James Gregory's Mathematical Work: A Study based chiefly on his Letters.

By Professor GEORGE A. GIBSON.

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James Gregory was the third son of the Rev. John Gregory, minister of Drumoak, a small parish near Aberdeen. His mother was the daughter of David Anderson of Finzeach in Aberdeenshire, and related to Alexander Anderson, a friend of Vieta and a teacher of mathematics in Paris. Gregory is said to have received his first lessons in mathematics from his mother, but in due course he passed on first to the Grammar School and then to Marischal College, Aberdeen, where he graduated. In 1663 his *Optica Promota* was published in London, and he spent some time in that city after the publication of his book in the hope of securing facilities for constructing a telescope on the principles he had laid down in the *Optica*. His efforts were however unsuccessful, and he went to Italy where he continued his mathematical studies. After a residence of three years in Padua he returned to Scotland in 1668. In 1669 he was appointed to the Chair of Mathematics at St Andrews; in that position he had a busy and, as the years passed, a rather troubled life, so that he was glad to accept a call in 1674 to be Professor of Mathematics at Edinburgh where, as he says in a letter to a friend in Paris, "my salary is double and my encouragements much greater." His Edinburgh professorship was however very brief as he died in October 1675. An interesting sketch of his life is given by Agnes Grainger Stewart in *The Academic Gregories*, a volume of the "Famous Scots Series."

Gregory's mathematical writings, published in his lifetime, besides the *Optica Promota* are as follows:—

1. *Vera Circuli et Hyperbolae Quadratura in propria sua proportionis specie inventa et demonstrata:* Patavii, 1667.
2. A reprint of the *Quadratura* with an important addition, *Geometriae Pars Universalis, inserviens quantitatum curvarum transmutationi et mensurae:* Patavii, 1668.

Exercitationes Geometricae: London, 1668.

Nowadays the *Quadratura* is chiefly known through the fact that in it the phrase "converging series" is first used in its technical sense. The modern meaning is not quite the same as that assigned to it by Gregory; the term originally meant a pair of sequences which tend to the same limit. If u_n and v_n are corresponding terms of the sequences Gregory called u_n and v_n "converging terms" of the converging series, while he gave the name "termination of the series" to the common limit.

The *Quadratura* is however of great theoretical importance from the fact that one of its main purposes was to prove, in the language of modern mathematics, that the circular and logarithmic functions are not algebraic, and while it was inevitable that in the conditions of the mathematical science of that day the attempt should be unsuccessful it was a striking illustration of Gregory's scientific and philosophic grasp. The *Quadratura* aroused considerable interest and led to a controversy with Huygens who misunderstood Gregory's point of view. Gregory's answers to Huygens appeared in the *Philosophical Transactions*, vol. 3, pp. 732–735 and 882–886. The second answer is dated 15th December 1668; we shall see later the significance of this date.

The matter of the *Quadratura* is outside the scope of this article; interesting notices of the theoretical side of the treatise may be read in an article by G. Heinrich, *Bibliotheca Mathematica*, 1901, vol. 2, pp. 77–85, and in Zeuthen's *Geschichte der Mathematik im XVI. und XVII. Jahrhundert*, pp. 303–306. I may say in passing that Zeuthen seems to me to be the only modern historian of mathematics who has appreciated the value of Gregory's contributions, and even he seems not to have been aware of the material which exists in the Rigaud *Correspondence*.

The sources from which the greater part of this article is drawn are Rigaud's *Correspondence of Scientific Men of the Seventeenth Century*, volume 2, and the *Commercium Epistolicum D. Johannis Collins et Aliorum de Analysi Promota*. (References are to the edition of Biot and Lefort; Paris, 1856.)

In the *Commercium* there are numerous references to Gregory, but the extracts from his letters are tantalisingly meagre. Much fuller information is contained in Rigaud, but even there we have often to depend on Collins's extracts from Gregory's letters.

Some important letters are represented both in the *Correspondence* and in the *Commercium*, and while the substance in these cases is identical there are sometimes variations in numerical coefficients of the terms of a series. These variations are slight, and when an error occurs it is usually, though not always, in the Rigaud extract and not in the *Commercium*; the error may thus be due to a slip of Collins when making the extract rather than to Gregory, though I think this is not always the case. As a rule I shall not refer to slips of this kind. If the originals of Gregory's letters are in the possession of the Royal Society it would be of great historical interest to have an authentic version of them.

It may be well to note here that, so far as I can discover, Collins up to the date of Gregory's letter of 19th December 1670 had sent him nothing of Newton's except the series for the zones of a circle (*Com. Epist.* No. 15, p. 76); this fact must be carefully noted if a just judgment on Gregory's work is to be made. The statement that Gregory was in familiar intercourse with Collins on his first visit to London (*The Academic Gregories*, p. 28) seems to me to be totally inconsistent with the language of Collins's first letter (Rigaud, p. 178); if Collins had been on familiar terms with Gregory he would hardly have addressed him in the words "Sir, it was once my good hap to meet with you in an alehouse, or in Sion College." That *once* seems to me to indicate a casual meeting. Even Zeuthen (*Geschichte*, p. 361) suggests a connection with Newton through Collins that I consider, in the light of the Rigaud *Correspondence*, to be quite baseless. Gregory was scrupulously careful about claiming anything that was not demonstrably his own.

The Binomial Theorem.

The first theorem I take is contained in a series of extracts, said to be in Collins's handwriting, from a letter of Gregory, dated 23rd November 1670. (Rigaud, pp. 203–212). The letter must have been a long one, and it is certainly crammed with important results. Gregory says:—"I suppose these series I send you here enclosed may have some affinity with those inventions you advertise me that Mr Newton had discovered. It was upon this account I so often desired you to communicate the same unto me. I shall also give here an approximation for the sines."

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The theorem is involved in the following problem (p. 209) "To find the number of a logarithm" or, as it is more fully stated on p. 212, "Given a logarithm to find its number, or to change the root of any pure power whatever into an infinite series." The solution is as follows:—

Given b, $\log b = e$, $b+d$, $\log(b+d) = e+c$ it is required to find the number whose logarithm is $e+a$.

