that is not found explicitly in Pólya or Lakatos. It is this: *proofs contain important mathematical elements that are not passed on to the theorems they establish, namely, methods*. For example, Rav reports that the assault on the Goldbach Conjecture led to the development of sieve methods. These techniques turned out to have wide application and are a significant piece of mathematical know-how. Another example, this one not given by Rav, would be diagonalisation. This device allows us to prove that the continuum is non-denumerable. Having seen the trick, we can use it again to prove that interesting subsets of the continuum (such as Cantor's dust) are also non-denumerable. Tarski and Gödel later used modified forms of diagonalisation in proofs of some of their most famous results. Rav offers further examples: the usual proof of the infinity of primes assumes that there are finitely many primes, $p_1, p_2, \ldots p_n$. The trick is to form the product of these primes and add one. The resulting number ($p_1p_2\ldots p_n+1$) cannot be composite since it is not divisible by any of the primes $p_1, p_2, \ldots p_n$. On the other hand it cannot be a prime since *ex hypothesi* we have all the primes already. So our original claim to have enumerated all the primes must be false. This familiar technique can be extended (for example to prove that there are infinitely many primes of the form $4n+3$). Another of Rav's examples is Lagrange's theorem that the order of a finite group is divided by the order of any of its subgroups. To prove this result we have to consider the cosets of the subgroup. Cosets do not appear in the statement of the theorem. Yet the coset-technique is perhaps as important as the theorem itself, since it turns out to be a powerful research tool.

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is 'undecidable' with respect to the axioms of neutral geometry. I am grateful to an anonymous referee on this point.

[16] E.g. John Franks, "Mathematics has a methodology unique among all the sciences. It is the only discipline in which deductive logic is the sole arbiter of truth." (Review of James Gleick's *Chaos* in the *Mathematical Intelligencer* 11-1, 1989 p. 68—quoted in Brown [1999] p. 182). For a collection of sober contributions to the 'death of proof' debate, see *Synthese* 111/2 (1997).

"Proofs" Rav concludes, "contain significant topic-specific information going beyond the statement incorporated into the formulation of the theorem. Or to speak metaphorically, *theorems are the headlines, proofs are the inside story*" (p. 22). Notice that Rav's methods are quite different from Lakatos' methods. Constructing cosets is a typical Rav-type method, while Lakatos-type methods are relatively topic-neutral patterns such as lemma-incorporation or monster-adjustment. Recall our earlier observation that demarcationist criteria must apply to any inquiry or argument whatsoever. The patterns found in *Proofs and Refutations* are logical—or if we prefer, 'methodological' in the philosophy of science sense. One can imagine subjecting a legal argument to proof-analysis and incorporating its lemmas as conditions. Indeed, a cynic might think that practical jurisprudence consists almost entirely of monster-adjustment to bring hard cases within the meaning of the act. Be that as it may, the point is that we can intelligibly harbour such thoughts because Lakatos' patterns are not specific to mathematics. It is not self-evidently absurd to go looking for them in non-mathematical contexts such as the law. Methods in Rav's sense, by contrast, are distinctively mathematical. Diagonalisation, sieve methods and coset-construction are unlikely to find direct employment outside mathematics. It is precisely because these methods are properly mathematical that the development of a new one counts as a significant mathematical achievement in its own right.[17]

Before we count Rav as a card-carrying dialectical philosopher of mathematics, we should note a limitation to the range of his philosophical data. In the article cited here at least, he restricts himself to published proofs, that is, to proofs in their final, polished forms. This allows him to rove freely across widely diverse fields of mathematics because he only needs to consult journals. He need not spend months poring over private correspondence or negotiating access to this or that literary estate. His conclusions gain great stability from this broad evidential base. However, this restriction exposes him to certain dangers. The first is the cult of genius. Rav notes (correctly) that the employment of research methods such as those already mentioned could never be derived by topic-neutral logic from the conditions of the theorem to be proved. Their development and use is, he says, therefore *creative*. This term is unobjectionable provided that it is not offered as an explanation for the emergence of the technique in question. We expect even the most novel technique to have some sort of rationale. For the dialectical tradition in philosophy, an appeal to individual creative genius does not explain an intellectual event any more than talk of magic can explain a material one. We are not content to chronicle the development of mathematics; rather we hope to understand it. Consequently we will wish to dig behind the proof in search of the intimations, prototypes, false starts and significant clues that led up to the advent of some given mathematical technique. This material is not normally included in the final published version, in spite of Lakatos' proposal that mathematicians adopt his 'heuristic style' of presentation. I do not say that Rav himself has fallen into the cult of genius, only that in restricting oneself to published documents, one exposes oneself to it.[18] As a practising mathematician Rav must see behind

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[17] David Corfield records Atiyah, Mac Lane and Thom on the neglected importance of mathematical 'tricks' (Corfield [1998] pp. 295-296). For Thom, the "major achievements of mathematics are due to artfulness". Mac Lane offers the explanation that these 'tricks' are really ideas in disguise. We should therefore not be surprised if some tricks grow up to be theories. For example, considering permutations of roots began as a proof-strategy; the Galois correspondence is a theorem; finally we have a body of knowledge called 'Galois theory'.

