Preface

It is the task of the historian to give back to the past its sense of the future.

—unattributed quote.

This book is based on a course on the history of mathematical analysis that I taught for four years to third- and fourth-year undergraduates at the University of Warwick from 2005 to 2008. It concentrates on the history of topics in analysis, starting with three topics that were active around 1800 and it stops just short of Lebesgue's introduction of measure theory.

It is a historical approach, concerned with how ideas came about, what problems people were addressing, why they thought these ideas would help, and what they made of what they discovered. Very often, the mathematicians were surprised and puzzled too. The thread to follow is what interested them, not to get lost in the details of mathematics, which can indeed be unclear. That said, many of these topics are the staple ingredients of courses in mathematical analysis. Much of this material in this book illuminates this classical analysis by showing what can go wrong and so motivates the need for carefully worded theorems. But such examples should not be regarded as unwelcome guests at the feast, traps for the unwary, and certainly not as merely providing 'catch questions' for problem sheets and exams. Rather, they are examples of what can happen, clues to what else is to be discovered, stages on the endless path to giving a correct and comprehensive account of a subject. And, as this book hopes to make clear, a certain degree of insight is needed to appreciate which are the important ones, the fruitful examples to pursue, and we shall see that mathematicians have legitimately differed about this.

While giving the course I was frequently reminded of one of the most striking problems involved in teaching mathematics: indicating how valuable new mathematics is possible and actually gets done. A student in any of the sciences may easily enter university with some idea of what is happening in research in their subject, but despite the growing numbers of very welcome popularisations of mathematics in recent years it remains almost impossible to bring, shall we say, any of the Clay Problems to an audience of beginners (with the possible exception of the P versus NP problem). The problem is especially acute with the purer and less utilitarian branches of mathematics.

It is my belief that working mathematicians assimilate a certain body of knowledge and make it their own. They know how to use it to tackle certain problems, but they also have an idea of what they would like to know but do not. With some further work they may form a programme for getting there, conjecture a useful interim result, discover a misunderstanding, even a mistake in what they thought they knew. The attempt to obtain new results shapes their sense of what they know and what they would like to discover: a lemma, an estimate, an example, a counter-example, perhaps even the long sought-for result. I have tried in this book to convey my sense of how the analysts of the nineteenth century found their ways, imperfectly, from what they knew to what they wanted to know.

I found it curiously helpful to use Maple to generate pictures of functions. These graphs are not proofs, they may well be misleading, but they are suggestive, and they provide a way of looking carefully at the functions themselves. I hope they form a bridge between the student's understanding and that of the well-educated mathematician of more than a hundred years ago.

I have not respected the modern distinction between real and complex analysis for the simple reason that it did not operate in the nineteenth century. Indeed, as will be explained, neglecting it proved to be one of the sources of one of the finest discoveries of real analysis: functions that are continuous on an interval and differentiable nowhere (see Sect. 20.4).

It is my hope that this book will be part of a four-volume series, the first volume of which, *Worlds out of Nothing* (2011), covers topics in the history of geometry in the nineteenth century, the third, now almost completed, will be on algebra in the same period, and the final volume of which will be on the history of differential equations. Like the first book, this was originally a course of 30 lectures, structured around three assessment points. I do not want to repeat the advice about writing essays that I gave in the earlier book, although I stand by it, but I have taken the opportunity to treat three chapters as an opportunity not only to look back at what has been discussed but to think about how it can be described and to address the student more directly. The point at issue is the difference between a list of facts and a good essay. A good essay is an argument; it expresses a point of view which it supports with evidence. It is attentive to other points of view, refuting them as necessary. It is an attempt to persuade.

There can be many different but equally good answers to an essay question. This depends partly on who forms the intended audience, partly on the author's sensibilities, partly on the space and time available. As a teacher I favour the short essay because it is hard: it is difficult to select the most important items, relatively easy to reply with 'everything'. It is, of course, difficult as a teacher not to reward students who uncritically but astutely follow the 'party line', especially when, as here, the author has strong opinions. To that end students should be encouraged to track down other books in their university library, and I have made some suggestions as this book proceeds.

