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Geometry at Cambridge, 1863–1940 ☆

June Barrow-Green, Jeremy Gray *

Centre for the History of the Mathematical Sciences, Faculty of Mathematics and Computing, The Open University, Milton Keynes, MK7 6AA, UK

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Abstract

This paper traces the ebbs and flows of the history of geometry at Cambridge from the time of Cayley to 1940, and therefore the arrival of a branch of modern mathematics in Great Britain. Cayley had little immediate influence, but projective geometry blossomed and then declined during the reign of H.F. Baker, and was revived by Hodge at the end of the period. We also consider the implications these developments have for the concept of a school in the history of mathematics.

Résumé

L'article retrace les hauts et le bas de l'histoire de la géométrie à Cambridge, du temps de Cayley jusqu'à 1940. Il s'agit donc de l'arrivée d'une branche de mathématiques modernes en Grande-Bretagne. Cayley n'avait pas beaucoup d'influence directe, mais la géométrie projective fut en grand essor, déclina sous le règne de H.F. Baker pour être ranimée par Hodge vers la fin de notre période. Nous considérons aussi brièvement l'effet que tous ces travaux avaient sur les autres universités du pays pendant ces 80 années.

MSC: 01A55; 01A60; 51A05; 53A55

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* Corresponding author.
E-mail address: j.j.gray@open.ac.uk (J. Gray).

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0. Introduction

This paper is the first thorough study of geometry at Cambridge in the period in question, and forms one outcome of a much larger project on mathematics in Britain that has recently been completed [Barrow-Green, 1996–1999]. As such, it contributes to the accumulating picture of specific, national mathematical communities.[1] Here we document the arrival and subsequent growth of a branch of modern mathematics in Great Britain. From some perspectives, geometry does not seem to have had the status of analysis in the period of Hardy and Littlewood [Rice and Wilson, 2003]. We find that in its day it was considered to be at least equally important, although less successful in strictly intellectual terms and less lasting in its influence.

The paper covers the period from 1863, when Cayley was appointed the first Sadleirian Professor, to 1940, not only because the Second World War temporarily changed everything, but also because by then a recognizably modern, professional situation was established. Between those years, developments at Cambridge fall more or less naturally into three periods that are reflected in the structure of this paper. First comes Cayley's time at Cambridge. We discuss his work and its significance, and attempt to evaluate his role within Cambridge. The careers of A.C. Dixon and Charlotte Angas Scott notwithstanding, Cayley had few if any pupils. The second period runs from the death of Cayley in 1895 to World War I. This period is marked by the appearance for the first time of a cluster of geometers, Ronald and Hilda Hudson and F.S. Macaulay being among the best remembered, and ends with the emergence of H.F. Baker as Cayley's natural successor as a geometer. There was at this time also a general concern to modernize Cambridge mathematics and institutionalize research, which culminated in the important reform of the syllabus of 1906. There was a new richness to Cambridge life by then, which is reflected in the work of Whitehead before he turned to the philosophy of mathematics and the treatments of special relativity given by the mathematicians Cunningham and Robb. The third period, from World War I to World War II, is highlighted by the existence of a group of international significance (Coxeter, Hodge, Roth, Todd, and others). This is the period of Baker's Saturday afternoon tea parties, in effect a high-powered research seminar, the first of its kind at Cambridge, and their influence is reflected in the strong hold of geometry on the prestigious Smith's Prizes.

A vigorous school of mathematics produces many people who, one might say, are invisible at research wavelengths but emit a teaching or an expository light. At Cambridge, in the middle period, J.H. Grace and W.H. Richmond were teachers of note, while those confining themselves to the elementary parts of advanced topics include such as A.B. Basset and C.M. Jessop. We also briefly consider the work of Cambridge-trained geometers outside Cambridge. But the primary focus of this paper is the emergence of a distinctive geometrical school at Cambridge itself; our discussion of the concept of school takes note of [Parshall, 2004].

Andrew Warwick's *Masters of Theory* is likely to be the definitive study of the training of mathematicians and the production of mathematical physics at Cambridge in the 19th and early 20th centuries. But it turns sharply towards physics in its concluding chapters, and offers little information about pure mathematics and pure mathematicians at Cambridge. This paper is offered as a short account of those who perhaps belonged among the pure mathematicians who referred scoffingly to the Cambridge tradition in applied mathematics as "water, gas and electricity" subjects [Warwick, 2003, p. 434].

[1] See, for example, [Parshall and Rice, 2002], the proceedings of a three-day symposium at the University of Virginia, 27 to 29 May 1999.

1. Cayley's Sadleirian Professorship, 1863–1895

Arthur Cayley was undoubtedly the leading British pure mathematician of his generation. His contributions to geometry illustrate his virtues as an original thinker and help to document the way British mathematicians gradually sought to improve the links with the mathematics of continental Europe, which they had for some time kept at a distance. His working practice also shows the limitations of his contribution to the life of pure mathematics at Cambridge.

Cayley came to Cambridge as a result of the reorganization of the terms of the will of Lady Sadleir, who had died in 1706. In her will she had made provision "for the full and clear explication and teaching of that part of mathematical knowledge called algebra" among the colleges of Cambridge. Lecturers were ultimately appointed in all the colleges, but by the 1860s it was felt that these arrangements had ceased to function adequately, and that her wishes would be better executed if the bequest was focused on a single, new professorship in mathematics, not necessarily in algebra. Cayley took up this position in 1863 and was to hold it for 32 years, until his death in 1895.

In those years, geometry had returned to the forefront of Cayley's many interests, and since geometry was central to mathematics at the time it is worth considering his contribution in some detail. Tony Crilly has profitably compared Cayley to a botanist.[2] Cayley was a discoverer of mathematical objects and a classifier of them. Thus, starting with his discovery in 1847 of the 27 lines on a cubic surface (Cayley discovered that there are lines, Salmon counted them), Cayley and other geometers across Europe soon found all the various different surfaces of the cubic species, and classified them by the nature of their singular points and the number of lines they carry. In common with most continental geometers of his generation, Cayley generally thought of geometry as complex projective geometry, so that he could ensure a close correspondence between the algebra and the geometry. In particular, he could hope to count the objects his algebra led him to discover, whenever they were finite in number. Like his contemporaries, he also worked generically. A result is true generically if it is true most of the time, but not necessarily all of the time. The 27 lines exist on nondegenerate cubic surfaces, but not, for example, on the cubic with equation $xyz = 0$. This highly degenerate cubic is made up of three planes and admits infinitely many lines. Cayley himself said that "[I] generally ignore special cases" [1863, p. 169].

It would not be useful to describe all of Cayley's work on geometry, so one example which illustrates the strengths and weaknesses of his method must suffice. In 1863 he had turned again to scrolls, which he had noticed a decade before would be worthy of study. A scroll, or ruled surface, is a surface in projective space generated by straight lines. Cayley defined a scroll in terms of three curves in space. Usually, but not always, these curves will be nonintersecting. Let us call these curves C_k, C_l, and C_m; they will be called the generators of the surface, and are assumed to be algebraic curves of degrees k, l, and m, respectively. The scroll consists of all the lines that meet each of these three curves. For example, if the three curves are skew lines, the scroll is a hyperboloid of one sheet. Cayley and Salmon found that there was now much to calculate. For example, given the degrees of the generating curves, the degree of the algebraic surface so obtained is generically $2klm$, but can be less if, for example, the surface intersects itself or if the generating curves have singular points.

In his paper of 1863 Cayley looked for the number of projectively different types of scroll of degree 4 (called quartic scrolls). The problem is far from trivial, as we learn from a subsequent memoir by

[2] In his forthcoming biography of Cayley, *Arthur Cayley: Mathematician Laureate of the Victorian Age*, to be published by Johns Hopkins University Press, and in several discussions with us about him.

Cayley [1869].[3] Here he found himself in disagreement with earlier claims of Chasles, who had found 14 types of quartic scrolls; Cayley could find only 8. When Cayley published his findings, Schwarz wrote to him and also published an article describing two more types of scroll that Cayley had dismissed as special cases. As Cayley's article was going to press, Cremona in Milan joined in with two more cases, bringing the total in the end up to 12 [Cremona, 1914–1917]. The involvement of Schwarz and Cremona shows two things: the slippery nature of generic reasoning; and the importance of the work Cayley was doing. The general manner of arguing that Cayley employed is noteworthy for another reason too: it is not a relentless march of calculation. Cayley's formidable powers of manipulating algebra were subordinate to a geometrical insight telling him what to calculate and how.

As for the significance of his botanist's eye, one memoir stands out [Cayley, 1871], in which Cayley stopped a programme of Clebsch's in its tracks. In the late 1860s Clebsch had turned to considering algebraic surfaces and had sought to generalize Riemann's concept of the genus of a curve in two ways, as a number defined in terms of projective features of the surface (the degree and nature of singularities) and in terms of the number of linearly independent integrands of double integrals the surface admits. Riemann had shown that for curves the genus was equal to the number of holomorphic integrands on the curve. Clebsch hoped that the genus of a surface of degree n, defined birationally as $\frac{1}{6}(n-1)(n-2) \times (n-3)$ minus various terms determined by the singularities of the surface, should equal the number of holomorphic integrands on the surface.

It was at this point that Cayley intervened. The number of integrands is certainly nonnegative. But when Cayley calculated the genus using Clebsch's formula for scrolls he found that it was negative. In fact, it was $-\frac{1}{2}(n-1)(n-2)+\delta+\kappa$, where δ is the number of double lines and κ the number of cuspidal lines the surface contains. It follows that the two numbers introduced by Clebsch for the genus are different. Cayley's proof was first for scrolls which are cones on a curve of degree n that have only cusps and double points. Then he indicated how a proof for more general scrolls should go, and finally, he conjectured the result that for any rational surface whatever the genus of the scroll is the negative of the genus or deficiency of the plane section.

One further example is worth mentioning, because it illustrates Cayley's range and his grasp of the connections between seemingly different parts of mathematics, in this case complex analysis and geometry. This is his reformulation of the study of Kummer's quartic surface with its 16 nodal points [Cayley, 1870]. The surface, as we shall see, was to be a touchstone for many of the developments in geometry in Cambridge and elsewhere. Kummer had discovered it while pursuing his interest in line geometry [Atzema, 1993, p. 127]. Cayley became interested in the surface and observed that a quartic surface may have any number of nodal points between 0 and 16, which was already known to be the maximum. This was the start of his attempt to classify all quartic surfaces, going beyond his earlier study of scrolls. The work was topical; Plücker had, for example, just produced 14 wooden models of quartic surfaces. Cayley's classification was thorough going and was to have an unexpected payoff in 1877. Cayley had by then been led to read the papers by Göpel and Rosenhain, which were valuable for their generalization of Jacobi's 4 theta functions of one variable to 16 theta functions of two variables, called double theta functions. Göpel had generalized Jacobi's identities connecting fourth powers of theta functions to 16 identities between the double theta functions, which could in turn be used to prove the identities Rosenhain had discovered between squares of the double theta functions. Cayley was led to compare the 16 identities

[3] See also [Loria, 1896, p. 82] and the references cited there.

with the 16 nodes on Kummer's quartic surface as Kummer had discussed it. He found that the Göpel and Rosenhain identities led naturally to a proof that the 16 squared double theta functions corresponded one to one to the nodes of Kummer's surface and that the squared double theta functions connected by a linear relation corresponded to nodes lying in the same singular tangent plane. Remarkably, Kummer had missed this connection entirely. Cayley published his results in the *Journal für Mathematik* and they excited wide interest, and have remained to this day the natural way in to the study of Kummer's quartic surface [Cayley, 1877].

These papers show very clearly that Cayley was making contributions of international quality in geometry, as the first English mathematician to do so since Newton. In the English context, they had a further significance, because they were intimately connected to his famous collaborations with Sylvester and Salmon. Sylvester's collaboration extended the range of Cayley's research, Salmon's made it more accessible. Cayley was also notable for the quality of his general expositions. On several occasions he wrote extensive memoirs on particular topics; one, for example, of just over 100 pages on the book by Clebsch and Gordan on the theory of Abelian functions [Cayley, 1882]. His contributions were of inestimable value. At a time when the British sorely needed to find out what was important in European mathematics, these memoirs not only were lucid, they were in English. Cayley was a living bridge to the Continent. However, in one respect Cayley was unfortunately typical of his European generation: Riemann's insights baffled him, as his muddled comparison of his philosophy of geometry with that of Riemann, given in the Presidential Address to the British Association for the Advancement of Science (BAAS) in 1883, shows [Cayley, 1883b]. Riemannian analysis and its deep connections to geometry were not to come to Britain until Baker brought them, with some help from Felix Klein, in the early 1890s.

An indication of how matters stood is given by Henrici's address to the BAAS in the same year, 1883, when he was President of the Mathematical and Physical Science section [Henrici, 1883]. Olaus Henrici was a German, with a Ph.D. from Heidelberg, under the supervision of Hesse, and another from Berlin, supervised by Weierstrass and Kronecker. In 1883 he was Professor of Applied Mathematics at University College, London. The tone of his address can be judged from what are almost its opening words: "Pure geometry seems to me to be of the greatest educational value, and almost indispensable in many applications; but it has scarcely ever been introduced at Cambridge, the centre of mathematics and mathematical education in England." He continued by documenting what there was not. Cayley and Sylvester were to be thought of primarily as algebraists, although they constantly make use of geometry. Thomas Hirst (see [Gardner and Wilson, 1993]), whom he regarded as a pupil of Steiner, was "The one English mathematician whose mathematical thought is purely geometrical." The great German geometers were seldom read, and even the work of Monge was little known.[4] The neglect of pure geometry, Henrici was willing to concede, had perhaps been of little consequence to the progress of science in England, but it had severed Cambridge from the practical needs of the nation. Applicable geometry had therefore, he said, been developed in numerous science classes coordinated at the Science and Art Department of South Kensington in London, now rivaled by the City and Guilds of London Institute. We are concerned in this paper with Cambridge, in all its isolation.

