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—OF—

NERVOUS AND MENTAL DISEASE.

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Original Articles, Selections and Translations.

Art. I.—THE PATHOLOGY OF THE VASO-MOTOR NERVOUS SYSTEM.*

Lecture I.

GENTLEMEN: I call your attention thus early in our course to the pathology of the vaso-motor, or, to use a better known and more comprehensive term, sympathetic nervous system, because, when we come to speak of pathological conditions of the brain and spinal cord, more especially of circulatory disorders of those parts, they can be understood only when we refer to the nervous system in question. The experimental researches made on it the last few years, as well as certain pathological observations, point to it as one of the most important factors in not a few pathological processes. Some of these data have not found their way as yet into the text-books which are accessible to most of you. I will, therefore, take this occasion to set before you a pretty full outline of what is known at present in regard to this part of the nervous system. Of course, it is not my intention to describe fully its anatomy and physiology; but I cannot proceed without calling your attention to certain points

* There will be three or four of these Lectures. They are substantially the same as those delivered on the subject in question during the current year, at the Chicago Medical College.

under those heads upon which the pathology of the same is more or less directly based.

As you have been already informed, the nervous system is divided into two parts, the cerebro-spinal and the sympathetic. The first consists of the brain and spinal cord, and the intervening parts, viz.: the medulla oblongata and pons varolii, and also the cerebral and spinal nerves. The second comprises the chain of ganglia, on either side of the spinal column, connected together by intervening cords, and giving off branches, on the one hand, to the cerebro-spinal nervous system, and, on the other hand, large numbers of branches outwardly, which weave plexuses that accompany the arteries to their final distribution, especially those which supply the viscera and glandular organs.

There are several points, to which I now will direct your attention:

1. Its histological elements, and their distribution.

2. The relations which exist between this and the cerebro-spinal nervous system.

3. The seat of vaso-motor centres in the brain and cord

I. *Histological Elements, and their Distribution.*—The three elements of importance are: the nerve cells, nerve fibres, and the connective tissue.

The nerve cells of the sympathetic, when compared, as a whole, with those of the cerebro-spinal nervous system, are smaller, and, as a rule, perhaps, are found with fewer poles, or "caudate processes." They are often, to say the least, bipolar. They are simpler than those of the brain and spinal cord. They are found chiefly in the ganglia of the sympathetic, to the formation of which they largely contribute. But they are found elsewhere. Small ganglia are found in organs like the uterus and heart, especially the latter. Concerning the ganglia found in the substance of the heart, I will speak to you at another time. Small ganglia, or even single nerve cells, have been found in many glandular organs, on the walls of the muscular vessels, and beneath the mucous membrane of the alimentary canal, in which latter part a very fine *rete*, or network, of cells exists. Doubtless, small ganglia exist in many other parts of the body, so that various organs may have, to a certain extent, a self-contained nervous supply, in case of accident to the more central and remoter parts of the nervous system. Each one of the small ganglia, or even single ganglion cells, has, in all probability, a more or less independent action of its own, and so assures to each small territory a nervous supply. This is needful in that class of functions upon which the life of the individual momentarily depends. Every provision must be made for their continuance, so far as it depends on the nervous system. The cells are both sensory and motor. Either there are two classes of cells, having distinct properties, or each single cell has sensory and motor properties for itself. The latter I imagine to be the more common in the sympathetic, though it is not so in the cord and brain.

These cells, there can be no doubt, both receive and give off nerve fibres, the one sensory and the other motory. They are, no doubt, in some cases, connected together so as to form a chain or constellation of cells. But this association of cells together in the sympathetic nervous system is much less the rule than in the cord and brain. This shows, equally with other facts, greater independence in the different parts of the sympathetic than in the cord and brain.

Are any sympathetic cells found in the brain and cord?

Jacubowitsch, some years ago, announced the discovery, in certain parts of the cord, of what he considered to be sympathetic cells; and, more recently, M. Luys has pointed out a continuous column of these cells extending along the inner face of the posterior horn of gray matter in the cord, for its whole length up to the brain. Whether the cells pointed out by Jacubowitsch and M. Luys are really sympathetic cells, there is no means at present known for proving. But, for my own part, I do not see why it may not be that such cells exist in the cord.

It appears certain, as we will see, that sympathetic fibres go to the cord; and I see no reason why such cells may not exist there, though their existence has been neither proved nor disproved. My *opinion* is, they do exist.

I must now say a few words to you about the sympathetic nerve fibres. They belong mostly to a class I have already described to you as gray fibres. They do not usually have the white substance of Schwann, but consist of an axis cylinder and investing sheath.

In appearance they are pale, gray, translucent. But some fibres found in this nervous system do have the white substance of Schwann. They are chiefly found in the cardiac and great solar plexuses, and the nerves that contribute to these plexuses, especially the *splanchnic.* But I have never yet been able to satisfy myself whether these nerve fibres having the white substance of Schwann were not really cerebro-spinal nerves that were mixed up in the plexuses. I do not see any sufficient reason for regarding them as true sympathetic fibres. But I will have occasion soon to return to this subject.

These fibres are used for three purposes:

1. To connect the sympathetic ganglia, or the cells in them, with the parts that are influenced by them, such as glands, unstriped muscles, etc.

2. To connect the ganglia together, for purposes of associated action.

3. To connect the ganglia with the spinal cord and brain. I wish to speak to you briefly about these three classes of fibres.

1. *Those which connect the ganglia, or rather the sympathetic cells, with the non-nervous parts they are intended to influence.*

There are three or four facts in relation to these, to which I must call your attention:

First. They are, as a rule, comparatively short — compared, I mean, with the cerebro-spinal nerve fibres. According as a fibre is long, so is it likely to have its continuity broken. Hence, in part, the comparative frequency of paralysis of parts dependent on cerebro-spinal nerves, and the infrequency of the same accidents to parts supplied from the sympathetic. One may have paralysis of a voluntary muscle, or of all strictly voluntary muscles, and yet live. But the paralysis of the heart would be instant death. An army is in danger, all else being equal, in proportion as it is remote from its base of supplies. So in the nervous system exposed to accident and disease.

Second. Another remarkable fact in regard to these fibres is their tendency to weave plexuses. After the fibres leave the ganglia, or as they return to them, they commingle and weave; everywhere they are found, during their transition between parts, the most elaborate plexuses.

Why is this? Because, by means of this mixing and exchange of fibres, on the one hand, all the sensory fibres from one part do not go to one ganglion, but to many; and, on the other hand, all the fibres proceeding from one ganglion do not go to one part alone, but to many parts. So, if one ganglion is destroyed (and this often happens), no one part has its nervous supply cut off by reason of its connection with other ganglia.

This wide distribution of nerve fibres is effected through the plexuses. Here, again, is a difference, when compared with the cerebro-spinal nervous system, in which but few and simple exchanges of nerve fibres are made outside of the nerve centres.

Third. On the surface of *some* of these fibres there are found some remarkable little ganglion-like bodies. They are found especially upon the mesenteric nerves, and in the heart. What their purposes are cannot now be told, though I propose to examine this question hereafter.

Fourth. These nerves are divided into various classes, according to the function they are found to influence. Thus we have the *vaso-motor* nerves, or those which excite and control the actions of the small muscular vessels. This is the class to which I design more particularly to call your attention. We have *trophic* nerves, or those which are supposed to influence the nutrition of parts of the body. We also have secretory and calorific nerves, etc., comprised in the sympathetic. Hereafter we shall examine, as we best can, what are the grounds for making these divisions of sympathetic nerve fibres.

You should remember that, in speaking of different classes of nerve fibres, it is not meant that these fibres, as such, are really different in structure and function. The differences rather consist in the kind of apparatus with which they are connected. This fact, as it may now be regarded, was fully established by the researches of Vulpian and Philipeaux, and is probably true of all nerve fibres whatever.

2. *Those which connect the ganglia together.*—They may be regarded as simply commissural fibres. They associate the ganglia, or their cells, together, for the purpose of concerted or united action, or possibly for the transfer or equal distribution of nervous influence from one point to another.

3. *Those fibres which connect the sympathetic ganglia with the spinal cord and brain.*—This class of fibres is a highly important one. I will speak to you somewhat fully about them. They pass, in little bundles, between the ganglia on either side of the spinal column and the spinal nerves, which they join just after the emergence of the latter from the spinal cord. For each pair of spinal nerves, especially in the dorsal region, there are two branches which pass to or from the sympathetic. They are called *rami communicantes*, or *communicating* branches. One of them joins the

anterior, or motor root, and the other the posterior, or sensory root (or the ganglion on it), of the spinal nerve.

By means of these branches, small as they are, an important relation is established between these two divisions of the nervous system. The *nature* of this relation has, for a long time past, been an important question. Until recently, no direct way of settling it had been found; but it was chiefly a matter of conjecture.

How should it be settled which way the connecting fibres pass; whether *to* or *from* the spinal cord exclusively, or both ways? No dissection, nor any ordinary mode of experimentation could determine. It has been settled by a method discovered by an English physiologist—Augustus Waller—and, very properly, it takes his name—*Wallerian method.* It is as follows: He discovered, in the course of his numerous experiments on nerves, that when a nerve fibre is divided, and permitted to remain in this state, say several weeks, a microscopical examination of the divided nerve fibre shows that the ends have undergone change, but unequally so. This was found to be so constant a phenomenon as to excite surprise, and led to an inquiry as to its significance. As a result of his studies, he found that the end which had suffered change for the greatest length along the fibre, from the point of section, was invariably on that side of the cut toward which the nerve current normally flows. If it was a motor nerve, the greatest change was on the peripheral side of the cut. If a sensory nerve, on the central side. Insignificant as this fact may appear at first sight, it has proved the key to some vexed problems in neurology. By dividing the communicating branches and applying this method, it has been found that nerve fibres pass both ways, to and from the sympathetic. Those that pass *from* the spinal cord, follow the motor roots of the spinal nerves from which they are soon to be detached—at least in part—to form those of the connecting branches. Those that pass *to* the cord, and *from* the sympathetic, follow the course of the posterior or sensory roots of the spinal nerves. It is quite probable the former are motor nerves. What precise purpose they subserve, I will examine into a little later.

The latter are, with equal probability, sensory nerves. Whether, if they enter the spinal cord, they become connected with true sympathetic cells there, or with sensory cells proper of the cord, I will not at this moment try to determine. So, the two nervous systems (if indeed they should be so considered) have clear anatomical relations. Do these throw any light on their physiological relations?

Two classes of opinions have prevailed, at different times, as to the relations of the sympathetic to the central nervous system:

First. It has been regarded as a mere dependency of the central nervous system, from which it derives its power chiefly or alone. This was the opinion of Johnstone, Wutzer, G. R. Treviranus, Tiedmann, Arnold, and others.

Second. It has been looked upon as essentially distinct from, and independent of, the brain and cord. This was the opinion of Bichat, Gall, Blainville, Burdach, and others.

The facts I have cited at least point out that the sympathetic is subordinate in rank to the cerebro-spinal system. But neither these, nor any other known facts, in my judgment, establish either of the opinions I have mentioned as having prevailed at different times. I believe myself, as not unfrequently happens in such cases, the truth lies between these positions; that the sympathetic, on the one hand, is not a mere dependence of the cerebro-spinal nervous system; nor, on the other, wholly independent of it. That it has a certain degree of independence of the cerebro-spinal nervous system, is shown by many facts.

1. *Its Anatomical Distinctness.* — This is so marked as to leave no room for mistake. Each ganglion, or even ganglion-cell, if normal, I hold to be, in some sense, as an independent centre of action. Each sympathetic ganglion, as much as it may be dependent on its associate ganglia, is a center for reflex nervous action: either this or nothing. Each ganglion is a complete apparatus by itself, just as distinct, physiologically, as it is anatomically. Such considerations, if they do not prove independence on the part of the sympathetic, at least prepare the way for its admission. Then, its histological elements, cells, and fibres, differ from those, characteristic of the brain and cord, sufficiently to imply some fixed difference in action.

2. Then the order of its development in the embryo, as compared with the cord and brain, would seem to imply no small degree of independence of the central nervous system. It may be said to be developed, or at least *perfected*, first.

3. Then the way in which certain remedies act on it, as compared with the brain and cord, more especially the former, point to a rather wide distinction between the two nervous systems. Take chloroform, for example: If you bring a patient fully under

its influence, you abolish consciousness; sensation proper; voluntary motion, of course; and, at last, the reflex power of the cord to a great extent; still, the organic functions, dependent on the sympathetic in no small degree, continue to be performed almost as usual. I need not mention any other agent that acts on the nervous system, since I expect to speak of such when I come to general nervous therapeutics. But such an example shows a marked difference between the brain and cord, more especially the former, and what has been somewhat unhappily called the *organic* nervous system.

I might give you other reasons for distinguishing the sympathetic from the cerebro-spinal nervous system; but I will not now. But do not mistake me as stating the case too strongly for their independence. I have already tried to guard this point. Why not admit a certain degree of independence? The fact that there are vaso-motor centres in the cord, and perhaps brain, is no proof that they are not in a measure independent. But, as regards vaso-motor centres in the cord and brain, their exact seat, etc., I will speak a little later.

Before closing this outline of facts and opinions, as regards the anatomy and physiology of the sympathetic nervous system, I must notice in what senses it can be called sensory and motory. There are two forms of sensibility belonging to the nervous system as a whole, *conscious* and *unconscious*. The former is limited to a very small part of the nervous system at the base of the brain; the other form is enjoyed by most, and perhaps all, parts of the nervous system, whether sympathetic or cerebro-spinal. Both forms are possessed only by nerve cells, and not by nerve fibres. Nerve fibres may possess *irritability*, but not *sensibility*. A muscle may possess the former, but hardly the latter.

Sensibility without consciousness takes cognizance of those numberless centripetal impressions that never reach the seat of perception, wherever that may be, and which normally lead to reflex phenomena in the muscles or secreting, or other organs of the body. It never sleeps while we live, and remain healthy. It is alive to centripetal impressions as well when we are asleep as when awake. Take an instance: After the alimentary bolus leaves the stomach, it passes along the bowel. As it is passed along, it makes an impression on the mucous membrane of the bowel, which is carried back along the corresponding nerves to a nerve-centre. The impression is transmitted to the nerve-cells of the centre, and is there felt, though unconsciously so; and instantly, in obedience to this sensory impression, and in proportion to its urgency, a motor impulse is sent back along other nerves to the corresponding part of the muscular coat of the bowels. Now, all this happens, and the mind knows nothing about it, and has nothing to do with it, and ordinarily, and I may say fortunately, *cannot* know or do anything. This is the form of sensibility possessed by the sympathetic system.

By some it is held, I know, that it also has, under extraordinary circumstances, sensibility with consciousness; or, rather, that the sensory impressions it conveys find their way to the brain, when parts that are supplied by this nerve are strongly irritated. It is, however, not by any means established that the impressions in question are conveyed along this nerve. How can one be certain, in such cases, that there are not still some undivided cerebro-spinal nerves included in the part? It is my opinion that such sensory phenomena may be due to cerebro-spinal fibres, distributed to the parts in question through the sympathetic.

Having thus given you an outline of the anatomy and physiology of the vaso-motor nervous system, I will now call your attention to the classes of functions dependent on it; and, first, to its influence over the vascular system. On account of its known influence in this way, it has been called the *vaso-motor* nervous system. This name was first given to it by Stilling, in 1842. It should be remembered, we may remark in passing, that this name does not include the whole sympathetic nervous system.

The earliest experimental observations which went to show the influence of the sympathetic on the blood-vessels, were made by Dupuy (D'Alfort) with Brachet, in the presence of Dupuytren. They consisted in the extirpation of the superior cervical ganglia in horses. Dupuy noted the redness of the eyes, the elevation of temperature, and sweating, so often observed since in similar experiments. But the disordered action of the blood-vessels, which follows division or destruction of the part of the nervous system leading to them, did not attract his attention, only incidentally.

After a time Brachet * made experiments on dogs, in which

* *Recherches Experimentales sur les fonctions du systems nerveux ganglionaire, et sur leur applications a la Pathologie. Par F. L. Brachet, etc. Deuxieme edition*, 1837. Page 414, *et seq.*

similar phenomena were observed, with more detail and care; but their true meaning and significance were not known to him. Similar experiments and observations were also made by John Reid, in 1838; but he does not seem to have had any better notions of their true nature than Brachet. In 1840, Henle described, perhaps for the first time, at least clearly, the muscular coat of the arteries. He gave it the name of the *contractile coat.* He believed, from what he observed, that the nerves act on this muscular coat as on other muscular structures. But the true state of the case did not occur to him. After him, Stilling, in 1840, at Leipsic, chiefly by inference, as Henle had done, concluded that certain nerves excite and control the movements of the muscular vessels. Such nerves he called *vaso-motors;* and hence the name to-day.

These last two authors, though they used their hypothetical notions concerning the vaso-motor nervous system to explain certain pathological problems, seem not to have fully comprehended the full significance and practical importance of the relations of the nervous system in question to the vascular system.

Passing by the experiment of Biffi, of Milan, and of Budge and Waller, it may be said that the first conclusive and really luminous experiments were due to Claude Bernard, that prince among experimental physiologists. They were made in 1851. Once the phenomena were made manifest to his acute and reflective mind, he began to perceive their wide significance. His claim to priority was contested by Schiff, of Berne, on his own account and that of one of his pupils. But most fair-minded judges have not hesitated to ascribe the merit of priority to Bernard. In 1852, M. Brown-Sequard and M. Cl. Bernard published, almost at the same time—the former in the United States, and the latter in Paris—accounts of the phenomena due to the galvanization of the divided sympathetic nerves. Brown-Sequard certainly published his observations earlier; but Bernard affirmed that he had made his experiments in 1850. But these eminent neurologists may well afford to renounce, or divide, the fame of such discoveries. Since then, the number of experimenters in this line has so multiplied as to render it wholly unsuitable to the purposes of a mere outline of history, such as this is, to give even their names, for they are legion.

I must now call your attention to a few points in connection with the structure of the vessels to which the vaso-motor nerves are distributed. The arteries and veins, as you know, have at least three coats. The middle coat, in the larger arteries, is chiefly elastic; while the same coat in the smaller, and especially in the smallest arteries just previous to their termination in the capillaries, is wholly muscular. The same may be said, though in a less degree, of the veins, whose middle coat is largely muscular. As regards the capillaries themselves, there is pretty good reason for regarding them as elastic, but not contractile. Certainly, they do not have muscular tissue in their walls. They are mere blood-channels, just sufficient, and only sufficient, in thickness of wall to separate the blood within from the tissue without the vessel.

As regards the question, whether the blood-channels receive vaso-motor nerve fibres, there can now be only one answer. They do receive them, freely. But as regards the mode of termination of the nerve fibres in the muscular tissue, there is a singular discrepancy between the most skillful observers. The observations of earlier observers, such as Beale, Arnold, Lehman, etc., have been repeated and extended by Henocque. He describes three plexuses. *

1. A *fundamental* plexus, which the fibres weave *on* the external coat of the vessel, and composed both of gray and white fibres.

2. An *intermediate* plexus, situated *in* the external coat of the artery, and composed of fibres derived from the fundamental plexus. In penetrating the external coat of the vessel, so as to form this plexus, the fibres all lose their *neurilemma*, and acquire numerous nodules.

