

correspondence

Newton and Kepler

SIR,—I have only just read Dr J. W. Herivel's review in your journal (*Nature*, January 18, 163–164) of the recently published variorum edition of Newton's *Principia*. There I am fathered with I. B. Cohen's view—"originally put forward [in my article on 'Newton's early thoughts on planetary motion in'] *Br. J. Hist. Sci.* 2, 117–137 (1964)—that Newton "apparently learnt of Kepler's law of areas in 1678 and at once was able to solve the problem of Keplerian planetary motion" (Herivel's italics). Quite bluntly, I have never said anything approximating to that, with or without its emphasis, there or anywhere else.

In my 1964 article I did, to be sure, publish my (then) novel finding that nowhere in his extant early papers "does Newton make any mention of Kepler's area law"—with the qualification in a footnote thereto (page 124) that in his "deep study at an early age of Wren's . . . tract [on Kepler's problem, as published by Wallis in his *De Cycloide*, 80 (1659)] . . . it is just possible his attention was caught by the fleeting reference [there] to the area law"—and then went on to observe (page 131, note 48) that Newton possessed in his personal library a "well-thumbed" copy of Nicolaus Mercator's *Institutiones Astronomicae* (1676) where, as I wrote, "correct enunciation" of the planetary law is given. That it is, as Herivel now says, "stretching credulity to suppose that Newton had not . . . deeply pondered on this extraordinary law before 1679 [sic]" may or may not be everyone's modern reaction; but such an undocumented opinion is certainly irrelevant when, as I did, one comes to examine the available historical evidence. I know nothing of Herivel's recent scholarly activity in this area but after my own nearly twenty years of continuous study of Newton's scientific papers (including many in private possession which are not generally accessible) I will reiterate that I have as yet found nothing which establishes in any way that Newton was, before 1676, aware of the law's verbal enunciation or begins to hint that he "deeply pondered" its meaning at any time before late 1679. If Herivel knows better than I, let him state his sources.

As for the portion of Herivel's quot-

ation which he italicises, this, whatever its truth, greatly traduces the hypothesis which I formulated in my 1964 paper. There I made Newton's "sudden" appreciation of the validity of the area law in 1679 dependent, not on his presumed ignorance of the law's statement up to that time, but on his recognition of its theoretical necessity in his proof of the elliptical orbits of the planets under the condition (as Hooke laid it down in their correspondence in the early winter of 1679–80) that their undeviated motion be uniform and in a straight line. Herivel knows this very well since, on the appearance of my article 10 years ago, he mounted a hasty and often ill-considered but full-blooded attack on it in *Br. J. Hist. Sci.* 2, 350–354 (1965), in the course of which he allowed my hypothesis to be "possible [but] very improbable". That unsupported judgement is evidently now to be sanctified by similarly ruling out

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as "very improbable" a distortion of my original thesis which I have never held. May I ask Herivel to acknowledge as much and to retract his present aspersions?

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DR HERIVEL REPLIES: Dr Whiteside protests at my "fathering" on him the view of I. B. Cohen that Newton "apparently learnt of Kepler's law of areas in 1678 and at once was able to solve the problem of Kepler motion" (my italics). This view seems however, to have been suggested to Cohen by Whiteside's 1964 paper to which he refers immediately afterwards. This does not seem so improbable as it might otherwise appear from Whiteside's letter in the light of the following passage in his paper (page 128):

"We have said enough to justify our general contention that in the autumn of 1679 Newton was, if indeed he at all consciously then recognised its existence, still unwilling to allow Kepler's crucially important second (areal) law even an empirical place among the axioms of his astronomical thought."

I myself derived somewhat the same

impression as Cohen from a reading of Whiteside's paper. For in my "hasty and often ill-considered" paper of 1965 I said:

"to the various explanations already put forward, a new one has recently been added by D. T. Whiteside, namely, that Newton was prevented from solving the problem of Kepler motion before 1679 because up to that time he was either unfamiliar with, or had no confidence in, Kepler's second law of planetary motion".

As regards the second, italicised, part of Cohen's view, although I did not draw this conclusion myself I can quite understand Cohen's doing so. For if Whiteside's paper is interpreted as implying that Newton was unfamiliar with Kepler's second law up to the autumn of 1679, and if he required this law for his solution to the problem of Kepler motion in the following winter, then it does not seem unreasonable to conclude that it was his tardy acquaintance with this law which led to the solution of the problem.

In the light of Whiteside's letter I am quite happy to retract the phrase "This view originally put forward by D. T. Whiteside". But in fairness to I. B. Cohen, and for the sake of the historical record, I should want to replace it by the following: "this view was apparently suggested to I. B. Cohen from a reading of a paper by D. T. Whiteside".

As to the important question of whether or not Newton was consciously aware of the exact, areal form of Kepler's second law before the autumn of 1679, it seems to me that in the light of Dr Russell's findings the onus is on Whiteside to provide documentary evidence for a lack of familiarity on Newton's part. In the absence of this evidence I for one will continue to believe that Newton was perfectly well aware of the exact form of the law long before 1679, most probably from a reading of Kepler's *Epitome*. For if Dr Russell is correct in believing that this famous work of Kepler was "from about 1630–50 or beyond almost certainly the most widely read treatise on theoretical astronomy in Europe" (*Br. J. Hist. Sci.* 2, 20; 1964) then I find it very difficult to believe that it was not read by Newton. Documentary evidence is important, and without it history cannot be written, but its absence does not necessarily preclude the historian from drawing probable conclusions in other ways.