(1)–(4), the editor omits the details.)

Aage Bondesen showed that for $p + q \leqslant 1$ the inradius of the triangle with vertices $(0,0), (p,1), (1,q)$ is less than that of the triangle with vertices $(0,0)$, $(1,0)$ and $(p+q,1)$. Jordi Dou investigated the case in which a rectangle replaces the square. Victor Pambuccian points out that this problem is automatically solvable by the Tarski algorithm (see, e.g., A. Seidenberg, *A new decision method for elementary algebra*, Ann. Math., 60 (1954) 366–369). On the other hand, the editor wishes to add that (a) problems at this level or higher have almost never been thus solved in real time, (b) there seems almost no hope that the algorithm will produce proofs with intuitive appeal (at least for humans), and (c) the algorithm cannot, even in principle, generalize results to $n$ dimensions.

Also solved by Aage Bondesen (Denmark), Jordi Dou (Spain), and Esther Szekeres (Australia).

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## REVIEWS

EDITED BY ALLAN L. EDMONDS AND JOHN H. EWING
COLLABORATING EDITOR FOR FILMS: SEYMOUR SCHUSTER

### *Galois Theory*. By Harold M. Edwards. Springer-Verlag, New York, 1984, xii + 152 pp.

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Galois was a Frenchman who made deep and important contributions to mathematics at the age of 17, who nevertheless failed the entrance examinations for the École Polytechnique, who was imprisoned at the age of 19 for political activities, and who died aged 20 in 1832, shot in an early morning duel. So romantic a figure has naturally attracted a great deal of posthumous attention. Biographers have done their best, but a person who dies young does not leave masses of documentary evidence behind and most of what has been written about Galois the man is more journalistic than historical; there are many theories but few facts. For different reasons Galois the mathematician is hardly better understood. His discoveries are so advanced that they are far beyond the mathematical competence of trained historians; and few mathematicians have the training in historical techniques or the taste for historical research that a study of his mathematics would require.

Galois theory, which is what the theory of equations was changed into by Galois' work, is one of the three great contributions that he made to mathematics. The other two were the theory of 'Galois imaginaries', which is, in essence, the same as the modern theory of finite fields, and the theory of groups, which emerged from his theory of equations and from Cauchy's papers of 1845 on the theory of substitutions. The theory of 'Galois imaginaries' was published when Galois was 18 years old in his paper 'Sur la théorie des nombres'. His work on theory of equations was first submitted to the Academy in 1829. It was either lost or withdrawn and a new paper was submitted in February 1830 for the *grand prix de mathématiques*. This second version was lost amongst the papers of Fourier. A third version entitled 'Mémoire sur les conditions de résolubilité des équations par radicaux' was submitted to the Academy in January 1831, was read by Poisson (and possibly also Lacroix), was rejected in July that year and returned to its author. This is the manuscript, the so-called *premier mémoire*, that was retrieved by his friend Auguste Chevalier after Galois' death, that Liouville first published in 1846, and which contains the original exposition of Galois theory.

At the beginning of the last century the central problem of the theory of equations was to find a formula that would express a root of a polynomial equation

$$x^n + a_1 x^{n-1} + \cdots + a_{n-2}x^2 + a_{n-1}x + a_n = 0$$

in terms of the coefficients $a_1, \ldots, a_{n-2}, a_{n-1}, a_n$. It was hoped, and indeed required, that such a formula would be algebraic: that is to say, it would involve no operations other than $+$, $-$, $\times$, $\div$, and $\sqrt[k]{\phantom{x}}$ for various values of $k$. What was wanted were analogues of the formulae

$$x = -a_1 \quad \text{and} \quad x = \frac{1}{2}\left(-a_1 \pm \sqrt{a_1^2 - 4a_2}\right)$$

that solve the problem in case $n = 1$ or $n = 2$, respectively. For $n = 3$ and $n = 4$ such solutions had been known for over 250 years. In 1770–71 Lagrange appears to have come round to the idea that it might, just possibly, be the case that no such formula exists when $n \geqslant 5$. From 1799 to 1814 Paolo Ruffini, an Italian *savant*, published books and papers describing his proof of this impossibility, but his work was long, confused and confusing, and it had little influence on his contemporaries and successors. There were still many mathematicians who believed that the desired formulae should exist. Indeed both Abel in 1823 and Galois in 1828 believed that they had solved the quintic equation before they found their own errors and went on to elucidate the real state of affairs.

