The Physics and Physiological Action of Pneumatic Differentiation.

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THE PHYSICS AND PHYSIOLOGICAL ACTION OF PNEUMATIC DIFFERENTIATION.*

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Pneumatic differentiation is the process by which the air surrounding the body and that entering the lungs are rendered of different pressures.

It may be considered under three forms, which, for the sake of convenience of reference, we will designate as positive, negative, and alternate differentiation. The first or positive differentiation is where the air entering the lungs is maintained during both respiratory acts at a greater pressure than that surrounding the body. Negative differentiation is the reverse of this, the air surrounding the body being maintained during both respiratory acts at a greater pressure than that entering the lungs, alternate differentiation being the process by which the two other forms are alternated during respiration; in inspiration the air entering the lungs being of the greater pressure, during expiration that surrounding the body being greater.

The apparatus introduced to the profession by Dr. Williams, and known as the pneumatic cabinet, is the only one,

* Read before the American Climatological Association at its third annual meeting.

so far as I am informed, which enables us readily to apply to a patient any one of these three forms of treatment, and, as the clinical results reported by Dr. Williams and others are sufficiently encouraging to merit attention, it has seemed to me desirable that thought should be given to the subject of the physiological action of these forms of pneumatic treatment.

As the difference in pressure used is very slight, seldom exceeding that indicated by one inch of the mercury column, less than the fluctuation of the barometer from day to day, it is manifest that the actual pressure can be but an insignificant factor in the result, hence is of no importance whether the differentiation is accomplished by changing the pressure of the air breathed, or of that surrounding the body; the important fact being that they are rendered different. This will be referred to again farther on. The pneumatic cabinet acts by changing the pressure of the air about the body, the pressure of the respired air being that of the surrounding atmosphere.

The primary effects of the differential process will undoubtedly be expended upon the organs and functions of respiration and upon the circulation. Let us first consider the effect of what we have agreed to call positive differentiation upon respiration. In normal respiration, owing to the elasticity of the lungs and of the chest-walls, expiration is a passive act—that is, if no force is voluntarily exerted upon the chest-walls, the thorax will assume the position of expiration. In other words, the passive position of the thorax is that of expiration. Inspiration implies an active exertion, and must be accomplished against a certain amount of resistance, one element of which is the pressure of the atmosphere upon the outside of the body. It is obvious, therefore, that, as a portion of this pressure is removed, the passive position of the thorax will approach nearer to that of inspiration. In normal respiration the amount of force exerted by the elasticity of the lungs in full inspiration, according to Foster, is equal to the pressure measured by 30 mm. of mercury. A rarefaction of this amount, then, will just balance the elasticity of the lungs, and allow the thorax to take such a position as the elasticity of the thoracic walls will determine. The effect of a comparative increase of the intra-thoracic pressure will be to distend the air-passages and alveoli of the lungs. If, then, an effort of inspiration is made, the distension of the lungs will exceed that which can be accomplished by the same effort without the aid of the differential pressure, and the expiratory effort will be obstructed to the same degree as the inspiratory effort is aided, consequently the stationary and residual air will be increased. In other words, the subject of the differential pressure will be suffering temporarily from a condition analogous to that of emphysema. This is opposed to what has heretofore been written, all the papers which I have seen upon the subject taking the ground that the residual air is diminished; but this is plainly impossible, as no more than the usual force can be brought to bear in expiration, while more than the usual resistance has to be overcome. This result can be easily confirmed by experiment. A number of persons were subjected to the differential pressure of half an inch, and it was found that the chest girth was, on an average, three eighths of an inch greater upon forced expiration than under normal circumstances. As the residual air is determined by the capacity of the chest in forced expiration, it is evident that, unless the diaphragm rides higher during the differential pressure, the residual air must be augmented. During expiration the diaphragm is a relaxed muscle or a passive membrane, acted upon solely by the pressure above and below it, but, by the conditions of our experiment, the pressure upon its lower surface is less than normal, while that upon its upper surface remains the same; hence it will ride lower than under normal circumstances, and, so far from acting to offset the tendency to greater capacity of the chest caused by the greater chest girth during expiration, it will act to still further augment it, and still further increase the amount of residual air.

