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The mathematics of Arthur Cayley with particular reference to linear algebra

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Abstract

The mathematics of Arthur Cayley with particular reference to linear algebra

Anthony James Crilly

This thesis is principally concerned with Arthur Cayley's work on Invariant Theory, but also considers his contribution to matrix algebra and other algebraic systems, drawing on sources including unpublished letters between Cayley and his contemporary, J.J.Sylvester.

The history of modern linear algebra and Cayley's part in its development has been extensively researched in the last decade by Thomas Hawkins. However, little has been written on Cayley's contribution to Invariant Theory, a subject to which he constantly reverted over a period of fifty years. In comparison, his work on Matrix Theory was a minor interest.

The focal points in Cayley's passage through Invariant theory are investigated with reference being made, inter alia, to his correspondence with J.J.Sylvester which affords special insights into both the development of this Theory and the nature of their collaboration. Where appropriate, particulars of Sylvester's own work are given. Biographical details are included where these are believed to be unpublished or otherwise not generally available.

A survey of Cayley's mathematical thought is offered in so far as it may be determined from his scattered remarks.

Cayley pursued his algebraic researches on two distinct levels. First, he absorbed himself in calculation which led him to the combinatorial aspects of Invariant Theory and, secondly, he displayed a remarkable proclivity for systemisation, although this expressed itself in the classification of specific forms rather than in the development of an abstract theory as with the German algebraists.

The basic text contains four chapters on Cayley's work in approximate chronological order followed by a final chapter on his general mathematical thinking. The Appendices include a statistical survey of his work, a bibliography of manuscripts, including, of course, his letters to Sylvester and a number of little known photographs associated with Cayley and his times.

Reference System

The Harvard Reference System is used throughout the text. Thus [Smith, 1853b, 22] refers to page 22 of a work published by Smith in 1853. Details of the publication 1853b can be found in the Bibliography (Appendix D) in a chronological list under Smith's name. Abbreviated forms of this notation are also used. In cases where the authorship is clear [1853b,22] is used or simply [1853b].

Reference to the Collected Mathematical Papers of Cayley and Sylvester occur frequently. The full reference to these works is

[Cayley, 1852a , 40; CP2, 16]

which means reference has been made to Cayley's 1852a, page 40, which can be found in Cayley's Collected Mathematical Papers, volume 2, page 16.

In practice this full form is hardly used; typical abbreviations are:

[1852a; CP2, 16] or even [CP2,16]

Reference to unpublished material described in Appendices B and C is abbreviated by:

[App.B, 3 viii 1882] which refers to the Cayley-Sylvester correspondence and a letter dated 3rd August 1882.

[App.C, Klein, 8 iii 1887] which refers to a letter dated 8 iii 1887 in the Cayley-Klein correspondence listed in Appendix C. Authorship of letters may be made clear in the text in which case the abbreviated [App.C, 8 iii 1887] is used.

Footnote reference numbers appear sequentially throughout each chapter in superscript notation and footnotes are found at the end of each chapter.

Note In his [1852a] Sylvester introduced the terms invariant and covariant. In Chapter 1 these terms have been used anachronistically in place of Cayley's terminology used in the 1840s i.e. 'hyperdeterminant,' 'transforming function', 'derivative'.

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Introduction and early biography

Unlike other major scientific figures of the nineteenth century, no extensive Biography exists for Arthur Cayley, arguably the most influential English pure mathematician of his time. He has achieved the rare distinction of one whose influence permeates a whole subject. His view of mathematics as a total system made it difficult for him to concentrate his efforts in any one branch of the subject, although in modern times he is perhaps best remembered for his pioneering work in Algebra. Many mathematical entities and theorems bear his name.[1]

It is worth remembering that some areas of mathematics to which he gave an initial impetus have proved useful in fields far removed from pure mathematics. The best known example of this is the algebra of matrices. In other directions, physicists note his name in connection with the Cayley-Klein parameters[2], chemists with his work on enumerating isomers and astronomers with lunar theory.

When his algebraic contributions (for instance, Cayley's Theorem in group theory) are included in modern abstract texts, the impression may be conveyed that Cayley was an abstract mathematician. This would not be correct if by the term 'abstract mathematician' is meant one who proceeds from given axioms.Like other mathematicians of the time, he would be more accurately described as a discoverer of mathematical truths, rather than a mathematician proceeding from known starting points with faultless rigour. He lived at a time when algebra was in a state of transition from the algebra of quantity to a new 'symbolic algebra' in which symbols of operation played the central role. But in the new algebra the foundations were far from secure.

Cayley was born on 16th August 1821 at Richmond in Surrey. He was the second son (the eldest died in infancy) in a family of three boys and two girls. The Cayley family was a large and prominent English family[3] with a seat in Brompton in Yorkshire. Cayley's father, not in the direct line to the family title, was a member of the trading firm of Thornton Melville and Cayley which operated in the Baltic. The background of his mother, Maria Doughty, appears to be unknown.

During the early years of Cayley's life, the family lived in

St.Petersburg, in the course of his father's business. When they returned to England in 1829, they settled in the small and quiet village of Blackheath, then in the course of being transformed into one of London's first suburbs. There, Cayley and his brother went to a local private teaching establishment run by a clergyman[4].

At this stage, Cayley already gave signs of a talent for mathematics.[5] It was then observed that 'he had a great liking for numerical calculations and he developed a great aptitude for them' [CP8,x]. This computational dexterity is an important part of Cayley's mathematics and it lasted throughout his life.

In 1835 at the age of fourteen he left the school in Blackheath and entered the Senior Department of King's College, London. This is evidence of Cayley's intellectual ability, for the stipulated entry age of sixteen was usually strictly adhered to and students below this age would normally go into the Junior Department of the College.[6]

At King's, the express purpose of education was to prepare young men either for direct entry into commercial life or for entrance to Oxford or Cambridge [Hearnshaw, 1929a]. The curriculum provided a sound classical and religious education based on the teachings of the Established Church and its students emerged well trained and disciplined. The college had been created a few years earlier as a reaction against the successful University College which was based on utilitarian principles. The principal of King's, Hugh Rose, who later steered Cayley towards a career in mathematics, forcefully believed that religion was the only sound basis for education and pleaded for the 'study of literature as a cultivation of the mind, and of theology as the indispensable nutriment of the soul'.

In his biography of Cayley, Forsyth recalls that Cayley had no serious doubts on religious matters. Cayley was unwilling to take holy orders 'not that there was any religious obstacle in his way, for he was not harassed either by philosophical doubts or critical difficulties. His simple reason for remaining a layman was that, though devout in spirit and an active churchman, he felt no vocation for the sacred office' [CP8, xiii].

King's College was unlike any of the English public schools. It had 'Professors', not schoolmasters, and these Professors carried out research and gave Public Lectures, some of which were very popular. In the teaching of mathematics, King's College was well to the fore. In the public schools, mathematics generally occupied a very minor place in the curriculum, usually amounting to no more than basic training in arithmetic, a little formal algebra and Euclid. At King's the situation appeared to be different. A chair of mathematics was established at the outset and the occupant then, and during Cayley's period at the College, was the Reverend Thomas Hall, a fellow and tutor of Magdalene College, Cambridge, and 5th Wrangler in 1824. He wrote mathematical text books and occupied the chair at King's for almost forty years. Other members of the staff were equally well qualified.

Three years of study at King's College in Cayley's day meant studying a broad curriculum encompassing the classics, natural and experimental philosophy, English literature, modern languages, history and subjects connected with the various professions. While showing his ability in mathematics, Cayley also showed a liking for chemistry (the professor of Chemistry was John F. Daniell F.R.S.), a knowledge he put to use in his later mathematical research.

In 1838 Cayley obtained his qualification (Associate of King's College - a theological qualification) and proceeded to Trinity College, Cambridge, as was the custom for those who wished to read for a degree. For the first year at Cambridge Cayley's tutor was George Peacock (1791 - 1858) one of the founders of and main influence on the English school of symbolical algebra. Even though the tutorial role was not primarily a teaching role, it was widely known that Peacock took his tutorial duties seriously [Rothblatt, 1967a , 197] and it is likely that he had an influence on Cayley's mathematical interests. In 1839, Peacock, who had been Lowndean Professor since 1836, left the University to become Dean of Ely. A substantial influence would have been the popular and successful coach William Hopkins who coached Cayley for the Tripos Examination. At Cambridge, both the Classics and Mathematics played a prominent part in the curriculum. In the third year, for example, Mathematics was particularly encouraged. The subjects included Optics, Hydrostatics, Astronomy, the higher part of Newton's Principia and the geometry of three dimensions [Huber, 1843a, vol. 1, 531] .

Cayley showed an all round ability but it is his interest in analytical geometry that showed itself clearly in several early papers. Second only to the rise of Calculus under the impetus of the

'Analytical Society' at Cambridge, Analytical Geometry enjoyed a wide popularity in the first thirty years of the nineteenth century [Glaisher, 1886a, 19]. There were many texts available and at a level suitable for study by undergraduates.[7] It was in algebraic geometry that Cayley published his first paper [1841a] in the year prior to his graduation. He was something of a 'celebrity' even before the Tripos Examination. William Thomson, then a freshman at the University, wrote to his father in 1841 of a party where he met Cayley 'who is to be Senior Wrangler this year' [Thompson, 1910a, 34]

In his University career, Cayley was eminently successful. He was widely tipped to be Senior Wrangler and he did not disappoint his supporters. With his victory in 1842(see Plate 8)he automatically attained an elevated social position ensuring attention wherever he went. The Times reporter wrote at the foot of the year's honours list:

> Mr.Cayley, the Senior Wrangler, is a mathematician of extraordinary powers, nor are they limited to the particular branch of study in which his present honours have been won, but extend equally to other objects of academical pursuit [Times, 22.i.42, 5]

Throughout his career Cayley never suffered from lack of recognition. It was something that had to be borne, for Cayley by nature was a shy and retiring figure. In later years, when Cayley had reached a prominent position, the recognition reached a proportion of veneration.

When Cayley graduated in 1842, he had ample time to devote to his subject. The single hurdle had been the Tripos Examination, but in the subsequent life of a Victorian Don, there was little pressure. At the beginning he held a minor fellowship at Trinity College, a position of limited remuneration. To supplement his income he took a few private pupils and for leisure went on long walking tours. In the great Victorian tradition, he was a tremendous walker. The scientist, Francis Galton, met Cayley at the time and remembers an occasion when Cayley supervised a reading party in Scotland, shortly after the Tripos Examination in 1842. Remarking on his frail appearance, Galton describes an incident which indicates the individualistic side of Cayley's character:

> One morning he coached us as usual and dined early with us at our usual hour. The next morning he did the same, all just as before, but it afterwards transpired that he had not been to bed at all in the meantime, but had tramped all night through the moors to and about Loch Rannoch. [Galton, 1908a , 72]

Galton greatly admired Cayley and would have regarded him as a prime example of his own theories on creativity - he was zealous to a degree and had a capacity for hard work.

At Cambridge, Cayley found himself in the company of men interested in the furtherance of mathematics and science. An important figure in this respect was D.F.Gregory (1813-44). He was a Fellow of Trinity College and in the course of lecturing and examining at the College he came in contact with Cayley.[8] Gregory was a man of wide scientific interests and in mathematics he was well known for his foundation work in the Calculus of Differential Operations.

Gregory was the first editor of the <u>Cambridge Mathematical Journal</u>, a publication intended to put Cambridge at the forefront of mathematical research in England.[9] His wide scientific view included an interest in Analytic Geometry. In 1842 Gregory had begun writing a text book on geometry titled <u>A Treatise on the application of analysis to solid geometry</u> [1845a] . Its principal object was to develop a system of solid geometry by the use of equations and in a form suitable for the use of students. In preparing the book Gregory used material published in the pages of the <u>Cambridge Mathematical Journal</u>. In its preparation he received help from other mathematicians in Cambridge including Cayley [Gregory, 1845a, 132]. It deals with the application of algebraic expressions and equations in the geometry of three dimensions. In dealing only with quadric surfaces there was no pressing need for Gregory to develop new notation and the equations were presented in their full Cartesian form. One of the objectives of the book was to classify quadric surfaces and to do this using the Cartesian equations for the surfaces.

One important feature of the book is a striving for symmetry of expression in the equations. Not only Gregory, but many writers on analytical geometry in the articles in the early <u>Cambridge Mathematical Journal</u> sought the same symmetry in their equations. This attitude seems likely to have been learned from the earlier French writers on the subject. According to Boole [1840b] the methods in analytical geometry devoid of symmetry were to be avoided. In his [1841a] Cayley showed an allegiance to the symmetric method and later it was considered to be characteristic of his way of doing mathematics. When Gregory became ill, the editorship of the Journal fell to R.L.Ellis, and after Gregory's death in February 1844 Thomson was chosen as editor. Thomson had plans to expand the Journal and in this was supported by Cayley.[10] As Thomson recounts, in a letter [dated 2 vi 1844] to his father:

> of course the two great difficulties will be to get contributors of memoirs, and money enough to defray expenses, but as mathematical study is considerably on the increase, here at least, we are in hopes that in a few years' time we may have succeeded in doing something for the object. If the plan be carried out at all, the great object of course would be to make the journal as general as possible in this country, and to get it made known on the Continent. I have been speaking to Cayley since, and he quite enters into the plan. One great assistance he thinks would be that there is at present no journal of the kind in this country, and that the want is very much felt by mathematical men [Thompson, 1910a, 79]

The Philosophical Magazine and the Proceedings of the Cambridge Philosophical Society catered mainly for physical subjects. Gregory's earlier mathematical journal provided the forum for Cayley's initial papers and it is the first of his papers in this journal which is next considered.

Preface

References

1. To mention some of the best known instances: The Cayley-Hamilton theorem in linear algebra has been extensively studied both in this and the last century. It was given by Cayley in his 1858 memoir on matrices [1858a; CP2, 475]. Cayley's theorem in group theory which some mathematicians judge only to be of lesser importance than Lagrange's theorem and Sylow's theorems. It is stated by Cayley in [1878d; CP10, 403].
One theorem for which he was known in the nineteenth century but which has since fallen into obscurity is Cayley's Theorem on Pfaffians (A skew symmetric determinant of even order is the square of a Pfaffian)[1848a ; CP1, 412]. Another important theorem which has suffered the same fate is Cayley's Law for calculating the number of linearly independent covariants of a binary form [1856a; CP2,256].

2. Four parameters that specify a body's orientation. Cayley's name is linked to these parameters because he showed that quaternions (representable as a unitary second order matrix with unit determinant with the four parameters as entries) could be used to represent a rotation of axes [1845c; CP1, 123].

3. Its genealogy can be found in [Foster, 1875a]. Members of the family traditionally entered the professions: Banking, the Law, Medicine and the Church. The only other member to become a Fellow of the Royal Society appears to be Sir George Cayley (1773-1859) the sixth Baronet and pioneer aviator.

4. The school is at Eliot Place, Blackheath. It was run by the Reverend George Potticary.

5. A namesake and distant relative, Arthur Cayley, graduated as Fourth Wrangler in the Cambridge Tripos in 1796.
Cayley's brother, Charles Bagot Cayley (1823-1883) was not mathematical by inclination. At King's College, London, he came under the influence of Gabriele Rossetti, then Professor of Italian and father of Dante and Christina Rossetti. From him he gained a life long interest in Dante and Italian literature. He became one of the Rossetti family inner circle and led a quiet life as a scholar and

Ref. 5 continued

philologist. In 1866 he proposed marriage to Christina but was refused. [Packer, 1963a, 66].

6. The Senior Department of King's College in the 1830s was the forerunner of the faculties of Arts and Natural Science in today's King's College, London. King's College did not give degrees but prepared people for entry to Universities. The Junior Department later became King's College School, a separate institution.
7. The first book published in England exclusively devoted to analytical geometry was Lardner's book on Algebraic Geometry [1823a], although the ever popular general text by Dean Wood, (which first appeared in 1795) contained a chapter on the application of algebra to geometry. Other texts on the same subject were produced, including a text by John Hymers [1830a]. But the text which became the standard work for many years was Hymers' Conic Sections [1837a] published a short time before Cayley entered Trinity College to begin his studies. No doubt these texts posed little difficulty for Cayley but their existence emphasizes the importance with which the subject was regarded as part of the mathematical training in the Cambridge of the day.
8. Gregory was Moderator in the Tripos in 1842.
9. The setting up of the Cambridge Mathematical Journal is described in [Thomson, 1874a] and [Sharlin, 1979a].
10. Cayley helped Thomson in his early career and introduced him to the French mathematicians. He supplied a testimonial for Thomson in support of his successful application for the Chair at Glasgow in 1846. They were to remain lifelong friends. See Chapter 5 for Kelvin's thoughts on the occasion of Cayley's death.

Chapter 1

Early Years (1840 - 1849)

1.1. Introduction

The 1840s was a revolutionary period in the development of Algebra. After Hamilton's discovery of the quaternions, a number of symbolical algebras were discovered and in these developments Cayley took a definite interest. However, it is the other side of linear algebra which will be investigated in greater detail in this dissertation. That is, Cayley's interest in algebraic forms and determinants. Cayley's first paper [1841a] combines the theory of determinants with a problem in geometry. But of course the two papers on 'hyperdeterminants' [1845b, 1846b] were the contributions which determined the subsequent development of Invariant Theory.

These two papers in particular indicate an important facet of Cayley's mathematics. This is his exaggerated interest in calculation, which, in the theory of invariants meant the tedious calculation of actual invariants and covariants. Thus Cayley was most concerned with finding Processes, through which invariants and covariants could be found. In the theory of invariants, 'proof' was hardly mentioned. To 'discover' was everything.

1.2. Determinants and generalisations

Cayley's first paper [1841a] is interesting for several reasons. In the broad division of mathematics into Analysis and Geometry[1] Cayley felt obliged to enter this first paper under both headings. It is not entirely clear what Cayley considered to be Analysis, but it is wider than an older view of Analysis as the method of solving problems by reducing them to equations. It is likely that Cayley chose the geometrical topics for his category of Geometry and designated the residual as Analysis. There is no category for Algebra and what we would now consider to fall under that heading was grouped under Analysis. Cayley's [1841a] shows capability in drawing together algebra and geometry.

The content of [1841a] in part is concerned with answering the geometrical question:

how are the distances between five arbitrarily placed points in space related?

The solution was not new[2]. It had been given by Jacques Binet (1786-1856) in 1812 but Cayley provided an elegant solution. This was couched in terms of determinants which itself was an innovative step. It marked the beginning of a custom among English mathematicians of using determinants to express geometric relationships [Coolidge, 1968a, 96]. Cayley wrote determinants in the form of an array with the now familiar vertical lines on either side of the array.

The points were labelled 1, 2, 3, 4 and 5 and were assumed to lie in any configuration in ordinary Euclidean space. By assigning them co-ordinates and by denoting squared distances between points i and j by $\overline{ij}^2$ Cayley stated the result [1841a; CP1, 2]:

$$\begin{vmatrix} 0, & \overline{12}^2, & \overline{13}^2, & \overline{14}^2, & \overline{15}^2, & 1 \\ \overline{21}^2, & 0, & \overline{23}^2, & \overline{24}^2, & \overline{25}^2, & 1 \\ \overline{31}^2, & \overline{32}^2, & 0, & \overline{34}^2, & \overline{35}^2, & 1 \\ \overline{41}^2, & \overline{42}^2, & \overline{43}^2, & 0, & \overline{45}^2, & 1 \\ \overline{51}^2, & \overline{52}^2, & \overline{53}^2, & \overline{54}^2, & 0, & 1 \\ 1, & 1, & 1, & 1, & 1, & 0 \end{vmatrix} = 0 \quad \text{-------} \; (A)$$

The proof given in [1841a] depends on the product rule for determinants. A sixth order determinant whose entries are squared distances in 4 dimensional space can be expressed as the product (row by row multiplication) of two other sixth order determinants and furthermore these latter two determinants vanish when the fourth co-ordinate is taken to be zero. From the determinant identity and by putting the fourth co-ordinate equal to zero the expression (A) is therefore obtained. Although Cayley was content to give results for 3 dimensional space, his construction was perfectly general.

With little extra work a determinantal statement for points in n-dimensional space could be obtained but Cayley did not mention this generalisation.

Other results relating to points lying on spheres, points in the plane, points on circles and points on lines were also stated in terms of determinantal arrays. One advantage of Cayley's notation, and his later double bar notation, was that any mathematical entity could be placed between the vertical bars. In this way determinants of complicated entities, such as functions, could be easily expressed. This was observed in [Muir, 1906a, vol 2, 6] and it shows Cayley's skill in devising a suitable notation to suit the purpose. Cayley effectively makes use of the numerical place notation for the entries in the determinants. This is in contrast to his later work on matrices where he uses alphabetic notation or alphabetic notation with a single subscript. In later work he often fails to recognise the advantage of the double subscripted array which had been used by some authors in connection with determinants before 1840.

Cayley was attracted to this problem at various times of his career.[3] The paper [1841a] clearly indicate Cayley's potential as a mathematician but it did not contain any genuinely new results. It was a known result elegantly expressed and written in the spirit of the 'symmetric method' approach to Analytical geometry. Cayley's first important paper in the theory of determinants was his [1843a][4] Here Cayley revealed the fundamental position determinants occupied in his mathematical thought.[5] Determinants occur in all fields of his interest: the theory of elimination, the theory of linear equations, algebraic geometry, the theory of numbers and, in short, 'in almost every part of mathematics' [1843a; CP1, 63] .

In [1843a] he showed a maturity as a mathematician which is quite remarkable for a young man of twenty-two years of age. Up to this time he had published papers in the Cambridge and Dublin Mathematical Journal but [1843a] published in the Transactions of the Cambridge Philosophical Society, showed the range of his reading. He was familiar with the earlier work of Cramer, Bézout, Vandermonde, Laplace and Cauchy and also papers on determinants by Lebesgue and Jacobi. [1843a] is divided into two parts. The first part is concerned with the investigation of the properties of determinants associated with the 'quantity' U where

$$\begin{aligned} U = {} & x(\alpha\xi + \beta\eta + \cdots) + \\ & x'(\alpha'\xi + \beta'\eta + \cdots) + \cdots \\ & \vdots \quad \vdots \quad \vdots \end{aligned}$$

Associated with this form are the determinants which he chose to investigate:

$$KU = \begin{vmatrix} \alpha, & \beta, & \cdots \\ \alpha', & \beta', & \cdots \\ \vdots & \vdots & \end{vmatrix}$$

and two others FU and ꟻU. The function KU was later called the 'discriminant' of U.

In considering these functions, he was motivated in one direction by Cauchy and Jacobi whose work contained the general theory of determinants. In another direction he was conscious of the connection with the theory of reciprocal polars of surfaces of the second order. At this time Cayley did not use an array to describe the bi-linear form U itself, though it is for bi-linear functions that he used the matrix notation later in his [1855a]. Here he was interested in studying the determinant KU of the form U and determinants FU and ꟻU 'derived' from the form U and which were themselves bi-linear forms.

These derived functions FU and ꟻU he called 'Derivational Functions', a term which he described as follows:

> I would propose to denote those functions, the nature of which depends upon the form of the quantity to which they refer, with respect to the variables entering into it, e.g. the differential coefficient of any quantity is a derivational function [1843a; CP1, 63].

Here he meant functions which one obtained by formal 'differentiation' and the term was closely associated with 'derivative' a word used in the Theory of Invariants. In the Cambridge tradition, he distinguished between the function and the value of the function. In this paper he referred to the 'quantity' KU in distinction to the functional symbol K as in the method of 'separation of symbols'. Although most of the paper is taken up with the investigation of these special determinants KU, FU and $\daleth U$ it is the second part written on an entirely separate subject, which showed Cayley's power of generalisation. In this case, the notion of the ordinary determinant was generalised from a two dimensional array to higher dimensional arrays.[6] These newly constructed determinants were later known as 'cubic determinants.'

In his [1843a] Cayley gave no clue to his motivation or application of this generalisation, although he later found them useful in the search for invariants. But from a letter he wrote to Boole [App.C, 13 vi 44] he thought they might be useful in the Theory of Elimination.

In [1843a] the generalisation is described in the following way:

> Let the letters $\Upsilon_1, \Upsilon_2, \ldots \Upsilon_k$ ......(1)
> represent a permutation of the numbers
> $1, 2 \ldots\ldots k$ ......(2)
> Then in the series (1), if one of the letters succeeds mediately or immediately a letter representing a higher number than its own, for each time that this happens there is said to be a "derangement" or "inversion." It is to be remarked that if any letter succeeds letters representing higher numbers, this is reckoned for the same number of inversions.

Suspose next that the symbol

$$\pm r \qquad \ldots. \ (3)$$

denotes the sign + or −, according as the number of inversions in the series (1) is even or odd.
This being premised, consider the symbol:

$$\left\{\begin{matrix} A\rho_1\sigma_1 \ldots (n) \\ \vdots \\ \rho_k\sigma_k \ldots \end{matrix}\right\} \ldots. \ (4)$$

denoting the sum of all the different terms of the form

$$\pm r \pm s \ldots A\rho_{r_1}\sigma_{s_1} \ldots A\rho_{r_k}\sigma_{s_k} \ldots (5)$$

the letters

$$r_1, r_2 \ldots r_k ; s_1, s_2 \ldots s_k ; \&c. \ldots (6)$$

denoting any permutation whatever, the same or different, of the series of numbers (2) (and the several combinations of being understood as denoting suffixes of the A's). The number of terms represented by the symbol (5) is evidently

$$(1 . 2 \ldots k)^n \qquad \ldots. \ (7)$$

[1843a; CP1, 76]

In the full Laplacean expansion of an ordinary determinant the terms are expressed with one subscript unpermuted. In Cayley's extension he made provision for this by placing a dagger over columns to indicate that such columns were to be left unpermuted. Thus the full generalisation was denoted by:

$$\left\{\begin{matrix} A\rho_1\sigma_1 \ldots \overset{\dagger}{\theta} \overset{\dagger}{\phi_1} \ldots (n) \\ \vdots \qquad \vdots \ \vdots \\ \rho_k\sigma_k \ldots \theta_k\phi_k \end{matrix}\right\}$$

The ordinary k x k determinant is the simple case

$$\left\{\begin{matrix} A^{+}_{\alpha_1} & \beta_1 \\ \vdots & \vdots \\ \alpha_k & \beta_k \end{matrix}\right\}$$

or equally

$$\left\{\begin{matrix} A_{\alpha_1} & \overset{+}{\beta_1} \\ \vdots & \vdots \\ \alpha_k & \beta_k \end{matrix}\right\}$$

An example of this is the simple 'cubic' determinant identical to an ordinary determinant in the case k = 2.

$$\left\{\begin{matrix} A^{+}_{\alpha_1} & \beta_1 \\ \alpha_2 & \beta_2 \end{matrix}\right\} = \alpha_1\beta_1 . \alpha_2\beta_2 - \alpha_1\beta_2 . \alpha_2\beta_1$$

Cayley's extension is obtained when the number of columns exceeds 2. For example, the cubic determinant

$$\left\{\begin{matrix} A^{+}_{\alpha_1} & \beta_1 & \gamma_1 \\ \alpha_2 & \beta_2 & \gamma_2 \end{matrix}\right\}$$

which is formed from the terms:

$\alpha_1\beta_1\gamma_1 . \alpha_2\beta_2\gamma_2$ (sign +)

$\alpha_1\beta_1\gamma_2 . \alpha_2\beta_2\gamma_1$ (sign -)

$\alpha_1\beta_2\gamma_2 . \alpha_2\beta_1\gamma_1$ (sign +)

$\alpha_1\beta_2\gamma_1 . \alpha_2\beta_1\gamma_2$ (sign -)

Using the single letter notation in Cayley's style:

a	=	111	e	=	112
b	=	211	f	=	212
c	=	121	g	=	122
d	=	221	h	=	222

the foregoing cubic determinant is:

$$ah \quad - \quad ed \quad + \quad gb \quad - \quad cf$$

Expressions like this were subsequently found useful in Invariant Theory.

Two facts about 'cubic determinants' emerge from [1843a]. Firstly, the symbol for a cubic determinant can be written as a sum of symbols of the 'daggered' type, viz.

$$\left\{\begin{matrix} A\rho_1\sigma_1\ldots\theta_1\phi_1\ldots(n) \\ \vdots \\ \rho_k\sigma_k\ldots\theta_k\phi_k \end{matrix}\right\} = \sum \pm u \pm v \ldots \left\{\begin{matrix} A\rho_1\sigma_1\ldots\overset{\dagger}{\theta}_{u_1}\overset{\dagger}{\phi}_{v_1}\ldots(n) \\ \vdots \\ \rho_k\sigma_k\ldots\theta_{u_k}\phi_{v_k} \end{matrix}\right\}$$

This identity means that the cubic determinants generalisation is illusory, for it implies that any cubic determinant can be written as the sum of ordinary determinants.

The second fact is the main result of the paper. This is a 'product rule' for cubic determinants which have an even number of columns.

$$\{\overset{\dagger}{A}.k.2p\}.\{\overset{\dagger}{B}.k.2q\} = \{\overline{AB|}\,k.2p+2q-2\} \quad [\text{CP1}, 79]$$

The product rule for ordinary determinants is obtained by putting $p = 1$ and $q = 1$. This generalised product rule is an elegant extension of the 'ordinary' product rule.

It is quite likely that Cayley knew this extension at the time that [1841a] was written, for in this paper a generalisation was mentioned [1841a, CP1, 2]. At first Cayley did not find that cubic determinants possessed much importance. As he subsequently wrote to Boole:

> I attempted some time ago in the Cambridge Philosophical Transactions to investigate the properties of some such functions, formed by a permulatory rule analogous to what I suppose the above must be, thinking they might be applicable to the general theory of elimination but they do not seem to possess much importance. [App. C, Boole, 13 vi 44].

Contrary to this expectation, Cayley shortly afterwards found them to have useful connections with hyperdeterminants. They were later introduced by Sylvester[7] in the 1850s for use in the calculus of forms but they did not attract much attention until the 1860s when, according to [Muir, 1906a vol.3, 429], Continental mathematicians gave them some attention.

In his early mathematical career Cayley sought to generalise the notion of the ordinary determinant in other directions. One generalisation occurred in one of several papers written on Déterminants Gauches [1848b; CP1, 410]. The generalisation attempted here was an extension of the ordinary determinant, as was the previous extension to cubic determinants. But in [1848b] the extension was not directly related to cubic determinants. The motivation in this case was provided by papers written by Jacobi in connection with the solution of differential equations. The functions which arise from this work were referred to by Cayley as the 'functions of M. Jacobi' and they later became known as Pfaffians.[8] Under this new definition of a determinant both the ordinary determinant and the Pfaffian can be obtained as special cases:

> One obtains these functions (of which I repeat the theory here) by the general properties of a determinant, defined in the following manner: expressing by $(1\,2\ldots n)$ some function in which appear the symbolic numbers $1, 2, \ldots, n$ and by $\pm$ sign corresponding to any permutation of the numbers, the function
>
> $$\sum \pm (1\,2\ldots n)$$
>
> (where $\sum$ designates the sum of all the terms obtained in permuting these numbers in any manner) is one which is called Determinant [1848a; CP1, 411].

Following this he suggested an even further generalisation:

> One may further generalise this definition in admitting many systems of numbers
>
> $1, 2, \ldots n;\ 1', 2', \ldots n';\ \ldots$
>
> which then ought to be permuted independently between themselves and others; one obtains in this manner an infinity of other functions, mentioned in (Crelle, 30 (1846), 7). In the case of ordinary determinants, which I will not linger over here, one would have
>
> $$(1\ 2 \ldots n) = \lambda_{\alpha 1} \lambda_{\beta 2} \cdots \lambda_{kn}$$
>
> In the case of functions, with which we are concerned (the functions of M.Jacobi), one supposes n even, and writes
>
> $$(1\ 2 \ldots n) = \lambda_{1.2} \lambda_{3.4} \ldots \lambda_{n-1,n}$$
>
> where $\lambda_{r.s}$ are any quantities which satisfy the equations (1). [The equations (1), say,
>
> $$\lambda_{r.s} = -\lambda_{r.s}, \quad r \neq s$$
>
> the conditions for a <u>système gauche</u>] [1848a, 411] .

Following [Muir, 1906a, vol.2, 258] an example shows Cayley's construction. According to Cayley's prescription:

$$(1\ \ 2\ \ 3\ \ 4\ ) = \lambda_{12}\lambda_{34}$$

and $\sum \pm (1\ 2\ 3\ 4)$ is the form consisting of the twenty four signed products

$\lambda_{12}\lambda_{34}$	$-\lambda_{21}\lambda_{34}$	$\lambda_{31}\lambda_{24}$	$-\lambda_{41}\lambda_{23}$
$-\lambda_{12}\lambda_{43}$	$\lambda_{21}\lambda_{43}$	$-\lambda_{31}\lambda_{42}$	$\lambda_{41}\lambda_{32}$
$-\lambda_{13}\lambda_{24}$	$\lambda_{23}\lambda_{14}$	$-\lambda_{32}\lambda_{14}$	$\lambda_{42}\lambda_{13}$
$\lambda_{13}\lambda_{42}$	$-\lambda_{23}\lambda_{41}$	$\lambda_{32}\lambda_{41}$	$-\lambda_{42}\lambda_{31}$
$\lambda_{14}\lambda_{23}$	$-\lambda_{24}\lambda_{13}$	$\lambda_{34}\lambda_{12}$	$-\lambda_{43}\lambda_{12}$
$-\lambda_{14}\lambda_{32}$	$\lambda_{24}\lambda_{31}$	$-\lambda_{34}\lambda_{21}$	$\lambda_{43}\lambda_{21}$

If the *système gauche* conditions are introduced, the components of the form may be reduced to simply three products

$$\lambda_{12}\lambda_{34} \quad , \quad \lambda_{13}\lambda_{24} \quad , \quad \lambda_{23}\lambda_{14}$$

and therefore

$$\Sigma \pm (1\ 2\ 3\ 4) = 8(\lambda_{12}\lambda_{34} - \lambda_{13}\lambda_{24} + \lambda_{23}\lambda_{14})$$

where $\lambda_{12}\lambda_{34} - \lambda_{13}\lambda_{24} + \lambda_{23}\lambda_{14}$ is the Pfaffian algebraic form[9] which Cayley denoted by [1 2 3 4]. In Cayley's generalisation it is one eighth of a Cayleyan determinant. The result which Cayley gave in [1848a] that 'a skew determinant of even order is the square of a Pfaffian' [CP1, 412] became known as 'Cayley's Theorem' in the nineteenth century literature. To-day the Pfaffian is an algebraic form which has fallen into obscurity.[10]

1.3. Hyperdeterminants

As is well known, Cayley generalised the homogeneous polynomials in m variables considered by Boole to homogeneous functions containing n sets of m variables [1845b; CP1, 80] . Through these general functions, Cayley obtained functions with the invariantive property which were not obtainable by the Elimination method used by Boole. Cayley called these functions 'hyperdeterminants' because although they were not determinants themselves they could be expressed as functions of determinants. This is the traditional view but it is an oversimplification. This simplification is arrived at in most histories by assuming that Cayley ascribed only one meaning to the word 'hyperdeterminant.' In fact, when Cayley was doing this work he found many methods for finding invariants and among them two distinct methods both of which he called by the name Hyperdeterminant. The first is an expression which he called simply 'Hyperdeterminant' and this is developed in his [1845b]. The second meaning is that of 'Hyperdeterminant Derivative' and is a method for obtaining invariants. This method is put forward in his [1846b] and during the 1840s it was the method used by Cayley to generate invariants. In the 1850s Cayley formulated a new synthesis for the theory and in this reverted to a method associated with the earlier [1845b]. Broadly speaking, the method of [1845b] is a method of 'integration' and the method of [1846b] is a method of 'differentiation.' Both [1845b] and [1846b] are considered in some detail in this dissertation, but before this is done, Boole's own contribution is briefly considered.[11]

The decisive influence[12] on the young Cayley in his formative work in the theory of Invariants was George Boole. The theory began in earnest with the 'mustard seed' in Boole's pioneering [1841a] and [1841b] . This influence and the encouragement which Boole gave to Cayley in the course of his preparation of [1845b] and [1846b] is evident from the letters sent by Cayley to Boole at the time.

Boole's main theorem in his [1841a] which points the way to Cayley's future development is the following:

If Q_n be a homogeneous function of the n^{th} degree, with m variables, which by [a linear transformation] the linear theorems (80) is transformed into R_n, a similar homogeneous function; and if γ represent the degree of $\theta(Q_n)$ and $\theta(R_n)$, then

$$\theta(Q_n) = \frac{\theta(R_n)}{E^{\frac{\gamma n}{m}}}$$

[Boole, 1841a, 19].

The factor E is the determinant of the linear transformation. The expressions $\theta(Q_n)$ and $\theta(R_n)$ are the discriminants of the original homogeneous function and the transformed homogeneous function R_n respectively. Boole actually obtained $\theta(Q_n)$ by the method of elimination applied to the m derived equations. The example of the homogeneous function of degree 3 with two variables is a simple illustration of Boole's very direct method:

$$Q = Ax^3 + 3Bx^2y + 3Cxy^2 + Dy^3$$

By putting the derivatives $\frac{\partial Q}{\partial x}, \frac{\partial Q}{\partial y}$ equal to zero the 'derived equations' are obtained:

$$Ax^2 + 2Bxy + Cy^2 = 0$$

$$Bx^2 + 2Cxy + Dy^2 = 0$$

On eliminating x^2, y^2 the linear equations are next obtained

$$2(B^2-AC)x - (AD-BC)y = 0$$

$$(AD-BC)x - 2(C^2-BD)y = 0$$

and by a final elimination, the function [13]

$$\theta(Q) = (AD-BC)^2 - 4(B^2-AC)(C^2-BD)$$

The method is, as Boole admitted, tedious and the higher cases promised to be even more tedious. For these cases Boole suggested that J.J.Sylvester's method of Elimination could be used. Boole puts forward some applications of the computed value $\Theta(Q_n)$, including the reduction of quadratic forms to the sums of squared terms and the investigation of the solution of the fourth and fifth degree polynomials. Boole's speculation is based on his findings for the solution of the cubic which is easily expressed in terms of $\Theta(Q_n)$:

$$av^3 + 3bv^2 + 3cv + d = 0$$

with 'solution'

$$v = -\frac{b}{a} + \frac{1}{a}\left(\frac{\theta' + \sqrt{2\theta\theta''}}{4}\right)^{\frac{1}{3}} + \frac{1}{a}\left(\frac{-\theta' - \sqrt{2\theta\theta'}}{4}\right)^{\frac{1}{3}}$$

where

$$\theta = \theta(q) = (ad-bc)^2 - 4(b^2-ac)(c^2-bd)$$

$$\theta' = \frac{d\,\theta(q)}{dd} = 2(a^2d - 3abc + 2b^3)$$

$$\theta'' = \frac{d^2\theta(q)}{dd^2} = 2a^2$$

[Boole,1841b, 119]

Although Boole recognised the importance of the subject, he had no time to develop it further.[14] However, he did provide a spur for Cayley in the remark that rounded off his [1841b]:

> An equally important subject of inquiry presents itself in the connexion between linear transformations and an extensive class of theorems depending on partial differentials, particularly such as are met with in Analytical Geometry. It is not my intention to enter into the subject in this place, nor have I leisure either to pursue the inquiry, or to elucidate my present views in a separate paper. To those who may be disposed to engage in the investigation, it will, I believe, present an ample field of research and discovery....
> Lincoln October 21st 1841 [Boole,1841b, 119].

The interest awakened in Cayley after reading this paper prompted him to write to Boole. The brief correspondence [15] which passed between the two mathematicians from considerably different backgrounds and social class, presents a vivid picture of 'mathematics in the making'. By way of introduction to the master mathematician George Boole (1814-1864) then teaching in a school at Lincoln, Cayley wrote the following letter:

> My dear Sir,
>
> Will you allow me to make an excuse of the pleasure afforded me by a paper of yours published some time ago in the mathematical journal "On the theory of linear transformations" and of the interest I take in the subject, for sending you a few formulae relative to it, which were suggested to me by your very interesting paper. I should be delighted if they were to prevail upon you to resume the subject which really appears inexhaustible. [App.C, Boole, 13 vi 44].

Cayley's first work in the theory was presented in [1845b] and [1846b]. In one sense they represent a single work, the acclaimed commencement of Invariant Theory. It will be seen that considered individually they each contain differing approaches to the subject.

On the Theory of Linear Transformations [1845b].

Cayley's [1845b] extended Boole's work and provided the first tentative steps in the calculation of invariants. The precise extension was in the consideration of not only functions of m variables but functions of n sets of m variables. Cayley later described this theory as the tantipartite theory.[16] The 'tantipartite' theory as opposed to the 'unipartite' theory is the same kind of extension that was made in the generalisation of the ordinary determinants to cubic determinants and the later generalisation of the Cayleyan determinant. Cayley's hyperdeterminant functions possessed the invariant property and were more general than those functions discovered by Boole.

In [1845b] Cayley gave the first meaning to the word Hyperdeterminant which he had first announced in a paper written in connection with the work done by Eisenstein and Hesse [1845a; CP1, 114]. In [1845b] he wrote:

> Imagine a function u of the coefficients, which are simultaneously of the forms
>
> $$u = H_p \left\| \begin{array}{l} 1s'_{,}t'_{,}\ldots,\ 1s''_{,}t''_{,}\ldots,\ \ldots \\ 2s'_{,}t'_{,}\ldots,\ 2s''_{,}t''_{,}\ldots, \\ \vdots \end{array} \right\| \ldots \quad (A)$$
>
> $$u = H_p \left\| \begin{array}{l} \tau'_{,,}1t'_{,,}\ldots,\ \tau''_{,,}1t''_{,,}\ldots,\ \ldots \\ \tau'_{,,}2t'_{,,}\ldots,\ \tau''_{,,}2t''_{,,}\ldots,\ . \\ \vdots \end{array} \right\|$$
>
> &c.; in which u denotes a rational homogeneous function of the order p. The function H is not necessarily supposed to be the same in the above equations, and in point of fact it will not in general be so. The number of equations is of course $=n$.
>
> The function u, whose properties we proceed to investigate, may conveniently be called a "Hyperdeterminant." [1845b; CP1, 82].

Such notation for homogeneous functions had been used on an earlier occasion.[17] It is purely an abbreviation and gives no clue to the specific form of an actual hyperdeterminant. Such formulae were not known to Cayley and to find such a general rule became the primary object in the theory. In order to help clarify some of Cayley's highly complicated calculations we shall consider one of his examples: the tripartite function U with 3 sets of 2 variables

$$U = ax_1y_1z_1 + bx_2y_1z_1 + cx_1y_2z_1 + dx_2y_2z_1 + ex_1y_1z_2 + fx_2y_1z_2 + gx_1y_2z_2 + hx_2y_2z_2$$

$$u = H_2 \left\| \begin{array}{cccc} a, & b, & c, & d \\ e, & f, & g, & h \end{array} \right\|$$

$$u = H_2 \left\| \begin{array}{cccc} a, & c, & e, & g \\ b, & d, & f, & h \end{array} \right\|$$

$$u = H_2 \left\| \begin{array}{cccc} a, & b, & e, & f \\ c, & d, & g, & h \end{array} \right\|$$

The functions expressed by each of these are identical in this case. The third form above, gives the function

$$(ah - cf - de + bg)^2 + 4(ag - ce)(df - bh)$$

one invariant of the function U. It was actually computed using the 'cubic determinants' [1845b; CP1, 89].

Cayley observed that the general theory could be useful in the case of symmetrical hyperdeterminants, that is, those hyperdeterminants obtained by identifying the different sets of variables. In the case of the tripartite function described above, an invariant is obtained on setting the variables

$$x_1 = y_1 = z_1 = x$$
$$x_2 = y_2 = z_2 = y.$$

The tripartite function reduces to the cubic form

$$U = \alpha x^3 + 3\beta x^2 y + 3\gamma x y^2 + \delta y^3$$

by identifying the coefficients:

$$a = \alpha$$
$$b = c = e = \beta$$
$$d = f = g = \gamma$$
$$h = \delta.$$

When these identifications are made in the invariant of the tripartite function

$$(ah - cf - de + bg)^2 + 4(ag - ce)(df - bh)$$

the invariant

$$a^2d^2 - 3b^2c^2 - 6abcd + 4ac^3 + 4b^3d$$

of the binary cubic form is obtained. This last invariant is the discriminant of the binary cubic obtainable by Boole's elimination process.

Carrying out a similar process for the binary quartic form a function $\alpha\varepsilon - 4\beta\delta + 3\gamma^2$ was obtained and this is <u>not</u> the discriminant of the binary quartic. Cayley had found a new class of functions which had the invariantive property and it is the case of the quartic that this phenomenon first appears.

From the 'Hyperdeterminant' Cayley deduced a set of Partial Differential Equations which he wrote

$$\sum\sum\ldots\left(\alpha st\ldots\frac{d}{d\beta st}\right)u = 0 \qquad \alpha\neq\beta$$
$$= pu \qquad \alpha=\beta$$

$$\sum\sum\ldots\left(r\alpha t\ldots\frac{d}{dr\beta t}\right)u = 0 \qquad \alpha\neq\beta$$
$$= pu \qquad \alpha=\beta$$

In the equations Cayley has used the abbreviated form of writing a parameter by its index $\alpha st\ldots$

α, β are fixed and summation is taken over the remaining parameters $st\ldots$

For the tripartite function with 3 sets of 2 variables there are 12 equations. One such is:

$$\left(111\frac{d}{d211} + 112\frac{d}{d212} + 121\frac{d}{d221} + 122\frac{d}{d222}\right)u=0$$

or in Cayley's alphabetic notation:

$$\left(a\frac{d}{db} + e\frac{d}{df} + c\frac{d}{dd} + g\frac{d}{dh}\right)u=0$$

Cayley remarked that 'In every case it is from these equations that the form of the function u is to be investigated; they entirely replace the system (A) (Definition of Hyperdeterminant above) [1845b; CP1, 85]. Despite the importance of these equations Cayley disregarded these partial differential equations in [1846b] in favour of another method - the method of 'Hyperdeterminant Derivation.' However, partial differential equations were not completely forgotten and they reappeared in a later synthesis.

The importance of calculation

From the outset Cayley involved himself with the calculation of the invariants. A simple expression for Boole's discriminant, (i.e. the ordinary determinant), had been found and there seemed no a priori reason why such formation rules could not be found for invariants which were not determinants. In addition Cayley thought it worthwhile to calculate the invariant functions even though at the beginning the calculations seemed onerous. A short passage from a letter written from Cayley to Boole indicates Cayley's computational facility and patience:

> For four sets of two variables there is one value of F (an invariant) of the second order, another perfectly independent one of the sixth order, the completely developed form of which consists of 232 terms, which I have succeeded with a good deal of difficulty in working out. [App. C, Boole, 23 viii 44].

The invariant of order two is

$$ap - bo - cn + dm - el + fk + gj - hi$$

but the invariant of order 6 is colossal [1845b; CP1, 91] .

When Cayley came to calculate the symmetrical invariant, he found the following expression:

$$\begin{aligned}\theta U = {} & \alpha^3\varepsilon^3 - 6\alpha\beta^2\delta^2\varepsilon - 12\alpha^2\beta\delta\varepsilon^2 - 18\alpha^2\gamma^2\varepsilon^2 - 27\alpha^2\delta^4 \\ & - 27\beta^4\varepsilon^2 + 36\beta^2\gamma^2\delta^2 + 54\alpha^2\gamma\delta^2\varepsilon + 54\alpha\beta^2\gamma\varepsilon^2 - 54\alpha\gamma^3\delta^2 \\ & - 54\beta^2\gamma^3\varepsilon - 64\beta^3\delta^3 + 81\alpha\gamma^4\varepsilon + 108\alpha\beta\gamma\delta^3 + 108\beta^3\gamma\delta\varepsilon - 180\alpha\beta\gamma^2\delta\varepsilon\end{aligned}$$

It is in connection with this last invariant that the question as to independence was brought sharply into focus. Cayley evidently informed Boole of the form of θU.
Boole subsequently found the invariant:

$$ace \quad -b^2e \quad -ad^2-c^3+2bdc$$

and a result of great importance. The three invariants were connected;

$$\theta(U) = (ae-4bd+3c^2)^3-27(ace-b^2e-ad^2-c^3+2bdc)^2$$

This bond is symptomatic of the most important difficulty which occurs in the Theory. To Cayley meeting it for the first time it was a surprising fact and one which immediately caught his interest and he replied to Boole ' I have been quite delighted with the results you have sent me. The $\theta(q) = v^3-27V^2$ is a particularly interesting one' [App.C, Boole, 11 xi 44] . The writing of [1845b] did not progress as smoothly and quickly as was usually the case with Cayley's papers. During its preparation he wrote to Boole:

> I have not been able to work out anything further on the subject of linear transformations, indeed I almost felt myself come to a standstill for the present and have hardly attempted it. The question now gives me the idea of requiring some rather complicated combinatorial analysis and I doubt whether I have enough of that to bestow upon it. I have been working last week on the transcendental function [18] defined by
>
> $$u = x\prod\prod\left(1+\frac{x}{m\omega+n\sqrt{-1}}\right)$$
>
> [App.C, Boole, 7 ix 44]

Difficulties with questions of a combinatorial character which Cayley experienced at this time recurred throughout his later research. These combinatorial problems were in direct consequence of his direct and computational approach to the subject. The subsequent attention which Cayley and Sylvester gave to combinatorial questions is partially due to their interest in the computation of invariants.

Little more than a month after making this remark on combinatorial analysis, Cayley wrote again and advised Boole that the paper [1845b] was finished:

> I have just finished a paper on linear transformations for the next No of the journal [1845b], which I believe is to be printed soon. I shall be very anxious to hear your opinion of it [App.C, Boole, 11 xi 44].

Interlude between [1845b] and [1846b]

After the publication of [1845b], Cayley continued investigations in preparation for its sequel, [1846b]. In the second paper, Cayley gave another method for finding invariants, which should not be confused with the method in [1845b]. It is a systematic method based on differentiation from which it is theoretically possible to calculate all the invariants. From Cayley's viewpoint the drawback to the method was that it yielded invariants only after a great deal of cumbersome calculation.

A month after the publication of [1845b], he wrote to Boole with his plans for the next investigation of the Theory. The primary objective was calculation:

> Do you see any way of calculating in rough the degree of labor that would be necessary for forming tables of Elimination, Sturms functions, our transforming functions [invariants] &c. If one could get to any practical results about it, and they were not very alarming, it would be worth while I think presenting them to the British Association: but I am afraid the limit of possibility comes very soon: suppose one ascertained a result would take a century to calculate, it would be rather a hopeless affair. [App.C, Boole, II xii 44].

Tables of invariants were not presented to the British Association at this time. From Cayley's calculations in both [1845b] and [1846b], the 'highest' invariant calculated is of degree 4 for a binary form of order 9. One of the difficulties Cayley faced at the beginning was that he was working on the subject in isolation. The Cambridge school of mathematicians with an interest in analytical geometry might have taken a passing interest but no other contributor to the Cambridge Mathematical Journal took up the subject until Sylvester. Subsequent papers written by Cayley during this decade on the subject of Hyperdeterminants were published in Crelle's Journal where they would be read by the Continental mathematicians. His one contact was Boole to whom he wrote early in 1845:

> I wish I could manage a visit to Lincoln, I should so much enjoy talking over some things with you - not to mention the temptation of your Cathedral. I think I must contrive it some time in the next six months, - in spite of there being no railroad, which one begins to consider oneself entitled to in these days. [App.C, Boole, 17 i 45]

Despite the relative isolation he continued to work on the Invariant Theory and was especially interested in developing better methods to calculate the invariants. One feature of the second paper on linear transformations is a more widely applicable and systematic method for the calculation. This was his method of 'hyperdeterminant derivation' for the calculation of invariants. Cayley was working on this method in March 1845. A letter to Boole described this work:

> I have just found a property of hyperdeterminants which like most of the others gives another method of determining them (one would be glad not to have so many)[19] and which seems to me perhaps the most curious of all. [App.C, Boole, 5 iii 45]

The 'hyperdeterminant derivative' method of finding invariants is best illustrated by the very simplest example. This is the case of the quadratic, and even in this case the calculations are lengthy:

$$u = ax^2 + 2bxy + cy^2$$

Two binary quadratics are formed and denoted

$$u_1 = ax_1^2 + 2bx_1y_1 + cy_1^2$$
$$u_2 = ax_2^2 + 2bx_2y_2 + cy_2^2$$

and following Cayley's notation:

ξ_i denotes partial differentiation with respect to x_i

and η_i denotes partial differentiation with respect to y_i.

The result of applying the operator

to the product $U_1U_2^{(\xi_1\eta_2 - \xi_2\eta_1)^2}$

gives the required invariant

$$(\xi_1\eta_2 - \xi_2\eta_1)^2 u_1 u_2 = \overline{12}^2 u_1 u_2 = 4(ac - b^2)$$

This is of course the simplest case. To compute the invariant of degree 4 (the discriminant) of the binary cubic form by this method requires the product of 4 copies of the binary cubic operated on by:

$$\overline{12}^2\, \overline{13}\, \overline{24}\, \overline{34}^2$$

<u>On Linear Transformations [1846b]</u>

The overall goal Cayley set for Invariant Theory was set out in [1846b]. Briefly stated he said the future development should aim:

> To find all the derivatives [invariants] of any number of functions, which have the property of preserving their form unaltered after any linear transformation of the variables. [1846b ; CPI, 95]

Here the important word is 'find.' This was Cayley's principal motive and as a consequence,the importance of the algebraic process which generate invariants and covariants was paramount. Compared to the process for finding invariants, Cayley's interest in providing careful proofs is only slight. The objective is characteristically stated in the most general terms. It made provision for his previously stated tantipartite theory and allowed for the possibility of finding joint invariants of sets of functions. But to bring the task into the realm of the possible, he carefully prefixed this objective by a remark to the effect that only in the very special case of a single function in only two variables was there any real hope of completing the objective.[20] In retrospect this caveat had a prophetic ring. Writing at the end of the century, P.A.MacMahon concluded that invariants and covariants for binary forms of order less than or equal to 8,were known. For a single function in three variables, the listing of invariants was complete where the function was of order three or less, but only partial results were known when the function of three variables was of order four. For other higher forms and systems of forms the results were correspondingly fragmentary [MacMahon, 1910a, 636]. In the 1880s Cayley himself was still working on the binary quintic and had spent much time in the intervening years on the calculation of its invariants and covariants.

The finding of invariants was only part of the objective, and to Cayley, writing in 1846, the easier part of the problem. To the overall objective he added cautiously:

> there remains a question to be resolved, which appears to present very great difficulties, that of determining the *independent* derivatives, and the relation between these and the remaining ones. [CP1, 95; his italics]

In the 1850s, Cayley began calculating the invariants in a comprehensive way. Covariants were then actively considered in the execution of the general objective where the problem was to find the complete and independent system of invariants and covariants of a binary form. The paper[1846b]represented a bold approach, but the complexity of the task was not fully realised by Cayley. The definitions were given in a very general way.

A simple example of the working of Cayley's method of 'hyperdeterminant derivation' has been considered. In [1846b], Cayley defined the method quite generally.

The operator symbol $\square$ was given by

$$\square = F(\|\Omega\|^f, \|\Omega'\|^{f'}, \ldots) \quad \text{(an n-tuple)}$$

where

$$\|\Omega\| = \left\| \begin{matrix} \xi_1, \xi_2, & \ldots & \xi_p \\ \eta_1, \eta_2, & \ldots & \eta_p \\ \vdots & & \vdots \end{matrix} \right\|$$

and where

ξ_i , η_i were partial derivatives

and F a rational homogeneous function.

Cayley stated :'... the function $\square U$

is by the above definition a hyperdeterminant derivative. The symbol may be called "symbol of hyperdeterminant derivation," or simply "hyperdeterminant symbol." ' [1846b; CP1, 97] Here he was making use of the 'method of the separation of symbols' by separating the meanings of $\square$ and $\square U$ but in the calculation of invariants this distinction was not dwelled on at length.

In [1846b] Cayley investigated algebraic forms of low order and presented some general results through the use of his symbolic calculus. The presentation is deductive but the results were not only achieved by deduction. Factors which are perhaps ignored when discussing the work of mathematicians include the speculative element and the initial simplistic guesses. Cayley's speculations were obtained by analogy with known results. In considering forms of two variables of order six he had found two invariants of order 2 and 4 and in a letter to Boole speculated on the existence of an invariant of the third order:

> I should rather think there was one of the third order which I know nothing about; however, it is only on the analogy of the theory of functions of the fourth order that I imagine it to exist.

Continuing the speculation (later found to be false) he compared the state of affairs existing for even order functions with those which might exist for the odd order functions. In tabular form he wrote:

order	transforming functions of the orders
2	2
4	2, 3
6	2,3, 4

Then might that for odd functions be

3	4
5	4, 6
7	4, 6,8

Since clearly the orders must be even.

> This is only founded on the instances n = 2, 3, 4 and it contradicts your results for n=5, still it seems so natural that the number of the functions should depend very simply upon the value of n. The question then comes is there any practicable mode of finding the functions for the fifth and sixth orders. If my supposition about the degrees [of invariants] should be correct, it ought not to be so very difficult but with functions of the 12th order, one is afraid to think of it. [App.C, Boole, 27 xi 44]

As Cayley subsequently discovered [App.C, Boole, 13 xii 44] there is no invariant of the third order for a sixth order binary form.

The classification of invariants by degree of an invariant was suggested in [1846b]. The degree is the dimension of the coefficients in the invariant and a listing of invariants by degree was the course adopted during the early work. In the 1850s the calculation of invariants and covariants of forms of specific orders[21] was undertaken. For the difficult problem of calculating invariants, Cayley was from the first involved with combinatorial problems. In considering invariants of degree 4, Cayley found, for instance, that for binary forms of order f the number of invariants was $\Theta(f)$ and the number of relations between them was $\Theta(f-1)$ where $\Theta(f)$ denoted the number of divisions of an integer f into three or fewer parts [1846b; CP1, 104]. From this he deduced that the number of independent invariants was

$$E\left(\frac{f}{6}\right) \quad \text{for } f \text{ even}$$

$$E\left(\frac{f+3}{6}\right) \quad \text{for } f \text{ odd}$$

where $E\left(\frac{a}{b}\right)$ is the greatest integer part of $\left(\frac{a}{b}\right)$.
From this result Cayley was able to deduce that for a binary form of

order 9 there were two independent invariants of the same degree.

Cayley's interest in calculation and the question of independence led him to questions of combinatorial analysis. Lack of knowledge on these questions limited the extension of his initial methods to the most simple cases. Looking forward, he surmised that extensive work in partitions would be needed before much progress could be made. Another line of attack which he proposed at the time was the linking the theory of hyperdeterminants with the theory of elimination.

