JOSLIN (B. F.)

The physiology of respiration and chemistry of the blood, applied to Epidemic Cholera.

ART. IX. *The Physiology of Respiration and Chemistry of the Blood, applied to Epidemic Cholera.* By BENJAMIN F. JOSLIN, M. D. *Professor of Natural Philosophy, in Union College, N. Y.*

[Extracted from the Transactions of the Medical Society, &c.]

Physiology. 1. That important *relation* which subsists *between respiration and hæmatosis* was unknown to the ancients. The first rudiments of this discovery resulted in some measure from the investigations of Michael Servetus, to to whom we are indebted for a knowledge of the pulmonary circulation. It was ascertained by Lower, that the color of the blood was changed from purple to scarlet in the capillary vessels of the lungs. He inferred from his experiments, that this change was, in some way or other, effected by the air. This opinion, though partially revived by Cigna, was almost universally rejected, till Dr. Priestley confirmed it by a series of new and varied experiments, which, (as Dr. Bostock has justly observed,) "led him to the further discovery of a train of facts, which have served as the basis of all the information that has been since gained upon the subject." Confident that important applications would be made of these discoveries, and enthusiastically desirous of witnessing the future progress of his race, he intimates, that nothing but the confident expectation of hereafter resuming his enquiries, and the hope of "revisiting" this scene, could perfectly reconcile him to the "idea of ceasing to breathe, when" he "had but just begun to know what it" was "that he breathed."† He had discovered that the change in the color of the blood depended upon that part of the atmospheric air since called oxygen, a gas which he had also discovered. His experiments, together with Black's discovery of carbonic acid in the respired air, and Lavoisier's analysis of this gas, have led to the opinion, still generally, (and I believe justly,) en-

† Priestley on Air. Author's Preface, vol. 1, p. 40.

tertained, that the most important changes produced in the blood by respiration, are its oxygenation and decarbonization, and its consequent elevation of temperature. The greater portion of the oxygen which has been separated from the inspired air, enters into the constitution of the carbonic acid of the expired air, by combining with the carbon of the blood, somewhere in the course of the circulation. There is probably in the lungs a simultaneous endosmose, (inward impulse) of oxygen, and exosmose (outward impulse) of carbonic acid, or a disposition of both to penetration, by which the former is absorbed and the latter eliminated. The carbon which is to enter into the constitution of the latter probably exists as one of the elements of some proximate principle of the blood, or of the detritus of the system. It has been with much plausibility suggested, that the oxygen is conveyed to the carbon through the intermediate agency of the coloring matter—perhaps with the co-operation of the saline matter, which Dr. Stevens believes necessary to the change of color produced by atmospheric oxygen. It is to be regretted, that neither the proximate nature nor ultimate constitution of the sanguineous coloring matter has been minutely ascertained; but according to the analysis of M. F. Michaelis, the coloring matter of venous blood contains much less oxygen than that of arterial blood.* Delaroche, Dulong and Edwards have proved that there is a small and variable excess of oxygen, which is taken into the system during respiration;† whilst the last physiologist has settled the vexed question respecting the absorption of azote, and reconciled the discrepant results of Priestley and his successors, by proving that more is received into the blood or imparted to the air according to circumstances. This it is unnecessary here to dwell on; for the balance being sometimes in favor of the blood, at other times in favor of the air, it appears to me improbable that the absorption of this gas has any important in-

* Vid. Lond. Lancet, for March 31st, 1832.

† Bostock's Physiol. ii. p. 80.

fluence on the constantly essential processes of hematosis and calorification.‡

2. With respect to this last subject, that of *animal heat*, the results of Mr. Brodie's experiments were, for a while, thought by many to be fatal to every modification of the chemical theory, from those of Black and Lavoisier to that of Crawford. But the later and more careful experiments, and more correct reasonings of Drs. Philip, Legallois and Edwards, have tended to restore the chemical theory, so far at least, as it respects the general doctrine of the dependence of calorification on the absorption of oxygen and the production of carbonic acid. But although respiration must be considered essential to animal heat, some physiologists are still disposed to attribute calorification, in part, to the direct actions of the nervous and circulatory systems. The influence of these, however, appears to be indirect. The contact of the air with the blood, is promoted by innervation and circulation, whilst the latter diffuses through the system that caloric, which, in some part or other of the lesser or greater circulation, or both, is evolved by the union of oxygen and carbon, on a principle not very dissimilar to that of combustion.—A morbid increase or diminution in the circulation of a part may, in this way, occasion even a local elevation or depression of temperature; as in cases of inflammation and paralysis. In the former, a change in nutrition may perhaps have a slight and temporary influence; but the formation of the solids affords no chemical explanation of the heat of the body in general; as these solids must be reconverted into fluids before they are eliminated. In arguing that the change of temperature of a paralytic limb does not result from any direct influence of innervation on calorification, we might perhaps alledge the fact, that the paralytic part is in some cases above and in others below the ordinary temperature. Mr. Earle found it below, and this accords with my experience; but Dr. Abercrombie found it above, and Dr. Good sometimes

‡ That azotic principles in the food, however, have such an influence will appear highly probable from facts which I shall state under the head of Etiology.

above, but oftener below. In one case I have found it above, when sensibility was beginning to return. But even admitting that there may be some difficulty in referring some of these alledged facts to the dependence of circulation on innervation, I am still of opinion, that they are far from subverting the chemical theory.

3. Without relying upon the analogy between respiration and combustion, which may be less perfect than was once supposed—it will be sufficient barely to allude to a few of the numerous experiments and observations, which prove a necessary *relation* between the amount of *carbonic acid* produced during respiration and that of *caloric* evolved in the system. For example, Legallois effected a diminution in both, by placing an animal in such a position as constrained its respiration. Edwards ascertained that both were affected in a corresponding manner by the influence of the seasons. For in summer, less carbonic acid was formed, and likewise less heat was evolved. The latter fact was not a mere inference from the former, but was established by exposing the same kind of animal to the same freezing mixture in winter and in summer, and finding that in the latter season, the reduction of its temperature was, in a given time, six or eight times as great as in winter.

4. The *final cause* of this correspondence between the oxygen consumed and the temperature of the surrounding medium, is as obvious as it is interesting. This is a beneficent provision, by which, as well as by a variable cutaneous and pulmonary transpiration, the author of nature has, in some degree, defended man and the inferior animals against the vicissitudes of the seasons. But neither our limits nor the nature of our subject will allow us to dwell upon this interesting topic. I will only add, that this influence of evaporation on animal temperature, to which the body owes its power of resisting the heat of ovens and sudden elevations of temperature generally, and to which regard should be had in attempts to heat or cool the body by external liquid applications, was first suggested by Franklin, whilst the effect of diminished

aeration, in enabling us to resist the gradually increasing heat of summer, was first detected by Edwards.