Cayley's undoubted successes as a research mathematician were not matched by his impact either as a teacher or as an organizer. His contributions to British and indeed Cambridge mathematical life were largely written. A number of witnesses, Baker, Forsyth, Glaisher, Roberts, and J.J. Thomson among

[4] Reasons for the poor reception of Monge's work are discussed in S. Lawrence's unpublished Open University Ph.D. thesis [Lawrence, 2001].

them, testify that he was not an inspiring lecturer or tutor, although he offered an abundance of good will (see, for example, [Thomson, 1936, p. 47]). He preferred to communicate in writing, and there was little more to be learned from Cayley than could be gained by reading his (admittedly voluminous and usually clear) papers. He had no students, in any strict sense of the term, although a number of people grouped themselves around him. Of these, A.R. Forsyth, his successor as Sadleirian Professor, is best regarded as an analyst, as is J.W.L. Glaisher, although Forsyth lectured on differential geometry. A.C. Dixon, after a brief career as a geometer, switched to the study of dynamics, and then did interesting work on automorphic functions. Although Charlotte Scott always spoke warmly of Cayley, she too seems to have matured only distantly under his influence. All of which points up the absence in the Cambridge even of the 1890s of an institutional structure funnelling good students to tutors and assisting with their training.

Cayley did, however, exert a benign influence on the delicate matter of training for research. He supported moves to transform the Smith's Prize from a set of examination papers spread over four days (one for each professor) and taken by the brightest candidates immediately after the Tripos to a system where the Smith's prizes are "to be awarded annually to two candidates who shall present the essays of greatest merit on any subject in mathematics or natural philosophy." The prizes were awarded for the first time under the new system in 1885. The timetable ensured that candidates would be able to begin independent research, and indeed several successful entries are closely tied to subsequent publications. The intention was to push good mathematicians to do research of publishable quality, and away, it need hardly be said, from the slick superficialities of the Cambridge Tripos exam.

The balance between mathematics and natural philosophy, and the balance between different parts of mathematics within the Smith's Prize, has been made the subject of a separate and detailed study (see [Barrow-Green, 1999]) but it is worth noting how the successful pure mathematicians related to Cayley. Robert Lachlan won a Smith's Prize in 1885 for his essay *On systems of circles*. Berry (who won in 1887), Baker (1889), and Brunyate (1890) wrote on invariant theory; Segar (1892) wrote on algebra, and shared the prize with Bennett, who wrote on number theory. Dixon (1888) wrote on the connection between elliptic function theory and geometry, Manley (1895) on conformal representation.[5] All of these were strongly Cayleyan topics ([Cayley, 1883a] is a long memoir on the hypergeometric equation and the work of Schwarz). Cayley was one of the founders of the theory of forms, and the subject was a staple of Forsyth's university lectures.[6] Not surprisingly, bright young men at Cambridge beginning their research careers were drawn strongly to topics that Cayley had opened up. Only Bennett on number theory was at all remote from Cayley's research; the topic was nearer to Glaisher's interests.

Cayley's already indirect influence did not spill over into the undergraduate syllabus. The lecture lists for 1885–1895 reveal no university lectures on geometry at all, except for those given in 1888/1889 and 1891/1892 by Richard Pendlebury at St John's on projective geometry. Geometry was slightly more active

5 Baker's essay appeared as [Baker, 1889]. The titles of the other essays are listed in [Barrow-Green, 1999, pp. 308–309].

6 For much of the 19th century the responsibility for providing lectures in mathematics rested almost exclusively with the individual colleges, the only public lectures in mathematics within the university being those of the professors. From 1875 the public offering was increased through the informal establishment of courses of intercollegiate lectures which were open (for a fee) to students of all colleges. In 1883, as a result of Parliamentary reform, five new university lectureships in mathematics were established, with Forsyth as one of the first appointees; and with the new lectureships came a general coordination of mathematical teaching throughout the university. In 1926 the teaching was further consolidated by the creation of many new university lectureships with 16 appointments being made in that year alone.

in the larger colleges. At St John's Charles Taylor lectured every year on "Higher plane curves,"[7] and starting in 1890/1891 H.F. Baker gave lectures on various connections between analysis and the theory of curves. At Trinity W.W. Rouse Ball lectured in 1884/1885 on "Solid geometry" and in 1885/1886 on "The application of differential calculus to geometry;" Ball's obituarist [Whittaker, 1925, p. 449] said of him that he "cherished hope of making Trinity the chief center of mathematical discovery," but this ambition evidently did not extend to promoting geometry to the university at large. In 1891/1892 Arthur Berry began to lecture on pure geometry at King's College. Berry had graduated Senior Wrangler in 1885, and in 1887 studied at Göttingen; he was also a strong supporter of the movement for degrees for women and was for a time a staff tutor at the recently founded women's college, Girton.

The university situation for geometry improved a little in the 1890s. In 1891/1892 Robert Lachlan at Trinity began to lecture on "Pure geometry; projective geometry and higher analytical geometry," and he was joined in 1893 by Bennett at St John's and Whitehead at Trinity. Bennett lectured on "Projective geometry" and Whitehead more originally on "Absolute geometry (non-Euclidean)." Lachlan's comments in the preface to his rather undemanding book of 1893 are, however, revealing. He wrote (see [Lachlan, 1893, p. v]): "Hitherto the study of Pure Geometry has been neglected; chiefly, no doubt, because questions bearing on the subject have very rarely been set in examination papers. In the new regulations for the Cambridge Tripos, however, provision has been made for the introduction of a paper on 'Pure Geometry; — namely, Euclid; simple properties of lines and circles; inversion; the elementary properties of conic sections treated geometrically, not excluding the method of projections; harmonic properties; curvature.' ... I have endeavoured to treat every branch of the subject as completely as possible in the hope that a larger number of students than at present may be induced to devote themselves to a science which deserves as much attention as any branch of Pure Mathematics."

If the period of Cayley's time at Cambridge ends with some degree of attention being paid to geometry as a research topic, but much less as an undergraduate subject, a measure of what might have been is afforded by the example of William Kingdon Clifford. His involvement with Riemannian ideas was profound, and his early death robbed Britain of a chance of a different development. It is well known that Clifford translated Riemann's *Habilitationsvortrag* on geometry into English [Riemann, 1873]. His provocative inferences about the nature of geometry, and the responses of more conservative mathematicians such as Cayley, have been described at length in [Richards, 1988]. Clifford is one of four mathematicians to have his name on an important geometrical corollary of the Riemann–Roch theorem. He made a valuable contribution to the idea of a canonical dissection of a Riemann surface that helped make this difficult idea more intuitive. Perhaps even more remarkable was his intimate acquaintance with Grassmann's *Ausdehnungslehre* [Grassmann, 1862], at a time when even German mathematicians generally knew little about it. He went so far as to find Hamilton's theory of quaternions there, a view that was to be much discussed when others came to it later. Other contributions were more orthodox; we shall mention only a few. In 1878, two years before Veronese, he proved that an algebraic curve of degree n lying in a projective space of suitably high dimension always lies in a subspace of dimension at most n (so, for example, conics are always plane curves). He had his own generalization of quaternions to biquaternions (they are not the same as Hamilton's) and he discussed the zero curvature geometry on a torus. This caught Klein's attention, and became known as the study of Clifford–Klein parallels or the

[7] Charles Taylor went up to St John's in 1858 and graduated 9th Wrangler in 1862. He published three books on elementary geometry and in 1881 became Master of St John's.

Clifford–Klein space problem. It was the novel mixture of locally Euclidean geometry with nontrivial topology that caused the excitement.

Clifford was also a spellbinding lecturer, both at the popular level and when discussing research. Forsyth said of him "Clifford, it is true, could lecture and enchant his audience: and yet even his lectures ranged about the threshold of the temple of mathematical knowledge and made no attempt to reveal the shrines of the sanctuary" [Forsyth, 1897, p. 542]. Speculation on what his influence might have been is idle, but his example seems to illustrate the general rule that the acceptance of Riemann's ideas was a generational matter, easier for beginners (for example, Poincaré and Picard) than for the established (Cayley or Hermite).

2. From 1895 to 1918

2.1. *Baker's early years at Cambridge*

The second period in this account is not dominated by a single figure. Rather, it sees a number of isolated but significant achievements, marked only at the end by Baker's first steps to becoming the head of a distinctive group of geometers.

Henry Frederick Baker enjoyed such a long and productive life at Cambridge that it is convenient to break an account of his career at the First World War. He went up to the University when he was 18, in 1884, and published his last book in 1946,[8] when he was 80; he died at the age of 89. He was one of four men who were bracketed Senior Wrangler in 1887 and became a fellow of St John's College the next year. As we have seen, he began research in the theory of invariants, mastering the Clebsch–Aronhold symbolism. College lecturing took up much of his time after 1890, but he found time for two visits to Göttingen, where he studied intensively with Klein and met, among others, Burkhardt, Gordan, and Hurwitz. He was by this time working on his book *Abelian Functions* [Baker, 1897], published in 1897, which occupied him for several years.

He regarded the topic of Abelian functions as the best guide "to the analytical developments of pure mathematics during the last seventy years," as the opening words of the Preface of that book say. He spelled out the implications of taking that journey in ways that make it clear how he had digested the influence of Cayley. "The methods of Riemann," he went on, "are provisionally regarded as fundamental; but precise references are given for all results assumed, and great pains have been taken ... to provide for alternative developments." This meant both "the arithmetical ideas introduced by Kronecker and by Dedekind and Weber [and] the quasi-geometrical ideas associated with the theory of adjoint polynomials"; in other words, Brill–Noether theory as presented by Clebsch and Gordan and by Noether. For the theory of theta functions Baker gave further references to [Noether, 1890] and papers by Klein and Burkhardt. But the chief influence he acknowledged was that of Klein. The manuscript had been read by A.E.H. Love of St John's (who became a leading exponent of elasticity theory), and by J. Harkness of Bryn Mawr, the author with F. Morley of the first American textbooks on complex function theory [Harkness and Morley, 1898, 2nd ed. 1925]. Forsyth was thanked for his generous interest throughout the composition of the book. Forsyth's *Theory of Functions* [Forsyth, 1893] had been published in 1893,

[8] [Baker, 1946] is on the group of symmetries of the 27 lines on a cubic surface, or, more precisely, the simple subgroup of index 2 and order 25,920.

and although there is a generous overlap, it may be said to end where Baker's book begins, with the theory of Riemann surfaces.

So far as the theory of algebraic curves or Riemann surfaces is concerned, Baker referred to all the leading schools. The ones he called quasi-geometrical (note the distancing prefix) and arithmetical existed to supply rigor generally agreed to be lacking in the original, Riemannian presentation. By nonetheless regarding Riemann's ideas as fundamental, albeit provisionally, Baker was siding with Klein in hoping for a rigorous treatment of them that would be closer to Riemann in spirit. Abelian functions and theta functions of several variables were much less well understood in the 1890s. The arithmetic school was more or less restricted to one variable, where the correspondence between variety and function field is exact. On the analytic side, Riemann's theory of theta functions had greatly extended the ideas of Göpel and Rosenhain, who had only considered theta functions of two variables. In the hands of such as Krazer and Prym there was now an extensive, and forbidding, theory of the 2^{2p} theta functions associated with a Riemann surface of genus p. On the geometric side, as Clebsch and Weber had shown in the 1860s, Riemann's ideas had led to a good theory for dealing with curves; Clebsch, Cayley, and others had found applications in the theory of surfaces. Although a guide may, indeed must, leave some things out, it is not to be expected of a guide to the "analytical developments of pure mathematics during the last seventy years" that many of these topics would be left out, but Baker omitted the geometry of algebraic surfaces.

Baker restored the omission in his next book, *Multiply Periodic Functions* [1907]. There one meets the Kummer surface, a related quartic surface due to Weddle, and the topic of defective integrals. The topic of defective integrals plays two roles in the book. One is analytic, it contributes to the proof that every periodic function of several variables is a quotient of products of theta functions. Another is more geometrical, and exploits a theorem due to Poincaré [1886] concerning what are called defective integrals. As an illustration of what this means, consider this example of Baker's. A Riemann surface of genus three is rendered simply connected by a set of six suitably chosen closed curves on the surface, and generally any holomorphic 1-form on the surface will therefore have six periods (the six integrals of the 1-form on each closed curve). A 1-form (or integral) is said to be defective if some of these periods vanish. Poincaré's theory allowed one to deduce that if there is a defective integral on a Riemann surface of genus three then there is an integral with only two nonzero periods. Such an integral is an elliptic integral, and it follows that if a curve of genus 3 is defective it admits an elliptic integral. In the exceptional case when a basis of defective integrals can be found for the space of 1-forms, then every such integral is elliptic, and Baker showed in his book [pp. 265–269] that this occurs for Klein's curve of genus 3 associated with the famous group of order 168.[9] It seems that this result is original to him. As this example suggests, curves admitting defective integrals are often more than usually interesting geometrically. Baker gave several examples illustrating the following philosophy: take the curve obtained from a hyperelliptic surface, a Kummer surface, or a Weddle surface, in some natural way; show the curve admits defective integrals.

Baker here not only presented new results about familiar but important surfaces, he also propounded a way of studying them, using the full, Riemannian resources of Abelian function theory. As the period 1900–1914 proceeded, he found more and more illustrations, however, of the fecundity of direct geometrical and algebraic methods. He also set about acquainting himself with the much more potent techniques of the Italians, and in 1912 his Presidential Address to the London Mathematical Society was on the theory of algebraic surfaces [Baker, 1913]. The large and important subject of the birational theory

[9] The group is PSL(2; 7), the curve has projective equation $xy^3 + yz^3 + zx^3 = 0$. For a partial history of this famous object, see [Gray, 2000].

of surfaces has yet to receive its historian.[10] Indeed, Baker's historical account is still as good a place to start as any, and in its day was a splendid introduction. But it was not to be a subject on which Baker did any truly original work, and indeed its architects, Castelnuovo and Enriques, had in many ways finished their contributions too. The largely Italian and French edifice was in place, in time for the catastrophe of the First World War.

2.2. Other Cambridge researchers in geometry in the period after Cayley

Unlike Cayley, Baker was not alone as a geometer. However, as one of his contemporaries, J.H. Grace, was rightly to observe, "Cambridge geometers were never gregarious, and, in fact, it may be contended that the principal defect in pure mathematics, until quite recently, has been the want of effective co-operation between the erudition of riper years and the enthusiasm, the imagination, of youth" [Grace, 1916, p. lv]. Grace's remarks overall point up a disparity in Cambridge between the activities of the lone researcher and the teaching of geometry at Cambridge, and it seems best, therefore, to survey the achievements in research and teaching in this period separately.

