pendicular paddle, that in order to attain equal speeds, the perpendicular paddle wheel had to make two more revolutions per minute, "*of course involving a greater consumption of fuel.*" Now with the same diameter from outside to outside of paddles, the diameter of the circle of the centre of reaction will be less with the perpendicular than with the radial paddle, the relative proportions of the breadths of the two paddles being about as $2\frac{1}{2}$ to $4\frac{1}{2}$. Now the leverage of the paddles on the same engine will be as the radii of their respective circles of the centre of reaction, consequently the load upon the engine with the perpendicular paddle wheel will be less, revolution for revolution, than with the radial paddle wheel, and if the load be less, there will be required less steam to move it; so it is easily perceived that the "*of course*" involves a *less* instead of an *equal* consumption of fuel, revolution for revolution. The diameter of the circle of the centre of reaction of the perpendicular paddle wheel being less than that of the radial paddle wheel, there will, "*of course,*" be required more revolutions of it to go a given distance, but there will also be a proportionally less power required to make a revolution, in the same manner that a small wheel must turn oftener than a large one in traversing the same road, but I believe "*M.*" is the first discoverer that it "*of course*" involves a greater expenditure of power, and I leave him to the credit of his discovery, reaffirming the truth of the theory, data, and calculations of my original article on the two kinds of wheels.

With regard to the trial quoted by "*M.*" as made with the two kinds of wheels at Maudslay & Field's works, it was made without any sagacity of method, and both the experiment in its conduct and the conclusions drawn from it, are erroneous, and could easily be so proven had I not already exceeded the proper limits for this reply.

New York, November 9, 1850.

NOTE BY THE EDITOR.—We insert the reply of Mr. Isherwood to our correspondent "*M.*," omitting, however, and altering so much as was necessary to destroy the disagreeable personalities which Mr. I. has been pleased to introduce into the discussion. Our alterations have in no wise altered the sense or the strength of Mr. I.'s arguments and affirmations, which must, of course, go for what they are worth. He was entitled to reply, but our Journal cannot be made a vehicle for personal abuse.

On the Heat of the Vaporization of Water. By J. P. JOULE.*

The object was to point out the complex nature of the heat hitherto taken from the latent heat of steam. In the exact experiments of Regnault 965° was found to be the quantity of heat evolved in the condensation of steam saturated at 212°; of this quantity, 75° is the heat due to the *vis viva* communicated by the pressure of the steam, leaving 890°

* From the London Civil Engineer and Architect's Journal, for November, 1849.

as the true heat of vaporization of water. In a perfect steam engine supplied with water at 212° , and worked at atmospheric pressure without expansion, 965° will be the heat communicated from the fire to the boiler, 75° will be the heat utilized by conversion into force, and the remainder, 890° , will be the heat given out in the condenser.—*Proc. Brit. Assoc.*, Sept. 1849.

On Atlantic Waves, their Magnitude, Velocity, and Phenomena. By DR.

SCORESBY.*

During two passages across the Atlantic in 1847–8, I had opportunities for investigating certain elements respecting deep sea waves more favorable than had ever before occurred within my experience in navigation. These observations, it should be noted in the outset, and the results deduced from them, were entirely uninfluenced by, and separate from theory. They form but a contribution to this interesting branch of natural phenomena; but I offer them the more readily from the circumstance of their entire independency and speciality. It was in our return voyage from America that the highest seas occurred, when the circumstances adapted for interesting observations were singularly favorable; for, whilst the magnitude and the peculiar construction of the upper works of the ship—the Hibernia—afforded various platforms of determinate elevation above the line of flotation for observations on the height of the waves, the direction of the ship's course, with respect to that of waves, was generally so nearly similar as to yield the most advantageous agreement or accordance for observations on their width and velocity. These observations I shall extract, in their order, from my Journal kept during the homeward passage. My first observation worth recording, is under the date of March 5, 1848, when the ship was in latitude about 51° , and longitude (at noon) $38^{\circ} 50'$ W.—the wind then being about W.S.W., and the ship's course, true, N. 52° E. At sunset of the 4th, the wind blew a *hard gale*, which, with heavy squalls, had continued during the night; so that all sail was taken in but storm-staysail forward. The barometer stood at 29.50 at 8 P.M., but fell so rapidly as to be at 28.30 by 10 the next morning. In the afternoon of this day, I stood some time on the saloon deck or cuddy roof,—a height, with the addition of that of the eye, of 23 feet 3 inches above the line of flotation of the ship—watching the sublime spectacle presented by the turbulent waters. I am not aware that I ever saw the sea more terribly magnificent. I was anxious to ascertain the height of these mighty waves; but found almost every wave rising so much above the level of the eye, as indicated by the intercepting of the horizon of the sea in the direction in which they approached us, as to yield only the *minimum* elevation, and to show that the great majority of these rolling masses of water possessed a height of considerably *more* than 24 feet, (including depression as well as altitude,) or, reckoning from the mean level of the sea, of *more* than 12 feet. Exposed as the situation was, I then ventured to the larboard paddle box, which was about 7 feet

* From the London Athenaeum, August, 1850.