Algebra identified with geometry

Appendix III. On the History of Stigmatic Geometry

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généraux qui furent directement destinés à peindre les solutions imaginaires, ils sont trop indirect et trop compliqués pour devenir jamais admissibles." [But stigmatic geometry is more direct and less complicated.] "Tel est le jugement *final* qui convient à des spéculations dépourvues de direction philosophique, où l'on oublie le but, essentiellement géométrique de l'institution cartésienne. Elles manifestent une tendance absolue à développer isolément la peinture des équations quelconque au lieu de la subordonner à sa destination principale, comme élément nécessaire de la constitution propre à la géométrie générale." [But the chief ground on which I base the claims of stigmatic geometry to attention is that it is a simple geometrical idea carried out by a calculus based on the simplest geometrical relations — those of similar triangles. Hence Comte's "final judgment" is altogether premature and inapplicable.]

Appendix III.

On the History of Stigmatic Geometry.

The indulgence of the reader is requested for the following personal record of the various steps by which I have arrived at the general conceptions sketched above.

About fifty years ago my father taught me Euclid after Playfair, and Algebra from the "Elements of Algebra, by Leonard Euler, translated from the French; with the notes of M. Bernouilli, &c., and the additions of M. de la Grange, 3rd ed., by the Rev. John Hewlett, B.D., F.A.S., &c., to which is prefixed a Memoir of the Life and Character of Euler, by the late Francis Horner, Esq., M.P.," London, 1822, pp. xxx. 593. Although I fear I did not profit properly by such a book, it was something to have begun Algebra under the guidance of a mathematician like Euler. At present, his exposition of infinity, surds, negatives, imaginaries, geometrical ratio, &c., appears very defective. "Since all numbers which it is positive to conceive, are either greater or less than 0, or are 0 itself, it is evident that we cannot rank the square root of a negative number among possible numbers, and we must therefore say it is an impossible quantity. In this manner we are led to the idea of numbers which from their very nature are impossible; and therefore they are usually called *imaginary quantities*, because they exist merely in the imagination. . . . . Of such numbers we may truly assert that they are neither nothing, nor greater than nothing, nor less than nothing, which necessarily constitutes them imaginary or impossible."—*Ibid.* p. 43. (From this confusion to the clinant conception the reach was long.) About forty years ago I studied as a freshman Dean Peacock's *Algebra* 1830 (ed.), which first shewed me that the "imaginary" and "impossible" of other writers might become geometrically visible and possible. But I owe most at this period to discussions with my late friend Duncan Farquharson Gregory (fifth wrangler in 1837) from whom I derived the germ of the conception of *operation*, and not *quantity*, as the real meaning of algebraical expressions. About this time also I became acquainted with the works of Martin Ohm of Berlin, (*Versuch eines vollkommen consequenten Systems der Mathematik*, "An Attempt at a perfectly consistent System of Mathematics," in nine volumes, vol 1 and 2, second edition 1833, third edition 1853; vol. 9, 1852; *Der Geist der mathematischen Analysis, und ihr Verhältniss zur Schule*, "The Spirit of Mathematical Analysis, and its relation to a logical system," pp. 159, Berlin, 1842, translated and published by me in 1843; *Der Geist der Differential- und Integral-Rechnung, nebst einer neuen und gründlicheren Theorie der bestimmten Integrale*," "The Spirit of the Differential and Integral Calculus, with a new and more Fundamental Theory of Definite Integrals," Erlangen, 1846, which, together with many others of his nearly thirty volumes, I also translated, though I did not publish them),—and these occupied my thoughts for many years after I had taken my degree. Of course I mention no books of regular routine, to which belong Lagrange, and Lacroix, and Newton, &c., &c. But as yet I had hit upon no scheme for solving those difficulties of incommensurables and imaginaries, which even Ohm seemed rather to evade than to meet. In 1846 my thoughts were turned in another direction, and dropping mathematics altogether, I worked so hard at practical phonology that in 1849 I was completely prostrated, and remained for some three years incapable of head work. As I was recovering, I again recurred to my mathematical researches, and especially amused myself (I was not fit for real study) with Augustus De Morgan's *Trigonometry and Double Algebra*, 1849.