5. That correspondence between the degrees of aeration and those of animal heat, which has been already alluded to, *extends to the whole animal kingdom.* It will be found to exist, whether we compare the warm-blooded animals with those called cold-blooded, or the different species of either of these grand divisions with each other. The respiration of the lower orders of animals, is however so imperfect, and their temperature is, consequently, so little elevated above that of the media in which they reside, that the evidence of the extension of the above law to them has, till within a few years past, been merely analogical. But discoveries in electro-magnetism have recently suggested an extremely delicate thermometer, by means of which, the relation, that in the higher classes of animals, is known to exist between animal heat and aeration, is proved to hold even among different species of insects as compared with each other; those which produce more carbonic acid, possess a more elevated temperature. This may therefore be regarded as a universal law.

6. *Experiments on the Blood.* Great importance being generally attached to the deficiency of salts in cholera blood, and the following experiments, on blood nearly or exactly normal, differing perhaps in some respects, from those made by others, they may be worth recording. When disease existed it was slight, especially as to its effect on the blood. To prevent mistake it is necessary to state, that in all these experiments, I describe the color of the blood as seen by reflected light.

Venous blood was drawn from the arm of a man who had slight symptoms of fever, with gastro-intestinal irritation. The blood coagulated promptly and completely. The clot, as is usual, was dark colored beneath the surface. This was reddened by applying to it carbonate of soda in powder. On laying a lump of it on a dark portion of the coagulum, the surface in contact with the solid alkaline carbonate, though completely covered by it, was rendered of a scarlet color

much more rapidly, than another portion wet with a saturated solution, and exposed as freely as possible to the air.—The liquid which drained from the coagulum to which the dry soda was applied, as also the solution applied, was almost instantly rendered of a bright red color, not only at the surface, but throughout the interior, where no air was present, except the small quantity contained in the blood and in the solution. These experiments led me to suspect, that this alkaline salt has the property of changing the color of *any* dark venous blood to scarlet, and that less air is requisite than is generally believed.

The blood on which the next experiments were made, was drawn from one of the large veins of a hog, and received directly into four one-ounce phials, which were completely filled. Three were instantly corked, and one left open. Of two of these which were corked, I had, before receiving the blood into them, put two scruples of carbonate of soda into each. One of these was then uncorked and immediately put under the receiver of an air-pump, which was kept exhausted for ten or fifteen minutes, during which time considerable gas escaped from it. On removing it from the receiver, about one dram of water was added, to compensate for the gas and blood which had escaped in the form of froth, and thus to exclude atmospheric air. When filled it was immediately corked, and all the corked phials shortly after sealed, and kept at a temperature of about 60° Fah. The color of both portions into which the soda had been put, was changed to scarlet, at the inferior part which was in contact with the salt or near it; but that from which the gases had not been removed, was much brighter than the other, and its coagulum had much less consistence. The two others without soda were sensibly alike in color, except that the one exposed to the air was reddened at the surface. The blood reddened by the soda, began, after an hour or two, to become sensibly darker, but after three hours, was still much brighter than the other, and that from which the gases had not been exhausted, was but little brighter, at that time, than that which

had not been placed under the receiver. The next day, the order of brightness of the blood of the three closed phials, was completely reversed.

In the next series of experiments, a portion of florid blood drawn from the epigastrium of a man affected with slight chronic gastro-enteritis, was put into hot water. The coagula were blackened by it as usual. It has been asserted, that in such cases, the water extracts those salts on which the florid color depends. The following experiments proved, that the change of color is produced in such cases, neither by the extraction of salts nor by the extrication of oxygen.—Half an ounce of the same blood in its florid state, was put with an equal quantity of water, into an ounce phial and boiled in a water bath. Both the liquid and solid parts became of a light olive color and a gaseous substance escaped. To determine the effect of a lower temperature, and to remove the ambiguity occasioned by the escape of the gases, as well as to determine the combined effect of heat and an alkaline salt on serum and red globules when separated from the fibrine of the crassamentum, two ounce phials were filled with the serous and aqueous liquid, in which the clot had been blackened, and to which it had imparted a florid color. A scruple of carbonate of soda was put into one of them, and both were then corked and immersed in a water bath, which was gradually heated to 150°. At this temperature both became opake; that which contained the soda soon becoming perfectly black as seen by reflected light, the other a dull brownish red. A half-ounce phial was next filled with the same florid liquid, corked, sealed and immersed in water at 150° Fah. It soon became opake and brownish. A scruple of carbonate of soda was then added, which immediately rendered it black. Another portion was blackened by dilute nitric acid without heat. The blackness produced by the carbonate of soda remained after filtering through paper and exposure to air. A month afterwards, the black color remained unchanged, and was not affected by reheating the liquid to the temperature formerly employed. It is a curious

fact established by the above experiments, that carbonate of soda produces the same black color in sanguineous coloring matter diffused through diluted serum and heated to 150° Fah. that nitric acid does at 60°. As a change of temperature is thus shown to modify and at length to reverse the effect produced by the salt, it is not improbable, that a repetition of the experiment with different salts and at different temperatures, might lead to interesting discoveries and suggest improvements in venous injection. It appears from these experiments, that the effect of saline substances on blood at one temperature, cannot be inferred from experiments made at another. These experiments moreover prove that the blackening of crassamentum by hot water is not dependent on the extraction of its saline matters; and also, that the change of color produced in sanguineous coloring matter by heat, is not the result of the extrication of oxygen or any other gas. Indeed this change of color was produced by heat, when the phials were filled and perfectly closed, and was more remarkable when the saline matter exceeded the normal quantity.

Some of the preceding experiments were made (as has been stated) on serum and colouring matter only. In all the following experiments all the parts of the blood were in the same proportion as before its extraction from the vessels. Two 1 ounce phials full of venous blood, after having stood corked and sealed exactly six months, the one without any thing added, and the other with five grains of carbonate of potash, were heated in a water bath. At and above 140°, considerable gas was evolved, but no change of color was produced by heating them to 155°; the seals being then broken by the force of the gas and vapour, the experiment was discontinued. In the next experiment, 5 grains of carbonate of soda were put into a phial with 1-4 ounce of blood extracted by cupping. Heated to 142° it was a little darkened and at 150° became black. A small portion seen by transmitted light appeared of a dark yellowish green color. The coagula appeared to have been rendered somewhat fluid. On raising the temperature, little change of color or consistence was