A further and, if possible, still more conclusive demonstration of the proposition is the following: A person is subjected to the positive differential pressure of half an inch, and is required to expire as forcibly as possible. The pressure is removed and he is requested to continue the effort of expiration. An additional amount of air is driven from the lungs, which may be conveyed, by means of a rubber tube, to a bell jar inverted over water and measured, and it is found to be equal to about forty cubic inches. This is the amount which the residual air is augmented by the positive differential pressure of half an inch.

The effect of this process upon the respiratory function, therefore, is that all parts of the lungs will be more fully distended, but less than the usual amount of contraction will be allowed, and the increased effort of expiration affords exercise to the muscles of expiration. Excessive or long-continued pressure will undoubtedly tend to cause permanent emphysema.

The effect of negative differentiation will be, in a great measure, the reverse of this. The passive position of the thorax will become one of expiration to a greater degree than is normal; the natural tendency of the lungs to contract by their own elasticity will be aided by the comparatively increased pressure upon the thoracic walls; expiration will be to a greater degree than normal a passive act; and, when aided by a forced respiratory effort, will be more fully accomplished than is possible under ordinary circumstances; hence the amount of the stationary and residual air will be reduced. Inspiration will be rendered more difficult, and can not be carried to the same extent as under normal conditions; the lungs will be contracted and the muscles of inspiration strengthened by exercise.

The effect of alternate differentiation will be a combination of the effects of the two former. Both respiratory acts will be aided or rendered passive, the residual air will be reduced to a minimum, the alveoli contracted to as great an extent as possible during expiration, and fully expanded during inspiration. The lungs will thus be more fully ventilated.

Let us now turn to the consideration of the effects upon the circulation. The cavity of the thorax may be considered as being made up of two parts—one, the alveoli and air-passages communicating freely with the external atmosphere, the other consisting of the heart and intra-thoracic vessels communicating with the general vascular system of the body. The inner surfaces of both these cavities are normally under the same atmospheric pressure, the air space directly, the blood-space indirectly through the blood, which, filling the vessels of the body outside the thorax, is subjected, with the body at large, to the general atmospheric pressure. When the inspiratory effort is made, the cavity of the thorax being enlarged by the action of the diaphragm and other muscles of inspiration, not only does the air enter by reason of the atmospheric pressure, but the blood, urged onward by the same atmospheric pressure, acting upon the outside of the body, is driven into the blood-space of the thorax from the general venous system to a degree varying with the depth of inspiration, and with a force which, in a healthy man in full inspiration, is estimated as equal to that necessary to raise a column of mercury 30 mm. Even in ordinary expiration, owing to the elasticity of the lungs and their consequent tendency to contract away from the thoracic walls, the pressure upon the extra-thoracic portion of the vascular system is greater than upon the intra-thoracic by about the amount necessary to raise a column of mercury 5 mm. This is what is known as the thoracic aspiration of the blood. Now let us examine what effect the use of the cabinet will have upon this, supposing, in the first place, the pressure about the body to be diminished. It is obvious that, if the entire atmospheric pressure were to be removed from the periphery of the body, the intra-pulmonary pressure remaining normal, all the blood in the body would be drawn out of the thorax, or, more properly speaking, would be driven out by the intra-thoracic atmospheric pressure into the extra-thoracic vessels, and the circulation of the blood would cease. Any diminution of the peripheral pressure, the intra-pulmonary pressure remaining the same, would institute a tendency in this direction—that is, a tendency to reduce the blood-flow from the extra-thoracic portion of the vascular system into the thorax, and to increase the blood flow from the thorax into the extra-thoracic portion of the vascular system, thus reducing the amount of blood in the thorax. To put the same proposition in another form. The walls of the intra-thoracic portion of the vascular system are pressed upon centripetally by the direct pressure of the atmosphere. They are pressed upon centrifugally by the indirect atmospheric pressure transmitted by the blood from the periphery of the body. If the pressure upon the periphery of the body is reduced, the transmitted pressure, acting centrifugally upon the walls of the intra-thoracic vessels, will be reduced, but, the direct centripetal pressure remaining the same, they will be reduced in caliber, and the tendency will be to drive the blood out of the thorax, as we have already seen. An additional tendency in the same direction is due to the fact that, in the fuller inspirations accomplished by the use of the cabinet in the manner described, the small vessels of the lung will be more than usually stretched in the direction of their length, thus reducing their caliber and affording more than usual obstruction to the flow of blood through them. The tendency, however, to dam back the blood upon the right heart will be to a great degree obviated by the fact that the damming-back process will commence before the blood reaches the right heart—to wit, in the vena cava and its intra-thoracic branches, where, owing to the greater thinness of their walls, the pressure will be more effectual than in the thick-walled pulmonary arteries.