3. *Intramuscular* plexus, formed of excessively fine filaments, which are detached from the intermediate plexus, and terminate in the muscular fibres.

In each of these three plexuses, ganglion cells are found, especially the first two. Even in the case of observers who agree up to this point, there are rather wide differences as to the modes of termination of the nerve fibres in the muscular fibres.

The termination of nerves in the capillaries has been studied by Tomsa, of Kiew.† He reports that, after forming a plexus about the capillaries, very small fibres are given off, which enter the

* *Rev. des Cours Scientifique*, 1873. Page 88.

† *Nerven des capillaren Blutgefass. Centralblatt*, 1869. Page 562. (*Rev. Scientifique*, 1873, *et seq.*)

nucleoli found in the walls of the capillaries, or into the protoplasmic substance of the same. These latter researches have been confirmed by Kessel and Klein.* But the descriptions given by the above observers have not been accepted by all. Krause contends that the above described plexuses are but connective tissue. Neither Sappey nor Frey nor Engelman have been able to verify the observations in question, though they admit that the nerve fibres are in some way distributed to the muscular fibres of the vessels. All observers agree, however, as to the fact that the muscular vessels are well supplied with nerves.

It is needless, at this day, to quote observations to show that the muscular coat of the blood-vessels is highly contractile. This is admitted on all hands; and, after what has been said, it will not be necessary to enter into the history of the case to show that the vaso-motor nerves exercise a powerful influence on the small muscular vessels. It is now so well known, I shall content myself with a mere outline of the phenomena, rather than a history of progress.

The first really fruitful experiments which went to show that the vessels in question are under the control of the vaso-motor nervous system, were due to Bernard. Since then, they have been so often repeated and extended, by Brown-Sequard, Eckhard, Roever, Vulpian, and many others, as to give us an *embarras des riches* in this direction.

The observed phenomena may be summarized as follows:

1. If the sympathetic nerve leading to a part is divided or destroyed, the blood-vessels of the part immediately become enlarged, and so admit more blood than is usual. The sympathetic in the neck, leading to the head, has usually been selected to be operated on, for manifest reasons. Not only are the blood-vessels enlarged, and, consequently, the parts reddened, but the temperature is elevated, and, in fact, all or most of the vital activities of the part seem to be increased. All this follows division or destruction of the sympathetic.

2. But, now, if we cause a galvanic current to pass along that part of the divided nerve which leads to the affected vessels, they will immediately contract; and if the current is strong, and endures, they become smaller than natural. The part that was red and warm, now becomes pale and cool. When the current

* Quarterly Journal of Microscopical Science, 1873. Page 21, *et seq.* *Rev. Scientifique.* Page 89.

ceases to pass, the affected vessels regain their former state of enlargement, and so continue for a variable length of time. Now, how can we explain such phenomena? They have been explained in two ways:

1. The contraction of the vessels, when the nerve leading to them is irritated, all admit to be due to an irritative influence, conveyed along the nerves to the muscular tissue. About this there is no question. All regard it as an *active* phenomenon.

2. As regards the *dilatation* of the vessels, which follows division of the sympathetic nerves leading to them, contrary positions are held. Brown-Sequard, and, perhaps, the majority of physiologists, regard the dilatation as due to simple paralysis, or loss of vascular *tonus*, on account of being cut off from their proper nervous centre. The affected vessels not only lose their sensibility, but their normal excitant to action; they are paralyzed, and relaxed. They yield to the expansive pressure of the blood, and hence the congestion; their dilatation is *passive.* Such, I say, is the more ordinary and simplest explanation.

But there is another way of explaining dilatation, viz.: by assuming it to be *active*, instead of *passive*—just as active as the contraction is which follows galvanization of the divided sympathetic. The nerves, if they really exist, which excite the vessels to dilatation, are called *vaso-dilutors*, while the antagonistic nerves are called *vaso-constrictors*. The two kinds of nerves regulate the size of the blood-vessels, either independently of each other, or they always antagonize each other—according to circumstances, this one or the other prevailing.

The theory of their independent action on the vessels was worked out carefully by Schiff and Bernard, without much reference to each other. But it was abandoned by Bernard for the second view—that of active antagonism in acting, not on the vessel, direct, but on its own nervous apparatus. To discuss, now, which of these theories we shall adopt, especially which of the last two, would lead me to anticipate the whole question of what is called "inhibitory" nervous action. Hereafter, I expect to mention, and will then fully discuss, the interesting facts under this head.

To return: I have briefly spoken to you concerning the effect, on the blood-vessels of a part, which follows division and stimulation of the vaso-motor nerves leading to it. These results have been obtained by experiments on the lower animals. Have they been found true of men? Do lesions and irritations of the sympathetic, in man, lead to phenomena such as we have mentioned? Of course, if such phenomena are observed in man, they must arise, chiefly, on account of disease and accident. I will now refer to a few cases which will show that experimental results hold good for man.

During the late war, after the battle of Fort Donelson, while in charge of a hospital steamer on the Ohio river, a case came under my observation, in which there was a shot-wound in the left side of the neck, about two inches below the angle of the jaw, entering at the anterior edge of the sterno-mastoid muscle, and ranging backward and slightly inward, and issuing behind on the same side, close to the spinous process of the sixth cervical vertebra. At the time I saw the patient, and while under my observation, there was reddening of the corresponding side of the face, with a bluish tint, reddening, and a watery condition of the eye on the same side, much heat, and a feeling of fullness and dizziness in the head, and swelling of, and discharge from, the mucous membrane of the left nostril. The direction the shot had taken made it almost certain it had injured, if not divided, both the sympathetic and the pneumo-gastric nerves on the side in question. It was, at the time, a matter of surprise to me that the carotid artery had not been injured. Partly because I was not then fully aware of the importance of the case, and partly on account of the hurry and confusion incident to those times, I did not keep track of the case after it was discharged, at Cairo, to return home, in this State (Illinois). But I now have no doubt that the sympathetic was divided by the ball; and, if so, the phenomena agree perfectly with those observed in animals, after division of the sympathetic.

A similar case is mentioned by Dr. S. W. Mitchell, in his excellent work on Injuries of the Nerves.* The injury, in this case, was due to a shot in the neck, and gave rise to phenomena similar to those I have related.

Also, two cases of similar kind are referred to by Eulenberg and Guttman, in their recent and excellent monograph on the Pathology of the Sympathetic.† While wounds of the cervical

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* *Injuries of the Nerves and their Consequences.* By S. Weir Mitchell, M.D., etc. Philadelphia, 1872. Page 318.

† *Die Pathologie des Sympathicus auf Physiologischer Grundlage. Von Dr. Albert Eulenberg, Med. Dr. Paul Guttman, etc. Berlin*, 1873. Pages 8–9.

sympathetic are rare, lesions from disease are more frequent. I will call your attention to a few cases bearing on this point.

Within the past two years, a gentleman came under my care with severe erysipelas of the right side of the face and neck, complicated with marked malarial disorder. The case was unusually severe. In the course of a few days, there seemed to be some improvement, and the area of inflammatory disorder had much diminished. On the right side of the neck, however, midway between the lower jaw and clavicle, a large abscess was developed, which finally occupied all the space between the two points named. There was extensive sloughing and erosion of all subcutaneous structures, leading, finally, to fatal hæmorrhage. But, during the latter half of his illness, the phenomena such as follow division or destruction of the sympathetic were developed, with singular completeness. There was the extreme redness of the corresponding side of the face and head; increased heat, as compared with the other side; reddening of the eye; contraction of the pupil, etc. There was also, at times, very irregular action of the heart. The patient died from hæmorrhage and blood poisoning.

In this case I was almost certain the sympathetic had been, practically speaking, destroyed. At any rate, the phenomena found to follow destruction of the sympathetic in animals, were present in a marked degree. Cases have been reported of compression of the sympathetic by means of tumors, aneurismal, glandular and otherwise, in which similar phenomena have been observed, as by Willebandt, Gairdner, Coates, Ogle, Heineke, Verneuil, Eulenberg, Poiteau, and others.

There can be no doubt, then, that diminution of conductibility of the sympathetic leading to a part, or destruction of the same, produces marked circulatory disorder in the muscular vessels of dependent parts. Hereafter, we may see that this fact has extensive practical applications in pathology.

Having established this point, we will, for a few moments, give attention to the opposite condition of this nerve—or where it is stimulated to an unnatural degree. In the experimental cases, the face became paler and cooler, the pupil enlarged, and eyes prominent. A number of pathological cases have been observed, in which there was manifest irritation of the cervical sympathetic. Ogle describes a case * where there was aneurism of

* London Lancet, April, 1869.

the aorta, and tubercular infiltration and enlargement of the cervical lymphatic glands, in which, during the course of scarlatina, there was enlargement of the pupil, and increased action of the heart. He mentions another case where abscess of the neck produced similar phenomena. Similar cases have been reported, with more or less care, by Denme, Eulenberg, and others.

The same condition of congestion or inflammatory disorder of the substance of the sympathetic nerve (or neuritis), is met with in many of the now numerous cases of Basedow's disease, concerning which I am to speak to you in a later lecture. At that time I will describe the phenomena in detail due to *irritation* of the sympathetic. But suffice it now to say, that, in many cases, we have a condition of things shown, in all essential respects, the same as that observed when the sympathetic is galvanized in experimental cases.

Now, what has been found true for the cervical sympathetic, may prove true for all parts of the nerve. And I have no doubt this statement is perfectly correct. I will not now cite the numerous experiments that have been performed on other portions of the sympathetic, such as the solar plexus, because, in this outline of the subject, I cannot, without rendering my account tedious.

The two points that have been in a measure established on experimental and pathological grounds, are :

1. That *diminution in conductibility, or destruction* of the sympathetic leading to a part, is followed by dilatation of its muscular vessels, and consequent congestion, with a corresponding *increase* in the vital activities of the part.

2. *Irritation*, or *increased action* of the same nerves, no matter how produced, leads to *contraction* of the vessels of affected parts, and consequent *anæmia*, with corresponding *decrease* in its vital activities.

These facts, as they may be called, simple as at first sight they appear to be, will be found, if I mistake not, of great importance in aiding us to construe certain pathological phenomena, until now imperfectly understood. So much, then, for an *outline* of the facts which relate to the action of the vaso-motor nervous system, on the muscular coat of the blood-vessels.

I shall have occasion to return to these points more than once, in succeeding lectures. By means of the facts I have referred to, I expect to aid you, as it seems to me, to a clearer understanding of these important processes—*congestion* and *inflammation* (not to speak of other matters) — than you can obtain in any other way.

I have said to you already that, in the communicating branches between the sympathetic and the cord and brain, many fibres pass from the latter to the former. I wish now to make you acquainted with some facts which relate to the influence of injuries and disease of various parts of the spinal cord and brain, on the action of the vessels, through the agency of the sympathetic.

The first observations bearing on this point were made by Budge and Waller. They found that, when they divided the spinal cord in the lower cervical, or upper dorsal regions, the same disturbance followed, as regards the pupil of the eye, as when the cervical sympathetic was divided. It is known that, so far as the cervical sympathetic is concerned, its normal nerve-current flows toward the head, and that the nerve derives its communicating branches chiefly from the upper dorsal nerves, and, perhaps in some cases, from the last cervical. But the point of interest was, that oculo-pupillar phenomena followed a section or lesion of the cord in the region mentioned, precisely similar to those which result from section of the sympathetic in the neck. This region of the cord they named *cilio-spinal.* Since then, similar experiments have been made on different parts of the cerebro-spinal axis, with a view of locating vaso-motor centres for different parts of the body, by Brown-Sequard, Schiff, Biffi, Ludwig, and Thiry; the Cyon brothers; and, latest of all, perhaps, by Kronecker, at the private laboratory of Ludwig, at Leipsic. Beside these experimental researches, many pathological observations of interest have been made, touching this question of vaso-motor centres in the cord and brain. In the early part of my next lecture, I will give you a brief outline of this subject.

2

Art. II.—THE PHYSIOLOGY OF VOMITING.

By Dr. Antonio Evaristo d'Ornellas.

Translated from the Bulletin Generale de Therapeutique, 1873, 192-205.*

FOR more than a year, we have been carrying on a series of experiments on the physiological action of emetine; but, incidentally, the general phenomena of vomiting have attracted our attention, and led us farther in our investigation than was our original intent. We have thought it proper, therefore, to separate from a memoir on the action of that agent, which we propose to publish at a future time, some physiological conclusions on the act of vomiting, to which we have been conducted in the course of that therapeutic study.

Vomiting is an act designed to empty the stomach promptly by the mouth. It comprises the special sensation which provokes the act, as well as the particular phenomena thus produced. It can be induced in various ways; but the special sensibility which gives rise to it, resides in the mucous membrane of the stomach. It is by nausea and vomiting that the stomach shows its suffering, as it were, its own peculiar distress and pain.

Vomiting can be brought about by efforts of three kinds: intrinsic efforts, appertaining to the stomach itself; extrinsic ones, independent of the stomach, and appertaining to the expiratory organs; and mixed, intrinsic and extrinsic, combined and synergic. The intrinsic forces belong to the muscular tunic of the stomach; the extrinsic, to the diaphragm and the muscles of the abdominal walls (external and internal oblique, transverse and recti). It is most frequently due to the mixed causes, that is to say, to the compression exercised on the contents of the stomach, immediately by its own muscular fibres antiperistaltically contracted, and indirectly by the convulsive action of the abdominal muscles and diaphragm. Vomiting, due exclusively to the contraction of the abdominal muscles and diaphragm, that is, solely to abdominal pressure, is less frequent; and that due to

* The paper here presented is a translation of an article entitled, *Du Vomissement, contribution a l'etude de l'action des vomitifs.* The experiments referred to in the text, were published in subsequent numbers of the *Bulletin Generale de Therapeutique.*—Eds.

the exclusive action of the stomachal coats (vomiting *en fusee*), is still more rare.

The forces which act in the phenomena of vomiting, would themselves be unable to produce it if the contents of the stomach could pass off more easily by the pyloric than by the cardiac orifice. But we know that the latter is larger and more dilatable than the former; that the anti-peristaltic movements of the stomach tend to direct the contents toward the cardiac orifice, while the œsophagus, losing its retentive power (Longet, *Syst. Nerveux*, i., 316, 318, and 361), ceases to confine them to the gastric cavity.

We will not, however, occupy ourselves with the part which the muscles take in the act of vomiting, nor with the description of the well-known mechanism of that act, but pass on to study the *role* of the nervous system.

The stomach receives its nerves from the vagus and the sympathetic. The vagi form the only pair of the cerebro-spinal system which supplies the stomach, and at the same time the œsophagus and pharynx.

The vagus is composed of two nerves, which, together, represent a rachidian nerve, of which the anterior, or motor root, is represented by the spinal accessory, and the posterior, or sensitive root, by the pneumogastric. It should, therefore, be considered as establishing an insensible transition between the nerves of organic and animal life. It also bears a resemblance to the great sympathetic, the greater in proportion to the distance from its origin, so as to suggest that the vagus is an adjunct, or is complementary to the sympathetic.

The pneumogastric, the posterior root, although containing motor fibres at its origin (Cl. Bernard), is essentially a nerve of sensation; and, in the normal condition, transmits the peculiar sensations from the stomach, such as nausea, and probably, also, hunger, thirst, etc. (Longet, l. c., 347, 348; Exper. lxxxvi. and xcii.) The spinal accessory, or anterior root of the vagus, governs the intrinsic movements of the stomach, principally those which are necessary to the process of chymification. (Longet, l. c., 318, 325, 330, 346, 361, 362.)

The distribution of the motor and sensory divisions of the vagus is well known; the spinal accessory divides into two branches, one of which unites with the pneumogastric, and the other subdivides into anastomotic divisions for the first cervical pair; and into muscular branches for the trapezius and sterno-mastoid. The pneumogastric, after receiving the division from the other nerve, and furnishing branches to the apparatus of respiration and circulation, anastomoses with its fellow of the opposite side, and with the sympathetic, forming a plexus, giving a branch to the pharynx, and others to the œsophagus and stomach; and finally terminates on the left side in the latter organ, and on the right in the solar plexus, which, on account of its importance, has often been considered as the centre of nutritive or organic life.

The muscles which produce the extrinsic efforts of vomiting (the expiratory muscles), receive their nerves from the anterior root of this pair, from the fourth cervical pair, and from the two first lumbar pairs.

It might seem that the cervical, dorsal, and lumbar pairs of nerves, so important in the act of vomiting, since they exclusively supply certain of the muscles which aid in producing it, were independent of the influence of the pneumogastric; but this cannot be, since all the anterior branches of the rachidian nerves anastomose with the sympathetic, and receive impressions from the medulla oblongata by way of the spinal cord.

From the interesting facts disclosed by the investigations which we are about to communicate, we have been led to review these anatomical details of the vagi, and to assert that the stomach, the œsophagus, and the pharynx, receive their sensitive filaments chiefly from these nerves. It is, indeed, by a reference to this fact that we hope to prove that emetics are eliminated by the mucous membrane of the stomach and intestines, and that they provoke emesis by exciting the peripheral expansions of these nerves, and not by exciting first the nervous centres.

Our experiments on dogs have shown: (1) by what route the elimination of an emetic takes place; (2) with what slowness it acts when introduced by hypodermic injection; and (3) that it is certainly at the identical time when elimination takes place from the mucous membrane of the stomach that vomiting begins.

The fact of the elimination of emetine by the stomach, intestines, and liver, is perfectly demonstrated; for, after having killed dogs by means of strong doses administered hypodermically, we have made an alcoholic extract of the stomach and intestines, with their contents, and with this extract have been able to produce emesis in pigeons. (Exper. lxvii., lxxvii., lxxviii., etc.)

It has been further demonstrated, that emetine, introduced into the cellular tissue by subcutaneous injection, takes about three times as long to produce its effect, as it does when introduced directly into the stomach. In examining our experiments on men, dogs, and pigeons, one is convinced of this fact, if account is taken of the doses, mode of administration, etc. (Exper. lv., lviii., lxxvii., etc.)

It is also well demonstrated, that the moment when vomiting begins, coincides, *all* other things being equal, with that of the elimination of the emetine; for, if a dog is poisoned by a large dose of the alkaloid, we see by the inflammatory lesions of the stomach and intestines, that elimination has sensibly commenced at the time of the first vomiting, that is about forty minutes after the subcutaneous injection. (Exper. lxxvii.) Moreover, in man, as well as in dogs, the vomiting is accompanied, and, indeed, preceded, by an abundant salivation, indicating an elimination taking place, probably through the salivary glands.

It is easy to account for the mode of action of emetine by varying the method of its administration. It is thus seen that the excitation acts on the peripheral terminations of the nerves. In fact, intense and grave inflammatory lesions of the stomach and duodenum, with repeated and severe attacks of vomiting, follow the hypodermic injection of moderate doses; while, with repeated injections of minute quantities, continued through several days, there is little or no vomiting, and the lesions are pronounced only in the jejunum and large intestine, while the stomach is almost normal. In general, the seat of the lesions approaches nearer the lower extremity of the digestive tube, the smaller the quantities given, and the longer their period of administration. (Exper. lv., lix., lxvii., etc.)