observed till it reached 170°, when the whole was found to have lost its fluidity and to have assumed the consistence of currant jelly. This consistence was not again diminished by cooling, but on the contrary increased. A heat of 150° had rendered it black, whilst a heat of 170° had coagulated the whole, without however giving it the tenacity or color of the original coagula. Even when cooled it had little of the elasticity of crassamentum, though it had no fluidity. No gas was evolved during any part of the process. The next and last experiment was made with three 1 ounce phials full of blood, which may be distinguished as A, B and C. A was venous and arterial blood obtained by cupping, and simply sealed. B and C venous blood, the two portions exactly alike. To B, grs. v. carbonate of soda were added; to C, grs. v. of the carbonate of the same alkali. All being sealed were exposed to the same heat.—The following were the results: The color of A was not sensibly changed till heated to about 170° when it underwent a kind of coagulation, assuming a consistence and elasticity like that of a solution of glue when cold, its color simultaneously changing to an olive grey. B became black at 150°, (its color beginning to change at about 148°.) This blackness extended only as far as the previous reddening influence of the carb. sod., the rest remaining of the original dark red color. C was not in the least darkened at 150°; but where the bicarbonate had come in contact with it, it still retained that excess of brightness which the salt had communicated, till heated to some point between 160° and 170°, when it became dark but not as black as B. From a comparison of B and C, I infer that that salt of soda which contains most carbonic acid requires a higher temperature to render blood dark, and blackens it but imperfectly, even at a higher temperature. From these last experiments, and from a comparison of those on A with similar ones which have been described above, and which were made on the same kind of blood, I infer that the carbonate of soda with heat, blackens both venous and arterial blood. The coagulation of the mixed blood at 170° was effected by heat alone. These last experiments then, though made under circumstances

which developed some new and curious phenomena, tended to confirm and render more general the preceding conclusions in regard to the blood.

7. *Pathology.* I shall presently have occasion to allude to the striking resemblance between the blood of cholera and that of asphyxia, and to experiments on the latter similar to those just described. But I shall first endeavor to show that the physiological connexion between respiration, hematosis and calorification, which we had been before considering, is highly interesting in its *application to pathology*, and especially to *Epidemic Cholera*. The radical defects, which, in this disease, exist in these essential functions, should not be, as they too frequently have been, regarded as of secondary importance. The dark color of the blood in cholera is well known. To this we are to attribute the dark or bluish color of the surface of the body, which is not, however, identical with that of the blood, but depends partly upon the color of the medium through which the blood is seen. The coldness of the skin, tongue, breath, &c. I have frequently observed, and others have a thousand times described. In this disease, there is frequently no sensible difference between the color of the venous and that of the arterial blood. Many physicians have compared the blood, to tar or treacle. The blood drawn from a patient was found by Dr. Reid Clanny to be as black as tar, and to contain more than twice as much carbon as healthy blood. It was tasteless, and contained no carbonic acid or gas of any kind. The want of taste cannot be wholly referred to the elimination of salts, but affords evidence of a defect in the respiratory function; for the stronger taste is one of the properties acquired by this liquid in traversing the lungs; and as superior sapidity distinguishes arterial from venous blood, we might naturally expect it to distinguish venous blood from supervenous.

8. Whether the carbon which he obtained by his ultimate analysis, previously existed in a free state, as he, in my opinion, too too hastily concluded, is of little consequence. None of those who have objected to his views, have produced any ultimate

analysis, which militates against his conclusion, that there is a great excess of carbon, compared with that which exists in normal blood. In the controversy which has been carried on by Dr. Clanny and others, the object has been, either to prove or disprove the existence of carbon in the blood in a *free* state, and the excess of *uncombined* carbon in cholera blood. The decision of this point is probably of little importance in the pathology of cholera; at any rate, it does not, in the least, affect the question which I am considering. The manner in which the elements of that part of the coloring matter which contains carbon, are arranged, whether in binary, ternary or quaternary combination, has never been determined; but Brande, Engelhart and Michaelis have shown that the coloring matter of the blood consists of an animal matter, associated with a minute quantity of iron and some earthy salts; and this animal matter, of which the coloring matter appears chiefly to consist, was found by Michaelis to contain more than fifty-three per cent. of carbon. Now in what degree the carbon of the coloring matter may vary in different kinds of blood, I find no satisfactory data for determining. The furnishing of these, is perhaps a service which pathology has yet to expect from chemistry. But even conceding, that a given quantity of the coloring matter of cholera blood, contains only the same amount of carbon as the same quantity of the coloring matter of normal blood, (and no one has pretended that it contains less,) it can, I think, be shown, that the whole amount of carbon in the coloring matter of the former, far exceeds the whole amount in the coloring matter of the latter. For according to the researches of Dr. Thomson, professor of chemistry in Glasgow, the coloring matter of cholera blood, as deduced from the mean of his results, is "little short of *four times* the quantity of coloring matter of healthy blood." Dr. Clanny's *proximate* analysis afforded nearly the same result. This may be reconciled with the result of his *destructive* distillation in another instance, in which he obtained only about twice as much carbon from cholera blood

as from healthy blood, by considering that both fibrine and albumen contain rather more than fifty per cent. of carbon, and that the great increase of coloring matter, is partly compensated by the diminution of the sum of these other carbonaceous principles. That this will explain the apparent discrepancy, might be shown by the numerical results. But that the change in the absolute amount of fibrine is small, compared with that of the coloring matter, is evident from Dr. Thomson's testimony—that the fibrine and coloring matter of healthy blood added together, amount to less than one-half the coloring matter in cholera blood. Notwithstanding all that has been said by the opponents of hyperanthraxis, I am unable to discern, why the quantities of carbon obtained from two portions of blood, submitted under the same circumstances, to destructive distillation in a close vessel, may not correctly show the relative proportions that actually existed in them. The only sound objection refers to the *state* in which the carbon existed.

9. Now, that this vast accumulation of carbon in the blood of a cholera patient is absolute, and not merely a relative increase, resulting from incrassation, in consequence of the removal of its aqueous portion, by profuse evacuations or any other cause, we may readily convince ourselves from a comparison of the numerical results in Dr. Thomson's table, by which it will be found, that the proportional diminution of water in cholera blood is very small, compared either with the proportional increase of coloring matter as shown by his experiments, or of carbon as shown by Dr. Clanny's. How then, can it be true, that the addition of the dejections to the blood would restore it to its normal condition? Are not even Dr. O'Shaugnessy's results with respect to the albumen, opposed to the above conclusion which has been drawn from his analysis? The *history* of this epidemic is opposed to it. It has been long since and repeatedly observed, by those who have been familiar with the disease, in its most malignant and perfect form, that the most rapid and intractable cases, were

generally attended with slight, if any, alvine evacuations.—Not to cite other authorities, Mr. Orton at Bombay, and other surgeons in that vicinity, stated that in many cases there was no purging, in some no vomiting, and in others neither, and that these were by far the most dangerous cases, and that the patients died under them, often in an hour or two; and that, without spasms and with scarcely any vomiting or purging, all the secretions appeared to be in many cases entirely suspended.* Conceding the possibility, that in some cases, the contents of the alimentary canal may not have been examined, still, from what we know of its dimensions, and from the effects of equivalent evacuations in other diseases, a theory which can be defended only by a supposed accumulation in such cases, must be considered untenable. Has it not been chiefly in those places which have been but slightly visited, that we find pathologists disposed to found a theory on profuse evacuations? And under such circumstances, these may perhaps merit all the attention which has been bestowed upon them; although the most fatal alterations of the blood, and the suppression of urine and other secretions, must often, if not always, depend upon another cause.†

