Some account of Halley's Astronomiae Cometicae Synopsis

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SOME ACCOUNT

OF

HALLEY'S

ASTRONOMIÆ COMETICÆ

SYNOPSIS,

WHICH CONTAINS HIS INVESTIGATION

OF THE

ORBITS OF COMETS.

By S. P. Rigaud

JAM PATET HORRIFICIS QUÆ SIT VIA FLEXA COMETIS.

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ON

HALLEY'S

ASTRONOMIÆ COMETICÆ SYNOPSIS.

THE recent return of Halley's comet forms a great epoch in the history of Astronomy, and has occasioned many valuable dissertations both on the calculation of its elements and on the specific labours of those who have directed the methods and conducted the computations of the inquiry. In none of these has the merit of the original discovery been neglected; but the particular steps, by which the author of it advanced to his conclusions, have possibly not been brought forward so prominently as they deserve. The immense strides by which modern science has led us, with certain accuracy, to the minutest details, have naturally impressed the mind with wonder, and arrested on themselves the course of our thoughts. Still, however, there is no small interest attached to the foundation, on which a splendid edifice has been erected; nor can it be without its usefulness to examine the solid and careful manner in which it has been laid. A general account of Halley's investigations is to be met with in more than one established work on the astronomy of comets: but they deserve a fuller detail; there are some circumstances which may be added, some oversights which may be corrected, and the whole seems capable of a varied form, by which, in addition to the results, the progress of his inquiry may be kept also in view. Such a method will do more ample justice to the memory of a great man, and will exhibit, in his labours, a beautiful instance of inductive reasoning legitimately leading to the establishment of an important truth.

It is unnecessary to enter into the history of the many hypotheses, to which the appearances of comets have given rise, since Halley's calculations are confessedly derived from the views which sir Isaac Newton had explained in his Principia—"tanti viri vestigia insecutus eandem methodum calculo arithmetico accommodare aggressus sum" are Halley's own words[a]. Newton himself in the autumn of 1685 had lost two months in endeavouring to no purpose to determine the orbit of a comet, and in the following June he had not completely drawn out the theory of these bodies[b]. He transmitted, however, the third book of the Principia to the Royal Society in the beginning of April 1687. In this he had developed their nature, and (as we find it mentioned in a contemporary notice) had illustrated it, "by the example of the great comet of 1680–81, proving that, which appeared in the morning in the month of Nov. preceding, to have been the same comet, that was observed in Dec. and Jan. in the evening[c]." He gives four places, which he had deduced from his own elements[d], but in the second edition of the Principia, after the insertion of the same numbers, he adds a more extensive table of results, which had been contributed by Halley, who "postea...orbitam per calculum arithmeticum accuratius determinavit quam per descriptiones linearum fieri licuit[e]." It is probable that he engaged in his more extensive investigation about 1695; for there is a letter (preserved in the library of C. C. C.) dated Sept. 14. of that year, in which Newton says that some short time before, "Mr. Halley was with me about a design of determining the orbs of some comets for me." He had then found the elements which gave the observed places to a minute of that, which had appeared in 1683; but we have not particulars by which we can ascertain the dates of his succeeding labours. Halley's life was one of constant and active employment, and from 1698 to 1700 he was occupied by voyages to collect materials for his Magnetic Chart: it is probable therefore, that he accomplished his great purpose by repeated efforts, with intervals in which from a change of object he derived that relief, which common men can only obtain from rest and relaxation. Gregory published his "Astronomiæ Physicæ et Geometricæ Elementa" in 1702, and from that work we learn that Halley

[a] Phil. Trans. vol. XXIV. p. 1885. To save repetition it may be here mentioned, that wherever a Latin expression is introduced without reference to any particular author, the words may be considered as quoted from Halley's Dissertation.

[b] Newton's letters to Halley and Flamsteed in Gen. Dict. historical and critical, vol. VII. p. 797, 793.

[c] Birch's Hist. of the Royal Society, vol. IV. p. 530. Even Cassini imagined them to be two distinct bodies (Abrégé des Observations sur la Comète de 1680, et 1681. p. vi.) Newton gives Flamsteed the credit of having maintained their identity. (Princ. 1687, p. 494.)

[d] Principia, (1687) p. 494.

[e] Principia, (1713) p. 458.

had then determined the elements of all the comets to which he extended his inquiry; but their particular quantities had evidently not been communicated even to his brother professor, who could only speak of what "a D. Halleio factum esse audio[f]." In 1702 he was again interrupted by business, which took him to the Continent, and he did not publish the conclusions at which he had arrived till a later period, "neque antequam ad incudem probe reducti fuerint et multorum annorum studio quantum fieri possit politi."

His "Astronomiæ Cometicæ Synopsis" at length came out in the 297th number of the Philosophical Transactions for March 1705. No notice has been found in the minutes of the Royal Society of the time when the paper was communicated. Particulars of this kind were not formerly attended to with the regularity, which, of later years, has been adopted: the Transactions were not then published by the body, but by some individual, (most commonly one of the secretaries,) and it does not appear certain that he always confined himself to what had been read at the ordinary meetings. Halley also reprinted the work at Oxford, with some variations from the original of less importance, and with the entire omission[g] of the speculations at the end of it. This abridgment was not the result of any considerations of convenience, for the whole was contained in a sheet and a half of large folio, and more than two thirds of the sixth page are wholly unoccupied by letter press. Such a form was not calculated for its preservation, and few copies are now to be met with, excepting such as may have been deposited in old and permanent libraries. Costard, in his letter to Mr. Folkes on "the Rise and Progress of Astronomy amongst the Ancients[h]," refers to this publication as if it had been annexed to "the end of Scarburgh's Euclid," and the author of Halley's life in the Biographia Britannica says, in a sidenote[i], that the same account is given in Le Monnier's Théorie des Comètes. I have looked repeatedly with-

f Gregorii Astr. lib. V. Prop. 35.

