been recorded and that below the upper brown peat was a wetter, soupier peat. The forest-covered mat had risen a foot and a half from the previous year. Up to this time evidence pointed to the normal development that is to be expected in the filling in of northern bog lakes in their conversion to land. With the breaking loose of the mat and the insertion of what might be called an unconformity, the tree-covered mat has now shown fluctuations from year to year, the measurements being made with the same instruments within 5 feet of the same place each year and at about the same time late in July. These figures, as expressed in Table 1, have shown a rise of the tree-covered mat as much as 2.2 feet above the datum established in 1922 and although accompanied by lower stages have so far never reached the low level known before 1928.

TABLE 1

DEPTH FROM SURFACE OF SPHAGNUM TO SAND BOTTOM AT THE SAME PLACE ON THE MAT AT MUD LAKE BOG

Year	Feet	Year	Feet
1922	10.5	1931	11.0
1923	10.5	1932	11.0
1924	10.5	1933	11.7
1925	10.5	1934	11.5
1926	10.5	1935	11.8
1927	10.5	1936	11.25
1928	11.5	1937	11.5
1929	12.0	1938	12.7
1930	11.5	1939	12.3

If pollen percentage profiles are made in fluctuating parts of such bogs comparable measurement of depth is another problem to consider.

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HURRICANE INTELLIGENCE

A VERY unusual phenomenon in the field of mental testing was observed at the Massachusetts State College, Amherst, Mass., during the hurricane of September 21, 1938. In accordance with previous scheduling, a mental test was administered to the freshman class during the major part of the storm. In spite of very poor illumination (due to failure of electricity), falling trees and the characteristic weirdness both of sound and vision which prevailed, the freshmen showed a 20 per cent. superiority over the previous ten-year average. Other tests administered to the same freshmen show this class to be about average. This marked superiority under what would appear to be very adverse conditions has attracted much attention. Coincidence and chance do not appear to adequately explain these results. When all conceivable factors are considered, it appears plausible that the unusual amount of ozone in the air during the hurricane served as a mental stimulant to the freshmen. Authority for asserting the presence of relatively large quantities of ozone during the hurricane is expressed in a note in SCIENCE of November 24, 1939, by Dr. C. A. Peters.

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LUNAR RAINBOWS IN HONOLULU

I DO not know if readers of SCIENCE want to hear any more about rainbows in Honolulu, but I spent my boyhood there and can still remember the glorious sight of the lunar rainbows. I remember once seeing a double one. I think the reason why one sees such brilliant rainbows, especially on Oahu, is that several times a day a squall of rain is likely to originate in the cloud cap over the Koolau range and to travel southward down one of the valleys. While the mountain range that runs from east to west throughout the island is covered with a black cloud, a few miles out at sea the sun is shining brilliantly. There are no clouds there. Obviously, the conditions are ideal for the frequent formation of unusually beautiful rainbows.

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SCIENTIFIC BOOKS

ANOTHER INVENTOR OF THE CALCULUS?

James Gregory, Tercentenary Memorial Volume. Edited by HERBERT WESTERN TURNBULL, F.R.S. vii + 524 pp. London, 1939.

THE subtitle to this handsome memorial explains that the volume contains Gregory's "correspondence with John Collins and his hitherto unpublished mathematical manuscripts, together with his addresses and essays, communicated to the Royal Society of Edinburgh, July 4, 1938." James Gregory (1638–1675) is probably the most justly celebrated of nineteen notable members of a famous Scotch family whose ability persisted through several generations, in mathematics and in medicine. The nauseous, gritty mess known as "Gregory's mixture" or "Gregory's powder," was perpetrated by one of the medical Gregorys; and for all the reviewer knows to the contrary, it may still be inflicted on helpless bairns.

The hero of the present memorial was a mathematician. His short life fell in one of the major epochs of mathematical history; and had he been nearer the center of things, James Gregory might have left a far greater name than he has. Before beginning his bleak professorship at the University of St. Andrews in 1668, Gregory had profited by four years on the Continent, mostly in Italy, where he seems to have absorbed some of Cavalieri's pernicious nonsense about "indivisibles," which Newton had the mathematical insight to reject. Scotland in the seventeenth century was not exactly the locale an original mathematician would have chosen in which to develop his natural gifts. The story of Gregory's life, with many sidelights on the famous mathematicians and scientists of his day, has been woven into the science and mathematics with great skill by Professor Turnbull.