10. It is hardly necessary to remark, that the presence of an immense excess of carbon in the blood, manifests a defect in the decarbonizing process of the system. It may be less obvious, though hardly less certain, that the observed *absence of carbonic acid* in the blood, in this disease, depends upon a similar cause. A considerable quantity of this gas exists in healthy blood; and it might be asked, why the

* Good's Study of Med. I. 178.

† There is little doubt, that the secretion of urine may be both increased and diminished, by some agencies which have no direct influence on the action of the kidneys, or the quantity of serum in the blood. Even Dr. Cullen acknowledged his suspicion of this; opposed as it was to his favorite theory. His modesty and candor are worthy of the imitation of those who are ambitious of framing *complete* theories in medicine. He says, that "besides the increased quantity of water in the mass of the blood, or a stimulus particularly applied to the kidneys, there may be a medicine which, by a general operation on the system, may promote the secretion of urine.—My candour obliges me to mention this; but I do not find myself at present in a condition to prosecute the enquiry."—Materia Med. vol. II. p. 556.

same cause, from which, in this disease, the blood retains its carbon, does not also make it retain the carbonic acid. I answer, it is because the carbonic acid eliminated during respiration, derives its oxygen from the inspired air, and when little or no oxygen is absorbed, we should expect little or no carbonic acid to be formed. From the absence or deficiency of carbonic acid, I should infer, that in this disease, there exists *more difficulty in obtaining the advantages of inspiration* than those of expiration—in introducing oxygen to form carbonic acid, than in eliminating the latter when formed. I may express it by saying that *exosmose*, or the outward impulse, *predominates in the lungs*. It would be interesting to determine, whether, in the last stage of cholera, the bulk of a confined portion of air would not be increased by respiration; that of the carbonic acid separated from the blood, exceeding that of the oxygen absorbed. It is probable that life, for some time before its termination, is maintained by the oxygen, which, previously introduced into the blood, is now circulating with it, perhaps in union with the colouring matter, slowly combining with its carbon, and escaping from the lungs in the form of carbonic acid; and that the patient is in a situation similar to that of the animals which Dr. Edwards confined in hydrogen, and which were found to give out carbonic acid, though no oxygen could, in those circumstances be absorbed. It is not contended, that the want of sufficient oxygen to oxygenize and decarbonize the blood will enable us, in the present state of knowledge, to explain all the pathological phenomena; but it seems to have a more important influence than is generally suspected.

11. In the phenomena presented after death, there are many striking coincidences between cholera and asphyxia from other causes; the same fluidity of the blood, for hours after death, the same tendency of the body after death, to an an increase of warmth and diminution of lividity, as in cases where the respiration is suspended by hanging or drowning, or not established by a closure of the foramen ovale. I have known these phenomena to be presented after death in the

case of a premature child which was born at the end of the 7th month and lived till the fourth day after its birth; and I have observed some of them in cases of death by hanging and drowning, and I believe them to be characteristic of asphyxia. They frequently inspire the friends of the deceased with the vain hope of effecting a resuscitation.

12 .The following case will illustrate some of the foregoing remarks, as well as the effect produced on the blood by certain salts, which, since the experiments of Dr. Stevens,* have been supposed to perform an important part in the function of respiration. A son of Mr. V. V., ætat. eighteen months, had fallen into a cistern of water, and lain, as was supposed, a quarter of an hour or more, and had been taken out about half an hour before my arrival. We attempted to reestablish respiration, by inflating the lungs, not only by the mouth, but by a pair of bellows fitted to a flexible tube which was introduced into the trachea. The body was also rubbed with a stimulating lotion, with which I had succeeded in restoring warmth to a cholera patient after the commencement of collapse,† and which I considered it advisable to try in most cases of asphyxia. Other means were used, but to no effect. About two hours after the death of the child, the jugular vein was opened. A dark colored blood ran freely. Three or four ounces were taken. Its coagulability was so slight, that it required a plaster to arrest it. As the plasticite or coagulability of arterial blood as compared with venous, is a property acquired by respiration, it might be expected, that the supervenous blood of cholera and other species of asphyxia, would be more deficient in this property than ordinary venous blood. And such is the fact. In the above case, it coagulated very slowly and imperfectly after its removal from the vein, resembling, in this respect, the blood drawn during life, from cholera patients. After half an hour had elapsed, about one third of it had not coagulated; al-

* I have not had the advantage of perusing his work.

† This patient continued from this time to recover, and was restored to health.

though the temperature of the air was about 70°. The upper part which had been exposed to the air, was coagulated to a certain depth, and its color at the surface had become rather brighter. On inclining the vessel, the dark, thick and uncoagulated fluid broke through the coagulated crust, and flowed sluggishly across it, presenting an appearance somewhat similar to that of cholera blood, which from its consistence and blackness, has so often been compared to molasses and tar. The parts of the coagulum below the surface were also dark colored. It appeared evident, that air favored coagulation, and was more essential to the production of the florid color, but did not appear to effect the latter as readily and perfectly as in the case of normal or healthy blood.

Muriate of soda was then added to one portion, and carbonate of soda to another. The latter had a marked effect, rendering it florid. This experiment, and others made on normal venous blood, have convinced me that it is unphilosophical to infer from the effect of salts in reddening cholera blood, that the asphyxia which produces its dark color depends on the deficiency of saline ingredients in the blood, even though such deficiency should by analysis be shown to exist; for in the case above mentioned, and in others alluded to, a similar change of color was produced by the salts, although there was no reason to suspect any greater deficiency of saline ingredients, than ordinarily exists in venous blood. At least, there was no reason to suspect it, unless we are disposed to admit, that the saline ingredients are partly generated in the lungs—an idea which would appear rather unchemical, but which may be favored by the fact stated by Magendie, that "at the moment" when the blood passes through the vessels of the pulmonary cells, its taste becomes more distinct. As favoring this view, it may be worthy of remark, that the same superior sapidity which distinguishes arterial blood from venous, distinguishes normal venous blood from that of cholera. But this alternative is of no advantage to the theory of those, who attribute the asphyxia of cholera to the evacuations. I will only add, in relation to the above

case of asphyxia by drowning, that the circumstance that the color of the blood was less influenced by exposure to air, than ordinary venous blood, shows that a defect in this property is not peculiar to cholera, nor to disease of any kind, properly so called, but appears to be characteristic of asphyxia in general, whether induced by disease or suddenly caused by interrupting the respiration of an individual previously in health, when there have been no intestinal discharges to drain the salts from the system.