Take the series of continued proportionals

$$b,\ d,\ \frac{d^2}{b},\ \frac{d^3}{b^2},\ \text{etc.},$$

and another series

$$\frac{a}{c},\ \frac{a-c}{2c},\ \frac{a-2c}{3c},\ \frac{a-3c}{4c},\ \text{etc.};$$

let $\frac{f}{c}$ be the product of the first two terms of the second series, $\frac{g}{c}$ that of the first three, $\frac{h}{c}$ that of the first four, $\frac{i}{c}$ that of the first five, and so on. The number will be $e+a$ where

$$e+a = b + \frac{ad}{c} + \frac{fd^2}{cb} + \frac{gd^3}{cb^2} + \frac{hd^4}{cb^3} + \frac{id^5}{cb^4} + \frac{kd^6}{cb^5} + \text{etc.}$$

Now if we put their values for f/c, g/c, etc., the series is

$$b + \frac{a}{c}d + \frac{a(a-c)}{c\,.\,2c}\frac{d^2}{b} + \frac{a(a-c)(a-2c)}{c\,.\,2c\,.\,3c}\frac{d^3}{b^2} + \text{etc.},$$

and this is simply $b\left(1+\frac{d}{b}\right)^{\frac{a}{c}}$. That this is the solution desired is obvious since

$$\log\left\{b\left(1+\frac{d}{b}\right)^{\frac{a}{c}}\right\} = \log b + \frac{a}{c}\left\{\log(b+d) - \log b\right\} = e+a.$$

Gregory adds the remark, "hence with a little work but without difficulty any pure equation whatever may be solved."

Immediately after this problem there is another. "An example was desired about finding the first of 364 means between 100 and 106." The solution is:—put $b=100$, $d=6$, $a=1$, $c=365$, and the series required (which I need not write down) is the expansion of

$$100\left(1+\frac{6}{100}\right)^{\frac{1}{365}}.$$

The terms are correctly expressed as vulgar fractions, but some of the decimals are incorrect. Whether the errors are due to Gregory or to Collins is not quite clear, but the important point is that we have here the Binomial Theorem in its most general form. The method of stating it by the use of the fractions f/c, g/c, etc., is characteristic of the period, and is due chiefly to an inadequate algebraic notation. The phrase "an example was desired" suggests that Gregory had mentioned to Collins that he also had, like Newton, some theorems on series, and the suggestion is confirmed by some remarks of Collins in a preceding letter (Rigaud, p. 201).

Interpolation Formula.

The next extracts I take deal with the theory of Interpolation. In the *Exercitationes Geometricae* Gregory had described a method of constructing tables of logarithmic tangents and secants that was essentially an application of what is now known as Simpson's Rule for approximate integration. There is an interesting note on this passage of the *Exerc. Geom.* by G. Heinrich in the *Bibliotheca Mathematica*, 1900, vol. 1, pp. 90–92; Heinrich shows that Gregory's values when third differences are constant would not be correct for the general case. Gregory himself, however, had discovered the inadequacy of his method and gives a new rule. This rule is itself only a special case of a general theorem which he at once states (Rigaud, p. 209). In modern notation his theorem is that when $f(b)$ and the successive differences $\Delta f(b)$, $\Delta^2 f(b)$,... are known the value of $f(x)$ is given by the series

$$f(x)=f(b)+\frac{x-b}{h}\Delta f(b)+\frac{x-b}{h}\cdot\frac{x-b-h}{2h}\Delta^2 f(b)$$
$$+\frac{x-b}{h}\cdot\frac{x-b-h}{2h}\cdot\frac{x-b-2h}{3h}\Delta^3 f(b)+\text{etc.}$$

He gives two examples; one of these (Rigaud, p. 211) shows clearly the method of applying his theorem. The example is to find the cube of 23. Here $b=10$, $x=23$, and the successive differences formed from the cubes of 10, 15, 20, 25, 30, are

$$\Delta f(10)=2375,\ \Delta^2 f(10)=2250,\ \Delta^3 f(10)=750$$

whence $f(23)=1000+6175+4680+312=12167.$

The other application is to the problem of the *Exercitationes*. He has to find the area under the curve of tangents, where (*Exerc. Geom.*, p. 25) "the arc of the quadrant is extended into a straight line" (Note Pascal's phrase). The successive differences of the ordinates, taken for equal short intervals of arc, are supposed to be calculated and the value of $f(x)$ deduced by the theorem. This value is then integrated, as we should now say, with respect to x. The result obtained is correct except that in the denominator of his fifth term he has 164 instead of 160.

Gregory evidently thought that this theorem was valuable as he refers to it at least twice in this correspondence (pp. 230, 268). In the first of these passages he says:—"I wonder how ye speak yet of interpolation by the help of figurate numbers, seeing I sent you, a long time ago, a method much shorter and readier by a series." But his claims for his theorem are much more emphatically expressed on the page in which the application to quadrature is made, for he adds:—"However the differences be affected I can easily square the figure, and by this means all figures imaginable. I cannot also but advertise you that Mr Mercator's quadrature of the hyperbola is a consectary of this." It is a pity that he did not develope the method here referred to more fully; he may only refer to approximate quadrature, but a clear and detailed exposition would have been valuable. It would be going too far to assume that the series had blossomed out into Taylor's series.

Sines of Multiple Angles.

I now consider the theorems for the sines. In a later letter (Rigaud, p. 259) he refers to these, and I quote the passage because it shows that Gregory had a just estimate of their value for other investigations than those connected with the mensuration of the circle. He there says:—"I admire that ye fancy any difficulty in the cubic equation of three roots, seeing Des Cartes long since hath reduced it to the trisection of an angle, and the trisection of an angle can be turned infinite several ways* into an infinite series; some of which methods I sent you long ago, not only in trisecting an angle but also dividing it in a given ratio."

