Available online at www.sciencedirect.com
ScienceDirect
ELSEVIER
Historia Mathematica 35 (2008) 329–341
HISTORIA MATHEMATICA
www.elsevier.com/locate/yhmat

Reviews

Edited by Adrian Rice and Antoni Malet

All books, monographs, journal articles, and other publications (including films and other multisensory materials) relating to the history of mathematics are abstracted in the Abstracts Department. The Reviews Department prints extended reviews of selected publications.

Materials for review, except books, should be sent to the Abstracts Editor, Sloan Despeaux, Western Carolina University, Cullowhee, NC 28723, USA. Books in English for review should be sent to Adrian Rice, Department of Mathematics, Randolph-Macon College, Ashland, VA 23005-5505, USA. Books in other languages for review should be sent to Antoni Malet, Universitat Pompeu Fabra, Departament d'Humanitats, c. Ramon Trias Farga 25–27, Barcelona, 08005, Spain.

Most reviews are solicited. However, colleagues wishing to review a book are invited to make their wishes known to the appropriate Book Review Editor. (Requests to review books written in the English language should be sent to Adrian Rice at the above address; requests to review books written in other languages should be sent to Antoni Malet at the above address.) We also welcome retrospective reviews of older books. Colleagues interested in writing such reviews should consult first with the appropriate Book Review Editor (as indicated above, according to the language in which the book is written) to avoid duplication.

Jean le Rond d'Alembert, Oeuvres Complètes, Série I, Traités et mémoires mathématiques, 1736–1756, Volume 7, Précession et nutation (1749–1752)
Edited by Michelle Chapront-Touzé and Jean Souchay. Paris (CNRS Éditions). 2006. clix + 492 pp.

The volume of d'Alembert's *Oeuvres Complètes* here under review consists primarily of a new edition of d'Alembert's *Recherches sur la precession des equinoxes, et sur la nutation de l'axe de la Terre, dans le système newtonien.* This work, the first ever to give a correct explanation of the precession and nutation, was originally published in July, 1749, and has never until now been republished in its entirety, or submitted to sustained editorial scrutiny and detailed correction. These tasks have now been carried out brilliantly and with all imaginable care by Michelle Chapront-Touzé and Jean Souchay.

The volume contains as well (on pp. 369–405) an edition of a manuscript by d'Alembert, "Observations sur quelques mémoires, imprimés dans le volume de l'Académie 1749." D'Alembert here comments on three memoirs by Euler, contained in the volume of the Berlin *Mémoires* for 1749—a volume published in 1751. D'Alembert's comments were sent to Berlin for publication in the Berlin *Mémoires*, but were never published there during d'Alembert's lifetime. The first of the Euler memoirs that d'Alembert comments upon concerns the precession and nutation; it bears the same title as d'Alembert's treatise, except in omitting the final phrase *dans le système newtonien.* In it Euler makes no mention of d'Alembert's treatise—a fact which puzzled d'Alembert and wounded his feelings, and may strike us also as puzzling.

We would first acknowledge the enormous task that Chapront-Touzé and Souchay have carried out in their editing of these works. D'Alembert's treatise as originally published is an arduous and sometimes frustrating assignment for the reader. The diagrams for the propositions are crowded together on four plates at the end of the volume; they are inexpertly made, and lack the cues that could facilitate their intended, three-dimensional interpretation. In the text, numerous symbols, chosen with little apparent system, must be distinguished and remembered; typographical errors, *e.g.*, prime marks wrongly inserted or omitted, are a further source of confusion. At various junctures, the reader yearns for a reconnoitering guide to inform him where he has arrived in the complex argumentation he is slogging through.

Already in September, 1749, d'Alembert's friend, the Swiss mathematician Gabriel Cramer, had written d'Alembert, conveying some of these complaints.

The editors of the new edition have spared no effort to smooth the difficulties for the reader. Each proposition is accompanied by extensive footnotes, which serve to clarify the argument of the particular proposition as well as its place in the overall course of the reasoning, and to supply historical information illuminating why d'Alembert proceeds as he does. Such technical terms as "force," "puissance," etc., have different meanings for d'Alembert than they may suggest to us, and these differences are carefully explained. The footnotes swell the new volume to nearly twice the size of the original. All the diagrams have been redesigned, with dotted and dashed lines added to bring out their three-dimensional character, and each diagram is placed (as it should be!) adjacent to the proposition it illustrates.

