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Studies in Epistemology, Logic, Methodology, and Philosophy of Science

Roman Frigg · J. McKenzie Alexander · Laurenz Hudetz · Miklos Rédei · Lewis Ross · John Worrall *Editors*

Proofs and Research Programmes: Lakatos at 100

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Preface

Imre Lakatos was one of the most significant philosophers of science and mathematics of the twentieth century, and his ideas remain important and relevant today. As the entry on Lakatos in the *Stanford Encyclopedia of Philosophy* attests "Lakatos's influence, particularly in the philosophy of science, has been immense". November 2022 saw the centenary of Lakatos's birth, and the event was marked by an international conference held at the LSE—where Lakatos made his career after he had emigrated from Hungary to England—the conference focussing on the continuing influence and relevance of his work. With the exception of two papers, this volume consists of a selection of papers that were presented at the conference.

We are immensely grateful to Dr Spiro Latsis, without whose generous financial support the conference would not have been possible and this book would not have been published open access. We would also like to thank our colleagues in the Department of Philosophy, Logic and Scientific Method and the Centre for Philosophy of Natural and Social Science for logistical support with the organisation.

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Chapter 1
Introduction

Roman Frigg, J. McKenzie Alexander, Laurenz Hudetz, Miklós Rédei, Lewis Ross, and John Worrall

Abstract This chapter provides an introduction to the book. The twelve essays in the book fall into three groups. Essays in the first group address problems in the philosophy of mathematics; essays in the second group investigate foundational questions concerning Lakatos's philosophy of science; and essays in the third group apply Lakatos's concept of Methodology of Scientific Research Programmes (MSRP) to medicine. The book ends with an epilogue.

Although Lakatos is nowadays primarily known for his work in philosophy of natural science, and in particular for his Methodology of Scientific Research Programmes (MSRP), his first major contribution was his *Proofs and Refutations*—a groundbreaking work in the philosophy of mathematics. The central thesis of *Proofs and Refutations* is that the development of mathematics does not consist in the steady accumulation of eternal truths, as conventional philosophy of mathematics suggests. Mathematics develops, according to Lakatos, in a much more dramatic and exciting way, via a process of conjecture, followed by attempts to "prove" the conjecture (in his view, to reduce it to other conjectures) followed by criticism via attempts to produce counterexamples both to the conjectured theorem and to the various steps in the proof, resulting in the proof of a much modified version of the original conjecture.

Among the still open questions about Lakatos's views are: Does Lakatos's account really amount to a fully "quasi-empirical" view of the epistemology of mathematics to rival the traditional philosophies of logicism, formalism and intuitionism? Or is it instead "merely" an—albeit fascinating—account of how mathematical theorems are arrived at, an account which has no consequences for

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the epistemological status of those eventually arrived-at theorems? Is Lakatos's central example—the Descartes-Euler conjecture about polyhedra—itself too "quasi-empirical" to be representative of mathematics in general? Finally, did Lakatos outgrow his Hegelian roots? Or is *Proofs and Refutations* best, or perhaps even *only*, understandable as a thoroughly Hegelian work? Some of these issues are touched on in the contributions to the philosophy of mathematics section of this volume.

Turning, then, to his philosophy of science, Lakatos famously presented MSRP as a synthesis of the views of Karl Popper and Thomas Kuhn—preserving from the former the claim that theory change in science is a rational process, while allowing that the latter's account of how scientists regard and deal with experimental difficulties is altogether more true-to-scientific-life than Popper's. There is no consensus as to whether or not this "synthesis" succeeds. Nor is there any consensus about how to interpret Lakatos's central notion of progress and the associated concept of "novel fact". Another open issue is whether the insights underlying Lakatos's MSRP can be captured and thereby given a more solid foundation by the Bayesian approach to scientific reasoning. The view that those insights can be given a Bayesian justification was argued by Howson and Urbach in their *Scientific Reasoning: The Bayesian Approach*.

A large part of the continuing influence of Lakatos's ideas consists in attempts to apply his MSRP to identify and evaluate research programmes in special sciences such as medicine, psychology, economics, and sociology, as well as in disciplines like educational theory, informatics, and international relations, which otherwise receive scant attention in philosophy of science. These attempts often originate in the sciences themselves and are driven by practitioners' desire to understand developments in their fields, rather than by traditional philosophical concerns. Being relevant beyond the confines of professional philosophy is probably the best marker of a lasting influence.

The book consists of 12 essays, which fall into three groups. Essays in the first group address problems in the philosophy of mathematics; essays in the second group investigate foundational questions concerning Lakatos's philosophy of science; and essays in the third group apply his MSRP to medicine. The book ends with an epilogue.

The first group of essays begins with Philip Kitcher's "Mathematical Methodology". Lakatos regarded his *Proofs and Refutations* as a study in the "methodology of mathematics" or the logic of mathematical discovery. Philip Kitcher agrees that philosophy of mathematics has—both before and after Lakatos—concentrated on issues about the status of mathematical results and ignored issues about how those results emerged in the first place; and it has done so to its cost. Accordingly, Kitcher's contribution develops a mathematical methodology. He outlines the major changes that resulted in the mathematics of the late nineteenth century, indicates how those results emerged, and appraises them in terms of a notion of pragmatic progress (progress *from*) as opposed to any notion of teleological progress (progress *to*). Kitcher's methodology transcends Lakatos in many ways but is recognisably Lakatosian in spirit.