13. I consider the defect in coagulability as also common to all those cases where a want of due oxygenation is the sole or chief cause of death. Excessive exercise and violent mental emotions, when they occur suddenly, are said to produce this state of the blood; and it appears to me an interesting fact, that these are also among the causes which tend to prevent its oxygenation. Another correspondence not less curious is, that in the foetus, whose respiration has never been established, the venous and arterial blood, like that of the victims of cholera, is nearly identical; the blood is not coagulable, has an unctuous feel, and does not take the vermilion color on exposure to the air; and according to Fourcroy, it has its coloring matter darker and more abundant, and contains no fibrine. It therefore remarkably resembles cholera blood. Would it not seem from these facts and considerations, that the coagulation of the fibrine, and even its existence as such, are more dependent on respiration than has been hitherto suspected; and that the deficiency of this principle, as well as the existence of most of the other peculiarities which distinguish cholera blood from normal blood, result chiefly from its defective aeration, and are what might be expected in asphyxia from any other cause? In this generalization, by referring the want of coagulability of the blood in cholera, simply to want of aeration, I may be perfectly singular; and in regarding it as a characteristic of asphyxia in general, I am not sustained by the authority of Dr. Good, who mentions it among the characteristics of the electrical variety only. But I must consider his representation

as erroneous; for Broussais affirms that in gaseous asphyxias, the blood remains uncoagulable. Combining these two authorities, relating respectively to asphyxia caused by lightning and that caused by the irrespirable and deleterious gases, with my own observations on other varieties of asphyxia, I am led to infer the generality of the above law. That the tarry appearance of cholera blood results simply from want of aeration, is also confirmed by the fact, that the same appearance may be immediately produced by prussic acid, but never unless given in such doses as to occasion difficulty of breathing.*

14. *Pathogeny.* Those pathological remarks which I have to make in relation to the pathogeny or origin and developement of this disease, being more hypothetical and referring in part to desiderata, I prefer placing them under a distinct head. What electrical or other change is produced, by the unknown exciting cause of cholera, in the blood or nervous system, or what alteration in the pores of the cellular pulmonares, so as to diminish the absorption of oxygen, or in what degree the consequent alterations of the blood, by enfeebling the actions of the heart and obstructing the pulmonary circulation, exert upon the respiratory function a reciprocal and indirect influence, to which the effects of the alvine evacuations are accessory—are interesting problems, for the solution of which we may hope at some future stage of science to possess adequate data. May we not hope that some of the interesting discoveries and suggestions of Drs. Dutrochet, Mitchell and Faust, in relation to endosmose and exosmose† and the penetrativeness of fluids, may contribute to the elucidation of some of the phenomena of cholera? Possibly the secretions of the intestines and skin may be referable to exosmose, either induced by a change in the aeration

* Am. Jour. of Med. Sci. vol. xi. p. 501, from an European Journal.—Dr. Hertwig who made this discovery, blackened the blood by different acids, but could not, it appears, produce this effect by *nitric* acid.—Ibid. I have, however, ascertained by experiment, that nitric acid *does* render the sanguineous coloring matter black as seen by *reflected* (though not as seen by transmitted) light. Has this distinction between the reflected and transmitted light been made by those who have experimented on the blood?

† An inward and outward impulse.

of the blood, or as a collateral effect of the same cause, which produces that increase of exosmose, which may, and that diminution of endosmose, (or at least of the tendency of oxygen to penetrate inwards,) which, in my opinion, actually does take place in the lungs.

15. A defect in calorification and sanguification may exist, in a slight degree, in an early stage of the disease, and not become the most obvious characteristic till the last. Before any profuse alvine evacuations had taken place, I have, in several instances, observed a coldness of the hands and feet, a blueness of the under eyelid, and a preternaturally dark color of the blood drawn from the arm. In this stage also, Dr. Baird found the heat of the skin below the healthy standard. Dr. McIntyre notices a slightly discolored state of the under eyelid, as among the most frequent premonitory symptoms. Others have observed, that the dark color of the skin frequently prevails as a premonitory symptom from one to ten days, whilst there is no peculiarity in the evacuations. Dr. O'Shaugnessy, however, could not detect any change in the blood at the onset of the premonitory diarrhea.

16. That no other disease effects so remarkable a change in the composition, color and temperature of the blood, must be admitted; also that these alterations are disproportionate to the amount of alvine evacuations, whether we compare different cases of this disease, or this disease with others.—Although, neither the physiology of respiration, the chemistry of normal blood, nor the chemical pathology of cholera, is so complete, as to justify any positive opinion as to the precise time, nor any complete theory of the manner, in which these changes commence. Indeed, the pathogeny of most diseases is obscure; and pathology seldom detects the first links in the chain of morbid phenomena. In cholera, it can hardly be considered more fortunate with respect to some of the subsequent ones. There is no complete and satisfactory theory; and I have not the presumption to attempt to propose one.

17. I believe, that before this can be done, much remains to be learned, with respect to the sympathy between the gas-

tro-intestinal mucous membrane and the organs concerned in the aeration of the blood. Since this idea occurred to me, I have met with a remark of Broussais, which may defend me from the charge of singularity. He believes that typhus and other forms of gastro-enteritis, may result from derangements of the aeration of the blood in the lungs, derangements produced by certain aerial influences; and that then, "this phlegmasia, by reacting on the mucous membrane of the pulmonary apparatus, impedes to a certain extent the aeration of the blood, and that this is the chief cause of the lividness observed in the worst grade of those diseases."* I think it will be discovered not only that impressions on the mucous membrane of the lungs, may affect that of the alimentary canal, but that certain impressions on the latter may affect the former in a manner not recognized in the purely hydrodynamical theories of cholera asphyxia.