At last, at five o'clock in the morning of Palm Sunday, 20 March 1853, while residing at Redland, Bristol, I awoke suddenly with a conception of the application of algebra as a measure of quantity,—the germ of the algebra of proportion. This thought kept working in my brain for some months, till during a walk at Scarborough, on Saturday 13 August of the same year, I noted in my journal that I had "very satisfactorily arranged the whole subject in my own mind," but that as it had "helped to tire my head," I should "not even write it down." The next morning I awoke at six o'clock with the thought clear upon me, and I noted, "I have made a real discovery in mathematics, and have discovered the real theory of analytical geometry, shewing that Descartes's is only its simplest case." As it is not always possible to trace an original conception to the moment of germination, I trust that those who have had the patience to look through these pages, will not take it amiss, if I here continue the quotation from my journal, shewing the rough-hewn form of this conception; and, considering what an immense amount of labour was still required "to shape its ends," its very comprehensive nature. Regarding this statement as an historical document, I do not change a word, but I should mention that the Roman i represents $\surd(-1)$, equivalent to my present j.

"I will just register it here, in order to secure the date, and the germ of the idea, in case I should work it out further.

"Let u be a unit line, then if r, ρ be both real numbers, $re^{\rho \mathrm{i}}u$ represents a line of the length ru, and inclined to u at ρ radial angles, (2π radial angles = 4 right angles). This I have established long ago by a true process, but I have improved my method a little lately.

"If $f(x) = 0$, then the values of x are necessarily of the form $re^{\rho \mathrm{i}}$, and hence xu will represent a straight line on a plane; or its final terminal points. All the values, therefore, represent a series of disjointed points.

"If $f(x, y) = 0$, and any value x' of the form $re^{\rho \mathrm{i}}$ be put for x, an expression for y, $= y'$, of the same form $re^{\rho \mathrm{i}}$ will result. If then we take any function of x', y' as $F(x', y')$ the result will also be of the same form $re^{\rho \mathrm{i}}$, and therefore $F(x', y') \cdot u$ will represent a set of lines, or their terminal points. This is the most general case of geometry of two dimensions. To reduce it to Descartes's case, $F(x', y') = x' + y'$. As x' is of the form $re^{\rho \mathrm{i}}$, where r, ρ are independent, it can have an infinite number of values. Restrict them to those in which $\rho = n\pi$, therefore x' is of the form $\pm r$, or is possible. Putting these values, we get a series of values for y' which are possible or imaginary, and the result corresponds to a series of points as before. This is Descartes's case with imaginary values of y, perfectly explained. But to come to Descartes's case precisely, take $F(x', y') = x' + y' \cdot \mathrm{i}$, where x', y' are both of the form $\pm r$, then putting x for x' in the equation, $f(x, y) = 0$, we must only take the *possible* values of y which correspond to it, by the restriction already named. Substituting these in $x' + y' \cdot \mathrm{i}$, we determine a series of points by drawing two lines at *right angles* to each other, and marking the point of intersection. This is the case of rectangular co-ordinates. Oblique co-ordinates are easily shewn by taking $F(x', y') = x' \cdot e^{\alpha \mathrm{i}} + y' \cdot e^{\beta \mathrm{i}}$, where x', y' are possible. Now this is beautiful, and must be worked out well."