In Gregory's work the trigonometrical functions are lines, not ratios, but I shall use the modern definitions so that "the whole sine" is unity. The first expression is a series for $\sin \frac{a\theta}{c}$ (Rigaud, p. 206); given $\sin \theta = d$, $\sin 2\theta = 2d - e$ a series for $\sin \frac{a\theta}{c}$ in powers of e/d, that is of $2(1-\cos\theta)$, is stated. As with the Binomial Theorem he uses abbreviations for the products of certain factors, but it is simpler to present the series without such abbreviations and his formula is thus

$$\sin \frac{a\theta}{c} = \sin\theta \left\{ \frac{a}{c} - \frac{a(a^2-c^2)}{3!\,c^3}\,\frac{e}{d} + \frac{a(a^2-c^2)(a^2-4c^2)}{5!\,c^5}\,\frac{e^2}{d^2} - \frac{a(a^2-c^2)(a^2-4c^2)(a^2-9c^2)}{7!\,c^7}\,\frac{e^3}{d^3} + \text{etc.} \right\}$$

As an example he takes $a = 1$, $c = 225$.

Another formula is for $\cos a$. Here he has not expressed the coefficients in what would now be considered their simplest form; each numerator is a sum of two terms with a number of common factors, but it is simpler to state each numerator as a product, which is easily done. Thus we have the formula (Rigaud, p. 204)

$$\cos a = 1 - \frac{a^2}{2c^2}(2e) + \frac{a^2(a^2-c^2)}{4!\,c^4}(2e)^2 - \frac{a^2(a^2-c^2)(a^2-4c^2)}{6!\,c^6}(2e)^3 + \frac{a^2(a^2-c^2)(a^2-4c^2)(a^2-9c^2)}{8!\,c^8}(2e)^4 - \text{etc.},$$

where $e = 1 - \cos c$ and a, c are stated to be acute angles.

There is another formula (Rigaud, p. 207) but it is of a very complicated character and I do not reproduce it.

It is easy to reduce these series to the forms now used; for example, in the series for $\cos a$ let $c = 2\theta$, $a = mc$ and we have the

* That is, "in an infinite number of different ways," a peculiar idiom of Gregory's that occurs more than once.

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series for $\cos 2m\theta$ in powers of $\sin \theta$. The use of the versed sine $1-\cos c$ or $1-\cos\theta$ is due, I think, to the applications to the mensuration of the circle which absorbed so much of Gregory's attention.

Gregory gives no hint about the method by which he was led to these series. I do not think he established them in the way in which Newton found his series for $\sin nx$ (*Com. Epist.*, p. 106) or, at least, in the way in which De Moivre proved Newton's expression (*Phil. Trans.* 1698, vol. 20, pp. 190–193); Newton, like Gregory, gave no proof, though it is highly probable that his method was that of De Moivre. I am inclined to think that Gregory started from Vieta's theorems on the sines and cosines of multiple angles the proofs of which had been given by his relative Alexander Anderson, and that he proceeded as James Bernoulli did later (*Opera*, vol. 2, pp. 921–928) though adhering to Vieta's formulae more closely than Bernoulli did. Bernoulli in fact replaced Vieta's formulae by others. If Gregory had hit on the correct form for small integer values of the multiple his generalisation of the Binomial Theorem would suggest the final form of his series. To what extent his proof was rigorous it is impossible to say, but he used his expressions unhesitatingly in his investigations. His long concentration on the problem of the circular arc must in any case have led him to develope his trigonometry.

Series for the Mensuration of the Circle.

In reference to these formulae he says (Rigaud, p. 205):—"I have been more large in the approximations to the sines, and numbers of logarithms, because I suppose the former are more unknown. However these approximations to the arches I hint at are the same that I mentioned in my last answer to Hugenius." The date of that answer, as has been stated, is 15th December 1668, and in that answer he says regarding certain approximations (*Phil. Trans.*, vol. 3, p. 886) "these seem trivial to me since I can exhibit approximations which differ from the semi-circumference itself by an amount that is less than any assigned part of it, and they seem to me no longer to be wonderful since a sound demonstration is known." Of these approximations he now gives a specimen (Rigaud, p. 205). If r is the radius, d half the side of the square inscribed in the circle, and e the difference between the radius and the side of the square, the semi-circumference will be given by $4r^2$ divided by the series

$$2d - \frac{e}{3} - \frac{e^2}{90d} - \frac{e^3}{756d^2} - \frac{23e^4}{113400\,d^3} - \frac{260e^5}{7484400d^4} - \text{etc.},$$

"and the series may easily be produced so that it shall differ from the semi-circumference by an amount that is less than any assigned part of it; indeed, an infinite number of such series may be exhibited without any trouble." (The above series is reproduced in *Com. Epist.*, No. 36, p. 93, in a letter which is apparently derived from this passage in Rigaud.)

Of such series Gregory gives several in the letter from which I have extracted so largely. Thus after his formula for $\sin \dfrac{a\theta}{c}$ he adds (Rigaud, p. 207): "Hence too it may be found without difficulty that if r = radius, c = arc, $d = \sin c$, $2d - e = \sin 2c$, $v = \text{versin}\, c$, $t = \text{versin}\, 2c$

$$c = rv\Big/\left(\frac{d}{2} + \frac{e}{24} + \frac{11e^2}{1440d} + \frac{191e^3}{120960d^2} + \text{etc.,}\right)$$

$$= rt\Big/\left(2d - \frac{e}{3} - \frac{e^2}{90d} - \frac{e^3}{756d^2} - \text{etc.}\right).$$