The Cambridge geometer who in his day was regarded as the most gifted was Ronald W.H.T. Hudson. He was the son of W.H.H. Hudson, the professor of mathematics at King's College London, but his life ended in 1904 in a mountaineering accident when he was only 28. A brief account of his posthumously published book *Kummer's Quartic Surface* helps make precise some typical features of the Cambridge approach to geometry, and indicates the importance of the book. It opens with this observation: "The eight corners of a cube form a very simple configuration; yet by joining *alternate* corners by the diagonals of the faces we get two tetrahedra such that each edge of one meets two opposite edges of the other, and the figure possesses all the projective features of the most general pair of tetrahedra having this property."[11] If, with Hudson, we label the vertices of one tetrahedron $XYZT$ and the other tetrahedron suitably $PQRS$, then PS and QR meet both XT and YZ. Moreover, the tetrahedra are in fourfold perspective, for the lines PT, QZ, RY, SX are concurrent, at a point Hudson called P', and so on. So we have a configuration of 12 vertices lying in threes on 16 lines. Hudson noted that the study of tetrahedra arranged like this had been initiated by Stephanos, who noted that the tetrahedra belong to a pencil of quartic surfaces, the general member of which had been shown by Humbert to have 12 nodes [Humbert, 1891; Stephanos, 1879].

Here we see a simple configuration of points and lines, which might seem unpromising, connected to a question about a family of algebraic surfaces. Next Hudson described the symmetry group of the tetrahedra, and then he showed how to generate from it a configuration involving 16 different points and 16 different planes; he denoted such a figure by the symbol 16_6. He then investigated the symmetries of this configuration, and observed that the first person to prove that there are twelve 16_6 configurations that can be generated in this way was Weber ([Weber, 1878], followed by [Reye, 1878; Schroeter, 1886]).

Now Hudson was ready to introduce Kummer's quartic surface. It has 16 nodes, which will fit a 16_6 configuration, and Hudson described not only how to obtain an equation for the surface but also how to sketch its appearance. Later, Hudson showed that the tetrahedra and the 16_6 configuration occur together in the study of Kummer's quartic surface. He also worked through the classical theory of this surface, and looked at special cases, such as the wave surface, and a surface called Weddle's surface, which is

[10] For a glimpse of it, see [Gray, 1994] and the literature cited there.

[11] [Hudson, 1905, p. 1]. It is necessary to extend the edges, in this case to infinity.

birationally equivalent to Kummer's surface. Then, after a rather careful and slow account of the geometry of a space of four dimensions, Hudson showed that the 16_6 configuration arose by projection from a configuration of six points in a space of four dimensions. Projecting such a figure from an arbitrary point onto an arbitrary three-dimensional hyperplane produces a 15_6 of 15 points lying in sixes on 10 conics, and the 15_6 is the configuration of a 15-nodal quartic surface. A clever geometric argument then produced the (missing, as one might say) 16th node. Indeed, the whole quartic surface appeared as the intersection of a certain quartic variety with one of its tangent planes (the point of tangency being the 16th node). In this way, many properties of Kummer's quartic surface were produced from simple properties of an underlying figure in a way that minimized work with the equation of the surface itself.

Ronald's sister Hilda Hudson was another distinguished member of this mathematical family. Her mother and her sister studied mathematics at Newnham, as did Hilda between 1900 and 1903, when she graduated (bracketed equal with the 7th Wrangler). She stayed on at Newnham for a further year, then went to the University of Berlin in 1905, before returning as a lecturer in 1905. From 1910 to 1913 she was an Associate Research Fellow, and she was the only woman to give a paper at the International Congress of Mathematicians in Cambridge in 1912. She then spent a year at Bryn Mawr where Charlotte Scott was a professor, and most of the war years as a lecturer at the West Ham Technical Institute in London. In 1916 she was appointed on a grant from the Royal Society to work with Sir Ronald Ross, who had been awarded the Nobel Prize in 1902 for showing that the anopheles mosquito transmits malaria. She assisted him in the use of differential equations in epidemiological studies; they wrote several papers together later republished as a book [Ross and Hudson, 1931]. During the war she also wrote on aeronautical engineering. She was awarded the O.B.E. in 1919.

Hilda Hudson's geometric interests were in the theory of Cremona transformations. These are a class of transformations of the projective plane or of projective space. In the plane case, they are obtained from families of curves of a given, arbitrary degree, and they are used to disentangle the singular points of plane curves. It was a long-running theme of algebraic geometry in the period to show that any plane curve can be reduced to a curve in the plane having only a finite number of ordinary double points (or to a nonsingular curve in projective space).[12] Noether's theorem showed that every Cremona transformation of the plane is a product of quadratic transformations (where the curves used are given by quadratic equations).[13]

Hilda Hudson published nine articles on the subject between 1910 and 1913, and five more between 1924 and 1927, when she published the book on Cremona transformations on which her subsequent reputation rests. The book, which she dedicated to her brother, gives the definitive treatment in the plane case, and is notable for the thorough historical sketch she wrote in Ch. XVII. As she noted, the theory in three dimensions was much less understood, although Cremona transformations were therefore the natural candidate for resolving the singularities of algebraic surfaces. Cremona himself had given an example of a transformation defined by a family of quadratic surfaces, whose inverse involves cubic equations, so there is no theorem in dimension 3 analogous to Noether's theorem in the plane, and the structure of the group of Cremona transformations is very much harder to understand in this case.[14] Since there was no comprehensive theory to hand Hudson gave some general definitions and then turned to give

[12] For brief accounts of this, see [Gray, 1989; Walker, 1950].

[13] Noether's theorem was first proved in [Noether, 1870], with corrections and improvements by several authors, as Hudson [1927, p. 390] describes.

[14] Cremona's example is in his *Opere*, vol. 3, p. 309.

suggestive examples and particular cases, which she worked out in detail. She also pointed out how much more complicated things can get than her examples might imply.[15]

The only other Cambridge mathematician who made a lasting contribution to geometry in this period, and perhaps the most significant of all, was a marginal figure in Cambridge mathematical circles all his life. This was Francis Sowerby Macaulay, whose Cambridge Tract *Algebraic Theory of Modular Systems* [1916] was the first significant work on algebraic geometry in the spirit of Hilbert and Kronecker to be written in English.[16] He is appropriately remembered by algebraic geometers in the term Cohen–Macaulay ring (introduced by Zariski and Samuel in the second volume of [Zariski and Samuel, 1958]) and with Lasker is one of the few influences Emmy Noether cited for her own work; she had in mind [Lasker, 1905]. Macaulay's career makes him difficult to locate. He graduated from St Johns's as 8th Wrangler in 1882, obtained a DSc from London University in 1897 on the strength of his first paper [Macaulay, 1895], and from 1885 to 1911 taught mathematics at one of England's more ambitious schools, St Paul's School in London. J.E. Littlewood, G.N. Watson, and Ronald Hudson were among his pupils. He resigned in protest at being passed over as Head of the Mathematics Department, despite many successes.[17] During his time as a teacher he published steadily and was accordingly invited to address the 3rd International Congress of Mathematicians at Heidelberg in 1904, where he spoke on his work generalizing the Brill–Noether theory of the intersections of plane curves to higher dimensional varieties. This is certainly one of the few times a schoolteacher has been invited to present a paper at the Congresses.[18] In 1916 he published his Cambridge Tract *Algebraic Theory of Modular Systems* (republished with an essay by P. Roberts in 1994). After the war he settled in Cambridge and was a frequent attender at Baker's Saturday tea parties.

It is clear from Baker's somewhat perfunctory obituary of Macaulay [Baker, 1938] that he was by no means convinced that Macaulay's work was truly geometry. "In conversation," Baker wrote, "he was wont to regard the subject as a geometrical one; but ... many important differences ... do in fact disclose themselves ... It is doubtless true that the geometrical approach takes for granted many algebraic results of which the formal proof is at present incomplete ... gratitude is due to such as Macaulay, who seek to supply the evidence in a purely algebraic form. But up to now an intimate knowledge of the geometrical aspect seems a necessary part of the algebraist's equipment." This sense of distance between Baker and Macaulay may explain van der Waerden's reminiscence: "Most important work on the theory of polynomial ideals was done by ... Macauly [sic], a schoolmaster who lived near Cambridge, England, but who was nearly unknown to the Cambridge mathematicians when I visited Cambridge in 1933. I guess the importance of Macaulay's work was known only in Göttingen." [19] Except for one mention in volume V of Baker's *Principles of Geometry*, no Cambridge geometer seems to have cited Macaulay's major work. As we shall now see, Macaulay's papers are more solidly geometrical and more substantial than Baker thought in 1938. Baker's opinion may be ascribed in part to the fact that the papers were

[15] We are indebted to Miles Reid for the information that even 70 years later there still is much work to be done on Cremona transformations in space.

[16] See the forthcoming paper on Macaulay by Eisenbud and Gray.

[17] *The Pauline*, 29, 1911, 197–200, and *Governors' Minutes*, 1911, 21–23. We infer on other grounds that the retiring Head was Charles Pendlebury.

[18] Another Cambridge graduate who also spoke at the ICM was Robert W. Genese (8th Wrangler 1871), then at Aberystwyth, who gave papers at the third ICM, in Heidelberg [Genese, 1905] and at the fourth ICM, in Rome [Genese, 1909].

[19] [van der Waerden, 1971, p. 172].

written almost 40 years before he was asked to comment on them, at a time when Baker himself was more of an analyst.

Macaulay's early papers do not conform to Baker's few comments. They are indeed, as Baker said, "for the most part concerned with the knotty problem of the multiple points, and the intersections of, plane algebraic curves." And the writing is, as he said, "extremely acute." But this would seem to suggest a degree of narrowness, a suggestion supported by the further remark that Macaulay "would seem to have scorned to read all the foreign literature, especially the Italian, and was probably unaware of Bertini's paper."[20] In fact the problem of multiple points is a substantial one, which Macaulay pursued through nine papers written between 1895 and 1905.

At issue in those papers is the study of algebraic curves in the complex projective plane. Given two such curves, say C_m and C_n, of degrees m and n, respectively, by Bezout's theorem they meet in mn points. By the middle of the 19th century, it was widely believed that any curve through these points had an equation of the form $AF + BG = 0$, where the equations of the curves C_m and C_n are $F = 0$ and $G = 0$, respectively, and A and B are also polynomials in x and y. In an influential paper Noether [1873] had shown that this belief was false, notably when the given curves were tangent to each other or had singular points. The next year Brill and Noether [1874] had dealt with the question at length, and established the fundamental theorem in the subject, the Riemann–Roch theorem, which they were the first to call by this name. A related question concerns a single curve, say C_m, and k points on it, and asks what can be said about the curves of degree n that pass through these k points. What, for example, is the dimension of the family of all such curves? Cayley had tackled this question with his theory of postulation [1871], but his work had rightly been criticized by the German mathematician Bacharach, a student of Noether's (in [Bacharach, 1885]; see [Eisenbud et al., 1996]).[21]

The whole burden of Macaulay's papers is to extend the Brill–Noether Theorem to the most general setting and to prove the Riemann–Roch Theorem in that context, and his analysis raised the level of insight considerably. His criticisms of previous authors were trenchant, his remedies well argued, and contrary to Baker's remarks, his knowledge of contemporary literature was extensive (Bertini and Castelnuovo seem to have been significant influences). Macaulay particularly acknowledged the influence of Charlotte Scott, and they seem to have enjoyed quite an extensive cooperation. She wrote a lengthy account of his work, incorporating her own insights, for the American audience (see [Scott, 1902]). When he came to the vexed topic of the Brill–Noether Theorem his references were numerous and his comments about them sharp. In Macaulay's [1900, p. 382n], for example, Brill and Noether themselves are judged to have given an incomplete proof and that for only the simple case, which was taken over uncritically in Clebsch–Lindemann but, said Macaulay, was reworked correctly by Picard and Simart in the second volume of their treatise [1906]. Throughout these papers, Macaulay refers to the problem as a geometrical one requiring a geometrical solution. So the subtleties of multiple points are to be unravelled, he suggested, by formulating the concept of an infinitesimal curve (whose branches, so to speak, exemplify the nature of the singularity). In this way he sought to re-present the conditions imposed on a curve by the requirement that it pass through a given multiple point as equivalent conditions imposed by a set of distinct points.

20 The paper referred to is [Bertini, 1894].

21 It was to be a long time before accurate statements and rigorous proofs were supplied, and there is still no thorough history of the matter. For now the reader may consult [Bliss, 1923; Eisenbud et al., 1996].

What might be called Macaulay's geometrical period ended in 1905 with his Heidelberg address, which discussed generalizing the Brill–Noether Theorem to curves in spaces of n dimensions, and offered a (flawed) version of the Riemann–Roch Theorem in that setting. In a footnote to the published paper Macaulay tells us that Brill and Noether informed him at the Congress of the recently published book by the Hungarian mathematician J. König popularizing Kronecker's much more algebraic approach [König, 1903]. This work seems to have made a deep impression on him, for, a few minor papers aside, and for whatever reason, Macaulay fell silent in 1905 and when he began again in 1913 he wrote in the language of what, following Kronecker and König he called modular systems (see [Macaulay, 1913]).[22] In modern terminology, a modular system is an ideal in a polynomial ring in n variables, and because Macaulay's later works, including his Cambridge Tract [1916], enter ever more deeply into the algebraic and even structural theory of polynomial rings, they therefore belong in another paper.[23]

2.3. Cambridge teachers of geometry in the period after Cayley

All this marginalized research activity naturally failed to create anything like a school of geometry at Cambridge. Nonetheless several mathematicians there published papers and books, lectured and supervised. Of these, J.H. Grace and H.W. Richmond were the most important teachers. Grace's lectures were described by Edge as having "been the most brilliant and inspiring of that era." Edge regarded Grace as "a geometer of striking originality and fertile imagination,"[24] and his considerable local significance should not be obscured by his minor record of publications. Indeed, that includes his book with A. Young, *The Algebra of Invariants* [Grace and Young, 1903], which introduced the German symbolic method to an English audience, described Hilbert's contribution, and gave numerous geometrical applications. He also wrote on line geometry for the *Encyclopaedia Britannica* [Grace, 1910]. H.W. Richmond seems to have been one of those not uncommon people whose success as an undergraduate and graduate, and facility at producing minor papers on a variety of topics, gave them a lifelong feeling that they had not accomplished what they might have done. To quote from a poignant letter he wrote to E.A. Milne in 1945:

> Realizing the extent to which I belong to a past generation, I am reminded of one fact that may be overlooked or forgotten, and which would certainly have emerged if ever you and I had settled down to a talk about geometry.
>
> The side of geometry that appealed to me and fascinated me (in my undergraduate days and after) was the algebraic geometry of Steiner, Hesse, Cayley and Salmon, geometry of cubic and quartic curves and surfaces worked out by straightforward methods of elementary algebra. This was not exhausted but was largely although worked out, as I discovered when I began to research; although a new field was opened up in geometry of four dimensions. But certain it is that the old methods have failed signally to solve the problems that lie in the natural line of advance from the achievements of the old Geometers whom I have named, it became more and more obvious, to my regret, that the methods I had hoped to exploit were out of date and must be superseded. A sad confession.[25]

22 On the line that leads from Kronecker through König to Macaulay, see [Gray, 1997].

23 See Eisenbud and Gray, forthcoming.

24 See the obituary of William Proctor Milne, [Edge and Ruse, 1969, p. 566], where it is explicitly stated that these are Edge's remarks.