In fact, Cayley did not make much progress on the calculation of invariants in this decade. His first two papers contained the general objective and outlines of the intended path but apart from a note in 1847 in which he considered miscellaneous problems in the Theory and apart from papers which linked geometrical questions to Invariant Theory (discussed below), a hiatus occurred in Cayley's development of the subject. In his short Note [1847c; CP1,352] he did attempt to find an algorithm for producing invariants. Unfortunately his speculations were unsuccessful:

> It appears possible to me that all the hyperdeterminants which belong to the function V, can be found in eliminating between the equations (p in number) with which we are concerned, and that is so in the case where U [sic] is of the order 2p or 2p+1; at least this rule is verified for the functions of the second, third and fourth orders, and this appears (a priori) at least probable for the invariants of a degree much higher than those of the second degree, for which as one has just seen, it is effectively true.
>
> This being so, there will only be a number p of independent derivatives for functions of order 2p and 2p + 1; a conclusion I have not been able to demonstrate. [1847c; CPI, 353]

The speculation made by Cayley is untrue but it seemed reasonable as a working hypothesis. He knew the invariant systems (that is the constant derivatives) for the binary forms of order 2, 3 and 4 for which there are respectively 1, 1 and 2 independent invariants. What he did not know about the quintic at this time was that there were four independent invariants. One had been found [1846b;CP1,108], whereas the three others, of degrees 8, 12 and 18, were not known by Cayley until much later. His boldness in basing at least part of his argument on experience in such few cases is typical. He frequently conjectured results (and acted on them being true) from lower to higher cases on very slender evidence indeed and with hindsight some of these speculations were naïve. It was only in the next decade that questions relating to invariants and covariants were found to be more complicated than first thought.[22]

The binary quintic, which is the first difficult case in the Theory, became the hub of his later algebraic researches.

<u>Connections</u>

Why did Cayley take up the study of invariants so avidly at the beginning of his career and work on these problems throughout his life? At the outset, the subject was ~~virtually fundamental~~ to all of Cayley's other mathematical interests. The importance he placed on determinants with their usefulness in other branches of mathematics has already been stressed. With Hyperdeterminants he found a generalisation of determinants with even greater potential for application in other branches of mathematics. The indications were there. At the end of [1845b] he noted in connection with one of the discovered invariants:

> The function $\alpha\varepsilon - 4\beta\delta + 3\gamma^2$ occurs in other investigations: I have met with it in a problem relating to a homogeneous function of two variables, of any order whatever $\alpha, \beta, \gamma, \delta, \varepsilon$ signifying the fourth differential coefficients of the function. But this is only remotely connected with the present subject. [1845b ; CPI,93]

The reference here is to his work related to Hesse's automorphic functions. Cayley worked at this subject at the time he was submitting his [1845b] [App. C, Boole 21 x.44]. Hesse had demonstrated an algebraic property of the 'Hessian' ∇U where U is a homogeneous function of the third order in three variables x, y, and z. Hesse had demonstrated:

$$\nabla(U + a\nabla U) = AU + B\nabla U$$

where a is any constant and A,B were to be determined. Cayley gave a similar result for a function of two variables but any degree and in addition determined the coefficients which appear in his formula. What links this work with invariants is the appearance of the two independent invariants of the binary quartic form:

> Let U be a homogeneous function of order ν of two variables x, y and ∇U the determinant
>
> $$\frac{d^2V}{dx^2}\cdot\frac{d^2V}{dy^2}-\left(\frac{d^2V}{dxdy}\right)^2$$
>
> then one has
>
> $$(\nu-2)(\nu-3)^3\nabla(U+a\nabla U)=$$
>
> $$\{-\nu(\nu-1)(\nu-3)^2Ja+\nu(\nu-1)(2\nu-5)^2Ia^2\}U$$
>
> $$+\{(\nu-2)(\nu-3)^3+(\nu-2)(\nu-3)(2\nu-5)Ja^2\}\nabla U$$
>
> Representing by i, j, k, l, m, the differential coefficients of U one has
>
> $$I=ikm-il^2-jm^2-k^3+2jkl$$
>
> $$J=4jl-3k^2-mi$$
>
> such that I, J are functions of x, y of orders $3(\nu-4)$ and $2(\nu-4)$ respectively.
> [1845a ; CPI, 115]

At the same time, Cayley was also able to employ the theory of invariants in the study of elliptic integrals. Here the question was to reduce the algebraic integral

$$\int\frac{du}{\sqrt{U}}$$

where U is a binary quartic form.

Using the theory developed regarding invariants[24] I, J for the binary quartic, Cayley reduced the integral to a standard elliptic integral:

$$\int \frac{du}{\sqrt{(1+pu^2)(1+qu^2)}}$$

[1846a;CP1, 224]

The consequence of the theory of invariants with respect to geometrical questions were plain enough to Cayley. This is seen, for instance, in his [1847b] where he stressed the connection of different branches of his mathematical study. In the first part of the paper he gave three theorems, all of which were related to the question of tangents drawn to curves. Cayley demonstrated that these questions can be treated by the theory of elimination and the theory of invariants. This is seen in his statement of the third theorem:

> One finds the points of contact of double tangents, in combining with the equation of the curve [$U=0$] an equation $\Pi U = 0$ [ΠU is an invariant of U] of the order $(n-2)(n^2-9)$ with respect to the variables, and of the order n^2+n-12 with respect to the coefficients, that is to say, since there corresponds two points of contact to a double tangent, the number of these tangents is equal to $\frac{1}{2}n(n-2)(n^2-9)$: a theorem demonstrated indirectly by M.Plücker. [1847b; CP1, 344].

Cayley's work in algebraic geometry is vast (see Appendix A) and cannot be considered here. Cayley continuously utilised the intimate connection of formal algebra and its application to questions of geometry. Of the power of the analytical method he had no doubt. After stating the theorem mentioned above, he gave his reason for writing this paper:

My intention has been to give here a precise idea of the theorems to be demonstrated, so as to formulate a purely analytic theory of reciprocal polars. I have only put forward these theorems (without seeking to prove them), so as to show their link with the theory of elimination and with that of hyperdeterminants; the latter in particular that one needs to use, I believe, to demonstrate the formula given above

$$[Y] = \Lambda . U + (\alpha x + \beta y + \gamma z)^{n^2-n-6} (\Pi U),$$

and in order to find the form of the invariant by means of which one determines the points of contact of the double tangents. I will be pleased if these researches might in some way facilitate the solution of the problem of reciprocals of surfaces; which still remains completely unknown.

[1847b; CP1 345]

1.4. Algebra in transition

Sir W.R.Hamilton's discovery of the quaternions in 1843 was a decisive event in the history of 'higher' number systems. He published his work on quaternions in England in his[1844a] . The publication immediately had a catalytic effect. Other mathematicians[25] soon found other systems like the quaternions and these systems, like the quaternions, failed to obey the 'ordinary laws of arithmetic.'

Cayley was in the vanguard of this activity. He was the first person to publish a paper on quaternions after Hamilton. We go back to a letter written to George Boole in September 1844 to gauge Cayley's initial reaction to Hamilton's system:

> I was very much interested lately by a short paper of Sir William Hamilton in the philosophical Magasine, On a new system of imaginary quantities He considered what he term [ed] quaternions, expressions of the form
>
> $$x + iy + jz + kw,$$
>
> $$i, j, k$$
>
> being imaginary symbols satisfying
>
> $$i^2 = -1,\ j^2 = -1,\ k^2 = -1,\ ij = k,\ jk = i,$$
>
> $$ki = j,\ ji = -k,\ kj = -i,\ ik = -j$$
>
> The remarkable part of which is evidently that the factors of a product are not convertible, but as he observes, why should they be. The results that the supposition leads to are certainly quite consistent with each other and some of them very remarkable.
> [App.C, Boole, 7 ix 44]

The interesting point about Cayley's reaction is his immediate acceptance of the non-commutativity of the quaternions. In Cayley's case there was no apparent difficulty in subscribing to the new view of algebra which asserted itself in the 1840s. The new thinking was summarised by Boole in his Logic :

> They who are acquainted with the present state of the theory of Symbolical Algebra, are aware, that the validity of the processes of analysis does not depend upon the interpretation of the symbols which are employed, but solely upon the laws of their combination. Every system of interpretation which does not affect the truth of the relations supposed, is equally admissible, and it is thus that the same process may, under one scheme of interpretation, represent the solution of a question on the properties of numbers, under another, that of a geometrical problem, and under a third, that of a problem of dynamics or optics. This principle is indeed of fundamental importance;
>
> [Boole, 1847a]

Some mathematicians had little experience with symbolic algebra which did not obey the commutative law and associative law. In particular, Peacock, through his 'Principle' and with arithmetical algebra as the suggestive science, had a more limited view of Symbolical Algebra. In Peacock's view the commutative law and associative law were automatically assumed. Cayley felt no compulsion to adopt this approach and immediately accepted the quaternions as a valid system.

Boole took up quaternions but only contributed a single paper on the subject [1848a] . Cayley's initial paper [1845c] on quaternions is in two parts. The first part is concerned with his interest in determinants. The following passage from a letter to Boole draws attention to this and points to Cayley's cavalier attitude in his treatment of questions which required more than a formal treatment:

> The properties of determinants for instance are modified most curiously. But like, I forget what Jewish writing it was said of, the idea would require Camel loads of commentaries for its development; every [word] would require to be rewritten, and the new version would be ten times as long as the original, if all the formulae of analysis had to be adapted to the cases of the symbols it contained, denoting quaternions.
> [App.C, Boole, 7 ix 44]

Here Cayley considered yet another generalisation of the determinant. But here the generalisation is not a structural extension as has been previously discussed, but a generalisation involving the elements of the determinants themselves. For quaternions $\omega, \omega', \phi, \phi'$ Cayley defined the determinant by

$$\begin{vmatrix} \omega, & \phi \\ \omega', & \phi' \end{vmatrix} = \omega\phi' - \omega'\phi$$

where of course the quaternion factors cannot be interchanged as with the 'ordinary' quantities. A consequence of this definition as Cayley noted, is that a second order determinant vanishes if it has identical rows but does not necessarily vanish if it has identical columns. He further noticed that a quaternion determinant vanishes if four or more of its columns were identical.

Cayley defined a quaternion determinant in the general case by analogy with the Laplacean expansion for ordinary determinants. He proved no results about the general case and only investigated quaternion determinants of low orders. As with many of Cayley's theorems the general case is dismissed almost perfunctorily and this paper on quaternion determinants is an example of this practice. Cayley just has time to remark:

> Again, it is immediately seen that
>
> $$\begin{vmatrix} \omega, & \phi \\ \omega', & \phi' \end{vmatrix} + \begin{vmatrix} \phi, & \omega \\ \phi', & \omega' \end{vmatrix} = \begin{vmatrix} \omega, & \omega \\ \phi', & \phi' \end{vmatrix} - \begin{vmatrix} \omega', & \omega' \\ \phi, & \phi \end{vmatrix}$$
>
> &c. for determinants of any order.
>
> [1845c; CPI, 125]

Finally, in [1845c] Cayley mentioned linear equations with quaternion coefficients but only examined the question in the case of two equations in two unknowns. After [1845c], Cayley left the question of determinants with non-commutative entries until the time of his presentation of [1858a], the <u>Memoir on Matrices</u>.[26]

The ease with which Cayley accepted the non-commutative element of the quaternions was not commonplace amongst the English mathematicians. Augustus de Morgan, who by 1844 had published four papers on the Foundations of Algebra, had long considered the problem of constructing triplets and in his fourth paper on the Foundations of Algebra [1844a] he gave possible systems of triple algebra. But De Morgan did not disregard the commutative and associative laws and most of his systems obey both these laws, although he did consider non-standard moduli. Cayley was also interested in the de Morgan 'triple algebras:

> I have not seen De Morgan's paper on triple algebra. It is I suppose a good deal connected with Sir W Hamilton's quaternions, which is a most interesting theory. I think I mentioned it to you in a former letter: you can easily work out the fundamental results if you are so inclined from the mere idea - which is that of considering the general symbol
>
> $$\alpha + \beta i + \gamma j + \delta k$$
>
> i, j, k being imaginary roots of -1 which combine according to the laws
>
> $$i = jk\ ,\ j = ki\ ,\ k = ij$$
> $$-i = kj\ ,\ -j = ik\ ,\ -k = ji$$
>
> What are De Morgan's analogous assumptions! I heard an abstract of the paper read: - which was just sufficient not to tell me what I wanted to know about it. [App.C, Boole, 17 ii 45]

The year of 1845 was an important one for Cayley with respect to work in the new algebraic systems. He published papers on algebraic couples, the quaternions and the octaves.[27] Cayley's discovery of the octaves appeared in his [1845d]. Cayley did not emphasize the lack of commutativity and associativity of the new system but merely warned the reader that the system 'requires some care in writing down, and not only with respect to the combinations of the letters, but also to their order;' [1845d CPI, 127] The note was short but in it Cayley gave an extension of Euler's 'four squares' theorem.

Cayley also published [1845e] in which he listed a number of possible systems for 'algebraic couples'. In this he appeared to have been influenced by de Morgan's work on triple algebra. Cayley's systems were presented with alternative rules of 'multiplication' and alternative definitions of the moduli. The paper [1845e] is in response to an earlier paper by J.T.Graves on a similar subject [J.Graves,1856a, 315]. Graves only considered couples $ix+jy$ which obeyed the ordinary rules (commutativity and associativity) and these he named normal couples. The couples which obeyed the anti-commutative law he termed anomalous couples. In response to this paper, Cayley considered simply 'algebraical couples'. They are of the same form, $ix + jy$, but there is not the implicit assumption that they should satisfy a priori any particular law. The only requirement is that the system should be closed:

$$(ix+jy)(ix_1+jy_1) = iX + jY$$

and that the multiplication be determined by the multiplication of the symbols i and j between themselves. This (implicit) assumption of linearity was not thought necessary to mention. Cayley considered the different systems of 'couples' which arose from different relationships between the parameters which determine the multiplication table for i, j. The four equations which i, j satisfy were

$$i^2 = \alpha i + Cj$$
$$ij = \alpha' i + C' j$$
$$ji = \gamma i + \delta j$$
$$j^2 = \gamma' i + \delta' j$$

[1845e; CPI, 128]

With these systems there was no mention by Cayley of imaginary quantities. The ordinary complex numbers can be obtained as a special case of these 'couples' but this fact was passed over in silence. In [1845e] he was interested in quite general systems and he considered moduli of the general form

$$K(x+\lambda y)(x+\mu y)$$

The paper concluded with the classification of several formal systems and there was no attempt to examine the individual properties of these purely symbolic algebras.

In his [1884b] Cayley returned to the problem of double algebras. In this later paper Cayley's objective remained the same: to classify the different algebraic systems and to do this directly through the four equations which the symbols i and j should satisfy.

In June 1845, the British Association Meeting was held at Cambridge.[28] The three papers Cayley had published on new algebraic systems and this kind of work generally received substantial attention at the meeting. From the presidential chair Sir John Herschel (1792-1871), a competent mathematician and eminent scientist, enthusiastically encouraged further exploration and development. With respect to the new systems he said:

> Conceptions of a novel and refined kind have thus been introduced into analysis—new forms of imaginary expression rendered familiar - and a vein opened which I cannot but believe will terminate in some first-rate discovery in abstract science. [Herschel, 1845, B.A.Address, xxviii]

In mentioning Cayley by name, Herschel specifically referred to the paper on algebraic couples [1845e]. In the Mathematics and Physics section of the meeting Hamilton gave a paper: On the System of Quaternions and Charles Graves, professor of mathematics at Dublin gave a paper on triplets [C.Graves, 1847a]

Two years later, the British Association meeting was held at Oxford and by then the quaternions were even better known. On this occasion, Herschel was even more enthusiastic. The quaternions appeared to him as 'a cornucopia of scientific abundance' and his advice to mathematicians at the meeting was to 'study the quaternions' [Crowe, 1967a].

Hamilton's long search for the multiplication of triplets had been guided by geometric considerations of a kind based on the analogy with the complex numbers. When Hamilton made the essential step to the quaternions, Cayley perceived Hamilton's achievement as a discovery in symbolic algebra. This may have been due to Hamilton's presentation of his results, for he gave it in purely symbolic form and there was no hint that geometric considerations had played a part in their discovery. Cayley needed no geometric justification for quaternions of the kind that had earlier been put forward for explaining the existence of the square root of − 1. He would have been largely in agreement with de Morgan who, writing on the nature of his own triple algebra, said:

> The interpretation of these systems are very imperfect, and appear to present great difficulty; but their symbolical character is unimpeachable.
> [de Morgan, 1844a]

De Morgan had difficulty in finding a suitable geometric interpretation for his 'triplets' due to his non-standard definition of the modulus. In the case of the quaternions, a geometric interpretation was found by Cayley almost immediately. This finding he published as the other half of his first paper on quaternions [1845c] . Cayley showed the product

$$q^{-1}(ix + jy + kz)q$$

(where q is a quaternion) corresponded to a rotation of axes where the coefficients of the resulting pure quaternion were the co-ordinates of a point after a rotation of axes has taken place. Because Cayley had originally seen the quaternions as a symbolic algebra the geometric result appeared 'rather a curious one.'

Expanding the product $q^{-1}(ix + jy + kz)q$ as:

$$(1 + \lambda i + \mu j + \nu k)^{-1}(ix + jy + kz)(1 + \lambda i + \mu j + \nu k)$$

$$= \frac{1}{1 + \lambda^2 + \mu^2 + \nu^2}\left\{\begin{array}{l} i[x(1+\lambda^2-\mu^2-\nu^2) + 2y(\lambda\mu+\nu) + 2z(\lambda\nu-\mu)] \\ +j[2x(\lambda\mu-\nu) + y(1-\lambda^2+\mu^2-\nu^2) + 2z(\mu\nu+\lambda)] \\ +k[2x(\lambda\nu+\mu) + 2y(\mu\nu-\lambda) + z(1-\lambda^2-\mu^2+\nu^2)] \end{array}\right\}$$

$$= i(\alpha x + \alpha' y + \alpha'' z) + j(\beta x + \beta' y + \beta'' z) + k(\gamma x + \gamma' y + \gamma'' z)$$

where he found the coefficients α, β, γ make the transformation

$$x_1 = \alpha x + \alpha' y + \alpha'' z$$
$$y_1 = \beta x + \beta' y + \beta'' z$$
$$z_1 = \gamma x + \gamma' y + \gamma'' z$$

take one set of rectangular axes into another. This he had written on two years earlier [CPI, 28] and in this, as in later work, he was much influenced by a paper of Olinde Rodrigues.[29]

Furthermore, it was possible to interpret the coefficients λ, μ, ν in terms of angles to the resultant axis of the rotation

$$\lambda = \tan\frac{\theta}{2}\cos f, \quad \mu = \tan\frac{\theta}{2}\cos g, \quad \nu = \tan\frac{\theta}{2}\cos h$$

Cayley concluded that 'It would be an interesting question to account, a priori, for the appearance of these coefficients here.' Cayley's discovery also raised the question of whether such geometric transformations would be obtained from other symbolic algebras.

Hamilton was also aware of the use of quaternions as a rotation of axes:

> That important application of the author's [Hamilton's] principles had indeed occurred to himself previously; but he was happy to see it handled by one so well versed as Mr. Cayley is in the theory of such rotation, and possessing such entire command of the resources of algebra and geometry. [R.P.Graves 1882a, vol.3, 196]

From the first, Cayley accepted quaternions as a symbolical algebra and the quaternion operator followed as an application. But the quaternions seemed to Cayley to be not wholly abstract and he later reflected: 'it seems clear that the whole theory of quaternions was in its original conception intimately connected with the notion of rotation.' [1862a; CP4, 559]

Cayley's interest in symbolic algebra after 1845 waned for a time, apart from a note on the Octaves [1847a]. There he noted that the symbol $\Lambda^{-1} X \Lambda$ in [1845c] appeared to be without a geometrical interpretation in the case where X, Λ, represented Octaves. The failure to obtain satisfactory geometric interpretations for other symbolic algebras could only reinforce the importance of quaternions.

In 1848 Cayley travelled to Dublin and while there heard Hamilton lecture on quaternions. Hamilton gave a series of four lectures and they formed the basis for his Lectures on Quaternions [1853a]. While he was there Cayley met Hamilton [R.P.Graves, 1882a vol.2,605] and relations between the two men always appear to have been cordial. At the beginning of the first lecture Hamilton publicly praised Cayley's contributions to Quaternion Theory.

It is tempting to suggest that some of Cayley's later ideas on matrices were derived from Hamilton's 1848 lectures. [30] However, this is purely speculative. Cayley and Hamilton moved in the same mathematical society and each was interested in the others work. Hamilton's preoccupation with quaternions left him little time to appreciate the work Cayley was doing in algebraic geometry though Hamilton acknowledged the support he received from Cayley and others in his work on the quaternions:

> Now Herschel, Cayley, Donkin, Peacock, yourself [de Morgan] and others in England, to say nothing of my Dublin friends, have, as it seems to me, stepped out of their own ways to recognise and encourage my exertions. [R.P.Graves, 1882a, vol.3, 331]

In April 1846 Cayley had been admitted to Lincoln's Inn to read for the Bar. To stay at Cambridge would have eventually meant taking Holy Orders and evidently this course held no attraction for him. Fortunately he had the necessary financial means for training as a barrister. This choice of career, a well trodden path for Cambridge graduates of the period, suited his purpose. After three years, this profession would give him financial independence and sufficient leisure to pursue his mathematical interests. In the year he began the training, his mathematical output dropped but increased steadily during the following years.

Chapter 1

References

1. Cayley's classification of papers in [CP vol.1] compiled during 1887-8 was rudimentary. Under the broad headings of Geometry and Analysis it contains over fifty sub-classifications of which more than thirty contain only one paper. This type of classification scheme exists for volumes 1 - 7 of the Collected Mathematical Papers the portion which Cayley himself edited and for volumes 8 - 13 the portion edited by A.R.Forsyth after Cayley's death in 1895. This classification shows the width of Cayley's interests. An attempt has been made to group Cayley's work in Appendix A.

2. The problem and Binet's solution is discussed in [Muir, 1906a, vol.1, 123-130].

3. In his [1853b, 1860d, 1861e, 1888a and 1889d], Sylvester referred to [1841a] as Cayley's 'juvenile paper'.

4. [1843a] was read to the Cambridge Philosophical Society. He was elected to this Society in 1842. In 1845 he was elected to its Council but rather strangely [1843a] is the only paper he read to the Society in the period up to the time he went to Cambridge as Sadleirian Professor in 1863.

5. In later years (after 1870) Cayley still maintained that determinants were of the greatest importance. In conversation with Felix Klein, he is reported to have said that had he to give fifteen lectures on the whole of mathematics, he would devote one of them to determinants [Klein, 1908a, 143].

6. It may be in connection with cubic determinants that Cayley had an idea of 'cubic matrices' when he casually referred in his [1858a] to the idea of a matrix ' used in a more general sense.' In [Tvrda, 1971a, 347] reference is made to the possibility of Cayley having an idea of spatial matrices but no reference is made to cubic determinants.

7. Sylvester called them commutants when he worked with them in [1852a; SP1,305]. They were the subject of a question of priority between Cayley and Sylvester.

8. The name Pfaffian was given to these functions by Cayley. The functions were discovered by Jacobi in connection with a method due to J.F.Pfaff for solving differential equations [Muir, 1906a, vol.2, 268]

9. A later notation for Pfaffians was a triangular form:

$$\left|\begin{matrix} \lambda_{12} & \lambda_{13} & \lambda_{14} \\ & \lambda_{23} & \lambda_{24} \\ & & \lambda_{34} \end{matrix}\right| = \begin{matrix} \lambda_{12}\lambda_{34} \\ -\lambda_{13}\lambda_{24} \\ +\lambda_{14}\lambda_{23} \end{matrix}$$

This was developed by Muir [1882a] but this notation does not appear to have been used by Cayley.

10. Sylvester mentions hyper-Pfaffians in [App.B, 30 viii 1861].

11. [Boole, 1841a, 1] Boole introduced his work on the transformation of homogeneous functions by linear substitutions by referring to the work of Continental mathematicians and the work of de Morgan on Analytical Geometry.

12. Cayley gives credit to Boole for inaugurating the theory in a letter [App.C, Boole, 15 iv 45] written at the time Cayley was preparing his own papers. At a later date, Cayley waived any claim to inaugurating the Theory of Invariants [Henrici, 1884a, 4].

13. Boole suggested the term 'final derivative' in his [1843a] whereas Cayley used 'determinant.'

14. Boole briefly returned to the subject later with his [1851a] and [1851b] His initial [1840a] was followed by [1841a, 1841b and 1845a].

15. The letters written from Cayley to Boole were presented to Trinity College, Cambridge, after Cayley's death in 1895. They had very likely been returned to Cayley after Boole's death

15 (continued)

in 1864. The other half of the correspondence is not believed to be extant. (See Appendix B for the fate of some letters written to Cayley)

16. In Cayley's [CP1, 584] added during the compilation of the Collected Mathematical Papers he refers to his [1845b] as the paper in which he first stated the general objectives of Invariant Theory. However, it is in the second paper, [1846b], in which the general programme is stated most clearly.

17. Cayley's [1843b; CP1, 60] where he uses this notation to write a homogeneous function of the second order.

18. This work is presented in [Cayley, 1845f]. From his own vantage point as a specialist in Elliptic Functions, Glaisher [1895c] judged Cayley's early work on Elliptic Functions one of the most important English contributions to the theory of doubly periodic products.

19. The other method referred to in his letter to Boole were set out in his first paper on linear transformations and were ad hoc methods; a method Cayley refers to at the end of his [1845b] is obtained by observing that an invariant can be expressed in terms of the symmetric functions of the roots of the equation $u = 0$ but this method is special in that it can only be applied to binary forms.

20. According to [Turnbull, 1926a] the second part of the nineteenth century was the 'binary era' while in the early part of twentieth century the accent was on forms of many variables. The study of forms of three variables spanned both these periods according to Turnbull.

21. In the 1850s when organisation of results became more systematic, attention was focussed upon the parent form with the subsequent attempt to find its system.

22. The full list of invariants and covariants for the binary quintic is given in [Cayley, 1871a].

23. See [App. C, Boole, 21 x 44] for a communication to Boole on this result. See also [Muir, 1906a, vol. 2, 381] for discussion of technical details.

24. Since Cayley was frequently concerned with low order forms, curves of low order, the solution of specific polynomials of low degree, some invariants continually occur in his work. This is the case for the invariants I, J.

25. In England, Peacock, Boole, de Morgan and Gregory were all active in the development of symbolic algebra. In [Koppelman, 1971a] it has been shown that these mathematicians were concerned with the Calculus of Operations and it is from the mathematics which arises in this Calculus that the development of Abstract Algebra is viewed. According to Koppelman, Boole considered non-commutative operations as they naturally arose in his work on linear differential equations with variable coefficients. Boole's work <u>On a general method of analysis</u> won the Mathematical Medal of the Royal Society and was published in 1844.

26. He considered them in connection with matrices whose entries are themselves matrices. The generalisation

$$\begin{vmatrix} a, & b \\ c, & d \end{vmatrix} = \frac{1}{2}(ad + da - bc - cd)$$ was briefly

suggested [App B, 19 xi 1857]. Determinants with entries in non-commutative systems were neglected in the nineteenth century. Muir [1906a] mentions only William Spottiswoode and C.A. Joly. Spottiswoode in 1876, investigated determinants whose elements were alternate numbers.

Joly considered in 1896, low dimensional determinants with quaternion entries. His definition is almost identical to Cayley's definition and he did not significantly improve on Cayley's results.

27. They are often referred to as Cayley numbers but they were discovered independently by John T. Graves (1808-1870) in December 1843 shortly after Hamilton discovered the quaternions [CP1, 586]. Graves was Examiner in Law and Jurisprudence at London University. A friend of Hamilton, he was a member of the group interested in the new algebraic systems.

28. Cayley was present at this meeting as were George Boole and J.J.Sylvester. J.J.Sylvester had returned from America in 1844. He entered Inner Temple on 29 vii 1846 and was called to the Bar on 22 xi 1850 but he did not practise as a barrister. He was an active member of the Committee which set up the Institute of Actuaries and acted as an Actuary himself between 19 xii 1844 and 12 v 1855 for the Equity Law Life Assurance Society [Collingwood, 1966a, 587] and [Archibald, 1936a, 101]. The first letter in the Cayley-Sylvester Correspondence [App.8] is dated 24 xi 1847.

29. Cayley referred to Rodrigues' paper on numerous occasions. He regarded it as treating the entire theory of rotation (finite and infinitesimal rotations) as well as the analytical theory of the resultant axis [1862a, CP4, 581]. See [Gray, 1980a] for commentary on Rodrigues' original paper.

30. The first chapters of Hamilton's Lectures on Quaternions were virtually taken verbatim from his 1848 lectures but the latter chapters contain material added after the lectures took place. The lectures were held on the 21, 23, 26 and 28 of June 1848.

Chapter 2

The legal profession - Quantics, Matrices and the Computational element (1850-1862)

2.1. Introduction

By the beginning of the 1850s Britain was fast becoming an industrialised nation. This was no better symbolised than by the 1851 Exhibition, an event which set forth the country's leading position in the world. The increasing use of technology and the rapid improvement in communication brought by the spread of the railway system increased the tempo of change begun in the previous decade. Cayley pursued his mathematical interests against a background of security and relative financial stability. He was called to the Bar on 3rd May 1849 at the age of twenty seven.

By the time Cayley qualified as a barrister-at-law he had secured a reputation as one of the country's outstanding mathematicians. At the time of his election to a Fellowship of the Royal Society in June 1852 at the age of thirty, he had published over a hundred papers on mathematics.

A few years earlier he had met J.J.Sylvester and by the beginning of the decade their mathematical partnership began to flourish. They had similar mathematical interests and professionally both had studied for the Bar at the same time. Sylvester did not practise Law (though he was admitted to the Bar in 1850) preferring to continue as an Actuary for the Equity and Law Life Assurance Society. Cayley took up the Law as a profession and acted as a Conveyancing Barrister at Lincoln's Inn.

During the early years of their partnership the foundations were laid for much of their later work and the early part of this decade is judged (by Muir for example) to be one of their creative periods. During the 1850s Cayley produced work which from a retrospective viewpoint was 'ahead of its time.' This is especially the case with his work on groups and matrices. However, the theory of forms proved to be his central interest in algebra, and with Sylvester, Cayley devoted much of his attention to this theory.

Cayley's initial contributions to the Invariant Theory in the 1840s had been fragmentary. Joined by Sylvester at the beginning of the 1850s Cayley's work on the subject gathered momentum. Mathematicians on the Continent, most notably Charles Hermite (1822-1901), Francesco Brioschi (1824-1897) and Siegfried Aronhold (1819-1884) made important discoveries [CP2, 598]. In England and Ireland, Boole, William Spottiswoode (1825-1883) and George Salmon (1819-1904) played a supporting role to the energetic researches of Cayley and Sylvester.

Boole, who had encouraged Cayley in the earlier period, made his final contributions in his [1851a] and [1851b]. Boole was primarily interested in taking a 'connected view of the methods and the results already obtained' [Boole, 1851a, 87]. In [1851a] Boole gave yet another method for calculating invariants. Boole's method was simpler in chosen instances than the excessively cumbrous method of 'hyperdeterminant derivation' but was not universally applicable. He applied his method to the binary quintic and found an invariant $\Theta(Q)$ of degree 8. But the method did not yield a practical means for transforming equations of the fifth degree, as he had previously hoped in his [1841b].

2.2. Cayley, Sylvester and further determinantal generalisations

Sylvester, in his long paper On the Principles of the Calculus of Forms [1852a] vividly surveyed the subject, proposed generalisations and introduced terminology. But it was Cayley who provided the subject with a sound foundation in his much praised memoirs on quantics. The first memoir was published in 1854 and the last in 1878. These papers (to be discussed) represent the bulk of Cayley's work in Invariant Theory. Like both Boole and Sylvester, he was interested in a 'connected' view of the subject, and in the first memoir proposed a new basis for it. Before the memoirs on quantics are considered, Cayley's work in the Theory of Invariants published prior to the introduction of his first memoir [1854c] is considered. In this prelude there is occasion to remark briefly on the Cayley-Sylvester partnership.

In the early 1850s Sylvester independently found methods for generating invariants. Two of these methods were closely connected with Cayley's own methods, though it is apparent that Sylvester came to them from a different angle. In Cayley's [1845b] it was shown that though the Theory was based on a system of partial differential equations, invariants were actually found using 'cubic determinants.'

Sylvester found his own combinatorial method for generating invariants and naturally his combinatorial method had something in common with Cayley's 'cubic determinant' method.

In the other more important method, Sylvester found that invariants could be considered as solutions to partial differential equations. In this he was anticipated by Cayley who found a new approach to finding invariants of a binary form through two simple partial differential equations.

Firstly, Sylvester's combinatorial approach to the problem of finding invariants is considered. Sylvester made many brilliant discoveries in the theory of forms at the beginning of the decade. One such discovery was in the reduction of algebraic forms to canonical form and it was in a preliminary paper on canonical forms that he first referred to his discovery of a combinatorial method for

finding invariants. This he called 'a process of Compound Permutation' [SP1, 185] .
At the time, Sylvester was working on a host of other problems, the problem of canonical forms, the classification of the intersections of conics and the Theory of Determinants. But the method of compound permutation is promised:

> I have succeeded using an umbral notation in reducing to a mechanical method of compound permutation the process for the discovery of these memorable forms invented by Mr.Cayley, and named by him hyperdeterminants, which have attracted the notice and just admiration of analysts all over Europe, and which will remain a perpetual memorial, as long as the name of algebra survives, of the penetration and sagacity of their author.
>
> [SP1, 251]

After a few months, Sylvester published his work on the method of compound permutation, but before this happened, Cayley published his own thoughts on the method. The essence of Cayley's ideas was contained in his [1843a] , but in his [1852a] the ideas were extensively generalised.

Cayley's generalisations of the ordinary determinant made in the preceding decade were brought together in one grand conception - the Permutant. This idea, of astonishing generality, was prompted by the theory of forms.

The basic expression was the simple permutant, for, as was shown in [1852a] , a more general compound permutant could be written as the sum of simple permutants in much the same way that the cubic determinant could be written as the sum of ordinary determinants. (Chap.1, page 16). How permutants are derived from forms, Cayley explained in the following way:

> A FORM may be considered as composed of blanks which are to be filled up by inserting in them specialising characters, and a form the blanks of which are so filled up becomes a symbol. We may for brevity speak of the blanks of a symbol in the sense of the blanks of the form from which such symbol is derived. Suppose the characters are 1, 2, 3, 4 ..., the symbol may always

> be represented in the first instance and without reference to the nature of the form, by V_{1234} And it will be proper to consider the blanks as having an invariable order to which reference will implicitly be made; thus, in speaking of the characters 2, 1, 3, 4... instead of as before 1, 2, 4,.. [sic] the symbol will be V_{2134} instead of V_{1234} ... When the form is given we shall have an equation such as
>
> $$V_{1234} = P_{12} Q_3 R_4 \text{ or } = P_{12} P_{34} \ldots \&c$$
>
> according to the particular nature of the form.
>
> (...)
>
> The aggregate of the symbols which correspond to every possible arrangement of the characters, giving to each symbol the sign of the arrangement, [+ if the number of inversions of the characters is even, - if the number of inversions is odd] may be termed a [simple] Permutant
>
> [1852a; CP2, 16]

As an example of a permutant(V_{123}), where

$$V_{123} = a_1 b_2 c_3$$

is

$$(V_{123}) = a_1 b_2 c_3 + a_2 b_3 c_1 + a_3 b_1 c_2 - a_1 b_3 c_2 - a_2 b_1 c_3 - a_3 b_2 c_1$$

To indicate that some of the characters may be permuted between themselves, Cayley suggested the 'spatial' notation for a permutant as

$$\left(V_{\substack{\alpha \;\; \beta \;\; \gamma \ldots \\ \alpha' \;\; \beta' \;\; \gamma' \ldots \\ \vdots \;\;\; \vdots}} \right)$$

where the connection with the cubic determinant is apparent.

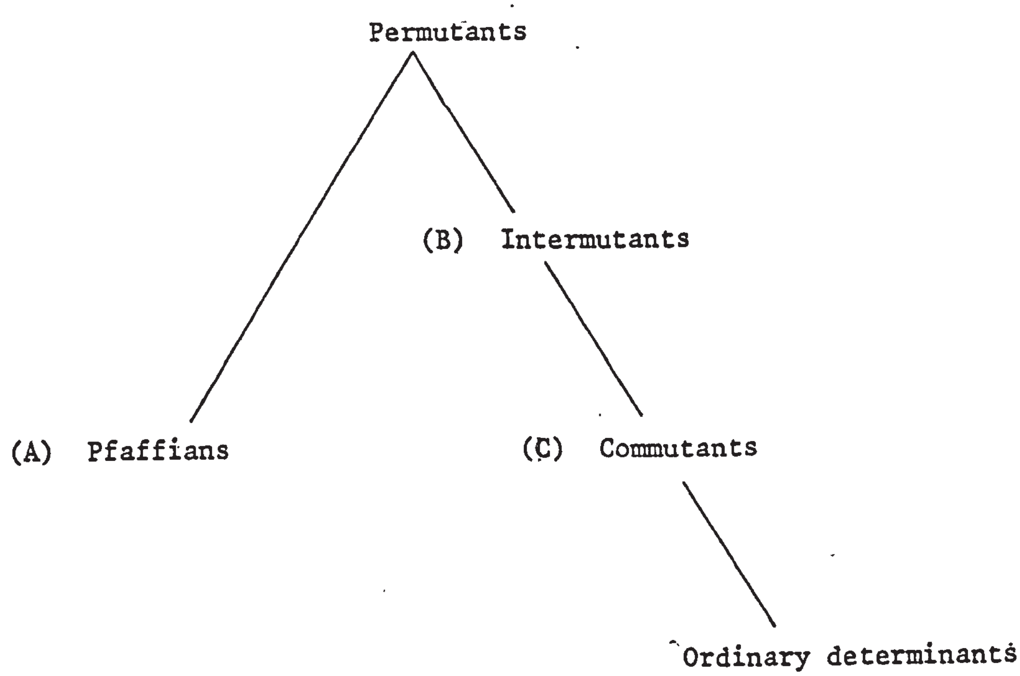
By specialisation, the other known algebraic forms could be obtained from the Permutant:

(A) The blanks [Positions to be occupied by characters] of a single set [Pfaffians] or of single sets [No name given] are situated in more than one column

(B) The blanks of each single set are situated in the same column [Intermutants, the earlier Hyperdeterminants]

(C) The blanks of each single set form a separate column. [Commutants or cubic determinants]

The 'family tree' of Permutants [Muir 1906a, vol.2, 267] shows their exceptionally general character when compared with the lowly common determinant.



The motivation for defining the permutant appears to be an attempt in designing a method for generating invariants in the same vein as the earlier cubic determinant.

As has been remarked, Sylvester was at the same time thinking along similar lines with a view to its application to the Theory of Invariants. This fact, coupled with the appearance of Cayley's paper, caused Sylvester to claim priority.

In the delicate question of ownership of mathematical ideas Sylvester felt that Cayley's discovery of the Permutant should be qualified. Even though Cayley and Sylvester worked in close collaboration, the line had to be drawn when questions of priority were at issue.
The subject was broached in a letter written to Cayley:

> As you appealed to me on the subject, I must say that I do not think that you are justified in publishing your views on the Method (as applied to Hyperdeterminants) of Compound Permutation of Umbral elements founded on or suggested by my communications to you on the subject which were meant as confidential, until I have first published my own account of the matter. It will then be right for you, to point out whatever part of the idea you may think is included in your former printed papers and to suggest any generalisations. As regards the principle of restricted permutations, I was aware of & even as you will see by my notes had enunciated the general notion thereof. Indeed I believe you acknowledge your inspirations on that point arose out of accidental observations on my part - but I owe to you the first simplified statement of its application in a particular case which however I repeat, it is quite certain from its direction my researches had taken, I must in algorithmizing the Permutant for the Hyphers of odd degrees have necessarily arrived at.
>
> To put out the method as your own and as only doing certain improvements of nomenclature to my suggestion would not I think be quite fair.
>
> [App. B, 20 iii 1851]

Cayley's reputation for being consistently fair in these matters is borne out by his reaction to Sylvester's letter. In an attempt to apportion his friend's contribution correctly, Cayley added a postscript to his paper on Permutants [1852a; CP2, 26].
In this Cayley referred to his own earlier paper on <u>Déterminants Gauches</u> [1848a] in which he mentioned a generalisation along the lines of a permutant though the idea was not developed at that stage [1848a; CP1, 411].

Sylvester discovered that the permutation method was wider in application than Cayley had at first thought.

Sylvester found that an invariant

$$ace + 2bcd - ad^2 - b^2e - c^3$$

of the binary quartic could be written as one of his commutants (Cayley's 'cubic determinant') , Cayley responded and found that the discriminant of the binary cubic, the familiar

$$a^2d^2 + 4ac^3 + 4b^3d - 3b^2c^2 - 6abcd$$

was not a commutant, but belonged higher up the permutant 'family tree' and was in fact an intermutant.

The importance of these determinant-like entities formed by permuting the suffices of symbols was that invariants could be obtained from them in the same way that some invariants could be written as ordinary determinants.

Evidently, Sylvester was not completely satisfied with Cayley's postscript. In a paragraph of his [1852a] Sylvester wrote:

> The commutants applied in the preceding theorems have been called by me total commutants, because the total of each line of umbrae is permuted in every possible manner. If the lines be divided into segments, and the permutation be local for each segment instead of extending itself over the whole line, we then arrive at the notion of partial commutants, to which I have also (in concert with Mr. Cayley) given the distinctive name of Intermutants. In order to find the invariants of functions of odd degrees, the theory of total commutants requires the process of commutation to be applied, not immediately to the coefficients of the proposed function, but to some derived concomitant form. I became early sensible of this imperfection, and stated to the friend above named, to whom I had previously imparted my general method of total commutation, my conviction of the existence of a qualified or restricted method of permutation, whereby the invariants of the cubic function, for instance, of two and of three letters would admit, without the aid of a derived form, of being represented. Many months ago, when I was engaged in this important research, and had made some considerable steps towards the representation of the invariant, that is, the discriminant of the cubic function of x and y, under the form of a single permutant, I was surprised by a note from the friend above alluded to, announcing that he had succeeded in fixing the form of the permutant of which I was at that moment in search. It is with no intention of complaining of this interference on the part of one to whose example and conversation I feel so deeply indebted, (and the undisputed author of the theory of Invariants,) that I may

> be permitted to say that, independent of the intervention of this communication, I must inevitably have succeeded in shaping my method so as to furnish the form in question; and that with greater certainty, after my theory of commutants had furnished me with the precedent of permutable forms giving rise to terms identical in value but affected with contrary signs.
>
> [1852a; SP1, 317]

After a further printed note from Cayley the matter was dropped. Sylvester frequently found himself involved in skirmishes in the journals but the slight altercation with Cayley concerning permutants was the exception rather than the rule. The importance they both attached to priority was one reason for their seeking publication at the first opportunity. Their partnership was not a collaboration in the true sense. Sylvester, in particular, appeared to require a firm arrangement, which made it clear how the rewards of partnership were to be divided. A modern arrangement such as a joint publication might have cleared some of the difficulties. However, joint papers were not undertaken in the middle of the nineteenth century, a period when 'individualism' was greatly admired. Sylvester and Cayley were individualists and though they helped each other and interchanged ideas, they really trod their own mathematical paths.

If Sylvester were on the point of solving a problem, he might even request Cayley not to send him material, for example:

> Pray do not send me (if you find it before me) the law of the development of
>
> $$\left(\frac{d}{dx}\right)^r \left(\frac{d}{dy}\right)^s$$
>
> as it will place me in an awkward position in publishing my memoir, if I appear compelled to borrow so essential a part of the investigation when I have obtained the solution which I consider myself on the [true?] road to obtain before long.
> I shall be delighted to [compare?] our respective methods & give all due honour & value to yours alongside of my own. With many parts of the subject claiming my attention at once it is of course impossible for me not to require time for doing the task which is set before me and to delay the consideration of some of them; add to this the impediments arising from the cares of life and business & not unfrequent fits of Disgust & tedium arising [from] long intervals of inactivity.
>
> [App. B, 2 xii 1854]

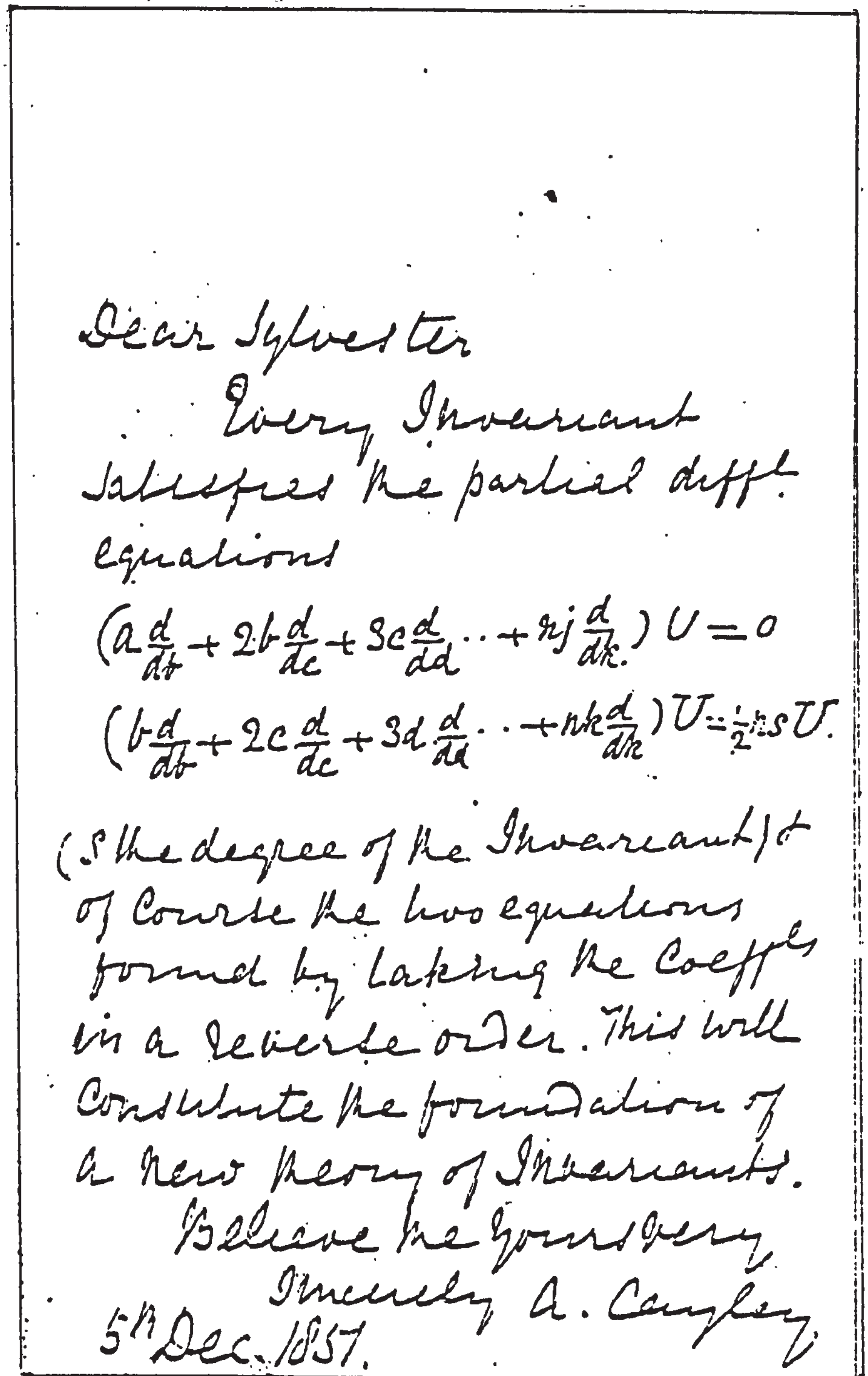
2.3. A new synthesis

In his [1845b] Cayley endorsed the 'partial differential equations' as a basis for Invariant Theory by stating that 'In every case it is from these equations that the form of the function u (an invariant) is to be investigated [1845b; CPI, 85] .

Apart from this reference, Cayley made no further mention of partial differential equations in the introductory papers and they did not provide the main method for the production of invariants during the 1840s. The principal method was the 'hyperdeterminant derivative' method as has been discussed previously. But it is a laborious method to operate, although it is capable of producing both invariants and covariants of algebraic forms [Elliott, 1964a, 107] . Cayley's early work chiefly dealt with 'constant derivatives' (invariants) although his use of this term showed he was aware of the more general functions (covariants) with the invariantive property. However, his original objectives for the infant Theory were so general that they easily took into account the possibility of 'non-constant derivatives.'

In the prelude to the Introductory Memoir on quantics [1854c], Cayley reconsidered the basis of the Theory. What is perhaps surprising was his intention to abandon the 'hyperdeterminant derivative' method and introduce partial differential equations as the basis for a new synthesis. The 'new' discovery of this synthesis he confided to his friend, Sylvester, in a letter (Plate 1) dated 5th of December 1851:

> Dear Sylvester,
> Every Invariant satisfies the partial diff[erential] equations
>
> $$\left(a\frac{d}{db} + 2b\frac{d}{dc} + 3c\frac{d}{dd} + \dots + nj\frac{d}{dk}\right)U = 0$$
>
> $$\left(b\frac{d}{db} + 2c\frac{d}{dc} + 3d\frac{d}{dd} + \dots + nk\frac{d}{dk}\right)U = \tfrac{1}{2}nsU$$
>
> (s the degree of the Invariant) and of course the two equations found by taking the coeffs in a reverse order. This will constitute the foundation of a new theory of Invariants.
>
> Believe me, yours very sincerely,
>
> A. Cayley.
>
> [App. B, 5 xii 51]



Dear Sylvester

Every Invariant satisfies the partial diff^{l}. equations

$$\left(a\frac{d}{db} + 2b\frac{d}{dc} + 3c\frac{d}{dd} \cdots + nj\frac{d}{dk}\right)U = 0$$

$$\left(b\frac{d}{db} + 2c\frac{d}{dc} + 3d\frac{d}{dd} \cdots + nk\frac{d}{dk}\right)U = \tfrac{1}{2}ns\,U.$$

(s the degree of the Invariant) & of course the two equations formed by taking the coeff^{ts} in a reverse order. This will constitute the foundation of a new theory of Invariants.

Believe me Yours very sincerely A. Cayley

5th Dec. 1851.

Plate 1: Letter [first page] from Cayley to Sylvester on the partial differential operator method. [App B, 5 xii 1851]
Original held at St. John's College, Cambridge [Sylvester Papers].

He had found a method [1854c; CP2,225] akin to the partial differential equations given in [1845b] but he did not give these specific equations in his [1845b] or [1846b].

The pair of differential equations given in the letter are not of equal status. The second equation merely expresses the fact that an invariant of degree s for a binary form of order n is of constant weight (weight of an invariant is equal to $\frac{1}{2}ns$) and is a consequence of Euler's theorem on homogeneous functions.

The important equations for binary forms are the first equation

$$\left(a\frac{d}{db} + 2b\frac{d}{dc} + 3c\frac{d}{dd} + \cdots + nj\frac{d}{dk}\right)U = 0$$

and the equations formed by taking the coefficients in reverse order

$$\left(k\frac{d}{dj} + 2j\frac{d}{di} + 3i\frac{d}{dh} + \cdots + nb\frac{d}{da}\right)U = 0$$

The equations written above apply only to invariants. But both Cayley and Sylvester were able to extend these considerations to the notion of covariants. Here the corresponding equations for a covariant U of a binary form are:

$$\left(a\frac{d}{db} + 2b\frac{d}{dc} + 3c\frac{d}{dd} + \cdots + nj\frac{d}{dk} - y\frac{d}{dx}\right)U = 0$$

$$\left(k\frac{d}{dj} + 2j\frac{d}{di} + 3i\frac{d}{dh} + \cdots + nb\frac{d}{da} - x\frac{d}{dy}\right)U = 0$$

The subtraction of the differential operators $y\frac{d}{dx}$ and $x\frac{d}{dy}$ involving the variables of the form ensure that a covariant is reduced to zero by the 'generalised' operators.

In the new synthesis, the partial differential equations method written above represented an advance in that they applied to covariants where the earlier differential equations were only capable of dealing with invariants [1854c; CP2,225].
What caused Cayley to abandon his earlier hyperdeterminant derivation method? MacMahon merely remarked that Cayley 'had little fancy for the hyperdeterminant derivation method, which, dropping from his hands, was carried on with great results by the mathematicians of Germany' [MacMahon, 1896a, 7].
But Cayley himself gave a small but useful clue in answering this question. The reason goes back to his intention of calculating the invariants and covariants. In his [1854b], the paper where he announced the new method, he noted that from the partial differential equations 'one finds quite easily the covariants by the method of undetermined coefficients' [1854b; CP2, 167].
Cayley did not drop the 'hyperdeterminant derivation' method on a suspicion of its theoretical weakness. He knew that it could be made a proper basis for the theory when he remarked that 'the method (hyperdeterminant derivation) appears to be the appropriate one for the treatment of the theory of the invariants or covariants of any degree whatever' but he added a rider which provided the clue for letting the derivational method fall into abeyance 'the application of it becomes difficult when the degree exceeds 4' [1858d; CP2, 516-517].[2]

Thus for Cayley, even though the 'hyperdeterminant method' was theoretically able to produce all invariants and covariants of an algebraic form, Cayley did not regard it as a proper basis for the theory because of its inefficiency as a method of calculation. The German mathematicians did use the derivational method but not to calculate the expressions for invariants and covariants in their full Cartesian form as did Cayley. They pursued a more abstract method.

The partial differential equations

Though the differential equations obtained by Cayley and Sylvester were similar it is interesting to note that they came to their ideas through entirely different considerations. Cayley's thinking is entirely algebraic (a retrospective explanation given while completing the Collected Papers in 1889):

> I believe I actually arrived at the notion by the simple remark, say that $a\partial_b + 2b\partial_c$ operating upon $ac - b^2$ reduced it to zero, and that the same operation performed upon $ax^2 + 2bxy + cy^2$ reduced it to $2axy + 2by^2$ which is $= y\partial_x\{ax^2 + 2bxy + cy^2\}$
>
> [CP2, 600]

Sylvester's discovery of the differential equations seemed to rely less on a formal observation. His reasoning commenced with the original meaning of an invariant. He described the underlying idea as one of continuous or infinitesimal variation and wrote:

> Again suppose that C [a function of coefficients of $\phi(x, y, z)$] alters neither when x receives such infinitesimal increment, y and z remaining constant, nor when y and z separately receive corresponding increments z, x and x, y in the respective cases remaining constant ... C will remain constant for any concurrent linear transformation of x, y z when the modulus is unity.
>
> [1852a; SP1, 326]

At the time Sylvester wrote these words he did not describe them further as he was aware of Cayley's priority and wished to allow his friend to state his results publicly.

Cayley dispatched his results to the editor of Crelle's Journal on 23 February 1852 and they appeared in his [1854b]. The paper contained as its essential result a statement of the fundamental theorem: the necessary and sufficient condition for ϕ to be a covariant of a binary form is that it satisfies the two partial differential equations. Cayley provided a proof of it and outlined its usefulness in the Theory of Invariants.

Sylvester's derivation of the two partial differential equations, unlike Cayley's formal observation, was based on the original meaning of an invariant.

Letting

$$\phi = ax^n + nbx^{n-1}y + \tfrac{1}{2}n(n-1)cx^{n-2}y^2 + \cdots + nb'xy^{n-1} + a'y^n$$

Sylvester made the substitution $x + ey$ for x with y unchanged.

ϕ is thus transformed to

$$(a+\Delta a)x^n + n(b+\Delta b)x^{n-1}y$$

$$+\tfrac{1}{2}n(n-1)(c+\Delta c)x^{n-2}y^2 + \ldots$$

$$(b'+\Delta b')xy^{n-1} + (a'+\Delta a')y^n$$

where

$$\Delta a = 0,\ \Delta b = ae,\ \Delta c = 2be + ae^2, \ldots$$

Taylor's Theorem applied to a function

$$I(a, b, c, \ldots, b', a')$$

implies

$$\Delta I = \left(\Delta a \frac{d}{da} + \Delta b \frac{d}{db} + \cdots\right) I$$

$$+ \frac{1}{2!}\left(\Delta a \frac{d}{da} + \Delta b \frac{d}{db} + \cdots\right)^2 I$$

$$+ \frac{1}{3!}\left(\Delta a \frac{d}{db} + \Delta b \frac{d}{db} + \cdots\right)^3 I + \cdots = 0$$

which is identically zero for all values of e in the case I is an invariant [$\Delta I = 0$ for an invariant]. The coefficients of $e, e^2, \ldots$ are hence zero. In particular the coefficient of e meant that:

$$\left(a\frac{d}{db} + 2b\frac{d}{dc} + \cdots\right) I = 0$$

[1852a; SP1, 353]

and by considering the end coefficient:

$$\left(a' \frac{d}{db'} + 2b' \frac{d}{dc'} + \cdots \right) I = 0$$

[1852a; SP1, 355]

Sylvester's insight was explained in terms of two and three variables and a general conclusion:

> And in general for a function of m variables, in partial differential equations similarly constructed (but not however arbitrarily selected) will be necessary and sufficient to determine any invariant: and it is clear that all the general properties of invariants must be contained in and be capable of being educed out of such equations.
>
> [1852a; SP1, 356]

Cayley emphasized the partial differential equations as the foundation of the subject and noted that the earlier 'hyperdeterminant derivative' method of calculation was subsumed under this new synthesis. Cayley did not dwell on the primitive notion of the invariant as an 'unchanging function' after a linear substitution of the variables of the algebraic form. Under the new synthesis, invariants and covariants had a modern ring: they were functions annihilated (to use Sylvester's terminology) by partial differential operators.

In the 'hyperdeterminant derivation' method covariants were obtained by a process of differentiation. From the partial differential equations, the covariants are in a sense integrals and Cayley used this terminology [1856a, 101; CP2, 250]. With his customary enthusiasm, Sylvester later in life reflected on the partial equation method as 'an engine that mightiest instrument of research ever yet invented by the mind of man - a Partial Differential Equation, to define and generate invariantive forms.' [1886b; SP4, 294].

The Introductory Memoir on Quantics [1854c]

Cayley's memoirs on quantics were written between 1854 and 1878. The first seven memoirs appeared between 1854 and 1861 and the last three rather later, 1867, 1871 and 1878. Even with the completion of the series, Cayley continued work on the subject up to the last years of his life.

The memoirs on quantics were from the outset so highly specialised and technically difficult as to put them beyond the immediate circle of those not fully involved with the research. This was the view of J.T.Graves on being invited to referee the Introductory memoir, noted its abstract character and the introduction of new terminology.
The opening paragraph to the first memoir indicated the intended scope of the series:

> The term Quantics[3] is used to denote the entire subject of rational and integral functions, and of the equations and loci to which these give rise; the word "quantic" is an adjective, meaning of such a degree, but may be used substantively, the noun understood being (unless the contrary appears by the context) functions; so used the word admits of the plural "quantics".
>
> [1854c; CP2, 221]

This introduction underlined Cayley's main purpose in writing the series: to state afresh the Theory of Invariants and to unify it under the 'new' principles.

