

**NEWTON AND FLAMSTEED:
REMARKS ON AN ARTICLE
IN NUMBER CIX OF THE
QUARTERLY REVIEW**

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Newton and Flamsteed: Remarks on an article in number CIX of the Quarterly review by
William Whewell

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WILLIAM WHEWELL

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REMARKS

ON

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OF THE

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TO WHICH ARE ADDED

TWO LETTERS,

OCCASIONED BY A NOTE IN NUMBER CX. OF THE REVIEW.

BY THE

REV. WILLIAM WHEWELL, M.A.

FELLOW AND TUTOR OF TRINITY COLLEGE, CAMBRIDGE.

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REMARKS,

&c.

It is, for the most part, a sorry occupation to rake up the quarrels and infirmities of persons who have long been the objects of gratitude and respect among mankind. Yet there are cases in which this, though a painful, may be a proper employment. When we have, brought before us, a vast quantity of new and curious documents and evidence, such as has recently been furnished in Mr Baily's singularly valuable and interesting "Account of Flamsteed," it becomes the business of those who care for the good name of great men to revise their opinions of the persons whose characters may be affected by the disclosures contained in such a work. It was therefore very reasonable that those who undertake the office of directing the judgment of the public, as far as it is concerned about literary novelties, should notice any new light which was thrown upon the well known quarrel of Halley and Flamsteed by Mr Baily's excellent publication.

But the Quarterly Reviewer who has taken upon himself this task, appears to me to have performed it without due attention to the most important circumstances of the case. In a dispute where it is very clear that there was great irrita-

tion and anger on both sides, he has taken for his sole guide the statements of one of the parties, written in the warmth of the moment, has identified himself with Flamsteed's most petulant feelings, and has not corrected them by any attention to the case of the opposite party. When the great body of Review Readers are called upon, in this temper, to cast away all their reverence for the most revered name of our nation, it must be right that some one should interpose a warning, and deprecate judgments of such levity and partiality. I would very gladly have left this task to some lover of truth who could give to it more time and labour; for the whole evidence is of very great extent; but, no such person appearing to come forwards, I will at least make a few remarks which may serve to arrest such a sentence as is moved for by the Reviewer.

It is to be observed that if we adopt the Reviewer's opinion, that Flamsteed was throughout a man bitterly wronged, and that there was an extreme of baseness and tyranny on the side of the persons with whom he quarrelled, we involve in our condemnation almost all the eminent literary and scientific men of the day: for we have, acting with Newton, and sharing in his views, not only Halley, the object of Flamsteed's intense dislike, but Gregory, Arbuthnot, Mead, Sloane, Wren. On the other hand we find no one speaking of Newton as Flamsteed does, except Whiston, whose judgment is perfectly worthless, for he was a prejudiced, passionate, inaccurate and shallow man, as might easily be shewn, and as is, I think, commonly allowed. Such a comparison of parties may at least make us pause.

One of the most important remarks bearing upon this unfortunate quarrel is, in my opinion, this: that, the subject of dispute being the publication of Flamsteed's observations and their results, Newton and Flamsteed, from the direction of their pursuits, took views so opposite, of the best manner and proper purpose of such a publication, that they unavoidably differed in their wishes, and might disagree with little or no blame on either side. The purpose for which Newton desired that the world should possess the best observations, was the confirmation of the great Theory of Universal Gravitation;—incomparably the greatest discovery ever made by man; and at that period, we may say, in the agony of that latent struggle by which the confirmation and general reception of great discoveries is always accompanied. We of the present day are accustomed to consider this immense step as effected at once, on the publication of the first edition of the Principia in 1687; but we may easily convince ourselves that this was not so. Even under the most favourable circumstances, a vast theory like this could not make its way at once. No man of Newton's standing (I believe) thoroughly accepted his views: Halley was sixteen, David Gregory nineteen years his junior. In England this acceptance of the theory required half a generation, in France and Germany, more than a whole generation. And during this interval, the result of the struggle depended upon the accordance of the theory with the best observations, which the Greenwich ones undoubtedly were. Upon these observations, then, depended a greater stake in the fortune of science than was ever before at hazard, and this Newton

knew well. How then can one be surprized at the earnestness and importunity with which he begs for Flamsteed's observations; and tries to soothe a jealousy and reserve which appear to have shewn themselves at an early period?

