A REPLY TO DAVID BLOOR

JOHN WORRALL*

In his *Proofs and refutations*,[1] Imre Lakatos portrays and evaluates various responses which mathematicians have made to alleged counterexamples to their theorems and proofs. The central suggestion of David Bloor's paper in the last issue of this *Journal*[2] is that a deeper understanding of this aspect of mathematical development can be achieved by showing that an individual mathematician's response to such counterexamples is characteristic of his social circumstances. Bloor also develops and endorses a particular interpretation of Lakatos's methodology of mathematics which makes it amenable to his sociological treatment. Finally, Bloor criticizes the editors of Lakatos's book (Elie Zahar and me) for having added various editorial footnotes at variance with what he regards as Lakatos's central message.

I think most of the disagreements between Bloor and myself stem from one substantive point about the nature of mathematics. I shall first try to explain this point, and then turn to Bloor's attempted explanation of the development of mathematics.

Suppose, to take Lakatos's favourite example, that a mathematician is considering the Descartes-Euler conjecture that, for all polyhedra, $V-E+F = 2$ (where V is the number of the polyhedron's vertices, E the number of its edges, and F the number of its faces). The mathematician is also confronted with a twin tetrahedron (two ordinary tetrahedra joined at one vertex) for which $V-E+F = 3$. Should this object be regarded as a real polyhedron and hence as a refutation of the conjecture? Would accepting the twin tetrahedron as a polyhedron involve 'stretching' the concept of 'polyhedron' and, if so, is that stretching licit or illicit? According to Bloor, Lakatos's central message is that any mathematical concept is always 'stretchable'. The boundary between polyhedra and non-polyhedra is not, Bloor makes Lakatos say, an immutable feature of some Platonic heaven but is, rather, man-made and hence always man-revisable. This means that, no matter how well-established a theorem about polyhedra may be, it is never safe from the threat of counterexamples. Even if we can show that there are no counterexamples to the theorem amongst what we now call polyhedra there can be no guarantee that we shall not at some future time change our notion of a polyhedron and hence create the possibility of further counterexamples. (It is, of course, important for Bloor's sociological conception of mathematics that the boundaries of mathematical kinds depend on our collective decisions, and may change as the social framework changes.)

Whether or not Bloor's interpretation of Lakatos is correct, he is obviously right that any theorem (provided it is not a logical truth) *can be* refuted by concept-stretching (though most concept-stretching refutations would be

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Despite the great haste in which this paper was composed, Alex Bellamy, Mike Hallett, Peter Urbach, John Watkins, and Elie Zahar all managed hurriedly to read an earlier draft. I am grateful to all of them for their helpful comments, and especially to Peter Urbach (whose criticisms led to substantial changes in the presentation and content of my arguments) and to Elie Zahar (as usual with him I find it difficult to decide whether he simply helped me clarify what I already knew, or whether he taught me it).

I should also like to thank the Editor of this *Journal* for inviting me to write this reply.

regarded by mathematicians as trivial and uninteresting). The only point that Zahar and I wanted to make in our editorial footnotes was this. Mathematicians do not only produce conjectures; they also produce *proofs* of them. In informal mathematics, counterexamples may hit the proof as well as the theorem: if the entity concerned satisfies the initial assumptions from which the proof proceeds but does not satisfy the alleged theorem, then the proof is shown to be inadequate as well as the theorem. Such counter-examples force the articulation of hitherto implicit assumptions. Some of these are incorporated as conditions on the theorem (for example, instead of claiming that for *all* polyhedra $V-E+F = 2$, we claim only that any polyhedron satisfying conditions C_1 to C_n has $V-E+F = 2$). Other hitherto implicit assumptions may become explicit axioms. Through this process, the proof becomes more and more rigorous and 'concept-stretching' less and less relevant *to the proof*. Eventually some modified form of the original informal theorem will be derivable within first-order logic from some explicit set of axioms. One of the properties which first-order rules of proof can be shown to have is 'soundness'. This means that any statement which can be derived in this way from the axioms of the system is, in fact, a logical consequence of those axioms. And this means that *no matter how we interpret or 'stretch' the descriptive terms* occurring in the theorem, no counterexamples to the theorem can occur within the system, since *every* interpretation which makes the axioms true will also make the theorem true. And this in turn means, of course, that no weight is carried by the descriptive terms and hence that 'concept-stretching' is irrelevant here.