In the first two memoirs, Cayley explained the foundations of the subject and in this the calculus of differential operations played a key role. The important result shown in the early part of the memoir is that invariants and covariants by the 'hyperdeterminant derivative' method were invariants and covariants with respect to the new method. This is shown by proving that the earlier method of writing an invariant:

$$A^p B^q C^r \ldots U_1 U_2 \ldots$$

is annihilated by the two differential operators, which in print, Cayley wrote as

$$\{x\partial_y\} - x\partial_y$$

$$\{y\partial_x\} - y\partial_x$$

Before proving the result, Cayley established properties of these differential operators. In doing this he did not axiomatise properties of general operators but used the properties of the specific differential operators and techniques familiar to practitioners of the Calculus of Operations. One such device deserves special mention as it recurs frequently in Cayley's work. He still used it in his last papers, for example in [1893a; CP13, 400]. This is the artifice whereby a symbol was allowed to operate on only part of the operand.[4]

Cayley's introduction to this technique in the particular case of a linear function was:

> In particular if P be a linear function of $\partial_a, \partial_b \ldots,$ we have to replace P by $P + P_1$, where P_1 is the same function of $\partial_a', \partial_b', \ldots$ that P is of $\partial_a, \partial_b, \ldots$ and it is therefore clear that we have in this case
>
> $$P.Q = PQ + P(Q)$$
>
> where on the right-hand side in the term PQ the differentiations $\partial_a, \partial_b, \ldots$ are considered as not in anywise affecting the symbol Q, while in the term $P(Q)$ these differentiations, or what is the same thing, the operation P, is considered to be performed upon Q as operand.
>
> [1854c; CP2, 226]

In the symbolic equation

$$P.Q = PQ + P(Q)$$

the left hand side $P.Q$ is the modern composition of operators with Q operating first, followed by P. The first term of the right hand side is the ordinary product of the two operators multiplied as ordinary algebraic quantities. The term $P(Q)$ is the result of applying the operator P to Q where Q is considered as an operand. This symbolic equation was seen as a splitting of composition of two operators into a commutative part and a non-commutative part.

By reversing the symbols P and Q to obtain

$$Q.P = QP + Q(P)$$

Cayley obtained the key result

$$P.Q - Q.P = P(Q) - Q(P)$$

These considerations bring out an important feature of this work on the Calculus of Differential Operations: the two roles played by symbols used in the calculus. At this time Cayley was just aware that a symbol of operation could be interpreted as an operator or as an operand. The newness of the idea of allowing an operator to operate on another operator (and how Cayley dealt with it)[5] is seen in his [1857d] :

> A SYMBOL such as
>
> $$A\partial_x + B\partial_y + \cdots$$
>
> where A , B &c. contain the variables x , y , &c. in respect to which differentiations are to be performed, partakes of the natures of an operand and operator, and may be therefore called an Operandator.
>
> [1857d; CP3, 242]

Having established some of the elementary properties of differential operators, Cayley used these to establish the first important result in the new synthesis for Invariant Theory. This asserted that the 'hyperdeterminant derivation' method is subsumed under the new synthesis. But Cayley did not show the two main methods of obtaining invariants and covariants were equivalent.

The <u>Introductory Memoir</u> was completed with a postscript which contained important implications for the later development of the Theory:[6]

POSTSCRIPT added October 7th, 1954 - I have, since the preceding memoir was written, found with respect to the covariants of a quantic

$$(*\between x, y)^m,$$

that a function of any order and degree in the coefficients satisfying the necessary condition as to weight, and such that it is reduced to zero by one of the operations

$$\{x\partial_y\} - x\partial_y \quad , \quad \{y\partial_x\} - y\partial_x$$

will of necessity be reduced to zero by the other of the two operations, i.e. it will be a covariant; and I have been thereby led to the discovery of the law for the number of asyzygetic covariants of a given order and degree in the coefficients.

[1854c ; CP2, 234]

2.4. Calculation of covariants and the finiteness question

In the problem of calculating the complete system for a binary quantic only the simplest cases - the binary quadratic, cubic and quartic - can be judged to be straightforward. In the case of the binary cubic

$$U = (a, b, c, d)(x, y)^3$$

there is a single invariant

$$\begin{aligned} \nabla &= (ad - bc)^2 - 4(ac - b^2)(bd - c^2) \\ &= a^2d^2 - 6abcd + 4ac^3 + 4b^3d - 3b^2c^2 \end{aligned}$$

and two covariants:

$$H = (ac - b^2)x^2 + (ad - bc)xy + (bd - c^2)y^2$$

This is the 'Hessian' covariant and is obtained from the expression

$$\begin{vmatrix} \dfrac{\partial^2 U}{\partial x^2} & \dfrac{\partial^2 U}{\partial x \partial y} \\[2ex] \dfrac{\partial^2 U}{\partial x \partial y} & \dfrac{\partial^2 U}{\partial y^2} \end{vmatrix}$$

The other is:

$$\Phi = (a^2d - 3abc + 2b^3, abd - 2ac^2 + b^2c, -acd + 2b^2d - bc^2, -ad^2 + 3bcd - 2c^3)(x, y)^3$$

The irreducible forms[7] U, ∇, H, Φ constitute a complete system for the binary cubic because every other covariant of the binary cubic is a rational and integral function of these four covariants. Cayley was not only interested in calculating the complete set of covariants but also in the covariants which could be formed from the complete set. The product of any two covariants is a covariant. So, for example,

$$U^2\nabla \ , \ \Phi^2 \ , \ H^3$$

are covariants of degree 6 and order 6 formed from the complete set. Cayley was interested in calculating the number of linearly independent (asyzygetic) covariants which existed for each degree and order. This question was complicated by the existence of linear dependences between the covariants. For instance, there is one linear dependency between

$$U^2\nabla \ , \ \Phi^2 \ , \ H^3$$

$$U^2\nabla = \Phi^2 + 4H^3$$

which reduces the number of linearly independent covariants of degree 6 and order 6 to only two.

Although the general aims of the Theory were clear enough, they were far too general to be successfully executed. Cayley's implicit objective became more specific than the embracing objective in [1846b]. The practical objective can be summed up in two parts:

(a) The determination of the complete system of irreducible covariants for a single quantic, the most important case being the simplest case: the binary quantic.

(b) To determine the dependences between covariants.

From (a) two subsidiary questions arose:

(i) Given a binary quantic of order n, what is the rule for stating the number of linearly independent covariants of a prescribed degree, and order?

(ii) Is the total number of irreducible covariants of a complete system finite?

Answers to (i) and (ii) were given by Cayley in the Second Memoir on Quantics [1856a] . In considering Cayley's work in the calculation of covariants this paper is the most important. (The Sixth Memoir [1859a] is perhaps the best known because it contains the application of the theory of quantics to Projective Geometry). One referee for Cayley's [1856a] was G.G.Stokes who reported:

> I had to devote many a long day's labour to getting up on the subject generally before I felt myself in a position at all competent to take a broad view of that wide untrodden field opened up to us by the researches of Messrs. Cayley, Sylvester, Spottiswoode and Boole.
>
> [Royal Society of London, RR.2. 45-46]

As is well known, Cayley gave an incorrect answer to the finiteness question (ii). For binary quantics of certain degrees, he drew the conclusion that the fundamental system of covariants was infinite. But in the combinatorial question of ascertaining the number of linearly independent covariants, Cayley was spectacularly successful. For this Law an important detail was left unproved, but this omission did not prevent him from basing much of his later work on its validity. It is this result which became known as Cayley's Law and is half of the main theorem which formed the centrepiece of his Second Memoir.

The main theorem

Cayley could hardly disguise his pleasure when he wrote to Sylvester announcing his success in finding the Law, for as he had earlier remarked, this was a 'problème qui a toujours bravé mes efforts' [1854b;CP2, 167] :

> Dear Sylvester,
>
> Eureka. Let
> $$(a, b, c, \ldots \between x, y)^n$$
> be a quantic. I consider the coeffts a, b, c.. as being of the weights[9]
> $$-\tfrac{1}{2}n, \quad 1-\tfrac{1}{2}n \quad \&c$$
> and x, y of the weights $\frac{1}{2}$,$-\frac{1}{2}$; every covariant is of the weight 0.

Write

$$\{x\partial_y\} = nb\partial_a + (n-1)c\partial_b + \&c = Y$$ suppose

$$\{y\partial_x\} = a\partial_b + 2b\partial_c + \&c = X$$

and let A be a rational and integral homogeneous function of the coefficients of the weight $-\frac{1}{2}s$

[The argument used by Cayley to prove this most important theorem now follows. It is based on the properties of the differential operators X and Y defined above. In terms of modern theory A is a characteristic vector though Cayley did not abstract the concept of characteristic vector and pursue its theoretical implications].

Then it is easy to see that

$$(XY - YX)A = sA$$

and substituting for A -

$$XA, X^2A \text{ [sic] } \&c$$

which are of the weights

$$1-\tfrac{1}{2}s, \; 2-\tfrac{1}{2}s \quad \&c$$

we derive

$$(XY-YX)A = sA$$
$$(XY-YX)YA = (s-2)YA$$
$$(XY-YX)Y^2A = (s-4)Y^2A \quad \&c$$

If then $XA = 0$ we have $XYA = sA$, the second equation becomes

$$XY^2A = Y.XYA + (s-2)YA$$
$$= sYA + (s-2)YA \quad \&c$$ viz.

$$XYA = sA$$
$$XY^2A = 2(s-1)A$$
$$\vdots$$
$$XY^{\theta+1}A = (\theta+1)(s-\theta)A$$

$\theta = s, s+1$ &c gives

$$XY^{s+1}A = 0$$

$$XY^{s+2}A = -(s+2).1.Y^{s+1}A$$

$$XY^{s+3}A = -(s+3).2.Y^{s+2}A$$

&c

Now suppose if possible

$Y^{s+1}A \neq 0$ then $XY^{s+2}A \neq 0$ &

$$\therefore Y^{s+2}A \neq 0$$

Consequently

$$XY^{s+3}A \neq 0$$

and therefore

$$Y^{s+3}A \neq 0$$

and so on <u>ad infinitum</u>

[which] is absurd for $Y^{s+\theta}A$ which is of constant degree and of a continually increasing weight must end with the term l^{θ} where l is the last of the coef^ts a, b, c ... and θ is the degree of A. Hence

$$Y^{s+1}A = 0$$

and this once proved we have [see [1856a; CP2,254-256] for complete details of the proof] .

<u>Theorem</u> If A be of the weight $-\frac{1}{2}s$ and satisfy the <u>single</u> equation $XA = 0$ then a covariant is

$$\left(A, YA, \frac{Y^2A}{1.2}, \ldots \right)(x, y)^s$$

Suppose that A is of the degree θ in the coefficients and take for A the most general form of the degree θ and weight $-\frac{1}{2}s$, or what is the same thing, reckoning the weights a, b, c as 0, 1, 2 &c take for A the most general form of the degree and weight $\frac{1}{2}(n\theta - s)$.

Then XA will be a form of the degree

θ and weight $\frac{1}{2}(n\theta-s)-1$;

and putting $XA=0$ the coefficients of A satisfy a certain number of linear equations there is no reason for doubting that these equations are independent - and if so The number of asyzygetic covariants

$((a,b,c..)^\theta(x,y)^s$ = No. of terms degree θ, weight $\frac{1}{2}(n\theta-s)$

<u>less</u>

No. of terms degree θ, weight $\{\frac{1}{2}(n\theta-s)+1\}$ [sic]

which is I believe the law for the number of asyzygetic covariants of a given order, and degree in the coefficients.

[App. B, late 1854 - early 1855; date estimated]

The importance of the theorem was two-fold. Not only did it count the number of linearly independent covariants, but it also provided a formula for finding these covariants. The formula made it clear that a covariant was determined by its leading term (later called a seminvariant). It is the formula used by Cayley to calculate his extensive Covariant Tables. That the formula was used by Cayley and Sylvester in all their subsequent work is confirmed by Sylvester in [1878h; SP3, 152].
The practical aspect of Cayley's method is illustrated by two simple examples associated with the binary cubic. In the 1850s Cayley was concerned with finding the invariants and covariants of low order forms so that these examples are by no means artificial at this point in the history.

EXAMPLE 1

Consider Cayley's problem of finding the covariant of order 2 and degree 2 for the binary cubic [1854b; CP2, 169]

In Cayley's notation the cubic form is written

$$(a, b, c, d \between x, y)^3 \qquad (n=3)$$

and the form of the covariant as

$$(A, B, C \between x, y)^2$$

(order s = 2, degree θ = 2)

The weight of A is $\frac{1}{2}(n\theta - s) = 2$ and hence A must be a linear combination of the elements ac, b^2 (the only products of weight 2)

$$\alpha\, ac + \beta b^2$$

By operating on this 'trial solution' with

$$X = a\partial_b + 2b\partial_c + 3c\partial_d$$

it follows that $-\alpha = +\beta$ and A takes the form (to a multiplicative factor)

$$A = ac - b^2$$

Using the operator $Y = 3b\partial_a + 2c\partial_b + d\partial_c$ and evaluating YA and $\frac{1}{2!}Y^2A$ the required covariant is found:

$$(A, YA, \tfrac{1}{2!}Y^2A \between x, y)^2 = (ac-b^2, ad-bc, bd-c^2 \between x, y)^2.$$

Example 1 shows how Cayley's algorithm can be used to evaluate a covariant for any binary form. First, the leading term is computed using the operator X and the remaining terms are computed by the repeated application of Y.

In Example 2 to follow, the simpler problem of computing an invariant is considered. This is a simpler task since there is no need to employ the operator Y. Example 2 (again for the binary cubic) illustrates the linear equations which Cayley referred to in his letter when he said 'there is no reason for doubting that these equations are independent.' That 'these equations' are in fact linearly independent was not proved by Cayley.[10]

EXAMPLE 2

There is only one invariant of the binary cubic and this is of degree 4. The parameters are $n = 3$, $\theta = 4$, $s = 0$.

The weight of the invariant is $\frac{1}{2} n\theta = \frac{1}{2} 3.4 = 6$

As the weights of the coefficients a, b, c, d are respectively 0, 1, 2, 3, the terms of the required products correspond to the partitions of 6 into four parts:

$0+0+3+3$	a^2d^2
$0+1+2+3$	$abcd$
$0+2+2+2$	ac^3
$1+1+1+3$	b^3d
$1+1+2+2$	b^2c^2

Thus the required invariant has the form

$$\alpha a^2d^2 + \beta abcd + \gamma ac^3 + \delta b^3d + \varepsilon b^2c^2$$

and the coefficients

α, β, γ, δ and ε

are determined by operating on this ' trial solution' by the annihilating operator:

$$X = a\partial_b + 2b\partial_c + 3c\partial_d$$

From this Cayley obtained his 'linearly independent' linear equations:

$$\begin{aligned}
6\alpha + \beta \qquad\qquad\qquad &= 0 \\
2\beta + 3\delta \qquad\qquad &= 0 \\
3\beta \qquad + 6\gamma + 2\varepsilon &= 0 \\
3\delta \qquad + 4\varepsilon &= 0
\end{aligned}$$

By taking the value of α to be unity, and solving the equations, the required invariant

$$a^2d^2 - 6abcd + 4ac^3 + 4b^3d - 3b^2c^2$$

is obtained.

<u>Arbogast's Rule</u>

To produce systematically the 'trial solution' in readiness for the application of the differential operator, Cayley made use of Arbogast's Method of Derivations.[11] Its value in the Theory of Invariants was as an algorithmic device from which combinations of symbols of the correct weight could be quickly computed. Using Arbogast's Rule, Cayley produced a table where the columns (reading from the left) contain elements of the same weight and where the columns are arranged (reading from left to right) in ascending weights. See Table 1 for the binary cubic.

[Weight]	0	1	2	3	4	5	6	7	8	9	10	11	12
	a^4	a^3b	a^3c	a^3d	a^2bd	a^2cd	a^2d^2	abd^2	acd^2	ad^3	bd^3	cd^3	d^4
			a^2b^2	a^2bc	a^2c^2	ab^2d	$abcd$	ac^2d	b^2d^2	bcd^2	c^2d^2		
				ab^3	ab^2c	abc^2	ac^3	b^2cd	bc^2d	c^3d			
					b^4	b^3c	b^3d	bc^3	c^4				
							b^2c^2						

Table I [1856a, 108; CP2, 258]

Table of elements of degree 4 for the binary cubic obtained from Arbogast's rule of the 'last and last but one'

[The arrows have been added to show Arbogast's Rule in operation. The horizontal arrows show the effect of operating on the 'last' letter while the diagonal arrows show the effect of operating on the 'last but one' letter; the terms in the column of weight 6 are the terms required for the trial solution which gives the invariant of the binary cubic] .

How Arbogast's Rule is used mechanically to obtain the table is explained by Cayley:

> To derive any column from the one which immediately precedes it, we operate on a letter by changing it into its immediate successor in the alphabet, and we must in each term operate on the last letter, and also, when the last but one letter in the term is the immediate antecessor in the alphabet of the last letter (but in this case only), operate on the last but one [different] letter. Thus a^3c gives a^3d but a^2b^2 gives a^2bc and ab^3.
>
> [1861a; CP4, 265]

<u>Counting Invariants and Covariants — Cayley's Law</u>

The actual counting of linearly independent asyzygetic invariants proceeded from the main theorem by an application of the theory of partitions. The part of the main theorem concerned with 'counting' is simply stated:

> <u>CAYLEY'S LAW</u>
>
> For a quantic of order n, the number[12] of asyzygetic covariants of degree θ <u>and order s</u> is
>
> $$P(0,1,2,\dots,n)^{\theta}\tfrac{1}{2}(n\theta-s)-P(0,1,2,\dots,n)^{\theta}\left(\tfrac{1}{2}(n\theta-s)-1\right)$$
>
> <u>where</u> $P(0,1,2,\dots n)^{\theta}q$ is the number of ways in which q can be written as the sum of θ or fewer terms with the elements $1,2,\dots n$ and each element occurring any number of times in the sum [1856a; CP2, 265].

Cayley's Law illustrates the combinatorial element in the problem of computing the invariants and covariants and this problem stimulated both Cayley and Sylvester to develop partition formulae alongside their work in Invariant Theory.
An example illustrates the notation and the working of Cayley's Law:

EXAMPLE

The actual partitions of 10 into 5 or fewer parts where the numbers are restricted to 1,..., 7 are:

37	127	1117	1234	11116
46	136	1126	1333	11125
55	145	1135	2224	11134
	226	1144	2233	11224
	235	1225		11233
	244			12223
	334			22222

and the number of these 'restricted' partitions

$P(0,1,\ldots,7)^5 10 = 26$

This calculation is used in an application of Cayley's Law (and which is needed later) in which the parameters are $n = 7, \theta = 5, s = 13$.

By Cayley's Law, the number of linearly independent covariants of degree 5 and order 13 for a binary form of order 7 is:

$$P(0,1,\ldots,7)^5 11 - P(0,1,\ldots,7)^5 10 = 30 - 26 = 4.$$

The Law was also used in an ingenious way to show the existence and irreducibility of covariants. The technique used by Cayley to discover the covariants and show that they were irreducible may be understood with reference to the binary cubic. For instance, how many covariants are there of degree 3 and order 3? By Cayley's Law the number of linearly independent covariants of this degree and order is

$$P(0,1,2,3)^3 3 - P(0,1,2,3)^3 2 = 3 - 2 = 1.$$

Composite covariants (formed from products of irreducible covariants already found) are not possible for this degree and order (because sums of degree and order in products must respectively equal 3, and 3) and therefore the single covariant already found must be irreducible.

The formula for the number of covariants of a quantic naturally led Cayley to consider Euler's generating functions. As well as a means of calculation, generating functions provided important clues to the existence of specific covariants and the syzygies which exist between them. The generating functions used in the 1850s were basic in comparison with the refined functions used in the 1870s. Their first application also led Cayley into serious error in the answer he gave to the finiteness question. In the expression:

$$P(0,1,2,\ldots,n)^{\theta} q - P(0,1,2,\ldots,n)^{\theta}(q-1)$$

the first factor is given by the coefficient of $z^{\theta}x^{q}$ in

$$\frac{1}{(1-z)(1-zx)(1-zx^2)\ldots(1-zx^n)}$$

The second factor by the coefficient of $z^{\theta}x^{q-1}$ in the same expression or equally the coefficient of $z^{\theta}x^{q}$

in

$$\frac{x}{(1-z)(1-zx)(1-zx^2)\ldots(1-zx^n)}$$

The number of covariants is therefore the coefficient of $z^{\theta}x^{q}$ in

$$\frac{1-x}{(1-z)(1-zx)(1-zx^2)\ldots(1-zx^n)}$$

Much of Cayley's work in Invariant Theory dealt with methods of expanding these generating functions.

The binary quintic

The covariant systems for the quantics of order less than five were easily established. But the binary quantic of order five (the quintic) presented a problem of a higher order of difficulty. Many covariants were calculated for his [1856a] but the quintic occupied much of Cayley's attention during the following thirty years.

By the beginning of the 1850s, Cayley knew of three invariants for the quintic (of the degrees 4, 8,12) and he thought furthermore that the degrees of the invariants of the quintic were 'evenly even'. This belief was dispelled when Hermite produced [1854a] an invariant of degree 18, the famous skew invariant.

The 'step up' in the order of complexity with regard to the quintic might have lulled Cayley into accepting a result for the quintic which was out of line with the results for the lower order forms.[14] His conclusion for the quintic was that the fundamental covariant system was infinite. The argument was based on generating functions.

Cayley found that the number of irreducible covariants of degree for the binary quintic was the coefficient of x^{θ} in the expression:

$$\frac{1+x+x^2+4x^3+6x^4+8x^5+9x^6+10x^7+12x^8+10x^9+9x^{10}+8x^{11}+6x^{12}+4x^{13}+x^{14}+x^{15}+x^{16}}{(1-x^2)^2(1-x^4)(1-x^6)(1-x^8)}$$

The long expression for the numerator was then factorised into

$$(1+x)^2(1-x+2x^2+x^3+2x^4+3x^5+x^6+5x^7+x^8+3x^9+2x^{10}+x^{11}+2x^{12}-x^{13}+x^{14})$$

The first factor in this expression was replaced by

$$(1-x)^{-2}(1-x^2)^2$$

and the second factor by the infinite product

$$(1-x)(1-x^2)^{-2}(1+x^3)^{-3}(1-x^4)^{-2}(1-x^5)^{-2}\ldots$$

Cayley then observed that the coefficient of x^{θ} could be made arbitrarily large and the conclusion he reached was that the number of irreducible covariants for the binary quintic was infinite [1856a; CP2, 270].

From the letters which passed between Cayley and Sylvester at the time it is apparent that they both experienced difficulty in calculating the covariants for the quintic. But even believing that there was no finite system for the quintic did not halt Cayley in his programme of calculation. In the <u>Second Memoir on Quantics</u> [1856a] thirteen distinct irreducible covariants of the quintic were calculated.

In dealing with invariants (as distinct from covariants) of binary forms, Cayley experienced similar difficulties on the finiteness question:[15]

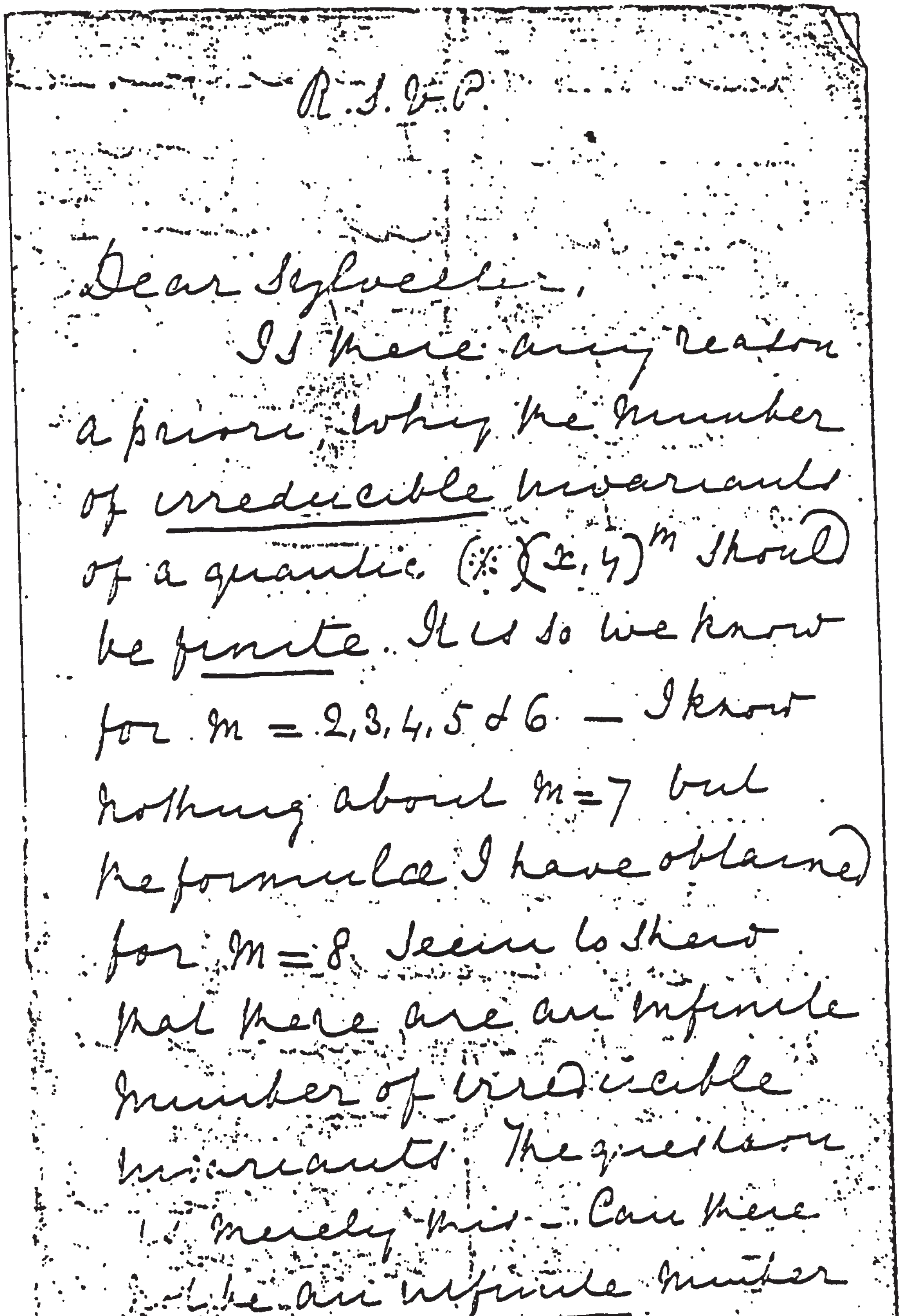
R.S.V.P.

Dear Sylvester,

Is there any reason a priori, why the number of <u>irreducible</u> invariants of a quantic $(*\between x,y)^m$ should be <u>finite</u>. It is so we know for m=2,3,4,5 & 6 - I know nothing about m=7 but the formulae I have obtained for m=8 seem to show that there are an infinite number of irreducible invariants. The question is merely this - Can there not be an <u>infinite</u> number [of] quantities I, rational functions of $m+1$ elements and such that any I is an irrational function of any $(m-2)$ I's say of $I_1\ I_2 \ldots I_{m-2}$ but so that there is no finite number of I's of which any other I <u>whatever</u> is a <u>rational</u> function.

My results for m=8 are I must confess of a very paradoxical form, I find that there is one irreducible invariant of each of the orders 2, 3, 4, 5, 6, 7, 8, 9, 10 one syzygetic equation of each of the orders 16, 17, 18, 19 one irreducible invariant of each of the orders 25, 26, 27, 28, & 29 which is as far as I have carried the development...

[App.B, [1854/55], year estimated]



R.S.V.P.

Dear Sylvester,
Is there any reason a priori why the number of irreducible invariants of a quantic $(*)(x, y)^m$ should be finite. It is so we know for m = 2, 3, 4, 5 & 6 — I know nothing about m = 7 but the formulae I have obtained for m = 8 seem to shew that there are an infinite number of irreducible invariants. The question is merely this — Can there be an infinite number

Plate 2: Letter [first page] from Cayley to Sylvester speculating on the number of irreducible invariants.
[App B, [October 1854] . Original held at St.John's College, Cambridge [Sylvester Papers] . The letter is undated but is likely to have been written between late 1854 and early 1855.

Covariant Tables

With the new synthesis for the theory Cayley did not delay in carrying out the calculations. In his [1845b] some tables had been given, but not in a systematic manner. In [1856a] he began to list his results for each binary quantic of order n=1,2,3,... His reasons for choosing the specific invariants and covariants to exhibit is indicative of his intention to catalogue. The choice is made on simplicity of form based on the alphabetic notation but is otherwise arbitrary:

> the following considerations seem to me to furnish a convenient rule for selection. Let the literal parts of the terms which enter into the coefficients of the highest power of x or leading coefficients be represented by M_α, M_β, M_γ,...
> these quantities being arranged in the natural or alphabetical order; the first in order of these quantities M, which enters into the leading coefficient of a particular covariant, may for shortness be called the leading term of such covariant,...
>
> [1856a; CP2, 270]

The covariants chosen according to this principle were described as being in their 'best form'.

The covariants of the binary quintic calculated in the <u>Second Memoir</u> were the covariants of degree less than or equal to five (plus an invariant of degree 8). They are all of modest length as, for example, the covariant of degree 5 and order 7 (see Table 2.) which would have required Cayley to solve 15 linear equations in 16 variables in order to obtain the leading term of the covariant.

In [1856b], Cayley computed an invariant of degree 12 and in [1858e] he included the table for the invariant of degree 18, the famous skew invariant discovered earlier by Hermite. The massive calculations necessary for the display of this invariant were carried out by the Irish mathematician, George Salmon, and its presentation in [1889b; CP2,299] took up five quarto pages.

George Salmon, the third member of the 'Invariant Trinity' had made his first contribution to Invariant Theory with his [1854a]. He took a particular interest in the calculation of invariants and covariants. According to Thomas Archer Hirst (1830 - 1892), the mathematician, and Cayley's close friend, the calculatory part of Invariant Theory almost became an end in itself for Salmon:

> He [Salmon] is a great calculator, fond of calculating for its own sake. I do not class him amongst the high mathematicians however. The mere ready-reckoning element is too prominent in him. I had often noticed that his books although excellent as a collection of theorems gave no compact rounded view of the subject and this defect was at once explained when I learnt that he writes his books in a fragmentary manner beginning to print before he had concluded what shall be the precise nature of the book. To my surprise, I found he was not a great reader that Cayley to him is just as difficult as to the rest of us and that it is only on those subjects upon which he has himself worked that he can even read Cayley. He is just beginning a book on Surfaces which he is writing in his usual manner.
>
> [App.C, Hirst Journal, August 1860, 3, 1548]

a^3ef ...	a^3f^2 ...	a^2bf^2 ...	$a^2cf^2 - 1$	$a^2df^2 + 1$	a^2ef^2 ...	a^2f^3 ...	abf^3 ...
a^2bdf ...	a^2bef ...	$a^2cef - 3$	$a^2def + 7$	$a^2e^2f - 1$	$abdf^2 + 3$	$abef^2$ ...	$acef^2$ ...
a^2be^2 ...	$a^2cdf + 7$	$a^2d^2f + 12$	$a^2e^3 - 6$	$abcf^2 - 7$	$abe^2f - 3$	$acdf^2 - 7$	$ad^2f^2 - 2$
$a^2c^2f + 2$	$a^2ce^2 - 10$	$a^2de^2 - 9$	$ab^2f^2 + 1$	$abdef + 26$	$ac^2f^2 - 12$	$ace^2f + 7$	$ade^2f + 4$
$a^2cde - 5$	$a^2d^2e + 3$	$ab^2ef + 3$	$abcef - 26$	$abe^3 - 19$	$acdef + 18$	$ad^2ef + 7$	$ae^4 - 2$
$a^2d^3 + 3$	$ab^2df - 7$	$abcdf - 18$	$abd^2f + 32$	$ac^2ef - 32$	$ace^3 + 6$	$ade^3 - 7$	b^2ef^2 ...
$ab^2cf - 4$	$ab^2e^2 + 10$	$abce^2 - 18$	$abde^2 - 8$	$acd^2f + 18$	$ad^3f + 3$	$b^2df^2 + 10$	$bcdf^2 + 5$
$ab^2de + 5$	$abc^2f - 7$	$abd^2e + 30$	$ac^2df - 18$	$acde^2 + 53$	$ad^2e^2 - 15$	$b^2e^2f - 10$	$bce^2f - 5$
$abc^2e + 5$	$abcde - 8$	$ac^3f - 3$	$ac^2e^2 + 6$	$ad^3e - 39$	$b^2cf^2 + 9$	$bc^2f^2 - 3$	$bd^2ef - 5$
$abcd^2 - 7$	$abd^3 + 9$	$ac^2de + 45$	$acd^2e + 52$	$b^3f^2 + 6$	$b^2def + 18$	$bcdef + 8$	$bde^3 + 5$
$ac^3d + 1$	$ac^3e + 22$	$acd^3 - 39$	$ad^4 - 39$	$b^2cef + 8$	$b^2e^3 - 27$	$bce^3 - 2$	$c^3f^2 - 3$
$b^4f + 2$	$ac^2d^2 - 19$	$b^3df - 6$	$b^3ef + 19$	$b^2d^2f - 6$	$bc^2ef - 30$	$bd^3f - 22$	$c^2def + 7$
$b^3ce - 5$	$b^3cf + 7$	$b^3e^2 + 27$	$b^2cdf - 53$	$b^2de^2 - 20$	$bcd^2f - 45$	$bd^2e^2 + 19$	$c^2e^3 + 2$
$b^3d^2 - 2$	$b^3de + 2$	$b^2c^2f + 15$	$b^2ce^2 + 20$	$bc^2df + 45$	$bcde^2 + 87$	$c^3ef - 9$	$cd^3f - 1$
$b^2c^2d + 8$	$b^2c^2e - 19$	$b^2cde - 87$	$b^2d^2e - 25$	$bc^2e^2 + 25$	$bd^3e - 12$	$c^2d^2f + 19$	$cd^2e^2 - 8$
$bc^4 - 3$	$b^2cd^2 - 11$	$b^2d^3 + 6$	$bc^3f + 39$	$bcd^2e - 52$	$c^3df + 39$	$c^2de^2 + 11$	$d^4e + 3$
	$bc^3d + 33$	$bc^3e + 12$	$bc^2de - 45$	bd^4 ...	$c^3e^2 - 6$	$cd^3e - 33$	
	$c^5 - 12$	$bc^2d^2 + 57$	$bcd^3 + 65$	$c^4f + 39$	$c^2d^2e - 57$	$d^5 + 12$	
		$c^4d - 24$	c^4e ...	$c^3de - 65$	$cd^4 + 24$		
			$c^3d^2 - 20$	$c^2d^3 + 20$			
± 26	± 93	± 207	± 241	± 241	± 207	± 93	± 26

$(\ldots)(x, y)^7$

The $\pm$ number at the foot of each column is Cayley's check on the correctness of the result. In the first column, for instance, the sum of the positive coefficients is 26, that of negative coefficients is -26 and the sum is zero as it should be.

TABLE 2 [1856a;CP2, 275]

The covariant of degree 5 and order 7 for the binary form $(a \ b \ c \ d \ e \ f \ )(x, \ y)^5$, a covariant of modest length.

Salmon as a member of the inner group of algebraists in England did much to promote the work of Cayley and Sylvester by his prolific production of text books on modern algebra (Invariant Theory) and algebraic geometry. His work in calculation was highly valued by Cayley as contributing to the eventual tabulation of covariants. In the Third Memoir on quantics [1856b] Cayley's classification of covariants continued. He calculated independent covariants for the binary forms of order six (6 covariants), seven (2 covariants) eight (8 covariants) nine (3 covariants) and twelve (3 covariants). To a twentieth century mathematician the calculations seem relentless. To Cayley and his colleagues the listing of results appeared to be of greater importance than a carefully presented argument. In the lower dimensional cases, the calculations of covariants (and as well, groups, partition tables, symmetric functions etc) were just within their grasp and they duly responded to the possibility of actually seeing the invariants and covariants.

There are two supportive elements missing in Cayley's mathematics which are taken for granted by modern mathematicians. One is a fairly secure and sophisticated theoretical framework and the other (which perhaps would have interested Cayley and Salmon more) is the availability of powerful computational aids.

For forms of three variables, even those of low order, results were more difficult to obtain. But through his experience of calculation Cayley had acquired great facility in dealing with algebraic forms. What would appear long and tedious to the modern mathematician was, perhaps, not so difficult for Cayley. In regard to calculations associated with forms of three variables, he wrote:

> The actual effectuation of the transformations would, it is almost needless to remark, be very laborious, but the forms of the results are easily forseen, and the results can then be verified by means of one or two coefficients only.
>
> [1861c; CP4, 335]

With the close of the Seventh Memoir on quantics [1861c] there was still much to do. A number of covariants of the quintic had been calculated but few of the dependences between covariants were known. In the succeeding case, the binary form of order six, few covariants had actually been calculated.

Cayley eventually produced complete tables for the binary quintic and sextic but in doing so he relied on the theoretical results of the German mathematicians. Of crucial importance was the fundamental finiteness theorem of Gordan. This was published in 1868. Like Cayley, Gordan was interested in the calculation of covariants but his results were given in the German 'symbolic form.'

2.5. Applications to polynomial equations

Apart from its intrinsic appeal, Invariant Theory occupied a central place in Cayley's mathematics because of its wide applications. In the theory of algebraic equations [16] the calculations of invariants and covariants was particularly relevant. During the second half of the nineteenth century in England, the theory of the algebraic equation was almost exclusively studied through the Theory of Invariants. The invariants and covariants already calculated could be utilised and this application provided a spur for more calculation. This was especially the case with quintic polynomial equation , one of the leading problems in the mid century period.

Hermite had given the solution of the quintic equation in terms of elliptic functions, but there were many questions remaining. Questions which interested Cayley included finding criteria for establishing the reality of the roots of the quintic and the problem of finding special forms of the quintic which were solvable by radicals.

Felix Klein discussed[1956a] two approaches to the subject of polynomial equations. One was to transform the equation by a polynomial transformation of the (n-1)th degree to make some of the coefficients vanish (Tschirnhausen's Transformation). The other was the approach via the resolvent equation whose own roots could be expressed as rational functions of roots of the original polynomial equation.

Cayley adopted both these procedures and found that invariants and covariants could be made to play a leading role in both of them. The tenor of the investigation is of course computational. Cayley was especially active with this work during the period 1860-1862.

In the first approach, Cayley's application of Hermite's transformation

$$y = (ax+b)B + (ax^2+3bx+2c)C$$

in the case of the cubic equation

$$ax^3 + bx^2 + cx + d = 0$$

suggests the method used.

Cayley rewrote Hermite's transformation as

$$(y - bB - 2cC) + x(-aB - 3bC) + x^2(-aC) = 0$$

multiplied by x and reduced using the cubic equation to obtain

$$dC \qquad + x(y - bB + cC) + x^2(-aB) = 0$$

and repeating the process

$$dB \qquad + x(3cB + dC) + x^2(y + 2bB + cC) = 0$$

Cayley wrote these three equations as

$$\left(\begin{array}{ccc} y-bB-2cC & -aB-3bC & -cC \\ dC & y-bB+cC & -aB \\ dB & 3cB+dC & y+2bB+cC \end{array} \right)(1, x, x^2) = 0$$

[1861a; CP4, 378]

Since these three linear equations were required to possess a non-trivial solution, the determinant of the matrix must vanish. This yielded the transformed equation:

$$y^3 + 3Hy + \Phi = 0$$

where H, Φ were the familiar covariants of the binary cubic form $(a, b, c, d)(B,C)^3$ [1861a ; CP4,379].

Cayley continued by giving corresponding results (the coefficients of the transformed equation were generally functions of covariants) for the binary quartic and binary quintic. As to be expected, the calculations for the quintic were the most involved. In fact, in this case, Cayley supervised two nineteenth century computers (Messrs. Davis and Otter) who were paid from a Government Grant Fund.

The second approach to the quintic equation was concerned with formulating resolvent equations. At the beginning of the 1850s Cayley was one of the few English mathematicians[17] aware that the insolubility of the quintic polynomial equation had been established by Abel and Galois. George Peacock had been completely unaware of the result at the time he published his [1845a][Kiernan, 1971a, 94] . Isaac Todhunter, publishing a textbook on the Theory of Equations was aware of the result but showed a lack of interest in the details: 'beyond equations of the fourth degree the general algebraical solution of equations has not been carried, and it appears cannot be carried' [Todhunter, 1861a, 3].

In formulating the resolvent equation, Cayley found that its coefficients were seminvariants. Following the work of J.Cockle and R. Harley[18] (the former had obtained a very simple expression for a resolvent of the reduced quintic $x^5 - ax + b = 0$), Cayley considered the general quintic. He obtained the sextic resolvent:

$$\phi^6 + C\phi^4 + E\phi^2 + F\phi + G = 0$$

The coefficients C, E, F, G of this polynomial were seminvariants. Cayley demonstrated the fundamental property of the resolvent equation: that the roots x_1, x_2, x_3, x_4, x_5 of the general quintic could be expressed as rational functions of the roots $\phi_1 \quad \phi_2 \quad \phi_3 \quad \phi_4 \quad \phi_5 \quad \phi_6$ of the sextic resolvent:

$$\phi_1\phi_2 + \phi_2\phi_4 + \phi_3\phi_5 = (*\between x_1, 1)^4$$
$$\phi_1\phi_2 + \phi_3\phi_4 + \phi_5\phi_6 = (*\between x_2, 1)^4$$
$$\phi_1\phi_5 + \phi_2\phi_3 + \phi_4\phi_6 = (*\between x_3, 1)^4$$
$$\phi_1\phi_3 + \phi_2\phi_6 + \phi_3\phi_5 = (*\between x_4, 1)^4$$
$$\phi_1\phi_4 + \phi_2\phi_5 + \phi_3\phi_6 = (*\between x_5, 1)^4$$

The expressions $(*\between x, 1)^4$ were known polynomials of the fourth degree [1861a; CP4, 310].

After this work was completed Cayley discovered that Jacobi had published the result in 1835 [1861c; CP4,324].

Seminvariants also arose in other questions concerned with the theory of equations. In the equation of squared differences, a construction which according to Cayley went back to Waring in 1763, the coefficients of the equation of differences were identified as Seminvariants [Cayley, 1860a]. The equation of differences gave criteria for determining the reality of roots of the cubic though for the quintic and higher polynomials the establishment of this criteria proved a difficult problem. For the cubic, it is relatively simple:

Let the cubic:

$$ax^3 + 3bx^2 + 3cx + d = 0$$

have roots α, β, γ

The equation whose roots are:

$$(\alpha-\beta)^2, \quad (\beta-\gamma)^2, \quad (\gamma-\alpha)^2$$

is (the equation of squared differences):

$$a^4x^4 + 18\underset{\sim}{h}a^2x^2 + 81\underset{\sim}{h}^2x + 27\nabla = 0$$

where $\underset{\sim}{h} = ac - b^2$, ∇ = discriminant of cubic. Both $\underset{\sim}{h}$ and $\underset{\sim}{\nabla}$ are seminvariants.

As a corollary to this:

$$\{(\alpha-\beta)(\beta-\gamma)(\gamma-\alpha)\}^2 = \frac{-27}{a^4}\nabla$$

[1860a; CP4, 242]

The criteria for the reality of the roots is therefore:

$\nabla > 0$	Two imaginary roots
$\nabla = 0$	Two roots identical
$\nabla < 0$	Distinct real roots

2.6. Matrices

In considering Cayley's work on matrices, it is possible to discern many connections with his other interests in algebra. Cayley appears to use matrices as a notation for algebraic forms but he was able to see connections between their theory and such diverse areas as the Theory of Groups and Hamilton's quaternions. These connections would have convinced him that matrices possessed a wider scope than a mere notational advantage in treating bilinear forms. But the links which Cayley mentioned as existing between different theories went undeveloped. At the time Cayley was at one of his peaks of production and many areas claimed his attention. A contributing factor to Cayley's lack of interest in pursuing the Theory of Matrices must have been their limited applicability to the Theory of Invariants. Whereas determinants and functions like them were useful in this Theory, matrices were inessential. By the late 1850s the Theory of Invariants had long dispensed with the need to deal with linear transformations per se. During this period the 'new synthesis' for Invariant Theory caused the subject to be approached through partial differential equations. But more than this reason is the fact that one of Cayley's main concerns in the Theory of Invariants was centred around the computational problem of actually producing covariants. Matrices were occasionally found useful but it was a limited use and their existence did not ease the problem of producing covariants. And even after his [1858a] Cayley frequently used the 'matrix' in the old sense of simply delineating an array from which determinants could be extracted.

It is likely that Cayley saw matrices when applied to transformations and bilinear forms as simply a condensed notation but not taking the place of the full Cartesian expression of the transformation or bilinear form. While he regarded matrices when applied to transformations as elegant (as in, for example [1880b]), transformations were most easily dealt with by Cayley through linear equations. Despite the few contributions by Cayley on matrices, he is remembered today primarily on account of his contributions to their theory. The reason for this lies in the subsequent importance of matrices coupled with the historical fact that in [1858a] he gave a clear exposition of their basic properties.

In recent years, the history of matrices during the last century has been extensively studied in a series of papers written by Thomas Hawkins.[19] In Hawkins' reconstruction of Cayley's thought [1877a] the importance of Cayley's [1858b] (the companion paper to his better known Memoir on the Theory of Matrices [1858a]) is emphasized. According to Hawkins, the importance of [1858b] was that it contained Cayley's motivation for introducing matrix symbolism. In [1858b] Cayley introduced the symbolism for application in the 'Cayley-Hermite' problem (To determine all linear substitutions of the variables of a non singular quadratic form which leave the form invariant). This problem has long been studied by Cayley, but until [1858b] he had not treated it using the single letter symbolism for matrices. In his [1977a] Hawkins concluded that 'Cayley occupies a special place in the history of that theory (Matrices) by virtue of his work relating to the Cayley-Hermite problem' [Hawkins, 1977a, 108] .

The intention here is to suggest a reason for Cayley being in a position to introduce this symbolism in respect to the Cayley-Hermite problem.

The 1858 Memoirs-prelude

Sylvester played a part in the eventual emergence of Cayley's [1858a] and [1858b] As is well known, it was Sylvester who introduced the word 'matrix' into mathematical language in 1850, but he then meant an array of numbers from which determinants could be formed. Cayley had earlier used the double bar notation for the same purpose in his early papers on determinants.

Sylvester's concern was with determinants, as his consideration for the multiplication rule for determinants shows.

A letter written in 1852 to Cayley by Sylvester shows a matrix being multiplied by another matrix. It is quite natural for Sylvester to be using row by row multiplication to obtain the resulting matrix as he was interested in determinants:

A consideration of the principle of your paper in the Cam Journal 1841 [1841a] has led me to the following agreeable extension of the common rule for the multiplication of Determinants which extends also to the Combination of Rectangular Matrices. The example will suffice to make you see the Theorem.

$$\begin{matrix} a & b & c \\ a' & b' & c' \\ a'' & b'' & c'' \end{matrix} \times \begin{matrix} \alpha & \beta & \gamma \\ \alpha' & \beta' & \gamma' \\ \alpha'' & \beta'' & \gamma'' \end{matrix}$$

$$= \begin{matrix} (a,b,c)(\alpha,\beta,\gamma)\,; & (a,b,c)(\alpha',\beta',\gamma')\,; & (a,b,c)(\alpha'',\beta'',\gamma'') \\ (a',b',c')(\alpha,\beta,\gamma)\,; & \cdots & ;(a',b',c')(\alpha'',\beta'',\gamma'') \\ (a'',b'',c'')(\alpha,\beta,\gamma)\,; & \cdots & ;(a'',b'',c'')(\alpha'',\beta'',\gamma'') \end{matrix}$$

according to the Common Rule...

[App. B, 21 ix 52]

Writing in 1853, Sylvester made use of matrices in describing linear substitutions. ' The matrix formed by the coefficients of substitution arranged in regular order is called the Matrix of Substitution, and is of course a square.' [SP1, 585] . Thus Sylvester was aware that a matrix could be a convenient way of representing a linear transformation. Sylvester also had a clear idea of the 'inverse matrix' as the matrix of cofactors(but without division by the determinant itself) but he did not consider the theory of matrices per se.

Hawkins has noted [1977a, 86] that the idea of using an array for representing transformation was also realised by other mathematicians at the time. It is likely that Cayley was referring to this theory of matrices in 1853 at the time he submitted his [1854a] on groups.

In his [1854a] Cayley made a passing reference to the Theory of Matrices[20]. It was Cayley who eventually presented ideas in a short paper to the editor of Crelle's Journal. This is the paper [1855a] containing the frequently quoted remark that the Theory of Matrices precedes the Theory of Determinants, by which Cayley meant that matrices logically precede determinants in their order of development.[21]

The paper [1855a] outlined some of the elementary properties of matrices (including the composition of matrices) and some of the advantages of the new notation in applications. As to applications, Cayley noted that matrices gave a convenient method for representing linear equations and quadratic functions. For example, a lineo-linear (bi-linear) function

$$\begin{array}{l} (\alpha\xi + \beta\eta + \gamma\zeta \dots)x \\ +(\alpha'\xi + \beta'\eta + \gamma'\zeta \dots)y \\ +(\alpha''\xi + \beta''\eta + \gamma''\zeta \dots)z \\ \dots\dots\dots\dots\dots \end{array}$$

can be represented by

$$\left(\begin{array}{llll} \alpha, & \beta, & \gamma, & \dots \\ \alpha', & \beta', & \gamma', & \dots \\ \alpha'', & \beta'', & \gamma'', & \dots \\ \vdots & \vdots & \vdots & \end{array}\right)(\xi, \eta, \zeta, \dots)(x, y, z, \dots)$$

The special case of a quadratic form in three variables

$$ax^2 + by^2 + cz^2 + 2hxy + 2gxz + 2fyz$$

would be written

$$\left(\begin{array}{lll} a, & h, & g \\ h, & b, & f \\ g, & f, & c \end{array}\right)(x, y, z)^2$$

or by

$$(a, b, c, h, g, f)(x, y, z)^2$$

a notation he introduced in his introductory memoir on quantics [1854c] .

After [1855a] the notation was used in a number of instances[22]. But although he indicated a theory of matrices he persisted in using the notation in different ways. (Even after his [1858a] the notation was used as an array from which determinants can be formed). One way was in a short paper [1857f] on a method in the theory of elimination. This paper illustrates the difficulty other mathematicians experienced with Cayley's preference for this notation and his liking for the 'homogeneous' notation in denoting a polynomial.

In [1857f] which Cayley published on Bézout's method,[23] matrices as notational devices were used purely as a static notation. Cayley's observation on Bézout's eliminant was expressed as

$$\frac{(a,\ldots \between (x,y)^n (a',\ldots \between (\lambda,\mu)^n - (a',\ldots \between (x,y)^n (a,\ldots \between (\lambda,\mu)^n}{\mu x - \lambda y} =$$

$$\left(\begin{array}{cccc} a_{00} & a_{1,0} & \ldots & a_{n-1,0} \\ a_{01} & a_{1,1} & & a_{n-1,1} \\ \vdots & & & \\ a_{0,n-1} & a_{1,n-1} & \ldots & a_{n-1,n-1} \end{array}\right| \between (x,y)^{n-1} \vee (\lambda,\mu)^{n-1}$$

a binary form of degree $n-1$ of two sets of variables.

[1857f; CP4, 38?]

The editor of Crelle [Borchardt] recognised the importance of the result but because he considered the presentation obscure, added a Note which expressed the result in the ordinary mathematical language of the day.[24] This Note was in terms of polynomials written non-homogeneously and is made without the use of matrices. After some correspondence between the editor and Cayley on difficulties over the matrix notation [App C, Borchardt, 20 ix 56] (which was submitted in April 1855) Cayley's paper was published in 1857.

The 1858 Memoir [1858a]

Cayley's output in the years 1857 and 1858 was prolific.[25] The principal result of his [1858a] is the celebrated Cayley-Hamilton Theorem[26], a result which is both spectacular and surprising. This was not only the view of Cayley on its discovery but also of twentieth century mathematicians who succeeded in generalising it. No doubt it was Cayley's discovery of this result which gave him the impetus to publish his paper. In addition to this Theorem, [1858a] contains a formalisation of the properties of matrices and an investigation of some of their properties.

There are two aspects of algebra in which Cayley is notably expert. In the Theory of Invariants Cayley was interested both in the calculation part of a theory and its formalisation. Central to this formalisation is the Calculus of Operations. In [1854a] on group theory Cayley began with the notion of an operation . In the Theory of Invariants the importance of differential operation is implicit. In [1858a] the idea of an operation is present but it is more difficult to detect. Cayley did not mention the 'Calculus of Operations' explicitly. But George Boole provided a clue: He was one of the referees of [1858a] In his report (dated 29 iii 1858) Boole saw Cayley's introduction of matrix algebra as a step in the Calculus of Operations:

> This memoir is an application of what has recently been termed the Calculus of Operations, to a particular branch of the Calculus of Functions. A matrix is a complex symbol denoting the operation by which from any set of quantities x, y, z, we form a set of linear functions of these quantities e.g.
>
> $$ax + by + cz\ ,\ a'x + b'y + c'z\ \&c,$$
>
> the number of such functions being in the class of Matrices chiefly considered by the author, equal to the number of the subject quantities. As operations such as the above may be performed in succession, as the results to which they lead are capable of addition and subtraction, as also, here as elsewhere, a direct operation supposes the existence of a corresponding inverse operation – the inquiry is suggested what are the distinctive laws of this class of operations, and to what special forms of Calculus, included under the more general calculus of operations, they give birth. This inquiry forms the business of the memoir, and its results are developed with clearness and ability. In certain general features they resemble, and

necessarily so, the results of all other special developments of the Calculus of Operations and they are certainly of an interesting character...

[Royal Society of London, RR. 3,55]

In the interpretation of a matrix as a 'complex symbol' Boole would have taken note of Cayley's brief explanation of the meaning of the matrix notation:

The notation

$$\left(\begin{array}{ccc} a, & b, & c \\ a', & b', & c' \\ a'', & b'', & c'' \end{array}\right)(x, y, z)$$

represents the set of linear functions

$$((a, b, c)(x, y, z), (a', b', c')(x, y, z), (a'', b'', c'')(x, y, z))$$

so that calling these [functions] (X, Y, Z),

we have

$$(X, Y, Z) = \left(\begin{array}{ccc} a, & b, & c \\ a', & b', & c' \\ a'', & b'', & c'' \end{array}\right)(x, y, z)$$

and, as remarked above, this formula leads to most of the fundamental notions of the theory.

[1858a, 18; CP2, 476]

It is not obvious that Cayley did regard the matrix as Boole describes but there is a striking similarity between this paper and his earlier paper on group theory [1854a; CP2,123]. In this Cayley outlined the principal properties of 'operations' and opened his [1854a] with the following passage:

> LET θ be a symbol of operation, which may, if we please, have for its operand, not a single quantity x, but a system $(x, y, \ldots)$ so that
>
> $$\theta(x, y, \ldots) = (x', y', \ldots)$$
>
> where $x', y', \ldots$ are any functions whatever of $x, y, \ldots$ it is not even necessary that $x', y', \ldots$ should be the same in number with $x, y, \ldots$ In particular x', y', &c. may represent a permutation of x, y, &c., θ is in this case what is termed a substitution; and if, instead of a set $x, y, \ldots$, the operand is a single quantity x, so that $\theta x = x' = fx$, θ is an ordinary functional symbol. It is not necessary (even if this could be done) to attach any meaning to a symbol such as $\theta \pm \phi$ or to the symbol 0.
> [1854a; CP2, 123]

The multiplication of matrices was a direct consequence of the 'compound' operation, but the addition of matrices does not follow so naturally.

The key remark in the quotation occurs at the part where he discussed the corresponding operation $\theta \pm \phi$ and the parenthetical 'even if this could be done.' This is taken to mean 'if it could be done in a particular example.' That this is the most likely interpretation follows from a later remark in the same paper in which Cayley made clear that whereas $\theta \pm \phi$ cannot be satisfactorily explained in the case θ and ϕ each refer to a permutation operation, it can be interpreted when θ and ϕ each involve quarternion imaginaries in which $\pm$ is defined. Even if the parenthetical remark means: 'if it could be done at all', his quoted remarks above show that Cayley was alert to the possibility of adding symbols of operation.

Having formalised the Theory of Matrices it is curious that Cayley did not develop it further. In a paper written in the very same year of his [1858a] Cayley employed a notation due to Gabriel Lamé (1795-1870). This is a grid notation for representing the transforming equations:

$$\begin{aligned} X &= \alpha X_{,} + \beta Y_{,} + \gamma Z_{,} \\ Y &= \alpha' X_{,} + \beta' Y_{,} + \gamma' Z_{,} \\ Z &= \alpha'' X_{,} + \beta'' Y_{,} + \gamma'' Z_{,} \end{aligned}$$

from (X, Y, Z) to $(X_{,}, Y_{,}, Z_{,})$ (the passive interpretation of a transformation) as

	$X_{,}$	$Y_{,}$	$Z_{,}$
X	α	β	γ
Y	α'	β'	γ'
Z	α''	β''	γ''

[Cayley, 1860c; CP3, 354]

The composition of transformation is also expressed in this grid notation in [1860c] Cayley used Lamé's notation[28] elsewhere (as in [1862a]). This notation is found in [Lamé, 1859a, 4] a book widely used by Cambridge mathematicians during the 1860s and 1870s.

The Oxford mathematician H.J.S.Smith (1826-1883) a pioneer in the Theory of Numbers contributed a paper [1861a] on linear equations to the Philosophical Transactions of the Royal Society. Smith's work was written in the language of matrices and it is possible that he read and was influenced by Cayley's memoir of a few years before.[29] However, Smith did not adopt Cayley's notation preferring to write the matrix with q rows and p columns as

$$\left\| \overset{q \times p}{A} \right\| \text{ or } \left\| A \right\| .$$

2.7. The Return to Cambridge

During the 1850s and early 1860s Cayley continued in his profession as a conveyancing barrister and pursued his prolific mathematical publication. Cayley was one of the leaders of pure mathematics in England.

A vivid picture of Cayley was given by his close friend, T.A.Hirst. Influenced by the interest in phrenology, Hirst wrote:

> This evening (Friday Dec 23) [1859] I called upon Cayley and we had a very interesting hour's talk on Curves of the Third Order a propos of Mobius, on a new method of his own for obtaining the equation of the squares of the differences of the roots of the quintic [1860a] and on my own subject of Derived Curves of Double Curvature. I explained what I was doing in which he expressed some interest. I was a little amused and encouraged too by his asking me for a definition of the rectifying plane. The great geometer had forgotten it for the moment. What a wonderful head he has not merely round but spheroidal with the largest diameter parallel to his eyes, or rather to the line joining his ears. He never sits upright on his chair but with his posterior on the very edge he leans one elbow on the seat of the chair and throws the other arm over the back. Yet he is a keen sighted and extraordinary man, gentle I think by nature and at once timid, modest and reticent. Often when he speaks he shuts his eyes and talks as if he were reading from an unseen book, and talks well too that one has to sharpen one's own wits to follow him.
>
> [App. C, Hirst Journal, 23 xii 1859, 3, p.1520]

Yet for this exceptionally gifted man there was no suitable academic position available and at one point he considered taking private pupils [App. B, 15 vi [1861]].

According to Sharlin [1979a, 72] employment prospects for research scientists in general were bleak in mid century Britain. Most research was done outside the Universities and those already in teaching posts and whose main interest was primarily in teaching had little aptitude for research.
On a few occasions [24] Cayley was tempted to obtain employment in a teaching institution but without success. In one attempt to find a position more congenial to his mathematical interests than the daily round of legal problems at Lincoln's Inn, Cayley's name became linked with the illfated Western University of

Great Britain at Gnoll College in the Vale of Neath in Glamorganshire [Williams, 1966a, 32] .

