

The points of difference are very slight, and are mostly in the less refrangible part of the spectrum, where the reductions of the stellar spectra present the greatest difficulty.

The general agreement is such as to leave no reasonable doubt that titanium is the main factor in the production of the dark flutings which characterize the Antarian group of stars.

This explanation of the dark flutings suggests that the appearance of bright flutings in the Antarian spectrum arises chiefly from effects of contrast. It does not, of course, exclude the possibility of the presence of bright flutings, such as might be indicated by local brightenings which are not exactly in coincidence with the edges of dark flutings.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Galileo and Marius.

GENTLEMEN,—

In the January 1904 number of the *Observatory* there is a letter on the disputed question of the first observation of Jupiter's satellites—a question which has been raised afresh by Messrs. Oudemans and Bosscha in 'Archives Néerlandaises des Sciences exactes et naturelles, publiées par la Société Hollandaise des Sciences à Harlem.'

Although referred to directly by these gentlemen in their memoir, I do not think it necessary just now to reply to what concerns myself personally; and besides it would be premature in every way to say anything until *all* the writings of Galileo relating to the Medicean Stars have been published in their integrity. This will be done in the course of next year at latest, in the National Edition of Galileo's Works which I am editing, and in the course of which I have to resolve difficulties of such a kind as cannot be adequately understood by those who have not seen and studied the autograph MSS., and who form their opinions on the inaccurate and incomplete publication brought out by Albèri in 1842-56. However, I will now permit myself to remark that Messrs. Oudemans and Bosscha have been led to make certain statements which I think more mature consideration will not permit them to repeat integrally and in a form so absolute.

With these, I repeat, I do not wish to occupy myself at present. Here I desire only to rebut what I find asserted in the letter of Mr. Lynn above referred to, where an argument used by Messrs. Oudemans and Bosscha, but void of any real foundation, is made the basis of a rather sweeping assertion. In their memoir we read (p. 140):—"Galilée se plaignit auprès du Prince Frédéric Cesi et le pria de demander aux membres de l'Académie dei Lyncei

‘de quelle manière il fallut répondre à Simon Marius, usurpateur du système Jovial, s’il fallut le faire en écrivant à Kepler ou au Margrave de Brandenbourg.’ Après ample délibération les Lynceans répondirent qu’ils préféreraient qu’une lettre fût écrite à Kepler, comme étant un astronome de la même Allemagne et bien informé, l’autre manière offrant quelque difficulté. Il paraît que Galilée, se ravisant, a estimé que la première manière présentait également quelque difficulté ; à moins qu’il ne faille admettre que la réponse de Kepler n’ait été nullement satisfaisante. Il est certain qu’une réponse de Kepler n’a jamais été publiée, ce que Galilée n’eût pas manqué de faire si Kepler avait jugé que les torts étaient du côté de Marius.”

Leaving aside the inferences which are quite arbitrary and unsupported by any documentary evidence, it is clear that in this passage Messrs. Oudemans and Bosscha do not affirm, as they could not affirm, that Galileo did in fact refer the matter to Kepler; yet Mr. Lynn says positively he did do so. His words are (p. 63, note):—“Prof. Oudemans points out that reference was made by Galileo to Kepler, but that the answer seems to have been suppressed.” Where is the proof that a reference *was* made to Kepler? and, above all, where is the ground for the insinuation that his reply was suppressed (and inferentially) either by Galileo himself or by his editors? Happily, the one and the others, commencing with Vincenzo Viviani and ending with myself, have such a well-founded reputation for literary honesty that they can afford to laugh at such insinuations.

As a matter of fact, I think I can say with absolute certainty that Galileo did *not* refer to Kepler, because he already had good reasons for doubting Kepler’s loyalty (on the occasion of the Martin Horcky episode), and because their relations, at first so amicable, suddenly cooled down after the first astronomical discoveries of the great Italian.

In fact, not counting the purely official letter of Galileo to Kepler of August 28, 1627, the relations of the two men may be said to have ceased towards the end of 1611, Galileo’s last letter to Kepler which has come down to us being dated August 19, 1610, and Kepler’s last to Galileo May 28, 1611. These altered relations are further indicated by Kepler’s letters to third parties, in which his references to Galileo show quite other than a benevolent disposition towards his fortunate competitor for fame.

Yours faithfully,

ANTONIO FAVARO.

Padua, 1904, Mar. 6.

[Prof. Favaro’s reputation and self-denying labours are so well known to us all that I am sure, on further reflection, he will see that the question of his suppressing anything in this matter could not come in. Surely if anyone was anxious to prove a point to the satisfaction of another, and left it to that person to decide which of two others he should refer to as umpire, it is fair inference that he

did so refer it. That Galileo suggested Kepler as one of the proposed alternative referees shows that he did not then consider himself debarred from writing to him. Possibly Kepler preferred not to reply to an accusation that Marius had claimed the first discovery of the satellites of Jupiter, which he certainly did not, since he gave his dates by the Julian reckoning. But Galileo was evidently determined to allow no one the credit of an independent discovery of celestial bodies, which he considered an honour reserved for himself alone. Prof. Oudemans has clearly proved that Marius had for some time been in possession of a telescope quite equal to showing the satellites; and that being so, why should we doubt that one who is known in many ways to have been a careful and accurate observer did actually see them? We all feel, with Prof. Favaro, that the honour of departed astronomers is dear to us; but there is no respect of persons in this, and it applies as much to Marius as to Galileo. The accusation of forgery, be it remembered, was made against the former by the latter.—
W. T. L.]

An Eclipsing Star of Long Period.

[Translation.]

GENTLEMEN,—

Under this title Miss Clerke has written, in the March number of your magazine, about my work "Enquiries into the Light-changes in ϵ Aurigæ." As every reader of Miss Clerke's article who has not read my work also must form an altogether wrong idea of its aim, I find myself compelled to make a statement of the result of my enquiries.

Apart from the last ten lines, my work is exclusively a photometrical investigation of the change of brilliancy of ϵ Aurigæ without any theory or hypothesis as to the cause of the light-variability. From the reduction and discussion of more than 2000 observations of brightness of the star which were made between the years 1842-1903, and those all by good observers, I proved the law of the changes of light of ϵ Aurigæ, and the result at which I arrived I gave (some unessential numbers and words being omitted here) on page 114 of my work in the following sentences:—

"The light-change of ϵ Aurigæ has a period of 27·12 years. Usually the star has a magnitude of 3·35; at minimum it declines uniformly in 207 days to the extent of 0·73 magnitude. It remains at this minimum for 313 days and then increases in 207 days to its former brightness, at which it continues for 25·13 years, the whole duration of the light-change lasting 1·99 years. The middle of the last minimum took place on 1902 March 31.

"The numbers given are still somewhat uncertain. It is possible that the minimum of 1874-75 differed from those of 1847-48 and 1901-02, and further it is not disproved, although

T 2