18. In insisting on the essential connexion of this disease with respiration, I shall therefore not be tenacious of the opinion, that the first impression is always made on the organs immediately concerned in this function; but I venture to state some considerations, which may serve to render it less improbable. Many diseases depend on a functional derangement of the lungs, unattended by any organic lesion of them. Not only an exposure of the surface of the body, but the inhalation of humid air, when the skin is perfectly protected by abundant clothing, can, without producing any obvious pulmonary or cutaneous affection, introduce serious mischief into other and distant parts of the system. If any one prefers attributing this to a suppression of the aqueous transpiration of the cellulae pulmonares, and we wave the argument which might be drawn from the remote sympathy of mucous membranes, on the ground that minute anatomy cannot demonstrate the existence of mucous membrane in the air cells, we might still ask, if some of those impressions which are attended by a diminution of aqueous transpiration, may induce an augmentation of quantity and depravation of qual-

* Physiology, part ii. chap. iii. sec. viii.

ity of the secretions of the mucous membranes, why may not some of those impressions on the lungs which are attended by a retention of that carbon which should be eliminated in the form of carbonic acid? The first case does not depend simply on a transfer of liquid—a derivation to the mucous membranes, of what the lungs have failed to eliminate; for the pulmonary transpiration is not mucus. But not only is this detention of halitus attended by an augmentation of the secretion of the mucous membranes, but by a vitiation of it; the mucus loses its normal character, and becomes acrid and irritating. There is therefore a peculiar sympathy between the mucous membranes and the lungs, which cannot be referred mechanically to the increased fullness of the vessels, resulting from a diminished discharge. As some of those impressions which occasion a diminution in one of the substances eliminated by the lungs, are followed by a specific alteration of the mucous membranes, why is it incredible that some of those impressions which occasion a diminution in another substance eliminated by them, might be followed by a different and specific alteration? But although Magendie entertains doubts as to the extension of the mucous membrane of the bronchiae to the air cells, analogy is in favor of it; and probably this mucous membrane is always affected in cholera, though this affection be different from catarrh and in some cases unobservable. Though the lining membrane of the air tubes occasionally appears to be normal in cholera, we are informed, that more frequently it is either lined with a yellowish secretion or presents a reddish tinge.*

19. *Etiology.* There are many reasons for believing, that during the prevalence of cholera, there is some wide spread miasm or other aerial epidemic influence tending to diminish the aeration of the blood. We have perhaps some indirect evidence, in the nearly simultaneous prevalence of certain diseases in which the blood is similarly affected though in an inferior degree. Is there not in many places, either antecedently or subsequently, an increased prevalence of cer-

* Medico-Chirurg. Review for Jan. 1833.

tain diseases which are attended with dark blood, such as measles, and typhus and other malignant and occasionally anomalous fevers? take for example, the fever in Connecticut, which preceded, and the typhus which often succeeds it. For weeks and months before the acknowledged incursion of cholera, there are frequently cases of disease which in these respects nearly resemble it, as I am convinced by my own observations and those of many other physicians.*

But to give more satisfactory proof of this connexion between cholera and respiration, I shall proceed to examine, whether the history of cholera does not present a class of etiological facts, which, considered in connexion with the results of experiments that have been made on respiration, without any reference to cholera, tend to confirm the foregoing views with regard to one of its principal if not essential features.

20. It has justly been remarked by Broussais, that "the aeration of the blood is the first and most important of the internal functions, that the interruption of which, can be least tolerated, and on which so many evils follow." But neither the respiration nor the evils which follow its diminution, are equal in all animals or in all constitutions. Priestley discovered, that different animals of the same species, produce different effects upon the air, according to their constitution. Others have since more particularly examined the subject, and ascertained many additional facts, in relation to those states of the system, and those of the air which promote the oxygenation or decarbonization of the blood. The lower the respiration required by an animal, the less will it suffer by a diminution of it. This is exemplified by comparative experiments on the warm-blooded and cold-blooded animals. Indeed, Dr. Edwards has established the general law, that the organic vitality is inversely as the calorific function; those animals which generate most heat, resisting asphyxia most feebly. We derive from this a confirmation of the foregoing

* In the interior of Canada, a disease similar to cholera, if not identical with it, is said to have prevailed among the Indians previous to the first announcement of its existence among the white inhabitants of this continent.

views in relation to cholera. For, a fatal epizootic, resembling cholera in the whiteness of the discharges as well as in the black color of the blood, has prevailed among different species of herbivorous and granivorous animals, simultaneously with cholera in man; but there is no instance, in which those animals which have a *low* respiration, as the cold-blooded, have been affected. The same seems to hold in the human species; and I believe it to be, in a great measure, owing to this inferior power of resisting the effects of a diminished respiration, that middle-aged men, who in health are known to consume more oxygen, in proportion to the whole air respired, than the young, are more liable to cholera. Infants, who consume much less oxygen, suffer much less injury from the action of that peculiar poison, which produces cholera and diminishes the chemical effects of respiration.—They are consequently much less liable to the disease. No one can suppose their comparative immunity to depend on any want of predisposition to alvine fluxes; to these, they are even more liable. In this respect, as in many others, cholera and diarrhea are subject to different laws. Finally, there is probably less difference between the respiration of the sexes, than between that of adults and children. The same remark is applicable to their relative liability to cholera. Generally, however, a difference in this respect has been noticed. And this is what the above principles would lead us to suspect; for, the temperature of a man is found to be a degree higher than that of a woman; hence the former might be known to possess a higher respiration, and might be expected to suffer more by the diminution of its chemical effects.

21. But in addition to age, sex and constitution, there are other circumstances, the influence of which is still more unequivocal. Crawford discovered the influence of external *heat*. His experiments and those of others have satisfactorily shown, that the quantity of oxygen consumed and of carbonic acid produced during respiration, is less as the temperature of the air is more elevated. All who have experimented on the subject, with but one exception, have detected

this influence of temperature. Crawford found that a Guinea pig, confined in air at the temperature of 55° Fah. consumed double the quantity of oxygen which it did in air at 104°. In the case of human respiration, Lavoisier and Seguin ascertained, that the quantity at 57° is to that at 82° as 1344 is to 1210. Delaroche, in his last series of experiments, made the average ratio about as six to five at the temperatures tried by him. He found, that by elevation of temperature, the production of carbonic acid was diminished, and the absorption of oxygen diminished in a still higher ratio. More recently Dr. Edwards has examined the effect of different seasons, and found that the *long-continued* actions of heat and cold affect the respiration as a vital function; the oxygen consumed being less in summer, even when the air in which the animal is confined at the time, is of the same density and temperature. Moreover, from the experiments above related respecting the influence of sudden changes of temperature, as well as from the known effect of temperature on density, it appears to me evident, that its physical changes between winter and summer, must be such as to make the immediate influence of heat conspire with its gradual physiological effects, and render the consumption in winter and summer still more disproportionate. I am not aware that it has been determined, whether the immediate effect of temperature depends upon the ambient or upon the respired air, or in other words, upon the impression of heat on the surface of the body, or on the pulmonary vesicles. For deciding this, I would suggest comparative experiments on the respiration of warm air in a cold room, and cold air in a warm one, by means of a communicating tube placed between two rooms of very unequal temperatures. But whatever be the place or mode of its first impression, the influence of heat in diminishing the consumption of oxygen may be considered established. On the other hand, few facts are better established, than the influence of hot climates and the warm season of the year in predisposing to cholera. The epidemic of 1817, which has since been spreading over the