[18] Romantics may complain that there must be a place for genius in our picture of mathematical practice. It depends on what the question is. Newton's genius cannot explain why he produced a version of the calculus, since Newton born into ancient Egypt or Mesopotamia would have applied his talents to the mathematics he found already in existence. On the other hand, if we wish to know why it was Newton rather than Barrow or Wallis who developed the calculus, then personal talent may be part of the answer.

the scenes and is hardly likely to forget that the production of a proof is usually a long-drawn-out process.

The second danger in looking at published documents only is that we may overlook what (following Polanyi) we may call the 'tacit dimension' in mathematical research. All human activity involves some sort of inarticulate know-how. At the most basic level, we are able to control fine movements in our limbs without being able to explain how. Those mechanics who work on the same sort of engine for a long time develop a hands-on feel that cannot be replicated in manuals. In empirical science researchers accumulate experience which guides them when explicitly-stated methods let them down. Many experiments produce unexpected results, but few of these indicate novel phenomena; most of them are just malfunctions. An experimenter needs a nose for relevance if every stray datum is not to produce a wild goose chase. In mathematics there is not normally anything physical to manipulate, but mathematicians do develop inarticulate intuitions which guide their judgments both in steering their own research and in evaluating that of others. The professional judgment-call has hardly ever been studied in philosophy of mathematics. We cannot hope to repair that lacuna if we restrict ourselves to publicly-available sources. Here again, Rav himself has the advantage of being a working mathematician—the danger is more acute for philosophers hoping to follow his lead.

Mary Leng: Participant-Observer Studies

These anxieties (associated with the study of published documents only) lead to a piece of work by Mary Leng of the University of Toronto. She set out to see whether the picture of mathematical progress given in *Proofs and Refutations* has any application in contemporary pure mathematics. To this end she attended a seminar series given by Professor George Elliott on "The Structure of C*-algebras" at the Toronto Fields Institute for Research in the Mathematical Sciences in the Spring term of 1998. Elliott was trying to develop a classification theorem for some suitably well-behaved class of these objects and the graduate students enjoyed a weekly update on his efforts. Detailed consideration of Leng's results would require detailed consideration of C*-algebras. Her approach is in a way the converse of Rav's. Where Rav ranges across a variety of fields, Leng looks long and hard at one phase in the development of one sub-field. Where Rav's argument stands on a broad base, Leng's case depends for its stability on deep foundations sunk into a narrow area. However, her (unpublished) paper does permit some observations of a general sort that do not require mastery of research-level algebra.

Leng identifies several strategies in Elliott's seminar, and we can sort them using our distinction between Lakatos-type methods (henceforth 'L-methods') and Rav-type methods (henceforth 'R-methods). The principal L-method is a variety of lemma-incorporation. Elliott hopes to prove a classification theorem for a subset of C*-algebras. He has reason to believe that a theorem is available based on an analogy with Von Neumann algebras and an earlier result due to Mortensen, which he hopes to generalise. However, he finds that he needs certain lemmas in order to proceed. He could stop the main inquiry for as long as it takes to prove these lemmas—but that could be a long time. Moreover, it may turn out that the final theorem does not need a certain lemma. Then time spent proving that lemma would have been wasted (unless it revealed some independent interest). Instead, the lemma is temporarily added to the conditions of the theorem. Thus rigour is satisfied and the main inquiry is not delayed. Once the big theorem has assumed its final form, the lemmas are re-examined to see if they really are required and if so how they might be proved. In *Proofs and Refutations* lemma-incorporation is prompted by actual counter-examples. Here, temporary lemma-incorporation is practised simply because mathematical rigour does not permit a proof to include significant unproven lemmas.[19]

Like all the patterns in *Proofs and Refutations*, this L-method can lead to degeneration if pursued mindlessly. If every difficulty is turned into an extra condition then the resulting theorem will be trivial. What is more, the method can only succeed if we have some independent reason to think that there is a significant theorem to be had. We need something to persuade us that the temporarily incorporated lemmas can be washed out once the main theorem has taken shape. It might be disastrous if some lemmas turned out not to be true of the domain we originally hoped to describe. These last remarks are instances of a more general truth: *methods cannot govern themselves*. They need to be guided by the partly-articulate feeling for the subject-matter enjoyed by the experienced researcher. This is true of both R- and L-methods. Diagonalisation and coset-counting appear in many different proofs, but they must be deployed with skill and understanding. Temporary lemma-incorporation keeps the inquiry on track and prevents the master-proof from becoming unmanageably large. There is no hope of specifying formally what 'on track' and 'manageable' might mean. We must rely on something like Polanyi's tacit knowledge.

