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To cite this article: John Herapath Esq. (1822) XXXIV. Reply to Dr. Apjohn's "Additional Remarks," &c. in The Annals of Philosophy for September 1822, Philosophical Magazine Series 1, 60:293, 193-195, DOI: [10.1080/14786442208652818](https://doi.org/10.1080/14786442208652818)

To link to this article: <http://dx.doi.org/10.1080/14786442208652818>

Published online: 29 Jul 2009.

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XXXIV. *Reply to Dr. APJOHN's "Additional Remarks," &c. in The Annals of Philosophy for September 1822. By JOHN HERAPATH, Esq.*

To the Editors of the Philosophical Magazine and Journal.

London, Cranford, Sept. 5, 1822.

DEAR SIRs, — IN the Annals for June I gave a formula for determining the specific gravity of aqueous vapours in contact with their fluids. The method of my finding this theorem, and the principles I employed, appeared to me so obvious and simple, that I could scarcely believe the formula itself could be new: and certainly did not imagine that my reasoning required extended illustration. Dr. Apjohn having however, in the present month's Annals, called the accuracy of the theorem in question, I perceive I must have miscalculated the obviousness of the principles, and therefore beg to trouble you with a demonstration of them.

Let us conceive a quantity of vapour having a specific gravity S to be confined at its proper tension τ in some vessel over its fluid at the Fahrenheit temperature F . Let us now conceive this vapour and its fluid to be gradually and equally cooled to some other temperature F' , conceiving at the same time, what is unquestionably admissible though perhaps not practically possible, that the vessel confining the vapour also gradually changes its capacity, so as always to preserve to the vapour an elasticity just equal to the tension it ought to have corresponding with its temperature; and let the ultimate tension be τ' and specific gravity S' . Then because by the commonly received principles water neither evaporates nor absorbs any of its superincumbent vapours, whilst the elasticity of the vapour equals the tension of the temperature, it follows that the vapour at F' is, the same, and of the same weight as the vapour at F ; that is, the mere contact of the water has produced no effect on the vapour beyond simple communication of temperature. But equal changes of temperature being similarly and under similar circumstances communicated, must, as far as I can perceive, have the same effects from whatever bodies the communication comes. The change therefore which has been made in the volume, and consequently in the specific gravity of the vapour in contact with its fluid, by the diminution of temperature, is just equivalent to that which would have been effected by an equal diminution, if the vapour had not been connected with its fluid. But when vapours are not connected with their fluids, our philosophers tell us that their changes are, *cæteris paribus*, the same as those of the

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B b gases.

gases. This established, the truth of the expression $S' = S \cdot \frac{\tau'}{\tau} \cdot \frac{F + 448}{F' + 448}$, which I have given, easily follows from what Dr. Apjohn has himself admitted; for, it being allowed to be true in gases, it must, from what I have shown, be equally true in vapours over their fluids.

In other ways the proof of this theorem might be easily derived, but I hope what I have said will be sufficient to convince Dr. A. that the absurdity of making one constant vary as another constant, entered into no part of my communication.

Many philosophers appear to have confused themselves by not attending to the manner in which the elasticity of vapours in juxtaposition with their fluids increases; and hence they have drawn an unwarrantable line of distinction between vapours and gases. They imagine that the rapid increase of elasticity in vapours, is to be attributed to the increased action alone, which they conceive the heat gives to the vaporous particles; whereas, if they had only considered that the specific gravity increases with the elasticity, they would have seen that it is not merely to the augmented action due to the heat, but to this, and the increased number of the vaporous particles conjointly, that the great rise of elasticity is owing. By the latter part of his paper Dr. Apjohn seems to have yielded to the same erroneous ideas, and has by this means near the end of his communication involved himself in an error a little too obvious, but which I willingly omit to notice out of respect to the candour he has displayed. The same feeling induces me to pass over one or two other things in his paper, which appear to me open to comment.

Though I have not the shade of a doubt concerning the truth of the formula in question, yet I perfectly agree with Dr. Apjohn that experimental proof is desirable; and hence I should be happy to see such an object effected. But in making these experiments I conceive something of attention, beyond what is needful to secure experimental accuracy, is necessary. For instance, if we set out from Gay Lussac's determination of the specific gravity at 212° , which Dr. A. seems inclined to recommend, we should be satisfied whether this specific gravity was determined from the pressure due to the tension of vapour at 212° , or from the pressure due to ebullition at 212° ; for these pressures are not equal, as philosophers have commonly imagined, but sensibly different, as I have shown pages 441 and 442, *Annals* for December 1821, and page 27 *Annals* for January 1822; the former being about the 1-7th of an inch greater than the latter. Again, it should
be

be observed that the formula I have given would bring out results conformable to the indications of an air thermometer. This is of little importance in temperatures beneath 212°; but in very high ranges, in which at least one experiment ought to be made, it would be of vital consequence.

I am, dear sirs,

Your obedient humble servant,
J. HERAPATH.

XXXV. On simultaneous Thunder-Storms. By Dr. T. FORSTER.

To the Editors of the Philosophical Magazine and Journal.

Hartwell, Sept. 3, 1822.

GENTLEMEN,—I HAVE already stated my opinion that the formation of thunder-storms, and other electrical phænomena, frequently took place simultaneously in very distant parts of the atmosphere. I have lately had an opportunity of confirming this opinion, and of witnessing several of the most violent instances of storms which have ever been recorded in the memory of the inhabitants of those districts where they occurred; and of comparing them with similar phænomena in distant parts of Europe.

On Monday evening, July the 29th, while travelling from Gex to Nyon, on the Lake of Geneva, we were overtaken by one of the most violent storms I ever remember to have seen: it formed very rapidly, for at half-past four o'clock the atmosphere was clear. I observed however that on descending Mount Jura, the Alps (which form a beautiful back-ground to the view of the lake below) were intersected with clouds: the *cirrostratus* seemed to unite the summits of the range of mountains by forming long lines of cloud stretched across from one hill to another, while the more elevated ground of Mont Blanc was involved in *cumulostratus*. The air very suddenly became totally obscure, and before 5 o'clock a violent shower of rain and hail came down in torrents, accompanied by thunder and lightning: it cleared off very suddenly, but as quickly returned again; and for the space of nearly an hour there was scarcely an interval of two minutes between the most vivid flashes of lightning. I distinctly noticed the two sorts mentioned by M. Van Mons; namely, the *fulguration* followed quickly by a short and loud clap; and the *fulmination* followed at a longer interval by rolling thunder. The fulguration or forked lightning darted into the lake from several distant portions of cloud at once, and was at times of a bright