There are other expressions of a similar kind which need not be transcribed. The peculiarity of all is that the series occurs in the *denominator* of the expression for the arc. The form seems somewhat strange, and it is not easy to see how he was led to adopt it or how he proved it. I offer a suggestion of a possible method of proof, but it would be absurd to lay any stress upon it. I think that Gregory, partly from a study of Pascal, partly from applications of the processes developed in the *Geometriae Pars Universalis*, was quite familiar with what we should now call the integrals of the sine and cosine (Cp. *Exer. Geom.*, p 23, at end of Prop. V.) A possible procedure, stated in modern symbolism, would be as follows:—First

$$\int_0^1 \sin ct\, dt = \frac{1 - \cos c}{c};$$

then by the series for $\sin ct$ $\left(\text{putting } t \text{ for } a/c \text{ and } c \text{ for } \theta \text{ in } \sin\frac{a\theta}{c}\right)$

$$\int_0^1 \sin ct\, dt = \sin c\left\{\tfrac{1}{2} + \frac{e}{24d} + \frac{11e^2}{1440d^2} + \frac{191e^3}{120960d^3} + \text{etc.}\right\}.$$

If we equate these values of the integral and put unity for the radius r we obtain the expression in terms of v or 1 − cos c. The expression in terms of 1 − cos 2c may be found in the same way from the equation

$$\frac{1-\cos 2c}{c\sin c} = 2\,\frac{\sin c}{c} = 2\int_0^1 \cos ct\, dt.$$

I have verified all Gregory's formulae for an arc of a circle, as given in Rigaud, by applying this method and find that, apart from occasional arithmetical errors, they are correct.

The various results that have been given up to this point are all taken from the letter of 23rd November 1670, and they are in themselves sufficient to establish Gregory's reputation as a great original thinker. Yet not one of these, except the single expression for an arc of a circle given in the *Commercium Epistolicum* (No. 36, p. 93), has ever been noted by a historian of mathematics; they have been preserved in Rigaud's *Correspondence*, but apparently have not been read. When I wrote the article on the Newton-Leibniz controversy which appeared in volume 14 of our *Proceedings* I was strongly impressed by Gregory's work, but I had not the necessary leisure to examine it carefully and it is only quite recently that I have found the opportunity of making a careful study of these letters. One is puzzled to know why Collins did not take steps to have the more important results, such as the Binomial Theorem and the Interpolation Formula, inserted in the *Philosophical Transactions*. Gregory gave him full permission to show his letters to anyone he pleased. "You need be in no strait to communicate what I send you to any person; for I am not at all anxious whether it be published under my name or that of any other" (John Stewart's *Quadrature of Curves*,* p. 359; *Com. Epist.*,

* Stewart, in the passage from which this extract is taken, gives some details respecting an examination of Gregory's papers (which were then in the custody of Dr David Gregory, Canon of Christ's Church, Oxford) made by himself and another gentleman. They "saw several curious ones upon particular subjects which are not in Print," but they found no treatise of the kind referred to by Newton in his second letter to Oldenburgh (*Com. Epist.*, p. 127).

No. 20, p. 80). Several of the theorems are referred to in document No. 36 (*Com. Epist.*, p. 95), but not in such a way as would enable a reader to grasp their real meaning or to apply them in practice. It is one thing to say that Gregory had a method of finding the root of any pure power so that he could without the help of logarithms find the root or obtain any mean proportional between unity and any given number; it is quite a different thing to state the series by which this could be effected. Had a selection from Gregory's results been communicated to the Royal Society (and the *Transactions* of the day contain much that is of far less value) Newton might have been induced to publish some of the very remarkable manuscripts he possessed which did not see the light of day till a much later date when their contributions to mathematics had been largely forestalled. A paragraph at the end of the letter on page 95 of the *Commercium* (of date 15th April 1675) intimates that Gregory, after he knew more of what Newton had done, was reluctant to publish his results on series in the belief that Newton had been first in the field and should have the credit of making known his discoveries to the mathematical world. This attitude of Gregory is a testimony to his generous and unselfish character; at the same time his reluctance to publish was unfortunate.

I now come to the famous letter of 19th December 1670. (Rigaud, p. 212; *Com. Epist.*, No. 18, p. 77.) "Sir, In my last to you I had not taken notice that Mr Newton's series for the zones of a circle (which you sent me a long time ago) together with an infinite number of series of the like nature may be a consectary to that which I sent you concerning logarithms, viz., given a logarithm, to find its number, or, to change the root of any pure power whatever into an infinite series. I admire much my own dulness that in such a considerable time I had not taken notice of this; nevertheless that I had taken much pains to find out that series. But the truth is, I thought always (if so be it were a series) that I might fall upon it by some combination of my series for the circle, seeing I had such infinite numbers of them, not so much as once desiring any other method."

Then to show that he thinks his conjecture to be correct he continues the series to other three terms, and adds the series for $\sin^{-1}x$, with the remark "I could give you several other series of

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this nature but perchance you know more of them than myself." (Collins did not send Newton's series for $\sin x$ and $\sin^{-1}x$ till the letter of 24th December 1670 (*Com Epist.*, No. 19, p. 78), that is, till after the dispatch of the letter just quoted.)

This letter seems to me to be instructive. The phrase "may be a consectary" is conclusive, apart from the other evidence, that Gregory had nothing to go upon but the series itself; he confirms his conjecture "by producing the series a little further." Again, while the frequent references in the *Commercium Epistolicum* have as their refrain the excessive difficulty Gregory had in hitting on Newton's method, his own attitude is quite different. He does not write as if he were elated at his success, or as if he had done something remarkable; rather he wonders at his own dulness in not hitting upon the solution much sooner. And well he might, because it is very obvious that he had all the material for the solution under his hand. The difficulty lay in his absorption in his own methods of handling the problem of the circle and hyperbola. When he went further afield he applied the Binomial Theorem as soon as the problem was reduced to one of quadrature. Thus in this letter of 19th December he says (Rigaud, p. 214), "I promised once to give you the proportion between a right line and a logarithm curve, which is this." He then shows that the arc is measured by the area under the graph of $\sqrt{(1+r^2/x^2)}$ and gives the value of this area from $x=b$ to $x=c$ as an infinite series. In a later letter (Rigaud, p. 227) he says, "I gave you also only one series for the measure of the logarithmic curve which I do not remember; and therefore in case I give you the same over again I shall give you two to complete the measure of it." He then gives the same series as before, but adds another "to complete the measure," that is, while the first series supposed r to be greater than c the second supposes r to be less than b. In other words, he was aware of the conditions, as we should now say, necessary for the convergence of the series.