The result of this determination was a paper which I read before Section A of the British Association at Glasgow. (On a more General Theory of Analytical Geometry, including the Cartesian as a particular case, *Reports of Sections*, 1855.) Through reading this paper I became personally acquainted with Sir William Rowan Hamilton, and heard of his *Lectures on Quaternions* (Dublin, 1853), and also of Michel Chasles's *Traité de Géométrie Supérieure* (Paris, 1852), which, with De Morgan's writings, henceforward became my constant study. In 1858 I became acquainted with Julius Plücker's works (*Analytisch-Geometrische Entwickelungen*, "Developments of Analytical Geometry," Essen, Part 1, 1828; Part 2, 1831; *System der analytischen Geometrie auf neue Betrachtungsweisen gegründet*, "System of Analytical Geometry founded on new methods," Berlin, 1835; *Theorie der algebraischen Curven*, "Theory of Algebraical Curves," Bonn, 1839). To these works I had been led by a mere reference at the foot of a page in Salmon's *Treatise on Higher Plane Curves* (Dublin, 1852), when I discovered how inadequately his very original views had been introduced to English readers, and how much they had suffered in the transit. The immediate consequences were three papers which I read before the Royal Society (On the Laws of Operation and Systematisation of Mathematics, 20th May 1859; On Scalar and Clinant Algebraical Co-ordinate Geometry, 22nd May 1860; On an Application of the Theory of Scalar and Clinant Radical Loci, 14th March and 20th June 1861; abstracts are given, and the last is printed at full in the "Proceedings" for those dates). In all of these the above given original conception was worked out in considerable detail. But, finding that this conception was incomplete, as it failed to give a proper direct explanation of those relations between pairs of points on a straight line, and pairs of rays having a common point of issue, which form the subjects of Chasles's *Géométrie Supérieure*, I applied my conception of clinants, expressed in a geometrical form, imitating Chasles's notation, to obtain an explanation of the "imaginary" cases which, as is shown in Appendix II., caused him so much difficulty. The result of my first work, which was necessarily incomplete, and indeed contained some positively erroneous views on imaginary tangents, was another paper read before the Royal Society, (On Clinant Geometry, as a means of expressing the General Relations of Points in a Plane, realizing Imaginaries, reconciling Ordinary Algebra with Plane Geometry, and extending the Theories of Anharmonic Ratios, 26 Feb. 1863, abstracted in the "Proceedings,") in which I gave geometrical constructions for the imaginary intersections of real straight lines with real circles, and introduced the term *stigmatic*. But the first clear statement of the new conception implied by this word, was given in a short paper read before the British Association at Bath, (On Stigmatics, *Reports of Sections*, 1864,) and this fructified into two very long papers, almost books, presented by me to the Royal Society, (Introductory Memoir on Plane Stigmatics, 6 April 1865; Second Memoir on Plane Stigmatics, 7 June 1866; both abstracted in the "Proceedings," the last at considerable length; but the actual state of my conception at that time can be appreciated only by those who consult the original memoirs preserved in the archives of the Royal Society). At Nottingham I exhibited and explained some of the results of this theory to the British Association, by means of large diagrams, shewing the geometrical meaning of "imaginaries," in involution, homography, &c. (On Plane Stigmatics, *Reports of Sections*, 1866.)

Since 1866 I have been deeply engaged with my treatise on *Early English Pronunciation*, which has so severely taxed my strength that I have several times broken down for months together, but during part of 1871 I was able to revert to my Stigmatic Geometry, and lay its foundations firmly by establishing the laws of its operations (Tensors and Clinants) upon the Euclidean theories of Proportion and Similar Triangles, and to work out the complete geometrical conception of algebra here presented.