What Abel did was to prove in 1824 that there is no formula of the required kind for equations of degree 5. Later he amplified his proof and extended it to cover explicitly equations of any degree $n \geqslant 5$. It is a common myth that Abel did this by proving the simplicity of the alternating groups $A_n$ for $n \geqslant 5$. He did not: the idea of a group did not yet exist and Abel did not invent it. There was no group theory needed for Abel's proof. What he used was a certain fact about the possibilities for the number of different functions that can be obtained from a given function of $n$ variables by permuting them amongst themselves; and this fact, although it has a natural and obvious group-theoretical setting, had been proved by Cauchy, in a paper published in 1815, by simple calculations with permutations.

It was left to Galois, almost certainly in 1829, to discover the need for groups in the theory of equations (though Galois himself was later aware that Abel, who died in 1829, had probably been thinking along similar lines about a year earlier). In the *premier mémoire* he treats equations with numerical coefficients and addresses a much more delicate question than Abel had answered. The question is, can a solution of such an equation be obtained by computing with known quantities, using addition, subtraction, multiplication, division and extraction of roots as the only allowable arithmetical operations; that is to say, does there exist a solution in terms of radicals? The answer is sometimes yes (as in the case of the equation $x^5 - 2 = 0$), and sometimes no (if by "known quantities" we mean rational numbers, then the equation $x^5 - 4x + 2 = 0$ is an example). Galois discovered that there is a group naturally attached to each equation, and he was able to analyse how that group changed when the domain of known quantities was extended and to produce a necessary and sufficient condition, expressed in group-theoretic terms, for the solubility of the given equation by radicals.

The *premier mémoire* has never been an easy paper to understand. It defeated Poisson in 1831. And the early exegetes, Liouville, Betti, Serret,..., had, in effect, to re-work the theory for themselves, though of course following (and acknowledging) the very explicit indications that Galois gives in his sequence of lemmas and propositions. The difficulty lies in his exposition. Globally he organises the theory very beautifully and straightforwardly, but locally his explanations show his extraordinary impatience. The whole paper is really no more than a sketch. The idea of a group, for example, is one of Galois' great innovations, but one learns what he has in mind only by reading the proof of his *Proposition I* and working backwards. It is true that at the end of the collection of definitions with which the published versions of the *premier mémoire* begin there is a brief passage setting out his terminology for permutations, substitutions and groups. But the explanation is exiguous in the extreme. Moreover, this is a late insertion that was added on that dreadful night before the fatal duel. A year earlier, when the manuscript had been read by the Academy referees, it had contained no explanation of what a group was supposed to be.

The unfavourable report by those referees, Poisson and Lacroix (though it seems doubtful if Lacroix had much to do with it), may be read in the published *Procès-verbaux* of the *Académie des Sciences*, *Séance du* 4 *Juillet* 1831. Poisson and Lacroix criticise the work on two very solid grounds. One of these is, in general terms, the criticism that I have made above, expressed like this:

> Quoiqu'il en soit, nous avons fait tous nos efforts pour comprendre la démonstration de M. Galois. Ses raisonnements ne sont ni assez clairs, ni assez développés pour que nous ayons pu juger de leur exactitude,... .
>
> [Be that as it may, we have made every effort to understand Mr Galois' proof. His reasoning is neither clear enough nor far enough developed for us to have been able to judge its correctness,... .]

The referees' other criticism is quite different. On the title page of his manuscript Galois had written

> On trouvera ici une condition générale à laquelle satisfait toute équation soluble par radicaux, et qui réciproquement assure leur résolubilité. On en fait l'application seulement aux équations dont le degré est un nombre premier. Voici le théorème donné par notre Analyse:
>
> Pour qu'une équation de degré premier, qui n'a pas de diviseurs commensurables, soit soluble par radicaux, il faut et il suffit que toutes les racines soient des fonctions rationnelles de deux quelconques d'entre elles.
>
> [Here will be found a general condition that is satisfied by all equations soluble by radicals, and which conversely ensures their solubility. Just one application is given, to equations of prime degree. Here is the theorem given by our analysis:
>
> In order that an equation of prime degree, which has no rational divisors, shall be soluble by radicals it is necessary and sufficient that all the roots should be rational functions of any two of them.]