In the case of C*-algebras there is a background history to inform Elliott's judgment. The first attempt at a definition of a C*-algebra appeared in a 1943 paper by Soviet mathematicians Gelfand and Neumark.[20] This definition took the form of six axioms, but they were not chosen arbitrarily. The aim was to produce an abstract description of an already well-established field of mathematical objects. The collection of all norm-closed self-adjoint algebras of bounded operators on complex Hilbert spaces was already familiar to mathematicians and in that sense relatively concrete (encouraging note to philosophers: nothing hangs on the mathematical detail here). The theory of C*-algebras began as an attempt to abstract the algebraically interesting features of this collection.[21] This history is the context in which tacit judgments are made. Such judgments are not blind guesses, nor are they philosophically intractable. We are not obliged to hand them over to anthropologists of science for 'naturalistic' treatment. Participant-observer studies such as Leng's allow us to get some philosophical grip on the 'tacit dimension'.

There is also, in Leng's paper, a good example of an R-method (that is to say, a specifically mathematical technique). Each C*-algebra has a family of structures associated with it. In particular, each algebra has its own lattice of closed, two-sided ideals (here again, nothing hangs on knowing what a closed, two-sided ideal is). There are mappings defined between these lattices, and sometimes these mappings turn out to be isomorphisms. Elliott's central thought (as reported by Leng) is as follows: for a special class of cases, algebras with isomorphic lattices are themselves isomorphic. In other words, we can discover relationships between algebras by looking at relationships between the structures associated with them. For a homely illustration, it is like showing that two houses with exactly the same wiring diagram must be the same buildings 'up to isomorphism' i.e. they were built from the same architectural plans. Clearly this thought can be used elsewhere in mathematics. It is, however, not topic-neutral. It can only find application where we have the possibility of isomorphisms between objects and between their associated structures. It is therefore an R-method.

Leng concludes, reasonably, that there is something Lakatosian going on in the development of C*-algebras, but only if we stretch our sense of 'Lakatosian'. There is lemma-incorporation, but it is not prompted by counterexamples. The story does not seem very

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[19] For fallibilists and social-constructivists the significant word in that sentence is, of course, 'significant'.
[20] Here I rely wholly on Leng's exposition.
[21] These six axioms included some redundancy which was eventually (over seventeen years) weeded out—another case of temporary lemma incorporation.

‘quasi-empirical’. She notes that the story could also be told using the terms of Pickering’s ‘dialectic of resistance and accommodation’. Here again we have an instance of a greater truth: general, topic-neutral methods have to be stretched in order to fit the details of specific cases. With repeated stretching they lose their characteristic shapes, and it becomes increasingly difficult to find a reason to prefer one to another. Having answered her original question about Lakatos and advanced algebra, Leng should (in my view at least) set aside the question ‘which *general* methodology best describes mathematical practice?’ because none of them has more than a little insight to offer.

David Corfield: Ends and Means

Leng’s work takes us inside the mathematical factory. By itself, however, it cannot serve as an exemplar for a developing discipline. It focuses so tightly on one context that we should need armies of philosophical infiltrators to get any sort of overview. Moreover, having settled the Lakatos question, we need a fresh research strategy. Otherwise we run the risk of falling into a merely descriptive exercise. This brings me to my third and final press-ganged recruit to the dialectical banner.

Like Mary Leng, David Corfield began with a question about the applicability of Lakatos’ vision of mathematics to the twentieth century.[22] In his view, Lakatos failed to see that translation into a relatively formal language does not mean death for mathematics. Concepts may still be stretched and dialectical patterns found even in a mature field where spatial and empirical intuitions have given way to explicit definitions. He has in mind such parts of mathematics as group-theory and algebraic topology. In spite of their technical development compared with (say) school geometry, these areas are not formalised in the sense required by mathematical logic, where the logical language and inferential rules are explicitly specified in advance of special axioms describing the subject matter in view. Thus Lakatos’ polemics against ‘static rationality’ and ‘formalism’ (the identification of mathematics with its metamathematical shadow) apply here too. Corfield speculates that Lakatos’ failure to envisage dialectics in twentieth-century mathematics is part of his Hegelianism[23]. He may have a point, since Hegelian narratives usually end with a sort of stasis. If the final pages of the *Phenomenology of Spirit* are to be taken seriously it seems that Spirit has been hanging about for almost two centuries with its hands in its pockets.[24] A more immediate explanation for Lakatos’ blindness to the dynamism of highly technical mathematics may lie in our distinction between topic-neutral L-methods and topic-specific R-methods. It is plausible that topic-neutral methods progressively give way to topic-specific methods as a discipline matures. In contemporary mathematics the topic-neutral methods have almost vanished. There will of course always remain the entailment relations resulting from the application of R-methods. If this conjecture is true then Lakatos failed to see any dynamism in the most advanced mathematics because the sorts of pattern he was looking for cannot easily be found there. It also follows that we should concentrate our philosophical attention on relatively topic-specific