In 1857 the only University Institutions with University status in England and Wales were Oxford and Cambridge, Durham, King's College and University College in London, Owen's College at Manchester and Lampeter in Wales. The proposed Western University was to be more adapted to the 'Wants of the Age' and Cayley was to be one of the seven resident professors. The position had its attractions, not least the generous salary of £500 per annum in comparison with a salary of about £300 at one of the ancient Universities.

In keeping with the philosophy of the proposed educational venture, the practical aspects of Mathematics were to be stressed and this aim to be attained by, for example, an intermediate course in descriptive Geometry and Higher Calculus with such final courses as Astronomical Observation and Trigonometrical Surveying. It seemed that the curriculum was modelled on the one which existed at the École Centrale in Paris. In the event the College never went further than the planning stage but in the meantime the proponents of the idea made full use of Cayley's name in advertisements which appeared in the Times and elsewhere. In retrospect, Cayley would have done well to have heeded the advice offered by Sylvester:

> I am sorry to see the unprivileged use the Gnoll people have made of your name. I never thought well of the scheme. I like it now much less than ever and most earnestly trust & hope that you will not associate yourself with it. [App. B, 14 ix 1857]

Despite these setbacks in his career, Cayley continued his mathematical work. There was plenty of interest in London, and Sylvester, when he gave his Lectures on Combinatorial Theory at King's College in 1859, attracted more than forty listeners. The London Mathematical Society was not yet formed but mathematicians who were members of the Royal Society found their meeting place at the Royal Society. Others met in the London Clubs.

Sylvester was elected to the Athenaeum Club, a meeting place for the Scientific and Literary, but Cayley appeared curiously reluctant.

Sylvester tried to persuade him:

> I wish (...) you might come among us; you would thus meet all the people you would most likely to wish to meet and know. Pity! that you are obstinately bent on declining a privilege that many rich men would give £1,000 or £2,000 to obtain or even more. You have only to speak the word and the Committee would bring you in among the 9 Muses over the heads of about 1500 expectant candidates.
>
> [App B, [1861?], date estimated]

In addition there were the informal meetings where mathematics was actively discussed. Hirst's Journal provides a valuable record of one of these meetings. The passage indicates the interest Cayley and Sylvester maintained in the teaching of mathematics at an elementary level:

> On Monday evening I went to Woolwich to dine with Sylvester. Cayley was there to meet me and we had a very pleasant and very simple dinner. Sylvester's researches on Commutants and the Integration of Equations of finite Differences formed the principal subject of conversation. Sylvester was full of brilliant ideas. Cayley pulled him in incessantly to obtain greater precision for Sylvester lacks the power of placing himself in his hearers position and appreciating what it is necessary to explain in order to bring them to his point of view. I understood the matter but imperfectly. On Elementary mathematics both threw out ideas which I must find time to examine, one by Sylvester was on the development of $\cos n\theta$ and $\sin n\theta$ in terms of powers of $\sin\theta$ and $\cos\theta$ (...) Cayley's suggestion had reference to the theory of determinants which he would define as a linear function of the elements in a row (or column) which changes sign when any two rows are interchanged. From this definition all properties might be easily deduced.
>
> [App. C, Hirst Journal, 9 xi 1861, 3,p.1593]

At Cambridge University in 1857 a new Statute was enacted whereby Lectureships which had hitherto been funded by a Trust (Lady Sadleir Benefaction) were to be phased out and replaced by the Sadleirian Chair of Pure Mathematics. When sufficient funds were available an election for the Chair was called for June 1863. The other candidates for the Chair [C.U.L., Add 6580] were all resident Cambridge Dons: Percival Frost, Isaac Todhunter (1820-1884), N.M.Ferrers (1829-1903), and E.J.Routh (1831-1907).

They were all well known teachers of mathematics but none of them could match Cayley's research achievement.

After his initial disappointments in trying to obtain a post, Cayley was at last successful and was elected to the Sadleirian Chair on 10th June 1863. The specific duty of the Sadleirian Professor was:

> to explain and teach the principles of pure mathematics and to apply himself to the advancement of that science.

Chapter 2

References

1. The existence of this letter is remarked upon by Cayley in the Notes [CP2,600] he made to the Collected Mathematical Papers when they were being edited for publication in 1889. The letter itself serves to underline his priority over Sylvester who discovered similar differential equations and published them before Cayley. Cayley dispatched his paper [1854b] on the new developments to Crelle's Journal on 23 ii 1852.

2. Cayley is speaking here of the degree of the covariant (the degree of the coefficients) and not the parent quantic. In particular, he is not saying that the method is difficult to apply for a binary quintic and higher order forms. Cayley employed his hyperdeterminant method computationally for invariants of degree less than or equal to 4 in [1846b].

In his [1892a], Emory McClintock, commenting on the computation of covariants by Transvection, said that the computation of low degree covariants of low order by 'transvection' is ' extremely troublesome'. Cayley dealt with the 'hyperdeterminant derivative' method infrequently after he dropped it as a method for finding invariants and covariants. But it did not disappear altogether. For example [Cayley, 1872c and 1892a].

3. [Roy.Soc. London, RR. 2. 42-3] Graves thought the introduction of the word 'Quantic' was unnecessary. Boole thought 'Quantic' was a bad notation being a Greek termination to a Latin adjective "which expresses nothing." [Letter Boole to de Morgan, 3 i 1855, D.M.S.Watson Library, University College, London; I am grateful to Mr. G. Smith for this reference.]

4. Vestiges of this technique still survive. The shifting operator $E(x) = x+h$ is required to have the property $E\phi(x) = \phi(x+h)$ in some parts of Numerical Analysis. De Morgan described this kind of 'operation' by saying that the symbol ϕ is diaphonous with respect to the operator E. [Brock, 1967a 101]. Gregory used this device to prove the Leibnitz formula for the nth derivative of a product of two functions.

4 (continued)

He wrote

$$\frac{d}{dx}uv = \left(\frac{d}{dx} + \frac{d'}{dx}\right)uv$$

and expanded this bracket using the binomial theorem. The differential operator $\frac{d}{dx}$ is considered to act on the function u only and the differential operator $\frac{d'}{dx}$ only on the function v [Koppelman, 1971a, 193].

5. Cayley found that the 'graphical notation' of tree diagrams was a convenient notation for expressing the formulae analogous to

$$P.\,Q = PQ + P(Q)$$

for the composition of n operators [1857d] .

6. The postscript said that if a function of the correct weight is annihilated by one of these operators, it must be annihilated by the other. The stipulation of 'correct weight' is necessary as seen in the example of the quadratic function

$$ax^2 + 2bxy + cy^2$$

The coefficient a (which is <u>not</u> of the correct weight) is annihilated by the first of the operators $a\partial_b + 2b\partial_c$ but not by the second operator $c\partial_b + 2b\partial_a$

Cayley's postscript was his first intimation of the concept of a seminvariant: a function of the coefficients of a form which satisfies one of the differential equations but not necessarily the other. Thus the coefficient a is a seminvariant for the quadratic function $ax^2 + 2bxy + cy^2$

Seminvariant functions received some attention by mathematicians in the late 1850s and early 1860s (Brioshi who called them peninvariants). The Irish mathematician, Michael Roberts (1817-1882) who called them the source of a covariant, showed that a covariant of a binary form could be uniquely obtained from a covariant and conversely. He showed the source of the product of two covariants to be equal to the product of their sources and restated Cayley's Theorem (see Chapter 2, p. 78) [Roberts, M. 1861a] .

6 (continued)

This meant that covariants of a binary quantic could be tabulated by their seminvariants. This was later found useful in dealing with the classification of covariants of binary forms of high order.

7. A covariant is irreducible if it cannot be expressed, as a rational and integral function of covariants of lower degree. In the case of the binary cubic, Φ is an irreducible covariant.

The covariants U, ∇, H, Φ are each irreducible, and form a complete set (or basis) for the binary cubic.

For the cubic there are three 'really independent' covariants because one of U, ∇, H, Φ can be expressed in terms of the others. For example:

$$\Phi = \sqrt{U^2\nabla - 4H^3}$$

(But Φ is an irrational function of U, ∇, H) [1854c;CP2,233]

8. This letter is undated but is likely to have been written late in 1854 or early in 1855. The <u>Second Memoir on Quantics</u> [1856a] was received for publication on 14 iv 55 and read 24 v. 55.

9. There are two conventions used by Cayley in connection with the weight (pesanteur) of a covariant. In the notation adopted in Cayley's letter

$$(a, b, c, \dots \between x, y)^n$$

denotes a binary quantic of order n

and $(A_0, A_1, \dots, A_s \between x, y)^{\theta}(x,y)^s$ a covariant of degree θ and order s.

<u>Convention 1</u> The weights of the coefficients a, b, c,... are

$$-\tfrac{1}{2}n, \quad 1-\tfrac{1}{2}n, \quad 2-\tfrac{1}{2}n, \dots$$

and the weights of x, y are taken to be $\frac{1}{2}$, $-\frac{1}{2}$;
then the weights of the individual terms of the covariants are zero.

<u>Convention II</u> The weights of the coefficients a, b, c,... are 0,1,2,...
and the weights of x, y are taken as 1, 0; then the general term of the covariant has weight $\frac{1}{2}(n\theta + s)$.

9 (continued)

The first term of the covariant is of the form $A_0 x^s$ and hence (in Convention II) the individual term A_0 has weight $\frac{1}{2}(n\theta - s)$

Cayley described Convention I as the most elegant way of expressing the fact that a covariant has uniform weight in all its terms. It is the convention he adopted in his early work and is the first one described in the letter.
Convention II is the second convention in the letter and the one adopted in [1856a] It was the Convention used throughout the series on quantics. [1856a; CP2, 254]

10. A proof that the linear equations were indeed independent was given by Sylvester in [1878a] at a time when doubts had been expressed as to the validity of Cayley's Law.

11. L.F.A.Arbogast (1759-1803) was a pioneer in the Calculus of Operations. He generalised Lagrange's approach to the Calculus, the approach which placed the Calculus on an algebraic foundation, in contrast to Cauchy's approach which introduced the limit concept. The general approach to the differential calculus based on algebraic considerations gained popularity in England through the Analytical Society. Cayley was influenced by Arbogast's treatment of the calculus as were other English mathematicians. A comparison of the two different traditions in the Calculus is given in Grattan-Guinness, Brit. Journal Hist. Science, 12 (1979), 82-88
The rule of Arbogast which Cayley used in his 1856a is an algorithm for producing the coefficient of x^r from the coefficient of x^{r-1} in the expansion of $f(a + bx + cx^2 + dx^3 + \cdots)$. As remarked in [Koppelman, 1971a, 161] this rule is highly combinatorial in character and it is the combinatorial aspect which Cayley made use of in his Theory of Invariants. He referred to Arbogast's Rule as the 'rule of the last and last but one.' See Cayley's paper of 1869 [CP8,471] for his explanation of Arbogast's Rule. See also [Cayley, 1878f]. In his 1881a Cayley still used this device in his search for invariants.
For a history of Arbogast's Rule see [Tanner, 1891a].

12. In general the condition $XA = 0$ gives

$$P(0, \ldots n)^{\theta} q$$

linear equations in $P(0, \ldots, n)^{\theta}(q-1)$ variables. These equations have $P(0, \ldots, n)^{\theta} q - P(0, \ldots, n)^{\theta}(q-1)$ linearly independent solutions if the equations are linearly independent. Thus Cayley's Law for the number of 'asyzygetic' invariants and covariants follows immediately if this assumption is made.

13. This is stated in a letter to Sylvester [App. B, 1851/1852, estimated date]

14. Cayley's conclusion that there was no finite complete system of covariants for the quintic might have been expected by the early practitioners in Invariant Theory. Turnbull [1941a] suggests that these mathematicians recognised that the quintic was essentially different from binary forms of lower order.

15. The letter (Plate 2) is undated but is likely to be late 1854 or early 1855. Cayley knew that a binary quantic of order m has m-3 'really independent' invariants. Cayley concluded that there was not a finite number of irreducible invariants forming a complete system for binary quantics of order seven and eight [1856a; CP2, 253].

16. Cayley was uninterested in the numerical solutions of equations. His computations were carried out in terms of the literal coefficients were concerned with the algebraic solution of equations. His results were useless for the numerical calculation of the roots of equations with numerical coefficients. Cayley's method of solution of the cubic illustrates this:

The cubic was written homogeneously

$$U = (a, b, c, d \between x, y)^3$$

From the invariant ∇ and the covariants H and Φ Cayley factored the fundamental relation

$$4H^3 = U^2\nabla - \Phi^2$$

and obtained

$$4H^3 = (\sqrt{\nabla}\, U - \Phi)(\sqrt{\nabla}\, U + \Phi)$$

16 (continued)

The cubic root of each factor is therefore linear because H is a covariant of order 2.

Hence the expression

$$(\sqrt{\nabla}\, U + \Phi)^{\frac{1}{3}} + (\sqrt{\nabla}\, U - \Phi)^{\frac{1}{3}}$$

is linear and it also vanishes if $U = 0$. It must be a factor of the cubic U .

Letting

$$X = (\sqrt{\nabla}\, U + \Phi)^{\frac{1}{3}}$$
$$Y = (\sqrt{\nabla}\, U - \Phi)^{\frac{1}{3}}$$
$$X^3 + Y^3 = (\sqrt{\nabla}\, U + \Phi) + (\sqrt{\nabla}\, U - \Phi) = \sqrt{\nabla}\, U$$

whereupon the cubic is completely factored into

$$\sqrt{\nabla}\, U = (X+Y)(X+\omega Y)(X+\omega^2 Y)$$

where $w^3 = 1$.

w a complex root of unity.

[1858e ; CP2, 542]

17. Galois' papers on the theory of equations were eventually published by Liouville in 1846 in volume IX of Liouville's Journal [Kiernan, 1971a, 99] . Cayley also published two papers in volume IX of Liouville's Journal . Cayley had published a paper in the same journal as early as 1844 and being known to the French mathematicians was likely aware of the importance of Galois' work at a very early stage.

When Cayley introduced his paper on the theory of groups [1854a] he explicitly referred to the idea of a group as applied to permutations or substitutions [Kiernan, 1971a, 102] .

18. Cockle and Harley were serious amateur mathematicians of the Victorian period. They were both interested in the quintic and they worked on the theory of this problem in the late 1850s and early 1860s in concert with Cayley. Cayley's [1861b], which crowned the previous fifteen years work of these mathematicians, was regarded as one of Cayley's important papers [MacMahon, 1894a] .

Sir James Cockle (1819-1895) was almost an exact contemporary of Cayley. His lowly position in the Tripos in 1841 did not stifle his enthusiasm for mathematics and it became a life long interest. He was called to the Bar in 1846 and made a career in the Law.

Robert Harley (1828-1910) was a self-taught mathematician and did not attend University. A close friend of George Boole, he was a pioneer in the Temperance movement. He was elected to the Royal Society in 1863.

19. See [Hawkins, 1972a, 1974a, 1975a, 1977a, 1977b] . In particular, see [1977a], an appraisal of Cayley's contribution to the Theory of Matrices.

20. Cayley noted:

> Again, in the theory of matrices, if I denote the operation of inversion [taking the inverse] and tr. that of transposition, (I do not stop to explain the terms as the example may be passed over), we may write
>
> $$\alpha = I, \quad \beta = tr, \quad \gamma = I.tr = tr.I$$
>
> [1854a; CP2, 123]

21. The difference between matrices and determinants was not firmly grasped by many mathematicians over a half a century later but Cayley never missed an opportunity of stressing the distinction. One instance was in an expository article on new terms in mathematics [1860] :

> Moreover, in a system of simple [linear] equations, if the coefficients arranged in the natural square order are considered apart by themselves, this leads to the theory of matrices, a theory which indeed might have preceded that of determinants; *the matrix, is, so to speak, the matter of a determinant.*
>
> [CP4, 594, my italics]

22. Cayley contributed seven papers to volume 50 (1955) of Crelle's <u>Journal</u> [pages 277-289, 299-317]. Apart from [1855a] which introduced the notation, three other papers utilised it. These papers were likely submitted at the same time. The last paper contains a date of dispatch of 24 v 1854. Subsequent to these papers but prior to the appearance of his [1858a] and [1858b], Cayley made sporadic use of the matrix notation as a means of considering arrays of coefficients. One of these papers [1857f] on Bézout's Method of Elimination is briefly discussed in the text.

23. Apparently Cayley discovered this method of Elimination around January 1853. Sylvester's reaction to the discovery is found in [App.B, Jan 1853]. Its importance to Cayley and Sylvester was as a process for finding covariants. It was called the Quotient Method by Sylvester [SP1, 553].

24. This comment is made on the basis of an entry in Hirst's Journal which recorded some discussion between Liouville and T.A.Hirst. Hirst learnt from Liouville that 'Borchardt, the editor of Crelle recognising the importance of the result obtained by Cayley, appended a note to Cayley's memoir and succeeded in reproducing the memoir in shorter space and in ordinary language with a decided gain in clearness.' [App.C, Hirst Journal, 18 xi 1857, 3, 1327].

25. In the period 1857-1858 there was a surge in Cayley's mathematical output (Appendix A). [1858a] was received 10 xii 1857 and read 17 i 1858. In December 1857 alone, six other papers were received by the Royal Society from Cayley and these were read in January 1858.

26. It is not clear why Cayley should have substituted a matrix into the polynomial $\det(A - \lambda I)$. Hamilton published his result some years earlier in his <u>Lectures</u>. Perhaps Cayley remembered Hamilton's conclusion. Cayley reviewed <u>Lectures on Quaternions</u> [Stokes, 1907a, 386] and he attended Hamilton's lectures in Dublin (see Chapter 1, p.48). Cayley refuted (in retrospect, 1894) the notion that he gained the idea of a matrix from quaternions [Knott, 1911a, 164].

27. The other referee was the Oxford mathematician, W.F.Donkin, who in a short report, wrote that the paper contained ' a real extension of the resources of symbolical reasoning.'

[Royal Soc. of London, RR.3.57]

28. Cayley showed a definite interest in new notations. In connection with substitutions $\begin{pmatrix} A \\ B \end{pmatrix}$ he used $\begin{array}{c} A\,| \\ |\,B \end{array}$ [App B, 16 xiii 1860]. The composition of substitutions could then be written

$$\begin{array}{c} A\,| \\ |\,B \end{array} \; \begin{array}{c} B\,| \\ |\,C \end{array} = \begin{array}{c} A\,| \\ |\,C \end{array}$$

Buccheim [1885a, 71] suggested this notation for matrix multiplication attributing it to Cayley.

Cayley's notation for the determinant formula for the inverse is an interesting one:

$$\frac{1}{\nabla}\left(\begin{array}{ccc} \partial_a\nabla & \partial_b\nabla & \partial_c\nabla \\ \partial_{a'}\nabla & \partial_{b'}\nabla & \partial_{c'}\nabla \\ \partial_{a''}\nabla & \partial_{b''}\nabla & \partial_{c''}\nabla \end{array}\right|$$

[1858a; CP2, 481]

where, for instance, ∂_a is the partial derivative with respect to a and therefore $\partial_a\nabla$ is the cofactor of a. cofactor of a.

In his [1858a] Cayley made the distinction between the matrix A (an array) and the symbol A as a single quantity. He briefly used the notation $\overleftarrow{A}$ for A considered as a single quantity.

29. Smith and Cayley were in communication by letter [App.C, Smith, 13 i 1858] during the period June 1857 to January 1858, when Cayley was preparing his Memoirs on Matrices [1858a] and [1858b] . The correspondence related to the transformation of quadratic forms as applied to the Theory of Numbers. As such it was concerned with linear transformations but matrices were not utilised. (see Cayley, [1857b]). Cayley recognised the importance of Smith's work on systems of linear equations and congruences but he did not appear to take up the subject himself [Royal Society of London, RR. 4. 242].

Chapter 3 Academic life - the middle years (1863-1881)

3.1. Introduction

Shortly after his election to the Sadleirian Chair, Cayley married and settled down to a quiet life at Cambridge. The lecturing duties were light and for years his custom was to give a single course of lectures in the first term of each year.[1] Though pure mathematics was dominant in the Mathematical Tripos and the application of mathematics to physical problems played little part in the education of undergraduates, pressures were gradually being exerted for the inclusion of 'applicable mathematics' in the curriculum. Two leading figures in this movement were James Clerk Maxwell and Sir William Thomson. A more critical voice was heard from the Astronomer Royal, Sir George Biddell Airy (1801-1892). He mounted a campaign inside the University for the teaching of mathematics which could be applied to physical problems and entered into a sharp correspondence with Cayley which stressed the importance of the proposed change.[2]

Elected in the 1860s, Cayley was one of the 'new professors' as distinct from an earlier breed with a more leisurely attitude to their University Chairs. The old guard was under no obligation to teach, to influence education or to even live in the vicinity of Cambridge. As a 'new professor', Cayley was required to both teach and carry out research. The latter presented no difficulty for Cayley but his classes attracted few students and he concentrated his attention on research. His lectures embodied the results of this work and being of an advanced character were of little use to the average undergraduate. Students of the Victorian Age had their attention focused on the 'paying work' of the Tripos and they turned to private coaches for the appropriate training.[3]

3.2. The foundations of Invariant Theory

The calculus which lay at the basis of the English approach to the Theory of Invariants was the Calculus of Differential Operations.[4] Cayley and Sylvester considered the foundations of this calculus and collaborated in this work though it was Sylvester who took the initiative and greater interest at this time.

Sylvester made few contributions to Invariant Theory between the time of his [1854a] and his professorial appointment at Baltimore in 1876.[5] One was his notable [1864a] and the others (to be discussed here) were his [1866a] and [1867a].

The papers [1866a] and [1867a] were, in part, an attempt to place the Calculus of Differential Operations on a proper footing. His first step was to explain the meaning of an operator[6].
An algebraical function ϕ in the two sets of elements

$$\left(a, b, c, \ldots ; \frac{d}{da}, \frac{d}{db}, \frac{d}{dc}, \ldots\right)$$

was an algebraic formula of the kind which arose in the Theory of Invariants for instance, $a\frac{d}{db} + 2b\frac{d}{dc} + 3c\frac{d}{dd} + \ldots$.

As a formula, Sylvester argued it was not an operator (he called it an operant or corpus) . To Sylvester, an operator was more than an algebraic formula. To make the distinction, he added a star symbol $*$ to the 'corpus' to obtain the operator $\phi*$ (For example, x^2 could be converted to the operator x^2*)
In his manner of speaking, the corpus was thus 'energised' and as he later described the distinction:

> I might have used the word vitalised to convey the same idea, - the operator being the operant endued with power of action, but none the less capable of being acted upon, calling to mind the relation between dead and living matter
>
> [Sylvester, 1867a; SP2, 608 f.n.]

The symbol $\phi*$ was an operator which operated on all that followed. The peculiarity of the symbol $\phi*$ from a modern standpoint is that it applied only to the symbols a, b, c,... and not to the differential operations $\partial_a, \partial_b, \partial_c, \ldots$
In one of Sylvester's simple examples [Sylvester 1866a; SP2, 568]

where

$$\phi = x\frac{d}{dx}$$

the expression

$$\phi * \phi = \left(x\frac{d}{dx}\right)* x\frac{d}{dx} = x\frac{d}{dx} = \phi$$

Mid century British mathematicians working in the Calculus of Operations allowed their operations to act on part of the operand and in this respect, an operation differed from the modern function. In the Calculus of Operations it was permissible in considering the meaning of $LM\,(x+y)$ (L, M operations; x, y operands) for L to refer to x only and M to y only.

Thus $LM(x+y) = Lx + My$

and $LM\,(xy) = Lx\,My$

So, for instance, the operation 1 being understood, it was permissible to write

$$M(xy) = xMy$$

In the star notation, the 'composition' of two operators was written $\phi * \psi *$ and compared to Cayley's earlier formulae[7] Sylvester wrote:[8]

$$\phi * \psi * = (\phi\psi)* + [\phi * \psi]*$$

$$\psi * \phi * = (\psi\phi)* + [\psi * \phi]*$$

The symbol $\phi\psi$ meant simply multiplications of functions (as ordinary quantities) while the symbol $\phi * \psi$ meant the operator $\phi *$ acting on ψ

Sylvester developed formulae in [1866a] for the composition of n operators of the type:

$$E_1 = \sum \left(a\frac{d}{db} + 2b\frac{d}{dc} + \cdots \right)$$

which he called an Extensor. While [1866a] was being printed, Cayley realised that Sylvester's work applied to a wider class of functions than originally thought. Cayley realised that ϕ, ψ could be taken as bilinear forms in the variables (a, b, c,...) and

$$\left(\frac{d}{da}, \frac{d}{db}, \frac{d}{dc}, \ldots \right)$$

Sylvester was also thinking along similar lines:

> I sit down at length to reply to your welcome note to which I referred in mine of yesterday. At the very moment it came to hand, I was intent upon generalising the theory of Extensors. In fact the whole of that day and the day before, struck with the fact of the theorems holding good for
>
> $$a\frac{d}{da}, \quad a\frac{d}{db} + b\frac{d}{da}, \quad \text{etc.}$$
>
> (also for $a\delta_b + 2b\delta_c \ldots + a'\delta_{b'} + 2b'\delta_{c'}$,
>
> as noticed in my paper sent to press) I had been preparing to set to work to ascertain the full extent of the generalisation when your note came to hand - I have embodied its contents with marks of quotation in a postscript [Sylvester, 1866a;SP2, 571] to my paper in the Phil Mag on the subject along with other new and I think important matters. I have said that "it occurred to you and myself independently that the theory was capable of generalisation" - which is conformable to the facts of the case (...), I cannot find your equation [Cayley 1854b; CP2, 164 ; Sylvester, 1867a; SP2, 609]
>
> $$e^{tE*} = (e^{tJ})*$$
>
> in the memoir you referred me to; will you cite for me the passage where it occurs or its equivalent that I may[mention?] [its?] nomination in my paper for the Phil Mag?
>
> I abandon the word Extensors and use Protractor in its place. Then I can speak of a Protractant as well as Protractor; also of Pertractants, Pertractors for the generalised lineo-linear forms in a, b, c, ...
>
> $$\delta_a, \quad \delta_b, \quad \delta_c$$
>
> Universally there is an Algebraical analogous to the logical break of Subject, Copula, [connecting word] Predicate viz. Operant-Symbol of Operation-Operand. The operator = operant energised by addition of the symbol of operation. [App.B, 24 xi 1866] .

From the observation that certain differential operators commuted, Sylvester was led to the theory of commutable matrices.[9]
On this he wrote to Cayley:

> The theory of Polycephalous Pertradantive systems implies that of Commutable Matrices. Have you worked out the condition of Commutability in your memoir on Matrices? For binary matrices the general solution is
>
> $$\lambda \begin{Bmatrix} a+\mu & b \\ c & d+\mu \end{Bmatrix}$$
>
> where giving the outside multiplier λ and μ any values all the matrices so formed are inter-commutable. For ternary matrices an interesting particular solution is
>
> $$\begin{array}{|c|c|c|} \hline & l & \\ \hline & & m \\ \hline n & & \\ \hline \end{array} \qquad \begin{array}{|c|c|c|} \hline & & \nu \\ \hline \lambda & & \\ \hline & \mu & \\ \hline \end{array}$$
>
> If you have determined the conditions of Commutability I should desire to quote your conclusions in my forthcoming Phil Mag Paper. Query. Has not H.J.S.Smith treated of this question. Does it not appear to you that the theory of Matrices is <u>absorbed</u> in that of <u>Pertractors?</u>; another interesting solution of
>
> $$\begin{matrix} a & b & c \\ d & e & f \\ g & h & k \end{matrix} \quad \text{※} \quad \begin{matrix} \alpha & \beta & \gamma \\ \delta & \varepsilon & \varphi \\ F & \eta & \xi \end{matrix}$$
>
> is
>
> $$\begin{array}{|c|c|c|} \hline & l & \\ \hline & & m \\ \hline n & & \\ \hline \end{array} \qquad \begin{array}{|c|c|c|} \hline & \lambda & \\ \hline & & \mu \\ \hline \nu & & \\ \hline \end{array}$$

where

$$\begin{vmatrix} l & m & n \\ \lambda & \mu & \nu \\ 1 & 1 & 1 \end{vmatrix} = 0$$

Have you stated anywhere the number of arbitrary constants which enter into the solution of

$$M * M' = M' * M$$

M, M' being two matrices each of the order i, of which the former may be supposed given?

[App. B, [?] xi 1866]

The letter illustrates Sylvester's somewhat sketchy thoughts on matrices. One point of interest is Sylvester's interpretation of a matrix as an 'operation'. This view is reminiscent of Boole's interpretation of Cayley's matrices which he gave as referee to Cayley's [1858a]. Sylvester considered the left hand matrix 'energised' and becoming an operator. The right hand matrix was regarded as an operand on which this operator acted.

In a reply to Sylvester, Cayley made a slight observation on the use of matrices. Cayley made few applications of matrices to the Theory of Invariants. In this application matrices were limited to their use in providing a compact notation for bilinear expressions in the Theory of Differential Operators:

In the case of two variables, if

$$P_1 = (ax+by)\frac{d}{dx} + (cx+dy)\frac{d}{dx}$$

then in the notation of matrices,

$$P_1 = \left\{\begin{matrix} a, & b \\ c, & d \end{matrix}\right\}(x,y)\left(\frac{d}{dx}, \frac{d}{dy}\right),$$

$$P_2 = \frac{1}{2}\left\{\begin{matrix} a, & b \\ c, & d \end{matrix}\right\}^2(x,y)\left(\frac{d}{dx}, \frac{d}{dy}\right),$$

$$P_3 = \frac{1}{6}\left\{\begin{matrix} a, & b \\ c, & d \end{matrix}\right\}^3(x,y)\left(\frac{d}{dx}, \frac{d}{dy}\right);$$

whence also

$$P_1 * P_2 = P_2 * P_1 = \frac{1}{2}\left\{\begin{matrix} a, & b \\ c, & d \end{matrix}\right\}^3 (x,y)\left(\frac{d}{dx}, \frac{d}{dy}\right) = 3P_3.$$

which accords with your theorem,

$$E_1 * E_2 * = E_2 * E_1 * = E_1 E_2 * + 3E_3 *$$

[Sylvester, 1866a SP2, 576]

≡ [Cayley, 1866d CP7, 8]

In Sylvester's second paper [1867a] he made a further generalisation hoping to provide a 'universal theorem for the multiplication of any number of operators, energised functions of x, y, z,... $\delta_x, \delta_y, \delta_z, \ldots$ freed from all restrictions as to the linearity of form in respect to the latter set.' [Sylvester, 1867a; SP2, 610]

Here $\phi_1, \phi_2, \ldots, \phi_r$ were algebraic functions of x, y, z,... ; $\delta_x, \delta_y, \delta_z, \ldots$ But now Sylvester introduced differential operators which act on the differentials $\delta_x, \delta_y, \delta_z, \ldots$ (but of course not on the symbols x, y, z). Thus

$$\delta'_x = \frac{d}{d\,\delta_x} = \frac{d}{d\left(\frac{d}{dx}\right)}$$

Sylvester's generalisation involved the functions

$$\Delta_{i,j} = \delta'_{x,i} \cdot \delta_{x,j} + \delta'_{y,i} \cdot \delta_{y\,j} + \delta'_{z,i} \cdot \delta_{z,j} + \cdots;$$

where $\delta_{x,i}$ and $\delta'_{x,i}$ were restricted to act exclusively on ϕ_i

One general result was:

$$\phi_1 * \phi_2 * \phi_3 * \ldots \phi_n * = [e^{\Sigma \Delta_{ij}} \phi_1 \phi_2 \phi_3 \ldots \phi_n]*.$$

Sylvester outlined his results [App. B, 30 xi 1866] and in response Cayley provided a proof of Sylvester's result.

The proof, as one might expect, is written in the shorthand of one familiar with the immediate problem. However, it is a proof which would have satisfied Cayley as to the theorem's general truth. Cayley committed to print many of his extempore writings without revision. If he had intended to publish a proof himself, there is no reason to believe the proof would have been any more complete than the thumb-nail sketch he gave to Sylvester:

" Write

$$\xi = \delta_x, \quad \eta = \delta_y$$

$$A = (x,y)^a (\xi,\eta)^\alpha$$

namely, A, any function of degrees a, α; and so

$$B = (x,y)^b (\xi,\eta)^\beta, \ \&c.,$$

and

$$A_{12} = (x_1,y_1)^a (\xi_2,\eta_2)^\alpha, \ \&c.,$$

but all suffixes are to be ultimately rejected. Then

$$\begin{aligned} B*A* &= (x_2,y_2)^b (\xi+\xi_1, \eta+\eta_1)^\beta (x_1,y_1)^a (\xi+\eta)^\alpha * \\ &= e^{\xi\delta\xi_1 + \eta\delta\eta_1} (x_2,y_2)^b (\xi_1,\eta_1)^\beta (x_1,y_1)^a (\xi,\eta)^\alpha * \\ &= e^{\Delta_{01}} B_{21} A_{10} * \quad \text{if } \Delta_{01} = \xi\delta\xi_1 + \eta\delta\eta_1 \end{aligned}$$

Similarly,

$$\begin{aligned} C*B*A* &= (x_3,y_3)^c (\xi+\xi_1+\xi_2, \ \eta+\eta_1+\eta_2)^\gamma \\ &\qquad (x_2,y_2)^b (\xi+\xi_1, \eta+\eta_1)^\beta (x_1,y_1)^a (\xi,\eta)^\alpha * \\ &= e^{(\xi_1+\xi)\delta\xi_2 + (\eta+\eta_1)\delta\eta_2} (x_3,y_3)^c (\xi_2,\eta_2)^\gamma \ldots\ldots \\ &= e^{\Delta_{12}+\Delta_{02}} C_{32} \, e^{\Delta_{01}} B_{21} A_{10} * \\ &= e^{\Delta_{12}+\Delta_{02}+\Delta_{01}} C_{32} B_{21} A_{10}, \end{aligned}$$

and so on

This seems the easiest proof of your general theorem."

[Sylvester, 1867a; SP2, 611]

The proof depended only on devices which were stock-in-trade to mid century practitioners in the Calculus of Operations. Cayley made use of the artifice whereby ξ was replaced by $\xi + \xi_1$ and use was made of Maclaurin's expansion for the symbolic form e^D where D represented $\xi \delta_{\xi_1} + \eta \delta_{\eta_1}$

Sylvester's later generalisations gave rise to ponderous formulae. Even Sylvester himself had doubts as to their validity but with characteristic optimism expressed the belief that 'even a wrong rule is preferable to anarchy and confusion' [1867a; SP2, 614n] . Looking back it is now clear that what was needed, was not more unwieldy formulae, but a more satisfactory basic calculus.

3.3. Quantics revisited

Since his arrival in Cambridge, Cayley had been attracted to Geometry as his main research interest. He returned to quantics with the preparation of the Eighth Memoir on quantics [1867a], the previous memoir in the series being [1861c]. In the year following the publication of the Eighth Memoir, Gordan was to publish his famous theorem for binary quantics, a theorem which established a watershed in the theory of algebraic forms. As Cayley delved further into the computational problems associated with quantics the great problem of the syzygies (or dependencies between covariants) became more apparent. As a consequence, the work in [1867a] showed signs of becoming increasingly more detailed. Writing six years after the preceding memoir in the series Cayley resumed the central subject of the earlier Second and Third memoirs - the binary quintic form. In these earlier memoirs [1856a, 1856b] all the covariants of degree five or less for the quintic had been given. In preparing [1867a] he remarked that 'it was interesting to proceed one step further, viz. to the covariants of degree 6' [1867a; CP6, 147].

To this end he produced in [1867a] two covariants of degree 6; one of order 2 and the other of order 4. To date Cayley's account for the quintic showed a total of seventeen irreducible covariants given in their full Cartesian form. But how long would he have continued these calculations believing as he did that the quintic possessed no finite set of irreducible covariants?

For the covariants of degree 6 though, there was a new phenomenon. Whereas for covariants of degree 5 there had only been one syzygy connecting them, for the covariants of degree 6, there were no fewer than seven such bonds (six of which were irreducible):

[sic] $E^2 + 4C^3 + A^3D - A^2BC = 0$ (order 18)

$-6ACD - EF - 4BC^2 + A^2H = 0$ (order 14)

$AL + 3DF - 2CI = 0$ (order 12)

$4B^2C + 12ABD - A^2G + E^2 = 0$ (order 10)

$AK + 2BI - 3DE = 0$ (order 8)

$AJ + 2BH - B^3 - CG - 9D^2 = 0$ (order 6)

and the reducible syzygy

$A\{AI + BF - CE\} = 0$ (order 16).

[1867a; CP6,151].

The way Cayley would have proceeded in the absence of Gordan's theorem is fairly clear. He would have calculated covariants of higher degrees for the quintic and both found and counted the syzygies between them. As it was, Gordan's result dictated the course of Cayley's later work and this was acknowledged in his Ninth Memoir on quantics [1871a] . This important result provides a convenient point for briefly comparing Cayley's methods with those of the German algebraists.

Gordan's Theorem and the symbolic method

Paul Gordan (1837 - 1912) in his [1868a] proved a theorem which established his mathematical reputation. The theorem demonstrated, through a 'symbolic method', that any binary quantic possessed a finite number of irreducible covariants. In the case of the binary quintic and binary sextic, Gordan computed the irreducible covariants using the symbolic method. For the binary quintic he produced the 23 irreducible covariants and for the binary sextic he produced the 26 irreducible covariants. This theorem is well known and at the time marked a real advance in the Theory of Invariants.

But while Cayley fully acknowledged Gordan's achievement, in [1871a] he expressed reservations. For several reasons, Gordan's results were not fully satisfactory to Cayley.

The symbolic method

The two constituents of the 'symbolic method' were a symbolic notation for writing algebraic forms and a method (transvection = Uebereinanderschiebung) for combining two forms when written in the symbolic notation.

Cayley had lightly touched on a way of writing a binary form similar to the German method in his Introductory Memoir [1854a] but this was not pursued. The method of transvection itself was one instance of Cayley's more general theory of 'hyperdeterminant derivation' which he introduced in his [1846b] but dropped in preference to the 'Partial differential equation' synthesis in the 1850s.

Cayley's notation for the binary quantic of order n was

$$(a, b, c, \ldots)(x, y)^n$$

while in the symbolic notation it was denoted by

$$(\alpha_1 x_1 + \alpha_2 x_2)^n = \alpha_x^n$$

In this notation α_1, α_2 were umbrae (or 'undetermined quantities') and were used to represent the actual values a, b, c,... However, they had no meaning when taken by themselves; the umbrae α_1, α_2 were only capable of interpretation in terms of a, b, c, ... when combined in expression of degree n. This was done by comparing the expanded form of $(a, b, c, \ldots)(x,y)^n$, with

$$(\alpha_1 x_1 + \alpha_2 x_2)^n = \alpha_1^n x_1^n + {}^nC_1 \alpha_1^{n-1} \alpha_2 x_1^{n-1} x_2 + \cdots + \alpha_2^n x_2^n$$

so that, for example:

α_1^n	represented	a
$\alpha_1^{n-1}\alpha_2$	"	b
$\alpha_1^{n-2}\alpha_2^2$	"	c

The product of letters a, b, c,... could be represented by the product of umbrae but the representation was not unique (For instance ac and b^2 were both represented by $\alpha_1^{2n-2}\alpha_2^2$)

The difficulty was avoided by using different but equivalent umbrae within products. In this way ac was represented by $\alpha_1^n \beta_1^{n-2} \beta_2^2$

and b^2 by $\alpha_1^{n-1} \alpha_2 \beta_1^{n-1} \beta_2$

The notation gave a succinct way of representing invariants and covariants.

EXAMPLE

In the case n = 2 (binary quadratic) where

ac represented by $\alpha_1^2\beta_2^2$ ......

and b^2 represented by $\alpha_1\alpha_2\beta_1\beta_2$ ....

Expanding the symbolic determinant written $(\alpha\beta)$:

$$(\alpha\beta)^2 \equiv \begin{vmatrix} \alpha_1 & \alpha_2 \\ \beta_1 & \beta_2 \end{vmatrix} = \alpha_1^2\beta_2^2 - 2\alpha_1\alpha_2\beta_1\beta_2 + \alpha_2^2\beta_1^2$$

an expression which therefore represented 2 (ac − b^2).

Invariants of binary forms were represented products of the kind $(\alpha\beta)$ while covariants additionally involved expressions $\alpha_x = \alpha_1 x_1 + \alpha_2 x_2$

Written in symbolic notation the complete set of covariants for the binary cubic was:

$\underline{U} = \alpha_x^3 = (\alpha_1 x_1 + \alpha_2 x_2)^3$ (the cubic, degree 1)

$\underline{H} = (\alpha\beta)^2\alpha_x\beta_x$ (covariant of degree 2)

$\underline{\Phi} = -(\alpha\beta)^2(\alpha\gamma)\beta_x\gamma_x^2$ (covariant of degree 3)

$\underline{\nabla} = (\alpha\beta)^2(\alpha\gamma)(\beta\delta)(\gamma\delta)^2$ (invariant of degree 4)

The Transvection Process

This process was used by Gordan to generate covariants. It was a special case of Cayley's 'hyperdeterminant derivative' method. Cayley's derivation method was concerned with expressions of the form:

$$\overline{12}^{\alpha}\,\overline{13}^{\beta}\,\overline{23}^{\gamma}\ldots U_1 U_2 U_3\ldots$$

in order to find invariants, where, for instance, $\overline{12}^{\alpha}$ denoted the differential operator $(\xi_1\eta_2-\xi_2\eta_1)^{\alpha}$ (see Chapter 1, page 30).

The definition of a transvectant was formal. The kth transvectant of α_x^n, β_α^m is the symbolic expression:

$$(\alpha_x^n, \beta_x^m)^k = (\alpha\beta)^k \alpha_x^{n-k} \beta_x^{m-k}.$$

However, the process of finding transvectants in general was not purely formal. (Transvectants could not be obtained by mere substitution in the case where binary quantics were not in the simple forms α_x^n, β_x^m). The actual formulae for finding the kth transvectant is obtained from the following considerations. Using Cayley's differentiation operator (the operator $\overline{12}$ above)

$$\Omega = \frac{\partial^2}{\partial x_1 \partial y_2} - \frac{\partial^2}{\partial x_2 \partial y_1}$$

and the identity:

$$\Omega^k \alpha_x^n \beta_y^m = \frac{n!}{(n-k)!}\,\frac{m!}{(m-k)!}\,(\alpha\beta)^k \alpha_x^{n-k} \beta_y^{m-k}$$

the kth transvectant of α_x^n and β_x^m could be written

$$\frac{(n-k)!}{n!}\,\frac{(m-k)!}{m!}\,[\Omega^k \alpha_x^n \beta_y^m]_{x=y}$$

This was identical (apart from the numerical factor) to a special case of Cayley's 'hyperdeterminant process.'

The irreducible invariants and covariants of the binary cubic form could each be obtained by transvection:

$$(\underline{U},\underline{U})^2 = \underline{H}\ ;\quad (\underline{U},\underline{H})^1 = -\underline{\Phi}\ ,\quad (\underline{U},\underline{\Phi})^3 = -\underline{\nabla}$$

The theorem in Gordan's [1868a] singled out by Cayley as of the greatest importance stated that: the covariants of a given degree n can be obtained by transvection from the parent quantic f and a covariant of degree $(n-1)$.

Recalling that $\underline{U}$, $\underline{H}$, $\underline{\Phi}$ and ∇ are respectively of degrees 1, 2, 3, and 4, the transvections for the binary cubic (given above) illustrate this theorem.

Cayley used Gordan's results in completing his calculations for the binary quintic. But Gordan's work did not remove the binary quantic from the field of mathematical research. From Cayley's computational standpoint especially, the problem was not 'solved' and even with Gordan's Theorem progress was slow. Sylvester indicated the difficulty of the computational problem when writing to Cayley on a different matter:

> But why should be expect to do this [to enumerate single cyclodes, Sylvester, 1869a] for all degrees seeing how limited our powers of enumeration extend in the case of Invariants which have been so long the subject of study? (this consoling reflexion has only just occurred to me)
>
> [App.B, 21 vi 1869]

<u>Cayley's Ninth Memoir</u>

With the aid of Gordan's results, Cayley completed the tabulation of the covariants of the quintic in the <u>Ninth Memoir</u> [1871a]. He had reached a milestone in the Theory. Looking back to the general objective of his [1846b]: 'To find all the derivatives (invariants) of any number of functions.....' (Chapter 1, p.31) the actual computed results seem fairly meagre.

According to Sylvester, Cayley's conclusion that the binary quintic had an infinite basis of the quintic held up progress in the Theory. It probably acted as a brake on Cayley's calculations between 1861-1867 but in seeking reasons for slow progress some recognition of the problem's computational complexity should be made. The next problem to be tackled was to find the syzygies associated with the quintic and higher order binary quantics. This was to be an even more difficult problem. Cayley had recognised its difficulty twenty-five years earlier as the question 'which appears to present very great difficulties' [1846b;CP1, 95].

But before he attempted these new computations he discussed the underlying reason for falsely concluding that the irreducible covariants for the quintic were infinite in [1856a] .

In the case of the binary quintic he calculated that there were ten composite covariants of degree 8 and order 14 with six linear dependences existing between them. From this he argued that only four (10-6) of the covariants were in fact linearly independent. From Cayley's Law, the number of linearly independent covariants was found to be five because

$$P(0,1,2,3,4,5)^8 13 - P(0,1,2,3,4,5)^8 12 = 5$$

The conclusion reached was that a covariant existed which was not reducible. Applying an argument of this kind to other covariants it could be concluded that an infinite number of irreducible covariants existed. As is well known, the six linear relations between covariants were themselves linearly dependent and in reality equivalent to only five independent linear relations.

From this consideration it was clear that the 5 composite covariants (=10-5) were accounted for by Cayley's Law and there was no irreducible covariant of degree 8 and order 14. Irreducible covariants could not be produced ad infinitum.

But Cayley's Law itself was based on the assumption of linear independence, a fact which was overlooked at the time and Cayley did not appear to have doubts as to its validity. Cayley's unawareness of the complication of the linear relations between covariants themselves being themselves linearly dependent (second order syzygies) was most likely a consequence of his calculatory approach. Working through from the simplest cases, calculating the covariants as he went, Cayley was obviously only experienced with phenomena he had actually encountered. By the time of the <u>Eighth Memoir</u> [1867a] he had not reached the point where the problem of the second order syzygies had shown itself. In the <u>Eighth Memoir</u> he had only calculated covariants up to degree 6 for the binary quintic form. Cayley's gradualist approach with an empirical outlook did not allow him to take proper account of difficulties he had not encountered.

Pragmatism dictated that higher cases were like lower cases unless shown otherwise.

Refined generating function methods

In the Ninth Memoir [1871a] Cayley adopted a new point of departure in the method of generating functions. In the approach adopted in the 1850s the basic Euler generating function had been primarily used for counting invariants. In the Ninth Memoir, Cayley made a slight change in the Euler generating function to obtain a refined generating function. But this new approach foundered in the case of the binary quintic form.

In the 1850s, the number of linearly independent covariants of degree θ and order s was obtained as the coefficient of $z^{\theta}x^{q}$ ($q = \frac{1}{2}(n\theta - s)$ = weight) in the ordinary Euler generating function expression. In the case of the binary cubic:

$$\frac{1-x}{(1-z)(1-zx)(1-zx^2)(1-zx^3)}$$

In his [1871a], Cayley changed the form of this generating function by a simple transformation of the variables. Cayley replaced x by $\frac{1}{x^2}$ and z by ax^3 to obtain the expression

$$\frac{1-x^{-2}}{(1-ax^3)(1-ax)(1-ax^{-1})(1-ax^{-3})}$$

The drawback of this form was its awkwardness for purposes of calculation and it gave no means of determining the complete system of covariants [Franklin, 1880a, 129] . It was called the Crude form of the generating function. Cayley then observed [1871a; CP7,339] that this could be written in the form:

$$A(x) - \frac{1}{x^2}A\left(\frac{1}{x}\right)\,..$$

where

$$A(x) = \frac{1-a^6x^6}{(1-ax^3)(1-a^2x^2)(1-a^3x^3)(1-a^4)}$$

A(x) was called the Numerical Generating Function and when expressed in least terms (No common factors in numerator and denominator) it was called the Minimum Numerical Generating Function [Cayley,1871a;CP7,339]. A(x) involves only positive powers of x and is the only part of the generating function relevant to the Theory of Invariants. For example, the number of linearly independent covariants of degree θ and order s is the coefficient of $a^{\theta}x^{s}$ in A(x).

This approach was successful for the low order cases of a binary quantic, but as already noted, it failed in the case of the binary quintic. In this case, Cayley was unable to find an expression for A(x) with a finite numerator [1971a; CP7,340] .

3.4. The way ahead - the 1870s.

After the appearance of [1871a] Cayley appeared to lose some interest in the Theory of Invariants. For a time, research associated with the quintic was suspended and his attention appears to have been drawn to geometrical questions. Sylvester's interest in Invariant Theory remained dormant until his appointment at Johns Hopkins University in 1876. After this date, both Cayley and Sylvester resumed Invariant Theory. They were both primarily interested in calculation, but their work on Gordan's Theorem and Cayley's Law continued to be of importance to them.

Reproving Gordan's Theorem

Cayley had presented a synopsis of Gordan's Theorem and its proof in his [1871a], but, as he remarked then, the proof was a difficult one to understand:

> I cannot but hope that a more simple proof of Professor Gordan's theorem will be obtained - a theorem the importance of which, in reference to the whole theory of forms, it is impossible to estimate too highly.
>
> [Cayley, 1871a; CP7, 353] .

This can be taken as a resolution on Cayley's part to supply such a proof. In the ensuing years neither he nor Sylvester gave up hope of supplying one for Cayley believed a much simpler proof would eventually be found [1871e, CP8, 566]. If Cayley and Sylvester could give a simple proof based on their own principles of ordinary algebra it would vindicate their own non-symbolic methods.

The complexity of Gordan's proof was not the only barrier to Cayley's whole-hearted acceptance of Gordan's method. Another obstacle was a consequence of his particular interest in calculating the invariants and covariants in Cartesian form. The symbolic method did not give an efficient algorithm for the computation of covariants. Indeed Cayley estimated that 429 derivations were needed for the computation of the 23 covariants of the binary quintic [1871a; CP7, 353] . For higher order binary forms the number of derivations needed was obviously much greater. Because Cayley was absorbed in calculation, the efficiency of the generating algorithm was paramount. As Gordan's method needed so many derivations in order to uncover the irreducible covariants, Cayley concluded that it was not suitable as a basis for the Theory [1878a; CP10, 378].

There were other drawbacks. Long calculation would be needed in passing from Gordan's symbolic covariants to Cayley's covariant expressions. So much so, that from Cayley's viewpoint covariants were better calculated de novo using the partial differential equation method.

Even from Gordan's standpoint there was the difficulty of deciding which generated covariants were in fact irreducible. There was no a priori procedure of deciding this other than a detailed examination of the covariants of each degree. In the case of the cubic, for instance, the transvectants:

$$(\underline{U}, \underline{U})^2 \;,\; (\underline{U}, \underline{H})^1 \;,\; (\underline{U}, \underline{\Phi})^3$$

were all found to be irreducible, yet the transvectants generated alongside these

$$(\underline{U}, \underline{\Phi})^1 \;,\; (\underline{\Phi}, \underline{\Phi})^1 ,\; (\underline{H}, \underline{\Phi})^1$$

were reducible, being respectively

$$\tfrac{1}{2}\,\underline{H}^2 \;,\; \tfrac{1}{2}\,\underline{\nabla}\,\underline{H} \;,\; -\tfrac{1}{2}\,\underline{\nabla}\,\underline{U}$$

The symbolic method was abstract. By using the transvectant process many covariants could be generated, but if the irreducible complete set were required (not just the establishment of finiteness), the reducible covariants had to be identified and discarded. In this way, the irreducible set of covariants was obtained by 'narrowing down' the totality of covariants generated. If reducible covariants were not identified, the German method would <u>over-estimate</u> the number of irreducible covariants. This procedure is very different from Cayley's where each covariant was tested for irreducibility as it was constructed. The success of Cayley's method depended on being fortunate enough to find the irreducible covariants. Thus Cayley was liable to <u>underestimate</u> the number of irreducible covariants.

The two approaches were complementary for if the German upper bound coincided with the English lower bound then it was likely that the correct result had been obtained. Through the different approaches, a friendly rivalry grew up between the two schools. A focal point for this rivalry was Gordan's Theorem itself. Sylvester on occasions became preoccupied with a desire to give a non symbolic proof of Gordan's theorem. In this he was joined by Cayley but only after Cayley had produced his Tenth and final memoir on quantics [1878a] .
Sylvester keenly felt the nationalistic rivalry which existed between the German and the English invariant theorists. In a letter to William Spottiswoode he wrote:[10]

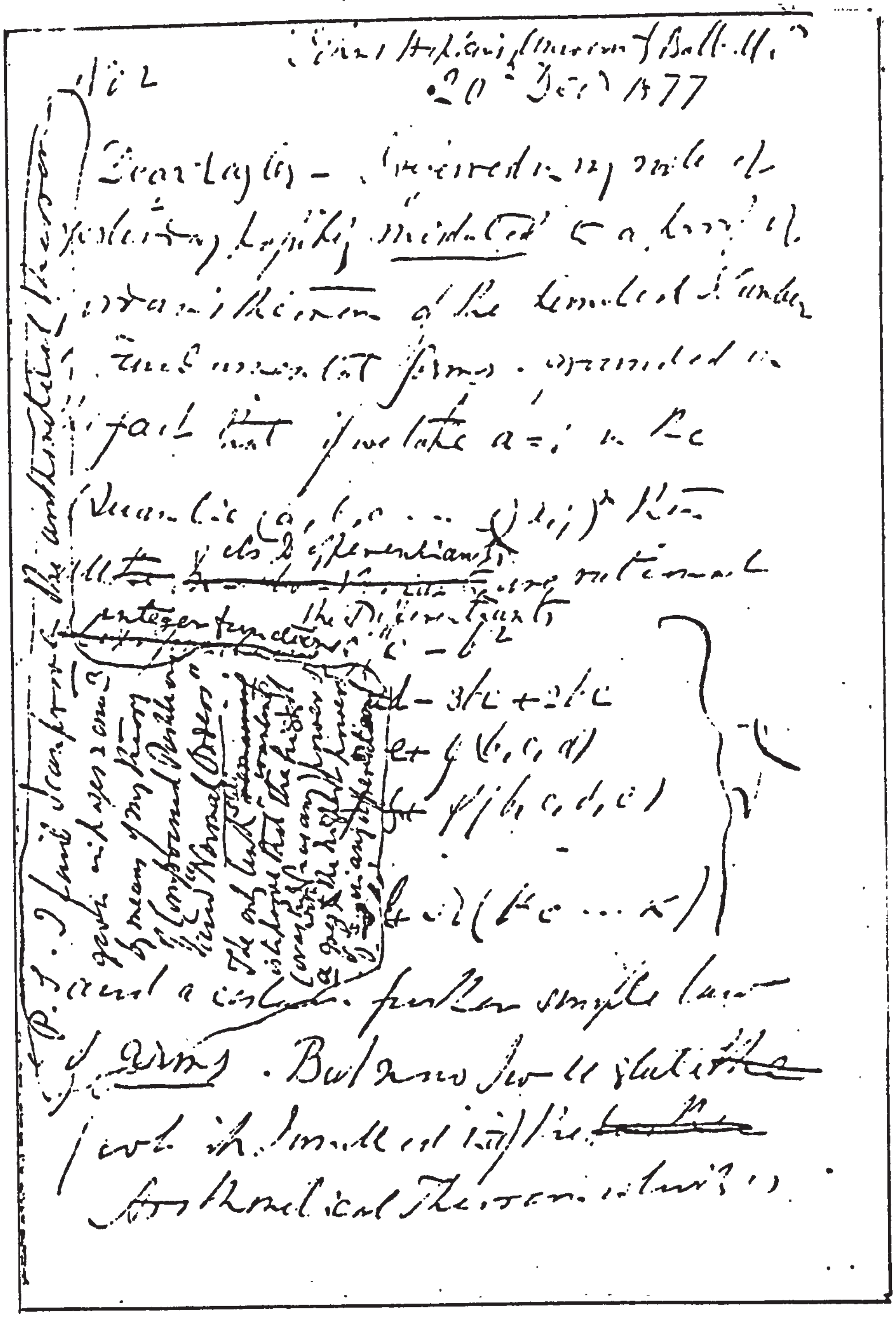
> The verification for the form of the generating function of the 8^{c} has been the result of several hours hard work - The piratical Germans, Clebsch & Gordan who have so unscrupulously done their best to rob us English of all the credit belonging to the discoveries made in the New Algebra will now suffer it is to be hoped the due Nemesis of their misdeeds. Nothing in Clebsch & Gordan is really new but their cumbrous method of limiting (not determining) the Invariants of any given form. This last of their work is now I think destined to be blotted out of existence.

Amid the interminable calculations involved with the generating functions, Sylvester is ever loyal to his and Cayley's method of partial differential operators and enumeration by generating functions. In a letter written a few days later (again to William Spottiswoode) he summarized a plan:[11]

> I believe to reduce (this subject (New Algebra) = Invariants) to Annihilation all that the school of Clebsch and Gordan by aid of method borrowed by the Germans without acknowledgement from Cayley & myself, have attempted in this subject. Their symbolical method is nothing but [the] method of Hyperdeterminants in disguise...

A little later in a letter to Cayley, Sylvester pointed out the weakness of the German abstract method:

> Gordan writes to me that he has proved their [Tables of Grundformen published in Clebsch] correctness - but the proof has never been produced - he says that any a priori proof by his method is not to be looked for - I mean a proof that the supposed grundformen being actually indecomposable. [App.B, 9 v 1877]



<u>Plate 3: Letter from Sylvester to Cayley on Gordan's Theorem.</u> [App B, 20 xii 1877] . Original held at St.John's College, Cambridge [Sylvester Papers] .

Both Cayley and Sylvester avoided the use of elaborate methods. It was in this vein that Sylvester attacked Gordan's theorem. If he could make the 'right' observation, Gordan's theorem could be proved without an elaborate theoretical calculus. On more than one occasion he thought he had succeeded. For example, in 1877 he wrote to Cayley:

> Since I wrote yesterday my ideas on the proof of Gordan's theorem have assumed a more precise form and what was surrounded by a sort of haze or nimbus is now perfectly clear and well defined so that I hope in the course of this very day to draw out my proof.
>
> [App.B, 21 xii 1877]

The German symbolic method, despite its spectacular success in solving the finiteness question for binary quantics was ignored in the English journals [Osgood, 1892a, 251] . Cayley showed more interest than Sylvester in the symbolic calculus and of course recognised its importance. But although he showed the relationship between covariants in the Cartesian notation and in the compact German notation [1871a] the advantages were not sufficient to persuade him to forsake his own methods.