At the beginning of the story of the Descartes-Euler conjecture, nearly everything revolves around the question of what we mean by polyhedron, around whether acknowledging some object as a polyhedron would constitute licit or illicit 'concept-stretching'. At the end of the story, at least as far as the proof is concerned, it simply does not matter at all what we mean by polyhedron. About Cauchy's early nineteenth-century proof of the Descartes-Euler conjecture,[3] two sorts of questions can be raised: are the basic assumptions from which he begins the proof correct, *and* are his inferential steps correct, is his proof valid? Very sophisticated and interesting considerations can be brought to bear on both questions. Poincaré's late nineteenth-century proof of (a very much modified version of) the same conjecture could, with a little effort, be represented in the usual first-order formalization of Zermelo-Fraenkel set theory. In this system, the permitted inferential steps may be specified in advance: they are those performed according to the two accepted rules of proof, *modus ponens* and generalization. Because *this* proof uses only finitely many rules which are specified in advance, whether or not it is indeed a proof can be checked, for example, by a machine. There is thus no serious sense in which such a proof is fallible.[4] And, because these rules of proof are 'sound', no counterexample to the proof can be found, no matter how the descriptive terms are stretched; that is, we can make the descriptive terms mean what we like and still never find an interpretation in which the axioms are true and the theorem false.

Of course, there is no guarantee that the rigorously proved version of the Descartes-Euler conjecture will be the last word on the subject. 'Polyhedron' will appear within our set theory as a defined term. Presumably it will cover all the cases which are intuitively regarded as clear cases of polyhedra (e.g. cubes), exclude intuitively clear cases of non-polyhedra (e.g. spheres), and will also decide the cases which intuition did not decide. However, someone may argue that our explicit definition in fact fails to satisfy these requirements, and in the ensuing debate our intuitions about the concept of polyhedron will certainly play a role. Lakatos calls this question of when formal definitions adequately capture our informal notions the 'problem of translation'. He subjects the problem to a brief but extremely clear discussion.[5] (Bloor criticizes Zahar and me for 'making nothing of' this problem, but the reason for this is simple: we had nothing to add to Lakatos's treatment of it.) Interesting questions may also be raised about the *axioms* of our set theory—are these acceptable, would we be better served by different axioms, by what criteria should we appraise the acceptability of axiom systems? Many such questions may be prompted by considerations of how successful our formal theory is in capturing informal mathematics.[6] I agree with Bloor that these are important questions which should be highlighted. But the glare seems to have blinded him to the important fact that *some* progress has been made: one question has been settled—that of proofs. We now *do* have systems within which absolute rigour is possible.[7] This important victory is, of course, in no way diminished by the fact that mathematicians will not standardly present their proofs in such a cast iron, rigorous form. The point is that present day proofs *could be* formalized in this way if desired.

Hence Bloor's point that 'concept-stretching' is always possible, and that no theorem is ever absolutely safe, needs to be importantly modified. In informal mathematics, 'concept-stretching' may refute a theorem without taking us outside of the system within which the theorem was informally proved. In formal mathematics, theorems can be modified only by modifying the system—by changing either explicit definitions or axioms. Because we can now give fully rigorous proofs no counterexamples to theorems can be produced within the system.

Both Bloor and Lakatos (in his original 1963–4 paper) seem to make the same mistake. They both tend to conflate two distinct questions: are our theorems about polyhedra now rigorously proved, *and* can our theorems about polyhedra never be replaced? The editorial footnotes in Lakatos's book were designed to make this point, and to argue (surely uncontroversially) that an affirmative answer to the first of these questions does *not* entail a similar answer to the second. It entails merely that a modification of such theorems can be brought about only by switching to a new system. I had thought we had made this point clearly enough in *Proofs and refutations*, but I am glad to have had the opportunity here to elaborate it at greater length.