Such had been our conclusions for some time, when it was our good fortune to find a fresh confirmation of them in an article previously unknown to us, by MM. A. Kleimann and R. Simonowitsch, of Zurich. These authors arrived at the same results by injecting wine of antimony into the veins of dogs. They observed not only that the phenomena of vomiting were delayed longer when the emetic was administered by the jugular vein than when given by the stomach, but they also found in the first matters ejected the antimony which they had injected into the circulation. These gentlemen justly considered the action of the salt to be peripheral; "that the salt produced a special excitation of the walls of the stomach, or the nerves there terminating," and that it had, in no case, a direct action on a "centre for emesis." (*Pfluger's Archiv*, 1872, p. 280.)

All that has preceded, has led us to conclude that vomiting is a reflex phenomenon; that it may be caused by direct action on the sensitive papillæ which the pneumogastric distributes to the stomach, as in our experiments; or that it may be provoked by an indirect action on the expansions of nervous filaments in other regions, as in vomiting caused by the sight or smell of certain objects. In these two cases, the action is reflex and involuntary; in the second, it is due to the imagination; the reflex act has its starting point in the brain itself.

For the phenomenon of vomiting, as for every reflex act,* it is necessary to admit the existence of a point of departure for the excitation, with transmission toward the centre (centripetal transmission), and a centre of reflexion with propagation toward the periphery (centrifugal transmission).

The reflex act of vomiting has, normally, the spinal nerves for its route of transmission, and the medulla oblongata for its reflex centre. We hope, first, to be able to prove from our experiments that the centripetal route of this reflex act is by way of the gastric portion of the pneumogastric. As to its centrifugal route, we have but little to say, as it is sufficiently established by the mechanism of the act itself. Its reflex centre will hereafter receive some attention.

Before we assume that the pneumogastric is the principal agent in causing nausea and vomiting, it is due to offer the results of some recent experiments.

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* Kuss divides the reflex actions according to the routes they follow, and their centripetal and centrifugal action. "For each of these movements is presented two routes of transmission: either by the nerves of the cerebro-spinal system, or the branches of the sympathetic. The largest number of the reflex acts follow, both to and from the nervous centres, the spinal nervous filaments: such, for example, are sneezing and vomiting. A second class, almost as numerous, is composed of those reflex movements of which the centripetal route is by a nerve of the spinal system, and the return by a branch of the sympathetic, most commonly a vaso-motor : such are the reflex actions of the greater part of the secretions, etc., etc. A third class, comprises those in which the centripetal impulse is carried along the nerves of the sympathetic system, and the centrifugal by the spinal nerves: such are the reflex respiratory movements, etc. Finally, we may mention a fourth and last class: those reflex acts of which both the centripetal and centrifugal conductions are by the fibres of the sympathetic: such are the obscure reflexes."

We would refer the reader to the *Cours de Physiologie* of the lamented Prof. Kuss, translated by Dr. Mathias Duval (Paris, 1872), from which we extract the above note, for all in regard to the study of reflex actions, as well as for their laws, discovered by Pfluger, whose name they bear, and confirmed by the researches of Chauveau.

In a number of dogs, we have simultaneously divided the two pneumogastrics in the neck, resecting about one centimetre in length of the nerve. In every case, immediately after the operation, we have seen the animal attempt to vomit. In fact, the section excited the central extremities of the divided nerve, and provoked vomiting by its reflex action on the medulla, in the same manner as the other reflex actions of the vagus may be produced, such as dyspnœa, augmentation of the pulse, regulation of the same, etc.

We have then, after a period of rest, injected into the cellular tissue of the same dogs emetic doses of emetine; and then, to our great surprise, they either have not vomited at all, or they have done so after a considerable lapse of time, more than three hours after the injection, and then only very little. (Exper. lxxxvi., lxxxvii., lxxxix. and lxli.)

What is it that hinders or retards the act in these cases? Is it that the elimination of the drug has been interfered with by section of the pneumogastrics? No; for at the autopsy, on the following day, we have found the usual gastric and enteric lesions. We are rather inclined to believe, that, by the operation, we have intercepted the nervous current which carries to the medulla and the brain the excitations from the alimentary passages. It is for that reason that the vomiting was either altogether lacking, or retarded in its appearance.

But then, it may be asked why the vomiting was produced in some of these cases, although slowly? We believe, with Prof. Cl. Bernard, that the organism is never limited to a single means for the production of such important physiological acts. We believe that the sensitive fibres of the grand sympathetic can replace those of the pneumogastric; in short, that the centripetal conductors of the impulse of vomiting are changed. The delay in the act is due to the time required by the sympathetic to appropriate to itself this new function. Without going further, does not the mere fact of the vomiting immediately consecutive to the resection, prove sufficiently that the pneumogastrics (excited by the operation) are the principal nerves of emesis?

We have also seen, to our great surprise, that the section of only one pneumogastric (either the right or the left) does not generally cause immediate vomiting, as does the resection of both together; and does not arrest nor delay its appearance when provoked by the subcutaneous injection of emetine; that is to say, by the section of only one pneumogastric, we still leave to the stomach the other nerve and its anastomoses; and that organ continues to act in nearly the same manner as if both remained intact. (Exper. lxl. and lxlii.)

So, the pneumogastrics are not only the nerves which bring about the act of vomiting, but one of them can replace the other; and the grand sympathetic may act vicariously for them both, just as the intestines exert themselves to eliminate urea after the extirpation of the kidneys (Dumas; Cl. Bernard). These substitutions, these replacements, are all properly to be comprehended in the mode of function of the nerve.

In fact, does not the whole system of the nerves of the life of relation form a unity? Are not the nerves of special sense often seen, in the normal state, to be the centripetal conductors of the excitation to vomiting, in place of the pneumogastric? Do not the two systems of nerves, those of organic life and those of animal life, form a well-connected whole, in which they anastomose and mix with each other, and with those nerves which, like the trigeminus and the trisplanchnic, serve as a transition between the two? There is nothing strange, therefore, that, in special cases, the sensitive fibres of the sympathetic should replace those of the pneumogastric; that is, that the grand sympathetic becomes the route of centripetal transmissions for the reflex act of vomiting, when the pneumogastrics themselves are in default.

As regards the second order of routes of transmission of the excitation (centrifugal routes), it is only needful to recall the fact that the motor nerves concerned in the act of vomiting, are those of the expiratory muscles. The knowledge of these muscles suffices to show which are the centrifugal nerves, as nearly all of them are voluntary, and their nervous supply is well determined.

Can the centrifugal routes of transmission change like the centripetal? Generally not; because the efforts in vomiting are, for the most part, extrinsic and dependent on the muscles supplied from the spinal cord. The change is possible in only one case: in the sudden and complete evacuation of the stomach (*vomissement en fusee*), which is due to the contraction of the muscular fibres of the gastric coats receiving motor fibres from the grand sympathetic.

The reflex centre for vomiting is located in the medulla oblongata, in the floor of the fourth ventricle, and very near to the centre for respiration; for are not the expiratory muscles the primary agents in the act? Indeed, according to the experiments of M. Grimm, of Zurich, tartar emetic only causes a rudimentary kind of vomiting while artificial respiration is being carried on, and that is interrupted by it, while ordinary natural respiration is never suspended by the act. Still, we do not agree with that investigator (*Pfluger's Archiv*, 1871: 205), that "these facts speak in favor of the theory that the emetic causes a special excitation of the centre for respiration," any more than they show that that center is the true reflex centre for emesis. We believe, rather, that these facts tend to prove that these two reflex centres are very near each other.

Can the reflex centre change, after the fashion of the routes of transmission? We believe it can, in one case, that of vomiting *en fusee*, when the stomach only contracts; and it is altogether a localized act. Then, the sensory fibres of the sympathetic from the stomach, have, for a point for reflexion, a nerve-center near at hand, probably the semilunar ganglia, and, for returning the impulse, the motor fibres from those ganglia. In this particular case, the sympathetic system alone acts in producing the vomiting.

Finally, we believe that, in general, the excitation to vomiting is produced by a direct action on the terminal sensitive fibres of the gastric portion of the pneumogastric, in the mucous membrane of the stomach and its continuation; above, in the mucous membrane of the œsophagus and pharynx; below, in that of the duodenum and other abdominal viscera. The excitation is less frequently produced by indirect action; that is to say, by action on the fibres of one or of many nerves of the organs of sense. In the last case, the centripetal movement is made towards the cerebrum, from it to the medulla (by the intervention of sensitive fibres, either cranial or sympathetic); and the simultaneous reflex action to the sensitive extremities of the pneumogastric, causes nausea; and to the termination of the motor nerves of the expiratory muscles, causes vomiting.

These two kinds of vomiting (by direct and by indirect action) have led us to distinguish two kinds of emetics: simple emetics and nauseants. The distinction is not a natural one, as characteristic examples of either kind are rare, and the two actions are found united in one substance. Prof. Gubler is of the opinion that an emetic is the more powerful, as it is more capable of inciting nausea, and that its action extends not only to the stomach, but beyond into the buccal, nasal, and pharyngeal cavities. There is no doubt but that in certain emetics the direct action may be due to a different substance from that which produces the indirect effect. For example, in ipecacuanha, the nauseant effect is due to an odorous principle, separable by ether (Magendie and Pelletier, *Journal de Pharmacie*, 1817: 157), and the emetic action to emetine, separable by water, and still more readily by alcohol. (Magendie and Pelletier, l. c., and Exper. lxi.) Observe how the two substances act: The emetine takes effect always on the gastric mucous membrane, on sensitive nervous fibres, while the nauseating principle acts, in an altogether different manner, on the glosso-pharyngeal and olfactory nerves of special sense, and produces emesis at the moment of injection, quite independently of the local gastric irritation by the eliminated emetine.

The preceding details explain perfectly all the different kinds of vomiting, and the various facts on which they depend.

There is also a species of vomiting produced by irritation of the uvula, or pharynx, which is due to the excitation of the sensitive filaments of the pneumogastric in these parts. There are also cases of vomiting caused by the excitation of the special nerves of sight, hearing, or touch. One individual throws up at the mere sight of a disgusting object; another, at the hearing of a sound, or the touch of an object that is repulsive to him. All these special excitations are doubtless received by the brain, and thence transferred to the medulla.

The familiar phenomenon of sea-sickness, the nausea and vomiting produced by the movement of a vessel, or the whirl of the waltz, is known to nearly every one. Is this a trouble of innervation, produced by congestion of the medulla (together with the whole encephalon)? May not this congestion itself be due to its unaccustomed movement, or to an abnormal excitation of the optic nerve with iridean disorders? or, is it rather the excitation of the gastric expansions of the vagus, by the abnormal movement, to and fro, of the liquid contents of the stomach? Although the phenomenon is obscure, it appears to us that sea-sickness is due to a combination of all these causes.

The acts analogous to vomiting, like regurgitation and rumination, find their explanation, the former in the direct excitation of the sensitive cardiac and œsophageal fibres, the latter in the previous intervention of the will. Organic maladies of the stomach, such as cancer, ulcers, etc., cause a local morbid irritation of the mucous membrane, with vomiting; perhaps by the deposition of a foreign matter; perhaps by destruction of the epithelium, and laying bare the expansions of the sensitive nerves.

The gravel, biliousness, erysipelas, dyspepsia, etc., cause nausea and vomiting by direct excitation of the mucous membrane.

The poisons of certain maladies, cholera, variola, scarlatina, measles, and uræmia, are eliminated in part by the mucous membranes, or the digestive passages, and give rise to disturbances like an emetic.

The abdominal affections, hepatic, renal, and vesical calculi, inflammations of the peritoneum, the intestines, and the liver, congestions of the uterus, etc., etc., find their explanation, as far as their attendant symptom of vomiting is concerned, in the distribution of the pneumogastrics.

Meningitis causes vomiting by its attendant congestion, and the same may be said of inflammation of the spinal meninges.

A cerebral tumor provokes vomiting in the same manner as it causes peripheral pain in a member; it most probably causes the peripheral stomachal sensation by compressing or causing congestion of the medulla. (Gubler.)

Violent pain, in surgical operations, etc., may occasion vomiting by provoking an abnormal reflex act, determined in its principle by congestion of the medulla oblongata (as is proved by acceleration of the respiration, etc.).

Hæmorrhages bring on vomiting, as they cause syncope, by inducing a lack of stimulus of the nervous centres.

From all these considerations, is it not of the highest importance to the therapeutist to be able to explain the reflex symptoms of vomiting in each particular disease? Certainly; but we must not forget that the pneumogastric alone does not suffice to explain all cases of nausea and vomiting, but that we must sometimes call in the agency of the sensitive fibres of the nerves of sensibility, and of the great sympathetic.

The mildness of the action of emetine, and its comparative innocuousness in the human species, when properly employed, have led us to undertake this study of the phenomenon of vomiting. It is based on experiments made at the therapeutical laboratory of the Ecole de Medicine, with Dr. Ernest Labbee; at the laboratory of the Jardin des Plantes, with Dr. Armand Moreau; and at the hospital Beaujon, with Prof. Gubler. These experiments form part of a more important memoir on the physiological action of emetine, which we shall soon have the honor to read before the Societie de Therapeutique.*

It is still necessary to mention, that our experiments have been made with the solutions of the white commercial emetine, from the manufactory of Merck, in Darmstadt. These aqueous solutions, sometimes in the proportion of $\frac{1}{50}$, sometimes $\frac{1}{20}$, although requiring a slight amount of acid, have received, in the one case, only a minute drop of nitric, and, in the other, a little citric acid; and are still neutral, not reddening litmus paper. In each experiment, we have taken care to indicate, by its initial, the acid which was employed to perfect the solution.

Art. III.—SYPHILIS; HEMIPLEGIA; APHASIA. RECOVERY.

By H. Webster Jones, A.M., M.D., Chicago, Aug. 15, 1873.

On the 1st of November, 1867, a resident of this city allowed himself intercourse with a courtesan; on the 10th, he had repeated connection with his wife, just returned after a long absence; on the 11th, he discovered a sore under the prepuce, which was pronounced by a reputable physician, on the 13th, a true chancre.

His alarm and dread of exposure, and the occurrence of suspicious symptoms in the person of his wife, led the patient to consult the writer, who had been her former medical attendant, and brought about an examination of her case, upon the 29th of November.

At this time, three small sores were found within the *ostium vaginæ*, which yielded to local applications and general treatment, and were never followed by the evidences of constitutional syphilis.

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* The conclusions arrived at by the author, in his investigations in regard to emetine, will be found given in full in the Periscope of this number of the Journal.—Eds.

After a gradual failure of health and vigor during the winter, and while under the constant observation of his first attendant, about the middle of April, 1868, Mr. H. was attacked with specific eruptions, confined mostly to the arms and wrists, and soon after by a low form of intermittent fever, accompanied by intense supra-orbital and temporal neuralgia of the left side. It was at this time that he came under my especial notice and care. He had alternate constipation and diarrhœa; a heavily coated tongue; a skin generally cool and moist; appetite and sleep had deserted him; and a sense of impending evil contributed to his feebleness of mind and body.

On the 5th of July, an intensely hot day, he became hemiplegic (on the right side) at my office door, and was barely able to indicate his name and residence, whither he was at once conveyed.

Upon arriving, three hours later, I found him sleeping heavily, with a flushed face, hot skin, pulse full, hard, and ranging at 70. He was easily awakened ; manifested pleasure in seeing me, and made several attempts to address me as usual, but with sentences and words entirely incoherent and abortive. His utterance of short words was perfect; but the final syllables of longer ones were either wholly unpronounced, or unintelligible. Intellection seemed quite normal, and he was glad to apprehend the possibility of recovery. When left alone with me, he solicited attention to the eruption upon his wrist, and was satisfied when assured it was no worse.

From this time, the improvement in his general health was very rapid; but the aphasia continued much the same for many weeks. By the 1st of August, he walked without a cane, took regular and abundant exercise, and fed himself with his right hand. He had become able to write his own name, but could not speak it, unless first pronounced by another; nor did he ever address his wife or children by their names.

He could frame short sentences correctly, as regarded all connecting words, but failed generally in the names of things, with constant substitution of words related in similarity of sound. He never read the papers, but liked to have another read to him slowly and distinctly. He passed much of his time in playing checkers and backgammon, which he did with all his former skill.

About the 1st of September, Mr. H. visited the interior of an adjoining State, in hope of more rapid convalescence; but he returned soon, suffering from a severe iritis of the left eye, which yielded to treatment, leaving but slight deformity of the pupil, or impairment of vision. Nevertheless, the aphasia was much less marked, and he even attempted to fulfill his duties as salesman, but failed for "lack of words," though his general knowledge of business seemed as good as ever.

Mr. H. now went to reside in a distant town, and I have known little of his health, until receiving the following letter, in response to a direct inquiry. He writes, April 4th, 1873 (*verbatim et literatim*) *:*

"DEAR DOCTOR :

"I am verry glad to receve a letter from you. I was in Chicago last fall and cood not find where you were. I wanted to see you verry much indeed.

"I am just as well as I can bee but Iye cant tolk well yet.

"I cant express my itears that is the grate matter with mee.

"I cant read verry well yet. I was at your office about fore years ago. I cood not read at all do you remember it. I can remember EVERY THING but I cant express it. I could not figur fore years ago. I can figur all right now. But I cant express the itears that is what I cant do.

"—— ——

. . . .

"Yours truly,

"—— ——."

The treatment given Mr. H. from November to April, 1868, cannot now be definitely ascertained. My own impressions, derived from his subsequent statements, are, that he took much medicine, with the idea that the liability to secondary symptoms and public exposure would then be rendered less. Whatever debility might thus have arisen, it is certain that his mental agony and remorse were terrific to endure and to behold, and were a large element among the causes of after symptoms.

Quinine, iron, and arsenic, were among the remedies given in May and June. Much of the former month was spent in traveling for his firm; and I presume the necessary irregularities of his life contributed to the failures of medicine to produce anticipated effects. He had taken, at his own request, a few doses of mass. hydrargyri, for constipation, just before his hemiplegic attack; and I have always felt that its effect, which was excessive, together with the prostrating influences of the great heat of July 4th and 5th, were the exciting causes of that occurrence. At this crisis of affairs, he was put upon the iodide of potassium, to which was subsequently added the syrup of iodide of iron. This medicine was continued, with brief intermissions, until shortly before he made the short trip which resulted so unfortunately in iritis. He had no iodides for two weeks prior to this attack, all evidences of specific disease having disappeared.

Upon his return, at the suggestion of Prof. E. L. Holmes, he employed the iodide of potassium, in ten grain doses, and had mercurial innunctions made upon the temple and brow of the affected side. This treatment was marvelously successful, and caused the rapid absorption of a band of lymph, which at one time threatened the obliteration of the pupil.

Mr. H. now left Chicago, and I have no knowledge of any subsequent treatment.

The salient points of interest in his case, are the history of hemiplegia and aphasia during the secondary stage of syphilis; the unfavorable influences of a terrible remorse; the variable relationship between the paralysis, the aphasia, and the secondary symptoms, as regards severity and rapidity of convalescence; the perfect intellection throughout, accompanied by an almost total loss of language, at first, and subsequent disability as to the names of persons and things; the present phonetic spelling employed (Mr. H. was an educated man); and, finally, the degree of recovery during a period of five years, with a presumptive absence of the signs of a syphilitic cachexia.