It remains to be considered what the effect will be upon the arterial blood pressure in the general circulation. The pressure upon the periphery of the body being reduced, as we have seen, less blood will flow into the thorax by the vena cava; consequently less will be delivered to the right heart. The pulmonary vessels being reduced in caliber, both by the comparatively increased centripetal pressure and during inspiration by the longitudinal stretching, still further obstruction will be encountered in the flow of blood to the left heart; and, less blood being delivered to the left ventricle, less can be delivered by it into the aorta. The walls of the veins and capillaries being less rigid than those of the arteries, will feel to a greater extent the effect of the diminished pressure; hence the blood, in passing from the arterial to the venous side of the circulation, will meet with less than the usual amount of resistance. Of the three factors, then, which go to determine the arterial tension, two—namely, the amount of blood delivered by the heart to the arteries, and the amount of resistance offered in the capillaries—will be diminished. The tendency will also be to diminish the third factor, the caliber of the arteries themselves increasing by reason of the reduction of the direct pressure upon them. Unless by some means the vaso-motor system is called into play to offset this tendency, it is evident that the arterial tension will be diminished. The blood, then, passing readily through the arteries and capillaries of the general circulation, will tend to accumulate upon the venous side, from which its flow will be retarded by the increased pressure to which it is subjected as it enters the thorax. The tendency, therefore, of positive differentiation will be toward the exsanguination of the lungs, and consequent relief of local plethora and congestion when they exist, and the lowering of the arterial pressure of the general circulation. Repeated experiments have shown the arterial tension to be reduced, as judged both by the finger upon the pulse and by the sphygmograph. In the case of one gentleman in fairly good health, but who suffers occasionally from slight functional disturbance of the heart, the pulse became intermittent under the differential pressure of half an inch, owing, as I suppose, to the reduced amount of blood delivered to the left ventricle.

It is sufficiently obvious that negative differentiation will have the opposite effect. The atmospheric pressure upon the periphery of the body urging the blood onward, not being counterbalanced by as great an atmospheric pressure within the thorax, will tend to an increased flow toward the thorax, and the increased pressure to which it is subjected as it passes from the thorax into the remaining parts of the body will tend to retard its passage thence; the lungs will, therefore, be subjected to a local plethora, and the tendency will be to congestions and hæmorrhages. The arterial tension in the general circulation will be raised, because, more blood being delivered to the left ventricle, more will be sent by it through the aorta into the general circulation, where a further tendency, acting to raise the arterial tension, will be encountered in the increased pressure to which the capillaries and veins are subjected.

During ordinary respiration, an aspiratory force is alternately exerted and suspended by the movements of inspiration and expiration respectively. Under the influence of alternate differentiation, during inspiration the peripheral air-pressure is diminished; hence the tendency of the blood toward the thorax is diminished; during expiration, on the other hand, the peripheral air-pressure is increased; therefore the tendency of the blood from the thorax is checked. Thus we see the effect of alternate differentiation will be to partially obliterate the undulations in the blood-pressure which normally exist consequent upon thoracic aspiration.

