Introduction: Setting the Scene

The dialogue takes place in an imaginary library, not so far in location and spirit from Cambridge's Whipple. Two alarmingly precocious students, Iota and Mu, are sitting at a table, Imre Lakatos's Proofs and Refutations (1976; compiled from essays published in 1964) in hand, and surrounded by stacks of books on historiography and eighteenth and nineteenth century mathematics. Essay due in less than a day, their particular conundrum is the strange history of the Euler-Descartes conjecture. For any polyhedron, the conjecture states, the number of faces plus vertices is two more than the number of edges. Lakatos has presented them with a quandry. They had vigorously researched all the great men, the key dates, the pivotal publications. But Lakatos wrote that there was more to that. Mathematics was not just a linear sequence of timely discoveries. It involved, instead, a winding and dialectical process of proofs and refutations, which Lakatos argued could be best understood through his heuristic method of history. \`What on Earth is a heuristic history?' our scholars wondered. \`What on Earth is a heuristic?' \`How am I ever going to finish this essay?' Keen to (at the worst) procrastinate or perhaps (at the best) to gain some critical insight which will propel them toward essay completion, Iota and Mu begin,...

Thesis: What is the history of mathematics?

Iota: I for myself must say that I have for quite some time been fascinated by this pretty little book. Apart from being so amusingly written, it also analyses quite profoundly, I think, the way mathematically rigorous arguments really develop. You have a conjecture, established by trial and error. Then you try to prove it. Then people start pointing out counter-examples of various sorts, sometimes refuting the conjecture, sometimes pointing out hidden assumptions in the proof. How are the contradictions arising in this process solved? Some mathematicians believe mathematical theorems and proofs can only be true, otherwise they are nothing at all, so they try to eliminate the contradictions by several different strategies. But the only way that actually brings an increase of knowledge is a non-dogmatic adaptation of your concepts and proofs to incorporate the inherent inner complexity of the problem. This is beautifully illustrated on the case of the Euler-Descartes conjecture.

Mu: But don't you see that this has nothing to do with the actual history? [Mu stops, stutters, and resumes] OK, that's perhaps too strong. Lakatos assembles all the attitudes of various historical actors together in a somewhat-historical and somewhat-philosophical narrative. But setting aside the basic accuracy of this description, I simply can't accept this dialogue between a bunch of students (as if all professional mathematicians are merely eager students!) working on the same problem in a single classroom as a representation of what those people were doing. It kind of suggests that all those mathematicians, from Euler through Cauchy, Lhullier, Gergonne all the way to Poincaré, approached the conjecture and its proof from the same general perspective, in the same context. It's as though they were reading the same sources, hearing the same arguments, feeling the same disciplinary pressures, and drawing from the same stock of ideal mathematical forms when building their refutations. But surely these people weren't thinking just about polyhedra for all those decades and centuries? And it's not just the Euler-Descartes conjecture! Even Lakatos' Appendix about the concept of uniform convergence, which is not set in a classroom, has the same flaw. Lakatos argues that the concept emerged in the process of proofs and refutations of Cauchy's conjecture about the universal applicability of Fourier series, but he ignores the broader context of Cauchy's goals and interests outside mathematical analysis, not to mention the harried political environment of post-Napoleonic France, the huge changes to mathematics as a discipline, the,... well, you get the point.

Iota: Well I guess you can't really cram two centuries of history into one small book without some distortion, can you? And of course the dialogue highlights the interaction, which won't be as prominent if you look at it from the viewpoints of particular mathematicians, but I see it as a strength of Lakatos' writing, not a weakness. His reconstruction is an abstract one, he distills the underlying dialectic of the historical process, and when you see these arguments side by side, you understand that this is the true essence of mathematical progress – at least in such rich and growing areas like polyhedra. Sure, these mathematical ideas didn't develop in a closed classroom, but as far as the essential nature of mathematics is concerned they may as well have. Worrying about where the ideas came from is just historical busy work. The true goal of the history of mathematics is to understand how the ideas fit together over time, and that's exactly what Lakatos shows. Moreover, Lakatos also manages to show the other directions mathematicians might take, like surrendering to counter-examples, denying their significance by treating them as monsters or exceptions etc. So he pays attention to historical accidents, but shows that compared to the mighty dialectic, they are just that: accidents, not driving forces.