I should perhaps add that, while at certain points of his paper (see particularly the first paragraph of p. 268) Bloor is definitely in disagreement with us on this point, there are other passages in which he seems to agree with us. But unless there *is* this basic disagreement I am baffled as to why Bloor thinks (as he obviously does) that we have different views on concept-stretching: on the question of whether concept-stretching can refute any theorem (provided we are allowed to step outside the system within which the theorem was proved) there is complete unanimity.

One issue on which Bloor and I certainly hold different views is that of editorial ethics. Bloor finds the practice of adding editorial footnotes like ours a 'striking oddity' (although I suspect that a survey of collected editions of philosophers' works would dispel this odd impression). Bloor conjectures that we added footnotes whenever we feared that Lakatos was becoming too radical. He further conjectures that this reflects a rather less enviable ('higher group') social position than that occupied by Lakatos.[8] No doubt

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this is all objectively correct. All I can report is our subjective impression that we added these footnotes whenever we felt that Lakatos's 1963–4 text was wrong, and especially when we conjectured that the 1970s Lakatos (with whom we enjoyed a sustained and intimate intellectual collaboration) would have found his earlier text incorrect. (He certainly realized that his old paper had many faults; this was why he delayed for so long the publication of his book.) As for Bloor's suggestion[9] that Zahar and I were afraid of the effect Lakatos's ideas might have in spawning sociologically-inclined papers like his own, I am more than happy to see Bloor's paper judged on its own merits. Also, as editor of Lakatos's book, I was, of course, quite happy to leave Lakatos's old ideas for everyone to see and to assess, both in themselves and in the light (if any) of the editorial comments. It is tempting to conjecture that it is really Bloor who fears a proliferation of competing views. This fear may perhaps be explicable by Bloor's 'high group' social position. (But can it be excused thereby?)

There are two points in Bloor's attempt to save Lakatos from his editors which deserve attention. The first is his correct remark (actually re-iterating an editorial footnote of ours[10]) that what is said about the eventual irrelevance of 'concept-stretching' depends on a distinction between descriptive and logical terms. Counterexamples within the system are still possible, even to correctly derived theorems, if we allow the 'stretching' of 'all' 'and', 'or', *etc.* But two counter remarks should be made. First, these logical terms crop up in all mathematical fields (*and* indeed in *all* intellectual discourse). All the terms which are specific to the study of polyhedra—polyhedron itself, face, vertex *etc*—may be stretched at will. Second, if all logical terms could be stretched, logic would be entirely destroyed and it is then difficult to see how we could make sense of any discussion. Any discussion, no matter how informal, rests on a core of logic. The fact that no sensible discussion can take place unless some terms are implicitly regarded as logical, seems a good reason for regarding some terms as logical.[11]

The second point which Bloor makes is that the editorial interventions reduce Lakatos's methodology to the very same philosophy—formalism—that it was intended to outflank. According to Bloor, Zahar and I want to reduce mathematics to a 'contentless web of inferences'.[12] This seems to be another manifestation of Bloor's basic mistake: his conflation of absoluteness of proof and absoluteness of theorem. Nothing in the editorial footnotes detracts from Lakatos's emphasis on the *content* of mathematics. The point is simply that the increasing rigour of proofs has taken all the content (all of the implicit assumptions) out of the proofs and firmly located it in the explicitly articulated definitions and axioms, on which the mathematical and epistemological debate can now concentrate. We certainly want contentless inferences, but not contentless mathematics.[13]

By ignoring the merits of our rigorous proof procedures, the naturalness of our logic, Bloor is in danger of failing to see perhaps the biggest obstacle to his sociological view of mathematics. Even if he could show that the boundaries of mathematical kinds depend on sociological factors, he would still face the problem of proof, of logic, which, at least on the surface, yields much less easily to the relativizing approach he advocates. Contradictions are pretty well universally regarded as unacceptable. Can this really be just a reflection of our social (and perhaps physiological) circumstances? Is not the argument that valid inferences must be accepted because they necessarily transmit truth somehow objectively compelling? Whatever the answer to these questions, Bloor is unlikely to win many converts by simply turning a blind eye to them.