g The reprint extends to the seventh line of p. 1898 of the Transactions.

h P. 25.

i Vol. IV. p. 2519, note (U.U.) Scarburgh's Euclid is a common book, at least on small paper, in which size it is not large enough for Halley's reprint to be bound with it. The writer in the Biographia Britannica from never having seen them together, may have been convinced of the error in Costard's statement; but it appears that he was not aware of the Synopsis having been published in 1705 at Oxford as well as in London. Indeed his manner of contradiction implies that he understood Costard's assertion to apply only to the first time when the subject was communicated to the world.

out being able to find the passage alluded to in the French writer, and possibly his authority, even if thrown into the scale, could not on this subject add much weight. The form of the page and the size of the types in the Synopsis, evidently mark it as a distinct publication from the Euclid. The two works indeed came out very nearly at the same time from the University press, and they are found bound together in the Bodleian, which probably led Costard (who was of Wadham college) to his erroneous conclusion. This reprint almost immediately followed the original publication. Halley mentions it in a letter to Dr. Charlett[k] of the 23 June 1705; he presented a copy of it to the Royal Society on the 20th of the same month, and had before deposited another in the Savilian library on the 8th.

It was not long before the Synopsis appeared in English[l]: and, although the translation was anonymous, there are circumstances which would agree well with its having been Halley's own. It was professedly made "from the original printed at Oxford;" and in the second volume of the Miscellanea Curiosa, a work of which he was the original[m] editor, it was reprinted in 1706, so that every page and every line in the one book answered exactly to those in the other. It contains indeed the conclusion which is only found in the Philosophical Transactions, but this same additional translation appears likewise in the Synopsis, as it is annexed to the English edition of Gregory's Astronomy in 1715. Now there are several circumstances, which will be hereafter discussed, from which we must conclude that this last came from Halley, who thus appropriates the whole to himself.

Whiston introduced the Synopsis into his Prælectiones Physico-Mathematicæ, but when he published them in 1710, he did not print it with the attention which it deserved[n], and the in-

[k] Letters by Eminent Persons (1813) vol. I. p. 139. From the letter to Charlett it would appear that one of Halley's objects in reprinting at Oxford was to be provided with copies, which he might distribute among his scientific acquaintance; but he speaks to his friend of "my small performance about comets, which no wais deserves a place in your catalogue or to bear the badg of the Theatre." This apologetical sentence is annexed to thanks for Charlett's "kind endeavour to give reputation and value" to the work, and therefore seems to indicate that it was published also for general sale. No minute, however, of this has been found in the records of the Oxford press.

[l] London, printed for J. Senex, 8vo. 1705.

[m] Derham's name appears in the title-page of the work, in 1726-27, but only as the revisor and corrector.

[n] There are several press errors which are left uncorrected; again, in p. 346 he describes Halley's expression, $z^3 + 3z = 12a$ as "æquatio cubica, cujus termini secundus et quartus desunt," (a mistake which

verted commas were carelessly added, which were to distinguish the text from the comment, with which he had endeavoured to illustrate it. In 1716 he sent out his book in English, making use of the translation of the Synopsis from Gregory's Astronomy. These republications in English seem to have led M. Pingré to take up a mistaken impression of the language, in which Halley first drew up the work; for he says[o], "Elle parut pour la première fois en 1705 dans les Transactions Philosophiques: Whiston la traduisit en Latin, y ajouta des commentaires, et la fit imprimer en 1710." This is an oversight, arising possibly from the author having satisfied himself with the French translation, which Le Monnier published in 1743, and which contained Whiston's Commentary. It was rather loosely done, but still may have been sufficient for the use that was required of it: an error, however, like this, though venial in a work of such extensive erudition as the Cométographie, acquires authority from the name of its excellent author, and therefore ought not to pass unnoticed[p].

We may now examine the contents of the Synopsis.

It is prefaced by some historical remarks which are but imperfect, and therefore need not be dwelt upon. The essential part begins at the conspectus of the elements of twenty-four comets: "tabellam immensi pene calculi fructum." If we had Halley's computation papers, there can be no doubt of the many trials, which we should find that he was obliged to make, before he could satisfy the conditions of his problem, and that each trial, as it must have been then conducted, would have appalled a man of common resolution, "ob calculi difficultatem non cuivis homini denuo suscipienda." He follows up his elements with an explanation of some of the means, which he had employed in ascer-

is retained in his English of 1716;) and in p. 357 there is a still stronger instance of carelessness. The size of the Ph. Tr. did not admit of the comets being distinguished in p. 1886 as to their motions being direct or retrograde. A notice of this was therefore introduced afterwards (at p. 1896) where the following sentence begins with "Quibus perpensis, ac collatis inter se cæteris horum cometarum elementis," &c. In the Oxford reprint, where there was no want of room, the particulars are inserted in their proper place, the notice is omitted, and the text goes on properly with "Collatis inter se," &c.; but Whiston, who was also able to insert the motions where they ought to be, and consequently to leave out the notice, still retains "Quibus perpensis," which, as there is no longer any antecedent to his relative, makes the whole sentence unintelligible.

o Cométographie, vol. I. p. 158.

p In the Biographie Universelle vol. XIX. p. 342. M. Nicollet has very naturally adopted this mistaken account.

taining their accuracy; but for the present purpose it may be useful to invert this order and consider his process of reasoning before we proceed to his conclusions.

