As for marine boilers, the day will come when fresh water alone will be employed, and then the directions for improvement will be to follow, as far as possible, in the track of the boiler of the locomotive.

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## MECHANICS, PHYSICS, AND CHEMISTRY.

For the Journal of the Franklin Institute.

### *Work and Vis-viva.* By DE VOLSON WOOD, Prof. C. E., University of Michigan.

The object of this article is to make some comments upon one published by Mr. Nystrom, upon the same subject, in the last March number of the *Journal.* Had he not grossly misrepresented me in at at least two points, in his criticism upon a former article of mine, it would hardly be necessary for me to reply to him, for I observe that his reasoning upon all the new matter of the article, is remarkably correct, and his ideas more definite, agreeing essentially with mine, and hence, as he observes, with the standard authors of the day.

He opens upon me with a masked battery in the form of an explanation, and then pours in a cross-fire in the form of cant phrases, until the casual reader might suppose that my arguments were annihilated; but the sequel will show that he has merely exposed his position at several vulnerable points, no one of which can stand a fierce assault.

He says, I have misconceived the meaning of the letter V, and says it means *mean* velocity. I willingly accept the explanation, but am I to blame for the misconception? Did he so limit it in the preceding article? Did he not use it at one time as a constant, and at another as a variable, without any mention of the double usage? And if so, does it tend to avoid "the great confusion which now involves the subject?"

In the next paragraph he makes a correction, which I gladly accept. I should have said in the case referred to, that the work $= \frac{1}{2}$ F V T.

In the following paragraph he thinks that my expression "F T = S," should be F V = S. Mine is evidently a misprint, and I presume his is also; for it should be V T = S.

Having cleared up these minor points, we proceed to the more important criticisms.

I have said, and he quotes the same, on page 181, that the equation F V T = M $V^2$ is a true equation without expressing the value of work. Then the writer adds: "Is it possible that the formula can be a true *equation of work,* without expressing the value of work?" In this he plainly intimates that I have called it "a true equation of work." Have I said so? Has my language *intimated* it? Is not this putting words into my mouth, and then taking advantage of them? Is it making a fair representation of my meaning? He then says: "I (Mr. Nystrom) contend that it expresses the true relation between work and *vis-viva,*" and afterwards proceeds to prove it. Let us see how he proves it. He commences the proof in the latter part of page 183.

He takes the case of a body free to move and acted upon by a constant force F, during a time T, at the end of which it has acquired a velocity V. He then truly finds F V T = *twice* the work done upon the body. See equation 7, p. 184. If, now, M be the mass of the body, moving with the velocity V, $M V^2$ equals twice the work. See equation 8, p. 184. It also equals the *vis-viva*, as he says a few lines below. Hence, in the equation $F V T = M V^2$ one member *is the vis-viva*, and the other *is not the work;* does it then express the true relation between work and *vis-viva?* and if not "whose reasoning must be very elastic?"

In regard to this equation the writer says, p. 184, that I "proved it to be '*an absurdity under every hypothesis.*'" Now, by referring to my article, p. 27, present volume, or even to his extract of it as given on p. 181, it will be seen that he has omitted the phrase, "except the first," which should immediately follow the above quotation; and hence would read, "*proved to be an absurdity under every hypothesis except the first.*" What was this *first hypothesis?* It was "that the body be free to move, and acted upon by a constant force." An examination of Mr. Nystrom's article, pp. 183 and 184, shows that this is *the very hypothesis* which *he* has used in establishing the equation under consideration. I leave it for the careful, candid reader to judge "whose reasoning must be very elastic." This may be a sharp way of showing up a writer, but is it the way "to advance cautiously?" Does it aid in removing "confusion?" Does it savor of that "due deliveration" which is essential to true "reform?"

It should be observed, still further, in regard to this equation, of which so much has already been said, that it is a true equation only when the body is free to move, and under a constant force. If the force varies as the distance, square of the distance, or inversely as the distance from some point, then it will not be true. So Mr. Nystrom gives a differential expression for work, on p. 184, which is

$$dW = F\,v\,dt,$$

the integral of which gives the work. Letting G be the acceleratrix, and he makes $v = G\,t$. Is this true, except when G is constant? and if not, is the expression

$$W = \int F\,G\,t\,dt,$$

on p. 186, true when F and, consequently, G is variable?

Will we find by solving it that the figure of work, when the force varies as the square of the velocity, is a regular pyramid, with right lines for its edges?

Is not momentum the *mass* multiplied by the velocity? and if so, should not the expression on p. 186 be

$$\text{Momentum, } \frac{M}{g}\,V = \int V\,dt,$$

in which M is the *weight* of the mass?