ART. IV.—THE INNERVATION OF THE SPLEEN, AND ITS CONNECTION WITH LEUCOCYTHEMIA.

By Dr. Med. Furst von Tarchanoff.

Translated from Pflüger's Archiv, VIII., I. August, 1873.

CONCERNING the innervation of the spleen, there is at present only as much known as may be stated in the following words: The excitation of its nerves, as well as the action of strychnia, quinia, eucalyptus globulus, etc., on the animal organism, produces a powerful contraction of the spleen; the section of its nerves, on the other hand, causes it to swell and increase in size.

Starting with the idea that the spleen, as an organ made up almost entirely of vessels, should be innervated in much the same manner as a blood-vessel, I undertook a series of such experiments as are usually instituted to observe the vaso-motor phenomena of the body, with this difference, however, that I observed the alterations of the size of the spleen, instead of the contraction of the vessels.

For this purpose, the dog which was experimented upon was first poisoned with curare, to the degree of perfect abolition of motion; and life was kept up during the experiment by artificial respiration.

I opened the abdomen carefully, by an incision of three or four centimetres along the linea alba, through which the spleen was drawn out and covered with a towel, to prevent its drying. The size of the spleen was determined with care previous to each experiment. In some experiments, I brought one cartoid in connection with Ludwig's kymographion, in order to ascertain the blood-pressure. The following results were obtained:

1. The excitation of the central extremity of the vagus by means of induction electricity (of considerable or medium intensity), continuing a minute or more, produced, in addition to the increase of the blood-pressure, a powerful contraction of the spleen, to the extent of about one or two centimetres. The excitation of the peripheral end of the same nerve, either caused a scarcely perceptible contraction of the organ, or none at all.

2. The excitation of the central end of the sciatic nerve produced, besides the increase of the blood-pressure, a contraction of the spleen, but to a less degree than in the previous case.

3. The excitation of the spinal cord caused a most powerful contraction of the spleen, to the extent of two and a half centimetres, and altered, also, its color and density, turning it to a slaty gray, and making it very hard; but this effect only followed when the splanchnic nerves remained intact, as these nerves contain the centrifugal fibres for the vessels of the spleen.

This contraction of the spleen did not disappear at the same moment with the cessation of its cause; but the contracted organ returned to its original size, only gradually; and after the blood pressure had been completely restored to its normal condition, the spleen still remained for some minutes in a contracted state.

From the facts stated above, it is clear that the diminution of the size of the spleen is an effect of the contraction of its vessels which depends upon the excitation of vaso-motor centres, either directly or indirectly, through the irritation of sensory nerves.

4. If the nerves of the spleen are consecutively cut, there follows, as is well known, an increase in size of the organ in those parts, the nerves of which have been severed, so that at the end of the operation, the whole appears very congested, soft, and enlarged. In some parts its size is increased quite one-half. This increase of size is very noticeable, when looking at the organ on its convex side, which is pressed out into a marked prominence.

In fact, this phenomenon takes place in the same manner as the dilatation of the vessels of the ear, or of other organs, after the division of the corresponding vaso-motor nerves; but it here possesses a special interest, as an increase in size of a whole organ to which has been ascribed an important part in the preparation of the blood. If one considers, at the same time, the noticeable enlargement of the spleen, and its function as the organ of the fabrication of the white blood corpuscles, the question readily arises, does not the operation of dividing its nerves have an influence on the number of these white corpuscles in the blood? In order to answer this question, I made the following observations:

I first determined the normal quantity of white corpuscles in the blood of animals before the section of the nerves of the spleen. For this purpose, I took, by means of a slight cut, a drop of blood from the ear of the animal, and passed it quickly, before coagulation could take place, to the object-slide, covered it with a piece of glass, and placed it under the microscope. In order to obtain the finest and most transparent preparation, I used only a part of a drop. Operating in this manner, the number of white blood corpuscles in the field of vision varied between six and fifteen.

After the section of its nerves, the swollen and enlarged spleen was returned into the abdominal cavity, and the wound closed. The first dog operated upon died in two or three days; but the last animal endured the operation very well, and survived it about a month.

The examination of the blood of the animals thus operated upon gave the following results: Two, three, or four days after the operation, there appeared, in the field of the microscope, a great number of white corpuscles, so that, in a direct line across the diameter, the number varied between forty and seventy. Indeed, this and the normal blood could be readily distinguished from each other, when mixed under the microscope; and any observer might, without difficulty, diagnosticate leucocythemic and normal blood.

The obviousness of this difference rendered superfluous any complicated arrangement for determining the number of the white blood corpuscles, and I therefore satisfied myself with this method of investigation. The leucocythemia thus produced is a phenomenon of only short duration. The fourth day after the operation, a diminution in the number of white corpuscles may be observed; and, at the end of a week, their number is the same as in the normal blood. Parallel with this phenomenon goes, also, the decrease in the size of the spleen, as I have determined by repeatedly opening the abdomen, and daily measurement of the organ. This phenomenon of the diminution of volume is analogous to the contraction of the vessels of the ear, after they have been dilated, in consequence of the division of the sympathetic nerves.

In order to establish the causative relation between the development of leucocythemia and the enlargement of the spleen, I have still to decide whether perhaps the operation of the opening of the abdominal cavity, and its consequent pathological effects, may not have had an influence in the production of the symptoms. As evidence in this direction, I have only to bring forward the fact that the opening of the abdominal cavity alone, without section of the nerve of the spleen, produces no leucocythemia; whence the conclusion is drawn, that the latter, in the cases described above, was, without question, a direct consequence of the enlargement of the organ.

These facts afford, therefore, a new proof that the spleen is an organ which fabricates the white blood corpuscles, and also show that leucocythemia may ensue as a consequence of an alteration of the nerve activity in the animal body. It also explains, in a sufficiently satisfactory manner, the cases of incidental leucocythemia in infectious diseases, in which the spleen is always enlarged.

It would be of the highest interest, on the ground of the facts given, to institute clinical observations on the spleen and blood of patients suffering with neuroses of the brain, and especially of the spinal cord.

The preceding investigations were prosecuted in the laboratory of Professor Cyon, in the Academy of Medicine of St. Petersburgh.

Art. V.—OBSERVATIONS ON THE ACTION AND USES OF CROTON-CHLORAL HYDRATE.

By Oscar Leibreich, M.D., Professor of Materia Medica in the University of Berlin.

From the British Medical Journal, Dec. 20, 1873.

I HAVE the honor of directing attention to a new remedy, which serves to corroborate the theory I have propounded with respect to the action of hydrate of chloral.

When chlorine gas acts on aldehyde, croton-chloral is formed, as has been demonstrated by Dr. Kramer and Dr. Tinner. In order to avoid a mistake which is apt to be caused by the name, I must here remark that this body possesses no relation whatever to croton oil, although its chemical constitution proves it to be the chlorated aldehyde of crotonic acid. Croton-chloral differs in its outward appearance from hydrate of chloral, by its being dissolved with difficulty in water, and by its crystallizing in small glittering tablets. Its action, though similar to that of hydrate of chloral, differs widely from the latter with regard to its physiological effects. Four grammes, or a drachm, of this substance, dissolved in water, and introduced into the stomach, produce, in the course of from fifteen to twenty minutes, a deep sleep, accompanied by anæsthesis of the head. Whilst the eyeball has lost its irritability, and the nervus trigeminus shows no reaction whatever on being irritated, the tone of the muscles remains unaltered.

I have experimented with this remedy on maniacs during an attack of mania. They remained quietly sitting on their chairs in a deep sleep, their pulse and respiration being unchanged for two whole hours together. If anæsthesis had reached so high a degree in consequence of the application of hydrate of chloral, the patients would have dropped from their chairs, and both their pulse and respiration would have been considerably retarded. I have seen croton-chloral acting in the same way on healthy individuals. In some cases of tic douloureux, the remarkable phenomenon is exhibited that pain ceases before sleep sets in. I am sorry to say, however, that this remedy acts only as a palliative in this dreadful disease. I nevertheless prefer its action to that of morphia, because it has effects as good as the latter remedy, without being so detrimental to the constitution in general. I have never observed any unfavorable effects of croton-chloral on the stomach, or any other organ, although I have made frequent experiments with it.

The indications for the use of this remedy are to be found—1. In cases where hydrate of chloral is inapplicable on account of heart disease; 2. In cases of neuralgia in the district of the nervus trigeminus; 3. In cases where very large doses of chloral are necessary to produce sleep; I there recommend the addition of croton-chloral to hydrate of chloral.

Whilst examining the difference between the action of hydrate of chloral and that of croton-chloral, I have discovered the remarkable fact that it is not the first, but the second, product of decomposition of the latter substance which is brought into action, on account of the first being too rapidly destroyed. Croton-chloral, when subjected to the influence of an alkali, first forms allyl-chloroform, a trichlorated body, which is rapidly decomposed into a bichlorated substance called bichlor-allylene. Now, both chloroform and trichlorated substances act, as I have shown, in their first stage on the brain, in the second on the spinal cord, and in the third on the heart. The retardation of respiration is to be explained by the agency of these substances on the last-mentioned organ. Bichlorated substances act differently, as is proved by bichloride of ethylene. Even if the circulation of the blood in an animal has been stopped by this latter agent for one minute, life may be restored by artificial respiration, which is impossible whenever trichlorated substances have produced this effect, in which case the muscles of the heart remain paralysed. Well, in animals poisoned by croton-chloral to such a degree that both circulation and respiration are stopped entirely, artificial respiration is able to restore the action of the heart immediately, and the life of the animal may thus be saved. Bichlor-allylene, inhaled by the lungs, produces the same effect on animals as croton-chloral. We thus see these bichlorated substances acting on the brain, spinal cord, and medulla oblongata, but not on the heart, which explains the fact that both respiration and circulation remain unaltered in man by a medicinal dose. It is a highly interesting fact, however, that, under favorable conditions, we still are able to produce in animals the effects of the first product of decomposition, of croton-chloral—*i. e.*, of the trichlorated substance, or of allyl-chloroform. In order to observe these effects, it is necessary to introduce immense doses of croton-chloral into the body, when paralysis of the heart actually does ensue.

Art. VI.—ON THE RELATIONS OF THE HEART'S ACTION TO THE NERVOUS SYSTEM, AND TO MENTAL AND EMOTIONAL STATES.

By E. Cyon.

Translated from the Revue Scientifique, No. 21, 1873, p. 421.

[Note.—This article is part of a lecture delivered recently at St. Petersburg, in Russia, by E. Cyon, one of the Cyon brothers, who, as is well known to those that have kept pace with physiological research in the last few years, have made singularly interesting researches on the innervation of the heart. Their experiments have been repeated, by themselves, in the presence of the most competent and practical physiologists in Europe, and have been witnessed with great interest. In a future article, we shall collect all that is thus far known on the subject in question, and present it to our readers in its practical aspects. Though many of M. Cyon's views toward the conclusion of the article, are somewhat fanciful, they are nevertheless suggestive, and are introduced as having a certain value. The most important parts, however, for the practical physician, it will be readily seen, are in the first part of the paper.—Eds.]

THE heart possesses, like all the muscles, motor nerves, which communicate to its fibres a motor impulse. These do not act from the central nervous system, but from small nervous apparatuses, situated in the heart itself, and altogether independent of our will. These autonomous nervous centres act under the influence of excitations which they draw from the temperature and the chemical composition of the blood.

Besides these motor ganglia, the heart contains still other ganglia, designed to regulate and arrest the pulsations. These regulating centres are not in direct contact with the muscular fibres of the heart, and act on them only through the mediation of the motor ganglia. Their duty is to oppose a certain resistance to the transmission of the motor impulses produced by the motor ganglia on the muscular fibres. By this, they cause the ganglia, only, to dispense the vital force which they develop during the excitation, as it were, with a kind of rhythmic economy.

Without this regulating influence of these moderator centres, the motor ganglia would rapidly dissipate their provision of nervous force. The heart, contracting often, and with violence, ought quickly to cease its action, through complete exhaustion of its motor ganglia, and perhaps, also, its muscular fibres, which would not have had time to repair their losses by the absorption of fresh aliments from the blood.

These regulator ganglia of the heart do not, therefore, destroy the least portion of useful work, which should be executed by the motor mechanism, at the expense of the vital forces which develop there; but in regulating the transformation of physical forces into vital forces, or in the transmission of these last to the motor nerves, they distribute the labor over a certain duration of time, and cause the heart to discharge itself in a given period, either by few and powerful, or by frequent and feeble, pulsations.

These regulator and motor ganglia do not differ, the one from the other, merely in their physiological functions. Their anatomical structure, and their connections with the nervous fibres which enter and leave them, must contain certain differences, the exact character of which is not yet determined, but whose existence is incontestably proved by their physiological law, as all the chemical and physical influences which excite one kind of these ganglia, paralyze the other, and *vice versa*.

Without stopping to enumerate the various poisons of the heart, which act specifically on one or the other of these two kinds of ganglia, I shall limit myself to specifying two important agents, which excite these two nervous centres normally: oxygen, as well as the ascending variation of temperature, excite the motor and enfeeble the regulator ganglia; carbonic acid, and the descending variation of the temperature, paralyze the action of the motor and vigorously excite that of the regulator ganglia. I can only permit myself, for the present, to call your attention to this remarkable fact, the important significance of which I shall attempt to establish farther on.

Under the influence of the combined action of these nervous centres, the two halves of the heart contract simultaneously, at regular intervals. The mechanism of these movements includes still many other practical arrangements, by means of which the organ is enabled to accomplish its immense labor without interruption, and with perfect regularity.

It is with regret that I abstain from fully describing this admirable motor and regulator apparatus, in order to pass immediately to the analysis of the relations established between the heart and the brain, which give, on the one hand, an inimitable perfection to the hydraulic functions of the heart, and, on the other, determine its *role* as the organ of our sentiments.

Although receiving its motor impulses from its own ganglia (an arrangement which allows its pulsation to continue when it is separated from the body), the heart is connected with the brain by numerous nervous fibres. These fibres, under the influence of cerebral excitations, considerably modify the heart's action, both in rhythm and in the force of its contraction. By these fibres, also, the heart, on its side, can send to the brain a series of sensations, corresponding to the character of its movements, at a given time.

It is necessary to distinguish, here, two kinds of these nervous fibres: the centrifugal ones, which act from the brain to the heart, and the centripetal, which follow the opposite course. Of the first kind, we are at present acquainted with the nervous fibres which retard the pulsations which come from the brain, through the pneumogastric, and the accelerators, which reach the heart from the ganglia of the great sympathetic.

The excitation of the pneumogastric slows the pulse and augments its force; the accelerator nerve augments the number of the pulsations, and diminishes their power. The functions of these two kinds of nervous fibres, therefore, offer a repetition of the action of the motor and regulator ganglia, situated in the heart itself. This identity of action indicates that these cerebral nerves do not terminate in the muscular fibres, but in corresponding ganglia—the accelerator nerves in motor, and the pneumogastric in regulator ganglia.

The excitation of these nerves, therefore, only increases the action of the ganglia in which they terminate. The physiological *role* of these nerves presents, nevertheless, a fundamental difference: while the inhibiting fibres of the pneumogastric are permanently in a state of tonic excitation, those of the accelerators are only put in action under certain particular conditions. We know, among other things, that the excitants which act directly on the central ends of the nerves, are identical, in their general traits, with those which act on the corresponding peripheral ganglia. Oxygen, and the elevation of the temperature, especially, excite the accelerators; carbonic acid, and lowering of the temperature, act on the regulator nerves.

The brain can also intervene, in the mechanism of the circulation, by the intermediation of other centrifugal nerves, which terminate in the muscular fibres of the minute arteries.

The excitation of these last, starting from the sympathetic, diminishes the volume of the small arteries, while their paralysis augments it. Their hydraulic action may be compared to that of stop-cocks, placed between the minutest arteries and the capillary vessels, and which, opening or closing, to a greater or less degree, regulate the quantity of blood which can pass through any organ whatever of our body in a given time.

All the centrifugal nerves have a common object: that of modifying the activity of the heart, and the distribution of the blood, in the different organs of the body, according to their needs and their conditions. The functions of the animal organism undergo continual oscillations, sometimes normal, sometimes diseased, which render necessary considerable changes in the circulation—changes which ought to procure for the organs the quantity of blood needed at any given moment, and dispel or counterbalance the hurtful influences, contrary to the normal physiological function of the organs. The perfection of this apparatus consists in the fact that, in provoking the infinite variations in the number and force of the cardiac contractions, it makes no change in the amount of mechanical work which the heart ought to perform in maintaining the circulation of the blood.

Sometimes the centrifugal nerves, which directly lead to the organs of circulation, are excited at their central extremity in the brain; sometimes the excitations of the peripheral sensory nerves are reflected on the centrifugal fibres. These reflex excitations produce two kinds of effects on the cardiac and vaso-motor nerves: they either augment or diminish their activity.

By the aid of these reflex effects, the irritation of any part whatever of our body modifies the quantity of blood which courses through all the other organs, and, consequently, modifies the degree of their activity. The unity of our organism, composed, as it is, of thousands of millions of distinct cellular centres, all endowed, to a very high degree, with an autonomous existence—this unity is especially due to the reflex action of the sensory nerves on the vaso-motor and cardiac nerves. By these reflex actions, every irritation, every affection of one group of cells, instantly reflects itself on the action of the others. These reflex mechanisms do not, however, bear upon the subject before us, only in so far as they explain what a powerful influence the excitation of one group of peripheral nerves can exert on the quantity of blood which, at any given moment, bathes our nervous centres, and, consequently, on the dependent psychological action of these nervous centres.

I will not take any more time to speak of these reflex actions, but will say a few words concerning the most perfect of the regulator mechanisms contained in the nerves, which, as the principal sensory nerves of the heart, determine its *role* as the organ of feeling.

As an elastic pump, communicating with a complicated system of dilatable tubes, the heart contains in its cavities a greater or lesser quantity of blood, according as the diameter of the vessels which lead out from it is larger or smaller. A strong contraction of the smaller arteries considerably increases the resistance which the blood meets with on its passage from the arterial to the venous system, and may produce in the heart such a sudden accumulation of blood as to incur for it the same danger as in a boiler, when the steam tension has passed certain limits, viz.: the danger of explosion. The safety-valve, which gives way at a certain pressure, guarantees the boiler against explosion, by opening a way of escape for the imprisoned vapor.

The heart possesses, on its part, a mechanism which insures it against rupture, but incomparably more perfect and more complicated in construction than the safety-valve of the boiler. This mechanism is as follows: each time that the heart receives such an excessive quantity of blood as to threaten rupture, this dilatation excites its two sensory nerves; the excitation transmits itself to the brain, and produces a paralysis of the vaso-motor nerves, by reason of which all the little arteries of the body immediately dilate, and open a ready means of exit to the blood contained in the cavities of the heart; the heart then frees itself with ease; the pressure diminishes; and all danger of rupture disappears.