When Newton had made out the true nature of comets, he did not involve the numerical computations in more than necessary difficulty, by applying them at once to elliptic orbits. He contented himself with the supposition of parabolic motions, which though now so simple in appearance, were then of sufficient intricacy to require the first-rate powers to disentangle them. Halley also did not, at first, attempt to go, in this respect, beyond his master, and having examined all the comets, of which the observations appeared to be sufficiently precise, the parabola was found to give him the places in every instance with what he then considered to be sufficient accuracy.

Setting out from the equal description of areas in equal times, a very plain geometrical construction is shewn by Halley to lead to the quantity of parabolic anomaly, and to the successive lengths of the radius vector. When this, however, was to be reduced to practice, it required the resolution at each step of a cubic equation, and the values of the roots were again to be referred to the tangents and secants of arcs to which they belonged. These numbers were of constant recurrence in the calculations, but the parabola is always similar to itself, and whatever is determined for one of a given dimension, may by a simple proportion be extended to any other curve of this same species. Halley therefore fixed on the numbers which seemed most convenient, and formed a table which enabled him to verify the parabolic elements assumed for his comets, and to ascertain how far they agreed with the observed places of it. Between the axis and one of the focal ordinates, he took the parabolic space, which he supposed to be divided into one hundred parts; then, assuming 4 for the latus rectum, he calculated the angular distance of the radius vector from the perihelion, and the length of that line for each of these equal portions of the area. He shews by precepts and examples the great use which may be derived from this table, although he cautions his readers against depending on it, when the perihelion distance is so very small as it was for the comet of 1680: in such a case he recommends that the calculation should be distinctly made from the fundamental equation, instead of the quantities being taken out from his table by interpolation.

This table greatly facilitated the deductions, but was of no other service in determining the elements, from which those deductions were made. For this, which was the most difficult part of the inquiry, Halley seems to have considered that in a short tract nothing could effectually be added to what Newton had laid down on the subject, and as he had applied those propositions to the varied circumstances of each particular case, so future difficulties might in the same manner be left to the individual talents of those who might have to encounter them. Halley also intended his Synopsis to be only the introduction to a fuller treatise: his object in publishing was to announce the results at which he had arrived, and to preserve what, in case of any accident to himself, might otherwise have been lost. In the meantime to those who would employ themselves in similar investigations, he recommends[q] the study of those parts of the Principia which bear upon the subject, with the illustrations which Gregory gives of them in his Astronomy. He printed more afterwards on this subject, but it was only another edition of his Synopsis, which was altered and enlarged by much valuable matter, but was never completed into that larger work which he originally contemplated. If that design had been fulfilled, he would, no doubt, have enabled the world to reap still greater advantage from his experience, which must have suggested many useful precepts. Newton gave the general solution of this which he justly called "problema longe difficillimum[r];" but when he applied it to actual practice, he contented himself with shortening his work "per operationes partim arithmeticas partim graphicas[s]." Many of the calculations for the Mécanique Céleste were made by the hands of assistants. The time and thoughts of Laplace and Newton were too precious to be employed on what could be executed by others. Halley, therefore, was the first who ever strictly calculated a comet's orbit in the section of a cone: and for very many years he continued to be the only man, who had courage to undertake it. Bradley was the first to follow his footsteps by ascertaining the elements of the comet which appeared in 1723.

Newton had proved that at the same distance from the centre

[q] Phil. Trans. vol. XXIV. p. 1898. This was omitted in the reprint at Oxford, which seems to be confined to what was entirely Halley's own.

[r] Principia, lib. III. Prop. 41. (1687). The expression is retained in the later editions of 1713 and 1726.

[s] Principia (1687), p. 494.

the velocity in the parabola is to that in the circle as $\sqrt{2}$ to 1. Halley, therefore, by no very intricate process, was enabled to connect the motions of his comets with the revolutions of the planets. By this means he found that the parabolic calculations gave him with sufficient precision the elements of twenty-four comets, which had been observed between 1337 and 1698. He met with no instance in which he found it necessary to have recourse to the hyperbola, and he kept in mind the probability of their really moving in curves which would return into themselves. His first object in determining these elements was to give the opportunity to astronomers, by the test of his calculations, to ascertain whether the future appearance of any comet was new, or only the repetition of what had been before observed. He therefore employed every care in making the tabular view, which he drew up of them, as correct as possible—"cui adornandæ nullis sane peperci laboribus, ut perfecta prodiret, utpote posteritati consecrata ac cum scientia astronomica duratura." This indeed, though the most laborious, was the only way, in which the question could be determined: there had been many suppositions of the reappearance of particular comets, but all were mere conjectures founded on loose and accidental similarities, and, even if any of them had been true, there could have been no certainty of the fact from the manner in which the conclusions had been assumed. Halley, however, was rewarded for his exertions by being able himself to derive the first advantage from them. He richly deserved this good fortune; but still it was a great instance of favourable chance, that the only returning comet which was known till the beginning of the present century occurred in his list, and had a periodical revolution within the limits of it.