An extensive "General Introduction" (pp. xiii–cxxxviii) provides background for reading the treatise. Of the sixteen topics dealt with, we mention those that are especially helpful in clarifying what d'Alembert's treatise achieves.

(I, XI). The precession of the equinoxes, discovered in Antiquity, is a motion of the stars eastward with respect to the equinoxes, or (after the triumph of heliocentrism) of the equinoxes westward with respect to the stars; thus Hipparchus in about 130 B.C.E. found that in 200 years Spica's distance westward of the autumnal equinox had lessened by 2°. Isaac Newton was the first to propose a mechanical cause for this phenomenon, in the Moon's and Sun's gravitational action on the Earth's equatorial bulge. The steps of Newton's explanation are, however, seriously flawed, his mistakes being principally due to his lack of a correct understanding of the mechanics of rotational motion. An important accomplishment of d'Alembert's treatise was to explain, for the first time, the errors in Newton's explanation.

(II). With regard to the obliquity of the ecliptic (the ecliptic's inclination to the Earth's equator), d'Alembert in his treatise followed the astronomers of his day in assuming its constancy since ancient times at 23°29′. The larger values given by ancient astronomers were dismissed as erroneous (which they were in part). Euler in 1756 showed that, owing to planetary perturbation of the Earth's motion about the Sun, the obliquity is during the present age diminishing at a rate of about 47″ per century. This secular diminution is accompanied by a slight alteration of the precession of the equinoxes, called "planetary precession" to distinguish it from the much larger "luni-solar" precession caused by the Sun's and Moon's gravitational action on the Earth's equatorial bulge. D'Alembert in his treatise deals only with the latter.

(III). A major inducement leading d'Alembert to take up the topic of the precession was James Bradley's discovery of a nutation in the Earth's axis. This discovery, which Newton in his *Principia* had neither predicted nor imagined, was officially announced by Bradley in 1748, after it had been confirmed by observations of changes in the zenith distances of stars continued through twenty years. But already in the late 1730s Bradley had informed the astronomers of the Académie of the essentials concerning it. What Bradley found was that, putting aside the regular changes in zenith distance due to the mean precession of 50″ in longitude per year (with the latitude remaining constant), the stars undergo a libration in declination that is completed in some 18.6 years. This is the period of the recession of the nodes of the lunar orbit (its two intersections with the ecliptic) through 360°. Bradley reasoned that the Moon's gravitational action on the Earth's equatorial bulge would be of maximum effect in shifting the Earth's axis when the Moon's orbit was most inclined to the Earth's equator (this occurs when the ascending node of that orbit is in 0° of Aries), and of minimum effect when the Moon's orbit was least inclined to the Earth's equator (this occurs when the ascending node is in 0° of Libra). Bradley used a geometrical model due to John Machin to elucidate the expected variations in the precession and declination of stars, and it is this model that d'Alembert undertakes to substantiate theoretically.

(IV, X). Both Huygens and Newton had concluded from mechanical principles that a spinning Earth would be flattened at the poles. Newton deduced that, for an Earth originally liquid, homogeneous, and spinning, hydrostatic equilibrium would require the flattening,

$$\frac{\text{equatorial radius} - \text{polar radius}}{\text{equatorial radius}},$$

to be 1/229. For an Earth not homogeneous but denser toward the center (this is the more plausible model), the flattening would be less. Measures of a degree of the meridian at the Arctic Circle and at the Earth's equator, obtained by two expeditions sent out by the Paris Académie in the 1730s, yielded much larger values, incompatible with the plausible model. D'Alembert in his treatise accepted 1/178 for the flattening, but recognized and discussed the difficulty of accounting for it. The issue was finally resolved in the years just before and after 1800 by new measures and new analyses. (The value accepted today is 1/297.)

(V, VI). 1747 and 1748 were years of crisis for the Newtonian law of gravitation. The mathematicians Euler, Clairaut, and d'Alembert, seeking to derive the inequalities of the Moon from Newton's inverse-square law, had by September of 1747 reached the conclusion that that law yielded no more than half the motion of the Moon's apogee. (The apogee advances approximately 40° per year.) Meanwhile d'Alembert, starting perhaps as early as 1745, had been working to derive the nutation of the Earth's axis discovered by Bradley; it appears that by September, 1748 he had obtained the differential equations for the problem, but had obtained a result for the nutation that was far too large.

