

XLVII. Further remarks on the mode of taking lunar observations

Henry Meikle

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sheet iron; and besides the armourer's forge, vice, &c. contained a large quantity of other iron. The effect of this, together with the anchors, windlass necks, and other iron, was very remarkable on a compass placed in different parts of the deck near the foremast. [See Observations 18, 19, and 20 of the prefixed Table.]

11. As a compass placed on the midship line of the deck is subject to no anomaly fore and aft, in certain ships, on a north or south course [Inference No. 10], and as a compass in either side of the ship, opposite to the focus of attraction, shows no anomaly on a west or east course [Inference No. 8], the intersection of the line joining the two situations in opposite sides of the ship with the midship line traced fore and aft, will probably point out a situation directly over the top of the focus of attraction, where no anomaly on any course whatever will appear.

The *Esk*, in which I made my magnetical observations, had, as above stated, an armourer's forge near the larboard bow, which with the varying position of large quantities of iron work, composing our whale-fishing apparatus, contributed to vary this point where no anomaly is supposed to exist, and prevented me from ascertaining satisfactorily, at any time, its precise situation. I made indeed but very few observations with this view, and these I find neither establish nor refute the inference.

12. The anomaly of variation is probably the greatest in men of war, and in ships which contain large quantities of iron; but it exists in a very considerable degree also in merchantmen, where iron forms no part of the cargo, especially in high latitudes, where the dip of the needle is great.

Whitby, 3d Nov. 1818.

WILLIAM SCORESBY Jun.

XLVII. *Further Remarks on the Mode of taking Lunar Observations.* By Mr. HENRY MEIKLE.

To Mr. Tilloch.

SIR,—IN my paper on the lunar observations, a property of the ellipsis was referred to, which I suspect is hardly to be met with in an elementary treatise, if indeed in any other work, and shall therefore now give a demonstration of it in a more general proposition.

Theorem—The normal drawn from the transverse axis of an ellipsis is less than the semi-axis minor, when they do not coincide; but the normal produced to meet the conjugate axis is greater than the semi-axis major not coinciding with it; and the normal and normal so produced are in the duplicate ratio of the axes.

Let

Let AHB be an ellipsis, and GH the normal or perpendicular to the tangent KP which touches the curve in H. From F and L, the foci, draw FP, LK perpendicular to KP; join FH, LH.

Now it is a known property of the ellipsis, that the angle FHP = LHK, or that the triangles PFH, KLH are similar. Draw GQ, LR parallel to KP, and the triangles GLR, FGQ will also be similar.

Whence $PF - HG : HG - KL :: PH : HK$
 $:: FP : KL$, and

$HG = \frac{2FP \times KL}{FP + KL} = \frac{2CD^2}{FP + KL}$. But when H and E do not coincide, $KL + FP$ is greater than $2CD$, because CD^2 is known to be equal $KL \times FP$. Consequently, HG is less than CD the semi-axis minor.

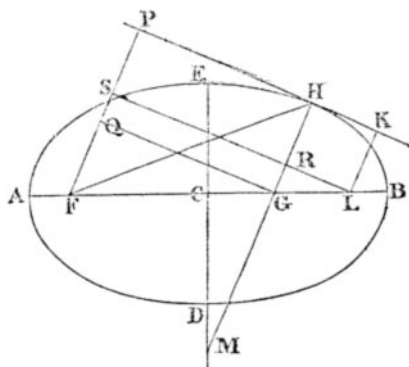
Again: Since FP , GH and LK are parallel, and the triangles FHP , LHK similar, we have $FP + KL : FP - KL :: KP : HP - HK :: FL : 2CG$. Produce LR to meet FP in S , and HG to meet ED in M . Then the triangles LFS , MGC will also be similar; for since MG is parallel to FS , the angle $LFS = MGC$, and the angles FSL , GCM are likewise equal, being right angles. Hence $FP - KL : FL :: CG : GM$. Consequently $FP + KL : FL ::$

$\frac{1}{2} FL : GM = \frac{\frac{1}{2} FL^2}{FP + KL} = \frac{2CL^2}{FP + KL}$; and $MH = MG + GH = \frac{2CD^2 + 2CL^2}{FP + KL} = \frac{2BC^2}{FP + KL}$. But when H and B do not coincide,

$FP + LK$ is less than $FH + HL = AB = 2BC$, because the side of a right-angled triangle is less than the hypotenuse. MH is therefore greater than BC . It is hence evident that $GH : HM :: CD^2 : BC^2 :: DE^2 : AB^2$, which is a very elegant property of the ellipsis.