Imagine for yourselves a central basin which supplies water for many localities, by means of discharging-channels. At the ends of these channels are sluices, the closing or opening of which is dependent on a special mechanism put in motion by the reservoir itself, by means of electric conductors. When, from the closing of a large number of these sluices, the water in the reservoir mounts to a height that threatens an inundation, the mechanism suddenly opens all these passages, permits the water to flow freely from the canals, and thus avoids the imminent danger. It is by just such a procedure that the depressor nerves guarantee the heart from rupture. The difference is only in the greater perfection of the cardiac mechanism which enables the heart itself to regulate its own labor, and to free itself from all danger in some fractions of a second.

The depressor nerves, while they play so important a part in the hydraulic action of the heart, form, also, as sensory nerves, the principal routes by which all the sensations of the heart reach our consciousness.

When the heart beats with regularity, and quietly, the individual is not conscious of any particular feelings. As the rhythm and force of the cardiac contractions are modified by the inhibitory or acceleratory nerves, we experience a series of feelings corresponding to the changes effected. These feelings may be of very different natures; and the expressions which I will cite express all their different shades of difference with more or less justice.

I have already had the honor to describe to you some physical and chemical factors which have the property of strongly exciting or paralyzing the accelerator and inhibitory cardiac nerves. Psychical excitations produce, perhaps, still more profound effects.

The centres for the cardiac nerves are situated in the medulla oblongata; that is to say, in that part of the central nervous system which, connected with all the cerebro-spinal nerves, may be regarded as the confluence where all the excitations propagated in the nervous system meet and intersect.

The psychic states, and the mental dispositions, to which man is subject, are infinitely varied in force and character. The variety and the diversity in the degrees of the oscillations which they produce in the pulsations of the heart, by means of the cardiac nerves, are nearly as great; and hence there results as great a diversity in the feelings which our conciousness receives through the depressor nerves.

The faculty of the centrifugal nerves of being excited by the mental conditions, and that of the centripetal nerves of communicating with exactitude to our consciousness all the irregularities produced by these excitations in the pulsation of the heart, these two faculties of the cardiac nerves include the conditions which make the heart the organ on which are reflected all the variations in our mental state; all the dispositions and all the emotions of the soul—joy or grief, love or hate, malignity or benevolence.

All these emotions express themselves, also, in the movements of the figure, in the voice, in the posture of the body, etc. Physiology and psychology have long interested themselves in these outer signs of our moral state, and have been able to re-unite and elaborate, systematically, the immense amount of material which man and animals offer to the observer. Darwin himself has recently attempted to give an explanation, with more or less success, of the origin of these signs.

The cardiac mechanism which I have described has been known for too short a time to permit us to give a complete table of the changes brought about in the pulsation of the heart by the different mental states. *En revanche*, we may the more easily suggest a rational explanation.

This kind of observations on the heart presents great difficulties; it is, first of all, needful that one should make them on himself. In order to re-unite them in number, a very happy combination of circumstances is needed, the more so as the changes in the pulsations of the heart, as well as the psychic conditions which produce them, are entirely independent of the will.

These phenomena, to be sure, cannot be observed at the will of the experimenter. The majority of mental states can not be called up at will—sudden fright, for example. In spite of these difficulties, we possess, at present, enough material to enable us to formulate certain fundamental theses on the state of dependence in which the cardiac movements find themselves to the psychic excitations of the brain.

All the agreeable and joyful emotions of the mind excite the accelerators of the heart; they therefore cause it to pulsate very rapidly, diminishing, at the same time, the intensity of each beat. The expressions, "The heart palpitates with pleasure," "The heart trembles with joy," are wonderfully characteristic of the pulsations following the excitation of the accelerator nerves. The facility with which the heart empties itself by this kind of contraction, preserving completely the uniformity of the circulation by an insignificant blood-pressure, causes the feeling of well-being so happily rendered in the expression "a light heart."

All the sad or oppressive emotions act principally on the inhibitory fibres of the pneumogastric nerve. According to their intensity, they retard the pulsations to a greater or less degree, by prolonging the intervals during which the heart pumps in a great quantity of blood, from which it can only relieve itself by contracting with a considerable effort. These efforts, accompanied by a certain amount of pain, provoke a series of sensations expressed by these words : "Torture of the heart;" "The oppressed heart;" "The heart contracts painfully." The French expression, "*Avoir le cœur gros*," expresses most accurately the state of the heart after excitation of the pneumogastrics.

Sad news, suddenly announced, or a prolonged, oppressive sensation, often give rise to heart-beats, justly characterized by these words : "The heart beats as if it would burst the chest." These tumultuous, extremely rapid pulsations, arise from a paralysis of the pneumogastrics. This acceleration, due to a paralysis of the inhibitory nerves, has an altogether different character from that observed during the excitation of the cardiac nerves by joyful emotions.

In these tumultuous pulsations, the feeling of pain, of inquietude, and of anguish, becomes nearly insupportable ; the heart is obliged to repeat the painful efforts very frequently, in order to expel a great quantity of blood by very energetic pressure. Sudden surprise, caused either by joyful or sad news, always causes a powerful excitation of the pneumogastric nerves, often to the extent of causing a complete arrest of the heart-beats, and fainting. This arrest is followed by an acceleration of the heart strokes, if the news was pleasurable, and a slackening, if the reverse.

The cardiac nerves have not the exclusive privilege of responding to the mental emotions. Many of the emotions react also at the same time on the vaso-motor nerves, and produce, by their mediation, corresponding changes in the vessels. For instance, the facial pallor from fright is due to a contraction of the minute facial arteries ; the reddening of the face, following sudden joy, or a feeling of shame, results from a paralysis of the vaso-motor nerves, a paralysis which, in the case of sudden joy, is brought about, not by a direct action on these nerves, but by an indirect action exercised on these vessels by the heart and the depressor nerves.

The intensity of the action of mental emotions on the heart, depends, first of all, on the degree of excitability of the nerves. The greater this excitability, the more pronounced will be the changes in the pulsations caused by the emotions, and the more vivid will be the sensation then experienced. In women and children the excitability of these nerves is much greater than in the adult males; in them, also, the mental states are expressed with more force and facility. The popular opinion, that women and children have softer and tenderer hearts, is, therefore, perfectly well-founded in this sense, in a physiological point of view.

Without wishing to enter into a detailed exposition of all the observations which relate to this subject, I would call your attention to the very interesting parallelism which exists between the action on the heart of certain physical and chemical excitants, and the action of psychical excitations. Oxygen and heat act on the heart in the same sense as the agreeable and joyous emotions, by exciting the accelerator nerves. Carbonic acid, and cold, like the depressing and sad emotions, excite the fibres of the pneumogastric, which slow the heart's action.

In other terms, the useful and necessary factors of life, whether they be physical or psychical, act, in a uniform manner, on one group of cardiac nerves; injurious factors on another.

In ordinary life, it is customary to attribute a warm heart to a good man, who is interested in the welfare of his kind, and a cold and hard heart to an egotist. Here, again, the relation established between the moral qualities and the heart's action is perfectly satisfied. In the man, who, as a mere spectator, is indifferent to the joys and sorrows of others, the heart-beats are light and tranquil, as if under the influence of cold. Its contractions, on the other hand, are frequent, as if under the influence of heat, in the man who takes to his heart the good of his kind.

If the relation which exists between the mental states and the state of the heart could be so well put, and so justly expressed, in popular phraseology, how much more reason have the poets to give, satisfactorily, and to state, with still greater exactitude, the innumerable reciprocal influences of the heart and brain!

Nothing is more striking than the universal resemblance of the descriptions of the cardiac sensations given by the poets of different ages and the most diverse peoples. Amarou, in Sanscrit; Petrarch, in Italian; Horace, and Heine, describe, in almost identical terms, all the intoxications and sufferings of the heart, induced by this emotion. The exigencies of an elevated style being altogether different in these languages, the similitude of the expressions can only be due to the similarity of the sensations which demonstrate the justice of the poetic fiction.

You see, therefore, that science and art, physiology and poetry, do not disagree in regarding the heart as the organ of sentiment.

Their accord on this subject is itself so complete, physiology explaining, with so great an exactitude, that which the poets have felt in such perfection, that poetry might profit by the indications of science, and shun, in its creations, such descriptions and such metaphors as are opposed to physiological truth.

For example, let the poets renounce, once for all, the privilege of making their heroes die of ruptures of the heart, caused by sudden grief: for, owing to the preventive action of the depressor nerves, a healthy heart ought not to rupture. Emotions, long continued, especially if they are of different characters and cause brusque alterations of its pulse, can alone cause death, but still slowly, by the gradual development of diseases of the heart, accompanied with all their proper pathological alterations of the organism.

It is, also, inexact to say that one can restrain his heart so as not to blush. Darwin himself, in his recent work, is often noticeable in this respect. According to him, a man can, by great and continued endeavors, attain the power to prevent blushing. In reality, it is impossible. All the nerves which go to the heart and blood-vessels are involuntary. Our will cannot, therefore, intervene in their action.

In only two ways can this apparent restraint of the heart be brought about. Either the individual does not really feel the emotions, or he has felt them so often that the excitability of the cardiac nerves has, by use, become exhausted to the degree of complete insensibility in this respect. The will plays no part. Owing to the involuntary character of the changes effected in the heart and vessels, under the influences of psychical emotions, these changes are the only exact evidence of the sincerity of our feelings. One can attain, by exercise, the power of feigning, by the voice, the facial muscles, by the expression of the eyes, and by hypocritical tears, sentiments which he does not really feel in the least degree. But the greatest of actors is unable to become pale at will, or to cause his heart to beat in the manner required by the sentiment which he expresses. Even if he had often previously experienced the feeling, he could not cause the corresponding change in his circulation, except by calling this sentiment to his remembrance.

In one of the charming novels of the Decameron, Boccaccio tells us of a physician that discovered, by accident, the true cause, before concealed, of the illness of a young man, by noticing the acceleration of the pulse, produced by the entrance of a pretty cousin of the patient. The physician might have been deceived in his diagnosis; the simple acceleration of the movements of the heart, as far as could be recognized by the pulse, might as well have resulted from other agreeable sensations. It would have been necessary, to still determine the precise character of the acceleration, to be able to positively affirm that it was due to love. It was not sufficient to merely feel the pulse, to arrive at that conclusion.

Physiology uses, to-day, for its researches on the pulse and the heart-strokes, graphic apparatuses giving exactly the number, force, and form of their movements. The sphygmograph gives the characteristic traits of the pulse very neatly; the cardiagraph the exact forms of the contractions of the apex of the heart. As yet, these apparatuses have been used only for physiological and pathological purposes; but it is clear that they may be very usefully employed for a purely psychological end. We can, by their aid, obtain graphic traces of the beats corresponding to the different emotions of the soul, such as love, jealousy, fear, sadness, joy, choler, etc., as we have already traces of certain diseases, like typhus, cholera, syphilis, fevers, etc. Dr. Lorrain, in seeking to obtain such traces of the pulse of an insane patient, during the access of insanity, made the first essay of a graphic study of the influence produced on the heart by psychical emotions. The scientific interest of these traces will be very great, but their practical value may be still greater. We may be able to find the measure of the sincerity of the sentiments which we affirm to others, or to ourselves. The so difficult art of reading the human heart may then be reduced to a certain sleight-of-hand, easy to acquire, in managing the cardiagraph.

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The reciprocal action of the heart on the brain subordinates our psychic functions to the state of the heart nearly in the same measure as that state depends on the functions of the spirit.

The modification of the beats of the heart may intervene in two principal fashions, in the direction of our ideas, and in the actions that result, either in causing sudden change in the quantity of blood which floods the nervous centres, or in sending to our consciousness a series of agreeable or painful sensations through the mediation of the depressor nerves. A sudden affluence of blood in the brain, or prolonged painful sensations, may drive a man, suffering from no mental disease, into ideas the most insensate, and acts the most criminal.

The judges in such an instance often find themselves embarrassed. Has the crime with which they have to do been committed under the fatal influence of somatic causes, or through cold-blooded calculation? Owing to the faculty of the human brain to provoke certain emotions by memory, the detailed recital of the crime might produce, in the accused, the changes in the heart-pulse corresponding to these emotions. The judges could, therefore, by the aid of the cardiagraph, determine, with great probability, the degree of responsibility in a given crime.

I would not tire your kind attention by a too long development of the fundamental idea of this discourse. I trust I have succeeded in causing you to cast a glance over the vast and interesting domain of physiology — a domain already partially explored, but which still promises rich harvests to savants, poets, and thinkers.

I shall consider myself very fortunate if I have succeeded in removing that dominant prejudice that, in the intellectual work of humanity, the sciences are in opposition with the arts.

Science and art, having both their primitive source in the ideal tendencies of mankind toward truth, work for the same end, though pursuing different routes.

I have the firm conviction that that part of physiology which employs itself in seeking out the laws of the beautiful, and endeavors to discover the particular structure of our organs of sense, and of our nervous systems; which forces us to love the beautiful, and to abhor that which is not; I am firmly convinced that that portion of physiology has a very different scope than is at this time imagined. When we have discovered the organic laws of the beautiful, then let us search for those which determine the good; for everything that is great, good and generous, is at the same time beautiful; all that is vile, malign and base, is also hideous. There are, therefore, fundamental dispositions in the structure of our brains which compels us to prefer the noble and good to the base and vile, as there is that in the structure of our organs of sense which causes us to love the harmony of sounds and colors, and to detest its opposite.

Physiology can create, in this manner, the basis of a scientific system of ethics, by which it may nourish and cultivate man's moral sense, as it gives the principles of harmony for the cultivation of our musical sense.

The object of my dissertation touches equally two domains, those of physiology and psychology, the study of the intellectual and physical functions. You have seen how physiology, founded on the study of the laws of mechanics and of organic life, bears itself, in our time, in the domain where the soul and body meet and where their reciprocal relations come before our view.

In the history of physiology, in these later days, there has been a time when, dazzled by the unexampled success obtained by the introduction of mechanical theories in explaining the phenomena of life, physiologists have imagined that it would suffice to bring in these same views, and the methods of research based upon them, to win as honorable laurels in the study of intellectual phenomena.

In the dependence of the psychic phenomena, on the degree of development of the brain, in the immense influence which many somatic causes, such as narcosis, delirium, madness, and microcephalism, can exercise on the march of our ideas—in all these facts, physiology hopes to find a number of points of departure, from which to carry its researches into the spiritual domain. The successes already obtained in psycho-physics seem to announce to physiologists that they will finally be able to explain that mystery which, for thousands of years, has baffled the most vigorous endeavors of the greatest minds of all the world. So, the infant, seeing at the horizon the apparent point of junction of the earth and sky, thinks that, from that imaginary limit, he can climb into heaven. The uselessness of his exertions first reveals the difficulty of his enterprise; but his mind will only admit its impossibility when the true relations of our planet to its system are explained to him.

The consciousness of the difficulty of explaining that which as yet remains inexplicable, has been realized by some physiologists. But the true end which they may reach will only appear after they have obtained the first important results from the application of exact physical methods to the study of the psychic functions. Then they can see clearly where are the limits of investigation—limits that the human mind can never pass.

As in the study of physical phenomena, the researches on the intimate essence of the psychological functions, the ultimate efforts of physiological study, should have, as their aim, the reduction of these functions to known movements of atoms—movements resulting from their own proper forces; in a word, to discover the mechanism of the atoms of the brain.

By continuing to follow the road indicated by Fechner and Helmholtz, and by continuing to study, minutely, the *physique* of those organs of sense which serve, so to speak, as mediators between the soul and body, physiology may, perhaps, some day succeed, at the expense of incredible efforts, in discovering the mechanism of the cerebral functions. Although intoxicated with our recent successes, we cannot hope for so brilliant a reward of our efforts, except in the far distant future.

But, then, at that time, when we shall be in possession of the knowledge of that mechanism; when the molecular movements which take place in our nervous cells during the creation of the highest products of the human mind are as plainly comprehensible as the mechanism of a simple calculating machine; when, to use a quotation from Dubois Reymond, "we shall know, nearly in its last details, the dance of the atoms, O., Co^2, P. and H. in the intoxication produced by music, and the tumult of neuralgic pain, then we shall be as far as at present from the comprehension of our own consciousness, and the *modus* of our thought."

Between the acquaintance with the mechanical processes producing thought, and the comprehension of the manner in which these processes form the thought, there is a gulf which the human mind can never pass.

It is sufficient to merely consider this question in its intimate sense, to convince one's self of the absolute impossibility of understanding the relation between the actions of the physical atoms of the brain and the fact that we feel pain, hear sounds, see colors, and experience pleasure; between the movements and our consciousness that they take place.

The impossibility of understanding that relation, is as great for the most elementary sensations of pain and pleasure experienced by any of the lower animals, as for the thought of Newton, the creative fancy of Shakespeare, or Raphael. It is equally impossible to understand why the atoms O., C., H., or N., and P., experience pleasure in forming certain combinations, and the reverse in others; and how such chemical combinations of elements can give birth to the law of attraction of Newton, or the requiem of Mozart.

The best physiologists of the present day, especially those to whom the physiology of the organs of sense and psycho-physics owe their greatest advances, are in accord on this fact, that the discovery of the mechanism of the intellectual functions is, in the study of the intellectual life, the extreme limit that neither the natural sciences, nor any other science, can ever pass.

This exact definition of the limit of human knowledge does not infer an avowal of the impotence of the natural sciences; far from it. It rather implies the great advance, for which the human mind is indebted to its cultivation, in freeing us, once for all, from unfruitful researches and numberless errors—tasks in which the greatest thinkers of all times have wasted their best energies. Once disembarrassed from the search after impossible chimeras, all those abilities can be applied to the elaboration of the various questions which still await their solution before we reach the ultimate limits of our intelligence.

We shall find in this labor a never-failing source of intellectual pleasure, and, for humanity, an inexhaustible measure of useful discoveries.

Reviews and Bibliographical Notices.

I.—RECENT PROGRESS IN CEREBRAL PHYSIOLOGY.

I. Experimentelle Untersuchungen ueber die Functionen des Gehirns. Von Prof. H. Nothnagel, zu Freiburg, etc. (*Experimental Investigations on the Functions of the Brain.*) Virchow's Archiv f. Path. Anat. u. f. Klin. Med. Band 57. 2d heft; pages 184–214.*

II. Recherches Experimentales sur le Fonctionnement du Cerveau. Par le Dr. Edouard Fournie. Paris: 1873. (*Experimental Researches on the Action of the Brain, etc.*)

III. Experimental Researches in Cerebral Physiology and Pathology. By David Ferrier, M.D., etc. (*Journal of Anatomy and Physiology*, November, 1873; page 152.)

IV. Experimentelle Untersuchungen ueber das Peripherische und Centrale Nervensystem. Von Professor Gudden, in Zurich. (*Experimental Investigations on the Peripheral and Central Nervous Systems.*) Archiv fur Psychiatrie und Nervenkrankheiten; Band II., 1870; page 693.

Since the vigorous repulse sustained by the doctrines of Gall, attempts to determine whether different parts of the cerebral hemispheres performed the same or different functions, were almost abandoned. The cerebral hemispheres, not to speak of any other parts of the encephalon, were looked upon, in a general way, as the organ and seat of the intellectual faculties, or that part of the body with which the mind was more intimately connected than with any other. Each hemisphere was, at least, generally regarded as one organ, any, or at least most parts, of which might perform similar functions. And the researches of Flourens, and others who followed his example, did not do much to unsettle this conviction.