From time to time, also, I revised my stigmatic nomenclature. Professor H. J. Smith had pointed out to me at Nottingham that my terms "stigmatic point, stigmatic line, stigmatic circle," for the present "stigmal, primal, cyclal," were misleading, as the latter were not properly speaking points, lines, and circles at all. I had hoped that the prefixed qualification "stigmatic" would have prevented any confusion, and I was unwilling to give up the old Cartesian confusion, arising from neglecting the *index* and regarding the *stigma* only, for the particular directions of the abscissa and ordinate in the old algebraical geometry. But slowly the necessity of inventing a new terminology became evident, and the difficulties of forming one which should be brief, unambiguous, euphonic, and suggestive of the old Cartesian and Chaslesian usages, were extremely great. It was some time before I could reconcile my philological prejudices to the necessities of the case; but finally I was led to the conclusion that the science of the nineteenth century was not to be bound by the rude habits of compounding words which grew up among the old Aryans, who in pre-historic times originated the mother of our European tongues. The example of chloroform (ter-*chlor*-ide *o*-f *form*-yle), chloral (*chlor*-ine + *al*-um) and many other chemical names, and in mathematics Sir W. R. Hamilton's cis θ (*c*-osine of θ + *i*-maginary ante *s*-ine of θ, see art. 26. viii.) led me to see that modern science requires an adaptation of the North American Indian *incorporative* system of speech. No scientific word can possibly convey its compound meaning by the meaning of the simple constituents of its name (compare astro*nomy* and astro*logy*). This is especially true in mathematics. It is there absolutely necessary that the conception should be explained at length, and then *docketed* by a name. For memorial and historical purposes the name should have reference to some salient points in that conception, but it is sufficient that this reference should be made by *abbreviations* of the names of those salient points, and these abbreviations may be either initial letters (as the usual G.C.M. and L.C.M. for *g*-reatest *c*-ommon *m*-easure and *l*-east *c*-ommon *m*-ultiple), or distinctive syllables (as the usual tan, log, &c., for *tan*-gent, *log*-arithm, &c.) Again, for the usual language of science, words should be employed which could be readily adapted to our inflectional system and converted into any European language. Hence they should have a Latin or Greek basis. This is the origin of my present nomenclature, explained as it is introduced, of which the characteristic is the Latin termination *-al* (a remnant of gener-*al*-isation), which has the advantage of forming both substantives and adjectives, and admitting of many additions. This *-al* (except in a very few words, as *lateral*, *normal*) becomes distinctive of the stigmatic theory, while the preceding letters in each word allude to the particular historical case which has been generalised. I hope, therefore, that my philological friends will hold me guiltless of murdering English, and consider that I have rather endeavoured to systematise the application of our old-world Eastern speech to modern science, by taking a hint from the new-world Western tongues. This nomenclature is made public for the first time in these Tracts.

Simultaneously with this improvement proceeded another, namely, the complete adoption of an algebraical form for my geometrical conception, consequent on my having established that all the laws of commutative algebra held for the geometry of proportion and similar triangles, so that, in fact, it was impossible to form any algebraical expression which had not a geometrical meaning. The converse is not true. It is only that part of *plane* geometry which depends on the relations of similar triangles that can be brought under the laws of commutative algebra. *Solid* geometry requires additional conceptions, (as the directional addition of spherical arcs, which is *not* commutative,) and hence requires another algebra (*quaternions*), embracing that of *plane* geometry (*clinants*) as a particular case, while relations of length and opposition of direction of a straight line (*tensors* and *scalars*) belong to both algebras. There may be also (I think there are) parts of plane geometry depending on higher principles, which will develop new algebras, with new "imaginaries," not here considered. Hence I have termed the results of my long investigation, "Algebra identified with Geometry," and *not* "Geometry identified with Algebra," and in my title have especially shewn *what part* of Geometry can be considered as identified with Algebra. Those who read these pages are particularly requested to note this distinction.

On reviewing these Appendices, and especially I. and II., it will be seen that difficulties in algebra arose from its foundation on arithmetic, essentially discontinuous, and its application to geometry, essentially continuous. All these difficulties vanish as soon as a purely geometrical basis is given to algebra, as in my theory of clinants, by shewing it to be, in every one of its phases, nothing but the calculus of the operation of describing triangles directionally similar to the variable triangles determined by a fixed base and a vertex continuously moving over a fixed plane. But to apply this principle to geometry, it was necessary to shew that the theories of functions, and of both coordinate and homographic geometry, were all particular cases of the correspondence of two such movable points. This is the first aim of my Stigmatic Geometry. When such a conception has once been firmly grasped, mathematical analysis can only be regarded as the expression of certain definite and simple geometrical operations which can be always performed. That there should be purely analytical forms in ordinary algebra, without any geometrical significance whatever,—a proposition again and again insisted on, by the most eminent mathematicians, in the citations of Appendix II.,—becomes henceforth inconceivable. This is my own view of my own work.

As the last dates in this history, let me note that the first page of these Tracts was written on Friday, 6 March 1874; that an explanation of the principle involved was given before the Mathematical Society on Thursday, 9 April 1874; and that this last page was finally corrected for press on Wednesday 29 April 1874.

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