Poisson and Lacroix reacted as follows:

> ... en admettant comme vraie la proposition de M. Galois, on n'en serait guère plus avancé pour savoir si une équation donnée dont le degré est un nombre premier est résolue ou non par des radicaux, puisqu'il faudrait d'abord s'assurer si cette équation est irréductible, et ensuite si l'une de ses racines peut s'exprimer en fonction rationnelle de deux autres. La condition de résolubilité, si elle existe, devrait être un caractère extérieur que l'on pût vérifier à l'inspection des coefficients d'une équation donnée, ou, tout au plus, en résolvant d'autres équations d'un degré moins élevé que celui de la proposée.
>
> [... accepting Mr Galois' proposition as true, one is hardly further forward towards knowing if a given equation whose degree is a prime number is soluble by radicals or not, because one must first decide whether this equation is irreducible and then whether one of its roots may be expressed as a rational function of two others. The condition for solubility, if it exists, should be an external characteristic that one can verify by inspection of the coefficients of the given equation, or at least, by solving other equations of lower degree than the one proposed.]

They concluded their report by advising that, since the author said that his main proposition was part of a more general theory, and since it was often the case that a complete theory was easier to understand than isolated parts of it, one should wait until the whole of the author's work was available before forming a final opinion; but as it then was, the part that had been submitted was not in a suitable state that they could recommend it for the Academy's approval.

With hindsight one may feel that this report was wrong. But I cannot think so: it seems to me to be a model of good refereeing. Can any of us be sure that in an analogous situation today we would react differently? I doubt it: it is an admirable report, sympathetic but firm. All that is wrong with it is that it deals with the work of an exceptionally brilliant and awkward man. Galois had no research supervisor who might have shown him how his discoveries should be properly written up. Besides, Galois was not a man who took advice easily. Another young mathematician might have taken the criticism to heart, re-written his work, published it and become famous. Galois took offence, returned to political agitation, died young and became famous.

After Galois' principal works were published by Liouville in 1846 his theory as it applied to polynomial equations was rapidly understood (understanding of the theory of groups beyond the little that had direct application to the study of equations grew rather more slowly), and it has been taught and learned with enthusiasm and pleasure ever since. Over the years the outward form of Galois theory has changed enormously. At the hands of Dedekind, Emmy Noether, Emil Artin and others the subject has moved away from its very concrete origins in the theory of equations and has metamorphosed into that part of abstract algebra that deals with fields and their automorphism groups. Today's student will find that the *premier mémoire* looks very different from Galois theory as it is to be found in modern texts such as Herstein's *Topics in algebra* (Chapter 5) or Stewart's *Galois theory*.

The book *Galois theory* by Harold Edwards is quite different from these. Like them it is an excellent textbook suitable for advanced undergraduates, but it takes the student back to those first few decades of the nineteenth century and returns to Galois' original conception of the subject. Notation is very similar to that of Lagrange and Galois; division of the text into short sections numbered consecutively is true to the style of that time. The first thirty sections contain an account of what had been done before Galois on cubic, quartic and cyclotomic equations and on symmetric polynomials, with particular reference to the work of Newton, Lagrange and Gauss. The remainder of the book is, in effect, a very much expanded version of the *premier mémoire* put into context and carefully explained. Edwards develops the theory in the form that Galois wrote it except that every lemma and theorem is properly proved, every $i$ is dotted and every $t$ is crossed. Where Galois was impatient and obscure Edwards is extraordinarily patient and clear. Even the question of how Galois groups may be calculated and how in principle, if not in practice, one may decide whether or not a given equation is soluble by radicals, is carefully and extensively treated (by methods due to Kronecker fifty years after Galois' death). This is, at last, a satisfactory and decisive response to the referees' two criticisms of Galois' work.