A problem with teaching the history of the mathematics of the nineteenth century to an English-speaking audience is that so little was written in English. In an Appendix I have given my own translations of extracts from a paper by Dirichlet on Fourier series, papers by Riemann introducing complex function theory and the Riemann integral, and the paper by Schwarz where he mapped a circle analytically onto a square. An extract from the English translation of Fourier's account of Fourier series is also supplied. A small number of other sources are now available in English including the collected works of Riemann, which is very welcome.

The Structure of This Book

The first three chapters set the scene by describing Lagrange's attempt to provide rigorous foundations for the calculus, Fourier's introduction of the series now named after him, and Legendre's work on elliptic integrals. All these topics are sources for much of the subsequent development, although only the second is likely to seem familiar. But Lagrange's ideas set the scene for Cauchy's, and the topic of elliptic integrals is not hard, was obviously important by the standards of 1800, and has surprising later ramifications.

The scene being set we now meet Cauchy, with two chapters on his work on real analysis and one on the much more confused state of his work in the 1820s and 1830s on complex analysis. The marked difference in his understanding was mirrored by the circumstances of publication, with important implications for decades. Three chapters follow on how Abel, Jacobi, and Gauss made complex elliptic functions out of real elliptic integrals, which I argue was one of the most important sources of subsequent interest in complex analysis (and more visible than Cauchy's work). Then come two chapters bringing elliptic functions and complex functions together, and we can pause to take stock.

The next part of the book picks up the topic of potential theory and the contributions of Gauss, Green, and Dirichlet, before turning to the topic of Dirichlet's work on Fourier series and Riemann's work on trigonometric series. Then come several chapters on the rival developments of complex analysis by Riemann and his successors on the one hand and Weierstrass and his supporters on the other. The lecture course included a chapter on minimal surfaces at this point, as an illustration of how new subjects in mathematics sell themselves by the unsuspected but powerful connections they make with other branches of the subject, but I have decided that that story is better placed in the planned history of differential equations.[1]

After a further pause for taking stock the book turns to topics in the real analysis of the later nineteenth century: uniform convergence and what lies beyond, what

[1]For a much fuller history of the study of minimal surfaces, see the forthcoming (Gray and Micallef).

were called ‘assumptionless functions’ and their strange properties, with the questions they raise about the nature of point sets and the properties of the integral, culminating in the collapse of the fundamental theorem of the calculus. Then comes the explicit construction of the real numbers and a look at the first proofs of the implicit function theorem (this replaces a chapter on the first rigorous methods in potential theory, which I have moved to the projected volume on the history of differential equations). The final three chapters before the last revision chapter look forward and deliberately look at topics that were not by any means resolved by 1900: paradoxes to do with length and area and the behaviour of the integral; the Cantorian theory of infinite sets; and some topics where topology finally surfaces.

Acknowledgements

In preparing this course and writing this book I made considerable use of three books: Bottazzini’s *The Higher Calculus* (1986); Bressoud’s *A radical approach to real analysis* (1994); and Hawkins’ *Lebesgue’s Theory of Integration; its Origins and Development* (1999). This blanket acknowledgement is intended to obviate the need for a dense set of footnotes throughout this book. Anyone wanting to know more about the history of analysis should take up these books as a matter of course. I had intended to augment the course by including measure theory, basing my account on the two long papers that Lebesgue himself wrote that provide a very sympathetic, and historical, introduction to the subject. Happily, that task has been carried out admirably by Bressoud in his book *A radical approach to Lebesgue’s theory of integration* (2008).

The material on the history of complex function theory largely comes from joint work with Umberto Bottazzini, *Hidden Harmony—Geometric Fantasies; The rise of complex function theory* (2013). But to put it that way understates the debt I owe to him. In the course of the many years we worked on that book I learned a great deal from him, not only, but obviously, about Cauchy, Weierstrass, and every aspect of the history of complex function theory, but about doing the history of mathematics . His influence is visible, to me, on every page—Umberto, thank you!

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