I turn now to Bloor's attempt to provide a sociological explanation of the various responses of mathematicians to counterexamples. Remember our puzzled mathematician confronted, on the one hand, with the Descartes-Euler conjecture, and, on the other, with a twin tetrahedron. One response he might make is to dub the twin tetrahedron a 'monster', not a genuine polyhedron, and hence not a genuine counterexample to the conjecture. Lakatos names this response 'monster-barring'. Another response ('monster-adjustment') would be to claim that seen aright (as two *separate* Eulerian polyhedra one on top of the other), the twin tetrahedron is no threat to the conjecture at all. Lakatos commends (in general) another, and more subtle, approach to counterexamples which he names 'the method of proofs and refutations'. He commends this approach because it is the one which is most likely to lead to mathematical progress. This is as far as Lakatos goes. He certainly makes no attempt to *explain* why some mathematicians were 'monster-barrers', others 'proofs and refutationists', *etc.* Neither did Elie Zahar and I produce any ideas in this direction. I was then most impressed by Bloor's bold acceptance of this explanatory challenge, especially in view of his explicit assertion right at the start that his explanation was to be 'speculative but *testable*'.[14] It is of course precisely the promise to make methodology testable which constitutes the main appeal of the epistemological naturalism (or 'scientism') championed by Bloor. Bloor's opponents will insist that the development of science and mathematics can never be explained purely in terms of natural and social factors; that we can, and must, make sense of the objective merits of scientific and mathematical theories; and that evaluations of these merits (evaluations not predetermined by natural or social factors) play an irreducible role in the development of science and mathematics. But, while his opponents squabble about what constitute 'objective' rights and wrongs, Bloor is promising to produce accounts of the growth of knowledge which can be simply checked against the facts.

And indeed, early in his account Bloor does produce a bold and apparently testable theory. Mathematicians' responses to proposed counterexamples are to be straightforwardly 'characteristic of different social structures'.[15] The important differences between social structures are to be categorized using the 'group' and 'grid' characteristics introduced by Mary Douglas in her study of primitive societies.[16] Very roughly, societies evince high group if strong barriers separate the members of the society from strangers; and a society is a high grid society if it exhibits many gradations of rank.

Bloor associates the 'monster-barring' response, for example, with 'high group, low grid' (rather monolithic and pollution-conscious) societies, and he associates the 'proofs and refutations' response with (more liberal) 'low group, low grid' societies. He tries to motivate his theory by arguing that these associations are natural—what one would expect. I must confess that I found these arguments uniformly unconvincing and the whole idea rather implausible.[17] But remembering that the most implausible-sounding theories have, on occasion, met with great success in empirical practice, I looked for tests of Bloor's theory.

The theory that mathematicians' responses to proposed counterexamples are determined by the grid, group characteristics of their social surroundings certainly appears highly refutable. Assuming that we can independently decide on a given society's grid, group characteristics, then we can predict, *via* the theory, that a mathematician belonging to that society will have been a 'monster-barrer' or whatever; and, conversely, given his type of response to counterexamples 'we should be able to predict [his] social circumstances'.[18] Unfortunately, this theory also appears highly refuted. A glance at the history of mathematics indeed reveals, if anything, a surprising lack of uniformity of intellectual style amongst similarly situated great mathematicians. This was noted, for example, by Poincaré, in the case of Hermite and Bertrand:

> They were scholars of the same school at the same time; they had the same education, they were under the same influences; and yet what a difference [in intellectual approach]![19]

Fortunately, Bloor has nothing to fear from counterexamples such as this. Despite initial impressions, it is not this testable theory that he holds, nor even a (no doubt more plausible but weaker) probabilistic version of it ('there is a high probability that a mathematician with social circumstances x will be a monster-barrer', etc.). Bloor admits that various considerations 'could destroy any neat correlation between isolated individual utterances and social locations'.[20] And he even admits on the last page of his main text that social factors are not the whole story concerning knowledge, since 'our psychological and physiological make up can never be ignored',[21] although, disappointingly, he never tells us precisely how these extra factors work. He does, however, assure us that the empirical character of his theory is not lost by making these concessions:

