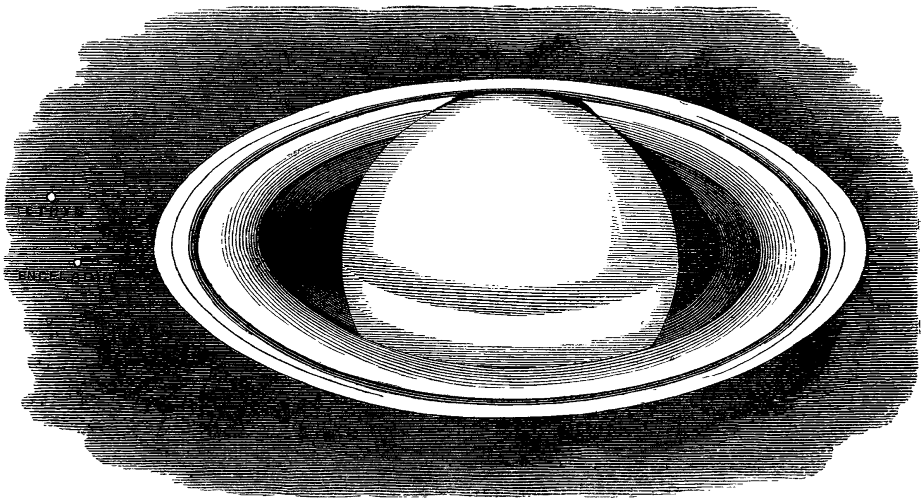
nearly its minimum distance for the present; five days' observations give:—

Epoch, 1856·015 302°·24 3″·806

The periastral time will not, I think, differ much from 1862·5.



"*Madras, January* 12, 1856."

Note on Solar Refraction. By Professor C. Piazzi Smyth, Royal Observatory, Edinburgh.

This term of "solar refraction" was given by Professor W. Thomson to characterise an effect which he had deduced theoretically from the dynamical theory of heat, and, if proved to exist, is pregnant with important consequences to every part of astronomy.

For it at once infers the necessity of the existence of a medium pervading space,—a medium, though rare, of similar constitution to our own atmosphere, and undergoing by necessity a condensation in the neighbourhood of the sun. Hence, he showed, that there cannot but arise a refraction of objects beyond the sun, when this body crosses their line of direction.

The theory could do but little beyond pointing to the fact of some amount of such solar refraction, while the exact amount could only be ascertained by astronomical measures. But with a comparatively small number of such observations, there seemed thus a promise of obtaining speedily a quantitative result,—a result, too, bearing immediately on the much-vexed question of a resisting medium, to approach which, at present, astronomers have scarcely any other method than that of cometary perturbations, wherein are mixed up so many other unknown quantities, and wherein the opportunities for observation are so rare, that generations may pass away before anything decisive is arrived at.

I determined, therefore, to inquire into the subject from Professor W. Thomson's point of view; and the best practical method at my command for testing this "solar refraction" seemed to be the observation of stars transiting the meridian in the neighbourhood of the sun; and for this purpose the large object-glass of the Edinburgh transit instrument was very favourable.

But although extraordinary precautions were taken in darkening the observing-room, and using various devices to improve the telescopic vision, it was found that the thick atmosphere of a town, and one so nearly on the sea-level, was almost always so brilliantly illuminated in the neighbourhood of the sun, that no stars could be observed, or even seen, under the desired conditions. On one occasion, however, in the past history of the Observatory, a unique state of the air enabled a star to be observed in what might be expected to be a possible refracting distance from the sun, while two others had also been observed the same day, at a distance so much greater, that they might safely, as a first approximation, be considered to be out of the range of disturbance.

Now, according to Professor W. Thomson's deduction from the dynamical theory of heat, the star in the neighbourhood of the sun, α *Orionis*, should have appeared closer to the other stars, β *Orionis* and α *Aurigæ*, on that day than at any other time of the year; and there were plenty of observations of the same stars during other months, when they all transited at night, completely out of reach of the solar influence.

What result, then, do the observations show? Why, after special computation, which has not sensibly altered the result from the original computation, made before the appearance of the dynamical theory of heat, α *Orionis* appears to have been visibly closer in R.A. to β *Orionis* by 0·06 of a second of time; and closer to α *Aurigæ* by 0·04 of a second of time.

The two results are, therefore, confirmatory of each other, and of the existence of the "solar refraction," and with that of a resisting medium filling space, and forming a material connexion still, and strengthening the idea of unity between the sun and all the planets.

But can we depend on this result? Or ought we to be satisfied with it? The mere arithmetic of it looks well; but those who have had much practice in striving after the highest attainable exactness, and know the innumerable sources of possible error in every astronomical operation, would very properly not be content when the effect sought for is so excessively minute, except with a large number of observations, and on many different stars; some, too, with the sun seen between them, and in the direction of N.P.D. as well as of R.A.