25 Richmond to Milne, 24 Feb. 1945, in Bodleian, Ms Eng. Misc. b.429, 417.

As with Grace, Richmond's real achievement was with the unwritten life of mathematics, and his observations on the developments in geometry in his lifetime shed considerable light on the situation in Cambridge. As we shall see, there was to be progress by conventional means in the geometry of four dimensions, but otherwise progress was denied to those who could not adopt or invent radically new methods, an achievement chiefly due to Hodge at the very end of the period we are considering.

The other Cambridge mathematicians in this period are more minor. What is more significant about them than their research is their role; they exemplify the preeminent part Cambridge played in stocking the growing university departments around Britain. They naturally took with them something of Cambridge's view of pure mathematics, with a strong emphasis on real and complex analysis and a growing interest in geometry, usually projective geometry. This widely disseminated view was to remain the norm until the advent of the "new mathematics" in the early 1960s, when projective geometry and hydrodynamics were cut back to make way for Bourbaki. It was also carried beyond the British Isles: Room went to Australia in 1935, and Coxeter to Toronto in 1936.

Among them, G.T. Bennett was the Senior Wrangler of 1890, and therefore the one who was famously ranked below Philippa Fawcett. Percy MacMahon described him, in a letter to D'Arcy Thompson, as "the leading geometrician in this country." [26] In 1929 he became a Fellow of University College London, where he had originally studied back in 1886–1887. His work embraced algebra, number theory, geometry, mechanisms, and dynamics, and he lectured on algebraic line geometry and the geometrical analysis of mechanisms. He became an FRS in 1914. Charles Jessop went to Cambridge in 1882 and became a lecturer at Armstrong College, Newcastle (later the University of Newcastle)[27] in 1893, where he wrote two useful books, one on line geometry [Jessop, 1903] and one on singular quartic surfaces. The publication of Jessop's book on line geometry secured the subject a regular place in the lecturing syllabus; from 1903 until the War began, Bennett lectured on it, and then Grace and afterwards Richmond took it up until it fell from the syllabus in 1925.

Robert Lachlan's research interests peaked with his Smith's Prize in 1885, when he was a mathematics instructor at the Royal Naval College, Greenwich. He returned to Cambridge as a coach in 1899. W.P. Milne, who graduated 4th Wrangler in 1906 became mathematics master at Clifton College, Bristol, where he remained until 1919, when he took up the professorship of mathematics at the University of Leeds, a post he held until retirement in 1949. His interests were the projective geometry of curves and surfaces. E.H. Neville came second in the Tripos for 1907 (the last year in which the list was ordered) and promptly became a fellow of Trinity. He became a Smith's Prize winner in 1912 with an essay on differential geometry, and the next year visited India, where he sought out Ramanujan at Hardy's suggestion and was instrumental in persuading him to travel to Cambridge. After the War Neville went to University

[26] MacMahon to D'Arcy Thompson, 23 August 1923, University of St Andrews muniments, 23774.

[27] The first decade of the 20th century saw the first significant wave of expansion in British universities, with a number of new universities being formed from colleges previously under the aegis of the University of London, such as the Royal College of Science, which became Imperial College (1907), and University College, Bristol, which became Bristol University (1909). In the north of England the constituent colleges of the Victoria University, Manchester, were separated into the independent universities of Manchester (1903), Liverpool (1903), and Leeds (1904), while Armstrong College became a university college of Durham University in 1909 (before becoming Newcastle University in 1963). Initially, the mathematics departments in most of these new universities consisted of a single professor and one or two lecturers, and the number of students graduating or being awarded a higher degree in mathematics in any one year was correspondingly small, with sometimes only one or two students achieving single honours in mathematics. The departments gradually increased in size, although the hiatus caused by the First World War inevitably took its toll.

College, Reading, which became a University in 1929, and built up the Department of Mathematics there, of which he was Head until his retirement in 1954. He wrote several books of considerable erudition, notably his [1921], but scrupulous accuracy and attention to detail made them forbidding to read and of little influence, as the full title of his [1922] may indicate. Finally, we should mention G.B. Mathews, who graduated as Senior Wrangler in 1883, and after one year as a Fellow at St John's became professor of mathematics at University College of North Wales at Bangor. He returned to Cambridge as a university lecturer in 1896, where, from 1903 to 1912, he lectured on line geometry. He became an FRS in 1897, and was back in Bangor from 1906 to 1919. His career shows a determined attempt to respond to German achievements. In 1892 he wrote two articles on Dedekind's theory of ideals, making him one of the first to appreciate that theory in the British Isles. In 1894 he wrote *A Treatise on Bessel Functions* [Gray and Mathews, 1894], and in 1906 he wrote the 6th Cambridge Tract, *Algebraic Equations* [Mathews, 1906], which was on Galois theory in the manner of Weber's *Algebra*.

2.4. Projective geometry as a Cambridge subject

A picture of what Cambridge mathematicians in the period prior to the First World War took geometry to be can be obtained by contrasting the books by Mathews and Basset with the work of the more substantial figure Whitehead.

Mathews's book *Projective Geometry* was published in 1914 [Mathews, 1914]. He set himself the task of communicating with the beginner (one supposes the Cambridge undergraduates of his acquaintance), and to that end he was prepared to make compromises with rigor. He did not seek out the minimum number of primitive terms, for example, and he made assumptions about continuity that confined his geometry to real projective geometry. He relied heavily on the concept of duality, but he took his beginners a long way: as far as line geometry and projective metrics. He was pleased to have produced a book that did not assume the concept of distance, and to have given an exposition of the theory of complex points as originally expounded by Staudt [1856] and simplified by Lüroth [1875].[28] It was hardly a modern approach to the subject, and indeed, as he said, Mathews's greatest debt was to Henrici, from whom he first learned projective geometry when attending Henrici's lectures at University College in 1878–1879. Henrici, as he put it, "had first made me realise that mathematics is an inductive science, and not a set of rules and formulae."

Basset had graduated from Trinity as 13th Wrangler in 1877, whereupon he entered the law, from which he retired in 1887 when he succeeded to a considerable estate. As a man of private means he devoted himself to mathematical research. In 1888 he published a book on hydrodynamics, in 1892 a book on physical optics, in 1901 *An Elementary Treatise on Cubic and Quartic Curves*, and in 1910 *A Treatise on the Geometry of Surfaces* (respectively, [Basset, 1901, 1910]). He was in his time a Vice-President of the London Mathematical Society, and he became an FRS in 1889. He died in 1930.

He offered his *Treatise* of 1910 as an elementary work. In it he argued that abstaining from the theory of (complex) functions and the higher branches of modern algebra still enabled one to provide a toler-

[28] This theory is not devoted to explaining how points on an algebraic curve may have complex coordinates; rather it is devoted to explaining away such points in terms of symmetries of the curve. An involution (a map of the projective plane to itself of period 2, sometimes also called a projective reflection), which analytically has two complex conjugate fixed points on a curve, defines, according to this theory, two complex points. The theory of complex points is a theory of involutions with no real fixed points.

ably complete account of the theory of cubic and quartic surfaces, the point and plane singularities of surfaces, and the theory of twisted curves. If Cayley and Salmon now stood in the background, in need of modernization, the foreground was occupied by C. Segre and E. Pascal's *Repertorio*, vol. 2 [Pascal, 1898], itself an encyclopedic reference work.

After surveying the general features of algebraic surfaces, Basset looked at cubic surfaces, twisted curves and the developable surfaces they generate, and then the compound singular points of plane curves and surfaces. Then came chapters on quartic surfaces and scrolls, Cayley's theory of scrolls in general, residuation (the Cayley–Bacharach theorem), and finally singular planes tangent to surfaces. Considerable attention is paid to the compound singularities of plane curves and to the singularities of surfaces (whether points or curves). But the emphasis is placed on techniques for analyzing individual singularities; no attention is paid to the theoretically important task either of resolving the singularities entirely or of finding a birationally equivalent curve or surface having only normal singularities. As with the previous book, there is an abundance of examples. The result is a book that falls short of important goals facing theorists at this time, and offers instead a botanical profusion in the spirit of Cayley. It was reviewed in the journal *Nature* by Bromwich [1910], who regretted the lack of models in the book and found the discussion of singular points muddled. The resulting correspondence would seem to bear Bromwich out, and also revealed that Basset had gone into print without ever reading such authorities as Jordan [1909–1915, vol. 1, Ch. 15] and Zeuthen [1876] on the subject.

Whitehead's two books, by contrast, return us to the synthetic side of projective geometry, and they occupy a position sometimes passed over too quickly between the more substantial and overtly philosophical *Universal Algebra* of 1898 and his later work with Russell on *Principia Mathematica* [Russell and Whitehead, 1910–1913]. A.N. Whitehead had graduated 3rd Wrangler in 1883 (with Mathews) and won a Smith's Prize the next year with an essay on thermodynamics. His Fellowship dissertation (now lost) was on Maxwell's *Treatise on Electricity and Magnetism*, at the time the subject of W.D. Niven's obscure lectures.[29] As a result Whitehead was regarded as an applied mathematician at Cambridge and indeed he never forgot the connection between mathematics and physics. As his biographer, Victor Lowe, commented: "His persistent interest was in mathematical theory as applicable to the world." [Lowe, 1975, p. 100]. Whitehead spent 26 years as a Fellow of Trinity College before leaving in 1911 to go to University College London. He was the first at Cambridge to lecture on non-Euclidean geometry, which he did in 1893 and again in most years after 1898. In the opinion of Littlewood, Whitehead was an exciting lecturer on foundations of mechanics and the foundations of geometry,[30] and Russell said that "Whitehead was extraordinarily perfect as a teacher" [Lowe, 1975, p. 94]. In 1912 he moved first to University College London (where he succeeded Cunningham), then to Imperial College London, and eventually to Harvard.[31]

The most substantial of Whitehead's books written during his time at Cambridge is his *A Treatise on Universal Algebra* [1898a]. The book opens with a discussion of pure logic (Mill, Jevons, Lotze, and Bradley) and symbolic logic (Boole, Schröder, and Venn). Spoken and written words, along with mathematical symbols, are regarded as signs and categorized in the manner of G.F. Stout (a philosopher at St John's and editor of the philosophy and psychology journal *Mind*). An n-dimensional geometrical space without a distance concept is discussed, which Whitehead called a positional manifold. Then the

[29] On Maxwell and Cambridge, see [Warwick, 2003], especially Ch. 6.

[30] See [Littlewood, 1986, p. 88].

[31] On Whitehead's life and works, see [Lowe, 1990].

author turned to the main topic of the book: Grassmann's calculus of extension. Whitehead may well first have learned of Grassmann's ideas from Homersham Cox, who had used them to study non-Euclidean geometry in 1883. Whitehead applied them to descriptive geometry, the geometry of conics and cubics, and the theory of forces in three-dimensional space (studied by Lindemann and, in Cambridge, by Robert Ball). There followed a chapter on non-Euclidean geometry (which Whitehead said on p. ix, rather too naively, was "after Cayley, the most general theory of distance in a positional manifold"). The book ended with a chapter on Euclidean geometry and the theory of vectors applied to such topics as curvature, forces, and hydrodynamics. Because of its interest in logic, Stout sent the book to the Scottish logician J. MacColl to review for *Mind* [MacColl, 1899]; its range rather frightened him.

It was, however, less frightening in continental Europe. In 1900 the Physico-Mathematical Society of Kasan in Russia met to award its Lobachevskii prize for only the second time. The choice lay between Wilhelm Killing, for the second volume of his book *Einführung in die Grundlagen der Geometrie* [Killing, 1893–1898], and Whitehead for his *Universal Algebra* and his essay "The geodesic geometry of surfaces in non-Euclidean space" [Whitehead, 1898b], which was strongly recommended by R.S. Ball. Lie's former assistant, Friedrich Engel, argued for Killing, and according to him the matter was eventually decided by drawing lots: Killing won (see [Hawkins, 2000, p. 180]).

The extent to which Whitehead's book was exceptional even in the Cambridge context can be measured by the comments of his contemporary, W.B. Frankland, precisely because Frankland was not a leading researcher. He was surely right that it was not widely known in Britain that "the absolute truth of nearly all Euclid's system is in question" [Frankland, 1902, p. 14]. The book from which this remark is taken is Frankland's attempt to put matters right, and is a notable essay in the popularization of mathematics. *The Story of Euclid* was published in the series entitled The Library of Useful Knowledge, one of many such series in vogue then and since.[32] It covers a good deal of ground, much of it Greek, but reaching via Wallis, Saccheri and Lambert to Lobachevskii and Bolyai and finally to Riemann, but not Poincaré. The scholarship owes something to Tannery and Halsted, and allowing for the limitations of the genre it is a clear and generally accurate historical account.[33]

After the *Universal Algebra* Whitehead wrote two books exclusively on geometry. These are his two Cambridge Tracts, *The Axioms of Projective Geometry* [1906] and *The Axioms of Descriptive Geometry* [1907], and the shift from the largely backward-looking *Universal Algebra* to the much shorter and yet more modern Tracts is striking. Their titles might suggest that these books represent the arrival of Hilbertian axiomatic geometry at Cambridge, although the choice of descriptive geometry also points in a more applied direction. In fact, the dominant influence was that of the Italians, and one recalls that it was Peano's example that inspired Russell to take up mathematical logic. Other influences at least as marked as Hilbert's came from other German authors and also American writers, who in turn were more influenced by Hilbert. We shall concentrate on the Tract on projective geometry, which, in view of Whitehead's growing interest in philosophy, is worth looking at for the philosophical positions adopted.

