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Review

Reviewed Work(s): Die Berliner und die Petersburger Akademie der Wissenschaften im Briefwechsel Leonhard Eulers. Teil I. Der Briefwechsel L. Eulers mit G. F. Muller, 1735-1767 by A. P. Juskevic, E. Winter and P. Hoffmann

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A. P. JUSKEVIC; E. WINTER; P. HOFFMANN (Editors): *Die Berliner und die Petersburger Akademie der Wissenschaften im Briefwechsel Leonhard Eulers. Teil I. Der Briefwechsel L. Eulers mit G. F. Müller, 1735-1767.* 327 pp., 2 plates, bibl. Berlin: Akademie-Verlag, 1959. DM 32.50.

Those who regard the scientific academy of the eighteenth century as a grove of pure devotion to learning will be disillusioned by this volume. Euler and Müller had been colleagues in the Petersburg academy for about fourteen years when Euler went to Berlin in 1741. Although their written correspondence begins earlier, while Müller was travelling for a decade as a geographer in Bering's second expedition to Kamchatka, 189 of the 207 letters published in this volume come from the period when Euler was acting as counsellor and virtual editor for the Petersburg academy while being director of the mathematical class of the Berlin academy. Their grey pages show us that administrations and administrators have changed little. Euler's abundant energy sufficed to provide an endless sequence of recommendations of mediocrities which would grace the files of a dean today. While Euler himself stood above petty quarrels and intrigues, he could not ignore them, since they made up almost the whole fabric of academy life, and the correspondence contains many direct and indirect references to them.

Since Müller, although an industrious author and a geographer of some fame, was of no great intellectual power, there is little that concerns mathematical problems, and the many remarks about natural science are more in the nature of news than of discussion. The most frequent topic is the construction of achromatic lenses, but Euler shows knowledge and interest in every aspect of science. While the editors have not provided an index of subjects, it is replaced by adequately referenced summaries in the interesting and thorough introduction.

It is a truism that everyone since 1750 has learned calculus and rigid dynamics directly or indirectly from Euler's books. Those who concern themselves with the dissemination of knowledge and those

who measure success by the whirl of the presses may ponder the fact that out of an edition of 500, 406 copies of the *Differential Calculus* remained unsold after six years, and almost as many another five years later. Copies turning up from dealers today are often unopened. In 1760 Euler finished his *Rigid Bodies*; by 1762, only twelve persons had subscribed, though no deposit was requested; when the book appeared in 1765, scarcely any copies were sold. At this same time the academies, always on the brink of bankruptcy, were pouring great sums into the collective efforts of the nobodies who filled their numerous offices and whose busy little lives have scarcely scratched a groove in the history of science.

Since the correspondence with Müller was official rather than private, only by implication can anything be learned of the miserable treatment Euler received from Frederick II after the death of Maupertuis, and of the true reasons causing him to let it be known that he would welcome a chance to return to Russia. Only in one letter, No. 169, dated 27 May/7 June 1763, does Euler give vent to his feelings at what was happening:

Dass Mr. d'Alembert die höchstansehnliche und einträgliche Stelle in Russland ausgeschlagen, wollte ich eher einer Furcht, dass die Sache zuletzt übel ablaufen möchte als der Philosophie zu schreiben. Da er seines unerträglichen Hochmuths ungeacht längst begreifen konnte, dass er sich dazu gar nicht schickte. Sonsten besteht seine Philosophie nach des H. Bernoullis Ausdruck in einer impertinente suffisance, dass er alle seine Fehler auf das unverschämteste zu vertheidigen sucht, welches ihm nur gar zu oft wiederfahren, so dass er seit vielen Jahren aus Verdruss nichts mehr mit der Mathematic zu thun haben will. In seiner Hydrodynamic hat er die meisten Sätze des H. Bernoullis, die doch durch häufige Erfahrungen bestätigt waren, gantz cavalierelement wiederlegt, da doch seine eigenen der Erfahrung widersprechen. Und er hat seinen Hochmuth noch nicht so weit überwinden können, dass er seinen offenbaren Irrthum hätte bekennen wollen.