Assuming some familiarity with the history of mathematics in 1638–1675, we shall note presently only three of the many items on which Gregory's reputation as a mathematician now rests. Even this sample would require several times the available space for adequate evaluation. Professor Turnbull's translations of Gregory's Latin, and his illuminating "explanations" accompanying them, will enable any reader of the volume to reach his own conclusions as to what Gregory actually did, what he may be credited with on circumstantial evidence, and what he might have done had his material circumstances been more propitious than they were. Concerning the two possibilities mentioned, it is a reasonable guess that historians of mathematics will find, especially in the first, material enough to keep them busy conjecturing for the next hundred years. For example, since the Collins who slips in and out of Gregory's correspondence like a friendly eel was the same Collins who called himself a friend of Newton, there are implicit in this stimulating book at least fifty full-length monographs and as many Ph.D. dissertations in the history of mathematics on the single theme of "Did Newton Influence Gregory more than Gregory influenced Newton?" That unanswerable questions can not, in the nature of things, be answered, will not deter critical scholarship, which sometimes seems to experience neither weariness nor common sense.

Gregory's first and most sensational claim to remembrance in the history of mathematics is that he discovered what is traditionally called Taylor's theorem in the differential calculus over forty years before Taylor rediscovered it—as we must say, henceforth, if we admit the cogency of Professor Turnbull's extremely able presentation of Gregory's case. The evidence is indisputable; sixteen detailed exhibits substantiate a *circumstantial* proof that Gregory was many times guilty of using Taylor's and Maclaurin's theorems without once committing either to paper. Innocent men have been hanged for less.

Gregory is all but convicted on a single numerical slip. To quote Professor Turnbull (pp. 356–7): "One numerical error occurs in two of the series—the coefficient 3233, which should be 3968—otherwise the series are correct. But this is precisely the error which occurs [elsewhere, in a calculation of successive derivatives] due to the mistaken coefficient 987 at the sixth derivative. . . . The inference is irresistible that these series were derived directly from these notes: else how are we to account for the existence of this solitary error in two distinct contexts? . . . In contrast to his [Gregory's] interpolation formula, . . ., which he stated explicitly in general form, . . ., the Taylor series occurs only in applications." Surely this is enough to condemn the accused? But no; British justice is British justice, even in Scotland, if not in Ireland; and the Scotch Gregory very justly gets the Scotch verdict of 'not proven' to which alone the facts in the case entitle him: "The reader must judge for himself whether this constitutes a claim that Gregory had discovered Taylor's theorem; but if he rejects the claim he is faced with the puzzling question how to account for his wealth of applications of a complicated theorem if the theorem itself were unknown to Gregory."

Regarding the first question, we merely suggest that some scholar of the curious compile a select anthology of coincidences and mistakes in mathematics which might have led to retroactive anticipations as spectacular as Gregory's had they been sympathetically interpreted. The second question, how a man can make numerous applications of a complicated theorem (or of an elaborate algorithm) without suspecting the existence of the theorem (or the algorithm), arises frequently in the history of mathematics. To mention a possible instance on which competent opinion is divided, there is the controversy over Fermat's alleged invention of the differential calculus. A more interesting example, and one closer in character to Gregory's possible but unproved use of Taylor's theorem, appears in the history of trigonometric series from Euler and Lagrange to Fourier. Here the facts are clear; and in spite of circumstantial evidence of the most plausible kind, it is generally agreed that the predecessors of Fourier who seem to have applied his theorem had no inkling that any such theorem existed. Another anthology of such illusory anticipations, with selections from the more heated arguments pro and con, might be a useful *vade mecum* for future historians who, it may be expected, will prove conclusively (1) that Gregory anticipated Taylor, (2) that he did not.