globe, commenced in summer in the hot climate of Hindostan; it has generally, in all climates, been much checked if not extinguished by winter; also on cold, elevated mountains. Its ravages in Mexico have proved, that it can rise to a great height above the surface of the sea in *warm* climates. In Russia, the southern regions were those where it spread most widely and rapidly, and those towns which it entered at the end of autumn, suffered but slightly.* It did indeed linger in winter, in some of the filthy ill-ventilated and highly heated rooms of that country. Air heated by stoves is in general not less salubrious, and in regard to many diseases more so, in consequence of its uniformity of temperature and exemption from cold currents; yet Russian stoves might be expected to conduce to epidemic cholera; though they would not produce it without the concurrence of other causes.† That the influence of highly heated rooms in predisposing to this disease, should be no greater than it actually is, and that this influence is much less than that of the open air of summer, is to be attributed to the greater dryness of the former, the favorable influence of which effects a partial compensation. Though, in heating a room containing no liquid exposed to evaporation, the actual quantity of water in the air is the same at all temperatures; yet heat, increasing the capacity of the air for moisture, diminishes its humidity. Thus heating a confined portion of air has an effect very different from that of a general elevation of the temperature of the atmosphere. This explains the aridity of rooms artificially heated. This aridity is greater in stove rooms, simply because they are hotter. In a room warmed by a brick stove, it requires a higher temperature to remove the sensation of chilliness, and render the warmth agreeable, than in one warmed by an open fire, or by radiation from iron pipes or through plates of mica. This fact, in my opinion, admits of the following generalization. A partial exposure

* Report of M. Moreau de Jonnes.

† The use of stoves properly constructed may be expected to conduce very much to health on the whole.

of the body to intense radiation or other strong heat, produces a general and pleasant sensation of warmth, although the clothing which invests other parts of the body, as also the ambient and respired air, may be comparatively cold. To return from this short digression—I may add, that even in Persia and Asia Minor, the influence of winter on epidemic cholera was manifest during several successive years.* This influence of temperature has been confirmed by the progress of the disease on the western continent, which at one time it threatened rapidly to overrun. It commenced in spring, and until the autumnal cold, nothing impeded its rapid march or changed its malignant character. Both were restored by the heat of the ensuing spring ; and again suspended by winter.

22. There are other agencies which correspond in producing a remarkable effect both on oxygenation and cholera.—Dr. Prout ascertained, by direct experiment, that the quantity of carbonic acid produced during respiration is diminished by long-continued and violent exercise ; by fasting ; by the depressing passions, or even *strong* mental emotions of any kind ; and by intemperate habits, and even the moderate use of alcoholic liquors. It is well known, that all these are powerful predisposing causes of epidemic cholera. The disease has been frequently favored by the fatiguing marches of armies, and the privations which they have suffered ; by the existence of poverty with its attendant evils of excessive labor and scanty food ; by the depressing passions, such as the fear of the disease itself ; and by intemperate habits, and even the moderate use of alcoholic liquors. The want of success which has generally attended the administration of alcohol in this disease would not of itself be conclusive, but it may have some weight. The foregoing views respecting the causes and nature of epidemic cholera, and a knowledge of the specific action of alcohol in diminishing the oxygenation of the blood, in all individuals however temperate and healthy, might have led us to anticipate that its influence in predisposing to this

* Rep. of French Acad.

disease would not be confined to the broken-down drunkard. This inference from theory is confirmed by experience. In relation to that form of the disease, in which, at the height of the epidemic in Vienna, it most nearly approximated to the perfect type, and in which the seizure was sudden, the evacuations almost or altogether wanting, the cramps severe, and the fatal termination in most cases in a few hours, it was observed that a middle age, vigor of constitution, and such a use of gin as had not materially affected it, were pre-disposing causes.* The last is a practice which diminishes the aeration of the blood, and the two former are circumstances, under which, as has already been shown, such diminution can be tolerated with least impunity. In relation to ardent spirits, as pre disposing to this disease, mistakes have arisen from too wide a distinction between *drinking* and *drunkenness*. These mistakes would be corrected by physiological views.

23. In the next place, it has been ascertained, that the quantity of carbonic acid produced, is less in the *night* than in the day time. Whether this depends directly on the absence of the sun or not, is not certainly known. From the well established relation between the aeration of the blood and animal heat, considered in connexion with the opinion which Dr. Edwards' experiments led him to entertain, that there is less animal heat evolved during sleep, we may conclude that sleep contributes in some measure to the defect of aeration.— Rest may be added, as *moderate* exercise increases oxygenation. But this has no material influence on the value of the above mentioned fact, except that it tends to confirm the influence of night, the usual season of rest and of sleep. Now it has been frequently stated, that the attacks of cholera are generally more frequent during the night. At Smyrna, in October 1831, and in some other places, the mortality it was said, occurred principally in the night. The French Royal Academy of Medicine stated in their report, that the invasion of the disease had generally taken place in

* Lond. Lancet for June 23d, 1832.

the night and toward morning. Now Dr. Prout found that the carbonic acid in the respired air, reached its minimum at half past eight in the evening, and remained at the minimum state till half past three in the morning. As the effects of this defective aeration of the blood, are accumulating during the whole of this period during which it remains at the minimum state, should we not expect that, in proportion as the influence of night predominated among the causes of the disease, it would manifest itself oftener towards morning? And is not the principle analogous to that on which depend the more wide and rapid extension and the increased severity, of cholera and some other malignant diseases which are connected, though less remarkably, with a defective aeration of the blood, at the close of that season of the year in which this function is at its minimum? A similar principle is applied in physics, to the explanation of the observed time of maximum temperature both of the day and year.

24. Again, Dr. Fyfe proved by experiment, that the carbonic acid was *reduced to nearly one half by vegetable* diet. Now this is the diet which has predominated in those countries, in those cities and in those classes of society, in which the disease has been most fatal, whether in Asia, Europe or America. It is true, some physicians have recommended vegetable food during the epidemic, interdicting only unripe vegetables, and a few kinds generally admitted to be peculiarly unwholesome. But this preference for vegetable food must, in my humble opinion, proceed rather from an incorrect theory, and from their experience in other diseases supposed by them to be analogous, than from their experience or that of the world in this disease.* In the report published by the authority of the French Academy, it is affirmed, that during the cholera at Calcutta, "those who lived on vegetable substances were first taken off; and that women and children seemed to be spared." I quote the whole passage, as the latter

* Most species of *grain*, however, being more easy of digestion and containing more azote than most other parts of vegetables, make a nearer approximation to animal food, and are hence less injurious than some other vegetable substances.

part affords some evidence, that this influence of vegetable food, is not, as some have supposed, referable merely to the debility and consequent irritability induced by it.

