XI. A defence of the new theory of the tides, in reply to Mr. Henry Russell's Observations

Captain Forman

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comes more predominant. Be this as it will, the apparatus may some day serve to ascertain the fact, since the spirit condensed by its means is strongly impregnated with that principle, and, if resinous, may be easily separated by simply mixing it with pure water; when the spirit, by having a greater affinity for the water, will disengage itself from, and allow the resin to precipitate.

To conclude: It may be observed that the most approved wine is not always produced from the best must, but frequently derives its superiority from fermentation and good management. In like manner will beer and other fermented liquors acquire their greatest perfection from the same source, which, for that reason, ought to claim and engross the attention of every brewer, or other person engaged in the manufacture of fermented liquors.

The invention of Madame Gervais above described, is further stated to have engaged the attention of several of the most eminent chemists and enlightened men in France, where a company has been formed, consisting of the Duc of Bellune, Count Chaptal, Viscount Chaptal, Count Dullau Dallemans, Count de Brissac, Mons. Froidfond de Bellisle, Mons. Gaston Deurbroucq, &c. &c., who have purchased Madame Gervais' patent in order to propagate the utility of her improved method of fermentation; which, moreover, is already successfully practised at the considerable brewery of M. Chappellet in Paris.

A patent for the use of the apparatus in this country has been obtained by Messrs. Deurbroucq and Nichols; and it has been tried with such complete success by Messrs. Gray and Dacre of Westham, in the county of Essex, that they have adopted the system, and are now, as far as the alteration to be made in their working-tuns will permit, fermenting on no other plan.

XI. *A Defence of the New Theory of the Tides, in reply to Mr. HENRY RUSSELL's Observations. By Captain FORMAN.*

MR. RUSSELL has at length produced the principle upon which his hypothesis is founded; and, as I am now able to meet him on equal terms, I have no doubt of speedily convincing your readers, that his theory (which by the way is as great a deviation from the old one, for which he expresses so much veneration, as mine is) is not sufficient to account for the phænomenon in question, and consequently cannot possibly be true.

"Let a stick," he says, "of about an inch in diameter, be loaded at each end with a steel ball, taking care that its specific

cific gravity be less than that of water : while swimming on its surface, by applying a magnet, the ball to which the influence of the magnet is directed will be sensibly elevated, although the magnet has not sufficient power to support the ball in the atmosphere. In this experiment the ball under the influence of the magnet is elevated by the superior gravity of the other ball, on the same principle" (*how is this proved?*) "that the flood tide is elevated by the superior gravity of the ebb, for take away the ball that is not influenced by the magnet, and the other immediately sinks; take away the ebb, and the flow immediately subsides."

Now I have always been taught to believe, that, in proving the possibility of one fact by the acknowledged existence of another, it was necessary for the two cases to be precisely analogous; but what analogy is there between two steel balls connected with each other by a stick, and the particles of water, all of which are perfectly independent of each other? Mr. Russell has very justly observed, that, if we take away the ball that is not influenced by the magnet, the other immediately sinks; and the cause of this is too obvious to require an explanation. So long as the two balls nearly balance each other, the stick is kept in a horizontal position, and they are both supported, not by the power of the magnet, but by the buoyancy of the wood; but the moment either of the balls is taken away, as the power of the magnet's attraction is not equal to the gravity of the one that remains, it cannot prevent it from sinking, and the stick, for want of being confined in a horizontal position, is no longer able to support it. Upon the same principle, as the particles of water are not connected with each other by a *wooden lever*, the downward pressure of the waters in one part of the world can no more lift up the waters in another, than the downward pressure of one of these steel balls could lift up the other after the stick that connected them was cut in two; and consequently, so long as the power of the moon's attraction, together with the centrifugal force of the waters, is not equal to the power of the earth's attraction, it is impossible to account for the rising of the waters, except by supposing that they are lifted up by the expansion of their own particles.

If the power of the moon's attraction, by taking off a portion of the gravity of the particles of water, be not sufficient to produce the quantum of expansion in the waters that my theory requires, the degree of the compressibility of water, even in the deepest parts of the ocean, must be too trifling to deserve any notice; and, supposing the waters to consist of hard particles, I cannot understand how a change in the specific gravity of a portion of these particles can produce any alteration in the re-

lative positions of the rest. Mr. Russell, in his veneration for old theories, will no doubt allow that the power of gravity decreases as the square of the distance increases; and, admitting this, he should inform us how the superior gravity of the waters in one part of the world can possibly produce a rising of the waters in another part at the distance of nearly 6000 miles, when, by the above rule, the gravity of the waters in this place is only the 144000th part less than it is in the other.