In her “Proofs as Dialogues: The Enduring Significance of Lakatos for the Philosophy of Mathematical Practice”, Catarina Dutilh Novaes focuses on what Lakatos’s ideas have to offer for contemporary philosophical work on mathematical practice. In particular, she highlights the influence of Lakatos’s *Proofs and Refutations* on the development of her dialogical account of deduction and mathematical proof, which relies on so-called Prover-Skeptic dialogues. Similarities and differences between Prover-Skeptic dialogues and Lakatosian Prover-Refuter dialogues are discussed with special attention to the roles of cooperation and adversariality. The article closes with a reflection on the broader philosophical differences between Lakatos’s “Hegelian” approach and Dutilh Novaes’s dialogical pragmatism: Lakatos aims at the dialectic development of mathematical *concepts,* disregarding individual human activities, while Dutilh Novaes’s dialogical account of mathematical proof is primarily about *human agents* and their interactions.

In their “Lakatos and the Euclidean Programme”, Alexander Paseau and Wesley Wrigley critically examine and revise Lakatos’s account of the Euclidean Programme (EP), which is a foundationalist account of mathematical knowledge inspired by Euclid’s Elements. In Lakatos’s view, a system of mathematical knowledge that is organised according to the EP starts from a finite set of trivially true axioms with perfectly well-understood primitive terms, and truth then “flows” from axioms to theorems via deductive channels. The authors critically examine various aspects of Lakatos’s account and suggest modifications that lead to an improved characterisation of the EP, consisting of seven principles. The proposed characterisation inherits some core ideas from Lakatos’s account (e.g. the idea of flow) but differs in various other respects. The outcome is an updated reconstruction of the EP in the spirit of Lakatos.

In “Proofs and Refutations, Non-Classically and Game Theoretically”, Can Başkent argues that the reasoning in Lakatos’s *Proofs and Refutations* is not governed by the rules of classical logic but instead exemplifies paraconsistent logic—a type of logic where it is not the case that everything follows from a contradiction. Başkent points out that inconsistencies play a fundamental role in the Lakatosian method of proofs and refutation. Crucially, when contradictions arise (e.g. due to counterexamples to a conjecture), one is not permitted to draw arbitrary conclusions. How one can move forward in the face of a contradiction is precisely what defines the method of proofs and refutations. Furthermore, Başkent argues that the strategic way in which inconsistencies should be handled according to Lakatos can be fruitfully analysed through the lens of game theory. This is illustrated using concrete examples from *Proofs and Refutations*.

Vincenzo Crupi’s “The Case of Early Copernicanism: Epistemic Luck versus Predictivist Vindication” is the first contribution of the second group, which concerns foundational questions about Lakatos’s philosophy of science. In his paper, Crupi investigates the issue of whether the adoption of the Copernican theory by Kepler and Galileo (as well as by Copernicus himself) was, as many have claimed, a matter of “epistemic luck”: these luminaries happened to make what was by later lights the correct choice but had no empirical justification for that choice at the time when they initially made it. The idea that Kepler and Galileo were ‘lucky’ has generally been based on the claim that—allegedly—any empirical phenomenon that might be taken to support Copernican theory could in fact equally well be accounted for on the rival Ptolemaic theory. In a widely read paper, Lakatos and Zahar argued that, to the contrary, once the notion of prediction is properly understood, the initial Copernican theory is seen to have enjoyed predictive successes not shared by its Ptolemaic rival and hence Kepler's and Galileo's theory-choices are vindicated. Crupi investigates whether Lakatos and Zahar's view stands up to historical and philosophical analysis.

The paper "The Bayesian Research Programme in the Methodology of Science, or Lakatos Meets Bayes" by Stephan Hartmann argues that, when understood correctly, Bayesianism is an instance of a progressive Lakatosian research programme in the methodology of science. This stands in stark contrast to Lakatos's own rather sceptical view about Bayesianism. To support its claim, the paper considers and then dismisses three challenges to Bayesianism. These arise in connection with indirect evidence, new types of evidence, and genuinely new evidence. Hartmann shows how these challenges can be met within the Bayesian Research Programme. He also shows that in order to be able to handle these challenges, one has to abandon a core tenet of traditional Bayesianism: that belief change has to be made via standard conditionalization. Instead of relying on standard conditionalization, belief change should be based on the "Principle of Conservativity": the requirement that belief change should minimize a certain distance between the probability measures representing beliefs.

Thodoris Dimitrakos' "Lakatos's Naturalism(s): Distinguishing between Rational Reconstructions and Normative Explanations" examines Lakatos's concept of "rational reconstruction" in the philosophy of science, defending its use against critics like Kuhn who claim it distorts historical records. After briefly discussing, and setting aside, some uncharitable criticisms of Lakatos's account, Dimitrakos identifies the real problem it faces: that Lakatos's attempt to provide both a historically informed philosophy of science and an account of scientific rationality led to problems of circularity. Dimitrakos argues that these problems can be resolved in three steps. First, one needs to distinguish between rational reconstruction, a philosophical tool for evaluating different theories of scientific rationality, and normative explanation, a historiographical category. Second, one has to reject Popper's "three worlds" conception, situating Lakatos's approach within a liberal naturalism. And, finally, one must replace Lakatos's inter-methodology evaluation process with a suitable intra-methodology process. In doing so, the chapter aims to show how Lakatos's work remains relevant to contemporary debates about the relationship between history and philosophy of science.

In his "Heuristic, Physics Avoidance, and the Growth of Knowledge", Jack Ritchie examines the notion of positive heuristic in Lakatosian philosophy of science, particularly in *Methodology of Scientific Research Programmes*. He begins by setting aside an alternative view of heuristic due to John Worrall (claiming that it departs too far from the source text), and then offers a different interpretation inspired by the work of Mark Wilson. On Ritchie's account, the positive heuristic fosters the growth of knowledge through a process often best understood as "modelmaking and improving". On this view, a central driver of progress is the construction and refinement of scientific models. The aim of these models is to convert empirical difficulties into mathematical difficulties. These difficulties include the construction of mathematically tractable models and providing plausible bridges between higher and lower-level models of the same phenomena. On the view that Ritchie provides, refutation is less essential to Lakatosian progress than sometimes supposed, with the incremental improvement of models playing a more central role.