Halley indeed had been struck with an apparent similarity, which has been dwelt upon more, possibly, than was necessary. He says, "crediderim equidem cometam anni 1532 eundem fuisse cum illo, qui ab Hevelio observabatur ineunte anno 1661: sed observationes Apiani quas solas de primo habemus, nimis rudes sunt, nec quicquam certi in re tam subtili ex iisdem elici potest." Now this is expressed with great caution. He clearly did not mean to support his hypothesis by supposing that modifications were to reconcile Apian's observations to it, but he hesitated in admitting the hypothesis, because the observations were not sufficiently accurate to support it. He omitted likewise all allusion to the circumstance in his later edition of the Synopsis; he can therefore be hardly said to have expected a reappearance in a revolution of about 129 years. The verification, however, of his remarkable prediction in another instance naturally called attention to what at any one time had even occurred to him as possible; and Dr. Maskelyne annexed a notice of this comet to the Nautical Almanac for 1791, Pingré[s] gave tables of its expected appearance in the Connoissance des Tems for 1789, and sir H. Englefield published an extended ephemeris for it in 1788. Mechain's prize dissertation, however, in 1782, justified the view which Halley had really taken, by shewing that the more ancient observations did not authorize any well-founded expectation of its return[t]: and no such return has been known to occur.

It was in another instance (already alluded to) that Halley found the reward which has immortalized his name. He observed an agreement in the elements that he had determined for the comets which appeared in 1682, 1607, and 1531; and the only difficulty, which made him hesitate about their identity, was a difference in the intervals of their returns. He suggests, however, that this may have been the consequence of perturbations produced by bodies which the comet may have approached in a distant range, which carried it almost four times as far as Saturn from the centre of the system to which it belonged. He ventured no further at first than to argue by analogy to this possibility from the alterations which are known to be produced by the same cause in the periodic revolution of Saturn; but as he found that a comet had been seen in 1456, the general description of which agreed with that of the others, the interval of time confirmed him in his opinion, and convinced him that it might be looked for about 1758[u].

[s] Pingré thought that by further search he had succeeded "à convertir le soupçon de Halley en certitude." Cometographie, vol. II. p. 133.

[t] Mémoires des Sçavans Etrangers, vol. X. p. 392. See also Olbers in Hindenburg's Magazine for Mathematics, 1787, p. 440, referred to in Brande's Journal, vol. XVII. p. 86.

[u] It may not be unworthy of observation, that even such a man as Halley may be led too far, when he suffers himself to speculate beyond the facts on which his reasoning is founded. For having laid down his remarkable prediction, he adds, "quod si hoc evenerit, nulla amplius erit dubitandi causa, quin redire debeant cæteri." It must, however, be allowed, that nothing could be more natural than such a supposition: and when we recollect how little we know of the nature of these bodies, and of the many changes to which,

When Dr. Hutton was employed on the Abridgment of the Philosophical Transactions, he entirely omitted this paper of Halley, because he says that it had been subsequently published in a much fuller and more complete state both in Latin and English[x]. It may be regretted that he should have suppressed an original document of so much interest. If we merely look to the more rapid improvement of scientific knowledge, it is certainly best to study the last and corrected thoughts of an author, but the history of human improvement is not without its use, and it is desirable to have the means of marking its progress. It is with this view that a fuller analysis has been given of what appeared in 1705, instead of taking its contents, as they have been taken by some writers, to be merely a subordinate part of what was afterwards published. This indeed is by no means an accurate view. When Halley had finished his Astronomical Tables they "were sent to the press in 1717, and printed off in 1719[y]," and the Synopsis was "at the same time[z]" inserted in them; but this new edition was in many parts essentially different from the first. The beginning[a] indeed is very nearly the same as in the Philosophical Transactions, but not without several alterations and corrections, and the ending, to which he has given the distinct title, De Motu Cometarum in Orbibus ellipticis, is so greatly enlarged and improved, that it may be considered as new. The Table of the twenty-four comets[b] has some of its columns varied both in their substance and arrangement[c], al-

both in consistency and motion, they are exposed, we must acknowledge that it is not more extraordinary that a regular revolution should be found to exist in them, than that this singular property should belong to so very few out of the many hundred which are known to have appeared.

[x] Phil. Trans. Abrid. vol. V. p. 201. Dr. Hutton speaks of an English Translation by G. T. gent. 8°. 1757. The title-page of the pamphlet would lead any one to this conclusion, but upon examination it would be found that the translation does not extend to the whole of the work. It is said to be made from the second edition.

[y] Preface to Tables.

[z] Ibid. These dates must be understood as belonging to the Latin text: the English translation of the work was not Halley's, but was drawn up when the precepts and preface were added at the time of their publication, after the author's death, in 1752.

[a] As far as "reditumque prædicere," which words are also in p. 1897. lin. 12. of the Phil. Trans.

[b] There had been comets in 1706 and 1707; but Halley did not add them to his list. In 1706 he published the tract "de Sectione Rationis," and in 1710 the folio edition of the Conics of Apollonius, portions of which he restored from the Arabic, with which language he had no previous knowledge: about the same time he also printed the Sphærics of Menelaus, "collatis MSS. Hebræis et Arabicis." In 1713, also, he became secretary to the Royal Society. All which must have carried his thoughts in a different direction.

[c] The four first columns are the same in each; they contain Cometæ anni, No-

though those parts which are retained exhibit no alteration in the numbers which they contain. In the general Table for Parabolic Motions there are some corrections, and two new columns of differences are introduced, which required no great labour for their computation, but which were very advantageous additions: they not only facilitated the use of the table, but formed a check for securing its accuracy.