On p. 183 he says: "It is perfectly absurd to say with myself and others '*that work is independent of time.*'" Why, then, does Mr. Nystrom say on p. 327, vol. xlviii, that "the foot-pounds of work means so many pounds raised through a space S, *independent of the time.*" But now he tries to show the absurdity by begging the question, and reasoning from my stand-point. He says, according to my reasoning "force is independent of work," &c.; but I suppose he means that *work is independent of force*, and adds, "a force of one pound can accomplish as much work as a force of 100 pounds; but at the expense of time and velocity only." This is true, and is no more than I contended for, which was, that the work is always the same when the expression $F S$ gives the same quantity, whatever be the relation of $F$ and $S$, or $V$ and $T$. Will the writer contend that because he contracts to dig a canal in six months, and completes it in three months, that, therefore, he has done only one-half the work? Have I *intimated* that "space cannot be accomplished without time and velocity?" I gave it as my *impression* that $F S$ is the *primive* formula for work; but have I objected to the expression $F V T$, when $V$ is constant, or $F v\, dt$ when $F$ is variable? In my article in the February number of the *Journal* for 1864, on p. 89 may be found the very equation which he has so freely used.

Is it necessary for Mr. Nystrom to resort to the elementary, although it may be a profitable exercise of "drawing chalk-lines upon a black-board," in order to convince him of what no one calls in question?

With regard to the term "mechanical power," or simple "power," which is represented by $F v$, I have raised no issue.

Students are familiar with the term, and all I wish to contend for is, that it is of the same *quality* as work, and that it is *the work* done in a unit of time under a constant velocity. In my last I expressed the desire that the term power might be restricted to this expression.

Let us see what results from Mr. Nystrom's stand-point. He says, p. 183, that "power is the differential of work." Do we not rightly infer from this that power is a part of work, an infinitesimal portion of it, and hence of the same kind or quality as work? Again, he says "$F V T$ is the expression for work." Now, if $T =$ one second, minute, or hour, do we not have $F V =$ *the work* which is done in a unit of time? which is also the mechanical power. Then is not the mechanical power THE WORK done in a unit of time? Is not this "the difference between power and work?" Again, if $V = 1$ and $T = 1$, the expression becomes $F \times 1_{vel} \times 1_t$; which is THE WORK DONE in a unit of time and unit of space; may not the English horse-power be the unit of work, even if it be the unit of power? According to this analysis, is not the unit of power *a unit* of work?

If this view be correct, and it follows immediately from his equation, is it true that "the difference between foot-pounds of power and foot-pounds of work, is the same as the difference between square feet and cubic feet?" (See p. 188.)

On page 187, he says: "The substance of the so-called moment of inertia $MR^2$, is work when $n$ is constant." How does this compare with what he has said before? Do we have the essentials of velocity and time in it?"

Mr. Nystrom has not criticised the article referred to in the November number of this *Journal* for 1862, more severely than I would. I used the word force in connexion with momentum and *vis-viva*, as it is popularly used. Mechanics say "the force of momentum," and mechanical writers say "living force." I do not defend the use of the terms, but, having accepted them, I see no reason for changing my views of the essential character of the *things* represented by them, call them what you may. A careful study of the article in the February number of 1864, would have made one or two of his criticisms unnecessary. I will pass over many important points, and modestly inquire, *where is his true reform?* I confess I do not see it, but trust I shall when he "brings it bodily out to sight."

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For the Journal of the Franklin Institute.

## *On the Use of the Double Eye-piece in the Determination of the Personal Equation.* By S. W. Robinson, C.E.

What is designed to be understood by the double eye-piece, in contradistinction to the binocular eye-piece is, that the former is designed for the use of a single eye of two observers, while the latter is intended for the two eyes of a single observer.

It was after I had conceived the idea, and constructed a double eye-piece, and had found it a very successful means of getting the personal equation, that I learned a similar contrivance had been used before. But my experiments upon it have been so successful, and the advantages of its use are so great, that I have thought them to be of sufficient interest for publication.

The principal advantage in its use consists in greatly diminishing computation for the reduction of the observations. By the usual method, with a reticule of fifteen threads, the first five only can be taken by the first observer, when he must give place to the second, who quickly gets into some posture and catches the last five. The thread intervals must be obtained by multiplying each of the *ten* equatorial intervals by secant of the star. These must be applied to the *ten* observations for only *five* results. By using the double eye-piece *fifteen* results are obtained from the same star passage over the same reticule of fifteen threads; the middle tally lost before being the best of the three in this case, while the only computing required is the simple subtraction of each of the fifteen signals by one observer from those of the other, respectively. Consequently the work of weeks is reduced to that of days, and also the results are believed to be much better, as it has already been found that the probable error of a single result is diminished about one-third. This is probably due in a great measure to the fact that the observers can retain their postures throughout the observation, and thereby evade the rapid transposition unavoidable in the use of the single eye-piece. In quite a series of observations taken by using the single eye-piece, for a personal equation which was found to be nearly zero, the person second in order, without reference to which, who necessarily took his position at the instrument hurriedly,