Samuel Schindler's "Beyond Footnotes: Lakatos's Meta-Philosophy and the History of Science" revisits Lakatos's approach to historical facts. Lakatos infamously claimed that the actual history of science could be recorded in the footnotes of rational reconstructions of science. Schindler points out that Lakatos's approach to actual history was more reasonable than that, not least because he argued that a philosophical methodology of science should aim to maximise rationally explainable facts, even though there should be no expectation that all historical facts will turn out to be rational. Schindler examines this idea in the context of the contemporary discussion about meta-philosophy. The paper then compares Kuhn's and Lakatos's approaches to science and argues that Lakatos's account, contrary to what he himself thought, doesn't have a more legitimate claim to rationality than Kuhn's.

The next two contributions form the third group of papers, which are dedicated to the philosophy of medicine. In his "Cholesterol and Cardio-Vascular Disease: Degenerating Research Programmes in Current Medical Science", John Worrall argues that the mini research programmes built to defend two extremely influential claims in current medicine have both consistently degenerated. If so, as he remarks, one would have expected those two claims to have been rejected as not evidence-based. But in fact, although the consensus on the first claim now shows some signs of breaking up, it remained in place for many years after degeneration set in; while the second remains almost universally accepted in medicine and remains the basis for accepted medical advice and treatment. The second part of his paper analyses this clash between expectation and reality, leading to a re-examination of Lakatos's distinction between internal and external history.

Anya Plutynski's "Trade-offs and Progress in Cancer Science" begins with the observation that almost all examples of research programmes analysed in terms of progress and degeneration by Lakatos and those influenced by him were from physics (or occasionally chemistry). One might therefore be tempted to object that MSRP, while a useful tool for analysing developments in basic sciences like physics and chemistry, is not usefully employed in other, more "special" or applied sciences. Plutynski raises this question and concludes that appropriately analysing developments in Cancer Science may require replacing Lakatos's notion of progress in science with one that recognizes the prevalence of trade-offs intrinsic to the culture of science.

The book ends with an epilogue, John Worrall's Scientific Theory-Change and Rationality: Lakatos and the "Popper- Kuhn Debate" in which he takes a look back at the "Popper-Kuhn" debate and Lakatos's attempt to resolve it. The Popper-Kuhn debate was one of the foci of attention at the famous Bedford College Colloquium held in the summer of 1965. What exactly was at issue in this debate? Was Lakatos right that Kuhn's account of theory-change in science denies that change is a rational affair by reducing change to "a matter of mob psychology"? Was Lakatos right that his MSRP provides a satisfactory "synthesis" of the views of Popper and Kuhn—one that preserves the rationality of theory-change? How has the debate progressed since 1965 and where does it currently stand? The chapter is the written version of a lecture Worrall gave at the conference *Centenário Imre Lakatos: matemática e ciência* in Sao Paolo in November 2022. We have kept the lecture in its original form. The points come across most vividly in this talk by a PhD student supervised by Lakatos himself.

Chapter 2
Mathematical Methodology

Philip Kitcher

Abstract In *Proofs and Refutations*, Imre Lakatos proposed to reorient the philosophy of mathematics. He suggested abandoning the search for a foundation for mathematics in favor of providing a methodology for mathematics. This essay attempts to pursue the methodological project in a different fashion. It sketches the long history of mathematics, and reflects on the kinds of benefits attained in a number of important transitions. It argues that these advances embody quite different gains. While diverging from Lakatos's quasi-Popperian framework for mathematical progress, I conclude that his proposed reorientation of the philosophy of mathematics would be an advance that is long overdue.

2.1 Introduction

Imre Lakatos began his career in philosophy with some remarkably original essays in the philosophy of mathematics. His Cambridge Ph.D. dissertation was published almost in its entirety as a four-part article, "Proofs and Refutations", in the *British Journal for the Philosophy of Science* (Lakatos, 1963–1964). After his death, this became a free-standing book (Lakatos, 1976), edited by John Worrall and Elie Zahar, in which further, previously unpublished, material was added. Two years later, the first half of the second volume of Lakatos's *Philosophical Papers* (Lakatos, 1978), edited by John Worrall and Gregory Currie, contained articles on the philosophy of mathematics (some in revised versions).

I am most grateful to the organizers of the Lakatos Centennial Conference at LSE for inviting me to deliver a paper, and to contribute to this volume, rightly celebrating the work of a major philosopher of science. I am also indebted to a knowledgeable and perceptive reviewer for comments that have enabled me to improve the final version.

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Lakatos plainly hoped to reform this area of philosophy. The Introduction to the book version of *Proofs and Refutations* is forthright:

> The purpose of these essays is to approach some problems in the *methodology of mathematics*. ... The recent expropriation of the term 'methodology of mathematics' to serve as a synonym for 'metamathematics' ... indicates that in formalist philosophy of mathematics there is no proper place for methodology qua logic of discovery. (Lakatos, 1976, p. 3)

Despite his firm understanding of then-current debates about the merits of various programs in the "foundations of mathematics", and of the set-theoretic and proof-theoretic results around which those debates centered, Lakatos contended that the discussions among logicians and philosophers of mathematics neglected important questions—indeed *the* most important philosophical questions about mathematics. Strenuous efforts to *reconstruct* mathematical knowledge in some preferred fashion completely bypassed issues about how the body of knowledge to be set in order had emerged. Obsessed with tidying up the corpus of mathematical knowledge, philosophers seemed completely uninterested in how mathematicians had obtained that knowledge in the first place. Hygienic proposals for arranging the corpse took precedence over understanding the ways in which the life of mathematics had proceeded.