The Tract on projective geometry gives an axiomatic approach in the manner of Pieri (Hilbert's name is mentioned less often), grouping the axioms according to the geometrical properties they formulate.[34] The introduction of numerical coordinates without reference to the concept of distance is regarded as

32 [Frankland, 1902].

33 Frankland graduated 3rd Wrangler in 1897 and became a Fellow of Clare College. His [1902] is based on his Smith's Prize essay of 1899, "The Theory of Parallelism" (later published as his [1910]), for which he had received an honorable mention.

34 The Tract on descriptive geometry, not to be discussed here, follows Veblen's system of axioms.

a fundamental idea; indeed "The establishment of this result is one of the triumphs of modern mathematical thought" [Whitehead, 1906, p. v]. Ideas of distance, congruence, and the existence of groups of congruence transformations were assigned to the Tract on descriptive geometry. Whitehead's view was that "Geometry ... is part of pure mathematics, and like all such sciences it is composed of Definitions, Axioms, Existence Theorems, and Deductions" [Whitehead, 1906, p. 1]. He regarded definitions as exclusively nominal; that is, as he put it, they assign a short name to a concept but form no essential part of the subject. An axiom is a propositional function (he referred to [Russell, 1903, Sects. 13 and 353]), so an axiom can be neither true nor false. An existence theorem may be deduced from purely logical premises—when it is a theorem of pure mathematics—or believed as an induction from experience—when it is a theorem of physical science. But then he undercut this austerity with the remark that "But if we abandon the strictly logical point of view, then definitions ... are at once seen to be the most important part of the subject" [Whitehead, 1906, pp. 2–3]. The view that existence in mathematics may be equated with consistency was disparaged as "a rash reliance on a particular philosophical doctrine of the creativity of the mind" [Whitehead, 1906, pp. 3–4]. Finally, he said that geometry is a department in the general science of classification; more precisely, geometry is the science of cross-classification. This rather unrecognizable view was illustrated with the remark that given a class of points, selected subclasses form the class of (straight) lines.

After this philosophizing, Whitehead turned to mathematics. He defined a plane in a way he claimed holds for every geometry: If A, B, and C are noncollinear points, then the plane ABC consists of all points X such that some line through X meets at least two of the lines AB, BC, CA not at A, B, or C. He then proceeded to give axioms for the projective plane, following Pieri's presentation in [1898]. Twelve axioms were put forward, of which the last is that any two lines meet. These permit the definition of a harmonic conjugate: D is the harmonic conjugate of B with respect to A and C if the four points are collinear and a complete quadrilateral can be found such that two pairs of opposite sides pass through A and C and two diagonals through B and D. Desargues's Theorem was then used to prove the uniqueness of the fourth harmonic point. Experts will know that there is a subtle point here (anticipated indeed by Desargues himself; see [Field and Gray, 1986]), as Hilbert had shown in the first edition of his *Grundlagen der Geometrie*, Desargues's Theorem does not follow from the basic axioms of plane projective geometry without extra assumptions, but it does follow automatically from the axioms of higher-dimensional projective geometry [Hilbert, 1899]. Whitehead proceeded by introducing axiom 13: there exists a point outside the plane. Then, following various authors,[35] he showed (as they had done) that without Axiom 13 one can have a projective space satisfying the first 12 axioms and the contradiction of Desargues's Theorem. So Desargues's Theorem is automatic in projective spaces of dimension 3 or more, but may be false in the plane.

Other properties of projective planes can be unexpected: the existence of a harmonic conjugate does not imply that A and C separate B and D. This had been noted by Fano [1892], using the finite projective space with 15 points. So Whitehead introduced Fano's axiom, by which time he had a system of 15 axioms, of which the last confined attention to three-dimensional spaces.

After spaces come transformations (in Ch. III). In Whitehead's view, the fundamental theorems of projective geometry are that a projective correspondence between two lines is completely determined when three distinct points on one are mapped onto the other, and that any correspondence between two

[35] The original treatments of Peano [1894] and Hilbert [1899], as simplified by Vahlen [1905], and especially that of Moulton [1902].

lines that preserves harmonic conjugates is a projective correspondence. Both, he said, could not be proved without further axioms, and he briefly rehearsed the history of the topic. Wiener had first noted, in his [1892], that the proof of the first fundamental theorem relies on the theorems of Desargues and Pappus, but he did not prove it. The first to do so was Schur [1898]. After this, Hilbert had shown in his *Grundlagen*, Ch. 6, that indeed the first 15 axioms do not imply Pappus's Theorem. Whitehead gave Vahlen's proof of this result in Ch. VIII. Whitehead particularly acknowledged the mimeographed notes of Veblen's Princeton lectures "On the foundations of geometry" published by Chicago University in 1905 [Veblen, 1905].

In Ch. IV Whitehead studied order properties, again following Pieri, and also Vailati [1895a; 1895b], and Russell [1903, Chs. 24, 25]. This led into somewhat topological questions, for example, the Lüroth–Zeuthen theorem that a harmonic system of points is everywhere dense (every segment of the line contains at least one member of the set). This had been discussed by Klein; Whitehead cited his *Nachtrag* in *Mathematische Annalen*, **7** [Klein, 1874]. Its proof was found to involve yet another new axiom, the 19th, which Whitehead called the Dedekind axiom by analogy with Dedekind's *Stetigkeit*. The remaining chapters dealt with involutions and co-ordinate systems[36] before the much heralded coordinates were finally reached in Ch. VII.

Macaulay, in his review of these two Tracts, credited them with being the first to give English readers "a general outline of abstract non-Euclidean geometry," before noting that the reader "may find them difficult, and may well wonder whether many parts might not have been put in a simpler and more attractive form" [Macaulay, 1908, p. 288]. He found that the conspicuous merits of the Tracts were marred by a very poor layout of the text, making it difficult to follow, by a strange avoidance of the usual nomenclature, and by, one might say, the author's philosophy.[37] Thus, geometry was defined by Whitehead as the general science of classification; Macaulay found this a very incomplete description, noting that geometry deals with operations that lead to classification, but that operations and classifications are different things.[38] Macaulay's criticisms have some justice behind them, but Whitehead's contributions were just more abstract and philosophical than those by Mathews and Basset, and they were the only ones capable of providing a springboard for further research in geometry.

This completes our account of the second period of geometry at Cambridge, except for two institutional matters that reshaped mathematical life in the final years before the First World War, the Tripos reforms of 1906 and Baker's appointment as the Lowndean Professor of Astronomy and Geometry.

2.5. Institutional changes

2.5.1. The Tripos reforms of 1906

The reform to the mathematics syllabus that was pushed through Cambridge in 1906 was a matter of self-defense. Under the leadership of J.J. Thomson the School of Natural Sciences had begun to grow, until it was widely thought that it was taking mathematicians away from the study of mathematics.

[36] Where yet more literature was cited, including [Hilbert, 1899, Sects. 24–30; Burali-Forti, 1895, 1899; Huntington, 1905, Sect. 3, Theorem 30].

[37] As is well known, Whitehead's interests fruitfully merged with those of Russell at this time. For a way in to the extensive literature on the work of Russell, see [Griffin, 1991].

[38] Russell's work on the philosophy of geometry has been omitted from this paper for reasons of space, and the reader is referred to [Griffin, 1991]. For a more recent consideration of Russell's ideas on geometry, see [Gandon, 2004].

Leading the debate,[39] E.W. Hobson quoted persuasive figures: an average of 105 students had completed the Tripos in the years 1881–1885, but only 56 in 1905. To counter this, a reform of the Mathematical Tripos was proposed that would produce a syllabus more in line with the apparent needs of the students and what mathematicians at Cambridge conceived of as the needs of the nation. This meant less artificial mathematics and more applicable mathematics, a move that J.J. Thomson supported. The system of ranking candidates (Senior Wrangler and so forth) was indicted for its sterility and abolished; the syllabus was reformed and brought more tightly under university control by constraining the autonomy of the colleges. The reform was phased in, so 1909 was the last year in which candidates were ranked in order of merit.[40]

More academic concerns also animated the mathematicians. Hobson deplored the unsatisfactory influence of the Tripos on even the best students, noting that the ideas of many Continental mathematicians "had never permeated the teaching of Cambridge to a sufficient degree to form a real school of mathematics." Whittaker agreed. In his opinion, many a graduate who left Cambridge "was quite unable to keep in touch with mathematical opinion in later life because ... he was not sufficiently advanced to understand what was being done in mathematics."

2.5.2. Baker's rise

Baker's rise to a position of influence over the state of geometry at Cambridge began with a series of academic affairs at Cambridge. In 1910 the ostracism Edwardian Cambridge meted out to Forsyth for allowing the estranged and badly mistreated wife of C.V. Boys to move in with him made Forsyth decide to resign not only his professorship, which he surely would have had to forfeit, but also his Trinity fellowship, which was unassailably his for life. (He became a Professor at Imperial College, and married the former Mrs. Boys.) Forsyth was succeeded as Sadleirian Professor at Cambridge by Hobson, in a move that upset Baker, who felt passed over. In 1911, Baker's interests turned even more toward geometry, and when, in 1914, the Lowndean Professor of Astronomy and Geometry fell vacant, Baker applied. This was not welcomed by the astronomers, although astronomy was well represented by two other professors at Cambridge, because Baker had never worked on astronomy, and the election on 22 December 1913 resulted in a tie between Baker and Philip Cowell (Senior Wrangler in 1892 and Superintendent of the Nautical Almanack Office). The Vice-Chancellor refused to use his casting vote, and the matter was passed to the Chancellor, Lord Rayleigh. On 5 January 1914 he decided in favor of Baker, who was then appointed, and (presumably with the intention of making amends) lectured on Poincaré's work on celestial mechanics. It was to be Baker's tenure of this chair that enabled him to exert such an influence in the third period of this study.

The fourth International Congress of Mathematicians was held at Cambridge in 1912, and Baker's address to the Geometry section gives a good indication of his position and his ambitions at that time.[41] He welcomed the "many distinguished geometers from other lands." Then, much as in his Presidential address to the London Mathematical Society, he sketched the current situation in the theory of algebraic

39 The debate is covered in the *Cambridge University Reporter*, vol. 36, 1905–1906, pp. 740–746, 873–890, 1039. The quotations come from pages 740, 873, and 885.

40 It seems difficult if not impossible to determine if these reforms had the hoped-for effect because of other changes made to the degree structure in Cambridge at the time and the onset of the First World War in 1914.

41 Information about the work of organizing this ICM seems impossible to come by. This account is based on the published Proceedings.

curves and surfaces "because of its connection with his own work," concluding, "These results, here stated so roughly, are, you seem, of a very remarkable kind. They mean, I believe, that the theory of surfaces is beginning a vast new development. I have referred to them to emphasize the welcome which we in England wish to express to our distinguished foreign guests, whose presence here will, we believe, stimulate English geometry to a new activity."

3. The First World War

The First World War wrought havoc on Cambridge as it did on every town in the combatant countries. Well away from the carnage, young men enlisted and were sent to fight in a form of struggle none could have imagined, for a duration that, after the first few months were over, none could guess. Genuine disagreements between the patriot and the pacifist turned bitter, and as the death tolls rose and the wounded and traumatized returned home in increasing numbers, the enormity of it all sank into every one's hearts.

During the War, enrolment at the University fell to 10% of its previous level. Teaching shrank accordingly, and mathematicians too old to fight at the front went to other kinds of war duties, notably research in ballistics and anti-aircraft gunnery.[42] After the gifted young physicist H.G.J. Moseley, who had done remarkable work on X-ray spectra confirming Bohr's quantum theory of atomic radiation, was killed at Gallipoli in 1915 at the age of 27, Rutherford began to lobby behind the scenes for selective exemption from the draft. Hardy supported him, and in the event fewer British mathematicians and scientists were killed in action than their French counterparts; German mathematicians seem to have been treated like the English. Even so, after the War, E.W. Barnes (who had tutored Ramanujan) said that "Of my pupils at Cambridge at least one half, and practically all the best, have been killed or maimed for life" [Kanigel, 1991, p. 261]. Among those who were not spared, none came to carry a more symbolic weight than E.K. Wakeford, who was killed on the Western front in July 1916 when only 22.

Wakeford entered Cambridge in 1912, having been taught at school, Clifton College Bristol, by W.P. Milne, possessed of "an amount of geometrical knowledge ... wholly astounding in a schoolboy" [Grace, 1916, p. liv]. He impressed all who knew him with his ability, published four of the six papers he is known to have written while still an undergraduate, and graduated among the best students in his year in January 1914. A further, unfinished paper was later described in a memoir by Baker [1922]. Wakeford enlisted when the war broke out. A minor injury early in 1916 enabled him to return home and resume his interest in the theory of canonical forms for invariants. Baker introduced him to the papers by Hilbert on the subject, and he set himself the task of finding explicitly the transformations that reduce a given form to a canonical one without recourse to the words "in general." This work was begun, and the problems clearly posed, when he returned to the Front in June. The few short words Grace wrote at the end of his obituary of Wakeford still ring eloquently of the pain and loss: "He only needed a chance, and he never got it" [Grace, 1916, p. lvii].

[42] See a forthcoming publication by June Barrow-Green.

4. Relativity theorists

It is of course widely known that Einstein published his revolutionary ideas on the nature of space and time in 1905. It is much harder to discover how these ideas were received, and to account for the perspective on them taken by people at the time. The European story is well told by Miller [1981], the English one by Warwick [2003]. Here we concentrate on how the geometric implications of the theory were drawn by A.A. Robb, after briefly summarizing the contemporary situation in physics.

Sir Joseph Larmor was the Lucasian Professor at Cambridge from 1903 to 1932.[43] With J.J. Thomson (2nd Wrangler in 1880, the same year that Larmor had come first), he laid the foundations of electromagnetic theory in such works as his *Aether and Matter* [1900]. That the book was dubbed "Aether no matter" by a humorous critic only shows how deeply Larmor, and indeed all British scientists, were committed to the ether; naturally this inclined them to reject Einstein's approach, which made the ether irrelevant. Also at Cambridge was Edmund Whittaker (2nd Wrangler in 1895 and 1st Smith's prizeman in 1897). In 1905, the year he became an FRS, he was a Fellow of Trinity and a college lecturer. In 1951 and 1953 he was to turn a lifetime's devotion to scholarship into his two-volume *A History of Theories of Aether and Electricity*, with its controversial argument that relativity theory belongs properly to Poincaré and Lorentz. Historians have argued against this odd judgment—see for example [Miller, 1981]—but it refuses to go away and it may well be colored by the Cambridge opinions of almost 40 years before.