The first of these problems was resolved by Clairaut. By December, 1748, he had realized that his first result for the motion of the Moon's apogee would likely be modified significantly by higher-order approximations. The following May he presented to the Académie a solution incorporating the first- and second-order approximations, and yielding almost all of the observed motion of the apogee.

Since the spring of 1746 d'Alembert had been working on the elaboration of a lunar theory, and by mid-summer of 1748 had practically finished it. In the course of this work he had become increasingly convinced that the Moon's inequalities would prove in accord with the inverse-square law. Preferring not to compete with Clairaut in working on the problem of the Moon's apsidal motion, he turned full-time to the problem of the nutation—a problem that, so far as he knew, no-one else was working on, hence 'virgin.' The flaw in his earlier approach to this problem, he soon found, had been the failure to take into account the rotational motion of the particles of the Earth about the Earth's axis. The new problem was to deal with the changes in these motions and find how they influence the motion of the axis. In his solution of this problem d'Alembert invoked d'Alembert's principle—a principle he had earlier introduced and applied to problems in which the rotational axis was fixed. In the case of the precession the axis was free to move. This is the nub of the difficulty; and the fact that it was resoluble with the aid of d'Alembert's principle was the triumph of d'Alembert's solution. His book was in press by May of 1749. It supplied new confirmations of Newton's inverse-square law.

(VII). In this section Chapront-Touzé and Souchay provide an analysis of the innovations of d'Alembert's treatise in mechanical concept and procedure. These include: the notion of an instantaneous axis of rotation, understood as the ensemble of points of the solid which have zero velocity at the instant considered; and the devices and concepts involved in applying d'Alembert's principle to derive the motion of the Earth's axis.

Twenty years ago the present writer published a comparative study of d'Alembert's and Euler's memoirs on the precession and nutation ("D'Alembert *versus* Euler on the Precession of the Equinoxes and the Mechanics of Rigid Bodies," *Archive for History of Exact Sciences*, **37** (1987), 233–273). In it I claimed that d'Alembert in applying his principle succeeded only by a double error of sign. Chapront-Touzé and Souchay find no evidence for this error, and I am no longer convinced that any such error occurs; I must conclude that my claim emerged out of confusion. D'Alembert did not deserve this slight.

I remain persuaded, following Truesdell, that d'Alembert fails to give the correct basis for his principle. In Chapter II of his treatise on the precession, he enunciates this principle as follows. Consider a particle of the Earth having mass μ. Let Ψ be the accelerative force exerted on μ by the Sun, and Ψ' the accelerative force exerted on it by the Moon; the motive forces exerted by the Sun and the Moon on μ will then be $\mu\Psi$, $\mu\Psi'$. Let the particle μ have the velocity u at time t, and the velocity u' at time $t+dt$. "If," says d'Alembert, "we regard the velocity u as composed of the velocity u' and another velocity u'' which is infinitely small, the system of all the particles of the body (the Earth), each animated by the velocity u'', must be in equilibrium with the forces Ψ, Ψ'."

Much is left tacit in d'Alembert's statement and application of his principle. The forces of constraint that keep the body rigid are left unmentioned. Yet according to Euler, "the internal forces destroy each other mutually, so that the continuation of the motion requires external forces..." ("Discovery of a New Principle of Mechanics," *Leonhardi Euleri Opera Omnia*, II, **5**, 81–108). This equilibrium of the forces of constraint, implied by the supposition of the Earth's rigidity, is the very source of d'Alembert's principle. In Truesdell's articulation of this (*The Rational Mechanics of Flexible or Elastic Bodies, 1638–1788*, in *Leonhardi Euleri Opera Omnia* II, **11**(2)), if $\boldsymbol{a}$ (**boldface** means vector) is the acceleration of any mass-element of the rigid body, then $\boldsymbol{a} = \boldsymbol{a}_f + \boldsymbol{a}_c$, where $\boldsymbol{a}_f$ is the acceleration of the mass-element that would result from the externally applied forces acting alone, and $\boldsymbol{a}_c$ is the acceleration that results from the mutual actions of the mass-elements. D'Alembert's principle comes from the fact that the accelerations

$a_c = a - a_f$ form a system in static equilibrium, so that

$$\sum \mu(a - a_f) = 0, \qquad \sum r \times \mu(a - a_f) = 0.$$

Here μ is the mass of a particle, and r its vector distance from the instantaneous axis of rotation. It was in effect the second of these conditions (an equilibrium of moments) that d'Alembert used in deriving the precession and nutation, but the vector expression of a moment was not available to him.