Were philosophers of mathematics embracing an odd variety of skepticism, Lakatos wondered, tacitly denying any genuine mathematical knowledge before the advent of formal logic? With characteristically acerbic wit, he pointed out that "Newton had to wait four centuries until Peano, Russell, and Quine helped him into heaven by formalising the calculus" (Lakatos, 1976, p. 2). Understanding the growth of mathematical knowledge, he thought, might relieve Newton of any strain on his patience. To achieve that understanding would require specifying the methodology of mathematics, that is, identifying the standards governing the rational progress of mathematics. For Lakatos, at this stage of his career, that meant adapting the correct normative theory of the natural sciences to the mathematical case. Convinced that Popper had supplied that theory, his title, and his four-part article echoed Sir Karl.

Despite the brilliance of his discussion of his major example—the historical development of ideas about the relations of the number of vertices, edges and faces of polyhedra, initiated by the Descartes-Euler conjecture—I don't think Lakatos solved the problem he had posed. Nor do I believe that the essays he wrote about mathematics identified the problem in its full generality. Nonetheless, this part of his philosophical work has always seemed to me a major achievement, one that remains underrated to this day. For he had formulated the right question, and offered a convincing treatment of some instances of it. Today's philosophy of mathematics would be far richer and far healthier if its practitioners had paid attention.

Perhaps their indifference stemmed, in part, from Lakatos's tendency to draw provocative corollaries. Viewing mathematics as growing according to a quasi-Popperian methodology led him to dispute the common emphasis on and understanding of proofs, and to wage a campaign on the search for foundations. Were card-carrying philosophers of mathematics so threatened by these implications that they dismissed his central project, and thus failed to confront the methodological question he had raised (and, apparently, thought he had answered)? Historians of science certainly bristled at the suggested role for history—consigned to footnotes, while "rational reconstruction" filled the text; and they could meet the suggestion with a compelling critique (Arabatzis, 2017).

Although I have always been far more sympathetic to Lakatos' approach to the philosophy of mathematics than most of those who have ventured into that area, I must confess to a previous distortion that has caused me to underrate his achievement (Kitcher, 1977). I subordinated the question of elaborating a methodology for mathematics to the project of disputing the standard view—then and now—that mathematical knowledge is *a priori*. Half a century on, though I continue to maintain my (mad?) heterodox views about the apriority of mathematical knowledge, I no longer take that question to lie at the center of philosophical interest. An account of mathematical methodology, drawn from historical studies of the growth of mathematics is far more important.[1] Lakatos saw that clearly, I did not.

What follows is a belated effort to correct one of the mistakes of my philosophical youth.

2.2 Large-Scale Transitions: Conceptual Innovation

Between the earliest mathematical practice about which we have a relatively clear and detailed vision, and the mathematics for which Frege, Russell, Peano, and Hilbert hoped to supply foundations, a vast number of changes occurred. Even more would have to be considered if we were to study the further evolution of the discipline in the twentieth century and in our own. I shall terminate my explorations of history with the *status quo* as of 1879. I do so for two reasons. First, I would be woefully out of my depth in trying to understand, let alone analyze, recent developments in mathematics—it's good historiographical advice to focus on mathematical changes that occurred a century or more before the historian's birth. Second, I am sympathetic to Lakatos's thought that the methodology of mathematics should start by trying to understand the emergence of the body of knowledge the would-be foundationalists intended to bring to order.

During the roughly four millennia separating the Babylonian techniques for what we would see as solving equations (simple equations and quadratic equations) from late nineteenth-century algebra, analysis, geometry, topology, and probability theory, changes occurred at a number of different scales. The smallest ones are the most philosophically familiar. A mathematician uses received ideas to prove a new theorem. The largest are those on which I shall primarily concentrate. They are the ones revealing the most dramatic enrichment of mathematical language and accepted methods of mathematical reasoning. New notation is introduced, sometimes to express concepts that have not previously figured in mathematics,

[1] I take mathematical methodology to be a normative discipline, one that tries to characterize the ways in which mathematics should grow. Like Lakatos, I inherit the historicist spirit of philosophers like Kuhn and Feyerabend, and believe that we understand how a form of inquiry should grow by examining its past successes—and perhaps its failures, as well.

on other occasions to provide a more perspicuous way of working with familiar concepts, one that enlarges the class of permissible methods. The replacement of Roman numerals with the language we have inherited from Arab mathematicians of the tenth century is an obvious instance of the latter. The (different) notations offered by Newton and Leibniz for their versions of the calculus exemplify the former.

Lakatos's most famous example, the career of the Descartes-Euler conjecture, is a mid-scale transition. It does not leave the language of mathematics unchanged—new concepts are introduced to classify polyhedra—but the novel vocabulary is applicable only to a restricted class of questions. That is: of questions that provoke mathematical interest. The principal differences between the approach to mathematical methodology I take here and that pursued by Lakatos stem from my beginning from a question I take to be crucial: What makes a proposed change worthy of mathematical interest? As we shall see, the sources of mathematical interest are quite diverse. Typically, they lie in a sense of some deficiency or incompleteness with the status quo. Mathematical advances clear up the puzzles and dissatisfactions that prompted mathematical inquiry. They do so in diverse ways. Hence, my mathematical methodology breaks with the narrow Popperian framework Lakatos employs. It retains, however, the spirit of the enterprise he began.

2.3 A Broad-Brush History

To begin addressing the crucial question, it will help to have a broad-brush treatment of the history of mathematics that focuses on some of the large-scale changes generating the mathematics of the late nineteenth century. The roots of elementary mathematics, the simplest parts of arithmetic and geometry, are buried deep in prehistory, and any attempt to uncover them must be conjectural. At some point in our deep past, probably long before the invention of writing, our ancestors introduced into their language words for numbers, for the basic arithmetical operations, for shapes, distances and areas. *Perhaps* they did so in order to avoid quarrels that arose from recognizably unequal division of resources, or to facilitate exchanges of goods. Trade among groups dates back at least twenty thousand years (McBrearty & Brooks, 2000; Renfrew & Shennan, 1982). *Perhaps* the Babylonian interest in equations results from complex regulations about the shares inherited by relatives of different degrees (or sexes). *Perhaps* geometrical problems have their origin in efforts to assign portions of land so as to satisfy different claimants. If we want an account of how basic parts of arithmetic, geometry, and algebra might reasonably have been adopted by people in Mesopotamia, India, and Egypt, possibly independently, possibly through cultural transmission, speculations of these kinds furnish how-possibly explanations.