Cayley's Law

This Law which lay at the base of Cayley's method was not completely proved until Sylvester gave his proof in [1877a]. To Cayley and Sylvester, and their direct methods, the Law was crucial but the result seems to have been ignored by the German mathematicians [Sylvester; 1881b; SP3, 524] . The correctness of Cayley's Law depended on the independence of the linear equations derived from the 'annihilation' $\Omega I = 0$ where Ω represented Cayley's linear partial differential operator. Cayley believed these equations were independent as 'there is no reason for doubting that these equations are independent' (Chap 2, p. 79) . Sylvester also held the equations to be independent not by any proof but by 'extrinsic evidence':

> the extrinsic evidence in support of the independence of the equations which had been impugned [Cayley's Law had been called into question by Faà de Bruno] rendered it to my mind as certain as any fact in nature could be, but that to reduce it to an exact demonstration transcended, I thought, the powers of the human understanding [Sylvester, 1878a; SP3, 117]

Sylvester was exhilarated at finding a proof. A letter to Cayley referred to the discovery:

> I have been fortunate enough in the last day or two to discover a rigorous proof of the theorem that the number of linearly independent differentiants (i.e. functions D which satisfy the equation
>
> $$(a\delta_b + 2b\delta_c + 3c\delta_d + \cdots \quad)D = 0)$$
>
> is equal to and cannot be greater than the difference between the two well known denumerants - in other words the Independence of the Equations given by your fundamental theorem in the second Memoir on Quantics.
> The independence only comes in as an inference - I disprove the possibility of the number being greater than the difference of the Denumerants by a wonderfully beautiful method and have sent the proof to Borchardt[12] for insertion in his Journal— [App.B, 6 xi 1877]

This was one of Sylvester's great triumphs and a result of some importance. Cayley's Law had been relied on for twenty years but it was Sylvester's proof which eventually established it. Sylvester's strategy for proof [1878a] has a modern ring; by assuming a certain set of forms linearly dependent, he deduced a contradiction.

It is possible that Sylvester's enthusiastic letters from America made little impression on Cayley even allowing for a natural interest in such a proof. Replying to Sylvester's letters, Cayley wrote:

> I have to thank you very much for several letters, tho' to be candid I have not been able to put myself sufficiently into your covariant theory to follow them in any satisfactory manner. I am always looking forward to the complete memoir which you are to write on the subject - and I am a little disappointed to find from the contents of your Journal that you throw off with only Notes on the subject (...) I hardly know what Mathematics I have been doing - Certainly not much - & I have been hindered from looking at my paper on the covariants of a quintic [App.B, 7 ii 1878].

The binary quintic and higher order forms

The reason for Cayley's lapse of interest in Invariant Theory after the publication of his Ninth Memoir on quantics [1871a] may have been connected with his inability to find a finite numerical generating function for the binary quintic. After Sylvester had proved the correctness of Cayley's Law he managed (at about the same time) to find the expression for finite numerical generating function of the binary quintic.

Sylvester wrote to Cayley on the discovery of this result:

> I have looked into your paper in the Q.M.J. [CP10, 278; misdated?] and find which I had an idea of (after inspecting y^r ninth Memoir - where also I found you had been beforehand with me in the particular shape given to the Crude Generating Function) that your process is identical with mine - The form for the Quintic you will find 'ere this reaches you (or ought to do so in the Comptes Rendus)[1877b; SP3, 58]
>
> [App. B, 9 v 1877]

Cayley returned to Invariant Theory with his Tenth Memoir on quantics [1878a] and his interest in generating functions on this occasion was rekindled by Sylvester's success.[13]

Cayley introduced a form of A(x) (the numerical generating function) which he called a Real Generating Function and this was used to find the first order or fundamental syzygies of the binary quintic.

For instance:

$$\frac{1-\Phi^2}{(1-U)(1-H)(1-\Phi)(1-\nabla)}$$ [cf. 1878a; CP10, 341]

is the Real Generating Function of the binary cubic.

Cayley's method depended on examining the Real Generating Function. In the case of the cubic it could be written (after cancellation of the factor ($1-\Phi$) as:

$$(1+\Phi)(1-U)^{-1}(1-H)^{-1}(1-\nabla)^{-1}$$

The expansion of this was required in order to find the number of independent covariants and the fundamental syzygies.

For instance, in the case of covariants of degree 6 and order 6 for the cubic, the expressions H^3 and $U^2\nabla$ were found in the expansion. But the covariant Φ^2 was not found in the expansion and Cayley concluded in this case that a syzygy existed between it and the other covariants (Here $\Phi^2 = U^2\nabla - 4H^3$)

An observation of this kind may have led Cayley to the method he applied to the binary quintic.

The covariants found in the expansion of the real generating function were named segregates and the others were named congregates. Thus a listing of the congregates was equivalent to a listing of the syzygies. Cayley provided a list of the degrees and orders of 179 fundamental syzygies in his [1878a]. Cayley's table is shown in Table 3. In the case of the cubic, the congregate Φ^2 for instance, was easy to identify but it was not clear how Cayley was able to write 179 congregates in the case of the quintic. As Sylvester remarked 'The method followed by the eminent author in singling out the fundamental syzygants does not appear (as far as I can make out) to be explicitly stated in his memoir' [1881a; SP3,499].

A comparison with Cayley's Eighth Memoir [1867a] published ten years earlier indicates the advance made in the Tenth Memoir [1878a]. The six fundamental syzygies found earlier (Chapter 3, p.130) are in the top left hand corner in the body of Table 3. The Tenth Memoir was concluded with a partial list of the actual fundamental syzygies of the quintic.[14,15]

		1	d	e	f	h	i	j	k	l	m	n	o	p	r	s	t	v	w	j^2	
0.0	1	b^5 10.10	ag^2 12.8	b^2 7.9	b 5.11	ag^2 13.9	b^2g 12.10	ag^2 14.6	b^2 9.7	bg 11.9	ag^2 15.7	b^2y 14.8	b^3 13.7	b^2y 15.9	b^2y 16.6	abg 16.10	b^3 17.7	b^5 23.11	a 19.5		1
3.3	d		6.6	6.8	6.12	7.7	7.9	ag^2 17.9	8.6	8.10	9.5	9.7	10.4	10.8	11.5	12.6	14.4	16.4	21.3	13.5	d
3.5	e			6.10	6.14	7.9	7.11	8.6	8.8	8.12	9.7	9.9	10.6	10.10	11.7	12.8	14.6	16.6	21.5		e
3.9	f				6.18	7.13	7.15	8.10	8.12	8.16	9.11	9.13	10.10	10.14	11.11	12.12	14.10	16.10	21.9		f
4.4	h					8.8	8.10	ag^2 18.10	9.7	9.11	10.6	10.8	11.5	11.9	12.6	13.7	15.5	17.5	22.4	14.6	h
4.6	i						8.12	9.7	9.9	9.13	10.8	10.10	11.7	11.11	12.8	13.9	15.7	17.7	22.6		i
5.1	j							ag^2 19.7	b^2y 18.8	10.8	ag^2 20.8	11.5	by 18.4	12.6	13.3	by 20.6	y 20.2	18.2	23.1	15.3	j
5.3	k								10.6	10.10	11.5	11.7	12.4	12.8	13.5	14.6	16.4	18.4	23.3	15.5	k
5.7	l									10.14	11.9	11.11	12.8	12.12	13.9	14.10	16.8	18.8	23.7		l
6.2	m										12.4	12.6	13.3	13.7	14.4	15.5	17.3	19.3	24.2	16.4	m
6.4	n											12.8	13.5	13.9	14.6	15.7	17.5	19.5	24.4		n
7.1	o												14.2	14.6	15.3	16.4	18.2	20.2	25.1	17.3	o
7.5	p													14.10	15.7	16.8	18.6	20.6	25.5		p
8.2	r														16.4	17.5	19.3	21.3	26.2		r
9.3	s															18.6	20.4	22.4	27.3	19.5	s
11.1	t																22.2	24.2	29.1	21.3	t
13.1	v																	26.2	31.1		v
18.0	w																		36.0		w

Each term inside this diagram is a deg-order indicating the congregate determined by an irreducible syzygy: viz. the congregate is the product of the outside covariants in the line and column containing the deg-order, and of the literal factor (if any) placed immediately above the deg-order. Thus, line *d* and column *i*, 7.9 indicates the congregate *di*, but, same line and column *j*, 17.9 indicates the congregate $dj \cdot ag^2$, $=adg^2j$.

Table 3

Reproduced from Cayley's Tenth memoir on quantics [1878a CP10, 347]. Table showing the degree and order of the 179 fundamental [first order] syzygies of the binary quintic.

Going beyond the binary quintic, mathematicians tackled the higher order binary forms. Supported by a young and enthusiastic group of mathematicians at Johns Hopkins University, Sylvester carried out the task vigorously. In a letter to Cayley, he wrote:

> I mentioned to you that it was my intention to complete my tables of the N.G.F's by calculating the 7^c and the 10^c and certain combinations in addition to those already worked by me - and would have preferred to have carried out myself the work which I had commenced - but do not desire to preclude you from taking possession of the case of the 7^c if you are particularly desirous to do so.
>
> But why not undertake the 9^c? That is a gigantic labor which I would most willingly relinquish to you and which I know would yield certain new and interesting results in the form of the N.G.F. especially as regards the denominator.
>
> [App. B, 15 vii 1878]

Using a theorem given by Sylvester, Cayley produced the minimum Numerical Generating Function for the binary form of order seven. (Table 4, p.148).

$$
\begin{aligned}
1 \quad & + a \ (-x - x^3 - x^5) \\
& + a^2 \ (x^2 + x^4 + 2x^6 + x^8 + x^{10}) \\
& + a^3 \ (-x^7 - x^9 - x^{11} - x^{13}) \\
& + a^4 \ (2x^4 + x^8 + x^{14}) \\
& + a^5 \ (x + 2x^3 - x^9 - x^{11}) \\
& + a^6 \ (-1 + 2x^2 - x^4 - x^8 - x^{10} + x^{12}) \\
& + a^7 \ (4x + x^3 + 3x^5 - x^9 + x^{11}) \\
& + a^8 \ (2 - x^2 - 3x^6 - 3x^8 - x^{10} - x^{12}) \\
& + a^9 \ (x + 3x^3 + x^5 - x^7 + 2x^9 + 2x^{13}) \\
& + a^{10} (-1 + 4x^2 - x^6 - 2x^8 - 2x^{10} - x^{14}) \\
& + a^{11} (5x + 3x^3 + 2x^5 - x^7 - 2x^9 - x^{11} + x^{13}) \\
& + a^{12} (5 + x^2 - 4x^6 - 6x^8 - 4x^{10} - x^{12} + 2x^{14}) \\
& + a^{13} (x - 4x^5 - 4x^7 - x^9 + x^{11} + 4x^{13}) \\
& + a^{14} (2 + 5x^2 + x^4 + x^6 - 2x^8 + 3x^{12} - x^{14}) \\
& + a^{15} (3x - x^3 - x^5 - 7x^7 - 5x^9 - x^{11} - x^{13}) \\
& + a^{16} (6 + 3x^2 + 3x^4 - 4x^6 - 3x^8 - x^{12} + 5x^{14}) \\
& + a^{17} (-x - 2x^3 - 9x^5 - 8x^7 - 4x^9 - 3x^{11} + 4x^{13}) \\
& + a^{18} (2 + 6x^2 + x^4 + 2x^6 + 2x^8 + x^{10} + 6x^{12} + 2x^{14}) \\
& + a^{19} (4x - 3x^3 - 4x^5 - 8x^7 - 9x^9 - 2x^{11} - x^{13}) \\
& + a^{20} (5 - x^2 - 3x^6 - 4x^8 + 3x^{10} + 3x^{12} + 6x^{14}) \\
& + a^{21} (-x - x^3 - 5x^5 - 7x^7 - x^9 - x^{11} + 3x^{13}) \\
& + a^{22} (-1 + 3x^2 - 2x^4 + x^8 + x^{10} + 5x^{12} + 2x^{14}) \\
& + a^{23} (4x + x^3 - x^5 - 4x^7 - 4x^9 + x^{13}) \\
& + a^{24} (2 - x^2 - 4x^4 - 6x^6 - 4x^8 + x^{10} + 5x^{14}) \\
& + a^{25} (x - x^3 - 2x^5 - x^7 + 2x^9 + 3x^{11} + 5x^{13}) \\
& + a^{26} (-1 - 2x^4 - 2x^6 - x^8 + 4x^{10} - x^{14}) \\
& + a^{27} (2x + 2x^5 - x^7 + x^9 + 3x^{11} + x^{13}) \\
& + a^{28} (-x^2 - x^4 - 3x^6 - 3x^8 - x^{12} + 2x^{14}) \\
& + a^{29} (x^3 - x^5 + 3x^9 + x^{11} + 4x^{13}) \\
& + a^{30} (x^2 - x^4 - x^6 - x^{10} + 2x^{12} - x^{14}) \\
& + a^{31} (-x^3 - x^5 + 2x^{11} + x^{13}) \\
& + a^{32} (1 + x^6 + 2x^{10}) \\
& + a^{33} (-x - x^3 - x^5 - x^7) \\
& + a^{34} (x^4 + x^6 + 2x^8 + x^{10} + x^{12}) \\
& + a^{35} (-x^9 - x^{11} - x^{13}) \\
& + a^{36} . x^{14}.
\end{aligned}
$$

$$1 - ax . 1 - ax^3 . 1 - ax^5 . 1 - ax^7 . 1 - a^4 . 1 - a^6 . 1 - a^8 . 1 - a^{10} . 1 - a^{12}.$$

Table 4

Minimum Numerical Generating Function for the binary form of order 7

Reproduced from Cayley [1879e] See also Sylvester [1878b, 1878c] .

The Minimum N.G.F is the generating function with no common factors in the numerator and denominator. Note the symmetry of the coefficients in the numerator about the middle term a^{18}

The calculations were formidable and the only way they could successfully be carried out was with the use of human computers. These were usually paid from special funds and at this time Sylvester, Cayley and Spottiswoode had a grant from the British Association for the Advancement of Science for investigating Fundamental Invariants of Algebraic Forms.[16] In America, Sylvester had the help of his department and in particular, Franklin[17]:

> Franklin is such a Calculator as probably has no superior (indeed I am sure he has none) in the whole world (for I have had experience of calculators before) and it seems a pity not to utilize such a force when it is at hand.
>
> [App.B, 11 i 1879]

The first stage of the problem was the calculation of the generating function but the real calculatory work appeared to arise in the subsequent treatment of the generating function. In this treatment Cayley and Sylvester had similar though different methods. But the methods shared the characteristic of being both direct and based on experience gained in the cases of low order forms. If the binary forms of higher orders possessed unexpected properties then Cayley's and Sylvester's pragmatic methods could (and did) fail. Cayley's treatment of his Real Generating Functions has already been considered. Sylvester's method, though different, would not have been considered wrong in principle by Cayley.

Sylvester developed a 'sifting' process (which he called the method of Tamisage) of the numerator and denominator of the generating function in a rather complicated way to enable the covariants and syzygies to be discovered.[18] He informed Cayley of the discovery of the method in a letter:

> It seems perfectly monstrous on my part to have allowed your kind and welcome letter to remain so long unanswered. I have been waiting to get to an[undoubted ?] result in what I am engaged upon before doing so and two or three weeks have passed like a shadow whilst I have been engaged in this research. I think I may now announce with moral certainty that my method [of Tamisage] completely solves the problem of finding the grundformen for binary forms and systems of binary forms (without mixture of superfluous forms) in all cases - I have sent an account of the method to the Comptes Rendus [1877b] . I ought to add that anterior to all

> verification this method could not give superfluous forms - but it is metaphysically conceivable that it might give too few grundformen - The principle [Sylvester called this the Fundamental Postulate] I proceed upon is that in interpreting the generating function we are not to assume the existence of more syzygetic relations than those which are necessary to make it consistent with itself and with the fact that every combination of Concomitants is a Concomitant. Even if this had not been true my method would have given a sure means of proceeding step by step from the lowest forms to higher ones until all were exhausted - but it w^d would take too long to go into this in a letter.
>
> [App.B, 23 iv 1877]

Sylvester's method of sifting the generating function (a 'numerical Winnowing process') would only uncover all the covariants if the postulate were true. This postulate was based purely on experience and not on any well formulated theory [App.B, 9 v 1877].

The Fundamental Postulate

The introduction of the Fundamental Postulate by Sylvester for the purpose of calculation illustrates an important difference between the English pragmatic approach and the method of the German algebraists. The Fundamental Postulate, which assumed that new syzygies and irreducible concomitants did not exist for the same degree and order, was based on observation of phenomena which occurred for binary quantics of the first six orders. Sylvester explained the role of the Fundamental Postulate:

> The law itself [Cayley's Law] for the case of a single quantic was first stated by Professor Cayley whilst the theory was still in its infancy.
>
> But besides this fundamental theorem, in order to deduce the tables of groundforms, a fundamental postulate still awaiting demonstration is necessary, which is, that no more linear relations between in - and covariants are supposed to exist than are necessary in order to satisfy the fundamental theorem. The application of this principle in such a mode as to substitute a finite for an infinite process, leads to the use of representative generating functions and the simplified method of tamisage. The validity of the fundamental-postulate which is in accord with the law of parcimony [a similar limitation on the syzygies which were supposed to exist] is verified by its conducting to results which have been proved to be accurate for single binary quantics up to the sixth order inclusive,... [Sylvester, 1879a; SP3, 309]

The relationship between the Fundamental Postulate and the working of the method of Tamisage is interesting. By Cayley's Law, the number of linearly independent covariants (say c) of a specified degree and order could be calculated. If the method of Tamisage uncovered k $(\geqslant c)$ covariants of this degree and order, acceptance of the Fundamental Postulate was equivalent to there being exactly $k - c$ syzygies or linear relations between these k covariants. If the Fundamental Postulate were false then extra linear relations would reduce the number of covariants found by the Tamisage process to a number below c. This would imply that the Tamisage method was incapable of finding all the linearly independent covariants.

Sylvester's belief in the correctness of the Postulate was strengthened by his success in showing that a irreducible covariant of a certain degree and order did not exist for the binary form of order eight. Its non-existence was suggested by the Fundamental Postulate but the proof, given by Sylvester, was independent of the Fundamental Postulate. Sylvester 'used this instance as another exemplification of the validity of the same very reasonable postulate' [Sylvester,1881b] .

3.5. Matrices and Linear Algebras

During the period 1863-1881, matrices appeared to hold little interest for Cayley. Virtually nothing was done by Cayley to further the work set out in [1858a]. From a modern viewpoint, the natural step would have been to consider the problem of 'diagonalisation' of matrices coupled with an eigenvalue and eigenvector analysis in this respect. As shown by Hawkins [1977a], Cayley played little part in the furtherance of Matrix Theory. Both Cayley and Sylvester were familiar with the characteristic equation through geometrical problems such as the intersection of conics. In these problems both the ordinary characteristic equation $\det(A-\lambda I)=0$ and the generalised characteristic equation $\det(\mu A-\lambda B)=0$ were essential to their analytic approach, but in these problems the calculation of the corresponding n - tuple was not explicitly considered. It was only when Sylvester reconsidered matrices in the 1880s that a name (latent root) was given to a root of the characteristic equation. For Cayley, these ideas remained in the geometrical sphere. For instance, he saw the study of principal axes as secondary compared with the theory of conics:
'I remark that, the theory of principal axes once brought into connexion with that of confocal surfaces, all ulterior developments belong more properly to the latter theory' [Cayley, 1862a].
In the theory of principal axes he dealt with the linear equations
$Ax=x$ and $\det(A-I)=0$ [CP8, 435]. But he emphasised that the principal axes could be better obtained by other methods (e.g. Rodrigues' Method).

Where 'diagonalisation' was really needed was in the problem of computing the power of a matrix. Cayley gave a solution for this problem in his [1858a] in the binary matrix case.
The solution was unsatisfactory being given in terms of trigonometric functions (after Babbage) and was purely formal. In 1872, when a comparison with the method of matrices and the competing theory of quaternions was instigated by the Scottish physicist and mathematician, P.G.Tait, the question was reopened.
Tait had written to Cayley in 1872 in the following terms:

> Thomson and I wish to introduce this [quaternion approach to finding square root of a strain] into the new edition of our first volume on Natural Philosophy - but he objects utterly to Quaternions, and neither of us can profess to more than a very slight acquaintance with modern algebra - so that we are afraid of publishing something which you and Sylvester would smile at as utterly antiquated if we gave our laborious solutions of these nine quadratic equations. [Knott, 1911a, 152].

Cayley's paper which compared the matrix and quaternionic method was: <u>On the Extraction of the Square Root of a Matrix of the Third Order</u> [1872a]. This paper was read to the Royal Society of Edinburgh and was connected with Tait's problem of finding the square root of a strain. The intention of the paper was to compare the solution of the problem using matrices with the solution given by Tait in the language of quaternions.[18] Quaternions attracted a good deal of attention in the early 1870s. They were promoted zealously by Tait as a compact notation admirably suited to the needs of Physics. Other mathematicians and scientists, such as Cayley and Thomson, found the older Cartesian methods (including the matrix as a Cartesian method) more manageable and intelligible. In his [1872a], Cayley used the method given in [1858a] for dealing with the Cayley-Hamilton matrix equation. Following Cayley the method is exemplified in the binary case: Given a binary matrix M, it was required to find the square root L.

From the Cayley-Hamilton Theorem:

$$M^2 - pM + q = 0$$

$$L^2 - rL + s = 0$$

and also

$$L^2 = M$$

These three equations were used to solve for L

Thus

$$L = \frac{1}{r}(M + s)$$

and r, s were then computed.

Cayley's test of the correctness of the result was by his much practised means of verification. From the calculated expression for L, L^2 was simply compared with M.

In the case of a third order matrix M, the matrix equations were correspondingly more difficult to solve, though in this case it was still possible to extract a formal result. The equations were more involved. Cayley was forced to solve them by a piecemeal method and the result was unsatisfactory. His intention was to give a formula for the square root and one which could be verified by direct multiplication.

After the dialogue with Tait, Cayley's interest in matrices lapsed until the short paper on finding

$$\left\{\begin{matrix} a, & b \\ c, & d \end{matrix}\right|^n$$

appeared [1880d]. Cayley was led back to this problem twenty years after he first gave its solution (in [1858a]) by a question in fluid motion [App. B, 5 xi 77].

Cayley provided the explicit result:

$$M^n = \frac{1}{\lambda^2-1}\left(\frac{a+d}{\lambda+1}\right)^{n-1}\left\{(\lambda^{n+1}-1)\left\{\begin{matrix} a, & b \\ c, & d \end{matrix}\right| + (\lambda^n-\lambda)\left\{\begin{matrix} -d, & b \\ c, & -a \end{matrix}\right|\right\}$$

where λ satisfied

$$\frac{(\lambda+1)^2}{\lambda} = \frac{(a+d)^2}{ad-bc}$$

and λ is the ratio of the characteristic roots of M although he did not give them this or any other name. The method Cayley used can be directly generalised to an $(n\times n)$ matrix A and is in fact a basic method for giving an expression of A^n in a closed form; as distinct from giving a procedure for computing A^n.

In the case $\lambda^m = 1$, he showed that M was periodic of order m while the case where $\lambda = 1$ (equal characteristic roots) was described as 'very remarkable' by Cayley in that the function M was not periodic at all. The paper was rounded off with a discussion of the behaviour of M^n as n tended to infinity.

Cayley's peripheral interest in matrices in the 1880s did nothing to rival the importance of his earlier memoirs of 1858. His work in matrices showed a preoccupation with matrices of low order. He made no attempt to consider a general theory of matrices to the extent of not even developing a proper notation for the general n x n matrix. In this respect Cayley had acquired a dislike of suffixes.

In the course of his early work in the 1840s Cayley had used subscript notation quite freely. But his later bias against its use is seen in a letter written to Felix Klein:

> I send herewith for the Annalen [Cayley, 1880e] [21] a proof of Schottky's theorem: I have a theory that mathematicians in general are too fond of suffixes, and it is partly intended to show how by the notation employed, of rows and matrices, it is to a very great extent possible to get rid of them The theorem itself from its great generality of form seems to me a very important one - I wish I had known it before writing a paper on the double functions which is to appear in the Phil Transactions. [App. C, Klein, 12 vii 1880] .

And in the theory of forms, especially in the setting up of tables, he made his opposition to suffixes very clear:

> I attach also considerable importance to the employment of the simple letters b, c, d, e and in place of the suffixed ones a_1 , a_2 , a_3 , a_4 &c. [1885e; CP12,275] .

Linear Algebras

Cayley was obviously familiar with Benjamin Peirce's Linear Associative Algebra [1881a] which had been privately circulated in 1870. The London Mathematical Society had received a copy and attention was drawn to the work by Spottiswoode's Address [1872a, 152] . Cayley turned to the consideration of linear algebras with two related papers published in 1881. The first [1881d] was on the Euler Identity [See Dickson, 1919a, 169] . The other paper [1881f] in response to the growing interest in theory of linear algebras in America was titled: On the 8-square Imaginaries. In this paper, Cayley continued his ideas given in his earlier papers [1845d] and [1847a] on the octaves published in the 1840s. The method he used in [1881f] bore a strong resemblance to the methods he used in [1852c] to prove the non-existence of the Euler Identity for 16 squares. As in [1852c], he introduced the units

$$E_0 = 1, E_1, E_2, \ldots E_7 \qquad E_i^2 = -1 \ (i = 1, \ldots, 7)$$

$$E_1E_2E_3 = e_1 \quad E_1E_4E_5 = e_2 \quad E_2E_4E_6 = e_4 \ \ldots\ldots$$

$$E_3E_4E_7 = e_6 \quad E_1E_6E_7 = e_3 \quad E_2E_5E_7 = e_5$$

$$E_3E_5E_6 = e_7$$

where e_i = 1 or -1

and where expressions like $E_1E_2E_3 = e_1$ was a shorthand for the six equations:

$$E_1E_2 = e_1E_3 = -E_2E_1 \ ; \ E_2E_3 = e_1E_1 = -E_3E_2 \ ; \ E_3E_1 = e_1E_2 = -E_1E_3$$

Cayley showed that for no values of the e_i was the resulting algebra an associative algebra. One of the novelties of the work was that the system represented a non-associative linear algebra. In presenting it, Cayley mentioned that he was unaware of any previous work on algebras for which the associative law did not hold. The signs were chosen to preserve the property of the product of the norms of two octaves being equal to the norm of their product. The values of $e_1 = e_2 = e_3$ could be chosen to be unity without any loss of generality and as a consequence:

$$-e_4 = e_5 = e_6 = e_7$$

By choosing [cf Dickson, 1914a, 169]

$$e_4 = 1$$

the following relations

$$E_1E_2E_3 = 1 \ \ldots \quad E_2E_4E_6 = 1$$

$$E_1E_4E_5 = 1 \quad E_2E_5E_7 = -1 \quad E_3E_5E_6 = -1$$

$$E_1E_6E_7 = 1 \quad E_3E_4E_7 = -1$$

from which (and writing $F_7 = -E_7$) the multiplication table of the 'Cayley numbers' was obtained.

3.6. An American invitation

Although Cayley took a brief interest in the Theory of Matrices and linear algebras at the beginning of the 1880s, his principal algebraic interest continued to be in the Theory of Invariants. The demands on Cayley's time for lecturing duties were slight and the classes remained small. To Sylvester he wrote:

> I have not been doing much mathematically - my lectures, on the theory of equations - I always go on with them very much from hand to mouth - took more time than they ought to have done. I think I shall write a book on the subject [written?] [App. B, 24 xii 1880, year estimated].

To which, Sylvester replied:

> I wish you would write a book on the theory of Q^{ns} as you propose. I wish [two?] times over that you would come over here - where you could more than double your emoluments as Professor and where you could command a class of 10 or 20 enthusiastic hearers and followers and really found a School. [App. B, 19 i 1881]

In the middle of the same year, Sylvester reissued the invitation:

> I wish you could come and join us here. I could promise you a class of some 10 at least of most intelligent and sympathetic auditors for your lectures: such men as Craig, Franklin, Mitchell, Ladd (although she is not exactly a man) myself, Story and several most promising young men who bid fair to keep the succession of the Craigs Franklins and the rest. In fact you would have a class of Glaishers for your auditors and the seed you might sow would fall upon a fertile soil...

and continued . . .

> We have not an idle student among us and no single case calling for the application of discipline has ever yet occurred - We number about 200 at present but sooner or later I am sure that a Boom will spring up in our favour and carry our numbers to a far higher figure.
>
> Newcombe, Hill and other mathematicians are in our immediate Vicinity at Washington and Anapolis and Craig and I aspire to convert our so called "Mathematical Seminarum" into the "American Mathematical Society". [App. B, 12 v 1881]

Chapter 3

References

1. For Cayley's first two years the lectures were on Analytical Geometry. Cayley's manuscript of the Introductory Lecture (delivered 3 xi 1863) is held at Columbia University, New York. From 1887-1894 Cayley gave two courses of lectures in each academic year. The titles of these courses are given in [CP8, xlv-xlvi] but the titles did not always correspond to their content.

2. The Correspondence is contained in [Airy, 1896a] and discussed in (Chapter 5, p 223).

3. See [Rothblatt, 1967a] It describes the changing attitudes to teaching and research in Victorian Cambridge.

4. See [Koppelman, 1971a] for the early history of British Algebra (especially in the first half of the nineteenth century) as a development in the Calculus of Operations. Cayley and Sylvester were influenced by this Calculus and this is seen in the foundations of Invariant Theory.

5. The previous paper (prior to his [1864a]) written by Sylvester on Invariant Theory had been his [1854b] In that year he had been anticipated by Hermite in the discovery of the skew invariant of the quintic and he was dejected by this according to a reminiscence in [1869; SP2, 714] According to Sylvester the most important result in his [1864a] was the establishment of invariantive criteria [SP2, 452] for the reality of the roots of the quintic equation [1864a; SP2, 380] .

6. Cayley used the term operation [1854c] but this gave way to his use of operator [1858d] .

7. During the 1850s it had become apparent to Sylvester and Cayley that an operator itself could play the rôle of an operand (the entity on which a symbol operates). Cayley observed [1857d] that a differential operator of the type

$$A\partial_a + B\partial_b + \cdots$$

'partakes of the natures of an operand and operator' and could be called an 'Operandator'. Thus a symbol ϕ was capable of representing both operand and operator.

7. (continued)

Sylvester's $\phi *$ represented that operator and the formulae given in the text corresponded to Cayley's earlier [1854c] :

$$P.Q = PQ + P(Q)$$
$$Q.P = QP + Q(P)$$

8. Example illustrating the working of these formulae

$$\phi = a^2\partial_b \qquad \psi = ab\partial_c$$
$$(\phi\psi)* = (a^2\partial_b\, ab\partial_c)* = (a^3b\partial_b\partial_c)*$$
$$[\phi*\psi]* = (a^2\partial_b * ab\partial_c)* = (a^3\partial_c)*$$

9. Several other branches of algebra impinged on Sylvester's generalised theory of differential operators. According to Sylvester, the 'marvellous property of these operators is that they form a sort of closed group' explaining that two operators combined were equivalent to a third operator. [Sylvester 1866a; SP2, 569] . In his biography of Sylvester, H.F.Baker [SP4, xxxvii] noted that Sylvester wrote nothing on the abstract theory of groups.

10. Letter dated 19 xi 1876 held [Sylvester Papers] St.John's College, Cambridge.

11. Letter dated 25 xi 1876 held [Sylvester Papers] St.John's College.

12. [1878d; SP3, 229] Further references to this theorem in Sylvester's works are: [1877a; SP3,55][1877b; SP3, 93] [1878a; SP3, 117][1886b; SP4, 363]

13. For comparison of generating function methods see [Franklin,1880a].

14. Sylvester also tackled the problem of the syzygies. He produced tables for the quintic and the sextic binary form and his result for the quintic agreed with Cayley's [1881a; SP3, 506] . But Sylvester did not attempt to list the fundamental syzygies in their explicit form as did Cayley.

15. In his [1886a] Hammond showed that one of Cayley's calculated fundamental syzygies was in fact reducible. This reducibility implied that 11 other uncalculated syzygies were reducible so that the total number of fundamental syzygies was in fact 167. Hammond calculated all the syzygies of the quintic in his [1886a].

16. This grant of £50 in the first instance was for the computation of the 'Fundamental Invariants of Algebraical Forms'. Cayley, Sylvester and Spottiswoode were in charge of allocating funds [App.B, 21 viii 1878] . The Grant spanned the years 1879-1882 and was used for calculating the tables of the binary quantics of orders 7, 8, 9 and 10 [Sylvester, 1879a; SP3, 311] .

17. Fabian Franklin (1853-1939) was a Hungarian emigré who studied civil engineering in his early years. His work in mathematics was mainly in Number Theory and Invariant Theory. In 1895 he left mathematics and began a career in journalism [Wilson, 1939a] .

18. For the binary cubic, the 'method of Tamisage' began with the (in Sylvester's terminology, Canonical = Representative) generating function:

$$\frac{1 + a^3x^3}{(1 - ax^3)(1 - a^2x^2)(1 - a^4)}$$

A positive sign in the numerator indicated a (primary) covariant of degree and order (3,3) (A negative sign indicated a syzygy). The denominator indicated (secondary) covariants of degree and order $(1,3)$ $(2,2)$ and $(4,0)$

The information about a primary covariant is removed to the denominator. (In this case by multiplying top and bottom by $1 - a^3x^3$) to obtain:

$$\frac{1 - a^6x^6}{(1 - ax^3)(1 - a^2x^2)(1 - a^3x^3)(1 - a^4)}$$

The process is repeated with the generating function in the new form. Eventually all the covariants appear on the bottom line and the coefficients in the numerator negative. At this stage the search for syzygies could begin [1877b] .

19. See [Cayley, 1895b] for Cayley's argument for the use of co-ordinates in favour of quaternionic methods.

20. Sylvester investigated the problem of finding binary matrices M which satisfy $M^k = I$ for given k [1881d; SP3,556] . His interest in the problem seems to have been awakened by a

detail in the theory of invariants[App.B, 24 xi 77]. Unlike Cayley he generalised the problem and this was most likely the source of his renewed interest in matrices in the early 1880s.

21. As it happened, Cayley regarded part of this work as a development of the notation of his [1858a]. Cayley introduced a form with matrix coefficients:

$$(*)(u,v)^2 = (a,h,b)(u,v)^2 \ldots$$
$$= au^2 + 2huv + bv^2 \ldots$$

where a, h, b were square matrices and

u, v rows $(u_1, \ldots, u_p)$, $(v_1, \ldots, v_p)$

But for Cayley, $(a,h,b)(u,v)$ was in reality the general quadric function of $2p$ letters and the use of matrices was condensed notation for writing this form.

Chapter 4. Johns Hopkins University - later years (1882 - 1895)

4.1. Introduction

Largely through Sylvester's influence, mathematics was a well established discipline at Johns Hopkins University by the time Cayley arrived there at the beginning of 1882. During his stay Cayley gave a course of lectures on Abelian and Theta functions, one of his foremost interests at this time.[1] He also attended a series of lectures on matrix algebra given by Sylvester. A letter to Hirst described his research and the daily round at Johns Hopkins:

> I have been getting [on] satisfactorily enough with my own work on the subject [Abelian and theta functions], making out to myself the very beautiful manner in which Clebsch and Gordan in their book[2] arrive at the Multiple theta functions - and completing the geometrical theory of the double theta functions as derivable from a nodal quartic-on two other days I hear Sylvester, who began a course of "three or at most four" lectures on Multiple Algebra, which he has gone on with since the beginning of the year - & they are not finished - nor likely to be.
>
> [App. C, Hirst, 31 iii 1882]

Cayley never seemed to work on one subject exclusively and in matrix algebra he found time to both encourage Sylvester and make some contributions himself.

4.2. Matrices and linear algebras

In a lecture given to the April meeting of the Mathematics Seminar (at Johns Hopkins University) in 1882, Cayley considered the problem of listing the double algebras which had the property of being both commutative and associative [1882b]. This was a renewal of his earlier interest in algebraic couples given in [1845e] and from the outset, the approach in [1882b] was by the same direct method as adopted in the earlier paper. From the requirement that multiplication in the algebra be a closed operation, the 'imaginaries' x, y were supposed to satisfy (cf. Chapter 1, page 44):

$$x^2 = ax + by$$
$$xy = cx + dy$$
$$yx = ex + fy$$
$$y^2 = gx + hy$$

Cayley regarded the problem as one of determining the relationships between the (ordinary) symbols a, b,..., h so that the resulting double algebra would be both commutative and associative. In order that these properties would hold, Cayley deduced that:

$$x^2 = ax + by$$
$$xy = yx = cx + dy$$
$$y^2 = \frac{cd}{b}x + \frac{d^2 + bc - ad}{b}y$$

a result which gave the relationships:

$$e = c \qquad f = d$$
$$g = \frac{cd}{b} \qquad h = \frac{d^2 - (ad - bc)}{b}$$

The approach was, of course, classificatory and primarily depended on the solution of linear equations. From a modern standpoint, the weakness of the direct approach is that an arbitrary multiplication table representing an algebra may not be found amongst the representative tables. Cayley himself experienced this difficulty and at the Seminar he remarked:

> I did not perceive how to identify the system with any of the double algebras of B. Peirce's Linear Associative Algebra.....; but it has been pointed out to me by Mr.C.S.Peirce, that my system in the general case ad-bc, not equal to zero, is expressible as a mixture of two algebras...
>
> [Eisele, 1976a, vol.1, xvi]=[1882b;CP12,106] .

Cayley read a more substantial paper [1884b] on double algebras to the

London Mathematical Society. In this he gave tables defining non-equivalent associative double algebras over the complex numbers.[3] As noted by Dickson [1914a, 21] Cayley did not assume the presence of a principal unit e ($ex = x = xe$ for all x).

Cayley's work in [1884b] was an extension of B. Peirce's classification of algebras given in the important <u>Linear Associative Algebra</u> [1881a]: For double algebras, Peirce classified 'pure' algebras[4] [1881a, 120 - 122].
Cayley's seven systems included Peirce's 'pure' systems:

	x	y
x	x	y
y	y	0

a_2 in [B. Peirce, 1881a]
<u>commutative algebra</u>

	x	y
x	y	0
y	0	0

c_2 in [B.Peirce, 1881a]
<u>commutative algebra</u>

	x	y
x	x	y
y	0	0

b_2 in [B.Peirce, 1881a]
<u>non-commutative algebra</u>

<u>plus</u> the pure system not included in Peirce's list.

	x	y
x	x	0
y	y	0

<u>non-commutative algebra</u>

The 'mixed' systems given by Cayley, but not included by Peirce were:

	x	y
x	x	0
y	0	y

<u>Commutative algebra</u>

	x	y
x	x	0
y	0	0

<u>Commutative algebra</u>

	x	y
x	0	0
y	0	0

<u>Commutative algebra</u>

Although any multiplication table defining a double algebra was able (though a non-singular transformation) to be transformed to one of these seven types it was not possible using Cayley's analysis to determine a priori which type such a table represented. In particular Cayley did not attempt to classify these algebras through the use of invariants as was done later.[5] However, he must have been aware of some relationship between invariants and linear algebras as his analysis depended on the solution of quadratic and cubic equations [1884b, 62] .

This is a curious omission as Cayley was usually astute in recognising links between different branches of mathematics.

But even in algebraic researches where Cayley did state a connection between different theories, the calculation and classification of particular cases was put before the development of a general theory. It would appear that calculations for Cayley were not merely an adjunct to theory but an essential part of an algebraic problem itself.

Matrix Algebra

By the summer of 1882, Cayley had returned to England. Sylvester, reflecting on his own work on matrix algebra wrote to Cayley:

> I have not done anything more with Matrices. I have moreover been incapable so far of drawing up my projected memoir on the subject even with the aid of the copious notes I have retained of my lectures - Would there were any opening for me at home in England! [App. B, 3 viii 1882] 6

The marginal interest taken by Cayley in the theory of matrices during the 1880s was due mainly to Sylvester's enthusiasm for the subject but it did not absorb Cayley to the same extent. In the brief span 1881-1885, Sylvester was addicted to matrix algebra and during a flurry of activity, especially in 1884 on his return to England, he published frequent notes on the subject.

One aspect of Sylvester's work in the 'new science of multiple quantity' was his treatment of questions in the theory of equations where the coefficients were matrices. Sylvester considered equations such as $p_1 x = q_1$, $p_1 x = x q_1$, and the complete generalisation (his Nivellator Theory):

$p_1 x q_1 + p_2 x q_2 + \cdots p_n x q_n = c$. where p_i , q_i x and c represented square matrices. (For a full account of Cayley's and Sylvester's work during this period see [Hawkins, 1977a, 101-108]).

In his considerations of these various problems, Sylvester examined special cases. This was partly due to the exceptions be found to his putative theorems but also to a belief that the general case was learned from the knowledge of particular instances. A plethora of special cases caused Cayley to offer some advice: -

> I think you should take the bull by the horns and consider the general quadric equation
>
> $$xax + bxc + d = 0$$

i.e.

$$\begin{vmatrix} x & y \\ z & w \end{vmatrix} \begin{vmatrix} a_1 & a_2 \\ a_3 & a_4 \end{vmatrix} \begin{vmatrix} x & y \\ z & w \end{vmatrix} + \begin{vmatrix} b_1 & b_2 \\ b_3 & b_4 \end{vmatrix} \begin{vmatrix} x & y \\ z & w \end{vmatrix} \begin{vmatrix} c_1 & c_2 \\ c_3 & c_4 \end{vmatrix} + \begin{vmatrix} d_1 & d_2 \\ d_3 & d_4 \end{vmatrix} = 0$$

to fix the ideas, I have worked out the four equations which are as follows [... four quadric equations follow...] The general quadric

$$=^n (x, y, z, w, 1)^2 = 0 \quad 15-1 = 14 \text{ constants}$$

hence the four such equations

$$u = 0 \;,\; v = 0 \;,\; w = 0 \;,\; z = 0$$

if perfectly general would contain 56 constants, or combinatively $56-9=47$ constants & the actual number being as above = 14, there is plenty of room for geometrical relations between the four hypersurfaces.
[App.B, 30 vi [1884] ; year estimated]

Although Cayley's advice was unhelpful in finding a solution to the problem at hand, it illustrates his tactical approach of reducing an equation in matrices to the corresponding set of linear equations. This approach was also adopted for the simpler matrix equation px = xq. Sylvester was probably more attuned to matrices at this time and his method unlike Cayley's made use of latent roots.[7] In [1884b], Sylvester gave a necessary and sufficient condition for a non-zero solution to exist for px = xq (that matrices p, q have a latent root in common). Cayley tackled the problem head on. In a letter to Sylvester he explained how he was led to yet another problem:

> Your formula as to px=xq is of course quite right. I had begun working it out in the same way but made a mistake in the multiplication of the matrices - & I found some very pretty & consistent results belonging not to your problem at all but to the different one
>
> $$\begin{vmatrix} x, & z \\ y, & w \end{vmatrix} \begin{vmatrix} a, & c \\ b, & d \end{vmatrix} = \begin{vmatrix} x, & y \\ z, & w \end{vmatrix} \begin{vmatrix} \alpha, & \beta \\ \gamma, & \delta \end{vmatrix}$$
>
> (...) which I was thus led to by a mere accident, would belong as a very particular case of a theory more general than that of the functions of a single matrix - viz to the theory of the functions of a matrix x & the transposed matrix tr. x.
> [App.B, 11 vii 1884 ; year estimated].

Cayley published two papers on the solution of px = xq. [1885g] treated the question where p, q represented quaternions and [1885h] where p, q represented binary matrices.

In both cases, Cayley sought an explicit solution in terms of a formula but he did not attempt to generalise the problem nor did he attempt to characterize properties of p, q, x for which a solution existed. In the case of px = xq where p, q were quaternions (over the complex numbers) he produced a 'synthetic' solution. He communicated his working to Sylvester:

> I have solved $qQ = Qq' = 0$ in a very compendious form - thus
> Let $q = d + v \quad q' = d' + v'$ be given quaternions
> (d, d' the scalars v, v' the vectors) &
> put for shortness
>
> $$\theta = d - d'$$
> $$\alpha = v^2 - v'^2$$
> $$\beta = v^2 - v'^2 \qquad [v^2 = -x^2 - y^2 - z^2]$$
>
> then if we have the single relation
>
> $$\theta^4 - 2\alpha\theta^2 + \beta^2 = 0$$
>
> we have a quaternion Q such that
>
> $$qQ - Qq' = 0$$
>
> or what is the same thing
>
> $$\theta Q + vQ - Qv' = 0 \qquad \text{viz. putting}$$
> $$M = -(\alpha - \theta^2)\theta$$
> $$A = \beta - \theta^2$$
> $$B = \beta + \theta^2$$
>
> the solution is
>
> $$Q = (M + Av)(M + Bv')$$
>
> In fact for this value of Q we have identically
>
> $$\theta Q - vQ - Qv' = \{M - v.v'^2 + v'.v^2 + vv'.\theta\}(\theta^4 - 2\alpha\theta^2 + \beta^2)$$
>
> and therefore if $\theta^4 - 2\alpha\theta^2 + \beta^2 = 0$
>
> we have the required equation
>
> $$\theta Q + vQ - Qv' = 0 \quad [qQ = Qq'].$$

This is a solution for which Tensor Q = 0
[The tensor of a quaternion $d + ix + jy + kz$
is $d^2 + x^2 + y^2 + z^2$]
If $\theta = 0$ then also $\beta = 0$ & we have Tait's
solution, containing an arbitrary vector -
and for which Tensor Q is not = 0.
[App B, 2 ix 1884] = [1885g; CP12,301] .

Sylvester described this solution as 'a very elegant one, [App B, 4 ix 1884] . Cayley's method here is a good example of his skill as a formal algebraist, a skill for which he was much admired by his contemporaries. In describing the solution as 'synthetic' Cayley most likely meant that it was achieved without recourse to his normal approach of reducing questions of this type to a set of linear equations.[8]

The slightly later paper [1885h] treated the question of finding a solution Q of the equation qQ = Qq′ but from a matrix point of view. No use was made of latent roots; a solution was obtained from the resulting set of four linear equations. The final outcome was again an explicit formula for the required solution:

$$Q = \begin{vmatrix} -f\xi - g\eta - h\zeta, & (b\xi - a\eta)h\omega \\ -c\xi(a\zeta + g\omega), & c\eta - b\zeta + f\omega \end{vmatrix}$$

[1885h; CP12, 313].

It wa stated to depend on one arbitrary parameter (because of relationships between the parameters ξ, η, ζ, ω) and consequently cannot be correct. The important point is that Cayley was driving towards an explicit solution and he saw the problem as one of finding a formula. The problem of finding a genuinely general solution Q is difficult to solve without the use of the Jordan form of a matrix where it is equivalent to finding P with $J_q P = P J_{q'}$, where J_q and $J_{q'}$ are the Jordan forms of q and q′ .

It is significant that Cayley published two papers on a subject which had such obvious similarities. Of course he was well aware of the correspondence between quaternions and second order matrices. However, Cayley regarded the two theories as distinct theories, just as in his [1872a] where a comparison between matrices and quaternions methods was drawn. Similarities were observed (as in [1885h]) yet the two theories belonged to different traditions.

As an example of an algebra, quaternions lost some of their importance for Cayley and Sylvester when it was realised that binary matrices and quaternions were equivalent by virtue of the correspondence :

$$\begin{pmatrix} a & b \\ c & d \end{pmatrix} \longleftrightarrow (a+d) - \lambda(a-d)i + (b-c)j - \lambda(b+c)k$$

or equally

$$\begin{pmatrix} a+b\lambda & c+d\lambda \\ -c+d\lambda & a-b\lambda \end{pmatrix} \longleftrightarrow a+bi+cj+dk$$

where λ denotes the complex number $\sqrt{-1}$ as distinct from the quaternion imaginaries i, j and k.
By concentrating on the purely algebraic properties of quaternions and ignoring them as a suitable method for application in analytic geometry, Cayley and Sylvester were both criticised by P.G.Tait. This physicist was interested in real quaternions and saw them as universally applicable both in geometry and the physical sciences.

At this stage Cayley and Sylvester implicitly regarded a quaternion as a complex quaternion (Hamilton's bi-quaternion) and Sylvester brushed aside the 'futile limitation' of real quaternions [1885c; SP4, 275] .

In treating the purely algebraic properties of bi-quaternions and by regarding them as binary matrices, Cayley and Sylvester parted company with Tait. With little to say on the use of quaternions in Geometry and Physics, Sylvester was a little contemptuous of Tait's 'limited' view. Sylvester's opinion of quaternions is recorded in a letter to Cayley:

> I have laboured hard to make Tait understand the identity of Matrices and Quaternions and set out at length the rules for passing from one to another - but the enclosed extract will serve to show you that he is still walking in outer darkness: possibly my note of this day in answer thereto explaining how Matrices are subject to a law of Addition may act as a ray of illumination on this subject.
> [App.B, 3 ix 1884].

In the theory of linear associative algebras the algebra of quaternions and the algebra of matrices presented the two most important examples of the day. Of the two, quaternions were well known to mathematicians, whereas matrices hardly received any attention from British mathematicians until the late 1870s. According to [Crowe, 1967a], Hamilton had used his immense prestige to publicise quaternions. However, Cayley appeared to do little to accentuate the importance of matrices. Indeed matrices seemed to occupy a very minor place among his interests. Cayley did not appear to keep up with the work of other mathematicians on the subject, and when Sylvester and Cayley returned to matrices in the 1880s, Continental developments in the subject were probably unknown to them.[9]

Sylvester effectively finished his energetic researches in multiple algebra at the beginning of 1885. Cayley produced further papers[10] during the following years but they were relatively unimportant compared to his [1858a] and [1858b].

Cayley's survey paper on Multiple Algebra [1887a] was most likely prompted by Sylvester's idea of giving his Inaugural Lecture at Oxford on the history of the geometrical interpretation of imaginary quantities and his subsequent appeal to Cayley for help in its preparation.[11] Cayley separated ordinary algebra from Multiple Algebra. In Cayley's view, ordinary mathematics included ordinary algebra; the theory of real or imaginary magnitudes and the symbols of operation. Multiple Algebra referred to the theory associated with 'extraordinary' symbols introduced by Benjamin Peirce. An important property of Multiple Algebra was that it provided an analytic base for the geometrical theories established at an earlier time. But of course Cayley did not seek a geometrical justification for Multiple Algebra as had been done for complex numbers in an earlier age. The quaternions constituted a valuable theory but, for Cayley, the word was not method, as Tait and Hamilton would have it. Cayley was too imbued with the Cartesian method to take that step.

4.3. Calculation and binary quantics

Cayley's and Sylvester's approach to the calculation of covariants for binary quantics received a setback in 1882 when it was shown by James Hammond that the Fundamental Postulate was false.[12] Sylvester in particular had invoked that 'very reasonable' postulate (Chapter 3, page 150) in satisfying himself that his method gave the correct number of covariants and dependences.

The failure of the Fundamental Postulate

In his [1882a](presented 12 xii 1882), Hammond produced a covariant of degree 5 and order 13 for the binary quantic of order 7 and a dependence of the same degree and order. One version of the Postulate implied that a covariant and dependence could not co-exist for the same degree and order. But by painstaking calculations Hammond was able to find a counter example.

The number of linearly independent covariants of degree 5 and order 13 for the binary quantic of order seven was by Cayley's Law ($\theta=5$, $s=13$, $n=7$) equal to four. This was because

$$P(0,1,2,\ldots 7)^5 11 - P(0,1,2,\ldots 7)^5 10 = 30-26=4$$

(See Chapter 2, page 85 for this calculation). By considering the generating function for the seventh order binary quantic and applying the 'sifting' process it was found that these covariants were composite covariants. In terms of their degree and orders they could be represented by the products:

(1.7) (4.6)
(2.2) (3.11)
(2.6) (3.7)
(2.10) (3.3)

At this point Sylvester would have invoked the postulate to claim the non existence of any linear dependency between these covariants. However, Hammond found such a dependence:

(1.7) (4.6) = (2.6) (3.7) − (2.2) (3.11)

[Hammond, 1882a, 85] .

The existence of this dependence meant that Sylvester's sifting process only discovered at most three (of the four) linearly independent covariants. There was therefore at least one covariant incapable of being found by this 'sifting' process (Hammond's disproof did not falsify Cayley's Law).

In a letter to Cayley, Sylvester compared his own findings with those of Hammond:

> Hammond has sent me a disproof of the Postulate too late I ween for the prize as I had previously announced my own disproof of it in No.1 Vol.V of the Journal [1882a; SP3, 604] and in the Circular for November. It is quite different from mine and more explicit. He finds for instance a covariant of deg-order 5.13 to the 7ic which is a groundform but which the method of Tamisage does not serve to disclose and which consequently will not be found in my table of groundforms for the 7ic!!! [App.B, 25 xii 1882].

Cayley immediately responded to the news of the result:

> The extreme importance of Mr.Hammond's result, as regards the entire subject of Covariants, leads me to reproduce his investigation in the notation ...[1883b; CP11, 409].

Cayley was hopeful that the disproof of the postulate in the case of the binary form of order 7, might show other irreducible invariants and covariants to exist and thereby clear up difficulties associated with the binary seventhic. (As a general rule it was found binary forms of prime order were more difficult to deal with than those of composite order). In a letter to Sylvester, Cayley wrote:

> I have just this moment, as I am writing, received the A.M.J. - and I shall perhaps find that you have been before me, in what I was go [sic] to say - the disproof of the fundamental postulate will at any rate necessitate a revision of your table of the irreducible covariants of the seventhic, and I am in hopes[13] that it may turn out that there is an irreducible invariant of the degree 20: I find it difficult to believe that the expansion of the factor into an infinite series can be right. Is it possible that there can be between the invariants of the 4, 8 and 12 a syzygy of the degree 16, so as to give more than two new invariants of that degree. It occurred to me that

it might be worthwhile to study the relations of the invariant by means of the canonical form

$$(ax+by)^7+(cx+dy)^7+(ex+fy)^7+(gx+hy)^7$$

-the invariants being the functions of the determinants ad-bc, etc.
[App.B, 12 ii 1883].

According to [Morley, 1912a, 47] Hammond's counter example in the case of the binary seventhic was generally accepted by mathematicians as nullifying Sylvester's method for binary quantics of order greater than seven. However, Sylvester thought the binary form of order seven was a special case and believed the Postulate to be true for higher order binary quantics.[14] Morley [1912a] showed that the Postulate failed for odd order binary forms of order seven and above and for even order binary forms of order ten and above. Thus Sylvester's method would work for binary forms of order 2, 3, 4, 5, 6, 8; precisely the cases where his results were in agreement with those of other mathematicians.

To Cayley and Sylvester the binary seventhic was a more difficult case than the binary quintic. New methods were needed. Sylvester wrote to Cayley on the matter:

> In some previous note you referred to the $7^{\underline{c}}$ and the necessity of finding whether or not there is a 20.0 (i.e. invariant of the degree 20 attached to it). I had thought of the Canonical determinants method for ascertaining how this is but as you say of your own efforts without success - The $\underline{8}^{\underline{c}}$ is by the great labours of Von Gall and my own, saved from the general wreck [resulting from failure of the Fundamental Postulate] there was only one doubtful ground-form the 10.4 and that I may recall to you I have proved in the A.M.J. does not exist.
>
> [App.B, 26 v 1883]

In effect, the failure of the Fundamental Postulate destroyed the generating function approach as a method of discovering the irreducible covariants of a binary quantic. At the root of the problem was the existence of an unknown number of linear dependences between covariants - the so-called 'problem of the syzygies.'

4.4. Seminvariants

In the 1880s Cayley's attention was drawn to seminvariants rather than to the invariants and covariants themselves. Allusion had been made to these functions (which satisfied only one of his partial differential equations) at the time he wrote the Introductory Memoir on Quantics. The postscript to this paper [1854c, CP2, 234] indicated their later importance, for by the formula in Cayley's Theorem (Chapter 2, page 78) full covariant expressions could be retrieved from seminvariants.

A seminvariant is the coefficient of the first term of a covariant. The seminvariants associated with the covariants U, H, Φ, ∇ of the binary cubic are respectively:

$$a$$

$$ac - b^2$$

$$a^2d - 3abc + 2b^3$$

$$a^2d^2 + 4ac^3 - 6abcd + 4b^3d - 3b^2c^2$$

They are all 'annihilated' by the differential operator

$$a\partial_b + 2b\partial_c + 3c\partial_d$$

though only the last seminvariant in the list is annihilated by the companion operator

$$d\partial_c + 2c\partial_b + 3b\partial_a$$

and is therefore an invariant. Using Cayley's Theorem the full covariant expressions were easily obtained.

Cayley had outlined the meaning of the term seminvariant in [1860a; CP4, 241] and its importance in [1871e; CP8,566] but seminvariants did not appear to assume great importance in Cayley's general work until the 1880s. The impetus for Cayley's intensive study of seminvariants came from a discovery by P.A.MacMahon.

MacMahon's Discovery

In 1883 P.A.MacMahon (1854-1929)[15] made a discovery (the Correspondence Theorem) which effectively altered the course of Cayley's work in Invariant Theory. MacMahon linked the seminvariants with the theory of symmetric functions and showed that the theory of seminvariants could be considered as part of this well established theory.

MacMahon's correspondence theorem was concerned with the binary quantic

$$x^n + \frac{b}{1!}x^{n-1}y + \frac{c}{2!}x^{n-2}y^2 + \frac{d}{3!}x^{n-3}y^3 + \ldots$$

It stated that any (non-unitary) symmetric function of its roots was a seminvariant when the symmetric function was expressed as a function of the coefficients of the binary quantic [1884a, 131] [16]

MacMahon, and following him, Cayley, used the analogy between seminvariants and symmetric functions to give a universal method for forming linearly independent lists of seminvariants of a given weight and for finding the dependences between them.

In the 1850s Cayley had been concerned with partitions in counting the covariants of a form, but now the arithmetic statement of the problem in terms of symmetric functions (equivalent to partitions) offered something more. It gave a means for actually computing the seminvariants and their dependences.

The elementary symmetric functions could be computed if the weight were not too great.

$$\sum \alpha^2 = b^2 - c$$

$$\sum \alpha^2\beta^2 = \frac{1}{12}(e - 4bd + 3c^2)$$

$$\sum \alpha^4 = \frac{1}{6}(6b^4 - 12b^2c + 3c^2 + 4bd - e)$$

and these non-unitary symmetric functions corresponded to the seminvariants

$$b^2 - ac$$

$$a^3e - 4a^2bd + 3a^2c^2$$

$$6b^4 - 12ab^2c + 3a^2c^2 + 4a^2bd - a^3e$$

To the known relationships between the symmetric functions (in this case $\sum \alpha^4 = \sum \alpha^2 \sum \beta^2 - 2\sum \alpha^2\beta^2)$ the corresponding dependences between the seminvariants (and hence covariants) were automatically established. The relationships between symmetric functions were easily written in terms of partitions as here : [4] = [2][2] -2 [22].