As for your observations, you know I cannot communicate them to any body, and much less publish them, without your consent. But if I should perfect the moon's theory, and you should think fit to give me leave to publish your observations with it, you may rest assured that I should make a faithful and honorable acknowledgment of their author, with a just character of their exactness above any others yet extant. In the former edition of my book, you may remember that you communicated some things to me, and I hope the acknowledgments I made of your communications were to your satisfaction: and you may be assured I shall not be less just to you for the future. For all the world knows that I make no observations myself, and therefore I must of necessity acknowledge their author: and if I do not make a handsome acknowledgment, they will reckon me an ingrateful clown.—*Account of FLAMSTEED, p. 151.*

This the Reviewer has quoted; but he has not quoted what immediately follows, striking as it is.

And, for my part, I am of opinion that for your observations to come abroad thus with a theory which you ushered into the world, and which by their means has been made exact, would be much more* for their advantage and your reputation, than to keep them private till you die or publish them, without such a theory to recommend them. For such theory will be a demonstration of their exactness, and make you readily acknowledged the exactest observer that has hitherto appeared in the world. But if you publish them without such a theory to recommend them, they will only be thrown into the heap of the observations of former astronomers, till *somebody shall arise* that, by perfecting the theory of the moon, shall discover your observations to be exacter than the rest. But when that shall be, God knows: I fear not in your life-

* Erroneously printed "worse" in the work.

Theory
confirms
the data?

time, if I should die before it is done. For I find this theory so very intricate, and the theory of gravity so necessary to it, that I am satisfied it will NEVER be perfected but by SOMEBODY WHO UNDERSTANDS THE THEORY OF GRAVITY AS WELL OR BETTER THAN I DO.—p. 151-152.

I have several times, in reading this passage, felt a kind of terror at the peril to which the success, or at least the speedy success, of the greatest of physical truths is here represented as exposed.

With this consciousness of being in possession of such a truth, while Flamsteed's records of his observations contained the only language in which it could be made generally convincing, we may easily imagine that Newton could not help urging the publication and employment of the observations, in a manner which excited no sympathy in Flamsteed, unconscious of the nature of the then existing crisis in the history of astronomy.

Flamsteed was only four years younger than Newton; he never fully accepted Newton's theory, nor comprehended its nature. Like all astronomers of his time, he understood by "theory" only a mode of expressing *laws of phenomena*, not a new generalisation by which such laws are referred to a physical *cause*. When he talks of his own Theories of the Moon and Planets, it is in such a sense. The truth of what is here asserted is evident from many passages of the book now under consideration, but especially from a letter of Flamsteed's to his friend Lowthorp, dated May 10, 1700. When he was told that Newton had deduced all the inequalities of the moon's motion from the laws of gravity alone,

With some indignation I answered that he had been as many years upon this thing, as I had been on the constella-

tions and planets altogether: that he had made lunar tables once to answer his conceived laws, but when he came to compare them with the heavens, (that is, the moon's observed places,) he found he had mistook, and was forced to throw them all aside: that I had imparted above 200 of her observed places to him, which one would think should be sufficient to limit any theory by; and since he has altered and suited his theory till it fitted these observations, 'tis no wonder that it represents them: but still he is more beholden to them for it than he is to his speculations about gravity, which had misled him. Mr Hobbs boasted that his laws were agreeable to those of Moses. Dr Eachards tells him he doubted not of it, for being drawn from Moses' works, and copied into his, he might be sure they would agree, except the laws of Moses were flown, which he was sure they were not.—p. 176.

It is manifest here that Flamsteed attached no more value to Newton's laws of nature than he did to Hobbes' laws of nations.

Flamsteed's view of the value of observations and of their publication was probably nearly the same as that of the mere practical astronomers of all ages; that is, that the observations were to be compared with known laws, so as to improve their general accuracy; and that then, if the occasion occurred, additional laws of phenomena should be made out by conjectures empirically confirmed. But besides this view, in which there is nothing to blame but its limited character, he appears to have thought too directly of their value as the means of purchasing reputation. How otherwise are we to account for the jealousy with which he objected to Newton's combining Cassini's observations of the comet of 1680 with his? when it must have been clear, even with his own notion of a theory, that the truth of the theory would be the better established, the more observations it agreed with. This is Flamsteed's