In the first edition Halley had said "hactenus nullus ex observatis parabolæ leges respuit," but in the second this sentence is qualified and altered to "nullus ex hactenus observatis motus parabolici leges quoad sensum recusat." This modification was made with reference to a great and essential improvement in the latter part of the treatise. For in examining the comets which appeared to return, the true elliptical theory is there applied to their motions, instead of the approximations that had been at first obtained from the parabola.

The second edition of the Principia was published in 1713, and Newton notices in it[d] the strong similarity which had been found in the elements of the comets of 1682 and 1607. He points out also that if they should be the same "haud difficile fuerit orbem ellipticum cometæ hujus determinare." This expression evidently indicates that the determination had not then been made, for if it had, Newton would certainly have been informed of it. There is reason indeed to conclude, as we shall see hereafter, that Halley undertook the work at a subsequent, but not very distant, period; for he had entered upon the investigation, though he had not completed it, in 1715, about two or three years before he finally committed the results to the press.

Carrying ourselves back to the time when the discovery of so magnificent a fact was breaking on his mind, it is interesting to endeavour if possible to trace the course of the author's feelings. Halley having summed up his arguments on the comet of 1682, says, in the first glow of exultation, From hence I can undertake

dus ascend., Inclin. orbitæ and Perihelion in orbe; the 5th and 6th of 1705, containing the longitude and latitude of the perihelia, are omitted; the 7th, 8th, and 9th, Distantia perih. a sole, Log. dist. perih. a sole, and Temp. æquat. perih. Londini, are the 5th, 6th, and 8th of the second edition; and two new columns are introduced into it, the 7th containing the logarithm of mean motion, and the 9th giving the distance of the perihelion from the node (or the difference of the numbers in the 2d and 4th). The direct or retrograde motion of each comet is likewise marked, for which it has been mentioned that there was not room in the page of the Phil. Trans.

[d] P. 480.

confidently to predict the return of the same in 1758: "Unde ausim ejusdem reditum fidenter prædicere anno scil. 1758." In the first English translation this is softened into "I dare venture to foretell." As time advanced, some anxiety seems to have come on him, lest he should not have sufficiently established his great conclusion, and in 1715 the expression is reduced to "I think I may venture to foretell." It has been mentioned that he had originally noted the returns in 1682, 1607, 1531, and 1456; he had by this time likewise remarked that a comet had been seen in 1305, which gave him the addition of a double interval, and he afterwards found that a similar phenomenon had been noticed in 1380: he had therefore then six[e] consecutive appearances at similar distances of time; he therefore in his second edition again recovers his confidence, and says, "priorem sententiam paulo audentius tueri cœpi:" and, to verify his hypothesis, he undertook to calculate the motions in an elliptical orbit.

If Halley had had from given observations to find directly an ellipse which would answer to the places of the comet, he would most probably have then been unable to succeed. In very eccentric orbits a small error in the assumed places near the apsis will make a great variation in the curve of motion. Lexell found the periodic revolution of the comet of 1769 to be between 449 and 519 years; but M. Pingré from some observations deduced nearly the same quantity, while from others his time was extended to 1231[f]: a revolution of 2279 years came out for the comet of 1773, and Mr. Lexell justly concluded that it was impossible to determine its orbit: "la plus légère erreur dans les observations pouvant en occasioner une très considérable dans la durée de la révolution[g]." Halley had not, indeed, in the present instance, to contend with one of these extreme cases, but it may on the other hand be conceived how greatly his difficulties were increased, both by the roughness with which astronomical observations in former times were too often made, and by the want of rules for conducting the calculations. His previous inquiries had however most fortunately inverted the problem for him. He had not from the places observed in a single appear-

e Pingré (Cometographie, vol. II. p. 133) extends these returns to 1230, 1155, 1080, 1006, and possibly to 930 (vol. I. p. 354) and 855 (vol. I. p. 347). Other writers have conjectured that several additions might even be made to this list.

f Pingré, vol. II. p. 85.

g Ibid. p. 93.

ance to find the time of the comet's return, but from the sesquialterate ratio of the times and distances its return gave him the longest diameter of his orbit, to which he had to accommodate the approximate elements, which he had found from the parabola. This was exactly the view which Newton took, and, as he says, it was comparatively easy in the theoretical part of the consideration; but it must at the same time be borne in mind that it was accompanied by a great increase of arithmetical labour.

The times of the comets double reappearances gave an interval of 151 years[h]. Halley therefore assumed $75\frac{1}{2}$ for its mean periodic revolution. He had from the parabola a near value of the perihelion distance; and having by trial corrected this as well as the other quantities, he obtained the true eccentricity (and consequently the minor axis of his curve), together with the other elliptical elements. These he applied to particular cases, in the well-known way of finding the true by the means of the eccentric anomaly. But if for the parabola he had at each successive step to encounter a cubic equation, he had now a still greater obstacle to contend with in an infinite series; and, if he had not altered the construction of his table, he must have been involved in the greatest difficulty. To have continued the same argument of the mean motion, an equation must have been solved, for which he had no general method of proceeding, so that he would have found it necessary to have constant recourse to tentative approximations. He therefore made use of an arrangement adopted by Kepler in the Rudolphine Tables; and by taking the eccentric anomaly for his argument, found for each 12′ of the first 12 degrees, the analogous mean motions, true anomaly[i] and logarithm of the radius vector. Having provided himself with a subsidiary table of this kind, he calculated the places in his orbit for thirteen of Flamsteed's observations between August 19 and September 9. They came out near; and Halley himself was well satisfied "Theoriam nostram computi rigorosi examini subjectam, abunde comprobarent." The greatest differences were $+2' \ 24''$ in lon-

[h] $1682-1531=151=1607-1456=1531-1380=1456-1305$; the more early epochs vary a little from this, since $1380-1230=150=1305-1155=1230-1080$.

