

**SUPPLEMENT TO THE ACCOUNT
OF THE REVD. JOHN FLAMSTEED,
THE FIRST ASTRONOMER-ROYAL;
PP. 675-751**

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JOHN FLAMSTEED & FRANCIS BAILY

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Presented by the Author to

Harvard College

SUPPLEMENT

TO THE

ACCOUNT

OF THE

REV^D JOHN FLAMSTEED,

THE FIRST ASTRONOMER-ROYAL.

By FRANCIS BAILY, Esq., F.R.S.

&c. &c. &c.

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SUPPLEMENT.

THE following sheets are intended as a *Supplement* to my *Account of the Rev. John Flamsteed*, which was printed in the year 1835 at the public expense, for a limited distribution only; a work that has excited a degree of interest much greater than I could have anticipated, and probably arising from the prominent manner in which some of the circumstances, relative to the conduct of two eminent and distinguished individuals therein mentioned, have since been brought forward, by other parties, before the public. For, whilst on the one hand the brilliant talents of Halley and the still more transcendent mind of Newton have been set in opposition to the less splendid yet very useful labours of Flamsteed, and arguments drawn from this relative position of the respective parties, we see that on the other side the ardent zeal of the Astronomer Royal, his piety, his accuracy, his integrity, and his independent spirit, have been arrayed against the alleged misconduct of his two powerful opponents. On the accuracy or propriety of the various opinions that have been formed and expressed upon the principal features of the dispute in question, it is not my intention in this place to make any remarks: all the facts connected therewith, as far as I have been able to collect and arrange them, are before the public, and each reader will form his own conclusion according to his own particular view of the case.

But in the course of these discussions, certain topics have been brought forward on which it appears that some further explanation and information are

requisite to enable the general reader to arrive at an unbiassed and correct opinion. Could I possibly have foreseen the precise points that would most forcibly have attracted the public attention, and that would have been urged as the most powerful arguments on either side, I should have considered it my duty to have enlarged upon them more fully in my Preface to that work, for the purpose of enabling the reader to become better acquainted with all the facts and circumstances of the case; still leaving him to draw his own conclusions therefrom. I had indeed imagined that the abstract of the principal events recorded in Flamsteed's autobiography, which I drew up in the Preface above mentioned, was sufficient to prevent any misconception or misconstruction of the material facts, or any confusion of the several diversified matters that occurred in the long period of more than fifty years. In this, however, I appear to have formed a mistaken opinion; as I find that some persons, whose talents I highly respect, and from whose judgment I seldom consider it safe to differ, have come to a different conclusion on some of these points. But perhaps the confusion was one of the unavoidable evils arising from the mode adopted of publishing such a mass of documents in their *original*, rather than in a compressed and reduced state. It therefore becomes necessary to enter a little more at large upon some of the points on which such misconception or confusion seems to have taken place.

It cannot be disguised that the quarrel between Newton and Flamsteed relative to the printing of the Greenwich observations, has arrested a much greater portion of the public attention than any other incident recorded in Flamsteed's life; and indeed greater than its relative importance seems to merit: and Newton's admirers have, as might have been expected, shown a natural desire to remove from him every appearance of misconduct arising out of that dispute. In doing this however it seems to me that, in some instances at least, the tendency of their remarks has been to exculpate Newton not so much by a direct refutation of the charges adduced by Flamsteed, as, by attempting to lower the moral and scientific character of Flamsteed himself in public opinion, thus to show that Newton was most probably right in the line of conduct which he pur-

sued. This course however can scarcely be tolerated at the present day: neither is it just to the character of Flamsteed, (nor indeed to that of Newton, which stands too high in the general opinion of mankind to need such support) that the decision should rest on such grounds. The mere fact of mental superiority, which no one is disposed to deny, ought not to weigh one feather in the scale of justice: and the case must be decided solely on its own merits. As some of the arguments which I have heard adduced, appear to me to be founded on a misconception of facts, and on a confusion of circumstances and dates, I trust I may be excused from attempting to remove any cloud of this kind from Flamsteed's character and conduct: and shall therefore at once proceed to discuss the several points that have been urged in support of those arguments, and which, as far as I can classify them, may be arranged under the following general heads.

1°. That Flamsteed did not understand and therefore could not justly appreciate Newton's theory of Gravitation, and more especially his new theory of the moon: consequently that he was not fully aware of the great assistance that could be afforded, by his observations, in the formation and verification of that theory.