> . . . even if the individualistic predictions of the theory turn out to be wrong . . . It would be useful to know if there is any systematic variation in the extent to which individual beliefs cluster round the predicted characteristic style. Does it vary across the grid-group diagram, or change in response to other identifiable circumstances? . . . Or it may be possible to isolate the features of those individuals who do, and those who do not, conform to the predictions of the theory . . .[22]

Bloor's assurances will not carry much weight with my Popperian friends who will already be shouting 'untestable pseudoscience!' Certainly Bloor seems to have created for himself the classic game of 'heads I win, tails I win too'. If a prediction of the naive theory turns out to be empirically correct, this is, of course, a success; and it turns out to be false, this may be a success too, for we can now interpret it 'in terms of' the theory, compare it with other failed predictions, etc. Even if we apply less harsh standards and accept that programmatic suggestions—untestable as they stand—may be of scientific interest, we must surely require that they show this by producing refutable and partially confirmed variants. Bloor seems to start with a refutable but refuted theory and then retreats to an irrefutable programme. Where is the defensible 'speculative but testable' theory which Bloor promised?

The answer seems to be, I am afraid, 'rather a long way off'. Bloor's description of his programme inspires no great confidence in its ability to produce genuinely testable theories. First, an independent decision on the grid, group characteristics of a mathematician's society seems in fact very difficult to make. Mary Douglas's approach may apply unambiguously enough to more primitive societies separated by more or less obvious and rigid boundaries across which little intercourse occurs. But its application to more complex societies seems less straightforward. Suppose I want to place mathematician x on the 'grid, group diagram' and suppose that he is in fact a Catholic from Northern Ireland. Which society constitutes his 'social circumstances'? Is he Northern Irish Catholic (presumably high group, lowish grid)? Or is he Northern Irish, or British, or European (lower group)? One suspects that all one can do is try out various possibilities and see which one gives the right result. Moreover our mathematician (like, I should guess, pretty well every major mathematician in the modern era) is in contact with other mathematicians all over the world (if not directly then through their published works); does not this universal 'society of mathematicians' confute the whole approach? Bloor indeed admits that *any* 'individualistic' version of his theory is likely to prove unacceptable. On the genuinely sociological approach

> [a] direct inference to the structure of public knowledge from [mathematicians'] isolated individual beliefs would be a mistake, and vice-versa. Individual evidence is always to be treated by putting it in a context where its typicality and its contribution to the overall pattern can be assessed.[23]

This certainly guarantees plenty of work on Bloor's programme, interpreting the 'data' in terms of the programme. It also seems to be a guarantee against testability.[24]

Scientists, realizing that programmatic suggestions are ten-a-penny, do not usually publish their programmes before they have produced testable (and at least partially successful) variants.

Finally, Bloor seems to me to make several significant philosophical and mathematical errors. Whilst not central to our disagreements, some of these should perhaps be pointed out, lest anyone be misled. He misinterprets Platonism.[25] This doctrine is, of course, quite compatible with *our conceptions* of the boundaries between mathematical kinds being fuzzy and changing over time and hence is not inconsistent with anything Lakatos says. Bloor states, incorrectly, that a proof of a conjecture makes the conjecture itself more vulnerable.[26] 'Local counterexamples' are not counterexamples to the conjecture/theorem at all. And his account of Cauchy's proof[27] makes it one big *non sequitur*. To show that an admitted truth (that $V-E+F = 1$, for an ordinary triangle) follows from the theorem concerned, and then to conclude that the theorem must be true, is to commit the fallacy of affirming the consequent.

NOTES

[1] Imre Lakatos, *Proofs and refutations: the logic of mathematical discovery* (edited by John Worrall and Elie Zahar), Cambridge, 1976.

[2] David Bloor, 'Polyhedra and the abominations of Leviticus', *The British journal for the history of science*, 1978, *11*, 245–72.

[3] A. L. Cauchy, 'Recherches sur les polyèdres', *Journal de l'école polytechnique*, 1813, *9*, 68–86.

[4] Of course we may make slips in proof-checking, or our proof-checking machine may break down. But this sort of practical fallibility is not the issue. All the philosophers (Descartes, Hume, Kant, etc.) who argued that arithmetic, for example, is apodeictic, admitted of course that we may make occasional slips in computation.