In the annexed figure, I have supposed the whole earth to be covered with water, with the moon perpendicular to B, where the waters, in consequence of her influence, have reached their utmost height. Now it follows, upon Mr. Russell's principle, that the waters which are rising between C and D must be lifted up by the downward pressure of the waters that are ebbing between B and C; and consequently that the influence of this downward pressure must extend to the distance of 6000 miles, which is about the fourth part of the whole circumference of the earth.

This, however, is not the only difficulty attending Mr. Russell's hypothesis; for if we suppose the waters (which is really the case in the Atlantic Ocean) to be confined within the boundaries l and h , there will, at this time, be a constant rising in every part, without any downward pressure to lift them up. Mr. Russell has endeavoured to explain away this objection, by observing that a sufficient quantity of water to produce the flow may be poured in from the Southern Ocean: but this argument completely overturns his own hypothesis; for, to say nothing of the absurdity of supposing that those waters can travel at the rate of three or four thousand miles an hour, which they must do to produce such an effect, the tides would then be raised, *not by the downward pressure of those waters which constitute the ebb*, but by an accumulation of water coming in from the Southern Ocean; and Mr. Russell has yet to explain in what way this accumulation is produced. Mr. Russell may perhaps be inclined to argue that the waters that constitute the ebb are seldom, if ever, at a greater distance from those that constitute the flood than two or three hundred miles; and I admit that they are not. But then the gravity of all these particles must be very nearly the same, and consequently the one cannot be lifted up by the superior gravity of the other when this

this superiority has no existence. Thus, for instance, the tide may be ebbing on the coast of Florida, in America, while it is rising on the coast of New England; but, as the waters which constitute the ebb are nearer the moon, their specific gravity must be proportionably less; and how then will Mr. Russell account for their being able to produce such an effect?

Mr. Russell denies that my theory is sufficient to produce a rising of the tides on that side of the earth which is furthest from the moon. To make use of his own words, he says, "He simply imagines a repelling power*, which I defy him to prove the existence of, and the work is done." This defiance comes with peculiar grace from Mr. Russell. I have written a book, in which I have proved at least the possibility of my hypothesis being true, by showing that similar effects can be produced by a couple of magnets. Mr. Russell, however, does not think proper to look into this book, but calls on me to write over again, for his satisfaction, what I have already written! Let him seek for the proof in either of the two pamphlets† which I have published on the subject, and refute it if he can; and in the mean time, I challenge him, and every other philosopher as well, to account for the rising of the tides, on *either* side of the earth, in any other way, without taking away the power of the earth's attraction altogether.

Mr. Russell has also accused me of mis-quoting his words in my reply to his first letter; but, as I am only charged with substituting a "no" for a "not," it is really a distinction without a difference. It may be more correct to say a magnet has *not* power than that it has *no* power to lift a scale beam, but the meaning of both is precisely the same; and, as there was no advantage to be derived from the change, it was not very likely that I could have been guilty of it intentionally.

With the exception of Mr. Russell's *new* hypothesis, all the theories of the tides that have attracted any attention, at least all those that acknowledge the moon's agency in producing the phænomenon, either suppose that the waters are pulled upwards by the moon's attraction, or that they are lifted up by a centrifugal force occasioned by the earth's revolving round the common centre of gravity of the earth and moon. In my "Remarks," I have proved that philosophers, in accounting for the phænomenon upon either of these principles, must have lost sight of the earth's attraction altogether; because, in either case, the waters are pulled *towards* the earth's centre by a force which is much greater than the power which endeavours to

* Which, by the way, never once entered my head.

† "New Theory of the Tides;" and "Remarks on the Opinions of Philosophers concerning the true Cause of their Rising."

carry them off. Facts, however, when they can be found, are the best arguments; and, in spite of Mr. Russell's objections, I shall make use of a handful of water in order to prove that both of these opinions are directly the reverse of what is really the fact. If we take up a handful of water at the time the tides are rising, it must be evident that, as this water is brought nearer the moon and carried further from the earth, the power of the moon's attraction must be increased and the power of the earth's diminished; and consequently, if the power of the moon's attraction had been sufficient to lift the waters up in the first place, it would certainly be sufficient to prevent them from falling when they were brought so much nearer to it. The same argument holds equally good with respect to the notion of the waters being raised by a centrifugal force; for as a drop of water upon the top of a high mountain travels through a much greater quantity of space in the same time, its centrifugal force must be considerably greater, while the power of the earth's attraction to restrain it will be proportionably less, than it is in any part of the ocean; and if this centrifugal force is not able to *prevent water from falling* when its power is greatest, how is it possible for it to *lift water up* in those places where its power is so much less? Wherever we have the opportunity of making the experiment, we always find that water falls downward when the support upon which it rests is taken away. It is evident then, that when the tides are rising, the waters must be pushed upwards by some power below; and, unless we can suppose that the bed of the ocean is lifted up by the moon's attraction, (when it has not power to lift a grain of sand upon the earth's surface,) there is no power that can produce this effect besides expansion in their own particles.