Mu: Wow, mighty dialectic! [sarcastically] I guess history's all just one big imaginary conversation!... we'll have to come back to that. But I'd like to dwell more on the previous point about the difference between the imagined driving force of the historical development and the real factors behind it. When you take some positions out of context and insert them into an imaginary dialogue, you're creating a logic that really seems irresistible – but those people might in fact have been driven by quite different motives than trying to reconcile one version of the theorem or another. I doubt that they had even roughly similar notions of what it means to do mathematics, what it means to be rigorous, what should be the goal of a mathematician etc. Remember, what counted as a good mathematical proof in Switzerland in 1760 was considered maybe a good starting point in France in 1800, and became an abhorrent example of everything wrong with mathematics for the French by 1820.

Iota: Oh my goodness! Have I been talking to an externalist relativist all the time?

Mu: A what?!

Iota: An externalist relativist! You know,... someone who believes that all possibilities in mathematics are equally plausible, and it's some vague and completely non-mathematical social and historical forces which decide which theories and ideas come to be accepted by mathematics as a whole.

Mu: As far as I'm concerned, you can believe that there is one mathematics, one correct concept of rigour and one correct definition of polyhedra out there, if you like, but you can't deny that what people want to achieve, what they consider satisfactory and valuable and all such questions have a huge impact on the development of anything, including mathematics. And that's all still compatible with internalist history. As for the externalist label, I think I don't deserve it, but I have to admit that I feel Lakatos simply ignores the reality outside discussions of mathematicians amongst themselves. I know that arguing for social influences on mathematics, as in Bloor's article 'Polyhedra and the Abominations of Leviticus', seems and often is just a ridiculous exercise of defending an SSK dogma. But the dialogue in 'Proofs and Refutations' implicitly suggests that serious mathematical discussion takes place in a vacuum, which is plainly false. Even in the classroom, there are social relations of power and authority which open and close mathematical possibilities. Who frames the question? Is the teacher just like the other students? Or do you really think that all that actually happened in the case of the Euler-Descartes conjecture was dictated only by your mighty dialectic? That there was always a firm link between the conjecture and its proof, that there was only one accepted version of the proof at a time, or that all changes came primarily because of the counterexamples and an increasingly better analysis of that one magical proof?

Iota: Of course not. The dialectic is simply a way of linking some arguments put forward in many different discussions of the same problem into a logical, coherent framework. What separated and motivated these discussions is a secondary point so long as the people entering the field later already read them as a single story. Thus the dialectic gradually establishes itself in the eyes of the people who read the summary of the discussions. These are found in papers, textbooks, and classrooms. Poincare didn't read David Bloor, but he did read Cauchy and Euler. Historical actors enter an imaginary classroom of their own, where Alphas and Betas and all the rest are arguing against each other, and they might choose to align themselves with one of the characters, or perhaps do what Epsilon did and solve the problem by a complete translation into some axiomatised domain. To see the development dialectically is completely natural, even if it didn't correspond to the way the history actually came about, and it helps you understand the history's properties more fully. After all, that's how the actors themselves would have seen their place in the development of mathematics. If you however reject all that had been said as useless and show only the results, which is what the dogmatists do, or deconstruct the dialectic into unconnected historical accidents, which is what you historicists and relativists seem to excel in, you do a disservice to mathematics. I only want to say that this dialectic becomes a real factor as problem areas mature, which doesn’t deny that there are potentially lots of things going into mathematical understanding. What we don't want to say is that the history of mathematics works the same as the history of any other set of concepts, like love or nationalism. There's something else going on that makes it mathematical.

Antithesis: Heuristics

Mu: So what is it, exactly, that Lakatos thinks mathematicians do?