In considering the phenomena connected with the circulation, we have been obliged to confine ourselves principally to the consideration of mechanical effects, disregarding in a great measure the influence of the nervous system. This is due to the inherent difficulties in dealing with the intricate problems which the latter presents; but, the action of the agents which we are considering being almost purely mechanical, it is fair to presume that their effects will be primarily mechanical, and, at all events, if our reasoning is correct, it at least shows the tendencies of these processes, and, so far as experiment is applicable to the subject, it shows our theories to be in accordance with the actual results.

Two statements are put forward by Dr. Williams and Mr. Ketchum of which I wish to speak, because I believe that they have done much to confuse the subject. The first is one especially dwelt upon in Mr. Ketchum's paper *—to wit, that, in some way which he fails to make clear, the effect of withdrawing a small amount of atmospheric pressure from the periphery of the body is radically different from that of adding an equal amount of pressure to the air communicating with the interior of the lungs, the former acting to expand the chest by a *vis a fronte*, the latter by a *vis a*

* "The Physics of Pneumatic Differentiation," "Medical Record," January 9, 1886.

tergo. A very brief examination of the subject will, I think, suffice to convince us of the error of this view. Under ordinary conditions of air-pressure, the thorax being at rest, the pressure about the body and within the lungs is exactly equal—namely, that of the general atmosphere. When an inspiratory effort is made, the thorax is enlarged; consequently a tendency to rarefaction is produced in the lungs, and air simultaneously passes in to equalize the pressure. On the other hand, when the thorax is contracted, a tendency to compression of the contained air is produced, and air passes out to equalize the pressure. If, however, the pressure of the air communicating with that within the thorax is rendered greater than that of the air surrounding the body, a new element is introduced, for the pressure of the air within the chest, not being fully balanced by the pressure on the outside, tends to expand it by pressing its walls outward, and it seems perfectly evident that it can make no difference whatever whether the inequality in pressure is produced by increasing the pressure of the air which enters the lungs, or by reducing that without. In either case the expansive force is the pressure of the air within the lungs. For, suppose the barometer to-day to stand at twenty-nine inches, and the respired air to be compressed to such an extent that it will raise the mercury column thirty inches. To-morrow the barometer may stand at thirty inches, and we will rarefy the air about the body until it will raise the column but twenty-nine inches. In both cases the pressure within the lungs will be the same—namely, the normal atmospheric pressure with the barometer at thirty inches; in both cases the pressure about the body will be the same—namely, one half pound to the square inch less. It is impossible to see where the conditions differ. That the thoracic walls can be sucked or drawn out by a force acting in front seems absurd when we remember that suction is merely the removing of a portion of the atmospheric pressure from one side of a thing and allowing the full pressure to exert itself upon the other; that it is not a *vis a fronte*, but a *vis a tergo*. To maintain that suction is a force acting directly to draw bodies after it, is, as a scientific proposition, on a par with the statement that nature abhors a vacuum. To put the matter beyond a doubt, I have reversed the breathing-tube of the cabinet, placing the subject of the experiment upon the outside, and compressed the air within the cabinet. The effects produced upon the residual air and upon the pulse, as well as the subjective experience of the person operated upon, were found to be identical with those obtained when he was within the cabinet and the pressure reduced to the same degree.