Euler found d'Alembert's statements of principle less than lucid. In a letter that reached d'Alembert on January 3, 1750, d'Alembert reports Euler as stating (my translation)

> ...that he had received and read my book, that he had already applied himself to this subject for some time, but not finding himself able to overcome all the obstacles he met, he had been obliged to abandon it entirely. He adds in this same letter that in truth he has found himself unable to follow me, but that after seeing in a general way how I vanquished the difficulties that had previously defeated him, he had recommenced his investigation in his own manner, and that he was so fortunate as to bring it to completion....

On March 5, 1750, Euler read his own *Recherches sur la Précession* at the Berlin Academy. Two days later he wrote d'Alembert again, this time giving a more extended account of his struggle to derive the precession (*Leonhardi Euleri Opera Omnia*, IV A, **5**, 306; my translation of the relevant passage):

> I applied myself repeatedly and for a long time to the problem of precession, but I always encountered an obstacle – the great number of circumstances that have to be taken into account, and above all this problem: given a body turning about any axis freely, and acted upon by an oblique force, to find the change caused both in the axis of rotation and in the motion. The solution of this is absolutely required for the subject you have so happily developed. But with respect to this problem all my investigations had been unavailing so far, and I would not have applied myself to it further, if I had not seen that the solution must necessarily be encompassed in your treatise, although I was not able to find it there, which at first increased so much the more my desire to develop your whole method. But I must also confess that I could not follow you in the preliminary propositions you employed, for your way of carrying out the calculation was not yet very familiar to me.... But now that I have succeeded better in the investigation of this same subject, having been assisted by some insights in your work by which I was little by little enlightened, I have come to be able to judge your excellent conclusions.

Chapront-Touzé and Souchay suggest that Euler's phrase "the preliminary propositions you employed" refers to d'Alembert's Chapter II, "Propositions de géométrie & méchanique, nécessaires pour la solution du problème." And these propositions are indeed complicated. In order to determine the effect of a torque, d'Alembert reduces the problem geometrically to a case of statics representable in a plane. Euler, by contrast, had for years been in the habit of reducing the action of torques to forces acting on given radius arms. Because of this, and also of his employment of the rules of spherical trigonometry, his argumentation is simpler and more straightforward than d'Alembert's.

In another respect, however, d'Alembert's argumentation is superior. He solves the second-order differential equation of his problem, and is thus enabled to compute the maximum possible angle between the Earth's axis of figure and axis of rotation; he shows that that angle is tiny. Euler appears to be dependent on this result to justify identifying the two axes, and approximating the moment of inertia of the Earth as that of a sphere rather than that of a spheroid.

After settling the problem of the precession and nutation to his own satisfaction, Euler went on to systematize the mechanics of rigid bodies. And so this branch of mechanics as we have it today, and indeed as it has been accepted since the late 18th century, shows everywhere the shaping and standardizing hand of Euler. The concept of 'moment of inertia,' the notion of principal axes, the equation for torque in analogy with $F = ma$ (namely, $\tau = I\alpha$), the 'Euler angles' in a coordinate system fixed within the rotating body—these formulations were Euler's.

As has sometimes been remarked, the route by which a discovery is first reached is not always the clearest route for later learners to follow. In a late memoir, Euler at last gave a fitting appreciation of d'Alembert's achievement:

> ...before the celebrated d'Alembert no one, so far as is known, undertook the investigation of this sort of motion [the precession]... For since the Earth, moving freely to and fro in the aether and acted upon by the forces of the Sun and Moon, does not so rotate that its axis remains always parallel to itself, its true motion cannot in the least be accounted for by the rules developed for the simpler kinds of motion. Whence this most acute man was forced to call to his aid much

more lofty rules, which are of such a character that by their help it appears possible to define any other motions whatever of this kind, however complicated, with the same success (*Leonhardi Euleri Opera Omnia*, II, **9**, 413).

Curtis Wilson
St. John's College,
Annapolis, Maryland, USA

Available online 1 April 2008