Philosophers, upon what principle I cannot imagine, have latterly conceived the notion, that, in the deep parts of the ocean, the waters are agitated and put in motion by a centrifugal force, occasioned by the earth's revolving round the common centre of gravity of the earth and moon; and that it is this agitation that produces the rising of the tides*. This opinion however is grounded upon no analogy, and is in direct opposition to evident facts. On every part of the earth's surface, in lakes and ponds, or wherever we may choose to place a drop of water, the centrifugal force, occasioned by the earth's motion, has not power to produce the least sensible rise; and as, the deeper the waters get, their centrifugal force will be so much diminished, while the power of the earth's attraction

* See Dr. Young's Natural Philosophy, and an article, on Cuthbert's New Theory of the Tides, in the 4th volume of the Quarterly Review.

must be proportionably increased, it follows of course that there can be no sensible rise (*except by expansion*) of any of the particles below. To argue, then, that the mass of water may be in agitation, while all the particles of which it is composed must necessarily be at rest, is about as rational as to-maintain that a number of cyphers, every one of which is nothing in itself, may amount to a considerable sum; and yet it is to buoy up this absurd hypothesis that my theory is rejected; while, at the same time, it is tacitly acknowledged to be true by all the philosophers, as is evident by their not venturing to oppose it in any way save that of *mere assertion*.

That water is compressible to a certain degree, has latterly been proved by facts which cannot be disputed; and, supposing the degree of compression to be sufficient, the necessary consequences of such a principle exactly correspond with the phenomena that really take place. Thus, for instance, if we suppose the sum of the expansion of all the particles of water at the depth of fifteen miles to amount to thirty feet, there would not be a rise of one inch* at the depth of only a hundred fathoms; and, as the waters will only have lost a portion of their gravity, they of course will not remain standing on a heap, but must necessarily roll from those parts, where the rise is greatest, towards the shores of the ocean, where, in consequence of the little depth of water, the rise is imperceptible. This principle, then, will at once explain, why the tides in many places run in an opposite direction to the moon's motion; why the flood tide, in every part of the world, always comes from the ocean; why there are no tides in lakes and shallow water: and, by a parity of reasoning, we have only to suppose that there is not a sufficient depth of ocean, near the coasts, to account for there being no tides in the West Indies and the Mediterranean Sea. The only rational objection, then, that can be brought against my theory is, that it requires a much greater depth of ocean, or a greater power of attraction in the moon, than philosophers are disposed to allow. But this objection, coming from the Newtonian philosophers, is in the highest degree absurd. If they would only give themselves the trouble to *think*

* In fact, it would hardly amount to a single line, because the particles of water at the depth of 15 miles would have a weight taken off them which would be equal to the diminution of the weight of all the particles above that depth; and consequently, taking the average, every particle of water in the ocean where it is 15 miles deep, would have a weight taken off it equal to the diminution of the gravity of $7\frac{1}{2}$ miles depth of water; while every particle, where the ocean was only 100 fathoms deep, would only be relieved of a weight that was equal to a diminution of the gravity of no more than 50 fathoms.

about

about the matter, they would immediately perceive that this objection affects their own theories with tenfold greater force. It is impossible to account for the rising of the tides, in the old way, without supposing the power of the moon's attraction to be even greater than the earth's; for, if the power of the earth's attraction was not sufficient to prevent the moon from lifting the particles of water off its surface, it would never afterwards be able to bring them back again, when its own power must necessarily have diminished by the increased distance, while that of the moon would be proportionably increased.

If, as the Newtonian philosophers suppose, the power of gravity decreased as the square of the distance increased, the power of the moon's attraction, at the earth's surface, could only be equal to the $\frac{1}{1440000}$ th part of the earth's; and in that case I readily admit that it would not be sufficient to produce the quantum of expansion that my theory requires. But this doctrine, after all, notwithstanding it is so generally entertained, is nothing more than a mere guess: it is grounded upon no analogy, is inconsistent with other opinions that are entertained by the same philosophers, and is evidently *not true*, because it is in direct opposition to indisputable facts. The Newtonian philosophers allow that the velocity of accelerated motion increases in arithmetical proportion with the distance, and not as the square of the distance increases; and, by the same rule, unless it can be shown that the diminution of the power of gravity is not constant and gradual, it must decrease inversely in the same proportion; that is, it must *increase*, from the point where it ceases, in arithmetical proportion with its distance from it; and consequently, before we can determine in what proportion the power of gravity in any body decreases, we must first of all ascertain to what extent it reaches. If the diminution of the power of gravity be constant and gradual, as has heretofore been supposed, it must accord with the above rule; and if it be not, it must be independent of every rule; so that, in either case, philosophers are evidently mistaken in supposing that the power of gravity "decreases as the square of the distance increases."