The letter of 19th December 1670 may be said to mark a stage in Gregory's development; he appears now to abandon his peculiar forms for the circular arc, to discard the old language of proportion in stating results, and to adhere to the series derived from the Binomial Theorem. But it is only fair to note how much he had already accomplished. The Binomial Theorem in Newton's general form, the expression for $f(x)$ in terms of Finite Differences by a theorem usually assigned to a much later period, series for the sine and cosine of multiple angles, and numerous series for the circular arc: these constitute a remarkable contribution to mathematics as the science then stood. It is certain that Newton had discovered the general Binomial Theorem before 1669 when the *De Analysi* was sent to Collins, but the series itself was only communicated in the first letter to Oldenburgh in 1676; it is equally certain that Gregory communicated it to Collins in 1670. I do not think there is any doubt at all, and Gregory himself fully believed, that Newton had anticipated him; but had Gregory been alive when the *Commercium Epistolicum* was published I think he would have had something to say about the numerous references to his difficulty in discovering Newton's *method of series.* He had discovered the Binomial Theorem, applied it in quadratures or integration, and produced the series for the logarithmic arc quite independently; his sole difficulty lay in a mathematical transformation, not in the method of series which he had already discovered for himself. The contention that the communication to Gregory of the series

$$2\,RB - \frac{B^3}{3R} - \frac{B^5}{20R^3} - \frac{B^7}{56R^5} - \frac{5B^9}{576R^7} - \text{etc.},$$

for the area of a zone of a circle of radius R and width B enabled him to discover Newton's *method of series* is too absurd; yet it is this contention that runs through the *Commercium.* Surely Document No. 36, and especially the summary on page 95, were enough to suggest that Gregory had been working quite independently in the field considered to be peculiarly Newton's own, and had made discoveries that Newton too had made but that had been sent to Collins before Newton's. Collins was a man to whom the mathematical workers of his day were deeply indebted, but I think he failed to understand the significance of the communications made to him either by Newton or by Gregory. Had he done so I am sure that Gregory's most important theorems would have appeared in the *Philosophical Transactions,* and these would almost certainly have called forth Newton's.

It is necessary to give some indication of the contents of the *Geometriae Pars Universalis* before I take up the discussion of some of the remaining letters. The book has received very little attention from historians and yet it has great merits.

During the first half of the 17th century, especially after the publication in 1637 of Descartes' Geometry, mathematicians had given great attention to the drawing of tangents to curves defined by an equation, to problems of rectification, quadrature and cubature, to the determination of centres of gravity, to the methods of finding maxima and minima, and the like. Many valuable results had been established, but no clear general principle had been evolved; the tendency was rather to attack each new problem by a method that was often peculiar to the problem or of very limited scope.

In a very interesting preface to the *Geometria* Gregory discusses briefly this tendency, and states that he has written the "Tractate" in the conviction that it was possible to state, particularly in the case of quadratures, certain general theorems depending on some essential property but readily applicable in particular cases. "It will be both shorter and more elegant," he says, "to apply the general theory to any particular case as the special properties of the figure suggest than to publish a whole volume about a single figure." He adds that he had found many traces of such a method in the writings of eminent geometers, but that the demonstrations were too often not general or not geometrical, and goes on to say—"what is mine and what is the property of others let the reader judge who compares my Tractate with the writings of other men; I make no claims lest I should seem to ascribe to myself what has been previously discovered, even though I was unaware of it."

That Gregory had read widely and to some purpose is evident on every page, but his indebtedness to any particular writer is not so easy to determine. He says that in the more obvious propositions he uses the method of Cavalieri, but on the other hand his chief purpose is to establish his propositions with geometric rigour, and therefore that method is, at most, suggestive. He seems to have been familiar with Schooten's edition of *Des Cartes* and the companion tracts and letters, and his fundamental proposition (Prop. 2) has close relations with Van Heuraet's letter on rectification. A more potent influence seems to me to be exercised by Fermat. Though Fermat published little or nothing in his own name yet some important contributions of his appeared in his lifetime; the *Dissertatio Geometrica* and the *Ad Laloveram Propositiones* were published in 1660, and his method for maxima and minima, and for drawing tangents to curves, appeared in Hérigone's *Supplementum Cursus Mathematici* (vol. 6 of the *Cursus*) which was published in 1644.

Gregory's exposition is heavily handicapped, for modern readers at any rate, by his practice of stating results in the form of proportions, and by the complete lack of any tolerable algebraic symbolism. In this respect the contrast with Newton's *De Analysi* is very striking. At bottom Gregory's reasoning is singularly clear, but it is obscured by the cumbrous phraseology in which it is expressed; the language of the ancient geometry was in fact intolerably prolix, and wholly unsuited to the new developments to which his own work was contributing so largely.

It would take me too far to give anything like a detailed account of the *Geometria*. The best description I can give of it is that it aims at providing a method of reducing the rectification of curves, the mensuration of the surface and volume of solids and the determination of centres of gravity to the evaluation of the area under a curve, and establishes some general theorems that are useful in the evaluation. In modern language one may say roughly that he reduces a problem to the evaluation of an integral. The actual evaluation however often depends on a "transmutation" that is limited to the particular case; this transmutation frequently takes the place of what in the case of integrals would be called a change of variable, and is usually ingenious.

Gregory gives unusual prominence to problems that depend on the arc of a curve, and the first proposition is a careful discussion, on Fermat's lines, of the length of a curve; upper and lower limits are determined that play an essential part in many proofs. The second proposition is fundamental, and may be stated in modern language without sacrificing the conception it embodies.