Iota: Well, they have approximate ideas, like what a polyhedron or a continuous function looks like. It seems like the process of proofs and refutations is about testing and evolving those ideas, in some way. So Euler and Descartes had this image of polyhedra based on their study of relatives of the five regular ones, and Euler's formula of V-E+F=2 was a conjecture which was actually true for anything they would have considered (84). But then the first proofs of Euler's formula had to contend with more and more possible counter-examples, and mathematicians then had to constantly revise what they meant by polyhedron, or even other concepts in the theorem and proof. So by constantly and dialectically discovering and challenging new ideas derived from previous ones, they improved their understandings of polyhedra and their geometry.

Mu: But that's what I don't understand. What's the heuristic doing in all of this? If all of the new understandings are coming from proof-analysis, then it seems like the mathematical heuristics are sort of like an ontological status-monitor which occasionally prints out updates of the discoveries of proof analysis. Surely Lakatos would see a greater role for a mathematician's intuitions and vague ideas than merely being the causes of one guilty lemma or another which proof analysis would then improve.

Iota: Well, heuristics certainly play a part in proof analysis, like when we look at examples. Some proposed polyhedra will force us to reconsider our proofs. But if I said that the cat I walked past in the alley last week was a counter example to Euler's theorem, you'd say I was being ridiculous. Lakatos seems fascinated by cases where some things start as monsters and end up as examples, like with Dirichlet's function which has the value 0 where x is rational and 1 where x is irrational (151). It started as an absurd monster function, and is now a textbook example for explaining Lesbegue integrals. And I suppose possible examples would themselves come from heuristic understandings. That's why you'd never suggest that a cat was a polyhedron in the first place. It's also why monsters and non-examples often look deceptively like genuine examples, and a prover has to constantly refine her understandings in order to see why they are false positives. That also explains how guilty lemmas come about: when someone writes a proof, she is often thinking less about formal definitions and lemmas than about heuristic ideas and vague notions. That's why proof analysis is so important.

Mu: This all makes sense when we're trying to understand how examples and proofs come about, but that doesn't seem to be Lakatos's point. I mean, I know he disagrees with the distinction between the context of discovery and the context of justification (42, 143n2), but his whole idea of heuristic histories as rational reconstructions (5, 146) seems far more interested in the latter context than the former.

Iota: It does seem that way, I suppose. Especially his appendices, which are the more polemical parts of the work, seem written with an understanding that the heuristic histories he advocates don't necessarily correspond with the so-called actual history of discovery. His focus on Rudin's text in analysis is all about trying to improve how mathematics is taught, not how its history might be explored or understood. But we shouldn't forget that good pedagogy needs good history, and the converse as well, though Lakatos talks about this less.

Mu: So he's interested in teaching and communicating new results, and heuristics are central to that process of spreading understanding. It's natural that he'd make unrigorous mathematical understandings and intuitions a central part of that story. I also don't think anyone these days would have a problem with his argument that changing intuitions and heuristics are central to the history. But does Lakatos think that heuristics are part of the actual substance of mathematics or mathematical dialectics? Ultimately, he leaves historically situated understandings of mathematics at the wayside, saying that the kind of understanding he seeks is of a history-free autonomous mathematical dialectic (146). Humans and their intuitions are the driving forces of mathematical development, but they somehow manage to be epiphenomenal.

Iota: I wouldn't say he throws out those situated understandings. History and its rational reconstruction, after all, are his main methods. But I think they're more crucial than even that. For Lakatos, his prized method of proof analysis is precisely how heuristic understandings get transformed into proper mathematics. The mathematician is always in an uneasy relationship with the formal aspects of her proof, and proof analysis is what sorts that all out.

Mu: But what does it sort out? It sometimes seems in Lakatos's history that there are naive guiding concepts and formal equations and all proof analysis does is make the equations more and more complicated and the guiding concepts more and more confused. Is that really what mathematics is, for him? What ever happened to centuries of thinkers trying to understand the world around them and its fundamental abstract underpinnings?