[5] Lakatos, op. cit. (1), pp. 121–3.

[6] On this problem see especially Lakatos's 'A renaissance of empiricism in the recent

philosophy of mathematics?', now reprinted in his *Mathematics, science and epistemology; Philosophical papers*, Vol. ii, Cambridge, 1978.

7 Poincaré was right that absolute rigour *of proof* has now been attained. Lakatos's pupils giggled at this remark (see Bloor, op. cit. (2), p. 269) because they misinterpreted it as meaning that mathematics (i.e. the set of mathematical theorems) is now absolutely fixed.

8 Bloor, op. cit. (2), p. 270. For the 'group' concept see below p. 75.

9 Ibid., p. 269.

10 Lakatos, op. cit. (1), pp. 125–6.

11 Moreover, Bloor's correct observation that 'no principle of an absolute kind' (op. cit. (2), p. 268) separates logical and descriptive terms should not be taken to entail that we have no good reasons for regarding our logical terms as 'truly' logical; (see, e.g., W. V. Quine, *Philosophy of logic*, Englewood Cliffs, 1970).

12 Bloor, op. cit. (2), p. 268.

13 For further developments of Lakatos's idea of appraising axiom systems see Mike Hallett, 'Towards a methodology of mathematical research programmes', *The British journal for the philosophy of science*, forthcoming.

14 Bloor, op. cit. (2), p. 245; emphasis supplied.

15 Ibid.

16 Mary Douglas, *Natural symbols: explorations in cosmology*, Harmondsworth, 1973.

17 Take, e.g., Bloor's attempt to justify the connexion of monster-barring with pollution-conscious societies. This seems, to my untutored eye, to start from a *non sequitur*. (Bloor infers from the very plausible premise that men will try to legitimate their social conventions by claiming that they are based in nature to the implausible conclusion that this socializes nature—'Nature becomes a code for talking about society' (op. cit. (2), p. 252).) Bloor's argument also seems to depend quite heavily on there being an emotional component to 'monster-barring', that it should involve 'turning in disgust' from alleged counter-examples. But of course this emotional component is by no means essential. (Bloor was perhaps misled by Lakatos's terminology: 'monster' is, of course, a technical term in biology.) Finally it just seems grossly implausible that a genius like Poincaré, who was engaged in critical and immensely fruitful debates on a variety of subjects with a variety of scientists, should have been simply reflecting his social circumstances in responding to polyhedra. (Bloor does not differentiate those cases in which 'monster-barring' would seem justified—say someone proposed a sphere as a refutation of the Descartes-Euler conjecture—from these cases in which it seems definitely obfuscatory. Surely this makes a difference to the sociological explanation.)

18 Bloor, op. cit. (2), p. 245.

19 Henri Poincaré, *The value of science*, 1913, reprinted New York, 1958, p. 16.

20 Bloor, op. cit. (2), p. 261.

21 Ibid., p. 266.

22 Ibid., p. 261.

23 Ibid.

24 I am assuming that Bloor himself would not count his 'explanation' of the (Bavarian) Seidel's achievements in terms of (Prussian) social factors as an explanatory success as it stands.

25 Ibid., pp. 248–9.

26 Ibid., p. 246.

27 Ibid.

DAVID BLOOR WRITES:

I enjoyed John Worrall's polemics and I am happy to meet his arguments. I shall try to ensure that my remarks are as terse as possible.

Lakatos defines formalism as the tendency to identify mathematics with its formal, axiomatic abstraction and to equate the philosophy of mathematics with metamathematics. This is exactly what Worrall wants to do with the notion of proof. In conformity with the second part of Lakatos's definition, Worrall states his desire to see the epistemological debate focusing on axiomatized versions of mathematics. The great interest of *Proofs and refutations* lay precisely in the fact that it reversed this trend. My claim that the editorial interventions went against the whole thrust of the book are therefore amply confirmed by everything that has been said.