Let A, P, B be the points (a, a'), (x, y), (b, b') on the curve given by the equation $y = f(x)$. Suppose a line of length h to move along the curve, remaining always at right angles to the plane of the curve and thus generating a cylindrical surface. It is required to find the area of this surface, and the solution is as follows:— Let the tangent and normal at P meet the x-axis at T and G respectively and produce MP (or y), the ordinate at P, to Q so that $MQ = PG$. Next take R on MP or MP produced so that MP is to MQ as h is to MR, and suppose the locus of R to be drawn. If R

passes from C to D as P moves from A to B the area under CRD measures the surface of the cylinder. Or, in modern notation,

$$MR = h\sqrt{\left\{1+\left(\frac{dy}{dx}\right)^2\right\}},\ \text{surface} = \int_a^b h\sqrt{\left\{1+\left(\frac{dy}{dx}\right)^2\right\}}dx.$$

The curve APB is supposed to be such that the ordinate MP either steadily increases or steadily decreases, or else can be divided into portions that satisfy this condition of "monotonic variation." The proof consists in showing that the surface can be neither greater nor less than the area under the curve; it is quite sound, but it belongs to the class that can only be given when the theorem to be established has been previously suggested. There can hardly be any doubt that the suggestion came from the substitution of the chord of a small arc, or of a short length of the tangent, for the small arc itself; or, in modern notation, ds/dx was taken equal to PG/MP. Gregory's conception of rigour, however, demanded an "apagogic demonstration."

Again the length h is not a general function of x. In the 2nd proposition it is a constant, thus providing a theorem for the length of a curve;* in the 3rd and 4th it is proportional to y, but in the 4th the theorem is adapted to integration with respect to y ($yds = PT \cdot dy$). In the *Exercitationes* the curve APB is a quadrant of a circle, and by taking h equal to the secant and the tangent respectively he finds the integrals of these functions. In Props. V. and VI. of the *Exercitationes* he has applications to the conchoid and the cissoid of Diocles, and introduces as an auxiliary curve the curve that was afterwards called the *versiera* or *witch* ($x(y^2 + a^2) = a^3$); incidentally (Prop. V.) he uses the integral of $\sin x$. Gregory is remarkably ingenious in the use he makes of these theorems; in later propositions of the *Geometria* he applies them to the determination of volumes of surfaces of revolution.

While his fundamental theorem thus lacks generality, since he finds it necessary to give a new proof for each new value of the function h, it is to be noted on the other hand that he considers carefully the case of an infinite ordinate, and shows how the

* This case is in substance the same as the proposition given by Van Heuraet in his letter to Schooten.

theorems apply in dealing with centres of gravity. His systematic reduction of integrals with respect to the arc, to use modern language, to ordinary quadratures was a definite step in the theory of integration, and his treatment is very clearly carried out.

For the application of the theorem a knowledge of the normal (or subtangent or gradient) is necessary, and Proposition 7 supplies the need "in the case of those curves which Descartes calls geometrical." The curve actually taken is

$$a^3 y^3 = c^3 (x^3 + ax^2).$$

In substance, Gregory calculates $\Delta y/\Delta x$, and after reduction puts Δx equal to zero; he thus obtains the ratio of y to the subtangent.* It is clear he had qualms about "coincident points." He says:—"We suppose the ordinate DG to meet the curve in the same point as the tangent FH *if only it be possible*"; G and H are the points on the curve which, in a later terminology, "ultimately coincide." The subtangent is often required and Gregory always quotes this proposition for its value, though in fact that value has to be calculated on the lines of the proposition as model—a calculation not at all unfamiliar in the case of simple curves.

After two propositions, useful for the discussion of cycloids and suggestive of Fermat, he establishes a theorem that may be stated in the form

$$\int_a^b y\,dx = \int_{a'}^{b'} TM\,.\,dy = \int_{a'}^{b'} y\frac{dx}{dy}\,dy$$

where (a, a'), (b, b') are the end points of a curve, and TM is the subtangent at (x, y). By means of this transformation he gives (Prop. 54) a beautiful proof of the quadrature of $a\,x^{p/q}$. Barrow quotes this Theorem, but presents it in a slightly different form (*Lect. Geom.*, XI., 10).

In the series of propositions 46 to 62 he discusses the mensuration of the surface of paraboloids and hyperboloids of revolution and of spheroids, and rectifies parabolic arcs. Of special interest is Prop. 58 in which he rectifies the curve

$$ay^{2n} = x^{2n+1};$$

* He uses the symbol o for Δx and calls it "nihil seu serum o." Is *serum* a misprint for *merum* or is it a form of *zero?* In the demonstration o is treated as an ordinary number until the last stage.

he works out in detail the case for $n=2$ (where the term $512lba^3$ should be doubled), and states that the result holds for every integral value of n. To what extent he verified the statement is not clear; his defective symbolism would make the work tedious and hard to print, but I think the proof was well within his competence. A generalisation of Prop. 62, stated without proof, is however incorrect.

In the investigation of the cycloids he applies his theory of "involutes and evolutes," but these terms have a totally different meaning from that of the present day. If $y=f(x)$ is the equation of a curve in Cartesian coordinates the "involute" (*figura involuta*) is determined by the transformation that would now be expressed, in the form, r and θ being polar coordinates,

$$y=r,\ y\frac{d\theta}{dx}=1$$

so that the involute is a curve given by polar coordinates. The original curve is called the "evolute" (*figura evoluta*) of the involute, and the area of the involute is half the corresponding area of the evolute. Barrow has some simple applications of this transformation (*Lect. Geom.*, XII., App. 3, Probl. 9, 10). Gregory's theory is a systematisation of previous practice.

In the course of the Tractate Gregory covers practically the whole field of quadratures as known at the time of writing, with the related applications to volumes and centres of gravity.

It is perhaps worth noting that in the Preface to the *Geometria* Gregory defines the exponential curve $y=ak^x$, shows how to construct it by points and states one or two properties, among them the following, viz., that the area between the curve and the x-axis to the left of any ordinate though "infinite in length" is finite in measure.