[i] The true anomaly in this table extended to 78° 27′ 22″, and columns of differences were added for the three quantities. As each species of ellipse required a special table, the only general auxiliary calculation, which Halley could provide, was for the areas contained between the chord and the various arcs of eccentric anomaly: this he subjoins at the end of his treatise.

gitude, and +2′ 48″ in latitude[k]; which he justly says may have arisen from errors of observation, inaccuracy in the places of the fixed stars from which the distances were measured, and the uncertainty of refractions, when the comet had scarcely ever more than 12° of altitude. "Utinam," he adds, "Jovis et Saturni motus intra tam arctos limites coercere licuit."

He next used the observations of Kepler and Longomontanus in 1607 as tests of comparison with his orbit. The epochs at which he found that the comet had come to its perihelion were,

1682......Sept. 4.
1607......Oct. 16.
1531......Aug. 25.

and as in the earlier instance there had been the longer interval, he now supposed the revolution to be 76 years, and increased his major axis accordingly. The observations indicating that the perihelion distance was to be increased in the same proportion, the similarity of his curve was not disturbed, and he was still able to use the same Table, by which he could ascertain his other elliptic elements. The differences of the observed and calculated places did not, however, now come out so near as before; they amounted in one instance to −34′ 43″ of longitude, and in another to +24′ 10″ of latitude; but he had in this case to discuss observations, which were very far from having the accuracy of Flamsteed's; and the tolerable agreement, which was in general obtained, was possibly all that in such a case could be hoped for. Mars, he had observed, when in conjunction, is repeatedly lost to us for a time in the sun's light, but when it again becomes visible, we have no doubt of its being the same planet: similar reasoning will apply to the comet, although from other causes it becomes invisible to us for a longer time in its aphelion.

The observations of the comet in 1531 by Apian are described in his Astronomicon Cæsareum[l], a work of great scarcity, which Halley was long before he met with, and which he had a difficulty after all in procuring. It is a splendid book, well worthy of so great a monarch as Charles the Vth, to whom it is dedicated; but little dependance for the required precision

[k] Taking the whole together, the mean error in longitude is +24″, and that in latitude is +20″: it may be noticed, however, that there is an appearance of order in the errors of longitude, both as to the signs and quantities, which might proceed from the assumed elements, rather than the variable nature of errors in observation.

[l] Atlas folio, Ingolds. 1540.

could be placed on either the instruments or the observers of those days. The positions of the comet are likewise given by Apian in azimuth and altitude, the former of which must have then been very difficult to observe, and of the latter there is none even so great as $11\frac{1}{2}^{\circ}$ from the horizon. It is not therefore wonderful that the elliptical results did not answer beyond a certain similarity, which however was sufficient to confirm Halley's conviction of the identity of the comet. By reducing the periodic time in this instance again to 75 years, it was found that a consistent approximation might be produced; but he thought it vain to attempt more, when the data were of that description, which admitted of no certainty in the minuter quantities which might be deduced from them.

To have shewn in this manner that the phenomena of the three comets might be produced by motion in an elliptic orbit, which was adapted to the intervals at which they had appeared, would have been no trifling addition to what Halley had previously achieved; but he did not content himself with it. There were variations in each set of elements, which might have been considered as objections to his theory; and by examining these more minutely, he confirmed the truth of his conclusions, and added precision to the details which they involved.

It is known that in consequence of perturbing forces the aphelia of the planets advance, and their nodes recede; whereas the perihelion and nodes of this comet (if supposed to be the same) had moved in the contrary direction; but Halley points out that the comet's motion was in each case retrograde, and consequently, that the same causes, which produced the effects just described on the planets, would make its aphelion recede and its nodes advance.

Again it might be objected, that the inclination was different at each appearance, and that the revolution between 1531 and 1607 was more than a twelvemonth longer than that between 1607 and 1682. This inequality indeed occurred to him at first as the greatest impediment to the admission of his theory; but in 1705 he had pointed out the general cause, which might have affected the duration of the periodic motion, and he now goes more particularly into the question. He calculates the number of days, by which the motion of Saturn may be altered by the attraction of Jupiter, and points out that the effect must be much more powerful in the case of a comet, on which the sun's influence is diminished by greater distance, and whose velocity is such that if it was only increased by one part in 120, the elliptic would be converted into a parabolic course. He shews likewise that the comet in the summer of 1681 was for several months closely under the influence of Jupiter, the effect of whose attraction from this proximity became then about $\frac{1}{50}$ of that of the sun; so that the nature of the comet's curve must have assumed a figure "flexu hyperboliformi Jovem respiciente, . . speciem curvæ admodum compositæ atque hactenus geometricis intractabilis." Jupiter likewise lay almost directly over the comet's path, and to the north, so as to have drawn it up from the ecliptic, and increased the inclination of the plane, in which it was moving[m]. Lastly, the comet, while it was beyond Jupiter, was drawn forward by the combined attraction of the sun and planet; but when it had passed that point, the action of Jupiter tended to retard it. Now, under all the circumstances of the case, the former state must have been of longer duration than the latter, and the velocity of the comet must therefore upon the whole have been increased. He clearly saw the consequence, although he did not, and possibly at the time was not able to estimate the exact amount of it. He says, "hæc levi tantum calamo a nobis tacta;" but the description is relative, and the strength of Halley's pen must be kept in mind when the sentence is applied to these remarks. The increase of velocity would of course carry the comet further off in its aphelion, and add to the length of time which must elapse before its return:—but his own remarkable conclusion ought to be expressed in his own words—"Proinde aucta hoc excessu velocitate propria cometæ, probabile fit reditum ejus non nisi post periodum longiorem 76 et amplius annorum, circa finem anni 1758 vel initium proximi, futurum." And the comet came to its perihelion in March 1759[n].