By the beginning of the common era, mathematical practice already outran the practical applications I have gestured towards as providing the initial rationales for introducing arithmetical and geometrical concepts. The integration of mathematics into a wide array of ventures surely exceeded its original, more limited roles.

Not only surveying and trade, but engineering, finance, and astronomy called for people with developed arithmetical and geometrical skills. But Euclid had already systematized geometry, introducing the idea of proving new theorems, whether or not they served any useful purpose. Mathematicians had formulated concepts for special types of numbers—prime numbers are only the most obvious example. Locus problems, equations of several degrees, and Diophantine equations all exercised the mathematical community. It is likely that these explorations were spinoffs from the more immediately practical techniques of arithmetic and geometry. Yet, as we shall see, they provided growing points for major expansion.

Fast forward to the early Renaissance. Although the work of using the mathematical framework inherited from the ancients to solve practical problems has intensified, generating further techniques and results within that framework, little conceptual expansion has occurred. One large achievement of the interval I've skipped over, is the provision of a notation that enables easier and more systematic methods for applying the fundamental arithmetical operations: doing sums—adding, subtracting, multiplying, and dividing. Try dividing MDXCI by XLIII—without translating. The traditional parts of mathematics—arithmetic, geometry, and the algebra of polynomial equations—have been further developed, both for practical purposes and with respect to solving "theoretical" problems of the kinds that have been recognized since antiquity: new Euclidean theorems, a few new solutions to locus problems and Diophantine equations, more results about prime and perfect numbers. But not much conceptually new.

Apart, perhaps, from one step whose full significance took centuries to manifest itself. Given that mathematicians' primary function seemed to be to use established techniques to help the bankers and the bridge-builders, it's hardly surprising that they were not held in high esteem within the academy. It's worth recalling that Galileo sought the title of 'philosopher' not 'mathematician'. Indeed, some mathematicians who devoted themselves to the abstract "impractical" problems, derived a significant portion of their income from performing for the high-born. Long before the age of television, an evening's entertainment for the privileged might feature a trip to the Pitti Palace to watch Niccoló Tartaglia or Gerolamo Cardano tackle problems that require a method for solving cubic equations.

For, possibly on the basis of ideas of earlier mathematicians (Scipione del Ferro is the leading candidate), Tartaglia had formulated a technique for attacking such problems. Unwisely, he let Cardano in on the secret, and was enraged when Cardano published it. His reaction is easy to understand. No entertainer wants a trade secret divulged.

Some modern students don't find the method easy to apply, and very few would have the skill to devise it.[2] Admiration for these mathematicians, and for their

[2] A clear explanation of the mathematical details underlying this historical episode can be found in (Cooke, 2008). With respect to other historical examples I employ, either this work or (Cooke, 2013) will provide helpful discussions.

contemporaries in the same line of work should be increased by knowing that they operated, as all their predecessors had done, without the aid of algebraic notation.

The introduction of a perspicuous way of formulating equations comes at the end of the sixteenth century (shortly after the careers of Cardano and Tartaglia), and begins the accelerated expansion giving rise to mathematics as Frege knew it. In 1591, François Viète, originally trained as a lawyer, published a book in which he offered a new notation (close to the one still employed) for representing algebraic equations. This enabled him to formulate explicitly the formulae underlying the solutions to quadratic equations (already achieved by the Babylonians), and to cubic equations (the methods of Tartaglia, Cardano, and other Italian mathematicians). Viète also recognized the relations between the sums of the roots of polynomial equations and their coefficients, as well as devising geometrical methods for tackling such equations.

His work paved the way for one of the most fruitful transitions in the history of mathematics. In 1637, Descartes made public his most important intellectual accomplishment, linking geometry to the new algebra in coordinate geometry. Rightly proud of the power of his method—as he frequently points out, the examples he provides illustrate how to solve infinitely many similar problems—he contrasts the scatter of results painfully generated by previous geometers with the systematic success he can offer the mathematical world (Descartes, 1954).

Armed with these tools, mathematicians of the mid-seventeenth century tried to extend them to classical geometrical questions that resisted solution: finding the lengths of segments of curves, constructing tangents and normals, computing areas, and discovering maxima and minima. Descartes himself, Fermat, Roberval, and Cavalieri all achieved some partial advances with these problems. A fully general approach came only at the end of the century, with the techniques of the calculus, independently (and differently) offered by Newton and Leibniz (Kitcher, 1983, pp. 230–241).

Though Leibniz won the notational contest, and his more uninhibited tolerance for infinitesimals eventually triumphed, Newton's conservative preference for tying the calculus close to geometry played a decisive role in the subsequent transformation of mathematics. For Newton proposed an approach to geometry connecting that branch of mathematics with the study of motion. He writes:

> In ye description of any Mechanicall line what ever, there may bee found two such motions wch compound or make up ye motion of ye point describeing it, whose motion being by them found by ye Lemma, its determinacon shall bee in a tangent to ye mechanicall line (Newton, 1967, p. 377).

By adopting this kinematic approach, Newton gains the ability to move to and fro between geometry and the theory of motion. Tools crafted for one domain can be applied in the other. In particular, the successes of the calculus in solving geometrical problems can be mirrored in kinematics, and form the platform on which Newton will erect his strikingly successful dynamics.