Cayley regarded MacMahon's discovery as one of great importance and it provided the basis for his work in the 1880s.
Sylvester was not so impressed by MacMahon's discovery; to him the result did not appear so novel. Remarking on the result to Cayley, he noted 'This is pretty (and likely to be useful) enough - but it does not seem to me to amount to a Revolution in the theory as it existed in the Pre-Mac-Mahnic times' [App. B, 9 viii 1883].[17]

Cayley's reply (see Plate 5) reiterated the importance:

> The great use of MacMahon's theory is in the means which it affords for making out the whole theory of the syzygies. It is a question of double partitions. thus
>
> Deg.
>
> | Weight | 2 | 3 | 4 | 5 | 6 | 7 | 8 |
> |---|---|---|---|---|---|---|---|
> | 8 | 2222 | 332 | ~~44~~
~~422~~
222·2
22·22 | ~~53~~
33·2 | ~~62~~
~~42·2~~
~~422~~
~~3·32~~
22·2·2 | | ~~8~~
~~6·2~~
~~5·3~~
~~4·4~~
~~4·22~~
~~3·3·2~~
2·2·2·2 |
>
> Of course 422 means $\sum \alpha^4\beta^2\gamma^2$, 4·22 means $\sum \alpha^4 \cdot \sum \alpha^2\beta^2$ and so in other cases.
> The sum of all the numbers gives the weight and the sum of all the first numbers gives the degree. Now consider for instance the degree 4
>
> | | $= \alpha^4$ |
> |---|---|
> | 44 | 4 |
> | 422 | 22 |
> | 22·22 | 2·2 |
> | 222·2 | 22 |
>
> viz. the term in 44 which contains α^4 is $\beta^4 + \gamma^4 + \delta^4 = \sum \beta^4 = 4$ (for the number of roots being indefinite there is no occasion to distinguish between $\sum \alpha^4$ & $\sum \beta^4$)

Similarly in 422 coeff^t of α^4 is
$\sum \beta^2\gamma^2 = 22$; in 22.22 it is
$\sum \beta^2 \sum \beta^2 = 2\cdot 2$ and in 222.2
it is 22.

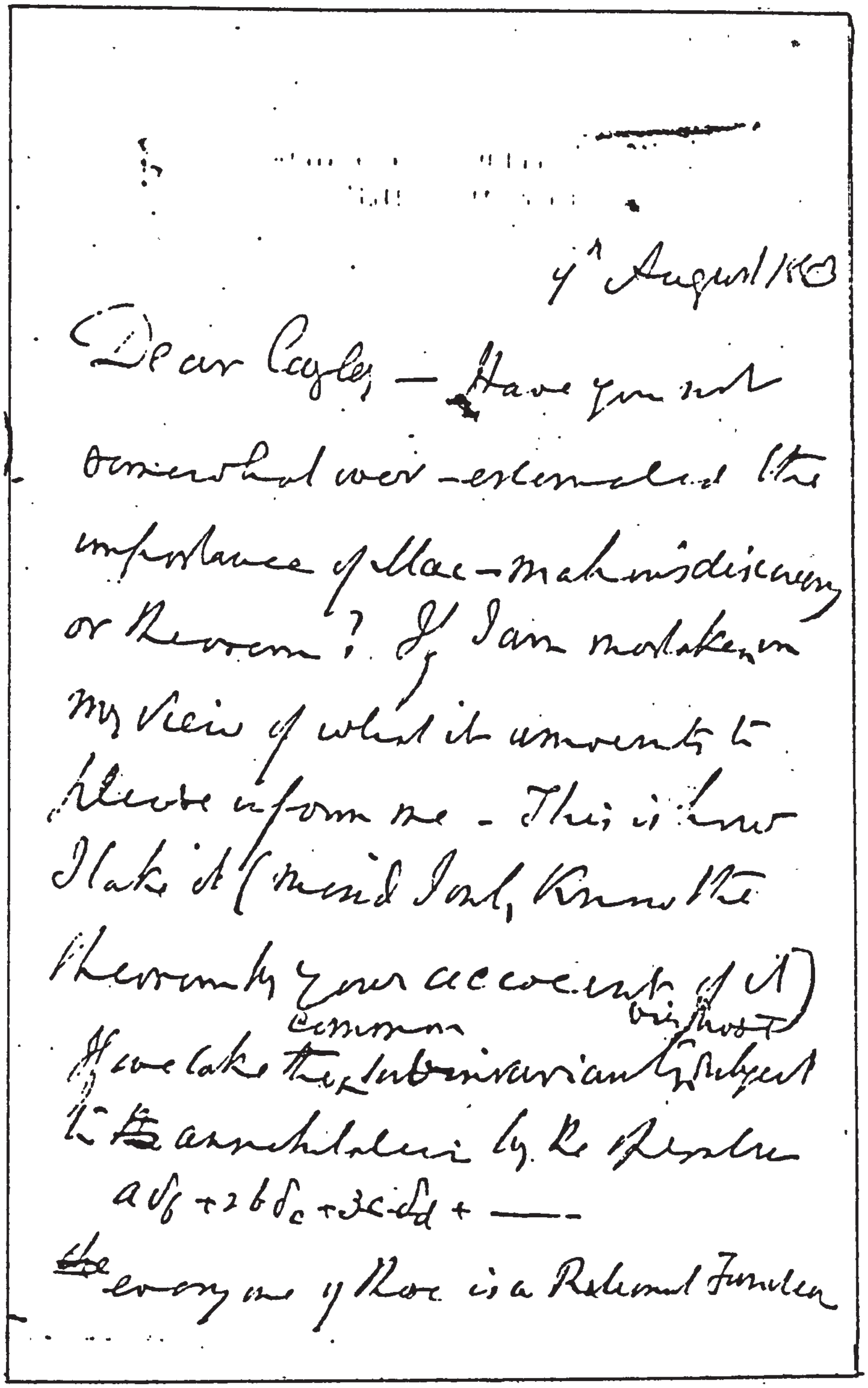
A preceding result at once verificable
is that there is a linear relation
between 4, 22 & 2.2
(the actual equation is 2.2=4 + 22[sic])...
[App.B, 11 viii [1883] , year estimated]

The principal interest in the MacMahon's Theorem was the method it gave for investigating the dependences between seminvariants. It reduced (at least it provided a potential reduction) the calculatory part of the theory of a binary quantic to a problem in arithmetic. The chain of reasoning justifying this reduction of a large part of Invariant Theory to a problem of arithmetic is straight forward: The covariants were in one to one correspondence with seminvariants which were shown by MacMahon's Theorem to correspond to non-unitary symmetric functions. The tabulation of symmetric functions is equivalent to the arithmetical problem of finding the partitions of an integer.

In character, MacMahon's discovery was similar to Cayley's observation on groups of a few years earlier in which the study of abstract groups was potentially reduced to the study of permutation groups.

MacMahon's result was especially interesting to Cayley. From the failure of the Fundamental Postulate both Cayley and Sylvester were deprived of their generating function approach for finding the covariants and dependences in the higher order forms. Additionally, Cayley was thoroughly familiar with symmetric functions (his [1857a] was a large work in the computation of Symmetric Function Tables) and this subject and the Theory of Partitions were never far removed from his calculatory approach to the Theory of Invariants.[18]

Cayley followed MacMahon's paper with a series of four papers published in the American Journal of Mathematics. The first paper [1885b] was a long paper on the theory of seminvariant as it related to symmetric functions; other papers [1885c, 1885d, 1885e] consisted of tables connecting seminvariants with symmetric functions.[19]



9th August 1883

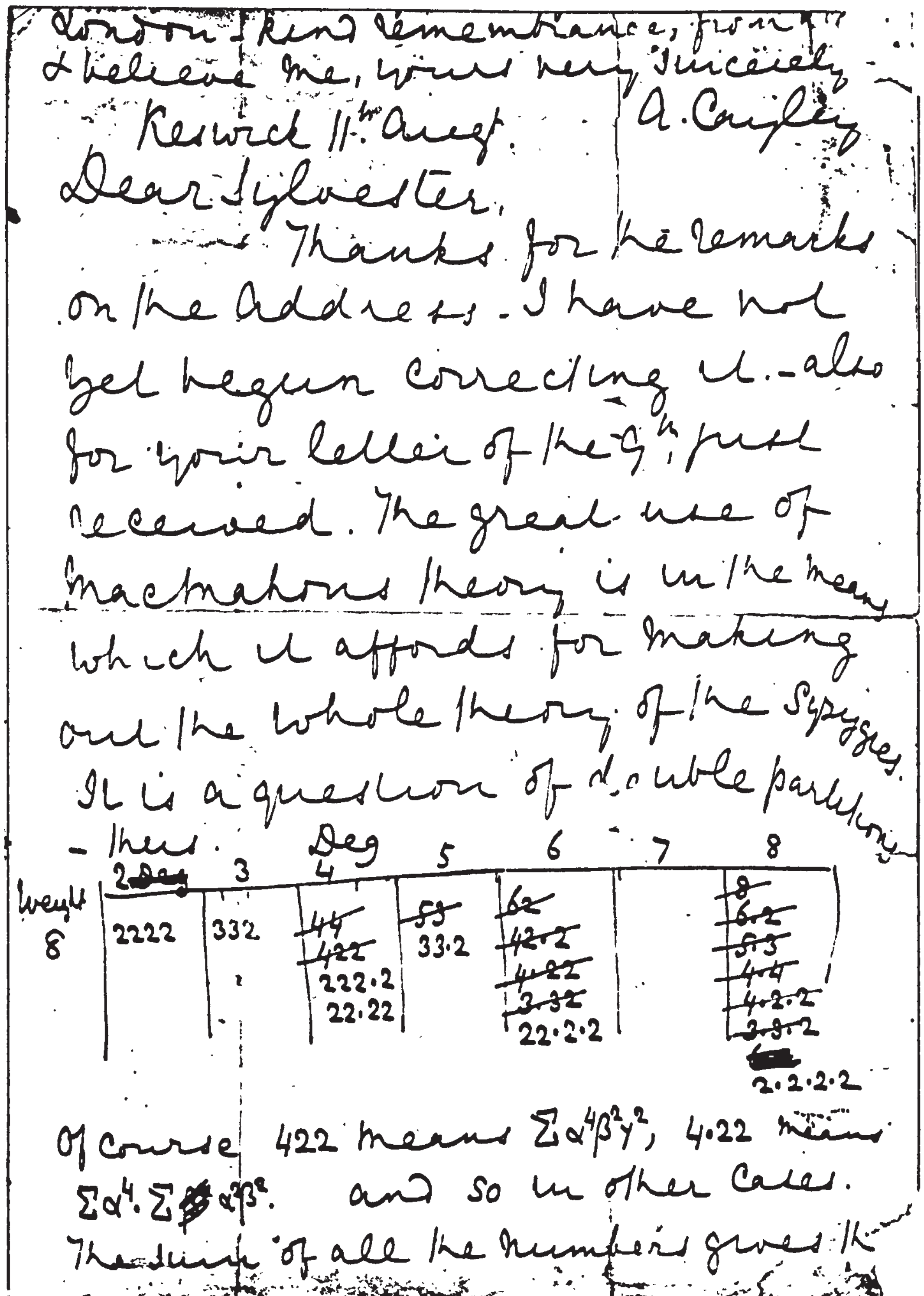
Dear Cayley, — Have you not somewhat over-estimated the importance of Mac-Mahon's discovery or theorem? If I am mistaken in my view of what it amounts to please inform me — This is how I take it (mind I only know the theorem by your account of it) If we take the common subinvariants of highest subject to annihilation by the operator

$$a\delta_b + 2b\delta_c + 3c\delta_d + \text{---}$$

every one of these is a Rational Function

Plate 4: Letter [first page] from Sylvester to Cayley regarding MacMahon's discovery in the theory of invariants.

[App. B, 9 viii 1883] . Original held at St. John's College, Cambridge [Sylvester Papers] .



London, kind remembrances, from
& believe me, yours very sincerely
A. Cayley

Keswick 11th Aug.

Dear Sylvester,

Thanks for the remarks on the Address. I have not yet begun correcting it. – also for your letter of the 9th, just received. The great use of MacMahon's theory is in the means which it affords for making out the whole theory of the Syzygies. It is a question of double partitions – thus.

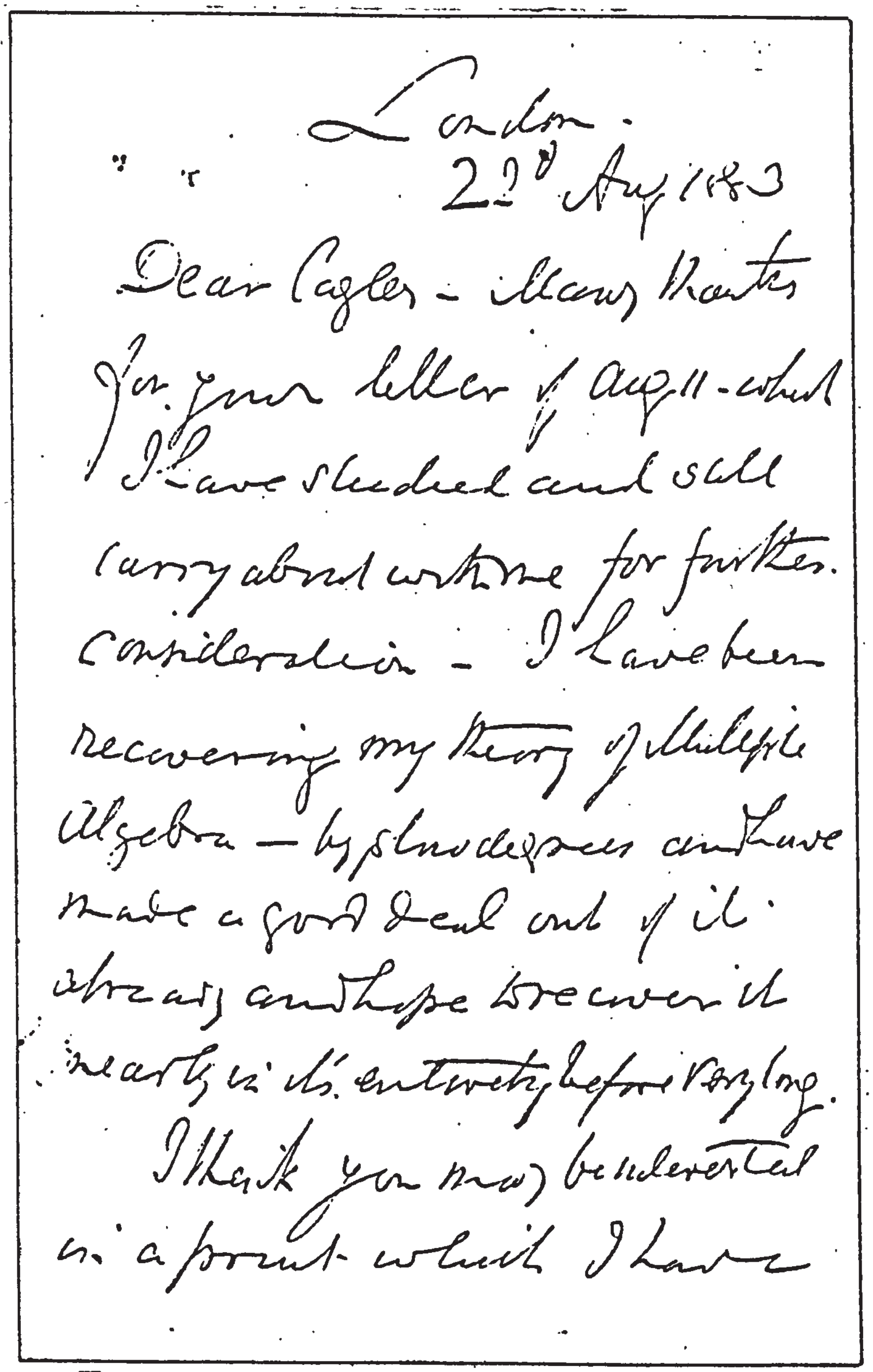
	Deg 2	3	4	5	6	7	8
Weight 8	2222	332	~~44~~	~~53~~	~~62~~		~~8~~
			~~422~~	33·2	~~42·2~~		~~6·2~~
			222·2		~~4·22~~		~~5·3~~
			22·22		~~3·32~~		~~4·4~~
					22·2·2		~~4·2·2~~
							~~3·3·2~~
							2·2·2·2

Of course 422 means $\Sigma\alpha^4\beta^2\gamma^2$, 4·22 means $\Sigma\alpha^4.\Sigma\alpha^2\beta^2$. and so in other cases.

The sum of all the numbers gives th

Plate 5: Letter [first page] from Cayley to Sylvester in reply to letter [displayed as Plate 4] explaining the importance of MacMahon's discovery.

[App.B, 11 viii 1883] . Original held at St.John's College, Cambridge [Sylvester Papers] .


London.
22d Aug 1883

Dear Cayley - Many thanks for your letter of Aug 11 - which I have studied and still carry about with me for further consideration - I have been recovering my theory of Multiple Algebra - by slow degrees and have made a good deal out of it already and hope to recover it nearly in its entirety before very long.

I think you may be inadvertent in a point which I have

Plate 6: Letter [first page] from Sylvester to Cayley in reply to letter [displayed as Plate 5] and on Multiple Algebra. [App.B, 22 viii 1883] . Original held at St.John's College, Cambridge [Sylvester Papers] .

According to MacMahon's resumé [MacMahon,1896a, 5] of Cayley's work at this transitional stage, the reduction of Invariant Theory to a problem of arithmetic became a reality through this paper on seminvariants [1885b] . Although MacMahon's result had relevance for finite binary quantics, Cayley's interest in this paper was confined to the binary quantic of infinite order:

$$a + \frac{b}{1!}y + \frac{c}{2!}y^2 + \frac{d}{3!}y^3 + \ldots$$

No doubt MacMahon had this work on seminvariants in mind when he remarked that about the year 1885, Cayley was involved with a vast amount of purely numerical work [MacMahon, 1896a, 7].

In his approach to the binary quantic of infinite order, Cayley needed a theory for the multiplication of symmetric functions (as a product of symmetric functions corresponded to a seminvariant.) It was the first step in the construction of an algebra of symmetric functions. Cayley's method was direct and the multiplication rule was obtained by observation in the simple cases [1885b, CP12, 240] :

$$\Sigma\alpha^3\,\Sigma\beta^2 = \Sigma\alpha^5 + \Sigma\alpha^3\beta^2$$
$$3.2 = 5 + 32$$

and the multiplication rule duly given was

$$l.m = (l+m) + lm$$

in the case for which $l \neq m$. In the case $l = m$, the corresponding pattern was:

$$\Sigma\alpha^2\,\Sigma\beta^2 = \Sigma\alpha^4 + 2\,\Sigma\alpha^2\beta^2$$
$$2.2 = 4 + {}_2.22$$

with the multiplication rule:

$$l.l = 2l + {}_2.ll$$

The idea was to obtain a formula for the multiplication of $(p_1p_2 \ldots p_s)$ and $(q_1q_2 \ldots q_t)$ but the actual formula obtained in the very simple cases proved to be very complicated. Cayley gave results for the case $(3^a2^b)(3^c2^d)$.

Commenting on Cayley's 'ingenious algorithm', MacMahon remarked:

> It gave the requisite facility in dealing with combinations of forms represented in the notation of partitions. The great advance thus made will be apparent when it is stated that it became comparatively easy to deal with forms of as high a weight as forty or fifty and to assign the syzygies.
>
> [MacMahon, 1896a, 5]

Symmetric Functions

When Cayley had published his tables of symmetric functions in [1857a] his intention was to facilitate the computation of the resultant of two polynomials.[20] These tables applied to the polynomial of indefinite degree:[21]

$$(1, b, c, \ldots \between 1, x)^{\infty} = (1-\alpha x)(1-\beta x)(1-\gamma x)\ldots$$

They gave the symmetric functions both in terms of the coefficients of the polynomial and inversely the coefficients of the polynomial in terms of the symmetric functions.

For the cubic

$$1 + bx + cx^2 + dx^3$$

the tables took the following form:

=	d	bc	b^3
[3]	-3	3	-1
[2,1]	3	-1	
[1^3]	-1		

This is the basic table and is read along the rows.

For example

$$\Sigma\alpha^3 = -3d + 3bc - b^3$$

or simply

$$[3] = -3(3) + 3(12) - (1^3)$$

The 'inverse' table for the cubic took the form

$\|$	d	bc	b^3
[3]			-1
[2 1]		-1	-3
[1^3]	-1	-3	-6

It is read down the columns. For instance

$$b^3 = -(\Sigma\alpha^3) - 3(\Sigma\alpha^2\beta) - 6(\Sigma\alpha\beta\gamma)$$

or simply

$$(1^3) = -[3] \; - 3[2\,1] \; - 6[1^3]$$

Cayley's tables written in 1857 are complete for polynomials up to and including degree ten. 22

On MacMahon's discovery, Cayley reproduced these tables in the case where the equation was written

$$1 + \frac{b}{1!}x + \frac{c}{2!}x^2 + \frac{d}{3!}x^3 + \cdots = (1-\alpha x)(1-\beta x)(1-\gamma x)\ldots$$

where the coefficients (involving factorial denominators) were substituted for the plain coefficients of the original polynomial. In the case of the cubic polynomial

$$1 + bx + \frac{c}{2!}x^2 + \frac{d}{3!}x^3.$$

[1885c; CP12, 263]

the table was

$=$	d	bc	b^3
[3]	-3	9	-6
[21]	3	-3	
[1^3]	-1		

All coefficients were to be divided by 6. The table showed, e.g.

$$[3] = \frac{1}{6}(-3d + 9bc - 6b^3)$$

or otherwise the seminvariant $d - 3bc + 2b^3$.

Perpetuants

A perpetuant was the terminology used for an irreducible seminvariant of the binary form of infinite order. Sylvester chose this name because perpetuants 'appeared and reappeared' as the leading terms of irreducible covariants of binary quantics of finite order. In one sense the discovery of perpetuants and their dependences was less difficult than the corresponding problem for finite binary quantics. According to [MacMahon, 1910a, 638] many of the technical problems associated with nth order binary quantics disappeared when the binary form of infinite order was considered. There was only one perpetuant of degree 1 (namely the seminvariant a) but of degree 2 there were an infinite number of perpetuants:

$$ac - b^2 \qquad \text{weight 2}$$

$$ae - 4bd + 3c^2 \qquad \text{weight 4}$$

$$ag - 6bf + 15ce - 10d^2 \qquad \text{weight 6}$$

$$\cdots\cdots\cdots\cdots$$

It was found that as the degree of a seminvariant increased there was greater chance that it could be reduced. Quite early in the work Cayley thought that all seminvariants of degree six were reducible; that is, there were no sextic perpetuants.[23]

Cayley's conjecture, if true, would have meant that all seminvariants for the infinite order binary quantic could have been expressible in terms of perpetuants of the first five degrees. Because of the intimate relationship between perpetuants and seminvariants of finite order quantics the correctness of Cayley's conjecture could have led to a proof of Gordan's Theorem.[24] At least we are told by MacMahon, Cayley's work in this direction 'led him to desire a purely algebraic proof of Gordan's theorem concerning the finality of the covariants of quantics of finite order' [MacMahon, 1896a, 6].

Cayley's and Sylvester's general method of treating algebraic questions was repeated for perpetuants.

This was their ' gradual progress along the scale' where in this case the scale meant n, the degree of a perpetuant. Cayley and Sylvester both produced generating functions (the number of perpetuants of weight ω and degree θ is the coefficient of x^{ω}) of the generating function:

$$\frac{x^{2^{\theta-1}-1}}{(1-x^2)(1-x^3)\ldots(1-x^{\theta})}$$

gradually in the special cases θ = 3, 4, 5. But although the dependences of degree 5 had been found, the sextic perpetuants $(\theta=6)$ and their dependences proved a stumbling block. Cayley could only surmise the result for θ = 6. This was the case in which he made his earlier conjecture and is likely the difficulty of the sextic perpetuants to which he referred in a letter to Dr. Craig of Johns Hopkins University:

> I am quite stopped by a question in Seminvariants partially solved by MacMahon & to which I have no doubt he refers in the paper[25] of his which you have - but we are neither of us at present able to make the next step: if I succeed in doing so, I should be rather inclined to undertake a treatise on the subject; but I do not at all see my way.
>
> [App.C, Craig, 12 viii 1885]

Sylvester was too immersed in matrix algebra to take part in Cayley's new investigations:

> Would that I could do anything to assist you you in your most interesting investigation concerning sextic Perpetuants: Unhappily I am out of that dream and out of the partition dream and have no present thought except for Multiple Quantity.
>
> [App.B, 3 ii 1884]

The theory of the binary quantic of infinite order owed a great deal to Sylvester's inception of the problem in his [1882a]. But his claims in a letter to Cayley regarding the propriety of the Correspondence Theorem were excessive. According to

Sylvester, the study of perpetuants was partly a result of Hammond's disproof of the Fundamental Postulate:

> What wonderfully beautiful work you & MacMahon appear to have been doing on Perpetuants! I must try and get back on the track. We owe it all in origin to Hammond and my Prize Advertisement. MacMahon will afford to forgo the credit of "discovering" the correspondence theorem to which he has not the shadow of a claim. It is Brioschis + my remark.
>
> [App.B, 2 iii 1884]

Cayley carried out much of his work on seminvariants in the mid 1880s in conjunction with MacMahon, as their Notes testify [App.C,MacMahon]. Contrary to Cayley's belief that sextic perpetuants were non-existent, MacMahon showed that a sextic perpetuant did exist for weight equal to thirty-one. MacMahon later proved (as did the German mathematician E.Stroh) that perpetuants exist for each degree but the lowest weight of a perpetuant of degree θ (>2) was $2^{\theta-1}-1$.[26]

Cayley's Law and Gordan's Theorem

Both these theorems received attention from Cayley and Sylvester during the 1880s. Sylvester had given his earlier proof of Cayley's Law (Chapter 3, page 142) but in his lectures on Reciprocants,[27] he provided yet another proof [1886a, SP4, 363] of this theorem. Cayley appears to have taken little interest in the proof of Cayley's Law. But one version of Sylvester's earlier proof [1878d; SP3, 229] attracted David Hilbert's attention and he provided his own proof in his [1887a, 20]. Hilbert's proof made use of differential operators similar to Cayley's differential operators.[28]

Unaware of Hilbert's proof, E.B.Elliott published a proof of Cayley's Law along similar lines in [Elliott, 1892a] [30]
This theorem, on which rested a large part of the calculatory theory of Invariant Theory, was vindicated by the series of different proofs which appeared about this time.

The other outstanding theorem in the theory of binary forms was Gordan's Theorem. Both Cayley and Sylvester still desired a non-symbolic proof of Gordan's Theorem. Through his work on perpetuants, Cayley believed a simple proof to exist. Sylvester, too, sought the elusive 'natural method' of proof [SP3, 572]. They both worked almost entirely with the Cartesian expression of the forms and to them the symbolic method was artificial. The symbolic method of expression was also difficult to understand. In his own way, Sylvester made this point (in a letter to the scientist, John Tyndall):

> His [Gordan's] own demonstration is so long and complicated and so artificial a structure that it requires a very long study to master and probably there is not one person in Great Britain who has mastered it. 31

What was hoped for was not a proof using the complex and abstract machinery as had been developed by the German mathematicians, but a proof which owed its power to directness and ingenuity. Their common attack in the attempt to establish Gordan's Theorem was to construct a basis for the covariants of a binary quantic. In this, they attempted to construct the basis for the seminvariants but they met their perennial problem: constructing the basis for the binary cubic and binary quartic was quite straight forward but the binary quintic was again found to be of a different order of difficulty.

In the case of the binary cubic, seminvariant basis consisted of

$$a$$

$$ac-b^2$$

$$a^2d-3abc+2b^3$$

$$a^2d^2+4ac^3-6abcd+4b^3d-3b^2c^2$$

And every seminvariant of the binary cubic was expressible as a rational and integral function of these [Cayley, 1871e; CP8, 566]

A more refined idea of a basis was introduced by Cayley. They were called by Sylvester, protomorphs [Sylvester, 1882a; SP3, 579] For a binary quantic of order n there existed n protomorphs but it was not possible to express every seminvariant as a rational and integral function (division by a factor a may be necessary) of these protomorphs or (base forms). For the binary cubic there were thus 3 protomorphs $a, \; ac-b^2, \; a^2d-3abc+2b^3$ Any seminvariant could be expressed as a combination of these, division by a being permissible. For instance, with the seminvariant ∇ :

$$\nabla = \frac{4}{a^2}(ac-b^2)^3 + \frac{1}{a^2}(a^2d-3abc+2b^2)^2$$

The idea was that the system of protomorphs could be used to obtain the basis for the seminvariants. However, as mentioned above, this process was found to be of formidable complexity in the case of the binary quintic [Sylvester, 1882a, 580] .

Sylvester had to admit (to Cayley) that his attempts to find a constructive proof were unsuccessful:

> My supposed proof of Gordan's theorem was a Delusion- but I have considerable hopes of being able to found one upon the method of Deduction aided by the actual application of this method [of protomorphs] to the Quintic (as a Diagram)
>
> [App. B, 6 x 1882]

However, both of them were persistent in their struggle to provide a proof. Four years later Sylvester was at Oxford and there he he attempted to turn repeated failure into success. He wrote to his friend:

> In my off moments I have been thinking again of Gordan's theorem and verily believe that I have found the proof (...) Hammond is settled here and we meet for several hours daily. He will check me if I am under any delusions as to the Gordanic business. [App.B,1 ii 1886]

and two weeks later the cheerless note:

> I nourish the undying hope that through the Protomorphs we shall be able to prove the finitude of the ground-forms of Invariants and Reciprocants by some simple process of reasoning. [App.B, 18 ii 1886]

MacMahon thought the Theorem would eventually be proved using symmetric functions:

> It is to be hoped that some of these facts [about symmetric functions] may help to forward the algebraical (as distinct from the symbolical) treatment of the theory of invariants; as yet, however, a purely algebraical demonstration of Gordan's great theorem concerning the finality of the ground covariants seems as far distant as ever.[32]

As is well known, Hilbert provided a proof for any quantic of any number of variables, not by constructing a finite seminvariant basis, but by an existence argument.

Cayley was sent a copy of Hilbert's proof after it had appeared in print in December 1888. From this he was stimulated to supply a proof for the binary case using his own methods. In a letter to Felix Klein he wrote:

> I have read with great interest Hilbert's paper in the Gott. Nachr.-which however I do not understand- and that in the last No. of the Math.Ann. It seems to me that if instead of applying this to the invariants of the binary function, we apply it to the covariants, or what is the same thing the seminvariants, we have a very simple & beautiful proof of the finite number of the covariants- and I have written this out and send it to you herewith.
>
> [App.C, Klein, 24 i 1889]

A week later, Cayley wrote to Hilbert admitting that he was unable to merge his theory of seminvariants with Hilbert's abstract method and so provide a constructive proof:

> My difficulty was an a priori one, I thought that the like process should be applicable to semi-invariants, which it seems it is not; and now I quite see (...) I think you have found the solution of a great problem.
>
> [Reid, 1970a, 33; letter dated 30 i 1889]

However, these doubts seem to have disappeared by the time Klein informed Cayley of an error in his reasoning but Cayley insisted that the proof should be published:

> Thanks very much for your letter: I cannot see that there is any doubt as to the proof which I sent you - it depends only on the leading coefficient of a covariant [seminvariant] being a function of the differences of the roots and seems to be perfectly general. I shall be very much obliged if you will publish it - and of course any objections to it can afterwards be published.
>
> [App.C, Klein, 22 ii 1889]

Following Cayley's direction, Klein published the paper [1889c] but the proof was found to be erroneous.[33]

4.5. Last Years

In one of his last contributions to the Invariant Theory Cayley investigated seminvariants with a view to establishing criteria for the reducibility for covariants. One argument which enabled Cayley to establish whether a covariant expression was reducible was by Cayley's Law (Chapter 2, page 85) but in [1893c] Cayley attempted to establish a criterion based on the seminvariants. Reporting on this work, MacMahon [1896a, 7] noted that the subject 'bristled with difficulties and exceptional cases' and concluded that Cayley's work in this direction was only partially successful.

The direct approach adopted by Cayley was pitted against a problem of great complexity made even harder by the lack of sophisticated techniques.

Looking back over the whole field of Invariant Theory the actual computed results must have appeared meagre to Cayley. Through his work in the <u>Tenth Memoir</u> on Quantics [1878a] and Hammond's work in the mid 1880s (Chapter 3, page 145) the complete system and the dependences of the quintic had been found. Thus the work done on the binary quintic had been successful. But what could be said about the binary sextic? Although a great deal was known in this case, the list of fundamental covariants was never completed by Cayley. Even for a mathematician of his computational skill the calculations would have appeared daunting. In [1881g] he listed 18 of the 26 seminvariants of the sextic and showed how the remaining seminvariants might be calculated. Although the complete list of covariants for the binary sextic had been given by Gordan in his revolutionary [1868a], Cayley still endeavoured to find the explicit forms. Cayley gave a partial list of 17 (out of 26) of the less lengthy covariants and published them [1894b]. While it was known how to calculate the remaining covariants it is understandable that they were left uncalculated when their sheer length is considered. The covariants X, Y and the invariant Z, for example, (all left uncalculated) consisted of 1002, 2012 and 1636 terms respectively. In [1894b][34] they were written:

$$X = (332, 338, 332)^{10}\ 29 \text{ to } 31\ (x,y)^2$$

$$Y = (668, 676, 668)^{12}\ 35 \text{ to } 37\ (x,y)^2$$

$$Z = (1636)^{15}\ 45\ (x,y)^0$$ Invariant (Calculated by Salmon).

Describing Cayley's work in the last few years of his life, MacMahon observed that 'he worked largely on his own initiative, although well acquainted with contemporary work on the Continent and in the United States of America' [1896a, 7].

In spite of poor health Cayley remained committed to his originally stated objective of [1846b] 'to find all the derivatives of any number of functions, which have the property of preserving their form unaltered after any linear transformations of the variables.'

Chapter 4

References

1. Cayley's lectures were mainly on Abelian functions according to a letter written by Sylvester [App. B, 6 ix 1882]. Cayley compiled a substantial memoir on this subject in two parts [1882c, 1885a]. A mathematical difficulty caused the break in the published memoir according to Cayley [App.B, 6 ix [1882]]; later Sylvester was able to write: 'Congratulations on the final success with your great labour which at one time you had abandoned in despair' [App.B, 12 vii 1884] Cayley's work on this subject may have prevented him from taking a greater interest in Sylvester's matrix theory.

2. Theorie der Abelschen Functionen (1866).

3. These double algebras do not include the ordinary complex numbers (1, i are not linearly independent over the complex numbers and consequently cannot be taken as x and y).

4. According to Benjamin Peirce, a double algebra was 'pure' if each of the symbols x and y were connected by an 'indissoluble relation' with the other symbol. If x and y of a double algebra could be separated into two mutually independent ($xy = yx = 0$) groups, the algebra was said to be a 'mixed' algebra. Peirce classified 'pure' algebras in his [1881a].

5. Later the seven non-equivalent double algebras were derived as a consequence of a general theory of linear associative algebras which involved invariants [Hazlett, 1914a, 6].

6. After the death of H.J.S.Smith in 1883, Sylvester was appointed as his successor at Oxford. Cayley was offered the vacant chair at Baltimore, but he reflected that although he was impressed by the University during his stay there in 1882: 'I am quite satisfied here Cambridge and as well for myself and my wife as on account of the children, cannot bring myself to the idea of abandoning England' [letter to D.C.Gilman, 23 ii [1883 [sic]] held at Johns Hopkins University, Baltimore, U.S.A.

7. He also gave an explicit solution for px=xq. If the latent roots of n x n matrices p were $\lambda_1, \ldots, \lambda_n$ and q were $\mu_1, \ldots, \mu_n$ and if the first i roots were identical, $x = UV$ would be a solution of px=xq where

$$U = (p-\lambda_{i+1}) \ldots (p-\lambda_n)$$
$$V = (q-\mu_{i+1}) \ldots (q-\mu_n)$$

[1884b, SP4, 177] .

The solution is inadequate for several reasons. Sylvester implicitly assumed the existence of distinct latent roots (Hawkins' 'generic reasoning') and his solution failed to give all solutions (when $\lambda_i = \mu_i \; i=1,\ldots,\omega$ for instance) and he was aware of this.

8. Cayley's formal solution is not watertight and does not give all the solutions. For instance in the case

$q = \lambda i$, $q' = -1$ a solution Q is $(1-\lambda i)$
In this case $\theta=1, \alpha=1, \beta=1$ and the equation $\theta^4 - 2\alpha\theta^2 + \beta^2 = 0$ is satisfied. The fact that $M=0, A=0, B=2$ means that Cayley's formal solution yields only $Q = 0$.
The formal nature of Cayley's work has been discussed by Hawkins in [1977a] .

9. Sylvester and Cayley most likely heard of Frobenius' work on matrices through a letter (sent to Sylvester 25 xii 1884 Sylvester Papers, St. John's College, Cambridge) from Buchheim. Cayley referred to Frobenins (1886) only in connection with work on differential equations [CP12, 394] . The English mathematician, Arthur Buchheim (1859-1888), an Oxford graduate, studied under Klein at Leipzig and sought to bring matrices to a wider audience by reconciling the ideas of Grassmann, Hamilton and Cayley [1885a] .

10. Cayley referred to Sylvester's earlier matrix papers in two letters [App B, 8 xi 87] and to Sylvester's Nivellator theory [App B, 16 xi 1887] [App B, 19 xi 1887] Cayley's further papers on Matrix Theory were [1887a, 1891a, 1891c, 1895a].

11. A continuation of [1887a] was indicated but none appears to be extant. Cayley's lectures (Michaelmas Term, 1887) were titled 'Quaternions and other non-commutative Algebras' [CP8,xlv-xlvi] [App B, 7 vi 84] But Sylvester gave his Inaugural Lecture on Reciprocants.

12. James Hammond (1850-1930) was the eldest son of nine children. He was educated at King's College, London and graduated from Cambridge in 1874. He had a facility for patient and accurate calculation and when Sylvester returned to Oxford in 1884, Hammond became his secretary. Hammond suffered from a gradually worsening paralysis but lived a long life [Elliott , 1931a, 78] .

13. See [Sylvester 1879b; SP3, 287] . Cayley's intuition was quite right on this occasion. The invariant of degree 20 was shown to exist by Hammond in 1890.

14. There were several reasons for this belief. It was the only low order binary form for which he and Franklin had not found a finite representative generating function [Sylvester, 1879b; SP3, 287] . In addition Sylvester had successfully shown the non-existence of a covariant by way of an implication drawn from the Postulate [Sylvester, 1881b] . Defending his method several years later, he wrote to Cayley: 'Has it ever occurred to you to consider why my method [of Tamisage] in spite of a possible error in the result does as a matter of fact give all and not only some of the seminvariants in all the cases to which it has been applied. viz. 5cs, 6cs, 8cs as shown by comparison with Clebsch and Gordans and as regards the 8ic by Von Gall's calculations.' [App.B, 11 v 1885] .

15. Percy Alexander MacMahon (1854-1929) was born in Malta into a military family and he himself became an officer in the British Army. According to [Baker, 1930a] he gained prominence in the mathematical world by his discovery of the Correspondence Theorem and his work on differential operators. Much of his work in combinatorial theory is close to the theory of symmetric functions.
MacMahon's Collected Papers are at present (1981) being edited by Professor George E. Andrews (M.I.T. Press) and Vol.1 (Combinatorics) has been published.

16. $\sum \alpha^p \beta^q \gamma^r$ ... is a non-unitary symmetric function in the case where no one of p, q, r, ... is unity.
MacMahon's Theorem strictly applies only for binary quantic of a 'high enough degree'; a non-unitary symmetric function for the nth order binary quantic is not necessarily a seminvariant.
In the case of the cubic

$$x^3 + bx^2 + \frac{c}{2}x + \frac{d}{6}$$

$$\sum \alpha^2\beta^2 = \frac{1}{12}(3c^2 - 4bd)$$

but $a\partial_b + 2b\partial_c + 3c\partial_d(3c^2-4bd) \neq 0$ and consequently $3c^2 - 4bd$ is not a seminvariant. In the case of $n \geqslant 4$ however the symmetric function $\sum \alpha^2\beta^2$ is a seminvariant.

On the contrary each seminvariant of the binary form of infinite order (a function annihilated by $a\partial_b + 2b\partial_c + \cdots$ ad infinitum) is a seminvariant of some nth order quantic and quantics of order $\geqslant n$ (the order n is determined by the highest letter (extent) appearing in the expression for the seminvariant).

17. Sylvester himself undoubtedly had a strong claim to part of the credit. He had published his [1882a] on seminvariants to binary quantics of unlimited order prior to the publication of MacMahon's [1884a] and felt inclined to dub it the MacMahon-Sylvester Correspondence Theorem [App. B, 28 iii 1884]. An important point arising out of Sylvester's [1882a] was the link between the failure of the Fundamental Postulate and the introduction of binary forms of unlimited order. Sylvester noted: 'Such a case (amounting to the Fundamental Postulate being false) does not present itself for quantics of the lower orders; it seems natural and logical therefore to seek for it in the case of a quantic of an infinite order' [1882a; SP3, 575].
See also [App. B, 30 iii 1884] for Sylvester's further comments on MacMahon's theorem.

18. Cayley gave tables for the unrestricted (no restriction on number of parts) partitions of an integer n for n = 1 to n = 18 [1881a]. These partition tables are unusual in that they gave actual partitions whereas later tables gave only the number of partitions of a given integer [Fletcher, 1962a]. But this was, of course, possible because he dealt with such small values of n. Like so many of the other algebraic subjects which attracted his attention, Cayley was a pioneer and his interest was focused on classification for small values of n.

19. Cayley's [1885e] computed the actual linearly independent seminvariants of lowest degree for weights up to and including 12. For weight 4, for example, Cayley found the linearly independent seminvariants $ae - 4bd + 3c^2$ and $a^2c^2 - 2ab^2c + b^4$. For Weight 12, there were 32 linearly independent (and lengthy) seminvariants [CP12, 284-285].

20. Cayley's [1857a] made many corrections in Meyer-Hirsch tables (1808). Cayley's tables have been recalculated many times. See [Fletcher, 1962a]. Euler and Cramer had used the symmetric functions for this purpose in the course of calculating the number of intersections of curves [Decker, 1910a, 4].

21. Cayley's notation

$$(1, b, c, \ldots \between 1, x)^{\infty} = 1 + bx + cx^2 + dx^3 + \cdots$$
$$= (1-\alpha x)(1-\beta x)(1-\gamma x)\cdots$$

is equivalent to the more familiar form

$$y^n + by^{n-1} + cy^{n-2} + \cdots$$
$$= (y-\alpha)(y-\beta)(y-\gamma)\cdots$$

through the transformation $y = \frac{1}{x}$. Cayley's arrow notation ($\between$) meant the polynomial written <u>without</u> binomial coefficients.

22. [Cayley, 1857a ; CP2, 423] Cayley noticed (as did E.Betti) the symmetry between the rows and columns in both the partition tables (taken separately) [Decker, 1910a].

Cayley-Betti Law: Coefficient of [P] in (Q) = Coefficient of [Q] in (P).

23. Cayley's paper titled: 'Sextic Perpetuants of any weight w, proof that number of =0', Johns Hopkins University Circulars, 3 (1884), 13. (Not listed in CP).

24. An irreducible seminvariant for a nth order binary quantic was not necessarily a perpetuant. For example ∇ is irreducible for the binary cubic though it was found to factorise when considered in terms of the quantic of infinite order:

$$\nabla = (ac-b^2)(ae-4bd+3c^2)-a(ace+2bd-ad^2-b^2e-c^3)$$

25. This was most likely MacMahon's 'On Perpetuants' American Journal Math., 7 (1885), 26-46. MacMahon computed syzygies of the infinite binary quantic. He showed that the simplest perpetuant was of weight 31 and correctly conjectured the form of the generating function for the perpetuants of degree 6.

26. MacMahon gave the complete system of perpetuants for the binary form of infinite order. Though infinite, the members of the system were identified by MacMahon [Grace and Young, 1941a, 326]. According to [MacMahon, 1910a, 638] the 'true method of procedure' for the study of perpetuants was due to Stroh who developed the theory using the symbolic method.

27. When Sylvester abandoned matrix algebra in 1885 he was attracted by the promise of a new 'invariant theory' (involving functions in which derivatives occur). In a letter to Cayley he wrote:

> Am very glad you take an interest in my new functions provisionally we may call them Reciprocants(...) You will see that the whole of the game so to say of invariants has to be played out over again on a new field and subject to new laws but giving rise to a parallel theory of groundforms of perpetuants and syzygies and revolving on the same order of ideas. [App.B, 24 x 1885]

Cayley took an interest in Reciprocants and wrote a survey article [1893a]. He did not include Lie's work in this article and it was only later realised by English mathematicians that the work on Reciprocants was subsumed under Lie's theory of continuous transformation groups [Elliott, 1898a].

28. E. Stroh published a proof of Cayley's Law using the symbolic method [1888a] .

29. Edwin Bailey Elliott (1851-1937) was an Oxford mathematician. He was born in Oxford and graduated there in 1873. His name became known to English mathematicians through his highly successful Algebra of Quantics [1964a] first published in 1895. This book summarised the achievements of the English school of Invariant Theorists and was written in the non-symbolic method.(It has proved invaluable in the preparation of this thesis) . Elliott carried the algebraic tradition of Cayley and Sylvester into the twentieth century [Turnbull, 1938a, 425].

30. Elliott effectively (but not in the same language) showed that the range of Cayley's operator Ω $(= a\partial_b + 2b\partial_a + \cdots)$ regarded as a transformation of a $P(0,\ldots,n)^{\theta}q$ dimensional space to a $P(0,\ldots,n)^{\theta}(q-1)$ dimensional space, was an onto transformation [Elliott, 1892a, 304] .

31. Letter dated 14 ix 1882 [Tyndall Correspondence, 4,p.1519, Royal Institution of Great Britain] . This comment of Sylvester's should not be taken too literally for he himself thought he had understood Gordan's Theorem several years earlier. However, the comment indicates the general difficulty the English mathematicians had in understanding Gordan's symbolic approach.

32. From P.A.MacMahon 'Memoir on a new theory of symmetric functions', American Journal Math., 11 (1889) 1 - 36 (Submitted 9 v 1888).

33. Cayley attempted to prove the theorem in terms of the covariants arguing that Hilbert's proof process would be very much simplified if it were applied directly to covariants or equivalently to seminvariants. Hilbert was aware that Cayley's proof was erroneous and the error was also pointed out by the Danish mathematician, Julius Petersen [1890a, 112] . According to Petersen,Cayley's method did not take into account seminvariants in which the degree did not equal the weight of the seminvariant.(e.g. the seminvariant $ae - 4bd + 3c^2$ is of degree 2 and weight 4). It was in dealing with just these seminvariants that the real difficulties occurred according to Petersen.

34. This notation for a covariant in, for example,

$$X = (332, 338, 332)^{10}\ 29 \text{ to } 31\ (x,y)^2$$

meant that the covariant was of the form

$$(A, B, C)(x,y)^2$$

where A possessed 332 terms of degree 10, weight 29
B possessed 338 terms of degree 10, weight 30
C possessed 332 terms of degree 10, weight 31.

Chapter 5

A view of Cayley's mathematical thought

5.1. Introduction

One particular facet of Cayley's mathematics makes any view of his mathematical thinking necessarily incomplete. This was his deeply rooted reluctance to discuss the nature of mathematics in his writings. Apart from his Presidential Address, delivered to the British Association for the Advancement of Science in 1883, Cayley rarely wrote about mathematics. According to [Roberts S, 1882a] the 'severe' style of British mathematicians was deliberately cultivated. Cayley's preference for it is shown by a comment he made in some work of Halphen's: 'I do not think so much talk is wanted before coming to the question' [App.C, Hirst, 19 vi 1878] he remarked to Hirst. J.J.Sylvester paid no heed to this convention but his predilection for embroidering his work with a philosophical commentary was exceptional. Sylvester was conscious of Cayley's astringent style when he sought Cayley's opinion:

> I hope you will not be too severe in your judgement on this departure from conventional rules [in 1884a] . I know that you do not in general approve of any deviation from established usage in dealing with mathematical subjects.
>
> [App.B, 8 xi 1884]

The general lack of explanation and failure to express viewpoints and motivation makes Cayley's British Assocation Address [1883a] of particular importance.

The views offered by Cayley on foundational questions are not so much of interest in themselves as they were not particularly original. They are the fairly orthodox views on the foundations of mathematics by an important mathematician of the nineteenth century, and it is from this that their interest is derived. While the contents of his Address illustrate Cayley's philosophical insights, Cayley did not generally channel his abilities into foundational questions and mathematical philosophy. As Glaisher specifically remarked, Mathematical Philosophy was the only mathematical subject which did not claim Cayley's attention [Glaisher, 1895a, 174]. However, he took more than a passing interest in foundational questions and in spite of his great contribution to the technical development of mathematics, Cayley must not be regarded as an unreflecting mathematician.

5.2. The 1883 Address - Foundations

Cayley's Presidential Address to the British Association for the Advancement of Science was given at Southport in September 1883. Cayley accepted the invitation to be President of the British Association in 1882 while he was in America. In a letter to Hirst he sketched out some likely topics for the Address:

> I am astonished at my own audacity[in accepting the Presidency] Glaisher wrote to me that you and Adams were going to send me letters of exhortation & persuasions, but I have not yet received the one from Adams. I think I shall make the Address on pure Mathematics - including of course geometry - and the various directions - imaginaries and imaginary space, hyperspace, complex and ideal numbers, Abzählende Geometrie etc, in which the science has in modern times extended itself. I think it will be possible to be fairly interesting to a largish part of a non-Mathematical Audience.
>
> [App.C, Hirst, 31 iii 1882]

On his return to Cambridge Cayley resumed work preparing the first draft. He informed Sylvester of his progress: 'I hardly see yet what I shall be able to make of it, I shall have to go over [it] again, with a good deal of difference in the point of view, much that was given - very well indeed - by Spottiswoode in his [Presidential] Address at Dublin in 1878' [App.B, 6 ix [1882], year estimated]. And in turn Cayley received support and encouragement from Sylvester:

> I think that taken as a whole the Address is exceedingly good(...) , I do not think that the multitude will be greatly edified by it as spoken - but the contre-coup of the judgement following its perusal will I think make ample amends and tend to support the merit of the Association as a body seriously bent on the promotion of science.
>
> [App.B, 3 viii 1883]

That a mathematical Address would not appeal to an audience used to popular science and stories of technological success was also the fear of the reporter for the Times newspaper [Times, 1 ix 1883, 7]. Nevertheless, when Cayley rose to give his evening lecture the pavilion (with a seating capacity of over 2,000 people) was filled to capacity.[1] In discussing this period of

British history, the historian, E.H.Carr, speaks of 'the positive belief, the clear-eyed self-confidence, of the later Victorian age.'[2] And it was this optimism applied to the future of mathematics which Cayley radiated at Southport in 1883. Cayley was writing for posterity as for the meeting itself and in the choice of subject material, he made little concession to the Association audience: 'I think it is right that the Address of a President should be on his own subject (...) So much the worse, it may be, for a particular meeting; but the meeting is the individual, which evolution principles must be sacrificed for the development of the race' [Cayley, 1883a, 4]. Cayley surveyed the subject from a historical perspective and with reference to recent progress. A significant part of the Address was devoted to the foundations of Number, Algebra and Geometry.

Time and Number

Cayley dissented from Hamilton's view that the idea of Number was based on instances of time. But like Hamilton, he was influenced by the philosopher, Immanuel Kant. In accordance with the Kantian view, Cayley held that time was not an empirical concept but was 'a necessary representation lying at the foundation of all intuitions' [1883a, 5] But Cayley rejected the basic notion of number as dependent on any concept of time. This he asserted in his Address but he had stated the view much earlier [1864a] :

> I do not admit the assertion, that the idea of number is derived from that of time, it appears to me that it is derived from that of succession in time or space indifferently. But I would rather say that the idea of cardinal number is derived and abstracted from that of ordinal number, viz. (distinguishing the expressions 'set' and 'series', the latter bring used to designate a set of things considered as arranged in a definite order).
>
> [1864a; CP5, 292] [3]

But where does the notion of ordinal number come from if not from time? Cayley explained that his basic starting point was a notion of 'plurality'.

At first glance, this seems to contradict Cayley's belief of ordinal number logically preceding cardinal number. But as he explained:

> We think of, say, the letters, a, b, c, &c., and thence in the case of a finite set - for instance a, b, c, d, e - we arrive at the notion of number; co-ordinating them one by one with any other set of things, or, suppose, with the words first, second, &c.,we find that the last of them goes with the word fifth, and we say that the number of things is five; the notion of cardinal number would thus appear to be derived from that of ordinal number.
>
> [Cayley, 1883a, 18]

In Cayley's way of thinking, logical precedence was obtained by subtracting properties (here the natural order relation) and thereby the conclusion that ordinal number preceded cardinal number. This is, of course, in distinction to the conventional modern (and Cantor's) view that cardinal number logically precedes ordinal number. The modern view assumes that an entity logically precedes another if the first entity has less structure than the second entity.

It is possible that Cayley arrived at these ideas in collaboration with Sylvester. In offering guidance to Cayley in his preparation for the Address, Sylvester reminded Cayley of past conversations:

> The observation of ordinal preceding cardinal number in logical conception you got I believe from me - but doubtless the parentage has escaped your recollection as it was a long time ago [1865?] when the subject was mentioned - the matter is of no importance but when I saw my baby nursed in your arms it was impossible to restrain a cry of natural affection and parental recognition.
>
> [App.B, 3 viii 1883]

Cayley, primarily an algebraist and geometer, did not seriously address himself directly to the problem of placing the real numbers on a firm foundation. He appeared content with the notion of real numbers being a 'continuous' extension of rational numbers. His lack of interest in the ultimate nature of real numbers was most likely a consequence of the English 'operational' approach to the differential calculus. This made little use of the limit concept and the derivative was considered as algebraic in nature. For Cayley, real numbers were simply magnitudes capable of continuous variation [1883a, 18].

And if Cayley rejected the idea that the natural numbers were based on time, still less did he appreciate how complex numbers could be placed on this foundation. He emphatically concluded: 'We do not have in Mathematics the notion of time until we bring it there.' [1883a, 19] .

Algebra

The emergence of symbolic algebra in the 1830s and 1840s removed the necessity for algebraic symbols to be interpreted. Cayley had clearly understood this when at twenty-three years of age he first encountered the quaternions (Chapter 1, page 40) . The quaternions were readily accepted by Cayley as valid even if they were not commutative. 'Why should they be' noted Cayley, stressing consistency as the important criterion which symbolic algebra should satisfy. The geometrical interpretation of quaternions found subsequently by Cayley was not an argument for justifying the existence of the quaternion entities. However, Cayley did not accept that symbolic multiplication was entirely arbitrary. This condition of consistency being understood, Cayley substituted 'utility' in place of the earlier 'interpretability.' Commenting on the quaternions in the course of a review of Hamilton's [1853a] Cayley wrote:

> Sir W.R. Hamilton's quaternion imaginaries are a set of symbols subject to laws of combination different from those of the ordinary algebraical symbols. Definitions in analysis as in any other science are not to be considered as arbitrary; they must satisfy the condition of utility as regards the science to which they belong, i.e. they must be such as to admit of being made the foundation of a system of dependent truths the development of which forms part and extends the limits of the science, and the interest of such resulting theory is a test of the value of the definition. The analytical theory of Quaternions is an eminently interesting and beautiful one, and the beauty is heightened by the singularity of the subject matter viz.symbols subject to laws of combination different from the laws with which mathematicians have hitherto been concerned. [Stokes, 1907a, 386] .

This is an echo of Peacock's own view of Algebra:

> Algebra may be considered, in its most general form, as the science which treats of the combinations of arbitrary signs and symbols by means of defined though arbitrary laws: for we may assume any laws for the combination and incorporation of such symbols, so long as our assumptions are independent, and therefore not inconsistent with each other: in order, however, that such a science may not be one of useless and barren speculations, we choose some subordinate science as a guide merely...
>
> [Peacock, 1830a, 71 his italics] .

But Cayley needed no subordinate science, for quaternions came to him as a ready made symbolic algebra.

A potential source for examining Cayley's algebraic viewpoint is his [1864a] 'On the Notion and Boundaries of Algebra'. The paper was an attempt to delineate the chief characteristics of algebra but the motivation for the paper being written was not explained.

Cayley excluded all infinite analysis and considered only the subject matter which had hitherto been described as finite Analysis. Similarly to De Morgan [1839a]., Cayley regarded Algebra as an Art and a Science. Algebra as an Art was judged to be the most important. This was concerned with operations which were either 'tactical' or 'logistical.' A 'tactical' operation described the strategy by which an algebraical result might be deduced while the 'logistical' operation referred to the ensuing manipulation. In modern language, Algebra as an Art referred to the art of using symbols and might loosely be described as calculatory or manipulative algebra. In Algebra as an Art there was no question of interpretation. Algebra as a Science (of lesser importance in Cayley's view) contained the predictive part of the subject:

> qua Science Algebra affirms a priori, or predicts, the result of any such tactical or logistical (or tactical and logistical) operations. [1864a, CP5, 293]

Algebra as a Science is concerned with the interpretation of symbolic results.

Geometry and Algebra

In the memoir on Abstract Geometry (1870) Cayley explained how an understanding of abstract n-dimensional geometry could be useful in dealing with algebraic problems:

> In fact whenever we are concerned with quantities connected together in any manner, and which are, or are considered as variable or determinable, then the nature of the relation between the quantities is frequently rendered more intelligible by regarding them (if only two or three in number) as the co-ordinates of a point in a plane or in space: for more than three quantities there is, from the greater complexity of the case, the greater need of such a representation; but this can only be obtained by means of the notion of a space of the proper dimensionality; and to use such representations, we require the geometry of such space.
>
> [1870; CP6, 456]

An instance of this approach was seen in Cayley's treatment of the matrix equation px = xq, where a four dimensional space was entailed (Chapter 4, page 167). If any branch of Cayley's mathematics was his speciality, it was Algebraic Geometry.[4] (See Appendix A for an indication of Cayley's path in Algebraic Geometry). The link between Geometry and Algebra was of course the 'method of co-ordinates.'

An indication of the extent that Cayley considered the method of co-ordinates to be the essential tool in geometry (as distinct from the synthetic method or the quaternionic method) is seen from his choice of 'Descartes and the invention of co-ordinates' as subject for his Inaugural lecture as Sadleirian Professor at Cambridge in 1863. And in his 1883 Address he briefly commented that 'Descartes' method of co-ordinates is a possession for ever.' [1883a, 37].

For Cayley, a Cartesian equation representing a curve or surface had the advantage of being able to convey its meaning immediately. This was in distinction to an equation expressed in a more compact form. This fact was the point of Cayley's well-known 'pocket map' allegory with regard to equations expressed in the quaternionic notation: 'I compare a quaternion formula to a pocket-map - a capital thing to put in one's pocket, but which for use must be unfolded: the formula, to be understood, must be translated into co-ordinates.' [1895b, 272].

Cayley's affinity for the Cartesian method and notation can be observed in his treatment of purely algebraic subjects. In the Theory of Invariants he was predominantly interested in the full Cartesian form of a quantic and its covariants. The compact notation which the German mathematicians used to great advantage did not hold the same attraction for Cayley. The suggestion here is that the symbolic notation for a covariant, like the quaternion, would have to be 'unfolded' to be properly recognised by Cayley.