The other statement referred to is that a most important, if not the main, effect of the positive differential process is connected with the use of medicated sprays or vapors introduced into the air-passages with the respired air, the spray or vapor, as Dr. Williams and Mr. Ketchum contend, being carried to the alveoli of the lungs during inspiration, and there deposited by the compression of the air caused by the commencement of the expiratory act. It is maintained by Dr. Williams that in this manner the tubercular disease can be reached and treated locally by the deposition upon the diseased lung-tissue of an antiseptic fluid. I believe this view to be wholly unwarranted. Waiving the question whether a sufficient amount of an antiseptic introduced into the lungs to disinfect them would not prove extremely injurious or even fatal to the patient, let us consider whether its introduction can be effected if desirable. It is well known that under ordinary conditions it is extremely difficult to introduce spray to any great depth into the air-passages, for the reason that it tends to be deposited upon the walls of the pharynx and in the larynx and trachea. If it should reach the bronchi, the cilia, whose express office it is to prevent the ingress of foreign material to the lungs, would most effectually bar its farther progress. Dr. Williams maintains that by means of the positive differential process—that is, the form in which the air surrounding the body is of a less pressure than that entering the lungs—the tidal air and vital capacity are increased and the residual air diminished, and that therefore the spray will be carried *farther into the air-passages.* But we found the reverse to be the case; the residual air is increased owing to increased difficulty of expiration. Hence the spray would be carried into the air-passages to a less depth than under normal circumstances. The arguments offered to prove that the spray or vapor is condensed upon the walls of the air-passages to any greater extent than usual seem to me to be based upon manifest errors. In the first place, it is necessary to distinguish between sprays and vapors, which Dr. Williams and Mr. Ketchum do not. A spray is a liquid in a state of such minute subdivision that it will float for a short time in the air. A vapor is the gaseous form of a substance which, at the ordinary temperatures and pressures, is either liquid or solid. One is governed by the laws of liquids, the other by the laws of gases. As the gentlemen have described the process used by them, they employ a simple spray formed by a rapid current of air passing over the mouth of a tube filled with liquid. So far as this is concerned, then, the laws of vapors have no application to the subject. It is conceivable, however, and no doubt in a measure true, that, if a solution of a volatile substance is passed in the form of spray into an atmosphere warmed to the degree of the upper air-passages, a portion of that substance will be vaporized, and we shall have a true vapor to deal with. Furthermore they can, of course, if they see fit, vaporize the substance in the first place before introducing it, so that we shall be obliged to consider the action both of sprays and of vapors. The word condensation as applied to sprays is simply a misuse of terms. Condensation means passing from the gaseous state to the liquid, and a spray can not condense, simply because it is already liquid. It can, however, be deposited upon surfaces with which it comes in contact, simply by reason of its mechanical adhesion for them. This mechanical adhesion is not altered by differences in the atmospheric pressure, and, as has already been shown, under the influence of the process under consideration, the spray carrying inspired air penetrates the air-passages to a less depth than under normal conditions, and the deposition of the spray upon their walls will be lessened rather than increased. With a vapor the case is different. The inspired air being charged with a vapor, the vapor will to some extent pass below the level of the inspired air by the law of the diffusion of gases. This will be true under any circumstances, and it is not alleged, neither is it possible, that the differential process can make any difference with this diffusion; the only effect it will have will be that, the inspired air not passing so far into the air-passages, less vapor will be carried in. Now, then, is it true that, under a comparatively increased pressure of the respired air, the vapor will be condensed to a greater extent than under other circumstances? To determine this point we must take into consideration the laws governing the condensation of vapors. They are simply stated in Ganot's "Physics": "Condensation of vapors may be due to three causes—cooling, compression, or chemical affinity. For the first two causes the vapors must be saturated." That is to say, they must be in a confined space, and the space must contain so much of the vapor that it can hold no more without becoming liquid.

Now, it is clearly impossible that the space of the air-passages could be brought under the conditions necessary to produce condensation of a medicinal substance by either cold or compression; first, because it is not a confined space, and, secondly, because no such amount of the vapor of an irritating substance as would be necessary to produce saturation could be introduced without suffocating the patient. It is true that this space is saturated or very nearly saturated with the vapor of water, and it is upon this fact that Mr. Ketchum seems to base his argument; but, even if condensation of a portion of this vapor were to be accomplished, it would have no effect upon the vapor of the medicinal substance, for the laws of condensation act upon each vapor independently of the presence or absence of other vapors, and, in order that the vapor of the medicament should be condensed by either cold or compression, the space must be saturated with that particular vapor.

Even if it were possible that the air-space could be so saturated, it is impossible that compression could be applied by means of Mr. Ketchum's device, and of course cold is out of the question. All the compression which is applied to the air at the commencement of expiration is the slight amount necessary to overcome the resistance offered by the friction of the bronchial tubes and the narrow opening of the glottis, and, as neither of these can be affected by the differential pressure, that process can not change the result. Mr. Ketchum, in his paper on "The Physics of Pneumatic Differentiation," * states that "manometric tests show that in respiration, under the circumstances stated, the expiratory effort is equal to a difference of 4·5 inches of the manometer." It would have been interesting if he had stated how he applied the manometer, as, in order to register the compression produced by the resistance of bronchial friction, it must have been in direct communication with the air below

* "Med. Rec.," January 9, 1886.

the bronchial tubes. Of course the result is an error, for the extreme force of a violent expiratory effort in a strong man is only sufficient to raise the manometric column four inches.* If, however, the compression of the air in the lungs were of any value, it could be produced easily enough without any apparatus at all simply by making a strong expiratory effort with the mouth and nose closed.

The first two causes of condensation, then, being excluded, it is evident that such condensation as does take place must be referred to the third cause—that of any chemical affinity the vapor may have for the mucous membrane or the fluid bathing it, including under that term that loose form of affinity which results in a solution. That some condensation does result from this cause is conceded, but that it is affected one way or the other by the differential pressure is neither claimed nor is it possible.

Dr. Williams, however, professes to have demonstrated by experiment that his medicinal substances are in some manner deposited in the alveoli. The experiments reported are: First, cinchonism has been produced by inhaling quinine. Second, symptoms of mercury poisoning have resulted from inhaling the spray of a solution of mercuric bichloride. Third, in a patient with an intercostal fistula, iodine and mercury were detected in the discharge therefrom, after the use of sprays containing them. Fourth, tracheotomy was performed upon a rabbit, and a cannula introduced which was connected with the breathing tube of the cabinet; air-pressure to the extent of nine tenths of an inch was removed from about the animal's body, and a mixture of China ink and water was sprayed into the tube for ten minutes. The autopsy showed pigmentation in the alveoli. In regard to the first and second experiments, some persons are very susceptible to the action both of quinine and of

* Foster's "Physiol."

mercury; enough of the drugs to produce the symptoms may have been absorbed from the mouth, pharynx, and upper air-passages, besides which there is nothing to show that a portion of the respective drugs was not accidentally swallowed. In regard to the third experiment, it is very probable that the suppurating cavity communicated directly with a bronchus. Of the fourth experiment, it seems enough to say that the introduction of a spray into a tracheotomy tube in the throat of a rabbit is a very different matter from its introduction into the mouth of a man, to say nothing of the fact that the pressure used with the rabbit was so great that it was necessary to resort to artificial respiration to enable it to expire against the pressure, a condition totally different from any we would be apt to use with a human being.

To sum up, then, we have found that, by means of the differential process in its three forms, we can increase or diminish the difficulty of expiration or of inspiration; we can increase or diminish the tidal air, the vital capacity, the stationary air, and the residual air. We can to some extent control the amount of blood in the lungs, and consequently control pulmonary congestion and hæmorrhage, and we can raise or lower the arterial blood-pressure. It is hardly possible that such wide-spread phenomena can be induced without producing other and secondary results through the influence of the nervous system and otherwise.

The result of my experience and study has been to convince me that a large share of the benefit derived from the use of the cabinet is due to the reduction of congestion, and consequently of inflammation, in the diseased lung by the differential pressure, in very much the same way as a bandage will afford relief to an inflamed joint. In addition to this, undoubtedly the increased expansion to which the lungs are subjected, and the passive exercise which they are afforded, will do much to modify their nutrition and increase their vitality. The subject is worthy of more thorough and systematic study than it has yet received, which study will, I am sure, convince the profession, if this effort shall have failed to do so, that the theories of Dr. Williams and Mr. Ketchum are erroneous, and that the value of the pneumatic process must rest upon other grounds; in saying which I do not wish to be understood to disparage the apparatus which those gentlemen have placed before the profession—which I believe, if understandingly used, to be of great value—but only to urge the importance of placing the matter upon its true foundation.

In conclusion, I wish to express my acknowledgments to my friends Dr. B. F. Westbrook and John A. Barrett, Esq., of Brooklyn, for valuable aid in the preparation of this paper.