The subsequent history of the calculus is a tale of free-swinging success, in which most of those who participate don't share the qualms of Newton and his successors about “infinitely little” quantities that can be “blotted out” at the appropriate moments. Only when the liberated appeals to thinking about terms that are sometimes (opportunistically) positive and sometimes (opportunistically) treated as zero start to interfere with problem-solving, or when clever substitutions in infinite series generate odd-looking conclusions, do mathematicians turn their attention to mopping up what’s become an annoying mess (Kitcher, 1983, pp. 264–268). So, through the eighteenth and nineteenth centuries, clearer conceptions of limit, continuity, convergence, differentiation, and integration emerge to allow mathematicians to deal with the full array of functions they want to consider. The end result is the real analysis of Weierstrass and his school—although some mathematicians, Kronecker and Dedekind for example, contend that further steps are required. Frege, of course, has a far more tender conscience than any of them (Kitcher, 1986).

Between Newton and the late nineteenth century, there’s an explosion of mathematical developments, and a concomitant change in the status of the mathematician. As the role of differential equations in physics becomes recognized, the idle games of mathematicians appear in a new light. The “useless” problems with which they toy generate concepts and methods for doing serious investigation of the natural world. Cardano and Tartaglia are no longer court entertainers. People like them deserve prestigious chairs in prestigious academies, and bountiful rewards for their ingenious play.

Let’s pick up one thread in a rich and complex tapestry. Mathematicians quickly discovered, that applying the Tartaglia-Cardano method to some cubic equations results in a bizarre designation of the roots. So, for instance, if the initial equation is

$$x^3 - 15x - 4 = 0$$

the method yields the result.

$$x = \left(2 + \sqrt{(-121)}\right)^{1/3} + \left(2 - \sqrt{(-121)}\right)^{1/3}.$$

Recognizing 4 as one root, Rafaello Bombelli was inspired to extend the usual arithmetic operations to expressions containing designations of the “nonexistent square roots of negative numbers.” So, using modern notation, he defined:

$$(a + ib)\,(c + id) = (ac - bd) + i\,(ad + bc) \text{ where } i^2 = -1$$

Is it then possible to choose a value of k so that

$$(2 + ik)^3 = \left(2 + \sqrt{(-121)}\right) \text{ and } (2 - ik)^3 = \left(2 - \sqrt{(-121)}\right)?$$

If so, the offensive terms can be eliminated, and the root will be given as 2 + 2. Bombelli saw that taking $k = 1$ would do the trick. But, as he also recognized, his ability to choose an appropriate value depended on his already knowing the value of the root: because of that, he knew that 8 – $6k^2$ would have to be 2. In general, without knowing the root, you'd just have to guess at the value for the pertinent parameter.

So, an apparent curiosity. Most contemporaries and immediate successors accepted Bombelli's own verdict on the new expressions: "subtle and useless." A few, however, continued to explore. As the new Leibnizian analysis played unrestrainedly with infinite series representations of functions and unchecked tolerance for substitutions, many unproblematic results emerged.

$$e^x = 1 + x + x^2/2! + x^3/3! + \dots$$
$$\sin x = x - x^3/3! + x^5/5! - \dots$$
$$\cos x = 1 - x^2/2! + x^4/4! - \dots$$

As Euler saw, if you extend these functions to allow complex numbers as arguments, you can obtain a remarkable identity:

$$\begin{aligned} e^{iz} &= 1 + iz - z^2/2! - iz^3/3! + z^4/4! + \dots \\ &= \left(1 - z^2/2! + z^4/4! - \dots\right) + i\left(z - z^3/3! + \dots\right) \\ &= \cos z + i \sin z \end{aligned}$$

which yields as a special instance (when $z = \pi$)

$$e^{i\pi} = -1$$

Euler's "beautiful identity."

Play with the cubic has thus started a line of development that leads to an extraordinary connection among trigonometric and exponential functions, mediated by numbers most mathematicians had dismissed. Yet this is only one part of Tartaglia's and Cardano's legacy. Systematic attempts to solve polynomial equations of higher degree yielded success with the quartic, but a frustrating sequence of failures with the quintic. These developments prompted Lagrange to seek an understanding of why particular idiosyncratic substitutions of variables transformed the original equation into an equation of higher degree (a polynomial of degree six in the case of the cubic) that could then be reduced by some formula used for lower degree equations: the sextic is a quadratic in the cube of the artfully chosen variable (Cooke, 2008, pp. 82–85, 92–101). Given the understanding of the relations between functions of the roots and the coefficients of the original equation (extended and deepened since Viète's pioneering work), he focused on permutations of the roots, considering those functions that were invariant under permutations. His work made it clear why the techniques for cubic and quartic were successful, and it inspired early nineteenth-century mathematicians, Abel and Galois in particular, to introduce the concept of a group. The night before the duel in which he would be killed, Galois wrote out a synopsis of his ideas, revealing why the quest for a method to solve the quintic was doomed.

One final episode from nineteenth century mathematics will complete my review of some important transitions. Late eighteenth century studies of complex numbers introduced the notion of the complex plane as an analog of the real number line. They inspired William Rowan Hamilton to ask if there were higher-dimensional numbers. Hamilton first sought a three-dimensional generalization. Relatively quickly he convinced himself of the impossibility of generalizing in ways that would preserve the features of the elementary arithmetic operations he took to be important. The four-dimensional case appeared much more promising, although he encountered recurrent difficulties in defining multiplication. (Over a period of many years, he would retreat to his study to work on the problem. According to legend, when he emerged his wife would ask "Have you discovered quaternions yet?"—and he would ruefully shake his head.) Hamilton filled many waste-paper baskets with potential multiplication tables, all of which failed. Finally, on a walk around Dublin, inspiration came, and he carved the multiplication table into the stonework of a bridge. The breakthrough was to abandon commutativity.