But within the past few years, the question as to whether the brain does not consist of various anatomically consolidated, but physiologically distinct organs, has been once more revived. But this time the question is raised from a different point of view than that of Gall and his followers. He sought to divide

* The November number of Virchow's Archiv came to hand too late to permit of incorporating the results arrived at by Nothnagel, as set forth at length in the number of the Archiv in question. We shall have occasion again to refer to this subject, and will then give his more recent results.

the brain into distinct parts, each of which was supposed to be the organ of some one of the mental faculties. But recent researches have tended to locate centres in the cerebral hemispheres—more especially its cortical substance—which exert a peculiar influence over distinct parts of the body, as, for example, particular groups of muscles. The activity that is now exhibited in the domain of cerebral physiology had its beginning in the discussions that took place at the French Academy, over the views of MM. Dax, Bouillaud, Broca, and others, in regard to the location of the organ of speech in the anterior portion of the left cerebral hemisphere, as indicated by the seat of organic lesion in Aphasia. Though the question then raised, and, since that time earnestly discussed by many of the most competent physiologists and pathologists in the world, has not been settled, it has been the means of directing an unusual degree of attention, not only to pathological changes in the brain, but to experimental investigations that have seemed to shed some light on the structure and action of that riddle of the organism—the central nervous system. We will now briefly refer to certain recent experimental investigations on the brain, which, if the results to which they have led be confirmed, have great practical importance.

They may be divided into three classes:

1. Those which consist in destroying small portions of the brain, and watching the effects produced.

2. Those which depend on irritating or exciting limited tracts of the brain, and observing the results which follow.

3. By mutilating the nervous system of very young animals, and then permitting them to recover and grow up, all the while watching their development.

1. *Those which Consist in Destroying Small Parts of the Brain.*—Investigations of this kind have been made, somewhat recently, by MM. Fournie and Beaunis, in France, and Nothnagel, in Germany. The modes of procedure were essentially the same in all these cases. We will at present call attention only to the researches of Fournie and Nothnagel, whose papers, cited above, are before us. The method of experimentation was as follows: The animal to be operated upon was generally put under the influence of chloroform, and then a small hole was made through the skull, down to the dura-mater, and, in this way, access to a certain portion of the brain was had, without any noticeable shock to the animal. This being done, a small but definite quantity of a concentrated caustic solution was introduced, by means of a delicate syringe, into that part of the brain selected for the operation. Nothnagel used a solution of chromic acid, and Fournie one of chloride of zinc, deeply tinged with aniline. In either case, a small, but comparatively limited, part of the nervous substance was completely destroyed; and this was done with so little injury to the brain in other respects, even when its central parts were operated on, as to render it somewhat certain that any

phenomena which might arise, that could be ascribed to injury of the brain, were due to the action of the caustic. Nothnagel operated on rabbits, and Fournie on dogs.

1. When the optic thalamus was the seat of lesion, various phenomena were noted, according to the seat and extent of the lesion. In case of total destruction of one of these bodies, there was total loss of sensibility on both sides of the body—according to Fournie. This simply confirms previous observations, and adds nothing new to our stock of knowledge. In speaking of the optic thalami as the seats of sensibility with consciousness, M. Fournie confounds sensation and perception. Vision was lost in two cases, in which the posterior half of the optic thalami was the seat of lesion. Another effect of lesion of the optic lobes observed was a sort of *galloping* motion of the limbs, which was often continued until terminated by paralysis or death. Somewhat similar phenomena, as regards motion of the limbs, were obtained by Nothnagel when the seat of lesion was the *nucleus caudatus* (which may be regarded as accessory to the corpus striatum); also, motor paralysis, in some cases, seemed to follow lesion of the optic thalami.

2. *Corpori Striati.*—In Fournie's experiments, sensibility was not often affected when the lesion was in this body; but, in some cases, there seemed to be impaired understanding. There were convulsions, often, at first, of different muscles, due to the irritative action of the caustic, followed, sooner or later, by paralysis. In other cases, the animal, if it walked, progressed in a circle, turning toward the affected side. In operating on the *nucleus lenticularis*, which may be regarded as part of the corpus striatum, Nothnagel noticed a curious displacement of the limbs. Those opposite to the side operated on were carried toward the median plane, while those of the other side were carried from it. Wounding of the anterior part of the *nucleus lenticularis* led to curvature of the spine, toward the unwounded side, depending on paralysis of the voluntary muscles of the side of the trunk corresponding to the lesion. So constantly does this lateral spinal curvature follow lesion of the nucleus in question, as to enable Professor Nothnagel to predict the seat of lesion in a case presenting the muscular phenomena in question.

3. *Cortical Substance of the Brain.*—Injection of many parts of the cortical substance led, apparently, to loss of memory and of the cognitive faculties, according to M. Fournie. Injection of various parts of the cortical substance led to disturbance in different groups of muscles. The result of M. Fournie's experiments was sufficiently definite to lead him to predict the time was soon coming when we could tell, by reason of disorder in certain groups of muscles, just what particular parts of the cerebral cortical substance is involved, yet his researches are less definite in this respect than Nothnagel's. He found that destruction of a small portion of the rabbit's brain, at a point corresponding to the outer end of the last frontal convolution, on the

anterior lobe of the brain, destroyed the muscular sense in the fore-limb of the opposite side. The limb could be placed in the most constrained position, and the animal would permit it to remain so, but would resist handling of the other fore-limb. After a few days, if the animal lived, it gradually regained the muscular sense in the affected limb. This showed that the seat of the muscular sense for the anterior limbs was not at, but near, the point of lesion. During the time that the muscular sense was lost, the limb retained its motor functions and sensibility as usual, except that its movements were not well co-ordinated.

Nothnagel also found a definite locality in the cortex of the cerebrum, near that for the muscular sense, but a little anterior to it, and on the lateral surface of the hemisphere, the injection of which to some little depth led to paralysis of the extremities of the opposite half of the body. This generally disappeared at the end of one or two weeks, and the animal regained the use of its limbs. Nothnagel, as it seems to us correctly, does not look upon this point as the centre for motion to the affected extremities, but that the point of lesion lies in the direct path of transit of the motor impulse, the ultimate seat of which must be sought elsewhere, and that there are other, though less direct, routes from it to the nervous apparatuses, that are immediately instrumental in exciting the muscles of the limbs to contraction, which routes are gradually opened in proportion as the animal regains the use of its paralyzed limbs. But there is one other way of explaining the fact that the animals regain the use of their limbs without a repair of the damaged brain. It is by supposing that the corresponding part of the opposite hemisphere assumes, slowly, the function of the part destroyed.

Both Nothnagel and Fournie found that injury done to particular parts of the white substance in the interior of the hemispheres led to muscular paralysis on the opposite side of the body. The paralyses were the more likely to ensue the nearer the approach to the *cornu ammonis.*

4. Injection of the cerebellum was found by Fournie to lead to lack of precision in aim, or lack of co-ordinating power. Moreover, he found that, in some instances, a lesion of the cerebellum in dogs led to a peculiar movement of the eyes, in the perpendicular plane.

We now pass to the second kind of experiments on the brain.

2. *Those which result from exciting limited portions of the Brain, and observing the results which follow.*—If the space which we can devote to this resume permitted, we would give an account of the experiments of Hitzig and Fritsche, which were detailed in Reichert's and Dubois Reymond's Archives for 1870. But, since a perfectly similar course of experimentation, more extended in its range, and complete and definite in results, has been conducted by Professor David Ferrier, of King's College, London, we will direct attention exclusively to the latter. Many

of the experiments were performed, or rather repeated, before the last meeting of the British Medical Association, at Bradford, and excited marked attention. Accounts of the experiments have been published in the *British Medical Journal* for 1873, and in the *West Riding Lunatic Asylum Medical Reports*, Vol. III., 1873. And from such sources we had intended to condense an account for THE JOURNAL. But the last number of the *Journal of Anatomy and Physiology* (November, 1873, page 152) contains an abstract of his researches by the author himself, which is so satisfactory that we take pleasure in transferring it to our pages:

The paper* contains the chief results of a research commenced with a view to test the accuracy of the views entertained by Dr. Hughlings Jackson on the pathology of Epilepsy and Chorea. As is well known, Dr. Jackson regards localized and unilateral epilepsies as dependent on irritating or discharging lesions of the convolutions about the corpus striatum.

In order to put this theory to the proof, the author determined to expose the brain in various animals, and apply irritation to the surface. The method of irritation was suggested by the experiments of Fritsche and Hitzig, who had shown that contractions of definite groups of muscles could be caused in dogs by passing galvanic currents through certain portions of the anterior regions of the brain.

The progress of the research ultimately led to the endeavor to establish the localization of cerebral function, not merely as regards motion, but also as regards sensation and the other faculties of mind.

The paper in the West Riding Reports gives the result of experiments on rabbits, cats, and dogs, and is confessedly only a preliminary installment of a more extended research.

Since this paper was written, the author has been engaged in the further prosecution of his inquiries, and at the British Association at Bradford, on September 19th, he gave an account of his more recent researches, at the same time entering more fully into the psychological explanation of many of the phenomena which are partly described in the published account. He has performed experiments on numerous monkeys, as well as other animals, but reserves a complete account of his researches for the Royal Society.

The method of experimentation which the author has adopted, is to place the animal under chloroform, and gradually expose the surface of the brain by successive trephinings and removal of the skull by means of the bone forceps. In this way he has been able to expose the whole hemisphere. After removal of the dura mater, the points of blunted electrodes, in connection with Dubois Reymond's induction coil, are applied to the surface of the brain, without injury to the cortical substance.

The first experiments recorded have special reference to the production of epileptic convulsions; and the mode in which the attacks begin, and the march of the convulsive spasms, are accurately recorded.

It was found that in rabbits, cats, and dogs, the application of the electrodes for a few seconds, induced almost immediately, on some occasions after the lapse of a distinct interval, violent unilateral epileptic convulsions. When the electrodes were applied, one at the anterior, and the other at the posterior part of the hemisphere, the convulsions were complete and violent in the whole of the opposite side of the body. As a rule, they commenced in the face, spread to the neck and upper extremity, and then invaded the hind leg and tail. Dilatation of the pupil, clonic spasms of the jaws, foaming at the mouth, and loss of consciousness, were induced when the fits were at their greatest intensity.

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* Referring to his paper in the *West Riding Lunatic Asylum Medical Reports*, Vol. III., 1873. London: Smith, Elder & Co., 15 Waterloo Place.

Occasionally, the spasmodic convulsions remained localized in one or other limb, or in some one muscle or group of muscles; and frequently, instead of a hasty epileptic attack, a series of choreic twitches alone were manifested, without any affection of sensibility or consciousness.

The march of the spasms is shown to be quite in accordance with the clinical observations of Dr. Hughlings Jackson in cases of unilateral epilepsy in man. Peculiar variations in the mode in which the attacks commenced, depending apparently on the position of the electrodes on the surface of the brain, led the author to approximate the electrodes and to apply very limited irritation, in order to discover whether the convulsive spasms were not due to over violent irritation of localized centres in the brain, whose special function is to govern and direct the action of these muscles for definite purposes, possibly such as might indicate volition and intelligence.

The results were such as to indicate, with a beautiful degree of exactitude, the localization in certain definite and easily defined regions the cerebral centres for various apparently purposive combined movements of the muscles of the limbs, as well as of the tail, the facial muscles, and the muscles of the jaws and tongue. These are all situated in the anterior parts of the brain, in advance of the Fissure of Sylvius, and the individual centres are marked off in the various external convolutions, of which wood-cuts are given. The general plan is, that in the superior external convolution anterior and posterior to the crucial sulcus the various movement of the paws, legs, and tail are centralized; and it is shown that the differentiation of these centres is to a great extent characteristic of the animal's habits; the centre from the fore paw in cats being much more highly differentiated than in dogs and rabbits.

The middle external convolution governs movements of the eyelids, face, and eyes, while the inferior and the Sylvian govern various movements of the whiskers, angle of the mouth, depressors of the lower jaw, and tongue.

In the convolutions posterior to the Fissure of Sylvius certain movements are described as resulting from irritation, viz.: of the ears, eyes, etc. In the paper as yet published, no attempt is made to explain the signification of these; but the author, from his later experiments, indicated at the meeting of the British Association that he had been able to obtain indications of the situation in these regions of the centres of special sense, sight, hearing, and smell. These results and conclusions are, however, not as yet detailed fully. The author indicates, in a note in the paper published in the West Riding Reports, that he had at that time explored the brain of the monkey, and satisfactorily localized the regions and the homologues of the centres already discovered in the brain of the cat, rabbit, and dog.

One of the more important conclusions drawn from the experiments is, that the region which governs the movements of the mouth and tongue in cats and dogs, is the homologue of what is known as Broca's convolution in man, viz.: the posterior part of the inferior frontal.

This, it may be stated, is further borne out by experiments on monkeys.

The pathology of Aphasia is thus rendered comparatively simple. The memory of words is situated in that part of the brain which governs the movements of articulation. It is shown, however, by the experiments, that the brain is symmetrical, find that the corresponding part of each hemisphere produces exactly the same effects on opposite sides of the body. Generally, the action is unilateral and crossed; but, as regards the mouth, the action is almost bilateral; and hence disease of one or other side alone does not cause paralysis of the articulating muscles, because the other side is able to govern as before. The occurrence of loss of speech, with lesion of the left side, is attributed to the fact that most people are left-brained, and that therefore a lesion of the left side causes such an interference with the voluntary recalling of words that the person is speechless, not because memory of words is utterly lost, as this exists in the undamaged side, but because he is unable to lay hold of the word which he wishes to express. With the education of the other side, however, the individual recovers the power of speech. During the interval of recovery of speech only automatic expressions or interjections are uttered, which are evoked by a sort of reflex action, and unconnected with volition. Among other points discussed is the hypothesis advanced by Dr. Broadbent, that associated movements of the body are bilaterally co-ordinated in each hemisphere. The author thinks the experiments which he gives indicate not an *anatomical* but a *physiological* co-ordination through the media of the lower ganglia.

The results of experiments in the corpora striata, optic thalami, corpora quadrigemina, and cerebellum, are also detailed.

The corpora striata are shown to be motor in function, and to govern all the muscles of the opposite side; representing all the muscles directed by the hemispheres, and having a physiological subordination to these as higher centres.

The optic thalami are shown to have no motor function; and the author attributes the interference with motion, which is sometimes described in connection with disease of these ganglia, to affection of the motor strands with which they are in close relation.

The corpora quadrigemina have a special relation to the eyes, and also to the extensor muscles.

Irritation of the nates causes great dilatation of the pupils. The action is crossed, but powerful irritation easily acts on both sides of the body. Trismus and opisthotonus are induced when these ganglia are powerfully stimulated.

The cerebellum is shown to have a function which has never been allotted to it, viz.: to be a co-ordinating centre for the muscles of the eye-balls. The author has only given the results of his experiments on the cerebellum of rabbits; but he has since extended and confirmed them in cats, dogs, and monkeys.

The various lobules of the rabbit's cerebellum are shown to have the power of directing the eyes in certain definite directions.

These cerebellar oculo-motorial centres are brought into relation with the cerebellum as a co-ordinating centre for the muscles concerned in the maintenance of the equilibrium; and these functions are indicated as mutually depending on each other.

A more complete exposition of the facts of experiment, and an account of the results obtained from a further investigation of the brain of the various classes of the vertebrata, is in process of publication.

Inasmuch as some of the practical deductions that might be made from the novel and interesting researches of Dr. Ferrier have been indicated in the quotation we have made, we will refrain from commenting on them, more particularly when we learn the author has in course of publication a more extended account of his researches. Besides, we may well delay a fuller discussion of the experiments above referred to, since they have been sharply contested in certain quarters. M. Dupuy, in his inaugural thesis (1873), entitled *Examen de quelques points de la Physiologie, du Cerveau*, denies, on experimental grounds, the validity of the results of either the experiments of Nothnagel or Ferrier. He believes there is not a single part of the cortex of the brain which will, every time when stimulated, lead to the same movements.* A similar position to that of M. Dupuy is taken by M. Carville.

3. *By Mutilation of Young Animals.*—In this article we have no space to more than mention the interesting researches of Gudden on the nervous system. They consisted in taking young

* *Gazette Medicale de Paris*, 3 *Janvier*, 1874, pages 3 and 4.

animals, immediately after birth, and removing or wounding portions of the nervous system, and then permitting the animal to grow up, mutilated in this way, watching all the while the effects of the lesion on the subsequent development of the animal.

The recuperative power exhibited by many of the very young animals was something wonderful, and without any parallel among older animals.

By his method of experimenting, he has fixed the centre for voluntary motion in the anterior lobes of the cerebral hemispheres. In this respect, there is a certain degree of correspondence with the results obtained by the other two methods. But, in this notice, we cannot pursue this subject farther. Perhaps, in the next number, we may find space to notice, with some detail, the researches of Gudden on the olfactory and optic organs, as they are given in the *Archiv fur Psychiatrie*, and during the year we hope to have the promised account of Dr. Ferrier, when we shall fully survey the practical results of these interesting researches.

II.—HAMMOND: INSANITY AND CRIME.

Insanity in its Relations to Crime; A Text and a Commentary. By William A. Hammond, M.D. New York: D. Appleton & Co., 1873. Pages, 77.

The first thirty-nine pages of this *brochure*, consist of a recital of three cases of murder, in each of which the plea of insanity was set up by the defence. The first two are taken from the "*Causes Celebres*," and the third from the 23d volume of the "*Memoires de l' Academie de Medicine.*" The remainder of the essay consists in what purports to be a commentary on these cases. The question raised is one of high practical importance, and demands, in the interest of justice and humanity, and in the face of trying exigencies, a truly scientific and careful treatment. It is written by one who has attained considerable celebrity in the sphere of the medical jurisprudence of insanity. We took it up with considerable expectation; but after attentive perusal it has been laid down with anything but satisfaction.

Here, as in other cases, much depends on the meaning we attach to leading terms. Dr. Hammond very properly asks, "What constitutes a crime?" The question is both a moral and a legal one, but especially the former. There have been two principal ways of answering it from what purports to be the moral standpoint. One considers the *spring* or *motive*, and the other the *consequences* of a criminal act, in the endeavor to estimate a crime, or to fix responsibility. From which standpoint does Dr. Hammond endeavor to estimate a crime? He says, after some discussion, much of it logically irrelevant, "It seems clear then, that, as Beccaria asserts, crimes are *only* to be measured by the injury done to society;" or, in other words, by their *consequences.* And this is the view that is professedly maintained throughout the essay. But in the eyes of most persons, it deprives a crime of all *moral* character whatever, to evolve all but its consequences. The moral element in a crime does not, properly speaking, lie in its *outcome*, but in its *inception.* It consists in this: that the one who does the criminal act, *knows*, before it is done, that it is wrong, and might have refrained from it, if he had so chosen; and yet, under such circumstances, the act is committed. Even a correct moral view of a crime cannot overlook the consequences to which it leads, but, as we understand the matter, must fix attention chiefly on the *knowledge* and *intent* of the criminal, as relates to his crime; and such is the uniform practice in courts of law. This is a case where the law depends on moral science, as in others it depends on medicine, or some other science or art for its data, or postulates.