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[22]See the references at the end of this article for a list of Corfield’s published papers on Lakatos.

[23] In ‘Argumentation and the Mathematical Process’.

[24] Hegel also claims that static Utopias such as Plato’s *Republic* are impractical because Spirit is restless (in *The Philosophy of History*—Egypt). Presumably at the end of History Spirit is driven by this restlessness to produce endless variations of itself without advancing in any way. Perhaps convinced Hegelians should worry about what mischief Spirit will fall into when it gets bored. On the other hand, Hegel’s narratives usually end at what was the present at the time of writing. Hegelians can argue that the whiggish conclusion to a typical Hegelian narrative seems like the definitive last word because at the time of writing, it is. No such argument is available to Lakatos. The story in *Proofs and Refutations* stops arbitrarily at Poincaré, and with little of Hegel’s *crescendo*. For more on the relation between Lakatos and Hegel see Larvor ‘Lakatos’ Mathematical Hegelianism’ (*Owl of Minerva* Vol. 31 no. 1, Fall 1999).

techniques. Indeed we should adopt this strategy simply in order to draw out what is characteristically mathematical in mathematics.

Whatever the proper diagnosis for Lakatos' blindness to the liveliness of advanced mathematics, Corfield is now forging a research strategy of his own. It starts from the identification of mathematical ends. Of course, the most general aim is the production of significant mathematical knowledge, but this formula is hardly informative. Corfield hopes to study the 'thick'[25] evaluative notions of mathematical culture. When mathematicians praise each other's work they use terms like 'beautiful', 'elegant', 'serious' and 'deep'. About the highest praise is to say that a proof advances our *understanding* of some phenomenon. This honorific is not handed out to just any valid proof—some proofs are valid but not insightful. Corfield's project is *not* to try to isolate the essence of mathematical depth or beauty. That would be hopeless because beautiful theorems are, no doubt, a motley collection with only a family resemblance to each other. Rather the hope is to study the means by which these ends are achieved. These means are what I have called topic-specific R-methods: mathematical techniques that can be applied more than once but are not so wide in their application that they become part of general logic. Corfield also hopes to study the evaluative discourse of mathematicians. When there is a dispute about the significance of a mathematical development, what considerations are advanced? Which are decisive? This part of the project suffers from the twin difficulties that first, mathematicians tend not to articulate their values with much precision and second, the evaluative documents (such as reports by referees and examiners) tend to be confidential. These difficulties are not insuperable, and the growth of popular mathematical writing may offer an indirect solution. We have already seen some journalistic interest in the notion of proof, and with it some evidence that mathematicians are more readily spurred into print if they feel that the public reputation of their discipline is at stake.

These few lines cannot convey the full richness of Corfield's programme. Indeed, he envisages the rise of a new discipline—a mathematical counterpart to the history and philosophy of science, or 'science studies'. This is to include psychology, anthropology and sociology as well as history and philosophy. Any study that might illuminate the process of mathematics is to be made welcome. The first order of business is to redress the philosophical neglect of twentieth century mathematics (aside from set theory and formal logic). Corfield also envisages rich engagement with mathematical educationalists (who have to date found little of any use in the philosophy of mathematics). It remains to be seen whether this level of inter-disciplinarity is sustainable. The 'science studies' movement has produced mixed and muddled results in part because the distinctions between contributing disciplines have not always been properly sustained. History and sociology, for example, produce quite different sorts of explanation. Lakatos' dynastic ambitions suggest a dialectical phenomenology of the sort outlined earlier that takes data from other disciplines but preserves its own distinctive point of view. Moreover, the level of mathematical knowledge required means that there can never be very many researchers in this field. Remember that our scholars must be both mathematically literate and simultaneously competent professionals in their chosen meta-discipline (philosophy, psychology, etc.). However, the whole enterprise is at such an embryonic stage that one cannot afford to carp. It is time, rather, to consider the practicalities of institutional support, access to materials and the dissemination of ideas among a coherent body of scholars.

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[25] Vocabulary borrowed from Bernard Williams.

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