

On the Discordances between the Sun's observed and computed Right Ascensions, as determined at the Blackman-street Observatory, in the Years 1821 and 1822; with Experiments to show that they did not originate in instrumental Derangement. Also a Description of the seven-feet Transit with which the Observations were procured, and upon which the Experiments were made. By James South, Esq. F.R.S. Communicated June 1, 1826. Read June 8, 1826. [Phil. Trans. 1826, p. 423.]

Astronomers have generally admitted that a certain discordance between the observed transits of the sun and its computed right ascension takes place, and have been accustomed to refer it to instrumental error, arising from the action of the solar rays on the parts of the transit instrument used.

The validity of this explanation, and the true state of the facts, are what the author proposes to investigate in this communication.

He commences his paper with a full description of the transit instrument used in the observations, (the work of Mr. Troughton,) and of which, as accurate drawings have already been exhibited to the Society, no account need now be given, further than to mention the connexion of the cones which form the axis and the tube by tension bars, acting in their interior by means of differential screws, and thus drawing the cones in forcible contact with the spherical shell, which forms the centre of the instrument.

This instrument was erected in 1820, and its adjustments being made, it was subjected for two months to every species of trial to ascertain their permanence, which proving satisfactory, the regular series of observations were commenced.

These observations are stated at length in the tables attached to this paper and forming part of it. The right ascensions of the sun, deduced from them, are compared with those deduced from the computations in the Nautical Almanac reduced to the meridian of Blackman-street; and at each observation, the state of deflection from, or exposure to the sun's rays, of the instrument is stated. This was purposely varied as much as possible, and in many instances the sun was purposely allowed to shine for a full hour on the brace covered with black cloth, to produce as great a difference of temperature as possible in them, which was estimated by an attached thermometer, and in some cases amounted to 16° . Yet no appreciable error in the times of transit was found to arise from this severe trial, especially when the means of several days' observations were taken, whether the computed places of the sun by the Nautical Almanac were used, or the corresponding observed transits of the pole star were taken as zero point.

But the general result of the whole series of observations is, that the sun's observed right ascension is always in excess of his computed.

This deviation goes in many cases to the extent of a whole second of time. The mean excess is from $0''\cdot6$ to $0''\cdot7$; and it appears from the tables here given to be subject to periodical increase and diminu-

tion, its maximum occurring in June and July, and its minimum in December and January.

But that such a result might not rest merely on the observations with one instrument, the author has collected observations made on corresponding days at the observatories of Greenwich, Dublin, and Paris, all which, without exception, give results very nearly agreeing with those of his own observations; and he finally concludes, that as they cannot possibly arise from instrumental error, or error of observation, he hopes to be able ere long to prove, to the satisfaction of the Society, that their cause is imperfection in the solar tables.

On the Existence of a Limit to Vaporization. By M. Faraday, F.R.S.
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&c. &c. Communicated May 26, 1826. Read June 15, 1826.
[*Phil. Trans.* 1826, p. 484.]

The object of this paper is stated by its author to be, to show that a limit exists to the production of vapour of any tension by bodies placed in vacuo, or in elastic media, beneath which limit they are perfectly fixed. The train of argument by which this is attempted to be demonstrated may be summarily stated as follows.

Assuming it as proved by Dr. Wollaston, that a limit exists to the earth's atmosphere, where in consequence of its rarity its molecules are so distant from each other, that the repulsion of any one on the molecule below it is just equal to its gravity, it is clear that in this case the force of gravity may be regarded as setting a limit to further expansion; and if we could exhaust a receiver to the degree of tension obtaining on the surface of the atmosphere, any further subtraction of air would produce no further diminution of density, but would merely produce a vacuum in the upper part of the receiver.

But cold, as well as rarefaction, diminishes the elasticity of vapours or gases, and therefore if the temperature be greatly diminished, the limit above alluded to, where gravity counteracts the elastic force, will be attained with a less degree of rarefaction; and if the temperature were sufficiently low, it is evident that air of any given degree of density would lose its disposition to expand in a direction contrary to gravity.

In the case of air, however, the cold required to render it inelastic at any sensible density would of course be excessive. But if we consider the vapours of very fixed bodies (as silver for instance), whose tension even at a white heat is insensible, it is almost certain that the ordinary temperature of the atmosphere is, with respect to that capable of maintaining it at a sensible tension, such a degree of cold as would effectually bring it under the command of gravity. Supposing then silver to cool from fusion, that moment when these forces became equal, would be the one in which vapour could exist above the silver; and at every lower temperature the metal would be perfectly fixed. But the author regards it as probable that this equilibrium at ordinary temperatures may take place with bodies