A similar parallel can be drawn in Cayley's treatment of matrices. The 'Cartesian form' of a matrix corresponding to the matrix expressed as an array of its elements (or of a linear substitution in terms of the linear equations) and the compact form corresponding to the single letter symbol representing the array. Cayley referred to matrices (when used to express a linear substitution of variables) as a very condensed notation in his treatment of the transformation of a bilinear form [1861d; CP4, 391] . In this he gave both the transformation in terms of the single letter symbolism and in terms of the corresponding linear equations. Cayley regarded the transformation of a bilinear form when expressed in the single letter symbolism as being of 'no difficulty' but he also 'unfolded' this condensed notation to give its meaning in terms of the linear equations.[5]

Though Cayley was one of the first mathematicians to grasp the importance of n-dimensional geometry (to Cayley this had little to do with reality and was termed an ideal space) his geometric researches were principally concerned with real Euclidean space. Cayley was an Euclidean geometer concerned with the study of curves and surfaces in this space. He identified this space with physical space and firmly upheld its a priori character:

> My own view is that Euclid's twelfth axiom in Playfairs form of it [Through a point not on a given line there exists a unique parallel line] does not need demonstration, but is part of our notion of space, of the physical space of our experience - the space, that is, which we become acquainted with by experience, but which is the representation lying at the foundation of all external experience. [1883a, 9]

He saw that the removal of Euclid's Twelfth Axiom gave rise to non-Euclidean geometries but these were understood to be immersed in Euclidean space. Because geometry meant Euclidean geometry to Cayley he was unable to contemplate a non-Euclidean geometry distinct from Euclidean geometry. D.M.Y.Sommerville judged that Cayley:

> never quite arrived at a just appreciation of the science. In his mind non-euclidean geometry scarcely attained to an independent existence, but was always either the geometry upon a certain class of curved surfaces, like spherical geometry, or a mode of representation of certain projective relations in Euclidean geometry. [Sommerville, 1958a, 158].

Complex Numbers

Cayley stressed the importance of complex numbers in geometry:[6]

> it is a notion of a complex number implied and presupposed in all the conclusions of modern analysis and geometry. It is, as I have said, the fundamental notion underlying and pervading the whole of these branches of mathematical science [1883a, 14]

By the 1880s the mysteries surrounding the existence of $\sqrt{-1}$ had largely been dispelled. No longer was there the need to interpret $\sqrt{-1}$ as a physical entity. Cayley's interpretation of complex numbers in geometry was based on the Principle of Continuity originally inspired by Kepler. Both Sylvester and Cayley invoked this principle freely. As a philosophical principle it lay at the core of their mathematical outlook.

As a youth, Cayley was aware that $\sqrt{-1}$ needed no physical justification. The 'right angle' interpretation which was not Cayley's, throws some light on Cayley's view of the connection between algebra and geometry. In a review of a paper (by A.J.Ellis) Cayley noted that it contained: 'Some of the views [of George Peacock and William Walton] in regard to imaginaries in algebra and geometry - which I in no-wise agree to - are perhaps as orthodox as my own [Roy. Soc. London, RR. 4.72]. The implication of this interpretation of $\sqrt{-1}$ meant that while Cayley would regard

$$x^2 + y^2 = a^2 \qquad \text{or} \qquad y = \pm\sqrt{a^2 - x^2}$$

as the equation of a circle in real space, Walton and Peacock would obtain another curve (for $x > a$) relative to a new axis at right angles to the x and y axes. Cayley studied curves in real Euclidean space and a geometric interpretation as an actual geometric entity corresponding to the symbol $\sqrt{-1}$ was not given. Cayley invoked the Principle of Continuity to preserve generality in his algebraic reasoning. Cayley's line of argument in an elementary case illustrates how the real geometrical picture was made to comform to the algebraic conclusions using this Principle:

> In the case of a straight line and a circle this [the curve of intersection] is a quadric equation; it has two roots, real or imaginary. There are thus two values, say of x, and to each of these corresponds a single value of y. There are therefore two points of intersection - viz. a straight line and a circle intersect <u>always</u> in two points, real or imaginary [1883a, 13]

But no satisfactory meaning could be offered for an actual imaginary entity save a constructional argument in which the reader was eventually called upon to 'imagine' the so-called imaginary points. Cayley's difficulty was that his geometry was concerned with real Euclidean space and not with a space over the complex numbers. From his need to preserve generality in his algebraic reasoning, Cayley accepted such geometrical statements as: any two (plane) circles intersect in two points; from any point in the plane there are always two tangents to a conic; two lines meet at a point. His justification for employing the Principle of Continuity in geometry was that it could not be contradicted by experience.

5.3. Cayley as a scientist

Cayley paid little attention to the traditional areas of applied mathematics. In this sense of 'Cayley as a scientist' his contribution could be described briefly. He worked on such subjects as Theoretical Dynamics, Astronomy, curve tracing apparatus, the Principles of Double Entry Book-keeping, and an unsuccessful attempt to design a machine for tracing engineering drawings.[8] This is not the meaning of the phrase 'Cayley as a scientist' as will shortly be discussed.

Cayley was ready to give help to scientific colleagues on mathematical questions (including the performance of extensive lunar calculations) but at the beginning of his career, as recounted to Boole, his real preference was for pure mathematics:

> I did in pursuance of our agreement make a feint attempt to read some physical optics, but I found myself, getting back always to my favourite subjects - linear transformations & analytical geometry, & gave up in despair.
>
> [App C, Boole, 3 xii [1845], year estimated][9]

Cayley's lack of enthusiasm for the questions which interested 'applied mathematicians' did not pass without criticism. His friend, Sir William Thomson, perhaps reflecting his own bias, (in a letter dated 31 vii 1864 to Helmholtz) lamented Cayley's lack of interest in the 'advancement of the world':

> The full working out of the solution, too, for the circular plate, shows no small amount of courage, skill, and well-spent labour. [Kirchoff's work on Plates] Oh! that the CAYLEYS would devote what skill they have to such things instead of to pieces of algebra which possibly interest four people in the world, certainly not more, and possibly also only the one person who works. It is really too bad that they don't take their part in the advancement of the world, and leave the labour of mathematical solutions for people who would spend their time so much more usefully in experimenting.
>
> [Thompson, S.P, 1910a, 433]

But although Cayley did not join with such men as Thomson, Stokes or Maxwell in their mathematical theories of the physical world, his own methods within mathematics were not so different as their own. Indeed it will be argued that Cayley's methods had much in common with the traditional activities of Victorian scientists generally. This is the meaning of the phrase 'Cayley as a scientist.'[10]

Cayley had more reason for pressing these primitive lines of attack than a mathematician today. To Cayley the axiomatic method was unavailable, whereas a modern mathematician is able to think of a specific mathematical entity in terms of an axiomatic framework. Cayley discovered these entities in a theoretical vacuum. Deduction, the concomitant of the axiomatic method, naturally played a lesser part in Cayley's mathematics. In his Notice on Cayley, the mathematician, Max Noether, referred to him as the 'natural philosopher amongst mathematicians' [1895a, 479]. And he meant by this, that Cayley was primarily a discoverer of mathematical truths with a strong leaning towards the Heuristic.

Sylvester compared (in the course of an Address to a non-mathematical audience) their work in the Theory of Invariants to the work of the physicist. The essential constituent of course was discovery:

> And, as it is a leading pursuit of the Physicists of the present day to ascertain the fixed lines in the spectrum of every chemical substance, so it is the aim and object of a great school of mathematicians to make out the fundamental derived forms, the Covariants and Invariants, as they are called, of these Quantics.
>
> [1877d; SP3, 76]

Thus Cayley was concerned with finding processes which would generate the covariants. As has been seen (Chapter 1, page 30) Cayley established a number of different processes for finding covariants. The criterion for deciding on the best process was based on its efficiency as a calculating device. A process might be valuable theoretically but it would be disregarded in favour of a process which was more efficient in the production of covariants.

As the Victorian scientist gathered his specimens, Cayley gathered his covariants. The outlook is palpably conveyed by a seemingly innocuous remark in conclusion to the Fourth Memoir on quantics [1858d]. After observing that a certain resultant of two polynomials would yield a succession of covariants, he remarked:

> The modes of generation of a covariant are infinite in number, and it is to be anticipated that, as new theories arise, there will be frequent occasion to consider new processes of derivation, and to single out and to define and give names to new covariants.
>
> [1858d; CP2, 526, my italics]

Classification

Cayley's intention 'to single out and to define and give names to new covariants' is the first principle of any general classification procedure.[11] Faced with the vast array of invariants and covariants, classification by name was a natural way of organising the infant theory and perhaps accounts for the introduction of an abundant terminology. 'Progress in these researches', wrote Sylvester at the beginning of the 1850s, 'is impossible without the aid of clear expression; and the first condition of a good nomenclature is that things shall be called by different names' [SP1, 280]. Cayley was extremely circumspect about the introduction of terminology into mathematics but he supported Sylvester in the endowment of the subject's spectacular vocabulary. In deriving their terminology from Latin and Greek it is evident that they were hoping to achieve a 'state of fixity' of terminology as was also striven for in the Natural Sciences. Although the words used were possibly less obscure to the British nineteenth century mathematician, it amounted to a private language between Cayley, Sylvester, Salmon and possibly a few others. If Salmon had difficulty with the terminology, the general reaction of the Continental mathematicians could only have been one of bemusement. While visiting Europe in 1857, Hirst recorded the views of Joseph Liouville(1809-82):

> He[Liouville] acknowledged their ability but he protested against their wilful obscurity. He considers Cayley and Sylvester to be in some measure the disciples of Cauchy in this respect. In order to attain a broader view of the subject, they lose precision. Ordinary phraseology hampers them, and without hesitation they coin a language of their own, useful to them, no doubt, but for others decidedly inferior to the ordinary language. To be precise and clear is equivalent in their eyes to being tedious. Rather than march over their difficulties and through their conquered territory with a firm, steady step, they leap and turn somersaults. It is possible that by so doing they are able to take a rapid and sufficient view of their subject, but others decidedly see better with their heads upwards.
>
> [App. C, Hirst Diaries, 3, 1327, 18 xi 1857]

Liouville failed to appreciate that aspect of Cayley's mathematical activity which dwelt on calculation and the advances Cayley made as a result of this work.

Induction and analogy

According to the mathematician, E.W.Hobson, writing in 1910:

> The actual evolution of mathematical theories proceeds by a process of induction strictly analogous to the method of induction employed in building up the physical sciences; observations, comparison, classification, trial, and generalisation are essential in both cases.
> [Hobson, 1910a, 520] .

Both Cayley and Sylvester were anxious to stress these aspects of their mathematics to scientific colleagues. In the Theory of Invariants, Hobson's characteristics had all played a part. That Cayley did not see Geometry as a purely deductive procedure can be seen in a response to an attack on the position of geometry in the mathematical curriculum by Sir George Airy:

Whereas Geometry (of course to an intelligent student) is a real inductive and deductive science of inexhaustible extent, in which he can experiment for himself - the very tracing of a curve from its equation (and still more the consideration of the cases belonging to different values of the parameters) is the construction of a theory to bind together the facts - and the selection of a curve or surface proper for the verification of any general theorem is the selection of an experiment in proof or disproof of a theory. [App.C, Airy, 6 xii 1867]

But as a method, induction from elementary cases in algebra and from physical geometrical models in geometry provided a fruitful point of departure for ensuing generalisations. When Sylvester wrote to Cayley on whether to apply to the Royal Society for a grant in order that a model of an algebraic surface should be constructed Cayley replied:

> you may with great propriety apply to the R.S. for a grant (...) I should be very glad if a few more algebraical surfaces could be modelled.
>
> [App B, 14 iii 1865] [12, 13]

To Cayley these models were as the 'drawings in the sand'. Cayley definitely did not subscribe to Induction (in Mill's sense) as the process through which notions of the primary elements (straightness, line) were obtained. As with many mathematicians of the nineteenth century, Cayley believed in certain innate ideas not derived from experience of any kind. The truths of geometry were truths because they were concerned with independently existing 'Universals':

> I would myself say that the purely imaginary objects are the only realities, the ὄντως ὄντα [really real] , in regard to which the corresponding physical objects are as the shadows in the cave.
>
> [Cayley, 1883a, 7]

Generalisation

While the study of particular cases occupied much of Cayley's attention, he was of course interested in the more general theory. In the case of determinants, the 'cubic' determinants' were immediately considered, leading to the ultimate generalisation in the Permutant (Chapter 2, page 60). But generalisation is not abstraction and Cayley should be considered as a 'generalising' mathematician rather than an 'abstract' mathematician.
Cayley's paper on groups [1854a] is well known and it serves to illustrate this distinction. In this work Cayley created a set operator, a concept which included the substitutions of Galois. G.A.Miller [1935a, 1, 427] considered (with hindsight) this paper to have inaugurated the theory of abstract groups. But the concept there introduced was not abstract (in the sense of axioms and deduction from axioms) but was rather a more general notion of a group than had hitherto been considered. The elements of Cayley's groups were operations not symbols satisfying a list of axioms. The set operators as a generalisation is evident from the wording of a letter he sent to Sylvester:

> I consider a substitution, when applied to an arrangement as corporified, and using the word group as primarily applicable to substitutions (or to my more general set operators as I propose to call them) I use the expression corporate or corporal group to denote a group of permutations. But the word group may be understood as denoting or including corporate group.
>
> [App.B, 18 viii 1860]

Cayley was interested in proving for his set operators the theorems which Cauchy had proved for substitution groups:

> the very theorem which I mentioned to you that I was in want of - which I wish you would consider - viz. that any group whatever of set operators every prime factor of the order of the group presents itself as the index of at least one operator.
>
> [App.B, 18 viii 1860]

Cayley touched on abstraction (in group theory) in the late 1870s [1878c, 1878d], but he did not appear to pursue an abstract development.[14]

In Cayley's work there are projects involving large scale calculations pertaining to a very special case and work involving conceptual extensions of immense generality. However, a theoretical development obtained from clearly stated axioms is missing. Cayley was able to obtain putative axioms by generalisation but as his reaction to Hilbert's proof of Gordan's Theorem (Chapter 4, page 188) perhaps showed, an abstract deductive approach to proof was beyond his ken. Without the theoretical developments and powerful techniques which are possible with the axiomatic method, Cayley was compelled to proceed with relatively unsophisticated methods.

Proof and Rigour

In reviewing Cayley's attitude to proof in mathematics it is interesting to know that Cayley's attitude was the rule rather than the exception. Referring to the Greek ideal of deductive proof from explicit axioms, Morris Kline remarked:

> It is one of the astonishing revelations of the history of mathematics that this ideal of the subject was, in effect, ignored during the two thousand years [200 B.C. to about 1870] in which its content expanded so extensively. [1972a, 1024] .

It is a paradox that British mathematicians, in particular, should have manifested such a casual attitude to proof, when one considers the central position which that bible of the axiomatic method, Euclid's Geometry, enjoyed in Victorian Education. Not only this, but, when moves were afoot to banish Euclid from the school curriculum, Cayley defended its retention. And in his Address he declared 'there is hardly anything in mathematics more beautiful than his Euclid's wondrous fifth book ...' [1883a, 21] .

How did Cayley reconcile his esteem for Euclid with the implication contained in a note from Sylvester?:

> I am revising the proofs of my paper on compound determinants (...). It is a large subject largely treated and (which will please you) contains no proofs whatever. It all rests on faith - There will be about two dozen pages of pure assertion in it.
>
> [App.B, 12 vii 1879]

The answer is that Cayley esteemed the content of Euclid but ignored the method. In Invariant Theory, as well as other algebraic researches, the object was calculation and taxonomy. Actual 'proof' was relegated to a secondary position.

However, Cayley was not opposed to proof in principle. The desire to provide a proof of Gordan's Theorem is ample evidence of this. But this was a theorem thought worthy of proof. Frequently a statement was regarded as true on the evidence of its truth for simple examples. This was the case for theorems on determinants, matrices and quantics of degree n (for example: Chapter 1, page 42; Chapter 2, page 76). But perhaps the most commonly quoted example of Cayley's failure to provide a proof occurred in his [1858a] where the subsequently named Cayley-Hamilton Theorem was stated but a proof provided only in the case of second order matrices. In fact, the absence of such a proof on this particular occasion was criticised by the referee, George Boole:

> One theorem referred to on p.2 of the Introduction [the Cayley-Hamilton Theorem] and illustrated in Articles 21,..., 24 is less elementary than the others. The author after giving a particular exemplification of it in the case in which the subject quantities are two in number, and stating that he has verified it in the case in which their number is three, adds "but I have not thought it necessary to undertake the labour of a formal proof." It certainly if generally true ought to admit of a symbolical proof not involving much complexity but resulting from the first principles of symbolical algebra - this being the kind of proof, which according to analogy and from the intrinsic character of the theorem ought to be sought for. And I must add that I cannot but regard the memoir as essentially incomplete without such a proof.
>
> Even with this defect, however, I have no hesitation in recommending the paper for publication in the Philosophical Transactions.
>
> [Royal Society of London, RR. 3.55] [15]

Cayley was unused to the discipline of proof, although it is just conceivable that a proof based on the identity

$$B \operatorname{adj} B = \det B \, I$$

would have been possible for n = 3. This identity was a known fact [Cayley,1858a; CP2, 481] . Glaisher's description of Cayley distinct roots. In considering only the 'general' case Cayley implicitly assumed by 'continuity' that similar properties held in the limit. The special cases could thus be disregarded. Cayley did not make a deep enough impression on the theory of matrices to appreciate that there could be a 'discontinuity of property' associated with matrices which possessed characteristic polynomials with equal roots. [19, 20]

Frequent use was made by Cayley of proof by the 'method of verification.' A specific result would be obtained by any means and it was subsequently verified that a correct result had been obtained. Thus in a problem to find $L = \sqrt{M}$ [1872a] where M was an arbitrary matrix, a formula was sought by a method chosen purely for its expediency. The result was verified by showing the symbolic identity $L^2 = M$.

Cayley's attitude to mathematical proof had a great deal of affinity to that of Felix Klein. According to Constance Reid [1970a, 146], Klein never possessed the patience to provide logically perfect demonstrations for theorems which he was convinced were true. Cayley appeared to only supply enough detail to convince himself of the correctness of the result. It was more exciting to discover results than ponder over a carefully reasoned argument in support of a result that one thought was true. As indicated by Kline, Cayley's attitude to proof was not unusual amongst a wide group of nineteenth-century mathematicians. His particular interest was discovery and in this enterprise logic played little part. One of Koestler's remarks is particularly apt:[21]

> A locksmith who opens a complicated lock with a crude piece of bent wire is not guided by logic, but by the unconscious residue of countless past experiences with locks, which lend his touch a wisdom that his reason does not possess.

Calculation

The task of the Victorian scientist was to locate and order and to collate and name. To 'Cayley the scientist' working in the Theory of Invariants, to locate meant to calculate the invariants and covariants. The task of calculation was a daunting one from the beginning of the Theory. Cayley was undeterred by the prospect of long drawn out calculations. His equanimity in the face of such calculatory work is seen in an early letter written to George Boole:

> I have a plan just now of effecting to a certain extent, the elimination of the variables between three quadratic equations: the results as far as I mean to expand it will contain about 7 or 800 terms, each of them the product of four determinants of the third order and linear in the coefficients of each of the equations. I can do so without any excessive trouble by a method given by Hesse.
>
> [App.C, Boole, 11 xi 44]

Although Cayley was interested in calculation it should not be inferred that he was interested in numerical calculation for the purposes of approximation. Cayley's interest in calculation was limited to the exact calculation which had a bearing on the theoretical development of a subject. The difference between the numerical solution of equations and the calculations of the symmetric functions was a case in point. A mild reproach from Sylvester for not taking more of an interest in numerical approximation is illuminating:

> Why should you despise this subject? [Quadrature Methods] or regard it only as a mere matter of Numerical Approximation? Your question "et puis?" [and after?] would have choked many a grand theory in the bud.
> [App.B, 20 xi 1862]

Cayley's love of calculation was both a help and a limitation. Through the calculative element he was able to develop an intuition by familiarity with elementary cases. But his adopted methods were not so successful with higher order binary quantics and in the 1880s he pursued the listing of invariants at the expense of developing an abstract theory. The original objective of [1846b] which was to 'find' the invariants was double edged. It meant that when Cayley considered theoretical questions he appeared to favour techniques which were efficient as devices for calculation, irrespective of their theoretical potential. This occurred in Cayley's adoption of the 'new synthesis' (Chapter 2, page 64). With hindsight it was seen that important theoretical results could be obtained by the derivational symbolic method.

Cayley's role in algebra was classificatory. He played a leading part in the establishment of algebraic theories but the discovery of mathematics was part of the Spirit of the Age and not axiomatic mathematics. A comment by A.N.Whitehead on the Natural Sciences is perhaps applicable to the ultimate scope of these classificatory methods:

> Classification is a half-way house between the immediate concreteness of the individual thing and the complete abstraction of mathematical notions. The species take account of the specific character, and the genera of the generic character (...). Classification is necessary. But unless you can progress from classification to mathematics, your reasoning will not take you very far.
>
> [Whitehead, 1927a, 37]

5.4. Cayley and Cambridge

The scarcity of University positions in mid nineteenth century Britain meant that even Cayley found some difficulty in securing a suitable academic appointment (Chapter 2, page 107).
By the time he was appointed to the Chair at Cambridge (in 1863 when he was 41 years of age), he had established an impressive record as a research mathematician but was relatively inexperienced as a teacher. On his appointment, research activities continued to occupy most of his attention, for the teaching duties attached to the Sadleirian Chair were light. For a number of years they consisted of only one term's course of lectures in an academic year. The organisation of the Mathematical Tripos Examination contributed to the fact that Cayley's classes attracted few students.

In its content, according to [Glaisher 1886a], the Tripos had remained fairly static during the period 1850 - 1873. The only change appeared to be a growth in Analytical Geometry and Higher Algebra. In 1867 there was movement towards the introduction of 'applicable mathematics' (subjects such as Electricity and Magnetism) being included. One of those in favour of reform was Sir W. Thomson who was against students 'wallowing in conic sections' in order to score high marks [App.C, 20 xii 1866]. With change imminent, Cayley resolutely affirmed the teaching terms of his Sadleirian Professorship: ' to explain and teach the principles of pure mathematics.'

Cayley's approach to the teaching of mathematics was not antipathetic to the needs of science. He took a positive outlook and regarded it his duty to teach mathematics for the subject itself with little attention paid to external considerations. By this means, he argued, students might understand mathematics and thereby be more proficient in its application. Cayley's position is summarised in a brief exchange of letters with Airy in 1867. Airy complained that students were wasting valuable time on such subjects as Analytic Geometry and 'useless algebra', subjects, he claimed, having little practical relevance. Airy, was something of a University politician who worked hard to reform the educational curricula. Cayley, the devotee of Pure Mathematics for its own sake, found himself on the defensive against an Astronomer Royal bent on establishing mathematics as a tool for the physical sciences. The different standpoints are apparent from their statements on the subject of partial differential equations. Airy thought that:

> the Partial Differential Equations are very useful and therefore stand very high (in ability to solve problems), as far as the Second Order ..... Beyond that Order they apply to nothing.
>
> [App.C, Airy, 8 xi 67]

Cayley's reply took due account of theoretical progress in the subject:

> As to Partial Differential Equations, they are "high" as being an inverse problem [Integration problem], and perhaps the most difficult inverse problem that has been dealt with. In regard to the limitation of them to the second order, whatever other reasons exist for it, there is also the reason that the theory to this order is as yet so incomplete that there is no inducement to go beyond it;
>
> [App.C, Airy, 6 xii 67]

Airy was primarily interested in the application of existing mathematics while Cayley saw the challenge of mathematics per se and was more far sighted. Cayley was not arguing against reform, but in defence of Pure Mathematics:

> But admitting (as I do not) that Pure Mathematics are only to be studied with a view to Natural and Physical Science, the question still arises how are they best to be studied in that view. I assume and admit that as to a large part of Modern Geometry and of the Theory of Numbers, there is no present probability that these will find any physical applications. But among the remaining parts of Pure Mathematics we have the theory of Elliptic Functions and of the Jacobian and Abelian Functions, and the theory of Differential Equations, including of course Partial Differential Equations.
>
> [App.C, Airy, 10 xii 67]

The reforms were agreed and came into operation in 1873. The University itself was also changing. In 1857 the function of the University was judged to be one of education but in the succeeding decades it gradually changed its philosophy to one of research. A Commission of 1877 sought to help the University become an institute for original research.

By the late 1870s there was at least a prospect of Professors with interests in research becoming less isolated from the principal concerns of the University. A glimpse of Cayley at this time is offered by Karl Pearson in a reminiscence of the day he sat for a Smith's Prize examination:

> The next day we went to Cayley's. His first words were, "Throw off your gowns, gentlemen, you will work more easily without them", and accordingly they were dropped in a heap in a corner of the room, and we set to work unencumbered. Of course I knew nothing of the topics of Cayley's paper. My chance of scoring marks in the Tripos had depended only on my applied mathematics, and my pure mathematics were but sufficient to help in the former branch. But I took things leisurely, as if nothing depended on speed, and worked as one might work in solving crossword puzzles on a train journey. Cayley did not appear at lunch; sandwiches, biscuits and other light refreshments were brought up on a tray, accompanied by a decanter of excellent port wine; Cayley had not spared his cellar. After sampling a glass, I tried to persuade my co-examinees to do so likewise; two, I think, took a driblet, but the future Smith Prizeman, speaking from his conscience, refused - he was true to what he had originally said in our first term. He had come to Cambridge for examination ends; perhaps he thought I was tempting him to drop the prize already well within his grasp. Back we went to our writing, I feeling the better for Cayley's port, and the others satisfied in their consciences that they had done the right thing under examination stress . Cayley evidently did not think good port at all incompatible with the discussion of invariants or higher algebra.
>
> [Pearson, 1936a, 32]

But the man who was perhaps influenced most by Cayley's mathematical interests was Andrew Russell Forsyth (1858-1942). One of Cayley's students, he was also Cayley's biographer and successor to the Sadleirian Chair in 1895. Forsyth recalled that few undergraduates attended Cayley's lectures because of their advanced character.[22] They were always on his latest research and 'old notes were never used a second time' [Forsyth, 1895a, xvi - xvii] .

The subjects taught were hardly ever examined. According to Forsyth the Theory of Invariants was ignored as an examination subject (other than in geometrical topics) in the Tripos[1935a,171] Even among the most advanced students Cayley, Sylvester and Salmon were a 'world-triumvirate in a dark continent of invariants.' An expressive account of a Cayley lecture is given by Forsyth (taken from a letter to J.J.Thomson[23] in 1935): [24]

> They [a course of Cayley lectures] were in the Michaelmas Term 1879, nominally on differential equations: the subject was never mentioned after the first ten minutes: instead, he discussed icosahedral functions, groups, covariants, and so on: as comprehensible by me at that stage as if he had been dealing with Chinese syntax. The audience was small: five of us sitting in a row on a form. There was no chalk: the single blackboard was used as a rest for Cayley's blue draft-paper manuscript, held several feet away from the nearest pair of eyes. Cayley held the manuscript more or less in place with his left hand; his right forefinger would move along a formula which he spoke aloud. Once or twice, he broke off, in order to write on a sheet of paper some "very important" formula: once, it was
>
> $$\cos(A+B)\cos(A-B) = \cos^2 A - \sin^2 B$$
>
> At the furthest end of the row sat Glaisher, reading proof sheets (Messanger or Quarterley, or something equivalent): Then Pendlebury, who could see nothing and could not hear much for even then he was a little deaf: Then R.C.Rowe, who mostly stood up in his place and looked over Cayley's shoulder while he tried to make notes on the scribbling paper at arms length on the desk in front of him: then myself, writing down whatever could be seen or heard, a disconnected jumble to be developed into something more or less coherent by much labour: finally, nearest the door, and looking slantwise across the distant blue paper on the board, J.D.H.Dickson, writing much, sometimes notes of his own, sometimes fragments of mine. Not much of a lecture, one would say: but the great man was at work, and we all believed in him.

An exception to the small number of students at Cayley's lectures was the Michaelmas term of 1881. In that year there were 12 students which he remarked was an "exceptional number."[25] Forsyth also gave an account of these lectures (in continuation of the previously quoted passage):[26]

> The course, to which your letter refers, was (I think) the course given in the Michaelmas Term 1881: somehow, Abelian functions had sprung into a semblance of popularity: by that date Cayley had begun to chalk on the blackboard, though never at ease, and he had many a half-furtive look at his watch long before the hour was up. The class had grown to nearly 15. Miss Scott was there.[27] W.D.Niven came, certainly for a time, and even now I can remember the surprised admiration Niven expressed for an algebraical theorem of Jacobi's. Once Sir William Thomson came; and he sat for the whole hour, without a single interrupting question. But again, it was not the information given us that mattered: again, it was the great man at work.

In 1882 the Tripos was altered to include an advanced part with an exam detached from the competitive order of merit examination. Something needed to be done if G.H.Darwin's view was accurate. In his Inaugural Lecture he noted: 'I think it is not too much to say that there is no vitality [in mathematics] here '[28] But the changes in the regulations which allowed for mathematics of an advanced character to 'count' as part of the degree came too late for Cayley. He had few post-graduate students and he did not lead a school of mathematicians as did Sylvester at Baltimore.[29] Some of the younger mathematicians desired a mathematical school similar to research schools such as Klein's Seminar at Göttingen. Cayley pursued his research as an individualist and personally worked on the kind of calculative work Klein would have delegated to students.

During the last part of the nineteenth century the Cambridge Mathematical School was criticised from many quarters on account of its insularity. In her reminiscences of Cambridge in 1889 Grace Chisholm Young had this to say:

> Mathematical Science had reached the acme of perfection. Through the long future ages, no new ideas, no new methods, no new subjects were to appear. The edifice of mathematical science was complete, roof on and everything. All that remained to be done was to consolidate and repair the masonry, and add to and correct the ornamentation.
>
> This was the view in those days, and the atmosphere was stifling to the young mathematician. Cayley, unconscious himself of the effect he was having on his entourage, sat, like a figure of Buddha on its pedestal, dead-weight on the mathematical school of Cambridge. [Grattan-Guinness, 1972a, 115]

From his own survey of the scene at Cambridge, Glaisher conceded: 'I am afraid that the old saying that we have generals without armies is as true as ever' [Glaisher, 1890a, 724] .

Arthur Cayley died at Cambridge on the 26th of January 1895 following a period of poor health. On the occasion his friend and colleague, Lord Kelvin, wrote:

> In Cayley we have lost one of the makers of mathematics, a poet in the true sense of the word, who made real for the world the ideas which his ever fertile imagination created for himself. He was the Senior Wrangler of my freshman's year at Cambridge [Preface, page 4] and I well remember to this day the admiration and awe with which, before the end of my first term, just fifty-four years ago, I had learned to regard his mathematical powers. When a little later I attained to the honour of knowing him personally, the awe was evaporated by the sunshine of his genial kindness; the admiration has remained unabated to this day, and his friendship has been one of the valued possessions of my life.
>
> [Thompson, 1910a, 950]

At this time, Invariant Theory was perceived by many mathematicians as occupying a central place in Pure Mathematics. Writing in 1897, Forsyth's estimate of its influence was unequivocal:

> It has invaded the domain of geometry, and has almost re-created the analytical theory; but it has done more than this, for the investigations of Cayley have required a full reconsideration of the very foundations of geometry. It has made its way into the theory of differential equations; and the generalisation of its ideas is opening out new regions of the most advanced and profound functional analysis. And so far from its course being completed, its questions fully answered, or its interest extinct, there is no reason to suppose that a term can be assigned to its growth and its influence.
>
> [Forsyth, 1897a, 548]

In one sense Cayley's Invariant Theory died with him.[30] His blunt methods resulting in cumbersome computation, often pursued with little regard for abstraction or proof, became unfashionable with succeeding generations. Cayley's expertise was in the finite processes of algebra and the development of formal calculi with little attention being paid to infinite processes. 'The absence of analysis reflected the isolation of British pure mathematics from the Continent' wrote Sir Edward Collingwood [1966a] and continued: 'By the turn of the century the isolation had been broken and the emphasis in pure mathematics here England as abroad, lay heavily on analysis (...) and in particular the theories of functions of a real variable and a complex variable.'

In Invariant Theory, Cayley's lines of attack fell into desuetude. Elliott's Algebra of Quantics, which embodied Cayley's non-symbolic method was published in 1895, the year of Cayley's death. While it carried Cayley's method into the twentieth century, Grace and Young's Algebra of Invariants, published in 1903 and written in the German symbolic notation, was thought to be more important by the rising generation of algebraists. With its streamlined approach to the subject, H.W.Turnbull [1941a] judged that a 'new era dawned for the teaching and progress of higher algebra.'

Invariant Theory has now lost its position as one of the great theories of Pure Mathematics. Yet many of Cayley's underlying ideas continue to inspire new mathematics. Cayley's contribution to Invariant Theory and to Algebra generally permeates the entire Subject:[31]

> Yet I doubt not through the ages one
> increasing purpose runs,
> And the thoughts of men are widened with
> the process of the suns.

Chapter 5

References

1. Cayley gave his address on 19 ix 1883. The British Association occupied an important place in Victorian Science. Among the 'technological success' promised for the 1883 Meeting was an electrical display.

2. E.H.Carr What is History? (1964 Penguin) page 8.

3. The notion of ordinal number taking precedence over cardinal number was not new. It was expressed by Hamilton in his Lectures on Quaternions [1853a] a book Cayley had reviewed [Stokes, 1907a, 386] . The question of the foundation of number had been actively discussed by both Hamilton and De Morgan during the 1830s. As is well known, Hamilton based the idea of number on Pure Time. De Morgan preferred not to use the word time but to base number on continuous succession of points on a line [1839a,176].

4. Cayley's best known contribution to algebraic geometry is most likely the sixth memoir on quantics [1859a] . C.S.Peirce deplored the fact that Cayley's sixth memoir was not more widely read: 'that immortal memoir of Cayley's - perhaps the greatest luminary, it was, of all my mathematical life' [Eisele 1976a, vol 3 (ii), 984] .

5. Cayley appeared to see little potential (during the 1860s) for matrices (other than a compact notation) when used to express linear substitutions Questions could be effectively dealt with by the corresponding linear equations.

6. It is likely that Cayley was not aware of von-Staudt's geometric construction of a complex number until very late in life. A letter to Klein [App.C,Klein, 23 vii 1889] gives the impression that Cayley had not read von Staudt before 1889.

7. Sylvester enthusiastically appealed to new uses of the Principle in a letter to Cayley [App.B, 18 iv 1861] .

8. Inference drawn from a letter written to Cayley by an engineer. [Sylvester Papers, St.John's College] .

9. This letter is catalogued 3 xii 1846 but it was almost certainly written one year earlier.

10. M. Kline quotes a letter (from Hermite to Stieltjes) drawing attention to the similarity of the objectives existing between mathematician and scientist: 'I believe that they [numbers and functions of analysis] exist outside us with the same character of necessity as the objects of objective reality; and we find or discover them and study them as do the physicists, chemists and zoologists' [Kline, 1972a, 1035] .

11. The initial development of the subject has much in common with other fast developing sciences of the period such as Zoology and Botany. The appearance of newly found covariants has an obvious parallel with newly discovered animal and plant species linked through genera. Both Cayley and Sylvester make frequent use of the terms species and genera in their work. An example where classification was perhaps more apparent occurred in the classification of plane curves. In the case of quartic curves, for instance, there were ten genera corresponding to the different Plückerian characteristics. With each genera there were many different species corresponding to the different ways in which singularities combined. In the case of the quartic, Salmon remarked "the number of species is so great, and the labor of discussing their figures so enormous, that it seems useless to undertake the task of an enumeration" [Salmon, 1879a, 213] . Cayley studied several genera in various levels of detail. The 'general' curve (free of nodes and cusps) with 28 bi-tangents was studied in detail by nineteenth century mathematicians but even by the 1920s this simplest of plane quartic curves was not fully understood [Hilton, 1932a, 333].

12. Cayley was fond of making physical geometric models and apparatus for drawing curves. As he wrote to Sylvester on the theory of curves:

> I should certainly be much interested in seeing the curve carefully drawn on a scale of sufficient magnitude to perceive the peculiarity of the form of the infinite branches as compared with those of an ordinary hyperbola. [App B, 31 x 1856]

The aid which Cayley derived from a carefully drawn diagram of a surface is apparent from a letter to Stokes (22 x 1855) [Stokes, 1907a, 385] . A description of Cayley performing experiments with the Möbius Strip at a British Association Meeting in 1873 is given in a reminiscence of Sir Oliver Lodge [1931a, 136] .

13. As a young mathematician, Cayley was greatly influenced by Plücker's work on curves. Plucker described his models of quartic surfaces to the British Association meeting in 1866. Copies of his models are presently (1978) on display at the Science Museum, London.

14. In this curious note (and also in [1878c]) the definition given was abstract ' a set of symbols α, β, γ such that the product $\alpha\beta$ of each two of them (in each order $\alpha\beta$ or $\beta\alpha$), is a symbol of the set, is a group' [1878d; CP10, 402] .

15. This quotation is a continuation of Boole's report (Chapter 2, page 103).

16. Peacock's principle of the permanence of equivalent forms stated: Whatever equivalent form is discoverable in arithmetical algebra considered as the science of suggestion, when the symbols are general in form, though specific in their value, will continue to be an equivalent form when the symbols are general in their nature as well as in their form. [Peacock, 1833a, 199]

Peacock's principle implied that $a^m a^n = a^{m+n}$ is true whatever a, m, n denote and therefore the binomial product theorem would have been 'immediate' by the principle.

17. Cayley returned to the binomial identity for an arbitrary index [1869; CP8, 463] . In this paper he did not query the validity of Peacock's principle,only noting that it should not be applied directly.

18. Cayley would undoubtedly argue that a polynomial with equal roots could be replaced by polynomial of lesser degree with the same (though distinct) roots.

19. A phenomenon which occurred in the earliest use made of the Principle of Continuity which treated the parabola as the limiting case of the ellipse and hyperbola.

20. In the 1880s and the latter part of the nineteenth century a greater interest was shown in rigorous argument. C.S.Peirce attended Sylvester's lectures (Chapter 4, page 162) on matrices at Johns Hopkins and in a later reminiscence (1897) was critical of Sylvester's use of the 'general' case:

20. (continued)

'I have forgotten almost entirely about those lectures. But I remember, or think I do that he [Sylvester] made much of the reciprocal of a matrix, as though that were the ipsissimum of the doctrine and especially of the modulus; and perhaps that was why he called the modulus the "nullity." At any rate, I asked what if the modulus is zero? A zero cannot have a reciprocal. Oh, he said, that is a special case; I am considering only what is true in general. He quite forgot that what mathematicians mean by saying that something is so and so "in general" may, for all that, hardly ever be true.'

[Eisele, 1976a, 2, 866]

21. From A. Koestler The Sleepwalkers (1959, London) page 340.

22. His single text book on Elliptic Functions [1876a] was advanced though Cayley did contribute complete chapters to some of Salmon's undergraduate texts. Cayley's detachment from teaching is apparent from his reported advice on the teaching of curves: 'a complete knowledge of invariants and covariants of ternary forms ought to be presupposed in the teaching of Higher Plane Curves' [C.A.Scott, 1895a, 141] .

23. For an account of J.J.Thomson attending a 'class' of Cayley's, see [Thomson, 1936a] .

24. [A.R.Forsyth,1 vii 1935,Cambridge University Library, Manuscript, Add 7654/F23] .

25. Cross-reference [App.C, Hirst, 31 iii 1882] .

26. As Reference 24 above.

27. Charlotte Angas Scott (1858-1931) was highly placed on the Tripos list in 1880. Women were not awarded Cambridge degrees at the time but a few years later she gained a D.Sc. from London University. She gave an impression of Cayley's lectures in her [1895a].

28. [G.H.Darwin. Scientific Papers, 1916, 5, 2].

29. Perhaps W.K.Clifford, J.W.L.Glaisher, J.J.Thomson, A.R.Forsyth and H.F.Baker account for Cayley's most important students.

30. But not entirely: some aspects of Cayley's Invariant Theory have been reconsidered in recent years due mainly to the current interest in combinatorics (1981).

Further back was the little known application of seminvariants in statistics. The Danish mathematician, T.N.Thiele (1838-1910) developed a theory of half-invariants in his [1931a] published in Copenhagen in 1889. From an algebraic standpoint they are identical to Cayley's seminvariants though the development was independent. Thiele wrote his half invariants (central moments)

$$\mu_1 = \frac{s_1}{s_0} \; ; \; \mu_2 = \frac{1}{s_0^2}\left(s_2 s_0 - s_1^2\right) \; ; \; \mu_3 = \frac{1}{s_0^3}\left(s_3 s_0^2 - 3 s_2 s_1 s_0 + 2 s_1^3\right); \; \ldots$$

where $s_k = k^{th}$ power sums of the observations $\alpha_1, \ldots \alpha_n$
The relevant facts can be found in [Dressel, 1940a] (I am grateful to John Aldrich for this reference).

The statisticians, F.N.David and M. Kendall, produced (without the assistance of a computer) symmetric functions in the 1950s considerably more detailed than Cayley's tables. Apparently these writers were unaware of Cayley's work at the outset of their own calculations. See [Fletcher, 1962a] for references to tables of symmetric functions.

31. Cayley's chosen ending for his British Association Address [1883a]. Quoted from Locksley Hall (written 1842) by Alfred Lord Tennyson (1809-1892).

PHOTOGRAPHS

(Plates 7 - 13)

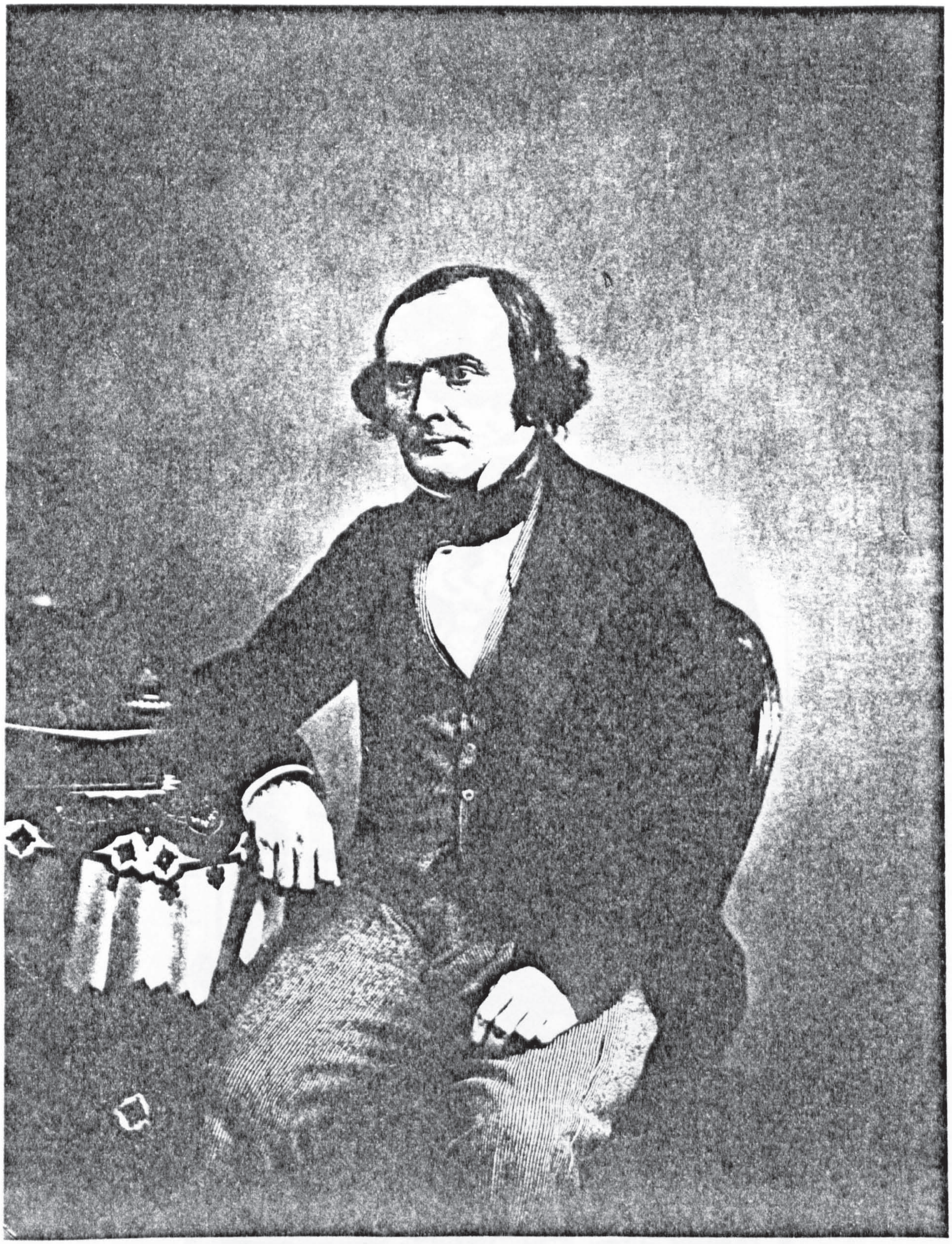


Plate 7

Portrait of Arthur Cayley (1821-1895)

Original (undated) held at Trinity College, Cambridge (Wren Library)

Plate 8: The Senior Wrangler, 1842

Reproduced from [Huber, 1843a] . For a detailed commentary on this portrait see [Watson, 1939a] .
Details of other published portraits of Cayley are given in [Watson 1939a] . Photographs of Cayley in later years are held at Trinity College (Wren Library).

Plate 9: The Cayley family home at Blackheath.

This was the Cayley family house at Blackheath (5 Montpelier Row) from about 1852 to about 1871. Early part of Cayley's life probably spent at 59 Lee Road, Blackheath [demolished 1961] and from 1847 to about 1852 at Cambridge House, The Grove [now West Grove], Blackheath [burnt down, 1881].

Plate 10: 2 Stone Buildings, Lincoln's Inn

Many of Cayley's papers were sent from this address. Cayley worked as a barrister (specialising in conveyancy) in this part of Lincoln's Inn. It is little changed from Cayley's day though it acquired a new facade following war damage in the Second World War.

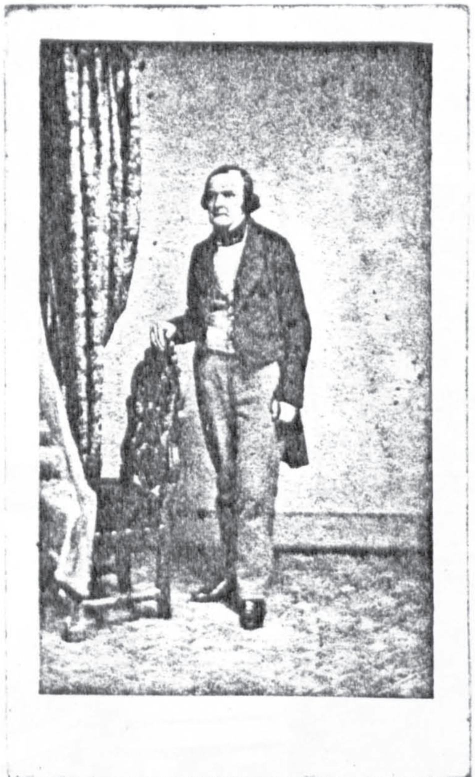


Plate 11: Arthur Cayley as a young man

[Same size reproduction, undated] .

This photograph is little known. It is kept at St.John's College, Cambridge [Sylyester Papers] . It is difficult to date exactly but seems to be a photograph of Cayley in his late thirties.

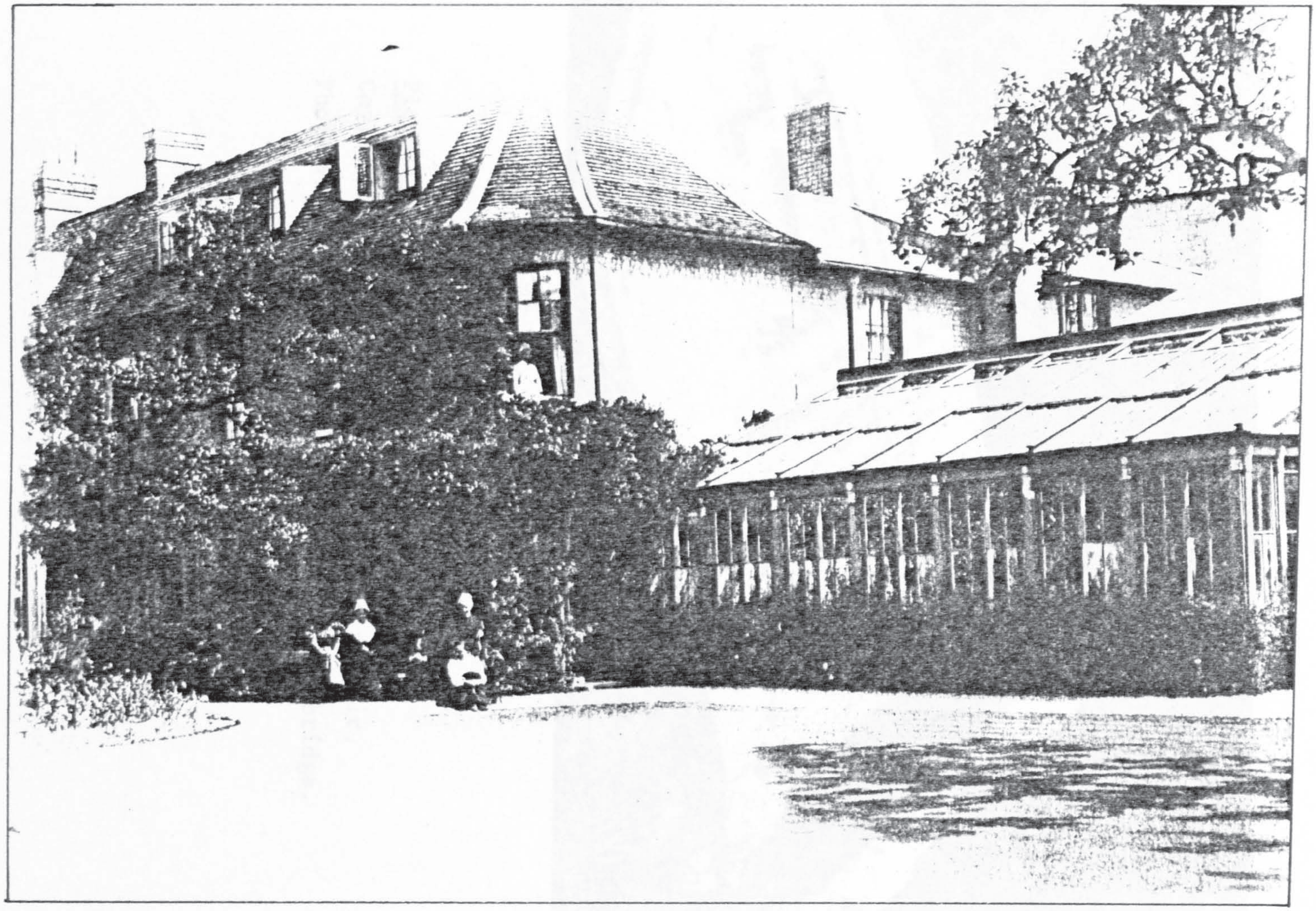


Plate 12: Garden House, Cambridge. Cayley's Cambridge Residence. The photograph is undated but was most likely taken around 1890. The seated figure is almost certainly Cayley. The Garden House Hotel now stands on the site of Cayley's residence. Original held at Cambridge University Library (Manuscript Library).

Plate 13: Gravestone of Arthur Cayley

Cayley's gravestone in sad state of disrepair.
Photograph taken at Mill Road Cemetery, Cambridge.

APPENDIX A

CHRONOLOGY

CHRONOLOGY OF CAYLEY'S MATHEMATICAL INTERESTS

APPENDIX A

Chronology of Cayley's Mathematical Interests

The Chronology is an attempt to chart Cayley's mathematical interests over his lifetime. In total, nine categories have been chosen. There are five broad divisions (Algebra, Geometry, Theory of Functions, Mechanics and a General division) but because of the emphasis in this dissertation, Algebra has been further sub-divided into Algebraic Forms, Algebraic Systems, General Algebra, Combinatorial Analysis and Group Theory.

The results of this categorisation are summarised in Chart 1.

In addition, a year by year pure page count is displayed graphically in Chart 11. This shows the pattern of Cayley's extraordinary mathematical production during the whole period 1840-1895.

1. Outline of Classification with Limitations on its Use.

The principal source used for the compilation of this chronology is the Collected Mathematical Papers. The division into categories is a modern one but I have been guided by Cayley's own classification as in, for example, [CP1, xv] and the Classification System for Pure Mathematics contained in the International Catalogue of Scientific Literature.

2. Classification Categories

The following categories have been chosen:

Algebraic Forms	Algebraic Forms (including classical Invariant Theory and Reciprocants)
	Theory of Elimination
	Determinants and their generalisations
	Linear Substitutions, Transformations
	Systems of Equations
Algebraic Systems	Hypercomplex Numbers
	Quaternions
	Cayley Numbers
	Theory of Matrices
	Multiple Algebra
General Algebra	Theory of a single equation
	Symmetric Functions
	Algebraic Logic
	Elements of Algebra, (including Binomial Theorem, Number Theory)

<u>Combinatorial Analysis</u>	Arrangements
	Partitions
	Factorials
	Probability
	Enumeration of Treees and Isomers
<u>Group Theory</u>	
<u>Geometry</u>	Curves, (including conics, planar and twisted curves)
	Surfaces
	Geometrical theory of the equation u = 0 (where u is an algebraic function)
	Co-ordinates
	Polyhedra
	Linkwork
	Hyperspace
	Non-Euclidean Geometry
	Topology
<u>Theory of Functions</u>	Algebraic Functions (elliptic, abelian and theta functions)
	Differential Equations
	Theory of Integration (including multiple integrals)
	Series, Finite Differences
	Logarithms, Trigonometry
<u>Mechanics</u>	Potential Theory
	Astronomy
<u>General</u>	Addresses
	Contributions to Encyclopaedias
	British Association Reports
	Smith's Prize Questions and Solutions

<u>3. Method of Classification</u>

The classification has been mainly carried out by direct appeal to the title of each paper and, in general a detailed examination of the contents of each paper has not been attempted. The contents of papers has been examined in the cases where terminology used in a title has been sufficiently arcane to make the classification unclear. One obvious difficulty with such a scheme is that a paper may easily fall into two or more categories. This is especially the case with a writer such as Cayley whose universal interests were coupled with a tendency to draw subjects together. For this reason it has been decided to count the number of contributions to each category. Some papers, therefore, have been listed as contributing to more than one category.

In compiling the classification certain conventions and ground rules have been adopted:

(i) Where a title indicates that a paper contains material which falls into more than one category, the paper is recorded under each category. The classification records contributions to categories though it is normally the case that each paper falls into a single category.

(ii) Papers are recorded by year of publication as year of presentation is not easily available for all cases.

(iii) Papers published by the Royal Astronomical Society are recorded under Mechanics.

(iv) Smith's Prize Questions with Solutions are catalogued under General. They have not been given a separate subject classification.

(v) Cayley's book on Elliptic Functions [1876a] is included in the classification.

The following writings of Cayley are not recorded in the overall classification:

(a) Cayley's long standing contributions to the Educational Times as a proposer and solver of numerous mathematical problems. These contributions were made between the following dates:

1863 - 1865 [CP5, 608]
1866 - 1869 [CP7, 607]
1870 - 1894 [CP10, 615]

(b) Cayley's contributions to basic texts. For example, Salmon's [1879a; CP11,217] and [1928a ; CP11,224] .

(c) Translations of Cayley's own papers.

Presentation of Results

The Chronology of Cayley's mathematical interests is shown graphically in Chart I. An estimate of Cayley's mathematical output throughout the period 1840 - 1895 is shown in Chart II.

Chart I

The following symbols have been used to indicate the number of contributions in any one year.

○ 1 - 3 contributions

● 4 - 6 contributions

⊙ 7 or more contributions

A continuous line shows a prolonged period of extensive publication. Chart 1 illustrates Cayley's catholic interests at all times.

In the Theory of Invariants his interest is continuous but there are periods where the interest is greater than in others. Noticeable periods of intense interest in the Theory of Invariants are 1854-1860 and 1878-1885 with, in comparison, a lull in the long period 1861-1877 (but with two exceptional years 1867 and 1871).
In Geometry there is a long period of continuous extensive output between 1857-1883 and in the Theory of Functions, 1871-1887.

Chart II

The upper graph indicates Cayley's total mathematical production by simple page count from 1840-1895. The lower graph indicates Cayley's annual publication by page count in European (French, German and Italian) and American journals.

To reduce the effect of time lag in publication, the actual page count has been replaced by a simple two point moving average; the number of pages in year n (shown in Chart II) is the arithmetic average of page count of work published in year n with that of year n + 1.

Cayley's voluminous production amounts to an average of

153 pages a year

taken over a period of 55 years. This is a lower bound in view of the comments made in (a), (b) and (c) above. Neither does this computed average take into account any of Cayley's verbal communications or deficiencies in the completeness of the *Collected Mathematical Papers*.

The average was consistently exceeded in his most productive years from 1856 to 1878. During this period, Cayley was between the ages of

35 and 57. There are two noticeable declines. The first decline occurred in 1863, the year of his appointment to the Sadleirian Chair at Cambridge. The second decline was in 1874. The drop in publication in this year, which under the averaging procedure is influenced by the 1875 production figure, coincides with the preparation period for his Treatise on Elliptic Functions [1876a].

Between 1855-1878 Cayley's production in foreign journals decreased in proportion to his total output. After 1878 the increase in proportion is accounted for by his many contibutions to the newly founded American Journal of Mathematics.