A study of the *Geometria* and the *Exercitationes* is quite sufficient to prove that the period of preparation for the Differential and Integral Calculus was near its close. Gregory's work alone, particularly his Proposition XI. and his constant use of the subtangent in problems of quadrature, shows how the two processes that we now call differentiation and integration were becoming associated; his exposition, in spite of the heavy handicap of his geometrical presentation and lack of appropriate algebraic methods, gives a clear conspectus of the principal results that had been established (and these were numerous) and of the general principles by which they might be coordinated. Had Gregory considered the integral curve instead of the curve, the area under which provided his solution, he might have anticipated Barrow; but he did not, and it was Barrow who first clearly stated the essential connection between the integral curve and the curve whose area it represents. At the same time it must, I think, be admitted that the full implications of Barrow's theorems were not obvious to his contemporaries, and that it required the genius of Newton and Leibniz to present the subject in such a way as created a new instrument that revolutionised the whole theory and practice of quadrature.

I now return to the *Correspondence* and I think that in the letters to be dealt with one can trace the influence of Barrow. A letter from Gregory, of date 5th September 1670, intimates the receipt of Barrow's *Lectiones Geometricae* and the delight with which he had read them. "I find that he excels in an infinite degree all who have ever written on these matters." (*Com. Epist*, No 16, p. 77). It is plain that he studied the book seriously for he adds, "by combining his methods of drawing tangents with some of my own I have found a general and geometrical method of drawing tangents to all curves without calculation; it includes not merely Barrow's particular methods, but his general analytical method given at the end of the tenth Lecture. My method is contained in not more than twelve propositions." What this method is we do not know; apparently the manuscripts in which it was developed are not to be found. (Did Gregory miss the implications of Prop. XI, Lect. X.?)

The letter of 15th February 1671 (Rigaud, p. 216; *Com. Epist.*, No. 20, p. 79) is, next to that of 19th December 1670, by far the best known; it figures prominently in the Newton-Leibniz controversy and is the source of "Gregory's Series." A list of seven series is given. If radius $= r$, arc $= a$, tangent $= t$ and secant $= s$ then

$$\text{(i)} \quad a = t - \frac{t^3}{3r^2} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \frac{t^9}{9r^8} - \text{etc.};$$

(ii) $t = a + \frac{a^3}{3r^2} + \frac{2a^5}{15r^4} + \frac{17a^7}{315r^6} + \frac{62a^9}{2835r^8} + \text{etc.};$

(iii) $s = r + \frac{a^2}{2r} + \frac{5a^4}{24r^3} + \frac{61a^6}{720r^5} + \frac{277a^8}{8064r^7} + \text{etc.}$

Next let log tan $a = t$, log sec $a = s$ and quadrant of the circle $= q$, $2a - q = e$; then

(iv) $s = \frac{a^2}{2r} + \frac{a^4}{12r^3} + \frac{a^6}{45r^5} + \frac{17a^8}{2520r^7} + \frac{62a^{10}}{28350r^9} + \ldots\text{etc.};$

(v) $t = e + \frac{e^3}{6r^2} + \frac{e^5}{24r^4} + \frac{61e^7}{5040r^6} + \frac{277e^9}{72576r^8} + \text{etc.}$

If log sec $45° = s$ and log sec $a = s + l$ then

(vi) $a = \frac{1}{2}q + l - \frac{l^2}{r} + \frac{l^3}{3r^2} - \frac{7l^4}{3r^3} + \frac{14l^5}{3r^4} - \frac{452l^6}{45r^5} + \text{etc.};$

(vii) $2a - q = t - \frac{t^3}{6r^2} + \frac{t^5}{24r^4} - \frac{61t^7}{5040r^6} + \frac{277t^9}{72576r^8} - \text{etc.}$

"You shall here take notice that the artificial radius (*i.e.* log r) = 0, and that when you find $q > 2a$, or the artificial secant of 45° to be greater than the given secant, to alter the signs and go on in the work according to the ordinary precepts of algebra."

To express these in modern form let $r = 1$, $q = \pi/2$. Gregory does not state how he derived the series, but it is highly probable that he obtained (i) by using the relation

$$a = \text{area under the graph of } \frac{r^2}{r^2 + t^2} = \int_0 \frac{r^2 dt}{r^2 + t^2},$$

and that (ii) was obtained by reversion of (i), while (iii) was derived from $s = \sqrt{(r^2 + t^2)}$. Again (iv) may be found by integrating (ii); (v) by putting e for a in (iii) and integrating. The series (vi) and (vii) were probably obtained by reversion of (iv) and (v).

David Gregory in his *Exercitatio Geometrica* (Edinburgh: 1684) gives several of the series that occur in the *Correspondence*, and among them (p. 41) the series for $\tan^{-1}x$, derived from the integration of $r^2/(r^2 + x^2)$. He refers to James Gregory's *Exercitationes* for the method of obtaining series for the artificial tangent and secant. It is perhaps a fair inference that James Gregory had left among his manuscripts copies (rough copies it may be) of the series he sent to Collins. David Gregory, in the opening pages of his tract gives a short account of his uncle's researches and refers to the correspondence with Collins, especially to the investigations on infinite series; he remarks that except for a few examples he had not been able to find in his uncle's notebooks any thing relating to the general theory of solving equations by means of series which is mentioned frequently in the correspondence. It is not quite clear whether David Gregory had the general Binomial Theorem; his expansions are usually of square roots or cube roots, and on pages 20–21 he explains the method of obtaining them by the case of $\sqrt{(c^2+x^2)}$, adding the interesting remark :—"In the same way *mutatis mutandis* any other pure root of a given quantity may be extracted. By inspection of a few terms at the beginning one can continue the series without the tedium of further calculation."