Here emerges the distinction that common sense instantly and correctly perceives, between cases the immediate consequences of which are the same, but the motives different. A horse kicks a man, and kills him. A man, with a full knowledge of the nature and consequences of his act, shoots another man, and he dies, as a result of the shot. In both cases, the immediate and plain consequences are the same. Though this is so, the horse is not looked upon as a criminal, but the man is. In what does the difference consist in these cases? Simply a difference of *knowledge*, or *motive.* This would be the almost universal answer. The man *knows* the act is wrong, but the horse does *not* know it.

How can we logically avoid this bald conclusion, if we accept the dictum that a crime is to be only measured by its consequences? If we are forbidden to regard the motive, even for purely legal purposes, we can only look to a difference in results. But as regards *immediate* consequences, there can hardly be said to be any difference; for, in each case, a man is killed. It can only be in the *mediate* or reflex consequences, that the difference lies. To determine what these are is something that Dr. Hammond does not undertake, and which we cannot now do. To answer this question satisfactorily, would involve a comprehensive and exhaustive survey of social relations. Dr. Hammond tells us, however, where to look for a definition of these relations; namely, to the *law.* In this case, we do not know just what scope Dr. Hammond intended the word law to have. But we presume it is to be taken in its technical sense; referring to legislative enactments, which find their ultimate expression in our statute books, or formal codes of law. If this is not the meaning, the term is too elastic and indefinite to admit of its use in discussions having a restricted and definite range.

He says, law is "only a set of rules and regulations by which society agrees to be governed for its convenience and protection." Beside this, he says there is "*no other guide* as to the restraints and obligations of individual members of society." Having defined a crime from a moral, he then defines it from a legal point of view. He says, "it follows that a crime consists *wholly and exclusively* in a violation of law; any act not expressly prohibited by law is *legal*, and cannot constitute an offence against society." (Page 45.) On these quotations we must offer a few remarks, and we must beg the reader's indulgence while we make them.

1st. Is it true "there is no other guide to the restraints and obligations of individual members of society" than the law affords? The answer will depend on the scope we give the term law. If by it we mean law in general, or in its most comprehensive sense, the statement is probably true. But if in the ordinary, restricted sense, we do not hesitate to say that in social life, taken as a whole, people are no more guided or restrained by the law, technically speaking, than by anything else that can be named. Dr. Hammond's mode of stating this case seems to us almost a complete inversion of it.

2d. Is it true that "any act not expressly prohibited by law is legal, and cannot constitute an offence against society"? Here, again, as before, the bad effects are seen, arising from a want of definition of an important and many-sided term. If in this place Dr. Hammond takes the word law in its widest sense, his remark may be regarded as just. If in the more restricted sense already noticed, it is not true. Are there no offences against society which are not catalogued in our statute books? We do not care to discuss a question the answer to which seems so plain. We have no hesitation in saying there are multitudes of offences against society about which the law says and does nothing.

Then, again, is it true, that "any act not expressly prohibited by law is *legal*"? We should certainly say no. How can we call an act *legal* simply because it is not "expressly prohibited by law"? At the best that can be said, it is neither legal nor illegal; the law is indifferent to it.

Dr. Hammond gives his reasons for measuring a crime by its consequences, rather than the motive of the criminal, or, better still, the motive and consequences together. They are:

1st Because, in looking at the motive to a criminal act, "we place ourselves at the mercy of any individual who, with strong reformatory ideas, which he may think it his duty to carry out, stops at nothing in the way of his good intentions." (Page 41.) Is it true that such cases baffle inquisition to such a degree that it may be said "we are placed at their mercy"? Is there no common standard of right and propriety, whereby one can distinguish, in most cases, between the impracticable and hurtful schemes of the visionary or maniac, and those dictated by sound judgment and a healthy propriety, as Dr. Hammond's statement would seem to imply? We do not believe it, and do not think many others do. Yet, this is one of the leading reasons why we should resign the task of laying bare the motives to a criminal act.

2d. His second reason why a crime should not be measured by the knowledge and motives of the criminal, is that men are "liable to err, and a *mistake* is frequently more productive of evil result than a deliberate crime." For example, says he, "let a captain of an ocean steamer make a *mistake* in his reckoning and lose his ship on the rocks, and it would have been better for him that the sea had engulphed him than that he should have lived to tell the story of his mistakes and good intentions." (Page 42.) Is it true that no distinction is to be made, as between the man who, knowing the danger, carelessly or deliberately runs his ship on the rocks and loses it, and the man who, in trying to do his best, should make a *mistake* in his reckoning, and likewise lose his ship? According to Dr. Hammond's views, both are equally guilty, or equally innocent, because the consequences are the same. But, does it not happen daily, in parallel cases, that one has his conduct approved, while that of the other is condemned? Is justice so helpless and indiscriminate in such cases as Dr. Hammond's statements would seem to imply? Almost everybody knows differently.

3d. The third reason why we should estimate a crime solely by its consequences, is this: He says, if crime "is made to consist in the intention, there can rarely be any certainty on these points, for a shrewd person may so cleverly conceal his real purpose as to make discovery out of the question. Morally, as between God and man, the intention constitutes the crime; but society cannot look upon sin and crime as altogether identical; and it has not the infallible correction of omniscience, which constitutes so prominent a faculty of the Deity." (Page 43.)

Now, is it true "there can rarely be any certainty" as to the "real purpose" of the criminal? We do not hesitate to say just the contrary, and to point to the every-day experience of courts of law to justify our position.

It is true we do not have such an exhaustive knowledge as the Deity has of men's motives to action. But if we should surrender the task of exposing criminal motives, because we do not know them as well as God does, we would, for the same reason, cease trying to do or know anything. The law, in all its procedures, recognizes the limitations and weaknesses of the human mind, and draws its rules of inquisition and evidence in full view of the fact that Dr. Hammond mentions; but notwithstanding which the law has ever been correctly seeking to expose the motives of criminals, and so it will ever continue. Our knowledge of human nature, and especially of motives to action, when comprehensively surveyed, is by no means so superficial and unreliable as to justify these views of Dr. Hammond.

He seeks again to help his case by suggesting a distinction which but few clear-thinking persons will be in a position to feel the point of. He objects to the view that "sin and crime are altogether identical." What respectable moralist in the whole list holds any such position? None that we know of.

But we have carried our examination, slight as it has been, of Dr. Hammond's views in regard to the nature of crime, farther than was intended, because we deem them unphilosophical, and calculated to do anything rather than throw light on the practical and perplexing questions he has treated from the standpoint of a medical expert.

He utterly misses one of the prime postulates to be steadily recognized in dealing with such questions as are comprehended under the title of his essay, and which lies at the ultimate spring of all human action deserving to be called moral and rational. The time, indeed, has been long since we have seen in one apparently so accomplished in other respects such lack of analytic power, of true psychological insight, and of philosophical grasp and penetration. One whole hemisphere of the subject, as we regard it, is in darkness. Dr. Hammond appears to us to belong to that class of clever workers and thinkers who look at all questions, even moral and religious ones, from the outside, forgetting, or overlooking, the living soul within, which acts and reacts from its own proper centre. Everything is external; nothing worth speaking of internal. He evidently, if one should judge from the views that pervade this essay, belongs to that school of moralists which, since the days of Jeremy Bentham, to go no farther back, has had many disciples. But, though they have among their number such eminent names as those of Mr. Bain, Mr. Mill and Mr. Spencer, as regards fundamental morals, we wish to range ourselves as heartily disagreeing with them in every essential particular.

But we beg the pardon of our readers for indulging in a strain of remarks which, however right it may be in itself, is not quite germane to the purposes of a medical periodical. And so, when we come to "*insanity* in its relations to crime," he says, in perfect agreement with what has preceded as to the *nature* of crime, no valid argument can be adduced against the punishment of the insane, even though they be *morally irresponsible for their acts*, by reason of delirium, dementia, morbid impulse, emotional insanity, or any other form of mental aberration. (Page 49.) Of course not, if you admit Dr. Hammond's premises. Because, whether a man is killed by a sane or insane person, the consequences are the same—a man is killed. Both are, *legally* speaking, equally responsible, no matter how *irresponsible* one of them may be *morally*. In agreement, also, with Dr. Hammond's view of the nature of crime, we find him saying (page 54) that "some of the insane are such monsters of depravity that they should be slain, upon the same principle that we slay wild and ferocious beasts." Of course. Dr. Hammond admits (on page 67), that "though some lunatics are responsible for their acts, there are others who clearly are not: whose intellectual faculties are so perverted or destroyed as to render them absolutely unaccountable for their acts. Punishment of them can only be justifiable solely in the interest of the safety of society, and should never extend beyond the deprivation of personal liberty." With this we heartily agree.

As regards the later parts of the essay, they are more practical, and are by no means always in harmony with the principles laid down in the beginning, and which we have in some degree endeavored to examine.

The practical conclusion as to the treatment of insane criminals, is that some of them should suffer, say capital punishment, while all others should be sequestered for life, if the tendency is homicidal.

With the first part of this recommendation we cannot agree. The *really* insane should never be hung as the outcome of legal or judicial procedure; that is, if the crime is a result of the insanity. But to state and argue this question would lead one over the whole ground of moral and legal responsibility.

With the latter part, we can heartily agree. If, in case of homicide, or rape, or any other heinous crime against society, the plea of insanity is set up, and truly maintained, then it ought to be so such persons should be sequestered for life, no matter what the show of subsequent sanity might be.

III.—THE HISTOLOGY OF THE CEREBRO-SPINAL NERVOUS CENTRES.

Die Histologie und Histiogenese der Nervosen Centralorgane. Von Dr. Fr. Boll. *Archiv f. Psychiatrie.* Bd. IV., Hft. 1. Also *Separat Abdruck.* Berlin, 1873.

Although this important memoir of Dr. Franz Boll has received notice in different journals, and more or less complete abstracts have appeared in several foreign periodicals, it has nevertheless seemed to us to call for an extended notice in this place, as probably the most extensive contribution to the literature of the minute anatomy of the central nervous system which has appeared during the past year. It includes, moreover, comparative statements of the views of his more recent predecessors in this field; and although these are sometimes criticised with perhaps more freedom than is consistent with the courtesy due from one scientific investigator to the researches of another, still, the work, as a whole, is valuable also on this account. We can, however, in this notice, give merely a brief outline of the principal views and statements of Dr. Boll, with only such mention of his discussion of the work of preceding investigators as may seem necessary for the proper understanding of his facts.

Dr. Boll's memoir consists of four principal sections, of which one is devoted to the connective tissue, another to the elementary forms of nervous matter, a third to the perivascular and epicerebral spaces, and the fourth to the structural development of the central nervous system. Beside these there are introductory and concluding chapters, the whole comprising one hundred and thirty-six octavo pages, of which fifty-five are given to the first of these subjects—the connective tissues of the nervous centres. This, as the author states in his introduction, was the starting point of the whole series of investigations; and he devotes the whole of his concluding chapter to a criticism of the different views which have been advanced by various authors, and to a *resume* of the principal results of his own researches in this especial field. It is in this part of his work, also, that he principally renders himself liable to criticism for the freedom of his remarks on the views of his predecessors. Such expressions as "a blow in the face of the facts," or again, "a complete lack of respect (ehrfurchtlosigkeit) for the objective conditions," are not the most temperate when used in speaking of the opinions or statements of a previous investigator, and, indeed, are unsuitable in any kind of scientific controversy. Criticism may be made sufficiently severe without resort to stronger language than may be used in polite intercourse, and there should, it seems to us, be an observance of some kind of parliamentary usage in scientific as well as in other discussions. This fault of temper in scientific controversy on the part of Dr. Boll, is the more noticeable because it is most exhibited in his concluding chapter, where he discusses the various opinions which have been given forth on the somewhat vexed question of the general structure of the neuroglia or connective tissue of the nervous organs, and sums up the conclusions arrived at by himself in his researches on this subject. Beyond this we have no unfavorable criticisms to make, but will pass on to notice, briefly, the principal points of his memoir.

In this first, and in some respects most important, section of his memoir, Dr. Boll gives a careful and detailed statement of the microscopic structure of the connective tissue, as observed, taking care to give his methods, often minutely, with each preparation. First of all, he mentions the unsatisfactory condition of our knowledge of the subject, the numerous contradictions between the various authorities, and the impossibility of obtaining anywhere in the literature already before the world a perfectly correct statement of the true arrangement of this important element of the nervous structures. His own views agree more nearly with those of two of the latest investigators of this field, Golgi and Jastrowitz, although he expresses his disagreement with them in several points.

Commencing with the white substance of the brain and spinal cord, Dr. Boll finds three different form-elements predominating in different regions of these nervous centres: the Deiters' cells, the interfibrillary granular substance, and the larger cubic cells, with comparatively few processes which are found only in the white substance of the brain. The Deiters' cells, so called because first correctly described by that observer, are the most characteristic; they consist mainly of extremely long and fine processes passing out in one or in all directions from a very minute protoplasmic mass, surrounded with a small quantity of granular matter. and bear a very close resemblance, in all respects, to embryonal connective tissue cells. They are found everywhere in the white substance, but chiefly in connection with the blood vessels, and, according to our author, form the only element composing the finest ramifications of the connective tissue septa of the white substance of the spinal cord. In opposition to Jastrowitz, Dr. Boll considers that no well-defined line of distinction can be established between these and the last-named form of connective tissue, the well-developed cubic or angular cells of the brain, as he has observed every grade of transition between the two. The star-shaped cells described and figured by Kolliker, have, according to Dr. Boll, no actual existence.

The latter part of the chapter is devoted to the connective tissue of the gray substance. Here the author distinguishes three or four different elementary forms. First, there exists, in certain portions of the gray cerebral substance, an anastomosing network of areolar connecting tissue, such as Kolliker assumes to be the general structure of this element in all of the nervous centres. Next, Dr. Boll refers a part of the so-called granular layer to the connective, and part to the nervous, tissue. The distinction, he admits, is not always easily made, and is absolutely impossible in simple cross, or longitudinal sections. Third, the gray molecular mass of the cortex, in regard to which he upholds the view of the majority of authorities, that it consists of a finely granular substance, and compares its appearance, under the strongest powers, to that of freshly fallen snow. By this somewhat peculiar *facies*, it is stated that it is readily distinguishable from the granular appearance of ordinary protoplasm, which it otherwise resembles. Dr. Boll briefly reviews the literature of the subject, and concludes that the view of Henle, R. Wagner, and others, and, more recently, of Rindfleisch, that this molecular mass is of the nature of nervous, rather than connective, tissue, may be considered as definitely decided in the negative.

The last elements in the connective tissue of the gray matter, are the Deiters' cells, which are found everywhere, but more abundantly in connection with the blood vessels. These are the only forms of universal distribution, and afford the only approach to the unity of structure or conditions throughout the connective tissue of these organs, which has been so sought for by various investigators, and against which our author takes every opportunity to make his protest. The only common principle of structure of the connective tissue throughout the nervous centres is, according to him, to be sought for in its development ; the embryonal or Deiters' cells and the granular *deckmasse* of the cortex forming the extremes, all the other forms being intermediate.

We have dwelt thus long on the first portion of Dr. Boll's memoir, because it is that department to which he has devoted the most attention, and in which he makes especial claims. The other portions are, in many respects, not less important; but we are forced to pass them by with briefer notice.

In the second of the four principal chapters, Dr. Boll gives a careful account of the details of the elementary structure of the nervous substance, as observed by himself, stating, in each case, his agreement or variance with the opinions already pronounced by previous authors. In this part, also, he gives very careful details of his methods of preparation; in fact, a large part of the chapter is taken up with descriptions of these methods, and cautions as to their use. Dr. Boll obtained many of his results with the method first given by Gerlach, consisting in treatment with carbonate of ammonia, and with chloride of gold and potassa. By this methed, used with certain precautions, the fine nervous network discovered by Gerlach, which is more or less developed in all parts of the cord, is shown to perfection. In the examination of many of the finer tissues here, as in the preceding chapter, great advantages were found in the use of chromic and osmic acids.

For the most part, the author, in this portion of his work, agrees with previous observers, though not constantly with any one. To review the details of his agreements and differences would require too much space for the present notice; and we can only give to a few of them a passing mention. In respect to the spinal cord, the author agrees, in very many points, with the views of Gerlach; but considers the evidence in favor of his theory as to the anterior and posterior nerve roots, and as to the presence or absence of the axis cylinder process in the cells of the columns of Clarke, as inconclusive as yet, and with our present means of investigation. He distinguishes three different cell-forms in the posterior cornua: 1. Ganglion cells, nearly as large as those of the anterior horn, usually with a very smooth nucleus, and a large smooth ribbon-like process. 2. Very narrow, spindle-shaped cells, with the processes at the extremities. 3. Small stellate cells, with very little cell-substance, and proportionately thick processes. Dr. Boll believes that these three forms were known to Deiters; but they are not clearly distinguished in his work.

In the white matter of the brain the author has observed small ganglion cells among the nerve fibres, easily to be distinguished from the connective tissue cells about them, as well as those which could not with certainty be referred to either form. These nervous elements were altogether lacking in the white substance of the cord. The small multipolar ganglion cells, which Deiters describes as existing in the gray molecular layer of the cortex of the cerebellum, were not seen in any of Dr. Boll's preparations. In regard to the cerebral gray matter of the cortex, he agrees with Steida in distinguishing in the brains of the smaller mammals (the only ones satisfactorily examined), the following four layers: 1. The outer cellless layer. 2. An outer nerve cell layer (small cells). 3. A middle stratum of large nerve cells; and, 4. The inner layer of small cells. The characteristic angular form of the nuclei of the isolated ganglion cells of the cortex cerebri is considered to be artificial, as in osmic acid preparations it always appears perfectly round.

In the third chapter, Dr. Boll distinguishes, with Golgi, a double space, shown by injection around the vessels. One of these, which has a real physiological existence, he calls "the adventitial lymph space;" and states that it communicates with the lymphatic vessels of the pia mater; the other, his "perivascular space," he considers to be entirely of artificial origin. After puncture of the cerebral substance, the injection following the lines of least resistance dilates this perivascular space, and, sometimes, extends under the pia mater into the so-called "epicerebral space." The limiting membrane of the three great nerve centres, is described in this chapter as made up of a form of the already described Deiters' cells, in which the fine processes are given out in only one direction, like a brush, the *pinselzellen* of the author.

In the fourth division of his work, Dr. Boll gives the results of a series of systematic observations on the development of the nerve centres in chickens, during incubation. Commencing with the third day, he divides, for the sake of convenience, the remaining period of incubation, that is, till the twenty-first day, into three periods, as follows:

1st Stage.—Extending from the beginning of the first formation of the hemispheres to the period when it resembles a thin-walled vesicle; including from the third to the sixth day of incubation. In this period, with strong magnifying powers, the nervous and non-nervous elements begin to be distinguishable.

2d Stage.—Extending from the sixth to the tenth day. In this stage the whole hemisphere can be brought under the lens, and examined with the highest powers. The ganglion cells now appear to take on their characteristic angular form, indicating the commencement of the formation of processes; and the granular ground substance shows, in places, a peculiar change, an arrangement of the granules in series and rows, and the vascularization of the organ begins to be apparent.