We no longer talk of Hamilton's numbers—"quaternions" as he called them. Instead, his work is absorbed, like that of Abel and Galois, in the abstract algebra, already well-developed in Frege's time, which is central to the mathematics of the twentieth century.

2.4 Mathematical Progress: Significant Questions

A whirlwind tour of a few of the major transitions giving rise to the body of mathematics for which a few mathematicians and a whole long philosophical tradition have wanted to find foundations. If we reject Lakatos's satirical suggestion that there was no mathematical knowledge until the foundationalists provided it—or, since it's not clear that they've yet succeeded in providing what's needed, maybe there's no mathematical knowledge at all—we are faced with two obvious, and connected questions. What made the transitions out of which modern mathematics grew *reasonable*? What made them deserve the title of *advances*? We require accounts of mathematical reasonableness and mathematical progress that accommodate the history I have handled so roughly and crudely.

Preliminaries: I should explain how I think about progress, in mathematics and in the natural sciences, and defend my preference for talking about reasonableness in this context, rather than adhering to the familiar idiom of rationality. Many people adopt a narrow view of progress, keyed to salient examples. Unless you are Don Quixote, your travels aim at a destination, and your progress is measured by the decreasing distance to your goal. Some kinds of progress are like that. Many are not. Children learning to play musical instruments make progress by overcoming their technical problems and expanding the limits of their interpretive skills. The technology of computers and smartphones makes progress by eliminating the glitches of the devices and increasing the range of things they can do. Teleological progress is progress *to*. Pragmatic progress is progress *from*. In mathematics and in science, progress is pragmatic progress. Moreover, although professional communities are often reasonably viewed as pursuing significant problems, ultimate authority on that issue lies with the broader human population. Solving the problems experts identify as significant ought to contribute to human progress in the broadest sense—another form of pragmatic progress.

If all goes well with a community of inquirers, that community will select and resolve questions contributing to the progress of the discipline and to enhancing human lives and improving human societies. Investigators do not always need to ask whether, and how, what they propose to do will bear on human interests. Much of the time, but not always, they can take it for granted that pursuing the kinds of questions they and their fellows single out as significant will do no harm, and may even have positive consequences (possibly quite remote) for human projects. As in much ethical life, they can operate by habit, not constantly interrogating themselves about the worth of their enterprises. Perhaps, from time to time, it would be good for them to reflect on that issue. As I would put it (with thanks to Rudner, 1953), the scientist *qua* scientist, or the mathematician *qua* mathematician, is an ethical agent.

Following the ideas about significance that have been inculcated during your apprenticeship is usually a reasonable strategy. Since the notion of rationality oscillates between two unsatisfactory senses, it is better to talk about reasonableness here. One sense is far too thin and puny to serve the methodology of mathematics or of any natural science. Nobody should be interested in guidelines for not acting madly. The other, an artefact of much work in philosophy of science, is embedded in a technical formalism, often elegant, but inapplicable to any number of different contexts of inquiry. The difficulties of specifying appropriate constraints on assignments of probabilities and utilities are all too well known. Methodology does better by seeking informal canons of good judgment.

That, I hope, clears the decks for approaching the kinds of historical episodes I have very briefly described, with an eye to eliciting characterizations of the problems and solutions through which mathematics expanded from Babylon to late-nineteenth-century Jena.

2.5 Arts of Significant Extension

Here are some very obvious features of those episodes. Start with Hamilton's search for quaternions. It's clearly motivated by the urge to generalize. Bombelli tentatively proposed to extend arithmetic to encompass new numbers, and his successors eventually recognized the fertility of doing so. Recognizing that complex numbers can be identified with ordered pairs of reals, Hamilton sees Bombelli's initial move as extending the arithmetic operations to cover ordered pairs of reals. Is further extension possible? Can you do it for ordered triples? No. For ordered quadruples? Yes, but it takes some effort, and the abandonment of a principle that holds for multiplying real numbers and complex numbers alike: $xy = yx$. Commutativity has to go. But there's a systematic relation that obtains when commutativity lapses: for some of Hamilton's new numbers, $xy = – yx$.

Let's stop for a moment and ask why Hamilton judged the case of triples to be insoluble. The answer: familiar constraints on the arithmetical operations would have to be amended, without offering any regular way of doing so. Extending the system of mathematics with which you are working to introduce expressions that satisfy a previously unsatisfiable requirement can typically be done, but, most of the time, it leads nowhere. A very simple example will make the point. Imagine yourself before the introduction of negative numbers. As things now stand, $m – n$ is undefined when $m < n$. I am an ambitious young mathematician, eager to make my mark. I decide to introduce a single new expression—'N' to denote The Negative (I have Heideggerian sympathies). For any values of m and n such that $m < n$, $m – n = N$. Have I succeeded in my goal?

"No," you reply, pointing out that I have now trivialized arithmetic by making all numbers identical. $1 – 2 = N = 1 – 3$. Switching terms with change of sign, $1 = 1 + 2 – 3 = 0$, and we're now off to the races. I've anticipated this rejoinder, and point out that standard arithmetic practices are not allowed in equations where N figures. So my extension preserves all of positive whole number arithmetic (Kitcher, 1983, p. 208).

I hope you find this response exasperating. You would be entirely justified in deriding my proposal on the grounds that it goes nowhere. Let's not worry for the present about what "going nowhere" means, but simply refer to extensions that go nowhere as "dumb extensions." Hamilton judges that all ways of defining the arithmetical operations for triplet numbers are dumb extensions, but that abandoning commutativity for quaternions leads to an extension that isn't dumb.