It is unfortunate that no record is to be found of Gregory's general theory. A considerable amount of attention is given to equations in the *Correspondence*, but not much can be inferred that throws light on that theory. The most definite statement occurs in a letter of 2nd April 1674 (Rigaud, p. 255), in which he states the method of calculating in the form of a series that root of the equation for a

$$b^n c = b^{n-1}(b+c)a - a^{n+1}$$

the first approximation to which is $bc/(b+c)$. The letter was evidently written in haste; it states the successive approximations in a somewhat peculiar form, but the solution can easily be gathered from the explanations given. In any case it shows that the reversion of a power-series would cause him no difficulty.

The conditions necessary for the convergence of a series do not bulk so largely in the correspondence as they would do now-a-days, but it is quite plain that they were present to Gregory. I have already referred to an example in the letter of 19th December 1670, and another example is given in a letter of 17th May 1671. He there says (Rigaud, p. 228) :—"I know that the segments of circles are of great use in practice; but, if the segment be little, Mr Newton's series which ye sent me is not of ready use, and therefore ye may make use of this. Let the radius be r, the sagitta of the circular segment a and $2ra = b^2$, the circular segment will be

$$\frac{4ba}{3} - \frac{2a^3}{5b} - \frac{a^5}{14b^3} - \frac{a^7}{36b^5} - \frac{5a^9}{352b^7} - \frac{7a^{11}}{832b^9} - \text{etc.},$$

and its whole arc will be

$$2b + \frac{a^2}{3b} + \frac{3a^4}{20b^3} + \frac{5a^6}{56b^5} + \frac{35a^8}{576b^7} + \frac{63a^{10}}{1408b^9} + \text{etc.}$$

These would be obtained by integrating

$$\sqrt{(2rx - x^2)} \text{ and } r/\sqrt{(2rx - x^2)}.$$

In the same letter he sends two series for the area between a hyperbola, an asymptote and two ordinates to the asymptote, and points out that both are needed for "the complete measure." Thus, although there is no general theory of convergence, such even as Newton sketches in the *De Analysi* (*Com. Epist.*, pp. 74, 75), there is no doubt that Gregory was alive to the fact that certain series are "not of ready use," that is, are slowly convergent while others are only of use when the variable is properly restricted. It is possible, I think, to go a little further. In the letter of 2nd April 1674, though his exposition is hurried, he indicates that after a certain stage the terms are *quam proxime* in geometrical progression, and that "therefore the whole sum of them *in infinitum* may be easily gathered." But it is impossible with the material at our disposal to form any definite conclusion in regard to the general theory of convergence: we are at least sure that Gregory in the simple cases he deals with was quite aware that some restrictions were necessary. (See also the letter to Dary, 9th April 1672, Rigaud, p. 240.)

Other series are to be found both in Rigaud and in the *Commercium Epistolicum*, but I do not think that they need be reproduced; they show how fully he had grasped the method of applying series to the problem of quadratures or, as we should say, to the evaluation of any integral, but they do not, so far as I have noticed, introduce any new principle. A considerable part of the correspondence discusses questions connected with the solution of equations. Gregory had a very high estimate of Hudde; "his two epistles, in my opinion, go beyond all who ever did write in Algebra, yea Cartes himself not being excepted" (Rigaud, p. 229). In a letter, of date 14th February 1672, he explains his method *de maximis et minimis* (which he says "is evident from Huddenius his 2nd epistle") for determining, as would now be said, the turning values of a polynomial, and thus finding the number of the real zeros and limits within which they lie. In another letter (Rigaud, p. 259) he says:—"That which ye intimate of the sum of the squares, cubes, biquadrates of the roots in a biquadratic equation is pretty obvious in any equation," and he writes down the sum of the powers, up to the seventh, of the roots of an equation of the 7th degree, adding "it is no hard matter to give the rule whereby to continue this *in infinitum.*" In the same letter he writes:—"I have now abundantly satisfied myself in these things I was searching after in the analytics which are all about reduction and solution of equations. It is possible that I flatter myself too much when I think them of some value, and therefore am sufficiently inclined to know others' thoughts, both (as ye say) as to the quid and quomodo of them; but that I have no ground to expect, till time and leisure suffer me to publish them." At the same time he shows himself to be impatient of the elaboration of special artifices for the solution of particular cases; "particular methods are infinite and hardly worth any man's pains" (Rigaud, p. 267). An interesting personal touch occurs in one of the later letters (20th August 1675; Rigaud, p. 271). Referring to "a very learned gentleman and a great admirer of Des Cartes" whose acquaintance Collins had made Gregory, after some criticisms of what seem to have been disparaging remarks that the gentleman had made about Hudde, adds:—"Yet this hinders nothing the esteem I have for the gentleman who (if I may judge *ex ungue leonem*) surely is a great algebraist, and, albeit probably he may be inferior to Mr Newton, is without question far beyond me whom ye are pleased too much to overvalue."

To the ordinary student of mathematics Gregory is now known almost solely as the man who first used the phrase "converging series" as a technical term, and as the author of the series for $\tan^{-1}x$. That series is comparatively unimportant, it is not a fundamental series like Taylor's, and it is very unlikely that it would have been associated with Gregory's name but for the part it played in the Newton-Leibniz controversy. Yet all contemporary references to Gregory show that he was considered to be among the first mathematicians of his day, quite apart from his fame as the author of the *Optica Promota*. Undoubtedly the books he published were of sterling merit, though I have found comparatively few references to them, but I think it is highly probable that it was through Collins's letters to various mathematicians that Gregory's reputation was so widely recognised. I hope that the presentation of Gregory's work in the foregoing pages will do something to prove that the estimate of his contemporaries was justified. The duties of his St Andrews Chair left him little leisure (Rigaud, p. 224), and he was drawn into petty disputes of a distracting kind; yet he was devoted to his subject, and he has left a record of which his countrymen may be proud.

In closing this paper, I cannot refrain from saying that I never return to the study of the origins of the Calculus without a feeling of shame that so little has been done to present Newton's contributions to the development of mathematics in a satisfactory form. The magnificent editions of Galileo, Fermat, Huygens—to name only a few—that have been issued in recent years are worthy memorials of great men. Is this country too poor or too indifferent to provide a like memorial to the greatest man of science whom it claims as its own?