The first account of Einstein's ideas in English is probably that of Lewis and Tolman at MIT [Lewis and Tolman, 1909]. This is the year that Minkowski published his lecture "Raum und Zeit" ([Minkowski, 1908], given in Cologne in 1908), which is generally taken to have set relativity theory before mathematicians in an irresistible way.[44] The first to take up the subject at Cambridge were Harry Bateman, who wrote on the subject with Ebenezer Cunningham, and A.A. Robb. Cunningham had gone up to St John's in 1899, become Senior Wrangler in 1902, and won a Smith's prize in 1904. That year he took up a lectureship in Liverpool. In 1907 he moved to University College London, and he returned to St John's in 1911, where he stayed until 1926. His book *The Principle of Relativity* [1914] is the first treatise on this subject in English, but as his obituarist W.H. McCrea noted in [1978, p. 116], "his eminence in relativity theory had been so long ago that scarcely anyone remembered it." It is a work of physics, concerned with physical quantities, the conduct of measurement, and the nature of reality, and we accordingly refer the reader to Warwick's account of it (see [Warwick, 2003, Ch. 8]).

Alfred Arthur Robb came to St John's from Belfast and has the distinction of being one of the few Cambridge mathematicians to treat the system with a rare degree of independence. A man of private means, he found the syllabus simply insufficiently interesting and graduated 52nd in 1897 (not even a wrangler), but managed to be accepted at Göttingen, where he successfully took a Ph.D. under Waldemar Voigt on the Zeeman effect. He returned to Cambridge and worked at the Cavendish Laboratory, where research into electromagnetism was concentrated and where he is remembered for his comic verse. During the War he resigned from work on what he suspected was the production of poison gas and drove ambulances; for this he was awarded a *Croix de Guerre*. He returned to Cambridge after the War and became an FRS in 1921.

Robb's first account of the special theory of relativity [1911] is a mathematician's work (see [Walter, 1999]). In it, Robb sought to present the axioms of geometry, where they are not purely logical, as "the

[43] He steadily amassed numerous honors; his knighthood came in 1909.

[44] For a thorough account, see [Walter, 1997].

formal expression of certain Optical facts" [Robb, 1911, p. 1]. He imagined light signals being sent out from a point A, reflected at a point B, and returned. The elapsed time for a particle, A, measured from an arbitrary starting point, he called the index (to distinguish it from "time"). If light is emitted from A at an instant with index 0, is reflected from another particle half a unit of distance from it, and returns to A, Robb said that the index at A was now 1. Thus each particle has its own internal clock. If N_d and N_r are indices of departure and return of a signal, he took $\frac{1}{2}(N_\mathrm{r} - N_\mathrm{d})$ as a measure of the distance the reflecting point was away, and $\frac{1}{2}(N_\mathrm{r} + N_\mathrm{d})$ as the index of the arrival of the signal at B. This argument may be conducted for particles at rest or in relative uniform motion (a state that he showed excluded rotations). The indices of events with respect to two systems in relative motion differ in a way that depends in a simple, explicit way on the relative velocity. The Newtonian law of the addition of velocities (along a line) does not hold, but Robb defined the rapidity ω of one particle with respect to another by the formula $v = \tanh \omega$, where v is the relative velocity of the particles, and showed that rapidity is additive. He then observed that if the geometry of any one system of points (i.e., an inertial or "permanent" system, as he called it) is Euclidean, then the geometry of any system in uniform relative motion will also appear to be Euclidean. Next he took three particles in uniform relative motion that coincide at an instant, and thereafter define a (changing) plane. If the relative rapidities of the particles are taken as distances in this rapidity space then, Robb showed, the trigonometry of this triangle shows that rapidity space obeys non-Euclidean geometry. Moreover, he noted, when the relative velocities are small rapidity and velocity may be identified, and the non-Euclidean triangle be treated as a Euclidean one. Also, the formulae agree with those of Einstein if the velocity of light is unity. He pointed out that the same formulae had been deduced from Minkowski's theory by Sommerfeld; the introduction of non-Euclidean geometry in this context was carried out independently by the Croatian mathematician Varicak [1912].

Robb's paper was subtitled "A new view of the theory of relativity," but that should not suggest that he agreed with Einstein's ideas. The preface to his book *The Absolute Relations of Space and Time* [1921] spells out the differences. He was, he wrote, directly inspired by the failure of attempts to detect uniform motion through the ether, and by discussions on this topic at the British Association meeting in 1902. Sir Joseph Larmor inspired him to try and formulate a clear idea of what is meant by the equality of lengths, and some years later while trying to resolve that problem he heard for the first time of Einstein's work, which made an unexpected impression on him. "From the first," Robb later wrote, "I felt that Einstein's standpoint and method of treatment were unsatisfactory."[45] In particular, the idea that two events could be simultaneous for one observer but not for another disturbed him. The result was this book which presented a theory that he regarded as absolute (because the relations of before and after are not dependent on any observer) and a method with "certain advantages as a study of the foundations of geometry."

The book was a reworking of an earlier account [Robb, 1914] but, as Robb said: "Unhappily at that period people were concerning themselves rather with trying to sever one another's connexions with Time and Space altogether, than with any attempt to understand such things; so that it was hardly an ideal occasion to bring out a book on the subject" [Robb, 1921, p. vi]. In his book of 1921 Robb presented his ideas based on the starting point that a single observer directly conscious of events can place them

45 [Robb, 1936, preface], which opens with the nice remark that "The present volume is essentially a second edition of the one which was published by the author in 1914 under the title: *A Theory of Time and Space*. An alteration of the title has been made, since it was considered that the word *geometry* conveyed a somewhat better idea of the nature of the contents of the book than did the word *theory*."

in the order in which they occur. He then elaborated a geometry based on the relation of "before" and "after" rather than "between." As a heuristic device he proposed that the reader consider vertical double cones at each point of four-dimensional space and define an instant B to come after another, A, if B lies in the upward facing cone with vertex at A (likewise B comes before A if it lies in the downward facing cone with vertex at A). All the familiar properties of "after" hold with this definition, except that an instant A may be neither before nor after an instant B without the instants A and B being identical. This ordering of events constitutes what he called the conical ordering of instants or points, which, however, he proceeded to define abstractly, as part of a geometry with 21 postulates. He compared his findings with other axiomatic treatments of geometry due to Peano (we do not know why he did not mention Hilbert; Whitehead had preferred to emphasize the Italian over the German, but after the War there was also a concerted anti-German move in science and mathematics that perhaps Robb supported).

The analogy with light cones was exploited to yield what Robb called the "optical line." The optical line between A_1 and A_2 corresponds to a common generator of two (half-)cones with vertices at A_1 and A_2; it is only defined for points each one of which lies on the cone of the other. An inertia line joins two points each of which is inside the cone of the other, and a separation line joins two points each of which is outside the cone of the other (the modern terms are time-like and space-like, respectively). These terms permitted him to define what he called a separation threefold, and he showed that the geometry of this threefold is formally identical with the ordinary (Euclidean) geometry of three dimensions. In his [1921] this followed from his theory of congruence, which yielded an appropriate theory of length; in the earlier version Robb did this using Veblen's axiom systems (see [Veblen and Young, 1910–1918]). The account ended with the introduction of coordinates and the deduction of the familiar analytic formulae for special relativity.

Robb's account has usually been found hard going. The uneasy relation to physics and the contemporary enthusiasm for axiomatic geometry combine to alienate the reader. The postulates, as Goldblatt [1987, p. 170] observed, "generally lack physical significance, or even a more geometrical intuitiveness and as [Suppes, 1973] remarked, 'the complexity of the axioms stands in marked contrast to the simplicity of his single primitive concept.' "[46]

The subsequent history of relativity theory at Cambridge is dominated by Sir Arthur Eddington's successful verification of the effect of gravitation on light in 1919.[47] This achievement famously did three things: it confirmed one of Einstein's predictions; it turned Einstein into a household name; and it helped to heal the divisions opened up by the war. The general theory of relativity thereafter presented two faces (both on show in, say, Whitehead's [1922]): the subtle physics and the technically difficult differential geometry and tensor calculus. As for the physicists, they soon moved in droves to the new field of quantum mechanics.[48] The mathematicians at Cambridge did not, however, take to the harder stuff. Neither Robb nor Cunningham wrote significantly on the subject, and by 1922 Whitehead had gone to Manchester and was in any case on his way to his new career in philosophy. Although L.A. Pars

46 Goldblatt's book gives a good modern account of Robb's work and the deduction from it of the Alexandrov–Zeeman theorem that characterizes space–time transformations as those linear transformations that preserve beforeness (also called causal relatedness) as composites of Lorentz transformations, scalings, and translations.

47 On Eddington's route to this famous piece of work, and on the slender evidential base for this achievement, see [Warwick, 2003, Ch. 9] and the references cited there.

48 Detailed accounts of this can be found in [Goldstein and Ritter, 2003].

won a Smith's prize in March 1921 for an essay *On the General Theory of Relativity*, the future of geometry as Cambridge was to lie almost entirely with Baker, who in 1918 turned 52.

5. From 1918 to 1940

5.1. Baker and his influence

After the War, Baker dedicated himself completely to geometry. His six-volume *Principles of Geometry* was begun in 1922 and finished in 1933, when he was 69. His aim in writing the books was, as he said in the "Introductory" to Volume I, "to place the reader in touch with the main ideas dominant in contemporary geometry." They give a good impression of the strengths and weaknesses of his approach. The first four, with the less than informative titles of *Foundations*, *Plane Geometry*, *Solid Geometry*, and *Higher Geometry*, were published by 1925. The last two, *Analytical Principles of the Theory of Curves* and *Algebraic Surfaces*, were published in 1933 and Baker's obituarists found less to enthuse about in them. Hodge observed of the last volume that "it now seems somewhat old-fashioned" [Hodge, 1957, p. 53]. Indeed, they are not as original as the previous four, but not only are they remarkable for some one of his age, but also they helped shape the research interests of the mathematicians he drew around him.

The first volume, *Foundations*, is modestly Hilbertian, although the point is not insisted upon and Hilbert's name is scarcely mentioned. It begins with a discussion of abstract geometry, based on properties of incidence, and dwells on the relationship between the theorems of Pappus and Desargues in two and three dimensions. He showed that Pappus's theorem was true if and only if the coordinates were commutative. Baker first established what he called general projective geometry, which is projective geometry with a minimum of axioms. He then discussed what he called real geometry, which is projective geometry with a concept of betweenness added. Volume II, *Plane Geometry*, deals largely with projective theorems about conics, but it also shows how non-Euclidean geometry arises via the specification of a nondegenerate "absolute" conic and covers the more advanced theorems to do with Pascal's hexagon (Steiner–Plücker lines, Kirkman points, and the like). Volume III, *Solid Geometry*, deals with the projective geometry of quadrics, taken singly, in pairs, and in families, and the cubic curves that rise from taking the intersection of two quadrics with a common generator. It ends with a long final chapter on the cubic surface. Volume IV, *Higher Geometry*, takes theorems about circles in the plane as the springboard for a discussion of geometry in spaces of dimensions 4 and 5. Kummer and Weddle's surfaces and the theory of cyclides are then treated in detail, starting from the figure of 15 lines and points in a space of four dimensions discussed by Hudson. This device of starting from a figure in incidence geometry and generating algebraic hypersurfaces from it was to be emulated by a number of his students. As Baker noted in the preface to this volume, it was the culmination of the previous three, being "the first written and the most revised, of the book, for which indeed, mostly, the earlier volumes were undertaken...."

By contrast, the fifth volume, *Analytical Principles of the Theory of Curves*, is, as its title says, analytic and Riemannian in spirit, while *Algebraic Surfaces* is more Italian, and deals largely with correspondences and questions in birational geometry. It has a chapter on the Schubert calculus, another on the birational invariants of an algebraic surface, and it indicates how the use of the geometry of four-dimensional space can be used. As we shall see, it was this volume that inspired the most research among the geometers at Cambridge, much of which is acknowledged in the volume itself because it grew out of lectures given over a number of years.

Similar ground was covered by Baker's assistant throughout these years, F.P. White. He was a lecturer at Cambridge from 1922–1961 and taught courses with such titles as Cubic Surfaces, Solid Algebraic Geometry, the General Theory of Curves, Particular Loci in Higher Dimensional Space, Differential Geometry, Introduction to Algebraic Surfaces, and Theta Functions in Geometry. Between them Baker's books and White's lectures offered Cambridge mathematicians a route from what they had learned at school to the frontiers of research for the first time.

Baker not only wrote his volumes of *Principles*. Throughout the period he organized a regular series of Saturday afternoon tea parties. Burnside, a regular correspondent of Baker's, and Macaulay were among those who came, and attendance was fairly strictly enforced, although intercession could be made for athletes, but otherwise the milieu was informal.[49] Standards, moreover, were high. T.G. Room recalled that many embryonic Smith's Prizes were heard there, and indeed Baker's students won Smith's Prizes six years out of nine, from 1927 until 1935. This was no tea party, it was a research seminar, the first of its kind at Cambridge (from 1931, when Hardy returned to Cambridge, the analysts ran a similar sessions on Wednesdays). For the first time research in geometry at Cambridge moved to bring about the "effective co-operation between the erudition of riper years and the enthusiasm, the imagination, of youth." An indication of the impact of the tea parties may be gained from the effect they had on P.A.M. Dirac, who attended them on arriving as a research student at St John's in 1923. O. Darrigol observes that "Dirac's ideas of q-numbers and his axioms for them most probably originated at Baker's tea parties," and points out that in Baker's *Principles of Geometry* there is an abstract noncommutative algebra tied to a non-Pascalian geometry.[50] He also observes that at a tea party in 1924 Baker claimed that any really interesting mathematics should find an application in the physical world, and that Dirac, who took as his leitmotif mathematical beauty, cited projective geometry as the first of his early experiences of this beauty [Darrigol, 1992, p. 644].

In the 1920s and 1930s academic life at Cambridge became much more structured and like the situation at leading universities elsewhere. Three noteworthy developments in mathematics at Cambridge in the 1920s were the steady appearance of women in the pages of the research journals published either by Cambridge or the London Mathematical Society, even though they were still unable to take any kind of Cambridge degree, the presence of a number of Indian mathematicians, and the arrival of the Ph.D. in 1924. Among the women, H.G. Telling, a Newnham graduate who taught at Bristol University, wrote four papers on higher-dimensional geometry. In 1930 Hanumanta Rao wrote on the representation of planes in five-dimensional space [Hanumanta, 1930], and in 1931 I. Brahmachari (who had a Ph.D. from London University) outlined a program for replacing complex coordinates with systems of points on the real line, in the spirit of Staudt's treatment of complex points [Brahmachari, 1931].