Let us look a little more closely at how Worrall achieves the cast-iron rigour of his formal proofs. Note the following, in which I have supplied the emphasis: 'Because we can now give fully rigorous proofs no counterexamples to theorems can be produced *within the system*' and 'a modification of such theorems can be brought about only by *switching to a new system*'. The reader who remembers *Proofs and refutations* will have the feeling that he has seen this strategy before. In Lakatos's book we were shown mathematicians who argued that alleged counterexamples did not really refute the theorem: that stood as perfect as ever; it was just that you had to keep to the right class of polyhedra. Of course you could *appear* to refute it if you switched to examples of a different *sort* of polyhedra, but as long as you did not switch classes, types, or sorts of polyhedra, no counterexamples were possible—and of course the types, classes, etc. were arrived at precisely in order to make the theorem invulnerable. Its invulnerability was a decision. Now this strategy is a quite general expedient that can be used in many different circumstances, and this is what Worrall is doing: he is 'exception barring'. He is like M. Berard telling us that we should not confuse false theorems with theorems subject to some restriction, only we must now read 'proofs' for 'theorems'.

The point that Worrall has not seen is that his editorial footnotes are just moves in the very game that *Proofs and refutations* so brilliantly described. What he says in his footnotes does not *correct* what is in the text, it *exemplifies* it. It was because the editorial interventions showed no awareness of this fact that I feared they may blur the significance of the book. That was the point of taking the editors to task, and Worrall's reply only deepens my anxiety.

Worrall does, however, have a rather remarkable reason for wanting to employ these protective strategies. He thinks that unless logic is shown to be beyond the reach of Lakatos's analysis then it will be 'destroyed'; discourse would collapse into chaos. We might not be able to specify or demarcate pure, formal, contentless logical structure, but it must be there: otherwise we could not 'make sense of any discussions'. This vague claim is obviously heartfelt but it is important to notice that Worrall is totally unable to substantiate it.

In fact there are entirely down-to-earth reasons why our discourse is orderly and conveys meaning. We do not have to try to explain it by taking issue with *Proofs and refutations*. It has nothing to do with the hidden securities of an unchallengeable logical truth. Discourse has its basis in habit, training, convention, and shared psychological dispositions. However it may be 'regarded', logic does not in fact lie behind or underneath these things: it is constructed out of them. It is supported by them, not vice-versa. These issues, which relate to the compelling and objective character of logical inference, are not things to which I turn a 'blind eye', as Worrall charges. I have indicated how they may be treated along naturalistic lines in chapters V, VI, and VII of my *Knowledge and social imagery,* and this is footnoted in the paper.

Now for the question of testability. My hypothesis about mathematical styles is said to be testable (but false) if it refers to individuals, but untestable if it is a sociological thesis. Worrall's refutation of the individualistic version is a masterpiece: evidently a 'glance' at the history of mathematics and an isolated observation by Poincaré will suffice. I would not dare to suggest that Poincaré was unreliable in what he reported, but the point is: what does his report mean in relation to the hypothesis? Was the difference between the mathematicians Bertrand and Hermite, to which Poincaré was referring, of the kind specified by the hypothesis under test? Was it about their responses to anomaly? No: Poincaré was not talking about responses to anomaly at all;

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in the paper in question he was talking about whether they were 'visualizers'. Hence he goes into florid details about how the two gentlemen comported themselves when lecturing; how they gestured; how they opened and shut their eyes; shunned contact with the world, etc. Despite its authoritative source, Worrall's evidence and test are worthless.

Now for Worrall's assertion that the sociological version of the hypothesis is untestable. This, remember, says that systems of knowledge have characteristic responses to anomaly which arise in specified ways from their underlying social structure. The only actual argument that I can find to support the claim that this is untestable concerns the problem of defining the relevant groups to which the style is imputed. This is indeed a genuine issue, and I refer to it in my paper. Its logical and methodological character can best be illustrated by an analogy. It is like the problem of trying to develop atomic theory without knowing the true molecular formulae. The problem is that there are a number of interrelated unknowns. The answer in such cases is always the same, whether in chemistry or sociology: it is necessary to begin by a plausible guess, build up the data and be prepared to adjust the original assumption so as to maximize coherence. Worrall must know enough about the *real* procedure of science to know that this is standard and accepted practice. It rules out point-by-point testing, but it does not detract from the empirical character of a theory when this is more broadly and realistically conceived. Or does Worrall think that most of our actual science is unscientific? I presume not. But then Worrall would do well to keep to the same standards as he moves from the physical to the social sciences, or he will open himself to the charge of arguing from expediency rather than principle.