The remark which Halley made with reference to the comets

[m] By referring to the table in the Phil. Trans., it may be seen that the comet's perihelion lay to the north. The column of "Latitudo Perihelii" having been omitted in the second edition, it might have been well to have introduced some notice to mark in each case the side of the ecliptic on which it was situated.

[n] It has been mentioned that Halley generalized too rapidly in thinking that because this comet returned, all the rest must do so likewise: in accordance with this idea he says, "Hic est cometarum quasi Mercurius, arctiore orbe et breviore periodo solem ambiens, dum cæteri omnes latius expatiantur." What would he have thought of Enke and Biela's comets ?

of 1532 and 1661 has been noticed in the account of the first edition of the Synopsis: whether he abandoned the idea of their identity without further examination, or whether upon trial he found that an elliptic orbit for the revolution of 129 years would not answer to the observations, is uncertain, as he tells us nothing on the subject. But if he now omits[o] all mention of this speculation, he notices another comet which had subsequently occurred to him as moving in a much longer period.

David Gregory died in 1709, and in 1715 an English edition was published, in two volumes, of his Astronomy. The translation is said by tradition to have been made by himself, and there seems to be good reason for believing that it was so. It is described in the title as being "done with corrections and additions," which would hardly have been hazarded by an editor; and traces[p] of the author himself are to be found in the additional matter. Halley, however, was probably connected with the publication; for there was the intention of printing a third volume, to contain astronomical tables, derived "from the true theory of gravity," and not "drawn from a different principle," like those which are said to have previously been in use. When it is recollected that Halley must at this time have been engaged in drawing up tables which will well answer to this description, there can be little doubt of his having contemplated this plan of publishing them. At all events it must have been, not only with his consent, but by his direct authority, that the Synopsis was annexed to the second volume; for this reprint contains much which was not in any of the previous editions, and which he must have himself inserted. Halley had at first expressed his belief "that the comet which Apian observed in the year 1531 was the same with that

[o] Possibly the case is stronger than that of mere silent omission; for after stating the arguments and facts respecting the comet of 1682 (the place in which the circumstances of 1661 and 1532 were originally introduced), he begins the next sentence by saying, "ut pari evidentia ac in nostro hoc anni 1682, non constat rediisse aliquem cometam. Verum si quid argumenti ex æqualitate periodorum et ex phænomenis similaribus peti possit, mirus ille cometa qui anno 1680 fulsit, unus idemque fuit qui anno 1106 . . . e solaribus radiis primum emersit."

[p] The eighth proposition of the third book, on the Cassinian curve, is altered and much enlarged. Hearne (in one of his MS. memorandum books) tells us that Gregory cancelled the sheet (E e), which contained the original Latin, in consequence of objections that Halley had pointed out to him against what had been printed in it. There might, therefore, be some suspicion of another hand in this place, if the additional matter had not been introduced by the following sentence (p. 397), which none but the author could have used: "In my second thoughts upon it, its different species and some properties . . . not sufficiently known presented themselves to me."

which Kepler and Longomontanus took notice of and described in the year 1607;" but in this text it is spoken of as "the same which Kepler and Longomontanus more accurately described in the year 1607." Now this is completely in unison with Halley's later examinations, in which he found that the observations of 1607, though less precise than Flamsteed's, enabled him to represent in an ellipse the places of the comet much more nearly than those of Apian. In this edition also, with reference to the same body, the writer says (in the first person), "I find . . . another double period of 151 years" for it; which is exactly the interval from which Halley set out for his elliptical calculations. More than two octavo pages[q] are then for the first time introduced, in which the very particulars are brought forward, with respect to the comet of 1680, which are found in the second edition of the Synopsis; and not only are these particulars alike, but the very same order of statement is exactly preserved in both publications. It is therefore impossible to doubt of Halley's having been himself the person who communicated and introduced these important changes. Indeed Newton, in the third edition of the Principia[r] (1726), gives him expressly the credit of having pointed out the revolution of the comet of 1680 in a period of 575 years; and Whiston, who had reprinted what he found annexed to Gregory, did the same in his Theory of the Earth[s].