The first Ph.D. in geometry was awarded to Frederick Bath in 1927 for a thesis on algebraic curves on del Pezzo's quintic surface, with the catch-all title of "Researches in the geometry of algebraic curves and surfaces," written under Richmond's supervision.[51] Baker's first Ph.D. student was T.G. Room, who wrote his thesis on "Geometric double configurations." A list of Baker's students in this period is in-

49 On the importance of athleticism at Cambridge, see [Warwick, 2003, Ch. 4].

50 [Darrigol, 1992, p. 288]. The example will be found in Baker's *Principles of Geometry* [1922–1933, I, Ch. 1, Sect. 3].

51 Bath later became an assistant lecturer at King's College London in 1924 and left in 1928 to become a lecturer at St Andrews, whence he moved into a successful career in administration (see [Edge, 1983]).

structive (the date of the Ph.D. is given in brackets; * indicates a Smith's prizewinner) since most of the Cambridge geometers of the day were his students, and a strong group they were: T.G. Room* (1927), W.L. Edge* (no Ph.D.), J.G. Semple (1930), P. Du Val (1931), H.S.M. Coxeter* (1932), J.A. Todd* (1932), D.W. Babbage* (1933), J.W. Archbold (M.Sc., 1934), J. Bronowski (1935), E.A. Maxwell* (1935), R. Frith (1937), D. Pedoe (1937), L. Roth (no Ph.D.), E.D. Tagg (1938). The run of Smith's prizewinners is impressive when one recalls that two Smith's Prizes were awarded each year, on topics ranging from geometry and analysis to physics. Moreover, the essays by Archbold, Bronowski, Frith, Pedoe, and Roth were all designated essays of distinction, and in 1936 du Val's, Todd's, and Room's essays in the Adams Prize competition were singled out for honorary mention; this was the year that Hodge won. W.V.D. Hodge* took a degree from Edinburgh before going up to St John's in 1923 and graduating in 1925 as a wrangler with distinction; he won his Smith's Prize in 1927.

As this list makes clear, Baker's influence and reputation grew steadily but slowly. Throughout the 1920s the Smith's Prizes went more often to topics in physics or analysis in the manner of Hardy and Littlewood, but a more even balance was struck throughout the 1930s.

Moreover, a number of the men mentioned above were able to secure positions at Cambridge, even in the difficult times of the early 1930s, before going on elsewhere, and in this period several Cambridge mathematicians went on to spread their message abroad. To cite the main examples: Room became a Fellow of St John's from 1925 to 1929 (and an assistant lecturer at Liverpool University from 1925 to 1927). From 1929 to 1935 he was a lecturer at Cambridge, covering such subjects as cubic surfaces, incidence theorems, quadrics in four and five dimensions, plane algebraic curves, and determinantal loci (see [Room, 1938]), and then he became a professor at the University of Sydney, Australia. Edge, who studied at Trinity, went on to be a lecturer there from 1929 to 1932, giving courses on plane algebraic curves, before he left to go to Edinburgh. Patrick Du Val lectured at Trinity on rational surfaces and elementary topology from 1930 to 1934 and also found time to study in Italy with Severi,[52] before going to Manchester University in 1934. He also taught for a time as a professor in Istanbul, before going to the United States for three years, after which he returned to England to take up posts at Bristol and then London. On his retirement he went back to Istanbul, where he taught for a further three years. Leonard Roth went to Imperial College London, where he remained until 1967, having spent 1930–1931 studying in Rome with Castelnuovo and Enriques. Semple, whose Ph.D. in 1930 under Baker was on two topics (*Surfaces of Intersection of Cubic Primals* and *Cubic Cremona Transformations of Four-Dimensional Space*), became professor of pure mathematics at Queen's University Belfast in 1930 before becoming professor of pure mathematics at King's College London in 1936, where he remained until 1969. The book by Semple and Roth, *Algebraic Geometry*, in 1949 is indicative of their still largely traditional interests at the time.

The strong concentration of algebraic geometers in London after the war gave rise to the long-running London Geometry Seminar, which continues to this day and is now called the London Geometry and Topology Seminar. It was founded by Semple, Roth, Wren, Archbold, and Scott, and later joined by Du Val. The meetings were apparently known for "the cut and thrust of their debate and for the general barracking that went on ... all done with hugely good humour" and it attracted speakers from all over the world [Tyrrell, 1987, p. 379].

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[52] Du Val's obituary (*Bulletin of the London Mathematical Society* **21** (1989) 93–99; see p. 93) records that while he held a Fellowship at Trinity he "spent a period in Rome, learning at the feet of the great Italian geometers of that time; he particularly admired Enriques, to whom he would often later refer to as his master."

We should note one other way in which this diffusion of mathematical knowledge was carried out: through conferences. For example, the 1926 Mathematical Colloquium of the University of St Andrews in Scotland focused on geometry and was attended by about 90 people, some from as far afield as Australia, Egypt, and India.[53] Richmond lectured on "Recent advances in geometry," Baker informally on "The passing of metrical geometry," and there was a forum on "Passing of Metrical Geometry" devoted to a series of examples in which the dependence of metrical on projective properties was shown. It seems to have been a successful meeting. At least Richmond thought it was, confiding to his diary that he was surprised that Baker had turned up, and that G.D. Birkhoff gave a very good talk on "The significance of dynamics for scientific thought."[54] The next St Andrews Colloquium, in 1930, included a lecture course given by Richmond on "Arithmetical properties of curves and surfaces" and one by Baker on "Rational curves and surfaces."

5.2. *A Baker school of geometry?*

This activity invites the historian to consider whether, and to what extent, Baker can be regarded as having been the leader of a school of geometers at Cambridge. The topic of schools in mathematics has recently been discussed by Parshall [2004]. She suggests that for a school to be present, there must be a leader, who might be charismatic but more likely is one who does inspiring work. The leader must advocate a particular approach to a coherent group of ideas, train students who go forth with confidence in the explicit and tacit knowledge they have acquired, and go on to do research that reflects external validation of the approach. She also suggests that with the death of the leader one may see the birth of a research subdiscipline.

It is clear that on this definition there was not a school of geometers at Cambridge before Baker's time. It is also clear, from the tea parties and from remarks dropped in their papers and books, that the geometers in Cambridge certainly gravitated around Baker and regarded him as the leader. Moreover, these geometers formed a reasonably coherent group, who identified themselves as such and discussed mathematics at length with one another. We shall give evidence below in support of the conclusion that they shared questions, methods, and indeed an approach to geometry that derived from Baker. One could consider adding to Parshall's definition the further thought that the members of a school must themselves be cohesive—they must feel that they "belong."

Be that as it may, the troublesome aspect of Parshall's definition of a school in the present context is the idea that a school must also succeed. The example she discussed at length is algebra at Chicago at the start of the 19th century, an undoubted success story. We shall see below that Baker's group meets all the sociological criteria to be a school, but fewer of the subject-based criteria. The members wrote papers and even books, they performed very well in the Cambridge context, as the list of Smith's prizes shows, and their achievements were well recognized in Italy, then the center of research in algebraic geometry. To that extent one sees evidence of external validation. But it will emerge that the approach they pursued was flawed, and lasting success was to elude them.

Parshall's analysis of the concept of a school in mathematics is cogent, and her discussion of how it can be adapted from its principal use in the study of the laboratory-based sciences is helpful. There is much to be said for precision in the use of the term. The present Cambridge case, unlike that of analysis with

[53] St Andrews Colloquia were held every four years. Unfortunately there do not seem to be detailed records of the proceedings.
[54] Entry from H.W. Richmond's diary for August 2–11, 1926, *King's College Cambridge Archives, Misc 73/7*, p. 61.

Hardy and Littlewood, which was a lasting success, would suggest that there is something to be said for separating out the sociological criteria for a school and basing a definition on them alone. Thus a school should have a leader who produces inspiring work organized round a specific approach, and who trains students to go out and extend and adapt that approach themselves. The enterprise should be recognized from outside as being of a particular, identifiable type. But room should be left for the possibility of unsuccessful schools, which are perhaps too narrow, or go into a decline, as well as for the powerful successful schools that go on to produce new directions in mathematics.

5.3. Baker's followers

As noted above, Baker's Presidential Address to the London Mathematical Society in 1912 was on the Italian theory of algebraic surfaces. Hodge, in his own Presidential Address to the Society in 1949 (published as [Hodge, 1950]), called it "one of the classical summaries of the subject," going on to say, "The address is indeed a landmark in the study of geometry in this country; ... Baker's address marks the beginning of an activity on the part of British geometers so intense that it has overshadowed most of the contributions made in this country in other realms of geometry" [Hodge, 1950, p. 148]. It is to the work of those geometers that we now turn.

The interests of the Cambridge geometers reflect Baker's preferences: markedly Italian in their sympathy, and often focused on extending what was known about objects in 3-dimensional space to higher dimensions. Cambridge geometers had a liking for unexpected properties and particular cases, rather than general theories.[55] For example, Charles G.F. James, who taught at Newnham and then at King's College before going to Liverpool University in 1924 (he died in 1926), published a paper [James, 1922–1923] generalizing an old observation of Corrado Segre's. Segre had shown that in 4-dimensional projective space, planes that meet four given lines meet a fifth line that is determined by the original four. James produced a similar theorem about planes meeting a rational normal quartic curve in 4-dimensional projective space in three points. He noted that Baker, on hearing of this result, asked if Segre's theorem was a special case, and James showed that it was. Another paper of his [James, 1926] was picked up by Roth in his [1929]. This had to do with the study of algebraic surfaces in higher-dimensional projective spaces, where Roth considered enumerative questions about lines that meet the surface more than once. In a paper the next year Roth pushed these questions further, raising and answering questions about multiple lines and planes tangent to surfaces in three-dimensional projective space, which James had considered in the special case when the surface is a scroll.

In the 1930s the emphasis shifted to understanding the Italian theory of algebraic surfaces. These had been classified by Castelnuovo and Enriques in a major series of papers before the War, and they, Severi, and Bagnera and de Franchis had then written extensive accounts of important families within the classification.[56] Algebraic surfaces are markedly harder to understand than algebraic curves. It had been known since the time of Riemann that an algebraic curve is classified topologically by a single number, its genus, and that curves of the same genus may be distinguished birationally by their moduli. De facto,

[55] Our account concentrates on papers published in the *Proceedings of the Cambridge Philosophical Society*. It would be amplified but not otherwise altered if it embraced the papers published in the *Proceedings of the London Mathematical Society*, which, for reasons of length, we omit.

[56] From a long list of papers, the most important are [Bagnera and de Franchis, 1908; Enriques and Severi, 1909–1910; Enriques and Castelnuovo, 1914].

the classification was into three families: curves of genus zero, the rational curves, which are birationally equivalent to the projective line; curves of genus 1, the elliptic curves; and curves of higher genus, about which not so much was known. It had been known for many years, indeed since the paper by Cayley ([1871], discussed above), that algebraic surfaces have two genera (called their arithmetic and geometric genus), which may be different; surfaces for which the genera differ are called irregular surfaces and were difficult to study. The full classification produced by Castelnuovo and Enriques classified algebraic surfaces in terms of four numbers, which are birational invariants. It was also widely believed, with good evidence amounting almost to a proof by the 1920s that an algebraic curve in the projective plane with singular points may be transformed to an algebraic curve without singular points in a projective space of higher dimension, and into an algebraic curve in the plane with a standard and easily understood collection of singular points in the projective plane. Evidence for the analogous claims about algebraic surfaces was less solid.

The Castelnuovo and Enriques classification of surfaces was also dextrously arranged to present their remarkable result in the best possible light. Like the de facto study of curves, it divided algebraic surfaces into three families. The first family was the class of ruled surfaces (which contained, for example, the rational surfaces, those birationally equivalent to the projective plane). The second family contained, but was not exhausted by, the irregular hyperelliptic surfaces (those for which the arithmetic and geometric genera were different). In the third family came a large collection of irregular surfaces for which questions about their birational geometry could be reduced to questions about the projective geometry of a model of the surface in a suitable projective space. This was a genuine classification in that Castelnuovo and Enriques found that the families could be described up to birational equivalence by their four numbers, and that special cases within each family could be singled out by a more delicate analysis. In particular, the three families were distinguished by the value of one number alone, which was either zero, one, or greater than one. But impressive though it was it was by no means the whole story, and in particular the third family was a zoo of cases much in need of further analysis.

The Cambridge geometers became very aware that what might look very grand from a distance could look impenetrable or unworkable close up. Thus Roth opened his [1934] with the words "The general theory of surfaces has been developed without much attention to the simpler aspects of the subject and, in particular, to the problem of determining some elementary criteria of regularity and referability." He wrote a paper [Roth, 1935] on irregular surfaces that grew out of correspondence with Comessatti, which, as a letter published at the end of the paper attests, led the Italian to discover the first example of an irregular surface in four-dimensional projective space not referable to a scroll. Roth went on to take the examples of hyperelliptic surfaces discussed in the papers of Enriques and Bagnera and de Franchis which had complicated singularities in three-dimensional projective space and show how they could be given nonsingular models in higher-dimensional projective spaces. And Maxwell in his [1936, p. 185] wrote that "In spite of a large amount of general theory, it remains a surprising fact that the number of known irregular surfaces in space of three dimensions is quite small," the problem being that while the arithmetic genus was usually easy to find, computing the geometric genus was particularly difficult.

They chipped away at the problem, often happy to acknowledge the influence of Baker himself, and clearly talking to each other at the tea parties and elsewhere. Babbage opened his paper [1932–1933a] on isolated singular points on algebraic surfaces with the remark that "I wish to express my thanks to Professor H.F. Baker for his interest and advice. This paper was written in an attempt to clear up certain points suggested by his lectures, and a good deal of the matter of Sects. 2–4 is taken from them." [57] This may suggest that Babbage was not wholly convinced by the lectures. He then went on, in his next paper [Babbage, 1932–1933b], to take the 16 examples of algebraic surfaces shown to be inequivalent by Noether in his seminal paper [1875] but there described as singular surfaces in three-dimensional projective space, and show how they can be transformed birationally into nonsingular surfaces in a projective space of higher dimension.[58] This work of his connected with another, earlier, paper by him which was in turn taken up by Jacob Bronowski,[59] who noted also a paper by Baker. Babbage published an Addition to that paper, which was classifying surfaces of a certain kind, to the effect that he had missed an example which had earlier been noted by Roth. Roth was working through the description of all nonsingular surfaces whose plane sections were curves of genus four. Baker was interested in surfaces that are nonsingular in five-dimensional projective space and that project into surfaces with a particular simple kind of self-intersection in four-dimensional projective space. This paper arose from a lecture he had given, he said, in 1929, after which "in May or June of last year [1930], Professor Semple, dealing with rational surface, gave a very welcome corroboration of some of the results ... he has now added considerably to this by kindly reading through this Note, and making various suggestions" [Baker, 1932, p. 62].