Such a comment also seems in order when Worrall becomes gripped by a sudden but convenient bout of inductivism. He scoffs at people who propose research programmes ten-a-penny without partially confirming them first. What, I wonder, are Worrall's 'Popperian friends' shouting now? But even if Worrall is not being merely polemical, his remarks miss their target. Research programmes in the sociology of knowledge, let alone the sociology of mathematics, are not ten-a-penny. Douglas's grid, group theory is a rarity in anthropology, just as Lakatos's theory is a rarity in philosophy. If there were a babble of research programmes in the sociology of knowledge then there might be grounds for caution. Until that happy day arrives, I for one shall not feel inhibited in making proposals.

In summary: we have been treated to the spectacle of Worrall, the committed falsificationist, trying to remove as much of mathematics as possible from the clutches of that very doctrine. For my part I prefer the more interesting aims of *Proofs and refutations*, viz. extending falsificationism as far as possible, even into proof procedures. It is the great virtue of that book that it gives us an alternative to formalist and Platonist visions of mathematics. The proper response is surely to welcome and exploit these ideas, ignoring those who would stifle, or sidestep them. Finally, *of course* sociological theories such as those of Douglas are not beyond the reach of investigation or empirical checking. Historians deal quite routinely with social, structural, and stylistic phenomena and thereby show in practice the empirical character of assertions about these matters. I cited examples in my paper, such as Turner's work on the Prussian professoriate. The automatic identification of the 'sociological' with the 'untestable' derives either from muddle or from doctrinaire philosophical hostility. The former condition can be cured by inspecting examples of sociological history; the latter is best ignored.

JOHN WORRALL ADDS:

(1) It is ridiculous to accuse me of seeing no philosophical interest in informal mathematics. One of the many interesting features of informal mathematics is precisely the way in which its proofs are improved by gradual rigorization. This is the process Lakatos illustrates. Lakatos occasionally gives the impression that he sees no limit to this process of improvement. On this matter of detail Zahar and I thought Lakatos was wrong and said so. Nothing in the editorial footnotes or in my original reply to Bloor goes against the 'whole trust' of Lakatos's book (when properly understood).

(2) I am afraid that Bloor has not read Poincaré's remarks carefully enough. For Poincaré the fundamental *intellectual* divide between mathematics is that between 'logicians' (or 'rigorists') and 'intuitionalists' (or 'visualizers'). Hermite and Bertrand stand at opposite extremes of this divide, despite having had very closely similar backgrounds. Poincaré enters into 'florid details' about how these two mathematicians comported themselves only to illustrate what a range of effects this fundamental difference in intellectual temperament may have. The thesis that a mathematician's social circumstances will wholly determine his intellectual outlook and approach (of which his typical response to anomaly will be just one feature) seems to me to stand in no need of undermining. But if undermining were needed, then surely it is provided by Poincaré's observation that, despite being 'scholars of the same school at the same time; [having] had the same education; [and being] under the same influences', Hermite and Bertrand had diametrically opposed intellectual temperaments.

(3) Bloor's response to my claim that his sociological theory is, as it stands, scientifically worthless, is to assert that his theory is no worse off than the early nineteenth-century atomic theory of chemistry. In fact Dalton's theory was testable in areas other than that of chemical reactions mentioned by Bloor. But even there, the theory was *made* testable by the addition (guided by Dalton's simplicity rule) of (conjectural and revisable) molecular formulae. This achievement of a degree of testability (and a degree of confirmation) made the theory scientifically interesting. Nothing is easier (or safer) than to produce a research programme, especially if one does not even feel obliged to produce arguments for the plausibility of its basic assumptions. The difficult part is to produce refutable variants which survive empirical testing. Bloor has done only the easy bit.

EDITOR'S NOTE: Sadly, this conversation must rest here.