Iota: I see precisely the opposite happening in Lakatos's story. Yes, there's this swirl of confused understandings and faulty proofs, but Lakatos's proposed method of heuristic history lets us see that there's actually a solid core of ever increasing understanding which gets sedimented by this swirl. Mathematics needs essentialist histories in order to see the order in the chaos. Where did the definition of measurable sets come from? In a traditional view, it either makes no sense---except that it works---or it was a miraculous and inspired innovation. Neither view is satisfying. Instead, Lakatos teaches us to see Caratheodory's definition as a genuine mathematical discovery which came out of the process of proofs and refutations.

Mu: Except that Caratheodory's definition of measurability really was pretty much an historical accident. This is Bloor's complaint with Worrall and Zahar, Lakatos's posthumous editors. Yes, it's possible to read Lakatos as a Platonist, but I think it's a lot more interesting to see his argument as showing the essential contingency of mathematics. Mathematics is the dune which sediments out of the sandstorm of formalisms and heuristics, but there's nothing particularly special about that particular stable arrangement of sand. It's not like there's one true mathematics which the mathematical dialectic has been slowly revealing. Despite his criticisms of Popper (c.f. 142-143), Lakatos has more in common with him than he might admit. Proof-analytic disputation does get at the truth of things, in his view. The goal-directedness of mathematics probably also comes from Lakatos's interest in Hegel and Marx, both of whom appear as explicit influences in his argument (cf 146).

Iota: I suppose we can agree that in either case, mathematics involves a continual process of revising understandings, whether those are in the context of discovery or justification. Seeing history through proofs and refutations gives a powerful way of showing how vague ideas and evolving formalizations interact to force out new ideas and possibilities.

Synthesis: Eternal heuristics, changing history

Mu: That touches upon a crucial question: How should 'Proofs and Refutations' be read? As a philosophically inclined history of the Euler conjecture? I don't think so. I grant it a status of a textbook with historical illustrations. A textbook of the heuristic, dialectical method of proofs and refutations.

Iota: I don't agree with the term 'textbook'. Textbooks are rigid and infallibilist: you are expected to accept the authority of the writer, not to argue with him. This book has a thesis – it actually grew out of his PhD thesis, didn't it? And notice that his method is applied not only to the mathematical conjecture, but also reflexively to itself, because it is being built up in a similar fashion of gradual approximations and improvements.

Mu: Textbooks, though, don't have to be rigid and infallibilist: in fact Lakatos argues for a change of style of mathematical textbooks, and mathematical education in general, to something very similar to his book. So I still think it is an exemplary textbook. Moreover the teacher seems to stand above the classroom full of arguments. He is the one who possesses and transmits the knowledge of the almighty heuristic. Although his mathematical knowledge about the conjecture seems to evolve as the discussion proceeds, his metamathematical, methodical knowledge is perfect at the outset and doesn't need any input from the dialogue. No, the method isn't being built up: it's being revealed.

Iota: I can see where you're heading. I've also always felt a bit uncomfortable about the origin of the method of 'Proofs and Refutations'. Its elements are introduced without any historically documented basis, in the order dictated purely by its own logical structure. It really does seem like Lakatos invented it, like he didn't reconstruct it from the historical record. But do you remember what Lakatos says about C. Jordan and the concept of bounded variation? 'He did not invent the concept, he did not 'introduce it' – he rather discovered it in Dirichlet's proof in the course of a critical re-examination. To 'introduce' a concept out of the blue is a magical operation which is resorted to very often in history written in deductivist style!' (148)

Mu: Ah, you mean that Lakatos also discovered his proposed method in the history of mathematics in the course of a critical re-examination. Historiography, itself, then might be dialectic.

Iota: Yes. Lakatos offered a thesis. And it's up to us to analyze it, to find its guilty lemmas, its nagging counterexamples, and improve upon the procedure. While we don't know how much the process of proofs and refutations actually existed in the history Lakatos describes, we are offered a version of the process of history which demands consideration and reconsideration.

Mu: So even in disputing him we are affirming him! We might discover the right way to do history by arguing over his. That sounds kind of Platonic to me. What an ironic, but perhaps inevitable fate for a dialectician!