To give another example of the evident buzz of activity among this group, which surely contributed to the run of success with the Smith's Prizes and the search for jobs, both Semple and Todd wrote papers in 1930 on how varieties of lines in one projective space can be regarded as quadrics in another space, a theme as old as line coordinates but well treated by Segre (see [Todd, 1930; Semple, 1930]). Starting in 1934 Room wrote a series of paper on determinantal varieties following Segre's influential treatment of 1912.[60] Room commented in a footnote that such varieties "have been one of the subjects of discussion among the geometrical circle in Cambridge for some time past," and he particularly thanked Todd and Du Val for comments that he feared might otherwise appear unattributed [Room, 1934, p. 2]. As a final example, Coxeter began his career with a thesis on regular polytopes (a lifelong interest of his, as it was to transpire) that gave rise to three papers in the *Proceedings of the Cambridge Philosophical Society* [Coxeter, 1928, 1930, 1931]. Then, as he acknowledged [Coxeter, 1934, p. 466], a chance question by Du Val set him off on the study of groups generated by reflections—Coxeter groups [Coxeter, 1937; Coxeter and Todd, 1936]. He wrote: "In connection with his work on singularities of surfaces, Du Val asked me to enumerate certain subgroups in the symmetry groups of the 'pure Archimedean' polytopes n_{21} ($n < 5$), namely those subgroups which are generated by reflections.... The work involved being somewhat intricate, several slips would have been overlooked but for the information that Du Val was able to supply from the (apparently remote) theory of surfaces." [61] In 1936 Coxeter emigrated to Toronto, after spending two years as a visiting researcher at Princeton, and his illustrious career as a geometer belongs to Canada, not to Cambridge, but his devotion to the beauty of geometry, which began at school, was undoubtedly nourished by his immersion in Baker's circle of geometers.

57 This matter covered the Zeuthen–Segre invariant, the Segre–Severi invariant, and Noether's equation involving them.

58 Baker acknowledged this work in Volume VI of his *Principles of Geometry* [Baker, 1922–1933, VI, p. 232].

59 [Bronowski, 1933]. In the 1950s and 60s Bronowski turned to the history of science and was the erudite presenter on one of the first, and hugely successful, blockbuster science series, the *Ascent of Man* [Bronowski, 1978].

60 A determinantal variety is one defined by the vanishing of all $p \times p$ minors of a $p \times q$ determinant whose entries are linear functions of the coordinates in $\mathbf{C}^\mathbf{n}$, where $p \leqslant q$. See [Room, 1938] and the references cited there and [Segre, 1912, p. 825 ff.].

61 Du Val's papers appeared in the same issue of the journal as Coxeter's; see [Du Val, 1934].

All this activity is impressive, and much of it to Baker's credit. But it is possible to hear it in a minor key. Hodge himself noted that the British contributors to this branch of geometry had been early in the field but not often the initiators [Hodge, 1950, p. 149]. Throughout the 1930s each issue of the *Proceedings of the London Mathematical Society*, the English journal of preference for Cambridge mathematicians, carried much more on single branches of analysis than it did on geometry, or algebra for that matter. Baker was 64 in 1930, an age when few wish to shoulder the burden of inspiring a department. He did not bring to geometry then anything like the energy and freshness of vision that had enabled him to bring Riemann's ideas and all of contemporary algebraic geometry to Cambridge 35 years before. As the above quotations may have suggested, there was a degree of tentativeness about the theorems these people discovered. A number of authors would give two proofs of enumerative results and seem reassured when the computations agreed, which suggests that the arguments were not always thought to stand on solid ground. For example, in his [1936] Baker rederived an important lemma proved by Severi computing the irregularity of a surface. Baker noted that he himself had found it difficult to state Severi's lemma with precision and that, although Castelnuovo had written that the proof was valid, he then gave a different proof, one that relied on an enumerative formula due to Schubert. Baker did not explicitly say that this was a risky thing to do, but Schubert's results were known to be doubtful. However, all Baker could do was offer a proof of another theorem which "if accepted" would imply Severi's result. In this case, he was able to add a note at the end of the paper saying: "Since this paper was written, Mr. Hodge has shown the writer a proof of the theorem suggested .... The writer is much indebted to Mr. Hodge for criticisms of the present paper."

One reason for the comparatively minor achievements of these mathematicians, amid all the evidence of institutional success, may lie in the balance they struck between the analytic and the synthetic sides of geometry. In the "Introductory" to Volume I of his *Principles of Geometry*, Baker had written, "While the view is taken that all geometric deduction should finally be synthetic, it is also held that to exclude algebraic symbolism would be analogous to preventing a physicist from testing his theories by experiment; and it becomes part of the task to justify the use of this symbolism." There is always a tension in geometry between analysis and synthesis, but Baker's view may have been somewhat limiting. We have already noticed that Macaulay's rigorous algebraic methods were drawing no support at Cambridge. Whatever the explanation may be, we cannot resist quoting the opinion (forceful as was his wont) of a physicist, and later cosmologist, Fred Hoyle [Hoyle, 1997, pp. 89–90]. He was of the opinion that his tutor at Emmanuel College, P.W. Wood, "was the finest Cambridge geometer of his generation. The trouble lay in his style of geometry. Nowadays, mathematicians use an algebraic style, just as Wood did, but, in Cambridge up to 1939, the preferred style was different: almost everything was argued in words, which was really a negation of mathematics, for one of the most powerful weapons of mathematics lies in its notations, and the preferred style of Cambridge geometry (whose protagonists referred to themselves as 'pure' geometers) made little or no use of notations." Wood, said Hoyle, was an outstanding master of notation, and in Hoyle's opinion the inability of Wood's fellow geometers to match him at the manipulation of notation was exactly why they condemned his methods.[62] Never one to hold back, Hoyle concluded this passage in his autobiography with this sweeping comment "The Cambridge fad for pure geometry—or 'projective' geometry as it is more properly called—had been given impetus earlier in the century by H.F. Baker,

[62] Wood was the author of a Cambridge Tract [Wood, 1913], where he says in the preface that "Analytical methods have been used throughout in preference to synthetical."

whose unreadable volumes on the subject I still keep on my shelves to remind me of how far a fad can go."

Baker's achievement was to create a department where people talked to one another, and where someone else might take a leap of imagination comparable to his own in bringing Riemannian ideas to Cambridge. The man who did have the new depth of vision was W.V.D. Hodge. As noted above, he had passed through Cambridge as an undergraduate and then went to teach at Bristol. In 1930 he returned to Cambridge as a fellow of St John's, and in 1933 he became a University lecturer. At Bristol, Fraser (10th Wrangler in 1905) had been newly promoted to a readership in geometry. He had introduced Dirac to projective geometry in 1921, and now he encouraged Hodge to read the Italian geometers. Hodge was attracted to a problem of Severi's on the periods of a nonzero double integral of the first kind. He saw that a purely topological paper by Lefschetz [1930] could be used to find the answer, and the result was his paper "On multiple integrals attached to an algebraic variety" [Hodge, 1930]. The resulting controversy has become famous (see [Atiyah, 1977]). Lefschetz, who was quick on the draw and aggressive in his manner, insisted publicly that Hodge was wrong and wrote to him demanding that the paper be withdrawn. In May 1931 Max Newman arranged for Hodge and Lefschetz to meet in his rooms in Cambridge, which led to Lefschetz inviting Hodge to go to Princeton. Only there did Lefschetz come round, publicly retract his criticism, and become one of Hodge's strongest supporters [Atiyah, 1977, pp. 104–105]. He was then influential in setting up another important contact for Hodge: Oscar Zariski, with whom Hodge worked for several months at Johns Hopkins.

This deep acquaintance with contemporary work, going well beyond the usual Cambridge fare, pushed Hodge onward. His [1933] marks the arrival of his program to extend the theory of harmonic forms for general Riemannian manifolds, and therefore and in particular the theory of complex manifolds, beyond the stage where, in many ways, Riemann had left it. This culminated in the famous book [Hodge, 1941]. It was followed by the three-volume *Methods of Algebraic Geometry* written with Pedoe [Hodge and Pedoe, 1950–1954], which was intended to replace the somewhat dated six-volume work by Baker. The enterprise dragged on, and by the time the final volume came out in 1954, with its account of Zariski's methods, the subject of algebraic geometry had undergone yet another of its palace revolutions. That, coupled with lack of fluency, diminished the impact of the book, which is nonetheless full of fascinating material. On the other hand, Hodge theory continues to inspire a remarkable amount of research, amply justifying Hermann Weyl's hailing it as "one of the great landmarks in the history of science in the present century" [Weyl, 1954, p. 25; reprinted in Weyl, 1968, p. 616].

Hodge's Cambridge career in the 1930s is notable for another reason. The Adams Prize competition set in 1934 and awarded to Hodge in 1936 was the first time, since the prize's inception in 1848, that it was set on a geometrical topic. It was generally set on a topic in natural philosophy/physics rather than mathematics. The question called for an advance in existing geometrical theory under one of the four heads, three of which were quintessentially Cambridge topics and could well have been chosen with Hodge in mind[63]:

63 The fourth topic was the geometrical representation of the physical universe, such as, for example, Faraday's representation of electrostatics by tubes of force, Einstein's representation of gravitational effects by the curvature properties of Riemannian space, recent representations of gravitation and electromagnetism by projective 4-dimensional differential geometry, Wirtinger's general infinitesimal geometry of physics [1922], and the application of Hilbert spaces to quantum mechanics.

1. The theory of algebraic systems of curves on an algebraic surface, with such developments as show the intimate connection of the geometrical theory with the theory of multiply periodic functions.
2. The application of topological methods of the theory of algebraic loci, with the aim of obtaining geometrical results, besides theorems for the integrals associated therewith.
3. The general reduction of an algebraic surface to one without multiplicities, or an examination of a wide general class of surfaces.

Hodge's entry—its precise title is not known—was a connected account of his work that was rewritten over the next three years to form his celebrated work *The Theory and Application of Harmonic Integrals* [1941] (see [Atiyah, 1977, p. 107].

It lies beyond the scope of this paper to discuss the history of geometry at Cambridge after 1940, but it should be said that the situation following the war was very different, and topics that had been marginal there before fared better, and projective geometry less well than the situation in 1939 might have suggested. Topology, once represented by Max Newman and, to a lesser extent, by Du Val, and group theory, once the province of Philip Hall, blossomed as Cambridge showed itself able to innovate. Projective geometry, for all its apparent success, had not overcome the flaws built into it along with its glories in the 1910s, and these had been Italian accomplishments in the main. Despite Macaulay's example, the impact of modern algebra, all the work of Emmy Noether, van der Waerden, Krull, Zariski, Weil, and others, passed Cambridge by. The theory of algebraic surfaces was illuminated, certainly, but it was not enabled to escape the confines of the complex field in which it was studied, albeit in a synthetic spirit. After the War algebraic geometers abroad could see that Cambridge had little to offer, and the next generation of Cambridge mathematicians, most notably Michael Atiyah, a student of Hodge, followed Hodge's lead and plunged into topology. When the pressure built up to revise the syllabus and install key features of Bourbaki's vision of mathematics—much of it a response to the work of Hilbert and Emmy Noether in any case—projective geometry was a natural candidate for make way for the new mathematics. And that is what happened: projective geometry, along with hydrodynamics, moved into the area of specialist options and out of the core, mainstream provision. From there, it fell somewhat into disrepute, criticized for its various imprecisions, and made more unfashionable by its liking for particular cases when pure mathematics was reveling in a period of grand generalization. Times have changed again, and more highly regarded mathematics is being done these days on particular cases and in looking for examples. But whereas Hodge and Coxeter enriched geometry and connected it to other branches of mathematics, the projective geometers did not, and where their work has continued to have an influence it has also had to be modernized.

6. Conclusion

The history of geometry at Cambridge can be seen in different ways, depending on how it catches the light. A tradition that boasts the names of Cayley, Baker, Coxeter, and Hodge can seem illustrious, but it would be possible to argue that Cayley's contributions were less than the present weight of his name would suggest, while Baker's achievement was greater than his presently much lower reputation implies. Not only did he bring the theory of Abelian functions to Britain, but also in the second phase of his long, active life he cretaed a group of geometers in Cambridge that came close to rivaling the achievements of the group around Hardy and Littlewood in both Oxford and Cambridge, and which, dispersed across

Britain and beyond, did much to make geometry part of the modern pure mathematics that was otherwise heavily analytical. Yet, judged by the highest standards, Baker and his group did not succeed: there is no branch of the subject that was profoundly shaped by their work. Coxeter and Hodge may rather be said to have passed through on their way to somewhere other than the largely synthetic, heavily traditional domain identified by Baker. This suggests that for the historian the significance of geometry at Cambridge in the years 1863–1940 lies not in the isolated achievements of the best, but in the creation of a coherent group of mathematicians, who talked to each other and, most notably, to their Italian colleagues abroad, who actively promoted their subject at Cambridge and in the broader British setting, and who defined a subject which reached from its foundations to significant problems of research, and from intuitive, school-level mathematics to a place near the heart of the mathematical enterprise. For more than 60 years in the 20th century projective geometry was a defining part of mathematics in Britain, whether or not the mathematicians who placed it there had any spectacular discoveries to their names. The status of the subject has fluctuated ever since, as has its claim on the school syllabus, because the subject can seem less than rigorous and, even if rigorous, baroque.[64] These were features of the Cambridge group too, and contributed to its eventual eclipse, as higher-dimensional projective geometry was not seen to answer to the pressing concerns of structural mathematics or to its applications.

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[64] In this spirit, Beniamino Segre noted in his obituary of Roth that "new results—even when difficult and requiring hard work—could often appear (wrongly) to be mere exercises or trifling generalisations" [Segre, 1976, p. 196].

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