while every member of this decreasing sequence is independently greater than AD. Hence a limit exists and in this case it is the lower bound, AD, itself. Or we can say that given any small distance ϵ, B and C can move to positions C_n and B_{n+1}, so that

$$AC_n + C_nD - AD < \epsilon$$

or

$$AB_{n+1} + B_{n+1}D - AD < \epsilon.$$

In your review (Vol. XVIII, pp. 285-287) of Adolf Hurwitz' *Mathematische Werke* you call attention to a paper in which he shows how the functions of elementary analysis may be defined by an iterative production of monotonic sequences. I have had the easier parts of this paper translated and have been over the work with my senior boys. In addition to providing a great variety of interesting illustrations of sequences, the "rigidity" of his treatment proved to be an eye-opener to them. I hope to give a précis of the translation in a later number of the *Gazette*.

I am, Sir, Your obedient servant,

N. M. Gibbins.

CAJORI'S EDITION OF NEWTON.

Dear Sir,—With reference to my review of Cajori's edition of Motte's translation of Newton (*Gazette*, XIX, 49), Prof. R. C. Archibald points out that two of my statements need amendment.

(1) It is true that Davis attached no name to the translation of the *De Mundi Systemate* on his title-pages, but in the bibliographical section of the Life of Newton in his edition the ascription is definite (vol. 1, p. liii):

" *A System of the World*, translated from the Latin original; 1727, 8vo.—This, as has been already observed, was at first intended to make the third book of his Principia.—An English translation by Motte, 1729, 8vo."

This paragraph introduces two new dates into the story. The translation which we will agree to call Motte's was certainly issued with the date 1728, and I have no other reference to an edition in 1729. Also the plain implication of the paragraph is that there appeared in 1727 a version that was not in Latin and was not by Motte; this version if it existed has disappeared, but the only reason for supposing that the reference is to the Latin original of 1728 or to a lost edition in Latin is that otherwise the Latin work is omitted from the bibliography.

(2) As everyone knows, "the manuscript in Latin" of the *De Mundi Systemate* which still exists is not Newton's, but a draft in the handwriting of Cotes. It is not disputed that the existence of this manuscript is evidence that the treatise is authentic.

Another mistake is corrected for me by Mr. Zeitlinger. Castiglione says explicitly that the Latin version of the *Method of Fluxions* which he gives is a translation from Colson; the Latin original appeared for the first time, with the title *Geometrica Analytica*, in

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the first volume of Horsley's edition, 1779. That is to say, in 1744 this work also "demanded translation into the universal language"

Yours sincerely,

E. H. Neville.

Reading, 14th February, 1935.

The Editor, *The Mathematical Gazette.*

LAGRANGE'S EQUATION.

To the Editor of the *Mathematical Gazette.*

Dear Sir,—In the *Gazette* for February, 1935, Mr. R. J. A. Barnard, in an article entitled "Lagrange's Equation", criticizes certain statements which are alleged to appear in my textbook on Differential Equations. May I point out that there is a wide discrepancy between what Mr. Barnard imagined I said and what I really said ? I give two examples in parallel columns :

Barnard.	Piaggio.
P. 31. "Piaggio (p. 147, new edition), begins with the statement that the equations $\frac{dx}{P}=\frac{dy}{Q}=\frac{dz}{R}$, ...(1) and $Pp+Qq=R$, ...(2) *are equivalent because they represent the same surfaces.*"	P. 147. "We saw that the simultaneous equations $\frac{dx}{P}=\frac{dy}{Q}=\frac{dz}{R}$, ...(2) represented a family of curves . and that $\phi(u, v)=0$ . . . represented a surface through such curves. Through every point of such a surface passes a curve of the family, lying wholly on the surface. Thus equations (1) and (2) are equivalent, for they define the same set of surfaces."
P. 32. "Yet Piaggio calls it a 'Special Integral', and says that it cannot be deduced from the differential equation or from the given complete integral in the usual way."	P. 150. "It is sometimes stated that all integrals of Lagrange's linear equation are included in the general integral $\phi(u, v)=0$. But this is not so. But $z=0$ satisfies the partial differential equation, though it is obviously impossible to express it as a function of u and v. Such an integral is called special . they can be obtained by applying a suitable method of integration to the Lagrangian system of subsidiary equations. "

It may be conjectured that the first of these misquotations arose from Mr. Barnard picking out one sentence near the end of my page 147 and ignoring the two paragraphs preceding it. No such

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