TABLE 5

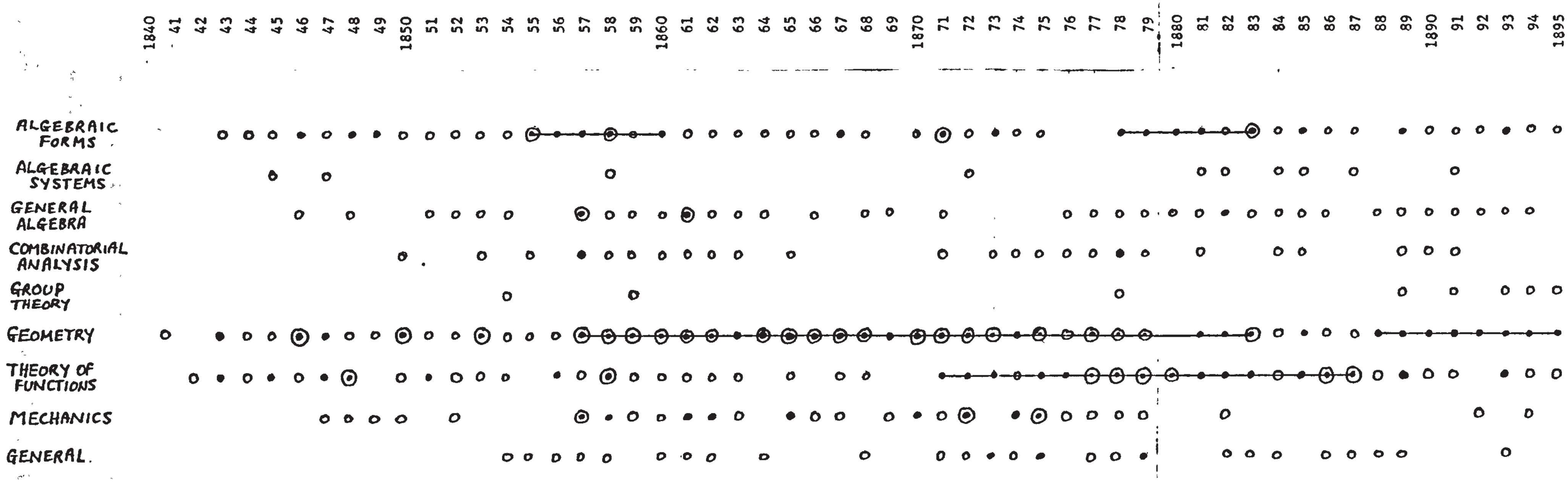


CHART I CHRONOLOGY OF CAYLEY'S MATHEMATICAL INTERESTS
1840-1895

TABLE 6

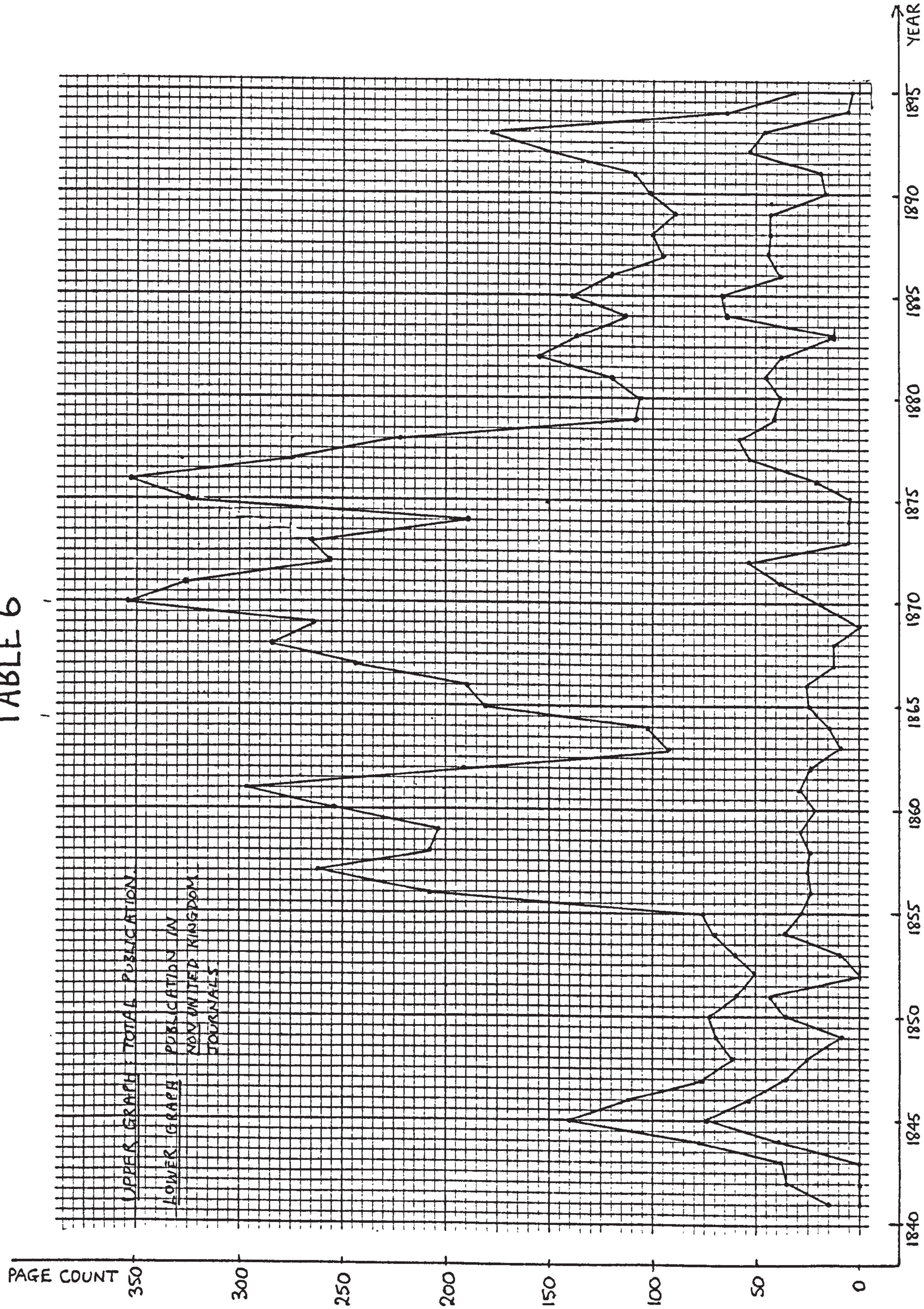


CHART II PROFILE OF CAYLEY'S MATHEMATICAL PRODUCTION 1840 - 1895

APPENDIX B

CORRESPONDENCE BETWEEN ARTHUR CAYLEY AND JAMES JOSEPH SYLVESTER

APPENDIX B

Correspondence between Arthur Cayley and James Joseph Sylvester St. Johns College, Cambridge

The Sylvester side of the Correspondence, (apart from a few exceptions) was received by St. Johns College on the death of Cayley's widow in 1923. On this side of the Correspondence, there are some five hundred items consisting of letters, notes and scraps written by Sylvester to Cayley. The correspondence began in the days when both were young men studying for the Bar and ended two years short of Cayley's death in 1895. The contents of these letters cover mathematical work on such topics as the Theory of Groups, Theory of Partitions, matrices, geometrical problems and above all, the Theory of Invariants.

The Cayley side of the Correspondence found its way to St. Johns College as part of Sylvester's notes and papers after Sylvester's death in 1897. Sylvester had earlier been a student at St. Johns College. This side of the Correspondence is much smaller than the Sylvester side. One reason for this is that Sylvester travelled extensively and was less organised in his affairs than Cayley. There are some sixty letters on this side of the Correspondence.

Thus the notion of a dialogue implicit in the word 'correspondence' is unfortunately missing. There are few occasions where the letters actually link together. (One such occasion is illustrated by Plates 4., 5 and 6).

On many occasions Cayley is silent and the Correspondence from which we have quoted extensively, often says more about Sylvester than his partner. However, the extent to which Sylvester depended on Cayley for personal support and encouragement is well illustrated.

The impression given by the fine portrait of Sylvester written by Fabian Franklin [1897a], is borne out on close examination of the Correspondence. Sylvester's bouts of depression were coupled with brilliant periods of unbridled enthusiasm for mathematics. Cayley, shy and retiring, is the ideal complement.

Sylvester even quarrelled with his friend. This is seen on a few occasions in the letters, but we are told by Franklin that Cayley merely left them unanswered until the one-sided quarrel was forgotten.

But in no sense was their working relationship a collaboration in the sense of Hardy and Littlewood. Sylvester was very much aware of the narrow line between actually having an idea and being recognised as its author. The seal of priority was publication and this criterion may well account for the speed which both Cayley and Sylvester committed their ideas to print.

Cayley's letters and papers were sent to Professor W. W. R. Ball at Trinity College, Cambridge on the death of Cayley's widow in 1923.[1] The vast quantity of material (one tin trunk and two brown parcels) comprised rough mathematical notes, draft manuscripts of memoirs and an extensive collection of letters that Cayley had accumulated during his long career. According to Rouse-Ball, much of

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[1]Mrs. Susan Cayley died 27.v.1923. There were two children: Mary (died unmarried, 14.vi.1950) and Henry (died 22.viii.1949 without issue). Henry Cayley J.P., A.R.I.B.A. (1870-1949) graduated (24th Wrangler) from Cambridge in 1890.

the correspondence dealt with the preparation of mathematical papers for publication and consisted of trivial matters such as the correction of misprints and the removal of ambiguities.

The letters received from deceased mathematicians were destroyed and those from living mathematicians were returned to their authors. The rough drafts which Cayley used in the preparation of his memoirs were destroyed except for a handful thought worth saving. These were sent to mathematicians thought to be interested in having a memento of Cayley and a list of these mathematicians is kept at Trinity College. (Wren Library, Add. ms. 0.6.6).

The Catalogue List

The distribution of the letters between 1847 and 1894 is extremely uneven. There are very many between 1883-1885 for instance, but comparatively few in the early years of their partnership. Many letters are without a date. In some cases it has been possible to assign an approximate date. When this has not been possible and this has especially been the case with fragments, information about these has been placed at the end of the Catalogue. In these cases an approximate date has been suggested based on the subject content of the fragment as well as other clues.

One final word of caution is needed for intending readers of this Correspondence. The letters are difficult to understand. One obstacle in the way of the modern reader is fathoming the meaning of Cayley and Sylvester's terminology. This private language was sometimes introduced only to be forgotten in the next moment. Another source of diffculty is Sylvester's desperately poor handwriting.

Notes on Catalogue Entries

First Column	date of letter
Second Column	'<u>CAYLEY</u>' indicates letter written by Cayley <u>to</u> Sylvester
	'<u>blank</u>' means a letter written by Sylvester to Cayley
Middle Column	round parentheses enclose <u>key words found in letter</u>.
	<u>x.p</u> indicates x pages in letter.
	square parentheses encloses a brief editorial comment.

1847

24 Nov	(reproductive, recurring equations, Fermat's Theorem, Eisenstein's formula) 4.p.
24 Nov	(rational functions, Legendre) 2.p.

1849

18 Apr	(differential equation) 4.p.
20 Nov	(surfaces of 3rd order) 6.p.
28 Nov	(hyperplanes, hypersurfaces, finite difference equations, involutes, deplars, determinant, conics) 16.p.
21 Dec	(equal roots, Sturm functions, conic test theorem, ellipse, hyperbola) 4.p.

1850

8 Jan	(personal note) 3.p.
3 Feb	(compound determinants) 4.p
7 Feb	(Collin's theorem) 3.p.
2 Mar	(rational functions, geometrical/ algebraic proofs, determinants) 8.p.
3 [Mar?]	(cubic, Aronhold's theorem) 5.p.
21 Mar	(multiplication of determinants, Cardinal theorem) 4.p.
27 Mar	(simultaneous transformation) 4.p.
5 Apr	(determinants, Jacobi, ultra-determinants, hyperdeterminants) 4.p.
20 Apr	(duplex transformation, cones) 4.p.
5 May	(theory of co-constituency) 4.p.
24 May	(intersection of conics) 8.p.
29 May	(contact of conics) 8.p.
[?] May	(double contact of conics, $\square U + \lambda V$) 3.p.
[?] May	(double contact) 4.p.

(1850)

18 Jun	(intersection of conics) 4.p.
26 Jun	(Pascal's theorem, extensions) 3.p.
29 Jun	(intersection of conics) 4.p.
3 Jun	(ditto) 3.p.
4 Sep	(elimination) 4.p.
29 Sep	(correction of proof sheets) 3.p.
25 Nov	(contact of conics) 4.p.
30 Nov	(ditto) 4.p.
5 Dec	(contact of conics, systems of curves general co-ordinates) 4.p.
19 Dec	(curve of intersection of surfaces) 4.p.
21 Dec	(points of flexure) 4.p.
26 Dec	(general equation of 3rd deg, Boole's theorem) 4.p.

1851

8 Mar	(contact problem, matrix, Hessian determinant) 4.p.
20 Mar	(hyperdeterminants, compound determinants, priority) 4.p.
21 Mar	(twenty-seven lines) 4.p.
24 Mar	(personal note) 2.p.
25 Mar	(reciprocal of curve) 3.p.
22 Apr	(Aronhold's hyperdeterminant of 6th deg., cubic function, Hessian) 9.p.
[?]May	(calculations) 1.p. [fragment]
18 Jun	(derivatives of canonical sextic) 4.p.
10 Jul	(even degree forms) 4.p.
[?]Jul	(ditto) 4.p. [estimated date]
30 Jul	(transformation of ternary quadratic system) 4.p.

(1851)

12 Aug		(surface of 2nd order) 4.p.
25 Aug		(terms in mathematics) 4.p.
2 Sep		(transformation of partial differential coefficients) 4.p
1 Nov	CAYLEY	(determinants of matrix, Hessian) 2.p
5 Dec	CAYLEY	(partial differential operator and invariants) 1.p.
[Oct 1851]	CAYLEY	(canonisation of functions) 2.p. [estimated date]
[1851]	CAYLEY	(reciprocal polars) 2.p. [estimated date]
[1851]	CAYLEY	(Sylvester's law for number of invariants) 2.p. estimated date

1852

5 Feb	(orthogonal invariants) 4.p.
13 Feb	(ditto) 8.p.
3 Mar	(covariant, Divellent) 2.p.
8 Mar	(polar reciprocal) 4.p.
8 Mar	(emanants) 4.p.
9 Mar	(multiple points) 4.p.
13 Mar	(reciprocity) 4.p.
17 Mar	(matrix, quintic function) [incomplete letter] 4.p.
23 Mar	(canonizant) 4.p.
25 Mar	(ditto) 4.p.
26 Mar	(ditto) 10.p.
29 Mar	(personal) 3.p.
7 Apr	(covariant) 2.p.
11 Apr	(discriminant, partial differential equations) 8.p.
	(sextic, invariants of sextic) 8.p.

(1852)

15 Apr	(sextic, invariants of sextic) 4.p.
16 Apr	(quintic) 4.p.
19 Apr	(polar reciprocity) 8.p.
[undated]	(ditto)
4 May	(multiplicity of point) 3.p.
[undated]	(double point) 1.p. [fragment; estimated date]
19 May	(evectant-polar reciprocal of curve) 4.p.
5 Jun	(Ferrers) 2.p.
19 Jun	(Sturms theorem, Hermite) 1.p.
	(ditto) [date estimated]
	(ditto) [date estimated]
21 Jun	(personal) 2.p.
20 Aug	(binary groups) 8.p.
21 Sep	(determinants, rule for multiplication) 4.p.
30 Sep	(theorem on triangles) 1.p.
15 Oct	(8 spheres in pyramid) 4.p.
16 Oct	(resultant of three quadratics) 4.p.
	(dialytic extensions) 3.p. [estimated date]
[Oct]	(orthogonal invariants) 4.p. [estimated date]
[Oct]	(Hesse's method, polar reciprocal of cubic curves) 4.p. [estimated date]
[undated]	(Socio-gredience) 4.p. [estimated date]
18 Oct	(Invariants of a system) 3.p.
26 Oct	(pair of quadratic forms, reduction) 1.p.

(1852)

28 Oct		(Cayleyans, Jacobian, Hessian, contact of forms) 4.p.
15 Nov		(biquadratic systems) 4.p.
		(Salmon's theorem) 2.p. [estimated date]
		(Salmon's theorem, Resultants) 6.p. [estimated date]

1853

[Jan]	CAYLEY	(Bezoutian quadratic) 1.p. [estimated date]
[Jan]		(Bezoutian quadratic, Evection, Hessian) 4.p. [estimated date]
24 Jan		(quotient scale, emanants) 3.p. [letter incomplete]
24 Jan		(quotient method, Bezoutian quadratic) 6.p.

1854

[May]	CAYLEY	(covariants of a quintic) 1.p. [estimated date]
27 May	CAYLEY	(quintic, Hermite's covariant) 1.p.
4 Jun		(invariants?) 3.p.
12 Oct	CAYLEY	(Number of invariants of a quantic) 3.p.
[Oct]	CAYLEY	(finiteness of invariants) 4.p. [estimated date]
[Oct]	CAYLEY	(law for number of asyzygetic covariants) 5.p. [estimated date]
17 Nov		(differential operator)
2 Dec		(development of $\left(\frac{d}{dx}\right)^{r}\left(\frac{d}{dy}\right)^{s}$ joint investigations) 4.p.

1855

11 Apr		(roots of $x^r - px^{r-1} + \theta$) 3.p. [fragment]
15 Apr		(roots of polynomial) 1.p. [fragment]
19 Apr		(Euler, partition problem) 3.p.
13 Aug		(personal letter) 4.p.
8 Sep		(Elimination, Woolwich, Stokes) 4.p.

1856

4 Jan		(personal letter) 4.p.
22 Feb		(partitions, personal letter) 4.p. [incomplete]
26 Feb		(motion of projectile) 11.p.
13 May		(prime circulators, Euler's problem) 8.p
22 Jul		(personal) 1.p. [fragment]
25 Aug		(personal) 7.p.
30 Aug		(complex numbers, cubic equation) 4.p.
6 Sep		(transformation of cubic equation)8.p.
16 Sep		(ditto) 4.p.
10 Sep		(progress in cubic form)
		(cubic forms) [estimated date]
26 Sep		(invariants of cubic forms) 4.p.
2 Oct	CAYLEY	(cubic forms) 4.p.
10 Oct		(cubic forms) 4.p.
20 Oct		(ternary cubic) 4.p. [estimated date]
23 Oct		(ditto) 3.p.
31 Oct		(invariants) 4.p.
		(ditto, Plücker theorem) 3.p.
		(cubic forms) 2.p. [estimated date; fragment]

(1856)

31 Oct	CAYLEY	(curves of third order) 3.p.
10 Nov		(cubic substitutions) 1.p. [fragment]
25 Nov		(invariant, quintic derivative) 3.p. [estimated date]
26 Nov		(ditto, Plücker's points) 3.p.
10 Dec		(differential equations $E\phi = \psi ; \exists\psi = \phi$) 3.p.

1857

19 Mar	CAYLEY	(personal, journals) 3.p.
7 Jul		(Euler's formula, cubic forms) 6.p.
11 Jul		(Euler's method) 4.p.
21 Aug		(personal, Elliptic functions) 4.p.
24 Aug		(Elliptic functions) 1.p. [fragment]
14 Sep		(personal, Gnoll College) 3.p.
24 Sep		(Elliptic functions) 4.p.
25 Sep		(ditto) 4.p.
		(ditto) 4.p.
26 Sep		(ditto, Jacobi's theorem, Gnoll College)
19 Nov		(Cayley-Hamilton theorem) 3.p.

1858

25 Feb		(personal) 6.p.
		(partition prob) 3.p. [estimated date]
14 Apr		(personal) 2.p.
27 Sep		(Symmetric functions, Philosophical Magazine) 4.p.
28 Sep		(partitions for two or more equations) 4.p.
30 Sep		(compound partitions) 8.p.

(1858)

1	Oct	(compound partitions) 4.p.
3	Oct	(ditto) 13.p.
16	Oct	(ditto, proof of theorem) 4.p.
21	Oct	(compound partitions) 6.p.
22	Oct	(ditto) 4.p.
26	Oct	(ditto) 14.p.
28	Oct	(partition theory) 4.p.
		(ditto) 4.p.
29	Oct	(compound partitions) 6.p.
2	Nov	(ditto) 8.p.
3	Nov	(partitions and Elliptic functions) 3.p.
4	Nov	(partitions) 8.p.
5	Nov	(partitions, prime groups) 4.p.
7	Nov	(partitions) 8.p.
10	Nov	(ditto) [incomplete letter] 4.p.
25	Nov	(ditto) 4.p.
27	Nov	(ditto) 7.p.
28	Nov	(ditto) 4.p.
22	Dec	(personal) 4.p.

1859

22	Feb	(compound partitions, ternary system) 8.p.
5	Mar	(Serrets function of 6 letters) 4.p.
29	Mar	(ditto, higher waves) 4.p.
1	Apr	(denumerant) 4.p.
2	Apr	(classes of ternary systems) 12.p.
4	Apr	(function of 6 letters, substitutions

(1859)

(4 Apr)		leaving function unaltered) 4.p.
5 Apr		(ditto , groups) 8.p.
7 Apr		(tabulation of ternary linear systems) 4.p.
		(normal order) 4.p.
16 Apr		(morphology) 12.p.
24 Apr		(definitions in partition theory) 9.p
26 Apr		(ternary systems) 4.p.
1 May		(ditto, morphology) 20.p.
3 May		(normal order) 4.p.
12 May		(compound partition, probability) 3.p
13 May		(Heliclic line) 4.p. [incomplete fragment]
18 May		(ditto, normal orders, generating function) 4.p.
23 May		(ditto, generating functions) 8.p.
21 Jun		(matrix, minors) 1.p.
29 Jun		(forthcoming lectures on partitions) 2.p.
6 Jul		(model curves) 3.p.
1859/60	CAYLEY	(polygons, Euler's theorem) [estimated date] 3.p.
1858/60	CAYLEY	(closed curves, polygons) [estimated date] 3.p.

1860

11 Aug	CAYLEY	(groups, inscribed polygon)
16 Aug	CAYLEY	(group, quintic equation, group of 20 and 120)
17 Aug		(remarks on Cauchy's notation in groups) 4.p.
18 Aug	CAYLEY	(six valued function group of substitutions) 5.p.
31 Aug		(Theory of ordination) 4.p.

1861

3 Jan		(series) 4.p.
4 Jan		(Astronomy, Liouville) 4.p.
26 Feb		(quadratic residues) 3.p.
27 Feb	CAYLEY	(quadratic residues) 2.p.
28 Feb		(A Diophantine problem) 8.p.
11 Mar		(Lineo-linear system) 4.p.
		(Chasles) 4.p.
21 Mar		(hyperboloid) 3.p.
		(minors) 4.p.
		(determinant of torsion) 4.p.
		(statical group) 3.p.
18 Mar		(Walton) 2.p.
22 Mar		(tractors) 4.p.
24 Mar		(determinants and geometry, distances, spheres) 4.p.
26 Mar		(radius) 4.p.
28 Mar		(5 lines) 4.p.
		(compound determinants) 4.p.
		(grand torsion determinant) 4.p.
29 Mar		(tractors) 3.p.
		(5 lines) 4.p.
30 Mar		(theory of metharmony) 6.p.
		(three circles and fourth) 2.p.
		(circle paradox) 4.p.
31 Mar		(hyperboloid) 4.p.
1 Apr		(sixth line) 4.p.
		(ditto, cofilature) 4.p.
		(ditto) 4.p.

(1861)

[3?] Apr		(hyperboloid, tractors)
5 Apr		(A syzygy) 4.p.
10 Apr	CAYLEY	(double points, cusps, six lines)
[11?]Apr		(Miller, anharmonic ratio and crystals) 4.p.
15 Apr	CAYLEY	(double points, cusps)
18 Apr		(experiment, torsion) 4.p.
		(hyperboloids, degrees of contact)4.p
		(systems of tangents, 27 lines) 4.p.
19 Apr		(lines in involution, simpliciter)3.p
22 Apr		(hyperboloids, conjugation of lines and points) 4.p.
24 Apr		(co-ordinates on hyperboloid) 4.p.
25 Apr		(27 lines) 4.p.
		(triple tangent planes) 8.p.
[?]Apr		(27 lines)
30 Apr		(cuboids, hyperboloids) 4.p.
30 Apr		(ditto, involution) 3.p.
30 Apr		(ditto) 4.p.
[?]May		(binomial triads) 4.p.
1 May	CAYLEY	(tractors, hyperboloids) 4.p.
2 May	CAYLEY	(Metharmony, 6 lines) 4.p.
3 May		(mechanics) 4.p.
3 May	CAYLEY	(Metharmony, involution) 6.p.
5 May		(non-intersectors)
7 May		(ditto) 1.p. [fragment]
7 May	CAYLEY	(skew surfaces) 8.p.
9 May		(27 lines, personal) 4.p.
9 May	CAYLEY	(classification of skew quartics)

(1861)

9 May	CAYLEY	(cubic)
11 May		(rule of symmetry) 2.p.
11 May		(reciprocity theorem) 2.p.
11 May		(rule of intersections) 4.p.
13 May		(27 lines) 3.p.
14 May		(ditto) 4.p.
15 May		(priority) 6.p.
16 May		(tangent planes) 4.p.
		(ruled surfaces, priority) 3.p. [estimated date]
21 May		(roots of equation) 4.p.
26 May		(personal letter) 4.p.
26 May		(ditto) 4.p.
15 Jun		(Cayley and teaching) 4.p.
27 Jun		(Chasles and geometry) 4.p.
15 Jul		(tactic, Kirkman's problem) 4.p.
15 Jul		(Kirkman's problem) 2.p. [fragment]
	CAYLEY	(Kirkman's problem) 3.p. [estimated date]
26 Jul		(theorem on tactic) 8.p.
15 Aug		(substitution group, list, Kirkman's problem) 6.p.
17 Aug		(groups, tactic) 4.p.
21 Aug		(factor group) 4.p.
30 Aug		(hyper-Pfaffians) 4.p.
3 Nov	CAYLEY	(roots of equation, Lagranges theorem) 3.p.

1862

[undated]		("on mathematicians") 4.p. [estimated date, letter from Florence]

(1862)

26 Feb	(personal, hypercumulants) 4.p. [letter from Florence]
9 Mar	(continued fractions) 4.p. [letter from Florence]
11 Mar	(minimum sum of squares) 4.p.
19 Mar	(Italians and invariant theory) 4.p.
9 Apr	(approximation to line in space)4.p.
10 Apr	(minimum linear function) 4.p.
15 Apr	(ditto, volume of tetrahedron) 4.p.
23 Apr	(sum of squares) 4.p.
14 May	(tetrahedron theorem wrong) 2.p.
23 May	(personal) 3.p.
10 Jul	(personal) 3.p.
21 Sep	(Euler's constant) 3.p.
7 Nov	(on integration) 2.p.
	(on $\Delta^m (\frac{1}{x})$) 7.p.
	(roots of equation) 2.p.
20 Nov	(approximation methods, Gaussian quadrature, double determinants, Woolwich) 6.p.
21 Nov	(approximation of area under curve, Gauss) 8.p.
22 Nov	(ditto, invariant) 4.p.
30 Nov	(multiple integral) 7.p.
2 Dec	(theorem due to Cauchy) 2.p.
5 Dec	(symmetric functions) 4.p.
6 Dec	(ditto) 4.p.
7 Dec	(Canonizant) 3.p.
8 Dec	(quadrature) 5.p.
13 Dec	(ditto) 3.p.
14 Dec	(ditto) 4.p.

(1862)

16 Dec		(quadratic transformation duplex)4.p
21 Dec		(invariants, quadratic transformation) 3.p.
23 Dec		(quadrature, duplex, invariants)7.p.

1863

2 Jan		(duplex, quadratures) 4.p.
2 Jan		(duplex, transformation) [estimated year, postscript]
3 Jan		(personal, Bishop Colenso) 4.p.
8 Feb		(personal) 4.p.
14 Feb		(double determinants) 4.p.
27 Feb	CAYLEY	(compound determinants) 4.p.
27 Feb	CAYLEY	(condensed notation) 3.p. [fragment]
5 Mar		(bipartite functions) 4.p.
5 Mar		(personal) 2.p.
18 Mar		(deploid, counters) 3.p.
7 Mar		(determinant) 3.p.
14 Mar		(ditto) 4.p.
1 Apr		(personal, double determinant) 4.p.
6 Apr		(symbolic multiplication, double determinants) 8.p.
10 Apr		(ditto) 4.p.
15 Apr		(life tables) 4.p.
20 Apr		(deploid) 2.p.
25 Apr		(counters in boxes) 4.p.
[?] Apr		(diplars, umbral matrix) 7.p.
8 Aug	CAYLEY	(six cross diagonal planes, potential theory of perspective, umbilicus) 4.p.

1864		
15 Nov		(No. of (4m+2) invariants of a quintic) 4.p. [estimated year]
1865		
14 Mar	CAYLEY	(amphigenous surface) [London Mathematical Society Collection, D. M. Watson Library, University College, London]
1866		
2 Feb		(motion of body) 7.p
4 Feb		(ditto) 8.p.
	CAYLEY	(elliptic functions) [part of letter reproduced in SP2,591 ; estimated date]
23 Feb		(mechanics, Cayley's work) 4.p.
7 Feb		(mechanics) 4.p.
23 Mar		(curves in space) 7.p.
1 Nov		(personal, operators) 4.p.
5 Nov		(E operators) 3.p.
12 Nov		(ditto, properties of E operators) 3.p.
20 Nov	CAYLEY	(problem on forces for Educational Times) 2.p. [original held Columbia University, D. E. Smith Historical Collection]
	CAYLEY	(on matrices) [part of letter reprinted in SP2,576; estimated date]
24 Nov		(polycephalons, pertradantive systems matrices) 4.p.
25 Nov		(ditto) 8.p.
29 Nov		(ditto) 4.p.
29 Nov		(ditto) 4.p.
[?]Nov		(ditto) 4.p.
30 Nov		(ditto) 4.p.

(1866)

	CAYLEY	(proof of symbolic operator equation) [part of letter reprinted in SP2,611; estimated date]
3 Dec		$(\log(1+x))^n$ (ditto) 6.p.
7 Dec		(ditto) 4.p.
8 Dec		(ditto) 2.p.
[?] Dec		(ditto) 4.p.

1867

24 Feb		(cubic forms) 4.p.
28 Feb		(geometrical derivatives) 8.p.
		(ditto) 2.p.
4 Mar		(ditto) 4.p.
5 Mar		(ditto) 5.p.
7 Mar		(unicursal derivation) 2.p.
8 Mar		(cubics) 3.p.
8 Mar		(ditto) 3.p.
9 Mar		(ditto) 4.p.
		(ditto) 4.p.
		(ditto) 4.p.
[?] Mar		(ditto) 8.p.
10 Mar		(cubics, contact) 2.p.
11 Mar		(ditto) 4.p.
12 Mar		(Plücker) 2.p. [fragment]
14 Mar		(twenty-seven tangents) 4.p.
		(ditto) 4.p.
18 Mar		(derivation, general transformation) 4.p.
19 Mar		(Clebsch's theorem) 6.p.
		(involution) 3.p.

(1867)

26 Mar		(double determinants) 4.p. [fragment]
30 Mar		(residuals, Berlin prize) 4.p.
20 Apr		(probability question, Educational Times) 4.p.
20 May		(probability) 2.p.
27 May		(personal) 3.p.
14 Jun		(Clifford) 3.p.
15 Nov		(Curves, priority) 4.p.

1868

28 Feb		(formula of reduction) 4.p.
4 Mar		(personal) 3.p.
? Oct		(quantics of even degree, partitions) 4.p.
8 Oct	CAYLEY	(binary quantic) 4.p.
28 Oct		(quantics, cyclodes) 4.p.
5 Nov		(ditto) 4.p.
6 Nov		(ditto, partitions) 4.p.

1869

2 Mar		(Ω functions) 2.p.
21 [Jul?]		(mechanical problem, roots, singular cyclodes) 8.p.
25 May	CAYLEY	(single partitions, generating functions) 3.p. [fragment]

1873

23 Dec	CAYLEY	(Intercalation theory of $\phi(x)/f(x)$) 4.p.

1875

9 Jun		(set of equations) 4.p.
11 Jul		(Roberts 3-bar motion, Hart's case) 5.p.

(1875)

12 Jul		(Roberts 3-bar motion, Hart's case) 4.p.
13 Nov		(combinatorial theorem, matrix)4.p.
18 Nov		(ditto, synthemes) 6.p.
9 Dec		(existence of nodes unicursal curves rectangular matrix) 7.p.

1876

[1876?]	CAYLEY	(curves, linkwork) [estimated year: possibly 1875?]
20 Feb	CAYLEY	(3 bar link work) [estimated year; possibly 1875?]
20 Feb	CAYLEY	(3 bar link work) [estimated year; possibly 1875?]

1877

26 Jan		(symmetric functions, Taylor's theorem, Salmon's theorem) 5.p.
14 Mar	CAYLEY	(number of covariants, functions of two variables) 4.p.
23 Apr		(Gordan's theorem, invariants) 6.p.
9 May		(invariants, partitions, tenary forms) 8.p.
10 May		(cubic curve) 4.p.
11 May		(ditto) 4.p.
		(ditto) 2.p.
23 May		(covariants, contravariants) 2.p.
28 May		(tenary forms)
20 June		(generating functions) [address London] 5.p.
5 Aug		(ditto) 4.p.
30 Aug		(nebular theory) 4.p.
1 Sep		(linear quadratic nebulars) 4.p.

(1877)

4 Sep		(sum of squares problem) 4.p.
12 Sep		(orthogonal invariants) 3.p.
26 Sep		(numerators, Bezoutiants) [from Johns Hopkins University] 3.p.
12 Oct		(generating functions) 4.p.
5 Nov	CAYLEY	(expression for A^n, double functions elliptic functions)
6 Nov		(theorem on invariants) 4.p.
7 Nov		(ditto) 4.p.
24 Nov		(Babbage, invariants, covariants) 6p
30 Nov	CAYLEY	(link work for x^2) [original extant published in part. <u>American Journal of Mathematics</u> <u>1</u> (1878), 386]
20 Dec		(chemical theory, Gordan's theorem) 4.p.
21 Dec		(ditto) 4.p.
23 Dec		(ditto) 8.p.

1878

7 Feb	CAYLEY	(personal, covariants of quintic, elliptic functions) [reply to previous letter]
15 Jul		(Numerical Generating Function, calculation) [from England] 3.p.
19 Jul		(ditto, personal) 2.p.
21 Aug		(ditto) 4.p.
28 Aug		(ditto) 4.p. [incomplete]
28 Aug		(ditto) 4.p.
7 Sep		(ditto) 4.p.
12 Sep		(ditto, Clifford) 4.p.
13 Sep		(ditto, seventhic) 3.p.
13 Nov		(seventhic) 4.p.
28 Oct		(ditto) 4.p.

1879

11 Jan		(calculation of Numerical Generating Function) 2.p. [continuation from previous letter: original held in D. E. Smith Collection, Columbia University, New York; published in [Archibald, 1936a.]]
3 Mar	CAYLEY	(personal) [reply to previous letter]
16 Jun		(personal, ground forms) 4.p. [incomplete]
24 Jun		(double skew determinants) 4.p.
3 Jul		(Pfaffian) 4.p.
5 Jul		(persymmetrical matrix) 2.p.
12 Jul		(personal) 4.p.
13 Jul		(law for persymmetrical determinants) 4.p.
15 Jul		(Table of ground forms for $10^{\underline{c}}$)4.p.
25 Jul		(ternary biquadratics, Gordan) 4.p.

1880

24 Apr		(method of Tamisage, primitive solutions, graphs)
Summer	CAYLEY	(octic curves in space) [estimated year]
6 May		(cubic curve, mixed concomitants, Tyndall) 4.p.
11 May		(ditto) 3.p.
17 May		(ditto) 4.p.
19 May		(ditto, and operators) 4.p.
7 Jul		(Bachman, forms, elliptic functions) 4.p.
2 Aug		(roots of unity) 3.p.
2 Aug		(ditto) 2.p.
3 Aug		(ditto) 2.p.
3 Aug		(ditto) 4.p.

(1880)

4 Aug		(roots of unity) 4.p.
4 Aug		(ditto) 4.p.
3 Sep		(16 squares, Lucas, J. R. Young)3.p.
24 Dec	CAYLEY	(theory of equations) [estimated year]

1881

19 Jan		(roots of unity) 4.p. [continuation from previous letter]
23 Mar		(Gordan's Theorem, Franklin) 4.p.
26 Mar		(Fundamental Postulate) 4.p.
2 May		(personal, cubic curves) 2.p.
3 May		(personal, roots of unity, norms, students) 4.p.
12 May		(personal, American Mathematical Society) 8.p.
18 Jun		(personal) [from England] 3.p.
21 Jun		(octavic quantic) 4.p.
28 Jun		(Cayley's lectures at Johns Hopkins University) 3.p.
19 Jul		(non-existence of ground forms) 4.p.
23 Jul		(ditto) 4.p.

1882

15 Jun		(totient integral) 2.p.
3 Aug		(Oxford chair, matrices) 4.p.
6 Sep	CAYLEY	(British Association Address) [continuation of previous]
6 Sep		(subinvariants) 4.p.
14 Sep		(Gordan's Theorem, Fundamental Postulate disproved) 4.p.
6 Oct		(quintic, Gordan's Theorem not proved, matrices) 3.p.
22 Oct		(Tables of symmetric functions, Cayleys Law) 3.p.
25 Dec		(Clebsch theory, Partitions) 4.p.

1883

1 Feb		(number theory) 4.p.
2 Feb		(vulgar fractions) 3.p.
3 Feb		(number theory) 2.p.
12 Feb	CAYLEY	(disproof of Fundamental Postulate) 3.p.
16 Mar		(partitions, Durfee's square) 7.p.
10 Apr		(compound partitions, elliptic functions) 6.p
13 Apr		(seminvariants, partitions) 4.p.
26 May		(partitions, Fary series, "Überschiebung") 10.p.
10 Jul		(Oxford chair) 4.p.
31 Jul		(personal) 2.p.
3 Aug		(Cayley's British Association Address, ordinal and cardinal number) 4.p.
6 Aug		(MacMahon) 2.p.
9 Aug		(MacMahon's discovery) 4.p.
11 Aug	CAYLEY	(ditto) [continuation of previous]
22 Aug		(multiple algebra) [continuation of previous] 4.p.
24 Aug		(involution of matrices of 3rd order) 2.p.
25 Aug		(ditto) 4.p.
5 Sep		(involutant, matrices) 8.p. [estimated year]
6 Sep		(invariants and matrices) 2.p.
12 Sep		(involutants) 5.p.
12 Sep		(cubic matrices) 4.p. [estimated date]
[?]Sep		(involutants) 4.p. [estimated date]
13 Sep		(ditto, cubic matrices) 9.p.

(1883)

14 Sep	(matrices, involutants) 4.p.
21 Sep	(ditto) 4.p.
22 Sep	(British Association Address) 8.p.
22 Sep	(quaternions)
15 Oct	(multiple algebra) 2.p.
20 Oct	(multiple algebra) 4.p.
8 Nov	(multiple algebra, involutants) 2.p.
8 Nov No.2	(multiple algebra, perpetuants) 2.p.
13 Dec	(Oxford chair) 12.p.

1884

20 Jan	(quaternions and geometry) 4.p.
22 Jan	(committee on invariants) 4.p.
24 Jan	(Klein's appointment) 4.p.
29 Jan	(pure analytic geometry) 4.p.
30 Jan	(commutativity of matrices, nonions) 4.p.
3 Feb	(ditto) 6.p.
5 Feb	(ditto) 4.p.
[?]Feb	(ditto, latent roots, matrices in involution) 4.p. [estimated date]
2 Mar	(correspondence theorem) 4.p
7 Mar	(derogatory matrices) 3.p.
8 Mar	(square root of matrix) 6.p.
	(transformations, substitutions)3.p.
14 Mar	(quaternion equations) 4.p.
16 Mar	(matrix equations) 3.p.
24 Mar	(symmetric functions) 4.p.
26 Mar	(correspondence theorems) 3.p.
27 Mar	(symmetric functions, quaternions)4.p.

(1884)

28 Mar		(non-associative systems) 4.p
30 Mar		(MacMahon's Theorem, Gordan's theorem) 4.p.
31 Mar		(Gordan's Theorem) 4.p.
2 Apr		(ditto) 4.p.
		(Correspondence Theorem, subinvariants) 4.p. [year estimated]
8 Apr		(Solution of $px = q$ in quaternions)4.p
20 May		(Hamilton's equation) 4.p.
27 May		(Solution of $x^2+qx+r=0$ in quaternions) 8.p.
7 Jun		(quaternions) 4.p.
23 Jun		(quadratics and quaternions) 4.p.
25 Jun		(personal) 2.p.
30 Jun	CAYLEY	(matrix equations) [continuation of previous] 3.p. [estimated date]
11 Jul		(quaternions) 4.p.
		(matrices) 4.p.
11 Jul	CAYLEY	(quaternions $px=xq$) 3.p.[estimated date]
[12?] Jul		(linear equation in matrices)
		(ditto) 4.p.
		(ditto) 5.p.
17 Jul		(ditto) 4.p.
17 Jul		(ditto) 4.p.
		(ditto) 2.p.
		(ditto) 4.p.
		(ditto) 4.p.
18 Jul		(ditto) 3.p.
19 Jul		(ditto) 6.p.
20 Jul		(ditto) 4.p.

(1884)

23 Jul		(E,) 4.p.
24 Jul		(linear equations in matrices) 4.p.
[? Jul]		(linear equations in matrices) [fragment, estimated date]
5 Aug		(ditto) 7.p.
29 Aug		(Tait, Nivellator) 3.p.
2 Sep	CAYLEY	(qQ = Qq') 3.p.
3 Sep		(quaternions and matrices) 4.p.
		(ditto) 3.p.
4 Sep		(px=xq, Nivellator) 4.p.
7 Sep		(matrices, homogeneous co-ordinates) 4.p.
12 Sep		(ordinary algebra) 3.p.
21 Sep		(nullity defined) 4.p.
22 Sep		(derogatory matrices, Hamilton's equation) 4.p.
24 Sep		(Harriot's Law) 3.p.
		(nullity) 5.p.
26 Sep		(cubic surface) 3.p.
27 Sep		(ditto) 2.p.
30 Sep		(multiple quantity) 4.p.
15 Oct		(plan of inaugural lecture, equation in quaternions) 4.p. [from Paris]
16 Nov [sic]		(equations in multiple quantity) 4.p [from Paris]
19 Oct		(ditto) 4.p. [from Paris]
20 Oct	CAYLEY	(Möbius transf, Poincare, MØ=Øu) 3.p. [estimated year]
22 Oct		(equations with matrix coefficients) 4.p.
24 Oct		(matrices) 4.p.

(1884)

2 Nov	(analytical geometry, Poincaré groups, distances) 4.p.
8 Nov	(multiple quantity,) 4.p.

1885

16 Jan	(personal) 3.p.
19 Feb	(variable plane) 2.p.
22 Feb	(ditto) 2.p.
23 Feb	(lectures on matrices) 6.p.
9 Mar	(perpendiculars) 4.p.
10 Mar	(partitions) 4.p.
15 Mar	(variable plane) 4.p.
16 Mar	(ditto) 2.p.
23 Mar	(ditto, derived partitions) 4.p.
28 Mar	(homological matrix, nullity, vacuity) 4.p.
31 Mar	(distances, bordered determinant)4.p.
2 Apr	(cycles, determinants, norms) 4.p.
6 Apr	(personal) 2.p.
9 Apr	(determinant) 3.p.
14 Apr	(distance, MacMahon) 4.p.
16 Apr	(cosine rule) 4.p.
17 Apr	(ditto) 4.p.
18 Apr	(distance) 4.p.
22 Apr	(Points in space) 6.p.
28 Apr	(niveau, homaloid, flat) 3.p.
7 May	(seminvariants, matrices, MacMahon's result) 4.p.
11 May	(ditto) 4.p.
16 May	(Magnus theorem) 4.p.

(1885)

20 May	(semivariants, MacMahon's transformation) 4.p.
21 May	(geometry, Magnus theorem, Cayley's kinectic matrix, equations of congruity) 4.p. [Letter held at Trinity College, Cambridge, ref. 0.6.6[17]]
18 May	(quadric, plane) 3.p.
	(axis of homology) 4.p.
18 May	(higher plane curve) 2.p.
30 May	(septic equation) 4.p.
12 Jun	(homology, projective geometry) 7.p.
	(ditto) 1.p.
19 Jun	(homology) 7.p.
[Jun]	(equations of homology, split matrix) 4.p. [estimated date]
[Jun]	(homology) 4.p. [estimated date]
20 Jun	(homology) 4.p.
22 Jun	(geometry) 1.p. [fragment]
	(homology) 4.p.
23 Jun	(Klein for Royal Society)
2 Jul	(homography) 4.p.
[Jul]	(homography, matrix) 2.p. [estimated date]
[Jul]	(homology) 4.p. [estimated date]
4 [Jul?]	(Kempe)
12 Aug	(correlated co-ords, knots) 4.p.
15 Aug	(Littles knot theory) 3.p.
24 Oct	(Reciprocants) 4.p.
	(Reciprocants) 2.p.
25 Oct	(ditto) 4.p.
27 Oct	(ditto) 2.p.

(1885)

29 Oct		(non-linear substitutions) 4.p.
1 Nov		(MacMahon, Hammond for R.S.) 2.p.
1 Nov		(reciprocants) 4.p.
1 [Nov?]		(reciprocants) 4.p.
2 Nov		(ditto) 4.p.
2 Nov		(ditto) 2.p. [estimated year]
9 Nov		(ditto) 4.p.
28 Nov		(ditto) 4.p.

1886

1 Feb		(Gordan's Theorem, reciprocants)4.p.
18 Feb		(partial differential operators, Gordan's Theorem) 4.p.
20 Feb		(reciprocants) 4.p.
16 Jun		(ditto) 4.p.
13 Jul		(Inaugural lecture) 4.p.

1887

8 Nov	CAYLEY	(px = xq) 3.p.
	CAYLEY	(orthogonal matrices) 2.p.
16 Nov	CAYLEY	(Nivellator, matrices) 3.p.
19 Nov	CAYLEY	(Nivellator, matrices) 2.p.

1888

24 Feb		(combinatorial problem) 2.p. [fragments]

1891

13 Apr		(partitions) 2.p.
17 Apr		(prime numbers) 5.p.
19 Apr		(prime numbers) 4.p.

(1891)	
27 May	(prime factor theorem) 4.p.
11 Jun	(Poincaré) 4.p.
22 Jun	(Dirichlet's theorem) 8.p.
26 Jun	(Goldback-Euler theorem) 3.p.
27 Jun	(general Dirichlet's theorem) 4.p.
17 Jul	(arithmetical series,) 8.p.
27 Jul	(prime numbers) 4.p.
1892	
24 Jun	(British Association tables) 3.p.
26 Jun	(triangle of reference) 2.p.
4 Nov	(Sego's function) 3.p.
1893	
30 May	(tables, calculations) 3.p.
1 Jun	(tables) 3.p.
28 Sep	(trisection of 60°) 4.p.
30 Sep	(trisection of angle) 3.p.
2 Oct	(quadratic) 3.p.
4 Oct	(tables) 2.p.
1894	
1 Oct	(personal) (original held John Hay Library, Brown University, Providence Long Island, U.S.A.) (I am grateful to Dr. E. Koppelman for this reference)

Unplaced Cayley Letters with Estimated Dates

[May 1861?]	CAYLEY	(binomial coefficients) 4.p.
[1869?]	CAYLEY	(single partitions) 2.p. [fragment]
[1877?]	CAYLEY	(group on 4 letters) 4.p.

Unplaced Sylvester Letters with Estimated Dates

[1851]	(wave surface) 2.p. [fragment]
[1852?]	(sign of tetrahedron) 2.p.
[1861?]	(hyperbolas in involution) 4.p.
[1858?]	(post-script - self residual points) 2.p. [fragment]
4 May [1859?]	(morphs) 4.p.
[May 1859?]	(announcing solution of Compound Partition Problem) 2.p.
[1861?]	(personal) 4.p. [fragment]
[1861?]	(tractors) 2.p. [fragment]
[1877?]	(double skew determinants) 2.p.
[1877?]	(Sylvester's address, sextic) 1.p. [fragment]
	(Real Generating Function)
[1883?]	(MacMahon's theorem) 4.p.
[1884/5?]	(personal - Tait, Royal Medal) 4.p.
[1893?]	(personal) 2.p. [fragment]

APPENDIX C

BIBLIOGRAPHY OF UNPUBLISHED MANUSCRIPT SOURCES

APPENDIX C

Bibliography of Unpublished Manuscript Sources

This Appendix contains details of manuscripts and letters of Arthur Cayley which are cited in the main text and details of other letters which may be useful to historians of mathematics. The principal collections of Cayley letters known to be extant are the Cayley-Sylvester Correspondence (Appendix B), the Cayley to Boole Correspondence and the Cayley to Hirst Correspondence. Cayley's correspondence with other scientists and mathematicians is scattered widely. Below we give the locations of some holdings.

Cayley to J.C.Adams — Seventeen brief notes
Most are undated
One is dated 1860
One is dated 1872

Subject: Astronomy, Questions for the Smith's Prize Examination, Attraction of an Ellipsoid.
The letters are held at St.John's College, Cambridge.

Cayley to C. Borchardt — One letter
Dated 20th Sept 1856

Subject: Remarks on notation, matrices associated with quadratic forms. Letter held at Niedersächsische Staats-Und Universitätsbibliothek, Göttingen.

Cayley to George Boole — Thirty-four letters 1844-1863
Thirty letters 1844-1849
also 1854, 1861, 1863(two letters)

These letters provide unique insight to work carried out by Cayley and Boole during the early period of Invariant Theory.
Subject: Invariants, integrals, theory of attraction and potential.
The letters are held at Trinity College, Cambridge.

Cayley to Dr. Craig — Six letters 1885-1892
Craig was a mathematician at Johns Hopkins University, Baltimore, U.S.A.
Subject: Covering letters for Articles to be published in

American Journal of Mathematics.

The letters are held at Johns Hopkins University.

Cayley to Thomas Archer Hirst

Forty letters 1859-1888

Hirst was a geometer and one of the main forces in the establishment of the London Mathematical Society.

Subject: Miscellaneous.

The letters are held in the L.M.S. Collection at the D.M.S. Watson Library, University College, London.

Hirst, Thomas Archer (1830-1892)

Journals of Thomas Archer Hirst

Contains valuable information and insights into world of London mathematicians of the nineteenth century. Cayley and Sylvester are frequently mentioned.

5 Volumes:

Vol 1	-	1847 - 1850	
Vol 2	-	1850 - 1855	
Vol 3	-	1855 - 1863	
Vol 4	-	1863 - 1884	
Vol 5	-	1885 - 1892	includes Index

Original Journal held at Royal Institution of Great Britain, 21 Albermarle Street, London, England.

Cayley to D. Gilman Ten letters 1881 - 1884

D. Gilman was President of Johns Hopkins University.

Subject: Administrative matters connected with Cayley's stay at Johns Hopkins in 1882. Also offer of Chair to Cayley after Sylvester's resignation.

The letters are held at Johns Hopkins University.

Cayley to Felix Klein — Twenty-seven letters 1878-1894

Subject: Group theory, Savilian chair of geometry, Hilbert's Basis Theorem.

Letters held at Niedersächsische Staats Und Universitätsbibliothek, Göttingen.

Cayley to P.A.MacMahon — Approximately fifty notes (mid 1880s)

Subject: Seminvariants, Theory of Partitions

Letters held at St. John's College, Cambridge

Cayley to James Clerk Maxwell — Three letters

Subject: Cartesian Ovals

Held at D.E.Smith Historical Collection, Columbia University.

Subject: On dissertations 30 vii 187? (Add 7655/II,209)
7 x 1876 (Add 7655/II,120)

Held at Cambridge University Library, England.

Sir William Thomson to Cayley — Three letters written by Thomson

One letter 20 xii 1866

Subject: On mathematical education

Letter held at Trinity College, Cambridge (Wren Library)

Two letters:

20 iv 1868 (Add 7655/II,29)
5 xii 1871(Add 7655/II,54)

Subject: Geometrical questions

Letters held at Cambridge University Library, England.

APPENDIX D

BIBLIOGRAPHY OF PUBLISHED SOURCES

Appendix D

Bibliography of published works

Publications in the text are cited using the Harvard reference system. The preamble to this thesis (Reference System) contains details of how works are referred from the text to this Bibliography.

Abhyankar, S.S.
1976a. 'Historical ramblings in algebraic geometry.........' American Math.Mon., 83 (1976), 409-448.

Airy, Sir George Biddell (1801-1892)
1896a. (Ed. by his son, Wilfred Airy). Autobiography of Sir George Biddell Airy (1896, Cambridge). (includes Cayley-Airy correspondence 1867-68)

Aitken, Alexander Craig (1895-1967)
1962a. 'H.W.Turnbull (1885-1961)', Biog.Not. Fellows Royal Soc., 8 (1962), 149-158.

Archibald, Raymond Clare (1875-1955)
1936a. 'Unpublished Letters of James Joseph Sylvester and other new information concerning his Life and Work', Osiris,1(1936) 85-154.

Baker, Henry Frederick (1866-1956)
1908a. 'On the invariants of a binary quintic.......' Proc. London Math.Soc., (2) 6 (1908), 122-140.

1930a. 'Percy Alexander MacMahon', Journal of the London Math. Soc., 5 (1930), 307-320.

Ball, Walter William Rouse (1850-1925)
1889a. History of the Study of Mathematics at Cambridge, (1889, Cambridge).

1912a. 'The Cambridge School of Mathematics, Maths. Gaz., 6 (1912), 311-323.

1960a. A short account of the History of Mathematics, (1960, Dover Reprint, New York) (First Edition, 1888).

Basalla, G.
1970a. (Ed. with others) Victorian Science (1970, New York)

Bell, Eric Temple (1883-1960)
1945a. The Development of Mathematics (1945 2nd Edition, New York and London).

Boole, George (1815-1864)
1840a. 'Researches on the theory of analytical transformations with special application to the reduction of the general equation of the second order', Camb. Math.Journal, 2 (1840), 64-73.

1840b. 'Analytical Geometry' Camb.Math.Journal, 2 (1840), 179-188.

Boole, George (continued)

1841a. 'Exposition of a general theory of linear transformations', Camb. Math. Journal, 3 (1841), 1-20.

1841b. 'Exposition of a general theory of linear transformations', Camb. Math. Journal 3 (1841), 106-119.

1843a. 'On the transformation of multiple integrals', Camb. Math. Journal, 4(1843), 20-28.

1845a. 'Notes on linear transformations', Camb. and Dublin Math. Journal, 4(1845), 167-171.

1847a. The mathematical analysis of Logic (1847, Cambridge)

1851a. 'On the theory of linear transformations', Camb. and Dublin Math.Journal, 6(1851), 87-106.

1851b. 'On the reduction of the general equation of the nth degree', Camb. and Dublin Math. Journal, 6(1951), 106-113.

Bottazzini, Umberto

1980a. 'Algebraische Unterschungen', Historia Mathematica, 7(1980), 24-37.

Boyer, Carl B.

1956a. History of Analytic Geometry (1956, New York)

Brioschi, Francesco (1824-1897)

1895a. 'Notice sur Cayley', Bull. des Sciences Math., 19 (1895), 189-200.

Bristed, Charles Astor (1820-1874)

1852a. Five Years in an English University (1852, New York)

Brock, W.H.

1967a. (Editor) The Atomic Debates (1967, Leicester)

Buchheim, Arthur (1859-1888)

1885a. 'On the Theory of Matrices', Proc. London Math.Soc., 16(1885), 63-82.

Cajori, Florian (1859-1930)

1929a. A history of Mathematical Notations, 2 Volumes, (1929, London)

1980a. A History of Mathematics (1980, 3rd Edition, New York) (First Edition, 1893).

Campbell, Lewis and Garnett, William

1882a. The life of James Clerk Maxwell (1882, London)

Cayley, Arthur (1821-1895)

CP The Collected Mathematical Papers of Arthur Cayley, 13 volumes + Supplement, (1889-1898, Cambridge) = (1963, Reprint, New York). (Cayley edited volumes 1 - 7, 8 (pages 1 - 38); A.R.Forsyth edited volumes 8-13; Forsyth did not compile the Index in the Supplement).

1841a. 'On a Theorem in the Geometry of Position', Camb. Math. Journal, 2 (1841), 267-271; CPI, 1-4.

1843a. 'On a Theory of Determinants', Camb. Phil. Trans,, 8 (1843), 1-16; CPI 63-79.

1843b. 'Chapters in the Analytical Geometry.....' Camb. Math. Journal, 4 (1843), 119-127; CPI, 55-62.

1845a. 'Note sur deux Formules.....', Journal f. reine u. angewandte Math., 29 (1845), 54-57; CPI, 113-116.

1845b. 'On the Theory of Linear Transformations', Camb. Math. Journal, 4 (1845), 193-209; CPI 80-94.

1845c. 'On certain results relating to Quaternions', Phil. Mag., 26 (1845), 141-145; CPI, 123-126.

1845d. 'On Jacobi's Elliptic Functions, in reply to the Rev. B. Bronwin: and on Quaternions', Phil. Mag. 26 (1945), 208-211; CPI, 127.

1845e. 'On Algebraical Couples', Phil. Mag., 27 (1845), 38-40; CPI, 128-131.

1846a. 'On the Reduction of $du \div \sqrt{U}$ .....', Camb. Dublin Math. Journal, I (1946), 70-73; CPI, 224-227.

1846b. 'On Linear Transformations', Camb. Dublin Math. Journal, I (1846), 104-122; CPI 95-112.

1846c. 'Mémoire sur les Hyperdéterminants', Journal f. reine u. angewandte Math., 30 (1846), 1-37. (Translation of 1845a., 1946a.)

1847a. 'Note on a System of Imaginaries', Phil. Mag., 30 (1847), 257-258; CPI 301.

1847b. 'Recherches sur l'Élimination......' Journal f. reine u. angewandte Math., 34 (1847), 30-45; CPI, 337-351.

1847c. 'Note sur les Hyperdéterminants', Journal f. reine u. angewandte Math., 34 (1847), 148-152; CPI, 352-355.

1848a. 'Sur les Déterminants Gauché', Journal f. reine u. angewandte Math., 38 (1848), 93-96; CPI, 410-413.

1851a. 'Note sur la Théorie des Hyperdéterminants', Journal f. reine u. angewandte Math., 42 (1851), 368-371; CPI, 577-579.

1852a. 'On the Theory of Permutants', Camb. Dublin Math. Journal, 7 (1852), 40-51; CP2, 16-26.

1852b. 'Correction to the Postscript......', Camb. Dublin Math. Journal, 7 (1852), 97-98, CP2, 27.

1852c. 'Demonstration of a Theorem relating to the Products of Sums of Squares', Phil. Mag., 4 (1852), 515-519; CP2, 49-52.

1853a. "On a theorem for the development of a factorial' Phil.Mag., 6 (1853), 182-185; CP2, 98-101.

1853b. 'On the Rationalisation.......', Camb.Dublin Math. Journal, 8 (1853), 97-101; CP2, 40-44.

1854a. 'On the Theory of Groups.....' Phil.Mag., 7 (1854), 40-47; CP2, 123-130.

1854b. 'Nouvelles Recherches sur les Covariants', Journal f. reine u. angewandte Math., 47 (1854), 109-125; CP2, 164-178.

1854c. 'An Introductory Memoir on Quantics', Phil.Trans., 144 (1854), 244-258; CP2, 221;234.

1855a. 'Remarques sur la Notation des Fonctions Algébriques', Journal f. reine u. angewandte Math., 50 (1855), 282-285; CP2, 185-188.

1855b. 'Note sur les Covariants.....', Journal f. reine u. angewandte Math., 50 (1855), 285-287; CP2, 189-191.

1855c. 'Researches on the Partition of Numbers', Phil. Trans., 145 (1855), 127-140; CP2, 235-249.

1856a. 'A Second Memoir on Quantics', Phil. Trans., 146 (1856), 101 - 126; CP2, 250-275.

1856b. 'A Third Memoir on Quantics', Phil. Trans., 146 (1856), 627-647; CP2, 310-335.

1857a. 'A Memoir on the Symmetric Functions.....', Phil.Trans., 147 (1857), 489-496; CP2, 417-439.

1857b. 'Two letters on Cubic Forms', Quart. Math. Journal, 1 (1857), 85-87, 90-91; CP3, 9 - 12.

1857d. 'On the Theory of the Analytical Forms called Trees', Phil. Mag., 13 (1857), 172-176; CP3, 242-246.

1857f. 'Note sur la méthode d'élimination de Bézout', Journal f. reine u. angewandte Math., 53 (1857), 366-367; CP4, 38-39.

1858a. 'A Memoir on the Theory of Matrices', Phil. Trans., 148 (1959), 17-37; CP2, 475-496.

1858b. 'A Memoir on the Automorphic Linear Transformation of a Bipartite Quadric Function', Phil. Trans., 148 (1858), 39-46; CP2, 497-505.

1858c. 'Supplementary Researches on the Partition of Numbers', Phil. Trans., 148 (1858), 39-46; CP2, 506-512.

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