2°. That Flamsteed showed an unwillingness, and even an objection, to furnish Newton with the requisite lunar observations to enable him to perfect that theory; a reluctance which consequently endangered the completion of that important work.

3°. That similar feelings induced Flamsteed to raise frivolous and vexatious objections to the printing of his astronomical observations, when it was afterwards undertaken by the Government: and that the delay was occasioned solely by his own misconduct.

4°. That in order to obtain the Catalogue of the fixed stars, for publication, which was sealed up by mutual consent, the Referees had a *right* to break the seal of the packet in which it was contained.

Now, these assertions have been so frequently and so strongly adduced (either wholly or in part) in palliation of Newton's conduct towards Flamsteed, in the disputes which arose between them in the latter period of their lives, respecting

the publication of the Greenwich observations, as already related in the preceding part of this history, that I fear I have not, in my Preface, been sufficiently circumstantial and clear in my attempt to connect the chain of varied incidents that befell Flamsteed during his long and chequered career. Indeed, such charges were so totally unexpected by me, and in my opinion so much at variance with the recorded facts, that I should have considered it as going out of the way, to have anticipated the necessity of any defence for Flamsteed's conduct. With respect to the first two charges, even supposing them to be true to the fullest extent (which however I by no means admit), they can have no connection whatever with the subject matter of these disputes: and the question of right or wrong must be decided on totally different grounds from any that are involved in those assumptions. For, Newton had left the subject of the lunar theory long before the quarrel which arose respecting the manner in which the Greenwich observations should be printed. The dispute on that subject is sufficiently painful, and its details in some points doubtful and unsatisfactory (since we have frequently only one side of the case to decide us in our inquiry), without our rendering it more distressing and intricate by mixing up with it matters that are wholly foreign to the subject. My object therefore is to clear the way for a more full and perfect understanding of the several subjects here alluded to, by endeavouring to remove certain misconceptions which I fear may have possessed the public mind, arising probably from a confusion of dates and circumstances, or from an inattention to the precise state of the science at the period under review. And in doing this, I trust I shall not be considered as attempting (as it certainly is far from my wish) to cast any improper reflection on the character and conduct of others: my sole object being to place those of Flamsteed in their true light.

As the *first* of the above-mentioned assumptions will lead me into the longest inquiry, I must entreat the patience and attention of the reader whilst I bring forward, for the sake of elucidating the subject matter of such inquiry, a few circumstances to show the state of the lunar theory immediately preceding the

publication of the *first* edition of the *Principia*, and the principal difficulties that were to be encountered in that research. It has indeed been justly remarked by Mr. Whewell, in his pamphlet entitled *Newton and Flamsteed*, when speaking of the discovery of the theory of Gravitation, that "we of the present day are accustomed to consider this immense step as effected at once, on the first edition of the *Principia* in 1687; but, we may easily convince ourselves that this was not so: even under the most favorable circumstances a vast theory like this could not make its way at once." The fact is that, with respect to the lunar theory, which more immediately concerns the subject of the present remarks, although its general principles were indicated by Newton in the first edition of the *Principia*, yet it was not till the publication of the second edition in 1713 that he more fully (but still incompletely) developed those principles relative to the lunar motions which have led to such important and valuable results in the hands of his *successors*: for, it lay dormant nearly half a century before it was rendered essentially serviceable to the purposes of astronomy and navigation. If, therefore, even after the appearance of this latter edition, we assent (as I think we fairly may) to Mr. Whewell's opinion that "no man of Newton's standing thoroughly accepted his views," it may very reasonably be inferred that, *prior* to that time (which is the period now under review), his principles, more especially as they relate to the moon, were not fully understood, and consequently not generally received. And it is obvious that, in this early stage of the inquiry, the most effectual mode of stamping their truth and their value in the public mind, would be to show the accordance of the theory with observations.

The motions of the moon are so apparently irregular, and subject to so many seeming inequalities, that (setting aside those minute ones which have since yielded to modern instruments and a more refined analysis) it is no wonder that so long a period should have elapsed before the more prominent irregularities (which were the first to be discovered) could be accurately accounted for, on any of the various systems that had, from time to time, possessed the public mind. Prior to the time of Ptolemy only one inequality of the moon's motion in longi-