So much for the arguments upon which this famous law is grounded; let us now see how far it is borne out by facts. According to the most approved astronomical calculations, the sun is supposed to be about 96 millions of miles distant from the earth*, and his power of attraction is supposed to be

* I have said 96 millions in order to avoid a tedious calculation, but 95 millions will make the argument still stronger in my favour.

200,000 times as great; while the moon is supposed to be 240 thousand miles distant from the earth, and her power of attraction 40 times less. The earth's centre is allowed to be very nearly 4000 miles from the surface of the waters, and consequently is 60 times nearer to it than the moon is, and 24,000 times nearer than the sun. Now 60 multiplied by itself, and this sum multiplied by 40, will make the power of the moon's attraction, at the earth's surface, 144,000 times less than the earth's; while 24,000 multiplied by itself and divided by 200,000, will make the power of the sun's attraction equal to the 2880th part of the earth's, which is 50 times greater than the moon's. I suppose I need not go about to prove that the power of the moon's attraction, at the earth's surface, is really greater than the sun's; because it not only is acknowledged to be so by the Newtonian philosophers, but the effects of the moon's influence on the tides is always apparent, while the influence of the sun upon the tides can never be discovered, except (and then it is doubtful) at those times when it may be supposed to be acting in conjunction with the moon. It is evident, then, that this far-famed law is grounded upon no principle, and is in direct opposition to an indisputable fact; and, unless the authority of Sir Isaac Newton's great name, unsupported by any argument, be sufficient to bear down truth by falsehood, philosophers can no longer have any excuse for making this supposed law an objection to my theory.

In my "Remarks on the Opinions of Philosophers concerning the true Cause of the Rising of the Tides," I have shown that the power of the moon's attraction, at the earth's surface, is, in all probability, equal to the 100th part of the earth's attraction, and may possibly be equal to the 50th; and consequently, unless the above arguments can be refuted, (and that can only be by setting aside a positive fact,) the power of the moon's attraction must be quite sufficient to produce expansion in the waters, without supposing an extraordinary depth of ocean. This pamphlet, as well as my "New Theory of the Tides," I have taken care to put into the hands of most of the leading philosophers in London*: and though they may persist in treating it with contempt, their refusal to look into it will not do away the force of the arguments it contains. If they can prove that my theory is not well founded, there may be some excuse for their neglect: but if they cannot, it may

* In addition to most of the Reviews and Philosophical Journals, it has been sent to the Board of Longitude and the Astronomical Society, and put into the hands of Sir Humphry Davy, Mr. Pond, and several other leading philosophers in town.

possibly be true; and if it should ultimately be received, their rejection will certainly not enhance their reputations with posterity; for, in that case, it will be inferred that they either wanted the discernment to perceive the force of my arguments, or else that, from a feeling of jealousy, which is unworthy of philosophers, they wanted the candour to acknowledge it.

I am yours, &c.

WALTER FORMAN.

NOTE.—We feel obliged, at the same time that we insert the above communication from Captain Forman in defence of his New Theory, to avow that his arguments have not convinced us, as we have not been able to reconcile them with the laws which govern the motion of fluids.—And we cannot help thinking that the confidence which he feels in his theory causes him to undervalue those conclusions to which the freest discussion and most profound inquiries on the subject have almost uniformly led.

We would not, however, be understood to deny an expansion in the waters of the ocean occasioned by the attractive power of the moon; as any power diminishing their gravity must necessarily in some degree produce that effect.—EDIT.

XII. *Method of determining the Proportion of Carbonic Acid in Mineral Waters*.*

DR. AUG. VOGEL of Munich found by repeated experiments, that the method of determining the quantity of carbonic acid contained in a mineral water, recommended as being the best by M. Thenard in his *System of Chemistry*, vol. iv. p. 159, is uncertain in its results.

M. Thenard recommends to introduce the mineral water into a retort, from which a bent tube leads into a solution of muriate of lime and caustic ammonia. After the water has boiled two or three minutes the whole of the carbonic acid gas is extricated, passes into the solution of muriate of lime, and combines with the lime through the influence of the ammonia.

Dr. Vogel has found muriate of baryt, as well as muriate of lime mixed with ammonia to be equally unfit to detect small quantities of carbonic acid. When he dissolved from three to four cubic inches of carbonic acid gas in one ounce of caustic ammonia, and put this ammonia into a solution of one part of muriate of lime, or of muriate of barytes in nine parts of water, neither of those liquids was affected by it; but precipitation would begin only when the quantity of carbonic acid was increased; or, without increasing the quantity of gas, the carbonates were precipitated by ebullition, which also disengaged a little ammoniacal gas.

* From Schweigger's *N. Journal. für Chem. u. Phys.* Bd. 3. H. 2. p. 204.

When