Introduction

This book is a quest to understand the transition from the traditional algebra of equation-solving in the sixteenth and seventeenth centuries to the emergence of 'modern' or 'abstract' algebra in the mid nineteenth century. The former was encapsulated in Girolamo Cardano's *Artis magnae, sive, de regulis algebraicis* (*Of the great art, or, on the rules of algebra*), a book commonly known then and now as the *Ars magna*, in 1545. The latter developed out of ideas inspired to a great extent by a seminal paper written by Joseph-Louis Lagrange in the early 1770s, his 'Réflexions sur la résolution algébrique des équations' ('Reflections on the algebraic solution of equations').[1] But what of the two centuries between? When Lagrange embarked on his lengthy 'Réflexions' in the autumn of 1771 he wrote:[2]

> *A l'égard de la résolution des équations litérales, on n'est gueres plus avancé qu'on ne l'étoit du tems du Cardan qui le premier a publié celle des équations du troisieme & du quatrieme degré.*
>
> With regard to the solution of literal equations, we are hardly any more advanced than at the time of Cardano, who was the first to publish that of equations of third and fourth degree.

Most of what follows in this book is essentially an investigation of that claim.

In one sense Lagrange was right: Cardano in 1545 had published rules for solving cubic and quartic equations. Although later writers had added several clarifications and refinements, none had succeeded in working out better or more generally applicable methods. As for fifth or higher degree equations, there was no reason to suppose that they would not in the end yield to similar solution algorithms but, except in a few special cases, there had been no progress in finding them.

In another sense, Lagrange was wrong. There had been many advances in equation-solving since the time of Cardano, some of them small and isolated, others of major significance. In the sixteenth century there had been no general 'theory of equations', only a collection of piecemeal methods and results. By the eighteenth century, however, and in particular during the 1760s, it could finally be said that a theory was beginning to emerge. This was a trend that Lagrange himself, with his keen sense of the history of mathematical thought, both recognized and confirmed in his 'Réflexions'. By examining in depth the writings of his predecessors Lagrange was able not only to generalize old results but to discover new approaches, and to establish the theory on fresh foundations. By the end of his lengthy investigation he was able to write something that to Cardano would surely have seemed inconceivable: that the theory of solving equations reduced to a calculus of combinations, or permutations, of their roots:[3]

[1] Cardano 1545; Lagrange (1770) [1772] and (1771) [1773]. For the double dating system used for articles cited in this book see the note in the bibliography.

[2] Lagrange (1770) [1772], 135.

[3] Lagrange (1771) [1773], 235.

> *Voilà, si je ne me trompe, les vrais principes de la résolution des équations, & l'analyse la plus propre á y conduire; tout se réduit, comme l'on voit, à une espece de calcul des combinations.*
>
> Here, if I am not mistaken, are the true principles of solving equations, and the most correct analysis to lead there; all of which reduces, as one sees, to a kind of calculus of combinations.

The hitherto untold story of the slow and halting journey from Cardano's solution recipes to Lagrange's sophisticated considerations of permutations and functions of the roots of equations is the theme of this present book.

As Lagrange was the first to acknowledge, his ideas rested on work that had been carried out by a number of people during the preceding two centuries. Nevertheless, later writers have continued to perceive the hundred and twenty years before Lagrange as an unfortunate gap in the history of algebra, a period during which little of any importance happened. Luboš Nový, for example, in his *Origins of modern algebra* (1973) recognized Descartes as a major figure but deemed him to have few successors:[4]

> From the propagation of Descartes' algebraic knowledge up to the publication of the important works of Lagrange, Vandermonde and Waring in the years 1770–1, the evolution of algebra was, at first glance, hardly dramatic and one would seek in vain for great and significant works of science and substantial changes.

A few lines later Nový qualified this statement by allowing that over this period algebra gained a new status as the 'language of mathematics', but he nevertheless continued to disregard specific changes or achievements.

Nový can be excused to some extent because the main focus of his text was algebra from a later period, 1770 to 1870. The same cannot be said of B L van der Waerden whose *A history of algebra from al-Khwārizmī to Emmy Noether* (1980) was supposed to offer a complete history of the subject, yet he jumped from Descartes in 1637 straight to Waring, Vandermonde, and Lagrange in the 1770s in the turn of a page, without even a nod towards the lost time in between.[5] Similarly Morris Kline in his 1200-page *Mathematical thought from ancient to modern times* (1972) presented his version of the theory of equations in the seventeenth century in a little under three pages, and in the eighteenth century before Lagrange in just one.[6]

More recently, Isabella Bashmakova and Galina Smirnova in *The beginnings and evolution of algebra* (2000) identified the creation of the theory of equations in the seventeenth and eighteenth centuries as one of the five key stages in the development of algebra, but devoted no more than half a dozen pages to the entire period from Descartes up to Lagrange.[7] Further, Bashmakova and Smirnova, like Kline before

[4] Nový 1973, 23.
[5] Van der Waerden 1980, 75–76.
[6] Kline 1972, I, 270–272; II, 600.
[7] Bashmakova and Smirnova 2000, 94–98, 100–102.

them, present disjointed results from Viète, Descartes, or Euler without any connecting historical or mathematical threads, so that we see only empty spaces between them.