This publication in 1715 gives us a valuable date. The passage on the comets of 1532 and 1661 is not omitted, which shews that Halley had not then relinquished the possibility of their being the same; and he concludes the remarks on the comet of 1680 in a manner which marks the progress he had been making in the inquiry. "I have lately (he says) found out a ready method to compute the motion of comets in elliptic orbs, of which perhaps shortly we may exhibit a specimen, giving this comet for an example[t]."

q P. 901—903.

r P. 501.

s P. 187.

t The writer in the Biographia Britannica, in speaking of Halley (vol. iv. p. 2519. note [UU]), says, that "in the second edition of the Miscellanea Curiosa in 1727, the doctor signified that he had made some progress in calculating the elliptical orbits of the two comets above-mentioned" (1680 and 1682), "declaring at the same time his resolution to complete it." There is possibly some mistake in this notice. After a diligent search no edition has been found of the second volume of the Miscellanea Curiosa of a later date than 1723. In the text of the Synopsis, as this contains it, there is no passage like that which is here alluded to. Derham may certainly have reprinted the second volume when he revised and corrected the first in 1726, and the third in 1727; and

This last passage would seem to indicate that, notwithstanding the superior evidence with which the return of the comet of 1682[u] is put forward in the Synopsis, Halley really returned in the first instance to that which had most arrested his earlier attention. The great comet of 1680 was that to which we are indebted for more particularly fixing the attention of astronomers on these important phenomena: it was probably the first on which Halley employed himself, and it seems to have been that for which he began his elliptical calculations. This is confirmed by the manner in which the same passage is expressed in the Latin of the Synopsis, "compos factus methodi qua calculus in orbe elliptico quantumvis eccentrico accurate et perfacile absolvitur." The method that would succeed with so eccentric a comet as that of 1680, might with confidence be afterwards applied to any other. Halley therefore might have been inclined to settle the question by making his trial at once on the extreme case. The result occupies the concluding part of the Synopsis. A comet was seen in 1106, which from similarity of appearance, and the place which it occupied in the heavens, had a resemblance to that of 1680, and by going back it was found, that at 531, and 44 years before Christ, remarkable comets were recorded to have been seen. These intervals were equal, and to verify the possibility of their being the same, Halley calculated the new table which was necessary for an ellipse, whose major axis was accommodated with the proper eccentricity to a revolution of 575 years. By the assistance of it he found nineteen places between Nov. 3, 1680, and March 9, 1681, which did not differ from those which had been observed by more, in any instance, than $-2' \, 31''$ of longitude, and $+2' \, 29''$ of latitude[w].

Notwithstanding this agreement, too much weight must not be attributed to the accordance. M. Pingré recalculated the orbit from the observations of this same comet, and he found the time

he may in that case have preferred the text of 1715 to that which was originally printed in 1705. But this supposition will not remove all the difficulty: for Halley in the later edition only mentions his having satisfied himself on a method of computation, and his having the intention of applying it to the comet of 1680. He completed this and the calculation for the comet of 1682 (which he had printed many years before 1727), but no work has been met with from which the progress can be traced between 1715, and the completion of the whole as published with the Astronomical Tables.

u "*Nostro* hoc anni 1682," p. 19, note o.

w The mean of the whole errors is $-35''$ in longitude, and $-6''$ in latitude.

of revolution to be 15864 years[x]: Enke also, though he determined an ellipse for it, found that a parabola answered better to the observed places than any other figure[y]. The earlier appearances are only connected by the equal intervals, and even these (if they do not for such very distant periods prove too much) can give no ground for expecting that the same uniformity shall be continued for another revolution. The perihelion distance brought it within the orbit of Mercury, and Halley himself points out, that in 1680 it passed so very near to the two inferior as well as the three superior planets, as to have all its elements subject to very great alterations. He adds that, if it had come thirty-one days later to its perihelion, it would have been scarcely as far from our globe as a quantity equal to the semidiameter of the sun: and he concludes by saying, "collisionem vero vel contactum tantorum corporum ac tanta vi motorum (quod quidem manifestum est minime impossibile esse) avortat Deus O. M.; ne pereat funditus pulcherrimus hic rerum ordo et in chaos antiquum redigatur." So strong was this idea in Halley's mind, that in 1724 he published two papers in the Phil. Trans.[z] on the probability of the general deluge having been produced by the shock of a comet against the earth. This alarming view seems to have been the consequence of his thoughts long dwelling on these bodies. He concluded his paper in 1705 in a different tone, " Quæ vero ab hujusmodi allapsu vel contactu vel denique collisione corporum cœlestium (quæ quidem omnino non impossibilis est) consequi debeant, rerum physicarum studiosis discutienda relinquo." Immediately before this passage he had even dwelt on a suggestion of Fatio de Duillier, that the sun's parallax might possibly be determined from the motion of comets, which nearly approach the earth ; and from what he there says, the general impression must be, that such an opportunity of settling this great question was rather to be wished for than deprecated. The whole passage is retained in the English annexed to

x Cometographie, vol. II. p. 26.

y Olbers in Braude's Journal, vol. XVII. p. 87.

z Vol. XXXIII. p. 118, 123. This seems to have been a subject on which Halley had continued long and repeatedly to speculate. He at first thought that the deluge might probably be attributed to a temporary alteration in the position of the centre of the earth's gravitation, or to a change in the position of its poles (Ph. Tr. vol. XVI. pp. 6, 406), and for the latter effect he had recourse to the possible influence of a comet.

Gregory's book, but was omitted in the second edition [a], very probably from Halley's having devised the more practicable method from the transit of Venus, which he inserted in the Philosophical Transactions for 1716.

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a Delambre (Hist. del' Astron. au dix huitième siècle, p. 133), after speaking of Halley's expressions of alarm, goes on to observe, "Le Monnier dans sa traduction s'efforce d'atténuer cette dernier réflexion, et il insiste sur l'occasion, que ces rencontres nous fourniraient pour mieux connaître la parallaxe du soleil, donc je ne vois pas un mot ni dans le Latin de Halley, ni dans la traduction de Lalande en 1759." From this remark it is evident that the historian either wholly overlooked the early paper in the Phil. Trans., or satisfied himself with the general impression of its containing nothing which was not to be found in the later edition of the Synopsis.