How, then, are such observations to be obtained? As far as my experience goes, there is no chance of obtaining them with any instruments at any observatory already established; and this by reason of the great depth of illuminated atmosphere through which such observatory must always look. But if our instruments could be temporarily transported to the summit of such a mountain as the Peak of Teneriffe, where 10,000 feet in depth of the grosser part of the atmosphere would be eliminated, there is every probability that a satisfactory result would be obtained in the course of a single summer.

Not only, too, would a knowledge of a most important element in the constitution of the solar system be procured, but we should have a proof of the cosmical character and universal bearing of the dynamical theory of heat,—a theory which has been elaborated by the mathematicians of our own time and our own country.

Observations hereinbefore referred to.

Date, 23^h, June 20, 1838.

Name of Star.	Distance from Sun in R.A.	Distance from Sun in N.P.D.	Transits (5 Wires) Corrected for Error of Coll. Level and Azimuth.	Tabular Apparent Places.
	h m	° ′	h m s	h m s
α Aurigæ	0 54	+22 22	5 3 58·91	5 4 43·70
β Orionis	0 52	+31 52	5 6 0·37	5 6 45·14
α Orionis	0 12	−16 6	5 45 39·29	5 46 24·12

From these numbers flow the following results:—

Names of Stars Employed.	Difference Observed.	Difference Computed.	Difference of Obs^d and Comp^d, or Solar Refraction
	m s	m s	s
α Orionis − β Orionis	39 38·92	39 38·98	+0·06
α Orionis − α Aurigæ	41 40·38	41 40·42	+0·04

In re-computing these observations for the special purpose now in view, I have not found any reason for altering the corrections for errors of collimation level and azimuth adopted at the time by Professor Henderson. Some sensible difference came out between our clock *errors*, but none between our *rates;* and this rate, which was alone of importance in the new inquiry, was shown to be under 0·01 of a second.

This resulting inappreciable effect of the clock-rate is, too, all the more satisfactory, inasmuch as I computed the corrections to the *apparent* places of the stars with new constants adapted to the instant of observation, and derived the *mean* places from all the Edinburgh measures made in the year in question.

As regards, then, the possible inaccuracy of the numerical corrections for error of instrument and clock, the upper limit must, I think, be considered to be less than the fraction representing the expected solar influence. But there is still the question of the sufficiency or power of accuracy of the original transits observed, especially seeing that they are each observed over five wires only.

To enable astronomers to form their own opinion on this point, I submit herewith the differences of each wire observed, from the time computed for it from the mean of the whole number of wires, the measured value of the intervals and the declination of the star.

Error of Observation of each Wire on the Mean of the Five.

Name of Star.	1st Wire.	2d Wire.	3d Wire.	4th Wire.	5th Wire.
	s.	s.	s.	s.	s.
α Aurigæ	+0·102	+0·058	−0·003	−0·109	−0·049
β Orionis	+0·032	+0·053	−0·036	+0·013	−0·062
α Orionis	+0·058	+0·046	+0·024	+0·041	−0·168

There may be some difference of opinion as to what the probable error of the mean for each star may be, but there can be little doubt of its being under the now declared quantity of "solar refraction;" and there can be no doubt at all as to the merit of the observer, Mr. Alexander Wallace, the assistant astronomer of the Observatory, the characteristic excellence of whose transits for many years past has enabled the three observations now under discussion to assume an importance which has seldom fallen to the lot of any other three transits.

Royal Observatory, Edinburgh, Jan. 9, 1856.

Note on the Orbit of α Centauri and on the Rings of Saturn. By Captain W. S. Jacob.

(*Extract of a Letter to the Editor.*)

"I have to communicate a matter of great interest regarding *α Centauri.* Finding from my observations communicated by last mail that the pair must have come to about their minimum of distance, I thought something like a good approximation to the orbit might be procured, especially as the observations of Richaud in 1690, and Feuillée in 1709, seem to bring both the period and perihelion passage within very narrow limits:* viz. the former between 77^{yrs} and $79^{yrs}{\cdot}5$, and the latter between 1862·4 and 1844·2. But on laying down an ellipse which would pass through the positions of 1834, 1848, and 1856, and computing intermediate points, to my dismay I found *enormous* errors, and the largest of all at those epochs which had been best observed, and where the observations were most accordant *inter se*, viz. about 1852–3. I then set to work to project the observed distances as well as angles into a curve, with the time for a co-ordinate, and on attempting to bring these into agreement, found them altogether incompatible, not only with each other, but with any kind of elliptic motion. Lastly, I took out the places independently from the two curves without

* These points will be more fully discussed in a paper about to be presented to the Society by E. B. Powell, Esq.