There are already several such vacuous logomachies in the critical history of mathematics. Another example from Gregory's own prolific century is the dispute concerning Barrow's hypothetical influence on Newton in the development of the differential calculus, in which equally competent scholars[1] have reached

[1] For example, J. M. Child, "The Geometrical Lectures of Isaac Barrow." Chicago, 1916; F. Cajori, *American Math. Monthly*, 26: 15–20, 1919. See also A. Dresden, *Bulletin Amer. Math. Soc.*, 24: 454–7, 1918.

diametrically opposite conclusions. A third faction in this particular dispute would claim that Newton's own explicit statement of what led him to his method, not known when the controversy was at its hottest, has abolished the topic of debate. Gregory now (p. 13) enters the lists as a competitor for the honor of having invented the differential calculus: "These notes are the silent but unerring witness giving Gregory the right to take his place with Barrow, Newton and Leibnitz as a principal discoverer of the differential calculus." This may be so; it is only for historians to decide. In the important detail of Gregory's claim to Taylor's theorem, it would seem that until positive evidence is discovered, and there appears to be but a slim chance of this after Professor Turnbull's painstaking search, the field for inconclusive speculation is wide open for all who care to cultivate it.

Passing to the second item, we note that Gregory's quality as a mathematician appears unmistakably in his conjectures (in modern terminology) that the numbers π and e are transcendental, and in his suspicion that not all equations are solvable by radicals.[2] Although he had been anticipated by Omar Khayyam and Fibonacci in conjectures of the second of these species, there appears to be no question that Gregory was original in his doubts. This is mathematics of a far higher order than mere algoristic ingenuity. Gregory's own attempts to implement his doubts were abortive, not being aimed in the right direction.

The third and last item of Gregory's mathematics to be noted here is his work in diophantine analysis. The evidence in this instance is fairly complete that Gregory was acquainted with the conclusions and at least some of the methods, including Fermat's of infinite descent, of his immediate predecessors and contemporaries. As to the quality of Gregory's work in this field, naturally it is in the pre-Lagrangian tradition, which prevailed from Diophantus to Euler, of ingenious devices, incomplete solutions, and disregard of existence theorems. It was not until nearly a century after Gregory's death that Lagrange inaugurated (1766–69) the civilized era in diophantine analysis with his complete discussion of the so-called Pellian equation (Fermat's equation). For the continued sane development of diophantine analysis, it can not be too strongly emphasized that the tradition of Diophantus, Gregory and Euler belongs to a memorable but buried past, and that Lagrange was the first to elevate the subject above haphazard ingenuity to a mathematical discipline. Still hopelessly in the Dark Ages of diophantine analysis, Gregory nowhere gives any indication that he had grasped the nature of the real problem in the subject, that of devising a non-tentative method for exhibiting all numbers, and only those, satisfying a given equation. Before Lagrange, Legendre and Gauss confined their efforts to single equations of degree not higher than the second, little bearing any resemblance to reputable mathematics was accomplished in diophantine analysis. The absurdly difficult problems attacked by Lagrange's contemporaries and predecessors, including Gregory, are a testimonial to lack of insight rather than to daring originality. Gregory's last (1675) and in some respects most individual venture, his problem of cubes, is in this venerable tradition of ingenuity without insight.[3]

This meagre sample of Gregory's impressive work in mathematics must suffice here. For a vivid picture of the man himself, and of his equally notable achievements in other departments of mathematics and in the science of his times, we must refer to Professor Turnbull's full and documented account. We have tried only to suggest that this extremely interesting volume may supply historians of mathematics with much new fuel for their interminable controversies. Whether such disputes as are likely to be engendered by critical evaluations of Gregory's claims add anything worth having to human knowledge, is a matter of opinion. Whatever the final verdict is to be, it seems likely that James Gregory will remain as he is portrayed in this book, a mathematician whose reputation might have overshadowed that of many others who have long since passed into the traditional history of mathematics, had fortune been only a little kinder to him.

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REPORTS

SUMMER CONFERENCES AT THE MASSACHUSETTS INSTITUTE OF TECHNOLOGY

A SUMMER program of technical conferences and courses on research and practice on the frontiers of science and engineering has been announced by Professor Raymond D. Douglass, chairman of the summer session of the Massachusetts Institute of Technology. This program, which supplements the regu-

[2] There seems to be a slight confusion on p. 383, where it is stated that Tschirnhausen solved (1683) the quintic and sextic algebraically "when the second and third highest terms were absent," although the following sentence states (correctly) that "Abel demonstrated the impossibility of such a solution, in general, for the quintic and higher equations."

[3] Contrary to the statement in p. 435, it has not been proved that the problem is impossible. In addition to the cited note by Fauquembergue, the remarks by Tannery on the page following the note should be consulted. Fauquembergue's attempted proof of impossibility is unsound.