Seine Zänkereyen mit dem gründlichen H. Clairaut können ihm bei Verständigen nicht anders als zur grösten Schande gereichen. Allein, hier trifft er ein esprit createur, un homme qui embrasse tout: Doch will er ohne Zweifel aus gleicher Ursache nicht hieher kommen, und er soll den Chevalier de Jaucourt an seine Stelle zum Praesidenten unserer Academie vorgeschlagen haben. Inzwischen

habe er sich auf das inständigste Anhalten dahin entschlossen, eine Reise nach Cleve zu thun, wo er gestern hat eintreffen sollen, um das gantze Schicksal unserer hiesigen Academie zu entscheiden. Soviel verlaudet, dass eine Menge Franzosen, lauter esprits createurs hieher beruffen werden sollen.

These paragraphs reflect but do not reveal the extent to which the intellectual history of the eighteenth century remains to be discovered. This volume, well annotated and edited despite a few dubious paragraphs here and there, furnishes a permanent source; much more, unpublished, remains to be seen and used before the science of the Age of Reason can emerge from the conventional fictions repeated in the standard histories of mathematics and science.

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H. W. TURNBULL (editor): *The Correspondence of Isaac Newton*. Volume I. 1661-1675. xxxviii + 468 pp., pl. Cambridge: published for the Royal Society at the University Press, 1959. \$25.00.

Scholars have long awaited the publication of the first volume of Newton's correspondence, prepared by Professor Turnbull. The project was initiated early in 1939 by a resolve of the Council of the Royal Society, and an editor was designated, Professor H. C. Plummer, who began the preliminary work in July 1939. Interrupted by the war, and held up by Professor Plummer's illness, the job fell to Professor Turnbull in 1947. This volume is the first happy conclusion of his labors, and in reviewing it we congratulate him and the sponsoring Royal Society for a notable contribution to the history of science.

The first volume of the correspondence—covering the exciting years of Newton's first paper on light and colors and the controversy it aroused—contains 156 letters and papers, of which only 19 have not previously been published. One of these is a pitiful fragment of a few lines written to Newton by his mother in 1665; another (Collins to Newton, 1671) informs Newton of Gregory's mathematical discoveries; another is an autograph draft by Newton of the construction of his new tele-

scope. A number of letters are from Henry Oldenburg, Secretary of the Royal Society, informing Newton (8 Feb. 1671-2) of the reception given his paper on light and colors and the decision to print it, or telling Newton (2 May 1672) of Cassegrain's telescope; while others either (2 July 1672 and 24 Sept. 1672) give Newton extracts from Huygens on Newton's telescope and theory of colors or (16 July 1672) discuss the speculum for a telescope. A letter from Collins (30 July 1672) discusses a few questions of mathematics and asks for Newton's opinion concerning Borellius' *De motionibus naturalibus a gravitate pendentibus*; and another (20 Aug. 1672) is purely mathematical, but contains what is described in an editorial note as "a rare instance of Newton accepting without qualification the mathematical result found by another." A document of ca. 1673 describes a combination of reflecting and refracting telescope; an extract of a letter from Towneley to Oldenburg (24 April 1673) suggests that Newton's "thoughts of colours" should be translated into Latin. Oldenburg (4 & 7 June 1673) tells Newton that Huygens has sent a copy of his new book *Horologium Oscillatorium* to be presented to Newton, and sends Newton an extract from a letter of Huygens discussing the Newtonian theory of colors; then (14 Sept. 1673) Oldenburg tells Newton of Boyle's new book which Boyle has asked him to send to Newton (and to Henry More and Isaac Barrow). A letter (6 Oct. 1674) to Dary deals with two methods of solving the equation  $Z^n + bz + R = 0$  for  $Z$ , and a letter from Collins to Newton (ca. July 1675) discusses mathematical news in general.

None of these hitherto unpublished documents casts a critical new light on Newton and his circle, but the remaining new document, a letter from Robert Hooke to Lord Brouncker (ca. June 1672) does. Neither this letter nor its contents apparently ever reached Newton. In it Hooke shows himself pleasantly conciliatory to a degree that contrasts sharply with the impression of his attitude conveyed to Newton by Oldenburg, as the opening lines of this document show: