

BOSWORTH (F.H.) et al

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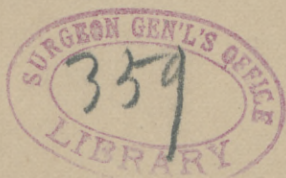
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ON THE RELATION OF
THE NASAL AND NEUROTIC FACTORS IN
THE ÆTIOLOGY OF ASTHMA.*

BY F. H. BOSWORTH, M. D.

THE first question that arises in the discussion of this topic is, What is the true pathological lesion which gives rise to a paroxysm of asthma? From the earliest days of medical literature up to comparatively recent times we find all authorities accepting the theory that a paroxysm of asthma is due to a spasmodic contraction of the small circular muscular fibers which have been demonstrated by anatomists to exist in the bronchial tubes even down to their smallest ramifications. It seems rather curious that this theory should not long ago have been questioned. All observers very properly recognize asthma as a neurotic disease, and muscular spasm is undoubtedly a manifestation of the neurotic temperament, and apparently on this trivial ground the theory has been accepted. If the paroxysm is due to muscular spasm, why should the attack occur, in most instances, only at night? Why should it be affected by changes in temperature? Why should relief be obtained by high altitudes?

* Read before the American Climatological Association at its fifth annual meeting, September, 1888.



Why should an attack be aggravated by a damp, bad atmosphere? There is everything in the repose of a quiet sleep which should protect one from an attack of asthma, and yet a paroxysm almost invariably comes on during the night. Changes in temperature are not usually recognized as having any marked effect on the nervous system, and yet a change in climate not infrequently precipitates a seizure of asthma. The sea-shore rather than mountainous districts is favorable to the toning up, as it is called, of the nervous system, and therefore asthmatics should suffer least at the sea-shore on this theory; yet the converse is true. Again, a paroxysm of asthma is ushered in suddenly, and during its persistence is characterized, it is true, by symptoms which are easily explained upon the theory of spasmodic muscular contraction of the circular fibers of the bronchial tubes, but the culmination of the attack is invariably marked by a more or less profuse sero-mucous exudation with cough and expectoration. This in no way can be harmonized with the spasmodic theory. I think, then, this view must be abandoned, as failing to explain the clinical history of asthma or the clinical history of a paroxysm. The first to call in question this spasmodic theory was Webber,* who demonstrated conclusively, to my mind, that an asthmatic paroxysm was due not to muscular spasm but to a paralysis of the vaso-motor nerves regulating the caliber of the blood-vessels of the bronchial mucous membrane. Under the influence of this vaso-motor paralysis coming on suddenly the blood-vessels in the bronchial membrane assume a sudden turgescence, under which the membrane becomes markedly swollen and congested, thereby encroaching notably upon the caliber of the bronchial tubes, constituting really the first or dry stage of inflammation, although differing from inflammation in that there is no tendency what-

* "Tageblatt der 45te Naturversammlung zu Leipzig," p. 159, 1872.

ever to the escape of leucocytes or the development of the second inflammatory stage. The membrane is swollen, dry, and non-secretive. The result of this swollen condition is to interfere with not only the entrance of air to the air-cells, but also to its escape, giving rise thus to dyspnœa, both in inspiration and expiration. This condition, persisting for a certain length of time, somewhat precipitately gives way in connection with a more or less profuse exosmosis of serum, and the attack subsides. In this theory of Webber's we have one which very completely harmonizes not only with the clinical history of asthma, but with the clinical history of the paroxysm. Furthermore, it recognizes the view that the disease is essentially a neurosis. Medical science has not yet reached the stage where it can tell us in what neurosis consists, or what essential pathological lesion constitutes the nervous habit. That it is dependent on some structural change, I think, can not be questioned, as all so-called functional disorders are, the term "functional" being simply one which we use to cloak our ignorance of true pathological conditions. If, however, there is any morbid condition which constitutes the individual a neurasthenic, it is vaso-motor paresis involving some region of the body. Thus, in what we formerly called spinal irritation, it has been demonstrated that the backache and other symptoms characteristic of the disease are due to weakness of the vaso-motor control of the blood-vessels of certain portions of the spinal cord. In the same way, in the disease we call asthma, the essential pathological lesion consists in a weakness of vaso-motor control of the blood-vessels of the mucous membrane of the bronchial tubes. If, then, Webber's theory of spasmodic asthma be accepted, as I think it must be, we have gone far toward understanding why there should be a close ætiological relation between this affection and a diseased condition of the nasal mucous membrane.

In a former paper read before this association* in 1885 I discussed quite fully the true physiological function of the nasal mucous membrane, and the intimate connection between that and the bronchial membrane. I need, therefore, but briefly allude here to the views then advanced. The great and important function of the nasal mucous membrane is exosmosis of serum, by which the inspired current of air shall be so far saturated with moisture as that in passing through the bronchial tubes the mucous membrane lining them shall in no way be disturbed by the absorption of moisture by the respiratory current. Furthermore, this exosmotic function is regulated with an extreme degree of nicety through vaso-motor control in such a way that the serous exudation shall adapt itself to the various changes of temperature and humidity of the respired air. There is thus established a very close and exceedingly intimate connection between the membrane of the one region and that of the other. I think that much in the same way as the respiratory movements of the thoracic walls are regulated by the so-called *besoin de respirer*, and the demand for oxygen on the part of the blood controls the rate and rhythm of these movements closely and accurately, in just the same way the serous exosmosis from the nasal mucous membrane is regulated to adapt itself to the condition and demands of the bronchial membrane. The blood-supply of the two regions is thus brought into very close and intimate relation, and is virtually under the control of the same system of nerves, so far does this sympathy between the two regions go. Very close observation can easily demonstrate that a plethora of the blood-vessels of the nasal mucous membrane tends to develop a like plethora in the bronchial mucous membrane, and, furthermore, that, if we produce an anæmic

* "Hay-fever and Asthma and Allied Affections," "New York Medical Journal," April 24, May 1, 1886.

condition of the turbinated tissues, an anæmic condition of the bronchial mucous membrane tends to follow. Given now still a third condition—that of vaso-motor paresis of the blood-vessels of the nasal membrane—the tendency is most marked to the development of a like weakness in the bronchial membrane. This is strikingly illustrated in cases of hay-asthma, which differs in no essential degree from ordinary spasmodic asthma except in the exciting cause of the paroxysm. The clinical history of every case of hay-asthma shows that the hay-fever attack precedes it invariably from one to ten days, the vaso-motor paresis of the blood-vessels of the nasal mucous membrane constituting hay-fever, being followed by the same pathological lesion in the bronchial mucous membrane, constituting an attack of asthma. This connection being established, it only remains to establish a connection between perennial asthma, or that form of asthma which is not connected with hay-fever, and the ordinary obstructive lesions in the nasal chambers. In a recent paper* I made an analysis of eighty of the cases of asthma taken from my own note-books which had been under treatment and in which the records were complete. This analysis demonstrated several points which are exceedingly pertinent to the subject under discussion. With reference to the existence of intranasal disease, such was found to be present in every case in sufficient degree to warrant the conclusion that it exercised a powerful causative influence in the production of the asthmatic attacks. This conclusion of course can easily be questioned, but I think the proof that my view was correct is shown in the fact that of these eighty cases forty-six were cured, twenty-six were improved, three unimproved, and five unknown, and this entirely as the result of treatment of the nasal disorder. The inference, I think fur-

* "Asthma, with an Analysis of Eighty Cases," etc., "American Journal of the Medical Sciences," September, 1888.

thermore, can safely be drawn that, if the attack of asthma is cured by curing the nasal disorder, the asthma was caused by the nasal disorder. Now, I do not mean to say that nasal disorder is the sole and only cause of every case of asthma. I think no one will dispute the powerful influence exerted by a neurotic habit, which all recognize as present in every case. Let us see what the conditions are which give rise to an asthmatic paroxysm. I think we shall find them covered by the three following heads :

1. The neurotic habit, as best shown in Salter's exhaustive work.

2. A diseased condition of the nasal mucous membrane, as shown by Daly,* this consisting simply of some obstructive lesion giving rise to a vaso-motor weakness of the bronchial mucous membrane through the intimate sympathy between the two regions already alluded to.

3. Some peculiar atmospheric condition which has never yet been determined, but which precipitates the paroxysm.

The first of these causes is recognized by all. On the second cause I have already written in the paper alluded to at sufficient length. I think the existence of the third cause must be accepted on clinical ground, for, as we know, the paroxysm is dependent on seasonal and diurnal conditions, probably atmospheric. Sir Andrew Clark,† in 1886, advanced the doctrine that hay-fever and asthma were virtually one and the same disease, a view which I had already advocated in my paper read before this association in May, 1885, the one disease being vaso-motor bronchitis, while hay-fever was a vaso-motor rhinitis; in the one case the hay-fever attack being produced, first, by a neurotic condition; second, a diseased condition of the nasal membrane; and third, the presence of pollen in the atmosphere. The

* "Arch. of Laryngol.," iii, p. 157.

† "International Journal of the Medical Sciences," January, 1886.

atmospheric condition alluded to above as the third cause of the asthmatic paroxysm is closely analogous to the pollen-laden atmosphere which produces hay-fever; but whereas in one case the pollen theory has been absolutely demonstrated by Blackley's incomparable experiments,* the peculiar atmospheric conditions which give rise to asthma have not been determined. Of course it is not safe to make the statement that every individual of neurotic habit who suffers from obstructive lesion in the nose will necessarily have asthma, and yet I think the conviction would force itself strongly upon any one who was brought largely in connection with cases of this sort that, with these two conditions present, we have elements strikingly favorable to the development of spasmodic asthma, and no slight probability of such development occurring. There is one statement, I think, that should be made in this connection, and that is that I think many cases are overlooked in a mistaken diagnosis of bronchial congestion, slight cold, incipient bronchitis, etc. Asthma is a far more frequent disease, I think, than is generally recognized, and I am very confident that many cases in the earlier stages are overlooked and the diagnosis not made until a well-marked paroxysm sets in with its attendant suffering and distress. Going back to the original topic of discussion suggested in the programme as to what relation the neurotic bears to the nasal element in causing asthma, the conclusion I have drawn I hope is sufficiently clear. Both elements constitute powerful predisposing causes of the disease, and furthermore, from my point of view, a case of asthma in which they are both not present is exceedingly rare. But when we come to consider the comparative relation of the two, I think the importance of the nasal element must be conceded when we consider the question of successful treatment. And this, I think it must be

* "Hay Fever," London, 1873.

conceded, is accomplished by the removal of any one of the three causative factors—the neurotic habit, the atmospheric condition, and the nasal disorder. Now, the treatment of asthma by general medication—or, in other words, the cure of this disease by therapeutic remedies directed to the control of the neurotic habit—does not present a brilliant chapter in medical literature. In other words, we possess no specific remedy for the disease. The nearest approach to this, I think, all will recognize in iodide of potassium, and yet, if we closely examine our note-books as to the permanently beneficial effects of this drug, our success has not been brilliant. Much less can be said of the use of belladonna, zinc, arsenic, and other so-called nervines. As regards the removal of the atmospheric condition, this consists simply in a change of residence; this, I take it, we all recognize as a temporary measure—the attacks recurring as soon as the patient returns to his former unfavorable atmospheric surroundings.

Turning, however, to that system of treatment which is based on the cure of the nasal disorder, I think I need but refer again to the statistics of my own eighty cases already quoted, which, if correctly compiled—and I certainly made every endeavor that they should be recorded not only with accuracy but absolute candor—the question is sufficiently answered. Basing, then, the question of the comparative importance of these two factors in asthma on perhaps the most important element in the consideration of the disease—viz., its successful treatment—I think it may be fairly claimed that of the three elements which enter into the causation of asthma—namely, a neurotic habit, the nasal disease, and atmospheric conditions—the nasal disorder far outweighs in importance not only the nervous but all other elements of causation.

Remarks by

E. L. SHURLY, M. D.,

DETROIT.

IN order to divest our subject of all ambiguity, let us establish our first standpoint upon the simple classification of *primary* or *essential asthma* and *secondary* or *symptomatic asthma*. For under one or other of these heads all cases, acute or chronic, may be classified which present that peculiar dyspnœa to which the name *properly* belongs. As we are aware, the ætiology of this peculiar dyspnœa has all along been enshrouded in mystery, especially of those cases which have appeared to show little if any structural change, and those which have been marked by a peculiar periodicity, known as hay-asthma; for the elucidation of its nature, therefore, various theories have been advanced, prominent among which are the *hæmic*, *inflammatory*, *Leyden's crystal*, *spasmodic*, *vaso-motor*, *peptic*, *nasal*, and other perhaps less significant ones.

It will be unnecessary, however, for the purposes of this discussion, to notice them historically and in detail; but I must call your attention particularly to the "spasmodic," "vaso-motor," and "nasal," the latter of which has been so ably presented to the profession of late by Dr. Daly, Dr. Roe, Dr. Bosworth, Dr. Ingals, Dr. Mackenzie, Dr. Sajous, and others.

Not fully convinced that the more recent and now prevalent ideas regarding the nature of the disease were rationally consistent with clinical observation, I instituted in the physiological laboratory of the Detroit College of Medicine a series of experiments upon dogs, and elicited the following information, according to the observation of myself and those who were present: Carefully dissecting out and

isolating a portion of the pneumogastric nerve in the neck, a finely pointed electrode attached to a faradaic battery was applied, with the result of producing marked contractions of the trachea, bronchial tubes, and diaphragm; the contractions were both circular and longitudinal, effecting an obvious shortening of the air-passages; in fact, always resulting in a squeezing, so to speak, of the pulmonary organs from below upward. With the fingers could be *felt*, during the time of shortening, little rugæ, especially along the trachea. Upon laying open the trachea down to the bronchi, we could *see* the mucous membrane assuming small folds not unlike the *valvulæ conniventes*.

For a while after the application of the stimulus, respiration seemed to become almost entirely suspended, but soon Nature, operating against this continued danger, would succeed in relaxing the diaphragm, a little at first, and then the bronchial muscles, until a slight inspiratory effort took place. And so on, despite the great contractile opposition maintained, a feeble but sufficient respiration was carried on, thus illustrating as plainly as could be the phenomena of an asthmatic paroxysm. With a feebler current of stimulation, the action was more plainly visible.

These experiments were continued for about half an hour upon each animal, with the same physical effect in each case, and without producing any *sensible engorgement* of the mucous membrane of the *bronchial* or *nasal passages*, or any other result indicating vaso-motor paresis.

These experiments illustrated as plainly as could be the respiratory phenomena of ordinary asthma, and I may state, however, that the same effects did not follow the stimulation of the carotid plexus, although our experiments in this particular were not satisfactorily made.

The next series of experiments consisted of exposing one of the *spheno-palatine ganglia*, which in a dog is diffi-

cult to do; in fact, I should not have succeeded but for the able assistance of our demonstrator of anatomy, Dr. J. B. Wright, who is also a graduate of a veterinary college. This ganglion (which, it will be remembered, presides over the innervation of the posterior portion of the nasal passages) was stimulated as in the other experiments, very carefully avoiding contact with muscular tissue, with the effect of producing contraction of the nasal, palatine, and upper constrictor muscles—in short, producing the phenomenon of sneezing, but no contraction of the bronchioles at all. Under a very powerful stimulus the extrinsic muscles of the larynx would occasionally twitch. Mechanical irritation, such as pinching of the ganglion, produced only slight contractions of the palatine and nasal muscles.

Following these experiments, we quickly opened the nasal cavities and found only *very slight*, if any, engorgement, no swelling of the turbinated bodies, and no excessive secretion. I am aware that a few experiments of this sort have been made in Europe and results published contrary to those obtained by me.

With other dogs I applied to the nasal mucous membrane, besides electricity, powdered sanguinaria, or an abundance of hay-seed containing rag-weed probably, two of them being kept in an atmosphere containing more or less hay-seed for ten days, but without the induction of asthma, although in one dog there was an abundant nasal secretion. I also injected one animal, beneath the mucous membrane of the inferior turbinated bone, with an eighth of a grain of pilocarpine nitrate, but without the effect of producing anything save a slight local irritation and some constitutional debility.

It is therefore impossible for me, in the light of physiological experiments, which I think were conducted carefully, to discard the so-called "spasmodic" theory for the

more recent one of *vaso-motor spasm* and consequent *blood engorgement*.

If the theory of vaso-motor paresis (advanced by Webber and advocated by Dr. Bosworth, in a recent able article upon asthma in the "American Journal of the Medical Sciences") accounts for the paroxysm of asthma, then, under the influence of an excitant producing such a condition, we ought to find at least considerable engorgement of the vessels within half an hour; whereas, in the experiments stated above, no such result followed.

Dr. Bosworth also objects to the term "reflex" in this connection, believing the term constitutes a mantle for our ignorance. While it is undoubtedly true that the term "reflex" is often misappropriated, its use is perfectly justifiable in its application to the disease under consideration, because we seldom know exactly what the *modus operandi* of the cause or causes leading to the paroxysm is.

However, assuming—because generally admitted even by Dr. Bosworth as a fact—the existence of a neurotic predisposition or habit (hereditary or acquired) underlying all cases of asthma, it will be unnecessary to discuss further whether it be a reflex or direct irritation that precipitates the paroxysm, only that asthma, properly speaking, is essentially a *spasmodic* disturbance of the bronchial nervo-musculature, independent in part of vaso-motor disturbance, and this, it seems to me, can be fully demonstrated.

Coming to the question of *intranasal disease* as the foundation of asthma and also hay-fever, I must take issue with the radical position held by Dr. Bosworth and others. In the first place, I must object to the wholesale condemnation of the noses of asthmatics which seems to prevail, for I believe with Mackenzie, of London, that there is no invariable standard of normality for nasal chambers any more than for arms, legs, eyes, or ears. What may seem a

normal condition in one individual may be abnormal in another. When the nasal passages are open enough to admit of easy respiration through them, vascular enough to warm the inspired air, and bathed with secretion or moisture in proper quantity and quality for such a physiological condition as to give the owner no thought or consciousness of possessing a nose, such passages are practically healthy, whether there be slight deflection of the septum or not, or whether a turbinated body presents the orthodox curves, position, and size or not.

I have been examining nasal passages more or less for the past eighteen years, and I can not say now that I have seen many typically symmetrical examples of Nature's art in this direction; yet I should be amazed beyond recovery to become possessed of the conviction that in all this time I had seen but few healthy nasal passages, and really *none* possessed by asthmatics. Within the past two or three years I have taken pains to examine the nasal passages of many asthmatics, both in hospital and private practice, without observing even a majority with what I regard as diseased conditions of the nasal passages, while the "sensitive area" promulgated by my friend Dr. J. N. Mackenzie has been also wanting in most of those coming under my observation. On this point I am convinced that its occasional occurrence is due to the fact that the nasal mucous membrane of some persons is more sensitive than others either throughout or in part—analogueous to similar conditions of mucous membranes of other parts, such as the rectum, vagina, and urethra; that it is generally met with in the noses of asthmatics or other persons is contrary to my experience.

That *intranasal disease* is a factor in the production, or rather prolongation, of some cases of asthma must be admitted, but that it is often the foundation is contrary to my observation.

I am sure that daily experience overwhelmingly shows a large number of cases of intranasal disease, such as deflections, neoplasms, and hypertrophies, in persons of neurotic habit, with a very small proportion of cases of associated asthma, and the same is nearly true regarding "hay-asthma"; although we must not forget that "hay-fever" and "essential asthma" are not one and the same disease, for the one is a peculiar constitutional disturbance characterized by a specific rhinitis allied to influenza, while the other is a neurotic disease of the respiratory apparatus, non-inflammatory ordinarily—at least at first—and not accompanied by coryza or rhinitis necessarily. The two diseases are undoubtedly allied, and sometimes coexist; there may be hay-fever with or without asthma, and asthma independent of rhinitis or hay-fever.

Now, if asthma depended upon intranasal disease almost wholly, we should find: 1. Nearly all cases of asthma presenting intranasal structural disease, and accompanied by rhinitis at first or during its duration. 2. We should observe asthma in a large majority of the cases of intranasal structural disease or neoplasms. And 3. We should be able to produce asthma by experimental or accidental irritation of the nasal mucous membrane at any season in individuals possessing the "peculiar neurotic habit."

Lastly, regarding the local treatment of nasal cavities with the *galvano-cautery*, other *caustics*, and *surgical manipulation* for the relief of asthma, I would say that although the valuable statistical article of Dr. Bosworth shows such really good results, I would, however, respectfully suggest that the real therapeutic value of such measures probably lies more in the therapeutic principle of *counter-irritation* than in the destruction of the causal nidus, for the following reasons: 1. Blistering and other counter-irritation over the neck and chest (though an old remedy) is

one which often has given as good results as the galvano-cautery, especially in my hands; this and other seasons I have treated some cases of hay-fever in this way, and also by the application of tincture of iodine (as suggested by Dr. Faulkner, of Alleghany City), with even better results than I have had with the galvano-cautery or saw. 2. The idea of destroying all of the peripheral nerve-ends without wide and absurd destruction of intranasal tissue seems impossible, owing to the anatomical structure of the parts; for how else can be reached the remote and hidden mucous surfaces of the nasal passages, which contain by far more of the sensory filaments than the parts which can be brought in view? I must confess that the results obtained by cauterization of the mucous surface have not been permanent in my hands except in two or three instances, which may be possibly due to lack of skill or perseverance. At the same time, when there are neoplasms or hypertrophies which really obstruct the nasal passages, the galvano-cautery or some such means ought to be employed.

Remarks by

W. H. DALY, M. D.,
PITTSBURGH, PA.

I MAY say, with no concealment of pride, that it has given me great pleasure to view the mass of valuable and pertinent work all over the civilized world that has followed my rather unpretentious record of observations, read before the Congress of the American Laryngological Association in New York in 1881, upon the subject of hay-asthma in its relation to nasal and neurotic factors. That paper seems to have marked a most important epoch in the study of this subject, for shortly thereafter followed Hack's valuable work, and J. N. Mackenzie, Roe, Bosworth, Sajous, Allen, and a host of others brought forward their experience—

all of them more or less verifying the observations I had published, and many of them enlarging and strengthening the views I there promulgated. Some of them, however, possibly went further than I was then prepared to go. But I desire it understood that I am now prepared to go further than I was when I made my original observations. And I am also, as I was then, prepared to concede to others that they are looking into the same truth from a different standpoint, and that I believe they are right, like myself, so far as I go into the complicated and intricate question of asthma and the nasal factors thereof.

Now as to Dr. Shurly's experiments. I have always valued experimental research as being entertaining and instructive. It is, however, subject to being misinterpreted by us, and if so, then it is misleading to a serious degree. I know it has the air of precision about it to look at results that have been arrived at after a careful summing up of observations made upon vivisections of the lower animals; but, as a matter of fact, the suddenly produced objective results on the lower animals are pathologically a vastly different thing from the subjective results of years of complicated diseased action in some of our cases of asthma in the human being. In the latter the nerve factor may be that which is most manifest. But is it *the* factor or the only one through which the medical man must view the pathological process of asthma? I think it ought not to be considered the only one.

Now, gentlemen, we have much yet to learn upon this subject, and I do not believe any of us have ever told the whole truth about asthma, or hay-asthma either. I apprehend the truth is so big that none of us small mortals can take it all in, even in one of our most comprehensive views. We are, to a certain degree, like the three blind men in the wise fable of Tolstoi. The blind men came to see the elephant

after their manner. They felt of the elephant, received their impressions of what it was like, and went away, each with a very definite notion of the appearance of an elephant. But their ideas differed widely. "An elephant," said one of the blind men, "is a kind of whip." He had felt of the tail. "No," said another blind man, "an elephant is a kind of horn." He had felt of the tusks. "No, no," cried another, "an elephant is a great round creature like a ball, and covered with rough skin." He had felt of the side. Each man had touched one little part of the big elephant, and thought that part was the whole. Each man was right so far as he went, and from his point of view; he began to be wrong the moment he insisted he had gone as far as anybody could go, and knew the whole of it. He began to be not only wrong but intolerant the moment he declared that his fellow, because he differed from him, must in the nature of things be either deceiving or deceived. These men forgot, or did not know, that an elephant was big. That is what was the matter with them. Is it not also the matter with us?

Now, gentlemen, do we not often forget that our truths are big and not easily reached? Do we not oftener spend valuable time trying to prove the fallacy of what another has honestly and correctly observed upon the subject of a big truth rather than strike out into a new line of observation and add more that is authentic regarding the same truth? I think we too often do this.

I am reminded of the occasion of the first day's meeting in the throat section of the great International Medical Congress at Copenhagen, in 1884, when much valuable time was taken up in the discussion of the relative superiority of inhalations, sprays, and insufflation of powder in diseases of the respiratory passages. The discussion was almost entirely in German, and by Germans, and the burden of it all was the

Teutonic enthusiasm with which each champion of either spray, powder, or gas sought to impress the others with its superiority and the utter uselessness of the other measure, until some of us were nauseated into the belief that we were attending a convocation of Teutonic medical men with one idea, and not a great international congress in which a dozen intelligent and civilized nations were ably represented. These latter remarks refer to the first day's proceedings only, and I am pleased to say the work assumed an international complexion the next day, where every nation's representatives alike had an opportunity to discuss any part of such truths as he was familiar with.

Men get hold of one little corner of a great truth and say, This is the truth. And they listen to another who has his hand on the same truth, but on another side, and answers, "That is error." We are apt, therefore, to allege more for our observations than they are really worth, and indeed consistent and moderate is the man who alleges only enough. In this connection I desire to say that what allegations were made by me, and what opinions and observations expressed upon that form of asthma known as hay-asthma eight years ago, in a paper entitled "Hay-asthma and Nasopharyngeal Catarrh," I believe now that I then took one of the blind men's views of a great truth, and told my story of the question as I saw it. But I have since examined it again and again, and observed more and more, and, with the accumulated experience of eight years and careful observation, I must say that the questions I then formulated are still more strongly tenable in the light of accumulated practical experience—viz., that there is a certain proportion of cases of hay-asthma and also of asthma, and I am convinced the proportion is a growing one, that can be cured by putting the naso-pharynx in a healthful condition.

This, gentlemen, is the corner of the great truth which

my hand touches. But I can not and will not say that my fellow, who appears to differ with me, has not also his hand on another part of the same truth.

Remarks by

ANDREW H. SMITH, M. D.,

NEW YORK.

WHILE agreeing with Dr. Bosworth that many cases of asthma have their origin in irritation of the upper air-passages, I believe there are also many cases having a different origin. There is no single locality which is always the seat of the reflex action. I have a patient, for example, who can bring on an attack at any time by imprudence in diet. A lobster-salad will cause a paroxysm with unvarying certainty. Here the gastric branch of the vagus is at fault. Such cases are by no means rare, and the term dyspeptic asthma is an entirely legitimate one. I am sorry that, in his physiological experiments, Dr. Shurly did not try the effect of irritation applied to the gastric nerve.

I can not agree with Dr. Bosworth that vaso-motor paresis is the main element in asthma. The very rapidity with which the dyspnoea follows irritation of the nasal membrane in his experiments speaks against the theory of tumefaction of the pulmonary mucous membrane resulting from vaso-motor paresis as the cause of the obstruction. Such tumefaction would require considerable time for its production, whereas the wheezing follows instantly upon touching the irritable nasal surface, thus indicating spasm of the muscular fibers encircling the air-tubes.

Still, I do believe that there is a form of asthma in which vaso-motor disturbance plays a prominent part, not alone by causing tumefaction of the pulmonary membrane, but also by retarding the flow of blood through the capillaries. Dyspnoea results as surely from retardation of blood in the

capillaries as from lack of air in the alveoli. Many years ago I observed that, while most patients with asthma were immediately relieved by the inhalation of oxygen, some were not at all benefited. This, I think, may be explained by supposing that in the latter cases the trouble is chiefly in the blood-vessels and not in the air-tubes; that the patient is suffering not from a lack of oxygen, but from a lack of blood to carry the oxygen to the tissues. The positive clinical evidence in favor of the existence of the vaso-motor function in the lungs, such as the occurrence of sudden congestion, etc., appears to me conclusive, notwithstanding the negative results of anatomical and physiological researches.

With regard to nasal irritation, I believe that abnormal contact of surfaces within the nose, especially if the contact is accompanied by pressure, will often produce results far beyond what would naturally be expected. For example, I have again and again seen obstinate catarrh immediately relieved by removing the pressure of an hypertrophied turbinated body upon the septum, and I do not for a moment doubt that many cases of asthma can be cured in like manner. I admit this the more readily because I was formerly of a different opinion. But I still think we are going much too far when we seek to establish it as a rule that asthma is to be cured by operative procedures within the nasal cavities. The problem is too complex to admit of such a simple solution.



REASONS WHY

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FOR

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