Moreover, that it was mode of living, and not idiosyncrasy, that rendered the Hindoos so much more liable to the disease than the English residents, may be argued from the fact, that the native soldiers, whose mode of living was more similar to that of the English, enjoyed a similar degree of exemption. Indeed, it was every where observed, that those who subsisted on vegetable food, were selected as the first victims. Perhaps it may be worthy of remark, that the unusual mortality in Paris, where at least twenty thousand of the inhabitants were carried off in a month, occurred during the season of Lent. Immediately after Easter the virulence of the disease rapidly abated. The proportion of vegetable food is usually great among a French population. May not the severe cholera of Montreal and New-Orleans in 1832, be cited as in some degree examples of its influence? On the ensuing spring, the disease, after its winter's sleep, awoke in the Catholic city of Havana, in the season of Lent. It numbered among its victims many of the most respectable and religious citizens, and produced a mortality unprecedented in the history of its ravages in the western hemisphere.* Were these isolated facts, they would merit less regard. But the influence of vegetable food has been generally observed to predispose to cholera, and even to epizootics strongly resembling it, but never, in any instance which I have heard, among animals exclusively carnivorous. I need only allude to the epi-

* Among other respectable individuals who fell victims, was the Archbishop of Havana. On some days 900 are said to have died out of a population of about 180,000. Not having access, however, to official documents, I extract this from the newspapers, as also the following account of the recommencement of the march of the disease in Louisiana, nearly at the same time. The following is from the Albany Argus of April 15th, 1833. "The Louisiana Republican, printed at Franklin, in the Attakapas region, says that the cholera has begun to assume, in that quarter, a more formidable appearance. At first, few cases proved fatal, except those which occurred among the colored population; persons of temperate habits were seldom attacked." [This shows the influence of alcohol.] "But of late, citizens *particularly noted* for their temperance have fallen victims." [This shows, perhaps, the combined effect of heat, vegetable diet and fast-

zootic which prevailed among several species of herbivorous animals in Scotland, and to the mortality among the fowls at Choisi near Paris; to their white alvine discharges; and the dark color assumed by their combs, affording, from their translucence, an index to the color of the blood.

From the effects of vegetable diet and abstinence from food, I must believe, that a fast in either sense, during the epidemic, would tend to aggravate the awful calamity which it might be proposed for the purpose of averting; yet, on the other hand, as strong emotions, and especially the depressing passions, have been shown to produce an influence similar to that of fasting, it is evident that a religious frame of mind, a calm and cheerful reliance on Divine Providence, must be among the best preservatives.

25. It might seem hardly necessary to remark, that the liability to the disease is not proportional to any *one* predisposing cause, but where one of them exists, there will be, caeteris paribus, a greater liability than where none exist. From inattention, however, to this obvious principle, and from observing that in some cases the absence of one, and in other cases, the absence of another of the predisposing causes afforded no security, the illogical inference has occasionally been drawn by some popular writers, that the disease, as it respects its causes, is perfectly anomalous.

26. I have shewn, that the correspondence between the causes of cholera and the circumstances which diminish the consumption of oxygen, extends at least to the seven following particulars. 1st, Heat; 2d, Long-continued and violent exercise; 3d, Alcohol; 4th, Fasting; 5th, The depressing

ing.] The St. Martinsville (Lou.) Courier of 22d March, gives a similar account of its prevalence in that place and its vicinity. "As it was nearly stationary during the winter, we thought that the salubrity of our situation would preserve us, but within the last three weeks, it appears to have extended its exterminating influence, and we have already to deplore the loss of several respectable inhabitants of our parish, as also a great number of slaves." Now it is worthy of notice, that the 24th of Feb. was the first Sunday in Lent; and it would seem that the extension of its "exterminating influence" commenced about a week afterwards. What intemperance did for the lower and dissolute, did not fasting and vegetable diet contribute to effect for the higher and religious classes? Those who are better acquainted with their habits can judge.

passions; 6th, Night; 7th, Vegetable diet. To these should probably be added—Cutaneous Filth. Want of cleanliness is known to be a predisposing cause of cholera. Now, by mechanically obstructing the pores of the skin, filth might be expected to diminish the aeration of the blood, which is known to be effected in part by the skin. There is here, as in the lungs, an absorption of oxygen and elimination of carbonic acid. There is in man, a cutaneous as well as pulmonary respiration; though it is less remarkable than in animals of the order batrachia, which will survive the loss of their lungs longer than that of their skin.

27. Among the circumstances which physiologists have found to diminish the quantity of oxygen, there are, in addition to those which have been enumerated, a few others, the influence of which in predisposing to this disease requires further investigation. These are, the use of tea; the administration of nitric acid; affecting the system by a course of mercury; and placing the body in a posture which impedes respiration. I am acquainted with no general etiological fact in relation to these, which militates against the foregoing views. As it respects mercury, the effects of this agent on the system, are so numerous and powerful, that its therapeutical agency in this disease cannot be urged as an objection. Thus even the physicians of the physiological school admit that it may relieve gastro-enteritis by revulsion, although its direct action tends to augment the inflammation of the mucous membrane of the alimentary canal. On the other hand, among the probable predisposing causes of cholera, there are some, whose influence on the consumption of oxygen requires further investigation. Among these, are certain changes in the hygrometric, barometric and electrical states of the atmosphere. These are so generally connected, that simultaneous and repeated observations, and a nice discrimination, would be requisite to determine their separate influences even on cholera. In August 1832, I observed at two different times, a considerable *depression* of the barometer and eleva-

tion of the dewpoint, and at both times, there was, I believe, a sudden and considerable increase of the disease both in Albany and Schenectady. As the epidemic, having commenced at different times in these two neighboring cities, was not in the same stage, these simultaneous changes must be attributed to a meteorological influence. If humidity was the cause, humidity as detected by the hygrometer, seemed to have more influence than rain. That the effect depended partly on an electrical change preceding a storm, is not improbable. Finally, there are many agents whose influences both on the consumption of oxygen and the production of cholera are alike unknown. If, as appears from numerous considerations above stated, there is any law connecting these influences, then experiments for determining the effect of these agents on respiration, might lead to important practical applications in relation to the prevention, if not to the treatment of the disease. In applying the results, however, regard should be had to the difference between the immediate and remote effects on respiration; these are frequently opposite.

28. The correspondence between the two classes of facts above-mentioned appears to me interesting, on account of the dependence of animal heat and the florid color of arterial blood on respiration, considered in connexion with the fact that in epidemic cholera, no pathological phenomena are more constant, than a dark color of the blood and a temperature below the normal standard.