When Bombelli originally announced his proposals for the arithmetic of complex numbers, he worried that he was proposing a dumb extension—that's what lies behind his description of them as "subtle and useless." Just modesty? I think not. Operating in the context of a method for solving cubic equations, assumed to be fully general, he wanted to be able to extract cube roots of complex numbers—to specify the value of $(a + bi)^{1/3}$ and $(a – bi)^{1/3}$ for any values of a and b, without guessing in advance the roots of the equation to be solved. In effect, he's recognizing his extension of the arithmetic operations to enable the supposed algorithm to work helps in just the cases where you don't need it. That's one specific way in which a candidate extension can "go nowhere."

By the time Euler celebrates the "beautiful identity", complex numbers have been embedded in a number of different contexts, and, most pertinently, they expose a connection between exponential functions and trigonometric functions—thus fostering the definition of the hyperbolic analogues of the trigonometric functions (that then turn out to have interesting physical applications—e.g. in studying hanging chains). The "beautiful identity" condenses all this, the aesthetic tribute fully deserved by the fact that when three interesting (and mysterious) numbers are connected in a mysterious way (raising numbers to powers starts with squares and cubes—who'd have taken raising the base of natural logarithms to the power $i\pi$ to be a paradigm for exponentiation?) you get an everyday negative integer.

Underlying Hamilton's judgment, then, is a long history. Practical problems lead Babylonians to investigate equations, and inspire a search for methods to solve them. They succeed with two important classes—simple first-degree equations and quadratic equations—even though they lack any perspicuous notation for making the methods explicit. Few cubic equations have the same practical payoff, but the game of solving them is fun to play (for mathematicians) and apparently fun to watch (the aristocratic audiences), and Cardano publishes (plagiarizes?) a method. Difficulties in understanding that method in a whole range of instances provoke Bombelli to generalize real arithmetic. As that generalization proves fruitful in a wide number of unanticipated contexts, "imaginary" numbers become accepted. Hamilton tries to generalize further. The three-dimensional attempt forces him to modify without providing any systematic understanding of multiplication: the extension is unlikely to bear much fruit. On the other hand, the multiplication table for quaternions offers a pleasing symmetry. In the instances where commutativity fails, we find anti-commutativity (ij = k; ji = −k). The algebra for these "numbers" appears worth exploring further.

	1	i	j	k
1	1	i	j	k
i	i	-1	k	-j
j	j	-k	-1	i
k	k	j	-i	-1

Hamilton's reasonableness consists in his emulation of a mathematical practice that has proved useful in the past. Pragmatic concerns enter into the judgments of all the major characters in this story. Tartaglia and Cardano play mathematical games, because they and their fellows enjoy the games, the games are harmless, and outsiders find them entertaining. Bombelli's modesty is grounded in recognizing that his extension won't do the work for which he undertook it. Euler's enthusiasm rests on seeing that the extension is useful for all sorts of questions that interest mathematicians, that some of the answers to those questions can play a role in investigations of natural phenomena, and that it provides aesthetic satisfaction (Edna St. Vincent Millay got it wrong: Euler as well as Euclid "has looked on Beauty bare.") Hamilton sees his own enterprise as potentially having all these virtues—and, despite the fact that we no longer think of quaternions as special numbers, history has justified his confidence.

Sometimes, the grounds for supposing an extension to be worthwhile are more straightforward than in the history I have reviewed. Consider the streamlining afforded by introducing Arabic numerals, or by Viète's notation. In the first instance, the change eases the daily work of all those who do arithmetic, whether they are mathematicians pursuing projects of no obvious practical significance or are accountants or shopkeepers or engineers. In the second, the benefits for extra-mathematical practice are less evident, but the new notation helps anyone who has to solve an algebraic equation, and, within mathematics, it aids the search for methods for solving the quartic (and, until Galois, ventures beyond.) Moreover, after Viète, the power of this style of notation is revealed in Descartes's coordinate geometry, and, even more spectacularly, in the emergence of the calculus, and in its scientific payoffs: first, in Newtonian dynamics, and then through the growing incorporation of differential equations in physics.

When mathematicians provide tools for addressing scientific problems, their discipline can no longer be disparaged as mere game-playing. It's no accident that Newton's work is done in the middle of a century during which the status of the mathematician is dramatically elevated. A Galileo active in the mid-eighteenth century would not have been so anxious to be known as a philosopher rather than as a mathematician. Moreover, Newton's paradigmatic achievement is bracketed by two other practically fruitful expansions of mathematics. Pascal considers how to divide up the antecedent stakes in unfinished games of chance, and establishes the theory of probability as a new mathematical discipline. Euler muses on the difficulty of traversing the bridges of Königsberg without retracing your steps, and takes the first steps towards topology. In all three instances, new mathematics is, we might say, *purpose built*, growing out of efforts to tackle practical problems outsiders can recognize.[3]

The extent to which pragmatic goals dominate can be appreciated by considering the career of the calculus from the 1680s to the late nineteenth century. For a very long time, the obvious difficulties with the methods practitioners employ—obvious enough to be lucidly pointed out by an Anglican bishop—were mostly ignored. For Newton and Leibniz, both dead by the time Berkeley's *Analyst* was published in 1734, the characterization of the central method would not have been news: both knew they were treating small quantities sometimes as positive and sometimes as zero. Yet, as even Berkeley acknowledged, the calculus was immensely successful. The successes prompted *one* of the major traditions of eighteenth century analysis simply to go on and to pile up results. Indeed, to couple the central method to another dubious technique—substituting freely in infinite series, oblivious to the odd results that sometimes emerged. After all, mathematicians knew enough to toss aside the "result" that.

$$-1 = 1 + 2 + 4 + 8 + \dots$$

[3] As the referee helpfully pointed out, Pascal and Euler seem to have clear goals: they want a mathematical apparatus for settling questions that arise for them. These are short-term goals, "ends-in-view". They do not define any long-term goal for mathematics (e.g. the complete mapping of mathematical reality), but are set by the problems arising in a particular historical context. So, we can see their pragmatic progress—solving a particular problem—in teleological terms, without supposing that the overall progress of mathematics is teleological.