When H comes to E , $HG = CD$, and $HM = \frac{BC^2}{CD}$ the semiparameter of the axis minor, which is also the radius of curvature at E ; and when H and B coincide, $MH = BC$, and $GH = \frac{CD^2}{BC}$ the semiparameter of the axis major, which is likewise the radius of curvature at B .

It might likewise be shown conversely, that if a variable straight



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straight line MH be divided in G, so that HM may be to HG in a constant ratio, and revolve so as to keep M and G respectively in DE and AB, two fixed straight lines crossing at right angles, and also that MH be always perpendicular to the path H describes: then shall the *locus* of H be an ellipsis, whose axes are in the ratio of \sqrt{HG} to \sqrt{MH} .

The principle of the elliptical compasses is much simpler than this; and differs from it, in that the bar to which the tracing point is attached is never perpendicular to the curve, but at the vertices of the axes; and also that the distances of the sliding dovetails from the tracer are equal the semiaxes.

A correspondent to the *Annals of Philosophy* lately objected to periodical mathematical works, for not admitting any thing unaccompanied by demonstration; and in effect proposed that demonstration be done away with in future. This, no doubt, he did for the same reason that smugglers wish to be disencumbered of "those oppressive laws," hoping thereby to get off his contraband doctrines with more facility. I cannot, however, second his motion, on the ground that no new doctrine on which any stress is laid can ever be safely let loose into the world without some evidence of its truth. It is this which makes the grand distinction between true science and unmeaning jargon. Things, it is true, which have often been demonstrated, may be quoted or given without demonstration. But many erroneous propositions are copied from book to book, without the least suspicion of their accuracy. A remarkable instance of this is the "Construction of the dialling lines," which, though admitted into our first-rate works of science, is nevertheless grossly erroneous. It does not appear that an absurdity becomes more correct by frequent copying, but it certainly becomes more dangerous.

Periodical mathematical works, however, are liable to a more serious charge than the one above mentioned; in that they are mostly made up of mere puzzling questions, totally useless and unconnected with science. Persons who torment themselves with such nonsense, might as well be assisting Sisyphus in rolling his stone, for any good they are doing either themselves or others, except it be to keep them out of mischief. But even this laudable end might be attained in solving questions serving some more useful purpose. I am, sir, your most obedient servant,

Berners Street, Oct. 1, 1819.

HENRY MEIKLE.

P.S.—In concluding the last paper on friction, I made a remark on a position which I then thought inconsistent with the case under consideration. The truth however is, that it is not simply so, but impossible that bodies of different weights with equal bases can make impressions proportional to their weights in

in equal times; because the lighter body will always be found to perform its task in the shorter time, if the resistance increase as any single power of the depth. This, if necessary, shall be demonstrated afterward. The statement referred to, therefore, does not imply any possible law of resistance. H. M.

XLVIII. *Illustrations and Corrections of two Papers on the Nature and Laws of Friction, with a Refutation of the Objections of Mr. MEIKLE.* By Mr. THOMAS TREDGOLD.

To Mr. Tilloch.

SIR, — IN your last Number Mr. Meikle has made some objections to a theory of Friction, which you did me the favour to publish in your Numbers for January and July. I hope in this letter to show that those objections are groundless; but though Mr. Meikle states that he has examined the steps of my inquiry, the real errors he has failed in detecting; and I am sorry to say that in two cases I have overlooked circumstances that ought to have been considered: therefore I take this opportunity of correcting them.

In your Number for January, page 5, it is stated that the indentation is as the extensibility; but it ought to have been directly as the extensibility, and inversely as the cohesive force:

That is, the general Prop. (2) ought to have been $I : \frac{P \times E}{C \times L \times B}$.

And then, Prop. (4) becomes $F : P \times E$; that is, the friction is directly as the pressure and extensibility and independent of the cohesive force. This correction is important, in as far as relates to the friction of different bodies, but does not affect the results when the materials continue the same. I am sorry that the effect of the cohesive force in resisting indentation was overlooked: the only reparation I can now make is to candidly avow it.

I must now proceed to examine Mr. Meikle's objections. In the first place, he states that my law of friction in uniform motions is in opposition to several experiments. Now it certainly must have been in your correspondent's power, and therefore it was a duty he had imposed upon himself, to refer to the experiments he alludes to; and to show that they were of an unobjectionable nature. As he has not done so, I can only say that such experiments are unknown to me.

It has been supposed that the friction is the same in uniform motions as it is in accelerated ones; but, independent of mathematical reasoning, the incorrectness of this supposition must be apparent when the distinct laws of these motions are considered. In the description of experiments, and in the usual statements of