Popular textbooks and general histories have tended to follow much the same pattern.[8] Meanwhile a recent spate of books on the origins of group theory offer similarly brief and somewhat random accounts of progress after Cardano but before Lagrange. Perhaps the most succinct statement comes from Mark Ronan: 'After these successes with equations of degrees 3 and 4, the development stopped.'[9]

This assertion from Ronan, like that from Nový quoted above, betrays a view that mathematics somehow progresses only by means of 'great and significant works' and 'substantial changes'. Fortunately, the truth is far more subtle and far more interesting: mathematics is the result of a cumulative endeavour to which many people have contributed, and not only through their successes but through half-formed thoughts, tentative proposals, partially worked solutions, and even outright failure. No part of mathematics came to birth in the form that it now appears in a modern textbook: mathematical creativity can be slow, sometimes messy, often frustrating.

This book attempts to capture something of the reality of mathematical invention by inviting the reader to follow as closely as possible in the footsteps of the writers themselves. That is to say, the reader is encouraged to put aside modern preconceptions and to approach the problems addressed in this book in the same spirit as the original authors, in the same mathematical language, and with the assumptions, and techniques that were then available. To a modern mathematician, trained to set up careful definitions and rigorous proofs, this may seem somewhat frustrating. The purpose of this book, however, is not to account for modern theory by recourse to historical material, but rather to work from the other direction, to understand how and in what form new ideas began to emerge, by following the historical threads that led to them, without either the benefits or prejudices of hindsight. Inevitably, of course, the ideas and themes we choose to focus on are likely to be those that we know to have been significant later, but the aim is to see them first and foremost from the perspective of their own time.

Internalizing the language, assumptions, and techniques of seventeenth- or eighteenth-century mathematical writers is not easy without immersion in mathematical texts of the period. To help the reader appreciate earlier styles of writing, notation has been left intact as far as possible; where it has been modernized for ease of understanding the original version is given in footnotes. Similarly, where sixteenth-century mathematical Latin has been translated into modern English, the original text is provided for comparison so that readers can see for themselves how much has been lost or gained in translation. On the whole this has not been done for eighteenth-century Latin or French, which in general translate fairly smoothly into English, except where particular words or phrases carry a force of meaning in the original that does not come over well in translation.

[8] See, for example, Struik 1954, 114–116, 134; Stillwell 1989, 93–96; Katz 2009, 404–414, 468–473, 671–673.

[9] Ronan 2006, 19; see also Livio 2005, 79–83; Derbyshire 2006, 81–108; Stewart 2007, 75. Du Sautoy 2008 has no references at all for this period.

Assumptions made by past writers can be hard to identify because they were so often just the mathematical common knowledge of the day. Hardly any of the authors featured in this book, for instance, ever specified what numbers could or could not be used as coefficients of equations. At a time when all teaching on equations relied on worked examples, equations were usually given easy integer coefficients, but that does not mean the methods or results were not thought to apply more generally. For Cardano, whose arithmetical world contained integers, fractions, and surds, we can deduce from certain of his statements that he assumed the coefficients of his polynomials to be integers or fractions only, but he never actually said so. After more general notation had been introduced, a distinction was made between 'numerical' and 'literal' coefficients, but still without specifying what kind of numbers the literal coefficients might represent. Such silence persisted into the eighteenth century, by which time literal coefficients could stand not only for numbers but for other algebraic expressions; one usually knows what was intended only from the particular context. It is probably safe to say that where the coefficients stood for numbers, those numbers were, as in Cardano's day, thought to be integers or rationals but in any case they were certainly real: there was no hint of complex coefficients in the eighteenth-century literature on equation-solving.

As for techniques, the modern reader will undoubtedly frequently see shortcuts and better notation that would save many pages of tedious writing. It is a little puzzling, for example, that Lagrange never resorted to some kind of subscript notation instead of running so many times through the alphabet. It is worth recalling, however, that when everything had to be laboriously written or copied by hand there can have been little time for re-writing, correcting, or polishing. In any case, we are not here to mark authors' work with 'could have done better' but to follow what they actually did. I have attempted to point out errors where they invalidate a result that at the time was thought to have been proved, or where they are likely to hinder the reader's understanding, but for the most part the mathematics has been presented in the way it was originally written.

This book is in three parts. Part I offers an overview in three chapters of the period from Cardano (1545) to Newton (1707); here the material is presented chronologically, with explanatory commentary either where the ideas are somewhat obscure in the original (as for Cardano and Viète) or where they are little known (as for Harriot). Part II covers the period from Newton (1707) to Lagrange (early 1770s); by now developments in equation-solving emerged not from relatively isolated texts following one another at irregular intervals, but from a number of different strands of thought which from time to time disappeared or resurfaced, and which often overlapped with each other. For this reason Part II has been arranged by themes, which though roughly chronological in their ordering are not strictly so. Part III is a short account of the dissemination and aftermath of the discoveries made in the 1770s.

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