As matters appeared to my correspondent to go on in a satisfactory state, I did not again hear from him until after a lapse of nearly a year.

"June 16, 1853.—The ice and salt has not been applied since I last wrote to you. There has, however, been no relapsing. The nipple has sunk or receded considerably since you saw it, but the tumour has almost disappeared—that is, there is very little hardness or tenderness remaining. There is, however, a hollow or kind of indentation across the breast, near the nipple, but not the slightest indication of a tendency to suppuration. There is, also, a frequent feeling of shooting or twinging pains."

In replying to this letter, I expressed regret that so long an interval had been allowed to elapse without using congelation, as there appeared reason to fear that a remnant of the disease was still present; and, in the next communication from the husband of my patient, (the last which I have received,) dated Nov. 15th, he mentions that the ice and salt had again been once applied. The only interesting circumstances noticed in this letter, respecting the condition of the breast, are, that "there is no hardness or tumour;" although there was occasional annoyance from the sticking of the lint to the skin in consequence of the "exudation of a gummy substance close round, but, so far as I can see, not out of the nipple."

(To be continued.)

OBSERVATIONS ON ANIMAL HEAT.

BY HENRY H. VERNON, M.D.

CIRCUMSTANCES have hitherto prevented me from replying to Dr. Cartwright's article of the 12th of November last, and after such an interval of silence I should not have troubled you to insert another communication of mine on the above subject, were it not that I am anxious to clear myself of the imputation of rudeness, and my article of September 3rd of the erroneous construction put upon it by your correspondent.

It is too late now to express any regret at the levity of style into which I was betrayed in my first communication; I beg therefore to assure Dr. Cartwright that no offence was intended; that if unwittingly I have abused him, I am far readier to disabuse him, and that I am perfectly willing to convert the expression "intellectual obliquity," which appears to have obstructed his oesophagus, into "intellectual spirality," or any other phrase indicating aberration that he may approve of.

I leave it to Dr. Cartwright's reflective mind to determine how much of assumption is contained in the proposition that "the nutrition of the lungs is carried on by the bronchial arteries *alone*." Of any experiments tending to establish such a belief I am quite ignorant; of the possibility of instituting experiments on the subject I am very sceptical; and, as far as analogy will throw any light upon the question, we certainly are not justified in denying to venous blood *any participation* in the processes of nutrition, else all reptiles should be small, and human fetuses with malformed hearts should forthwith defunge as soon as born, whereas cases unfortunately occur from time to time where individuals live twenty or more years with only three heart cavities, and a general venuity of the blood far greater than obtains in the pulmonary arteries of the normal subject.

There is no necessity to suppose any *enormous* activity of decay and repair in the lungs in order *partly* to account for an increase of one or two per cent. in the temperature of the blood on the *left* side or that of the *right*, nor would the language used in my first paper bear the construction Dr. Cartwright has been pleased to put upon it; the expressions "evolution of heat *unknown* in any other organ," and "difficulty in *some measure removed*," are surely cautious and moderate enough, and indicative rather of a desire to suggest an explanation, than to claim an exaggerated importance for any particular class of phenomena. I cannot but think that your correspondent has grievously mistaken the drift of Liebig's remarks on pulmonary physiology. The passivity of which that chemist speaks is an *animal* not a *vegetative* passivity—*i. e.*, a passivity *quoad* the mode in which interchanges of CO_2 and O take place between the blood carried to the lungs and air inhaled, these changes taking place according to circumstances over which the lungs have no control, they neither excreting CO_2 nor absorbing O , but these gases passing through the membrane, dividing them precisely as they would through any other membrane of the same thickness and permeability, and with the same conditions of atmosphere on the one side, and blood on the other. Liebig by no means denies great *vegetative* activity to the lungs; for aught he says to the contrary, the molecular

constitution of a lung might be entirely changed in twenty-four hours. So much for hasty quotation!

I do not love to parade the "evidences of design," but I would ask if there is no purpose to be seen in the distribution of *arterial* blood to the muscles of the bronchi and bronchie, structures performing an *animal* function whether the vesicular substance of the lungs possess glandularity or not?

I am at a loss to conceive what Dr. Cartwright means when he says that *one-third* of my letter is *gratuitous abuse*. Abuse of whom?—abuse of what? Let me assure him that my communication was an attack upon his paper, not upon him personally. I would have written the same if the Man-in-the-Moon or the Great Sea Serpent had fathered the document. Let me advise Dr. Cartwright to be more jealous for the interests of science, and less jealous of his own dignity.

Regarding the wonderful fact about H , I would suggest, that if that gas extracts more CO_2 when the blood has stood for some time than when it is perfectly recent, we may find an explanation of the phenomenon in remembering that albumen, which we have found to be a rather tenacious retainer of CO_2 , loses its essential characteristics after prolonged exposure to air and moisture, and like all other nitrogenous bodies, gradually degenerates, until it becomes subject to the ordinary laws of chemical affinity. If any action of the ingredients upon one another liberates the CO_2 , does it come from an organic or an inorganic source? If from an inorganic, what acid displaces it? and if from an organic, I would reply, that the reduction of organic matter to the condition of CO_2 is an *ultimate*, and not an *initial*, step in decomposition.

Having hitherto disputed all Dr. Cartwright's positions, it is a great relief at last to yield to our inherent good nature, and corroborate one at least of his statements; it is with great pleasure that we lay our hand upon our heart, and aver that to the best of our belief Dr. Cartwright is *not* a "profound chemist," and as to his being "a *bit* of a general," we cordially agree with him. How infinitesimal a *bit* of generalship falls to his share, is sufficiently apparent from the specimens we have had of his *generalizations*. The connexion between "animal heat" and "Alison's History of Europe" is too subtle and recondite for our obtuse intellect, but we are quite ready to believe that Dr. Cartwright derives great comfort, benefit, and enlightenment from a combined contemplation of the two subjects, albeit that such a mode of procedure is rather *altogetherish* than otherwise, and reminds us forcibly of the man who constructed an article on "Chinese metaphysics," by reading up the two words "China" and "metaphysics" in an Encyclopædia; and we cannot help adverting, at the same time, to the "Laputan philosophers," who judged of a man's political bias by means unmentionable to ears polite.

Dr. Cartwright's theory I leave to abler hands than mine to demolish, only reminding him that, if carried out to its legitimate conclusions, it lands him in the paradox of "causation without a cause," a result sufficiently startling to make it worth his while to rethink his case over, and try if by any means he can detect the origin of his self-deception. When I say "causation without a cause," I refer to the phenomena of *muscular contractions*, the only rational theory of which is to be found interwoven with the current theory of animal heat—destroy *one*, and *both* fall; nor is it *possible* then to construct another: we shall have arrived at *motion* without a *moving power*.

Kingston-on-Thames, 1854.

P.S.—Why does Dr. Cartwright take no notice of Nasse's suggestion as to the cause of the difference in temperature of the blood on the right and left sides of the heart!

ENUCLEATION OF CORNS.

BY FREDERICK JAMES BROWN, M.D.

I AM desirous of making known to my professional brethren a very simple operation for the removal of corns. It has long been known that a corn can sometimes be picked out, and that freedom from the complaint ensues. The method that I advocate is the same, with the preliminary step of a vertical incision, to the full depth of the indurated cuticle, at a marginal line surrounding the corn.

I accidentally discovered the method in my own person, and I feel so thankful for the relief that I have obtained that I consider it to be my duty to state the particulars to the profession.

In 1851, a corn formed on the pulp of the index finger of my left hand, as a direct consequence of practising upon a violin. Its size was about three-twelfths of an inch long, and one-