In the preface Edwards writes that he wanted to explain the theory "in terms close enough to Galois' own to make his memoir accessible to the reader". It is an admirable intention and one in which he will, I believe, be found to have fully succeeded. Nevertheless, it is perhaps a slightly dangerous one. Galois' own exposition is so sketchy that one might complete his arguments in several different ways and one can never be sure quite how much importance he himself attached to this point or that. It is no surprise therefore that I find myself cheerfully disagreeing with Harold Edwards on some details. Here are two examples.

(1) I see no historical justificaion for singling out the concept of "Galois resolvent" and giving it that name. All that Galois uses is the existence of a rational function $t$ of the roots $a, b, c, \dots$ of his polynomial equation $f(x) = 0$ which has the property that each root can be expressed as a function of $t$. (In modern terms $t$ is a generator of the splitting field of $f(x) = 0$ and its existence is guaranteed by the "Theorem of the primitive element." This is what Edwards [p. 35] calls a "resolvent" of the equation $f(x) = 0$; what he calls a "Galois resolvent" is a very special type of "resolvent".) Galois is very offhand in his proof of the existence of such functions; in his paper 'Sur la théorie des nombres' that was published in 1830 he dismisses the existence as being clear.

Furthermore, Galois himself acknowledges that the existence of such functions $t$ was known to Abel before him.

(2) The proof of *Lemme III* is a splendidly controversial matter. Poisson was unable to understand it and made a note on the manuscript to say so, but he accepted that the lemma was true by a result of Lagrange. Galois, incensed, appended "On jugera" to Poisson's note. Edwards feels that Galois was right and he gives a line of argument that undoubtedly completes the proof. But to do this he has to read very much more than is there into what Galois actually wrote, and I find his justification rather far-fetched. On balance I side with Poisson: it was up to Galois to be both clear and correct, whereas what he wrote is far too easily misunderstood.

I have other small criticisms of the book. For example, although it is primarily a contribution to mathematical exposition, not to the history of mathematics, I would have liked to see a paragraph or two about Abel's contributions and his influence (or lack of it) on Galois. Then again, the explanation of what is meant by solubility of cyclotomic equations by radicals is not entirely happy: elsewhere 'solution by radicals' involves using roots of equations $x^p - k = 0$, so why is a root of the equation $x^{p-1} + x^{p-2} + \cdots + x + 1 = 0$ not immediately acceptable as a radical in virtue of the fact that it is a root of $x^p - 1 = 0$? But all that these criticisms prove is that the author is right when he advises his students to 'Read the masters.' The reader must form his own judgment after reading what Galois and Harold Edwards themselves have written. That is one of the many points on which I am in complete agreement with him.

At the end of his famous testamentary letter, written on the night before the duel, Galois commends his manuscripts to Chevalier's care and writes

> il se trouvera, j'espère, des gens qui trouveront leur profit à déchiffrer tout ce gâchis.
>
> [there will, I hope, be people who will find it profitable to decipher all this mess.]

With his latest book Harold Edwards joins the select band of these *gens*. He has added another significant item to the new *genre* of mathematical publication that he created with his two earlier books *The Riemann Zeta Function* and *Fermat's Last Theorem*. Just as Galois' paper '... résolubilité des équations par radicaux' is very aptly named, so *Galois theory* has an unusually accurate title: this is not only a splendid textbook of that subject, but also an excellent contribution to the study of Galois the mathematician.

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*Winning Ways for Your Mathematical Plays, Volumes I & II*. By Elwyn R. Berlekamp, John H. Conway, and Richard K. Guy. Academic Press, New York, 1982. Volume I, xxxi + 426 pp.; Volume II, xxxi + 424 pp.

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**"It's only a game, like dying is only death."—Tom Paxton**

Imagine the following offhand conversation between two erudite mathematicians, Right and Left, at a prestigious institution of higher learning, as they pick up their mail.

Left: "Wow! Look at this! A book that shows you how to win at Dots-and-Boxes!"

Right (somewhat bored): "Splendid. Now you can win against your seven-year-old."

Left (unperturbed, but defiant): "Dots is a subtle game".