3d Stage.—Extending from the tenth or twelfth day to the end of incubation. In this stage no new characteristic changes appear; but those before observed increase, and develop themselves. In the latter part of this period, the histiological details of the nervous elements are somewhat obscured by the increase of intermediate tissues.

Several interesting points are discussed in this chapter; we cannot, however, give them the attention that perhaps is their due in this notice. Before closing this article, however, we will quote the summary, given by Dr. Boll, of the stages of development of the nerve fibres of the central nervous system.

1st Stage.—From the commencement to the sixth day of incubation; growth of spindle-shaped cells into axis cylinders.

2d Stage.—From the sixth to the eighteenth day of incubation; the white substance taking shape, but without noticeable alteration of structure.

3d Stage.—From the eighteenth day of incubation to the second day of life. Simultaneously with the appearance of the granular cells in the white substance the axis cylinder becomes enveloped with the medullary layer (white substance of Schwann).

We have endeavored, in this somewhat unsatisfactory notice, to give some of the principal points of Dr. Boll's memoir. It has seemed to us an extremely valuable contribution to the literature of the structural anatomy of the nervous centres; and we have not as yet seen in any English or American medical periodical anything like a complete statement of its contents. That we have not given this the necessary limits and character of the present notice are our only apology. The work is certainly worthy of the close attention of every student of this department of histology.

Editorial Department.

INTRODUCTORY.

WITH this number we begin the publication of a new medical journal. Of course, any one who chooses to accept such a responsibility, has the right to produce such a work.

But a due regard for the opinions of others, and justice to ourselves, seem alike to require that we should state, briefly, the reasons which have led us to undertake this enterprise. A few words, therefore, from the Editors, in relation to their JOURNAL, may not be out of place.

It is begun because it has seemed to the Editors that it is needed, and that, if conducted according to the plan adopted, it may be made useful.

Among the reasons which have appeared to justify the production of such a journal as the one proposed, we may point to the fact, as it seems to be, that nervous and mental diseases are becoming far more prevalent than formerly. This is not due simply to an increase of population. The causes of the increased prevalence of such diseases lie far deeper, and deserve most careful study. We cannot discuss them now. This may be attempted hereafter in the JOURNAL. They will be found, however, to lie, chiefly, in certain marked changes in the social and business habits of the people. Especially is this true of such habits and occupations as, either in their kind or degree, exhaust the power and augment the sensibilities of the nervous system, or tend to destroy a healthy equilibrium between the development and action of the nervous, as compared with other systems of the body. As society becomes more refined, such habits and occupations as have been mentioned will multiply. Within one generation, they have multiplied many fold in this country.

Among them, we may mention an increase in sedentary and mental occupations; in exciting professional labors in all the learned professions; in the exigencies, and often relentless demands, of journalism; in the nervous and mental strain connected with the conduct of vast commercial and other enterprises; and in the prevalent speculative transactions in our stock-boards and gold-rooms; in the various customs and practices of social life; in the inordinate use of stimulants and sedatives; in exciting forms of literature; in vicious systems of education; and in an increased prevalence of diseases affecting the more important viscera, such as those of the abdomen and pelvis. To this it must be added, that many of the changes induced in the nervous system, in the ways mentioned, are transmitted, in some degree, to the succeeding generation, which begins its course more neurotic than did the one that preceded it.

In such directions must we look for the causes of an increased prevalence of nervous and mental diseases.

We do not believe we misstate the real feeling of many members of the profession, when we say they are looking for more information than is easily accessible to them, in regard to the nature and treatment of such diseases. To discuss the nature, and, according to the best light we now have, the practical management, of disorders of the nervous system, is one of the chief aims of the Editors in projecting their JOURNAL. Then, it is hoped to render it a worthy medium for reporting for and to the profession, many observations of scientific and practical interest. Such cases must often occur, and ought to be made known; and we wish to announce that the columns of the JOURNAL are always open to communications having real scientific or practical value in relation to the nervous system and mind. Some such medium as the one proposed has become all the more necessary, since, in this country, at present, we do not have more than one periodical devoted, even in a partial way, to the field we have in view; and if any special department in medicine can make good its title to journals devoted to its interests, surely the one we propose to occupy is one of them.

Finally, it is one of our leading purposes to make the JOURNAL a means for collecting, from all accessible sources, the latest and most useful information in regard to the nervous system and mind, both in health and disease. One only needs to look abroad, at present, or to inspect current files of leading foreign journals, to be impressed with the remarkable activity that now animates the professions in Europe, in relation to the nervous system and mind. We will try to keep our readers abreast of progress in this direction. So much, in part, as to the *aim* of the Editors in undertaking their enterprise.

As regards the *plan* of the JOURNAL, a few words may not be out of place. Its contents will be disposed as follows:

I. ORIGINAL ARTICLES, LECTURES, AND TRANSLATIONS OF ARTICLES OF MERIT, AND SELECTIONS FROM FOREIGN PERIODICALS. As already intimated, the Editors will use their best endeavors to secure original matter for this department of the JOURNAL; but expect, for the present, to fill it chiefly with articles of suitable length, possessed of real scientific and practical value, from foreign periodicals and other works. In this respect, our resources will be ample.

II. REVIEWS AND BIBLIOGRAPHICAL NOTICES. This department ought to be one of the most valuable. We shall spare no pains to secure every new work or monograph of real merit, touching the nervous system and mind, with the aim of placing before our readers what they contain that may be new and valuable.

III. EDITORIAL DEPARTMENT. Under this head, we shall discuss such topics as have a vital interest in the range of the JOURNAL, but which cannot find a congenial place in any of the other departments.

IV. PERISCOPE. In this department of the JOURNAL will be found condensed articles and short notices of such items of interest as are not agreeable to the plans of the preceding divisions. The matter of the *Periscope*, as will be seen, is comprehended under three heads, as follows:

a.—The Anatomy and Physiology of the Nervous System, and notes on healthy psychology. Under this head will be collected, so far as the space devoted to it will permit, all discoveries of importance relating to these topics. In some cases, short articles may be admitted into the *Periscope;* but, as a rule, the matter will consist in brief abstracts or notices.

***b.**—Pathology of the Nervous System and Mind.* This title explains itself; and what has been said of the first division, under this general head, is applicable to the second and also the third.

***c.**—Nervous and Mental Therapeutics.* Particular attention will be given to this subject. It is our aim to place before our readers a clear account of every means that may be discovered that is valuable in the treatment of nervous and mental disease. The *Periscope*, as will be seen, comprises over thirty pages of close print: and, while the number of pages may vary with each issue, the Editors intend this first number shall be not far from an average specimen, as regards size. The remainder of the space will be occupied, hereafter, under two separate heads, viz.: *Miscellaneous Notes;* and *Short Notices of Books and Periodicals Received, and Bibliography.*

Such is an outline of the *aims* and *plans* of the Editors in behalf of the JOURNAL. We trust it may meet a real want in the profession. We do not ask for it any favor which the profession will not readily bestow, or that is inconsistent with a spirit of true independence.

NECROLOGY.—Dr. Charles Legros, the eminent French physiologist, died in Paris on the 25th of December last, at the age of thirty-nine years. The cause of his death was an infectious fever, supposed to have been contracted during his labors in the histological laboratory, which carried him off after only a few days' illness, the circumstances and conditions being the same as those which attended the death of the illustrious Bichat.

Though still a young man at the time of his death, Dr. Legros has left behind him a worthy monument in his numerous valuable contributions to medical science, more especially in the department of physiology. Among these we may notice, as coming within the province of nervous physiology and therapeutics, his memoirs on the vaso-motor nerves, on the physiology of the pneumogastrics, and on electro-physiology and therapeutics—the latter in connection with M. Onimus. Only a short time previous to his decease, a paper under their joint authorship appeared in the *Comptes Rendus*. Much was to have been expected from his ripened experience and abilities, and his premature death is a loss to science and the world.

Archives of Electrology and Neurology. — We have received the *prospectus* of a new medical journal, soon to be issued, semi-annually, with the above title. It is to be edited by Dr. Geo. M. Beard, of New York.

The Editor is well known to the profession, especially as one of the authors of one of our best and most practical works on the medical uses of electricity. From the plan that is set forth in the prospectus, we learn that it is to occupy a field similar to the one occupied by this journal. Each number will comprise from one hundred to one hundred and fifty pages. Terms of subscription not announced.

It gives us sincere pleasure to learn that such a journal as the one proposed is to be established in New York, with so active and accomplished an Editor. Most heartily do we wish the enterprise success.

For terms of subscription, and all that pertains to the business management of the Journal, see third page of cover.

Periscope.

a.—ANATOMY AND PHYSIOLOGY OF THE NERVOUS SYSTEM.

BRAIN.—C. Golgi (*Gaz. Med. Ital.*, Series VI., Tom. VI., 1873; abstr. by Boll, in *Centralblatt der Med. Wissensch.*) gives particulars of the structure of the cortical substance of the brain, obtained by means of a new process of preparing the tissues for microscopic examination. His method consists in leaving the piece of nervous tissue, after it has been hardened by bichrom. potass., for a considerable time in a silver solution of one-half to one per cent., until the nervous elements are completely blackened. By this method, he has been enabled to trace the basal process of the pyramidal cells to a much greater distance than any previous observer—as much, indeed, as six hundred micromm.; while the greatest distance attained before, by Koschewnikoff, was only one hundred micromm. For the distance of twenty to thirty micromm., Golgi describes the process as smooth and almost straight, and gradually diminishing in diameter. At that point, there is generally a tendency to become slightly crooked, or wavy, in its course; and then, some twenty or thirty micromm. farther, it begins to give out side branches. These sometimes commence to appear immediately behind the first sinuosities, and they continue to be given out, at regular intervals, as far as the axis cylinder process can be followed. They present a regularly smooth appearance, follow a slightly sinuous course, and become gradually finer, until lost to view. The secondary branches of the axis cylinder cannot be distinguished from some of those from the other anastomosing processes. They leave the axis cylinder process at a right angle, and, after a longer or shorter horizontal course, they turn outward toward the surface of the cerebrum; other branches leave these, and so on, to the third and fourth order, the whole arrangement showing a great resemblance to the divisions and subdivisions of the corneal nerves.

Golgi considers that he has proved that the final terminations of the branching process is in the connective tissue cells (Deiters' cells) of the cortical substance. This, he states, is true of the branches of the angular, as well as of the basal, processes, but is much easier demonstrated in the former.

In the cerebellum, also, the axis cylinder process of the Purkinje cells divides and subdivides in the same manner as that of the pyramidal ganglion cell.

Constitution of the Gray and White Substance of the Brain.—D. Petrowsky (*Pfluger's Archiv*, 1873: 367) gives the following as chemical constituents of the brain: Both the gray and white substances contain albumen, which is soluble in chloride of sodium; and both alike have about fourteen per cent. of undissolved material, in part phosphorus, in the gastric juice. The white substance is decidedly richer in constant constituents than the gray. The gray substance is more than one-half albumen, the white only about one-fourth. The former contains eighteen and seven-tenths per cent. cholesterin and fat, and five-tenths of one per cent. cerebrin ; the other contains fifty-one and nine-tenths per cent. of the first, and nine and five-tenths per cent. of the cerebrin. Lecithin is, however, found more abundantly in the gray substance than in the white.

J. J. Putnam (*Boston Med. and Surg. Jour.*, Sept. 11, 1873), describes Prof. Meynert's method of examining the brain.

The Pia Mater as a Coat of the Cerebral Vessels. — Dr. J. Batty Tuke —(*Session of Psychological Section Br. Med. Ass'n*, Aug. 7, 1873).—Dr. Tuke illustrated his paper by a series of microscopic preparations, in which he expressed his opinion that the so-called hyaline membrane, on the arteries of the brain, is really the normal sheath of the vessel, thickened by disease. He alluded to the statement of Rindfleisch, that the cerebral arteries do not enter naked ; and showed specimens in which he believed he could trace a thickened pia mater, graduating off into a purely hyaline membrane, as it penetrated the deeper portion of the brain. He also exhibited specimens in which a distinct membrane could be traced, apart from the vessels, lying in vascular tracts, from which the other coats had been removed. With regard to the hyaline membrane of the vessels of the pia mater, he coincided with the opinions of Gull and Sutton, as to its existence, but not with their theory of its formation, as he had found it constantly in cases in which no disease of the heart or kidneys existed. He believed it to be due to a thickening and opacity of the pia mater immediately investing the vessels, caused by hyperæmia ; that it was brought more prominently into view by the employment of re-agents, but that it could be seen without their use; that where it could be demonstrated in the pia mater, it could be traced, by careful dissection, passing inwards, as a sheath ; and that it could be more easily found in cases of disease than in healthy subjects. The general tenor of the paper was to indicate the existence of an anatomical sheath of the cerebral arteries, formed by an extension inwards of the tomentum cerebri, supporting the views of Lockhart Clarke, and Robin.—*British Med. Jour.*, Oct. 11 1873.

SPINAL CORD. — Pierret (*Archives de Phys.*, 1873 : 534) discusses the structure of the posterior columns of the spinal cord. He sums up the various data from embryology and normal and pathological anatomy as follows :

"The posterior column, in the human subject, contains, like the lateral ones, a great number of longitudinal commissural fibres, of various origin, some of which are of such length as to connect the lumbar enlargement with the posterior pyramids.

"These fibres rise from all points of the gray matter situated in front of the cervix cornu posterioris, and probably possess intimate relations with the posterior vesicular columns of Clarke.

"The fibres which approximate nearest to the commissure, seem to be the ones with the shortest track.

"By their union, these different fibres constitute a fusiform bundle, which contains the greatest number of them in the upper third of the dorsal region. In the cervical region, this bundle appears distinctly divided into two cuneiform bands, and is a little diminished in volume. This persistence of the division of the median column, in the cervical region, is a remnant of the condition which, in fœtal life, existed throughout the whole extent of the cord.

"The most superior fibres terminate, in part, in the ganglionary nucleus of the posterior pyramids.

"In general, the fibres of the median bundle are finer than those of the external lateral portions of the posterior columns of the spinal cord.

"The exterior parts of the posterior column originate directly from the radicular zones, which, in the embryo, appear some time previous to the posterior median bundles. Extending along the whole length of the cord, and within the posterior *cornu*, they do not have very intimate relations with the median portions, as is demonstrated by pathology, but are constituted by the different internal radicular fibres, which all, or nearly all, return into the gray substance of the anterior cornua, or into the radicular columns of Clarke, for the dorsal region.

"The different structure, the lack of synchronism in development, the different relations, and, finally, the special symptoms which result from the sclerosis of the one or the other of these two portions of the posterior columns, force an *a priori* conclusion that the functions of the two regions are different.

"The knowledge of the functions is to be obtained by a critical comparison of all the works on the posterior columns, or roots, up to the present date, and by experiments, instituted and followed up by the aid of the data contained in this work."

Variations in Nerves.—Dr. Jno. Curnow (*Jour. Anat. and Phys.*, June, 1873 : 308) describes a number of variations in the origin and course of nerves. Among these are the fission of the inferior dental, as far as the entrance into the dental canal ; the milo-hyoid, arising by a head from each division. The spinal accessory, in one case, stopped at the sterno-mastoid, leaving the trapezius to be supplied by branches from the third and fourth cervical ; in another, it divided into two branches, of which the larger entered the sterno-mastoid, and the other joined a branch of the second cervical, which, with another branch from the fourth, supplied the trapezius. These two show that the trapezius is probably solely supplied by the spinal fibres of the spinal accessory. In one case, the phrenic nerve was joined by a large branch from the middle cervical ganglion. In another, it supplied the anterior scalenus muscle, and then divided into two branches, between which passed the internal mammary artery. In still another instance, it was noticed, fissured from its origin at the fourth cervical, as far as the upper part of the thorax. The serratus magnus was not infrequently supplied by two nerves, a separate branch from the fifth cervical going to its first digitation. In one instance, a second, from the same origin, passed to its second and third digitation, while the rest of the muscle was supplied by a separate nerve from the sixth cervical.

Several instances of abnormal origin and distribution of the nerves of the arm are described; and, in one dissection, a very complex arrangement of the nerves from the lumbar plexus was noted, the ilio-hypogastric, ilio-inguinal, genital branch of genito-crural, anterior and posterior divisions of external cutaneous branch to iliacus, small branches to psoas, anterior crural, middle cutaneous, dividing into two branches before passing under Poupart's ligament, obturator, and accessory obturator nerves all being given off separately.

Proportion of Nerve and Muscle.—P. Tergast (*Schultze's Archiv*, IX.: 36-46; abstr. by Boll, in *Centralblatt der Med. Wissensch.*) has studied the proportion of the number of primitive muscle fibres to the number of primitive muscle bundles, in the eye muscles of the sheep. In these muscles, as also in the sartorius of the frog (Kuhne), the primitive bundles extend the whole length of the muscle; so Tergast counted the separate severed bundles, as they appeared in a cross-section through the middle of the muscle. The primitive nerve fibres were similarly determined from the cross-section of the nerve.

The results gave to one nerve filament in the obliquus inf., 3-4, in the obliquus sup., 6-7, in the rectus inf., 7-8, in the rectus med., 8, and in the rectus later., 10, primitive muscle bundles.

Therefore, in the cross-section of the eye muscles of the sheep there are some six to seven muscular bundles to every primitive nerve fibre. In the human species, for every seven primitive muscle bundles there are fully three nerve fibres.

Muscles which are not required for so exact a function as are those of the eye, receive a very much less number of nerve fibres. For example, the biceps of a young dog receive only one nerve fibre for eighty-three muscle fibres; and the sartorius of the same animal only one to forty or sixty. In the frog, the cutaneous pectoral muscles contain one nerve fibre to twenty-three or twenty-seven muscular fibres (Reichert); in the abductor minimi digiti, the proportion is one to forty, and, in the sartorius, one to sixteen and a half. In the ocular muscles of the frog, the proportion in the cross-section amounts to one to ten. In the muscles of the tail, in the mouse, there are present twenty-eight or twenty-nine muscular fibres to each nerve fibre.

Tergast has been able to discern divisions of the primitive muscular bundles, and also anastomoses, in the ocular muscles of the sheep—though they occur but rarely—and has figured some instances.

Alcohol and Absinthe.—M. Magnan (*Archives de Phys., Norm. et. Path.*, March, 1873, and *Revue Scientifique*, No. 36, 1873; abstr. in *Rev. des*

Sci. Medicales). The first work contains: 1st. The comparative experiments on the action of alcohol and of essence of absinthe; 2d. The experiments with absinthe, with the object to determine, during the convulsive crises, the influence respectively exercised by the brain, the bulb, or the cord, and the state of the cerebral and retinal circulation.

The immediate action of alcohol, in sufficient doses, reduced the animal to complete drunkenness. When a dog was submitted to the action of the poison for a sufficient time, there followed, about the fifth day, some irritability and impressionability; in ten days, nocturnal hallucinations, starts of fright, and insomnia, and, later the same day, persistent delirium. The troubles of motility appeared from the second month. The tremor commenced in the feet, passed to the trunk and the head, and augmented progressively in duration, extent and intensity. Never, in the experiments with alcohol, was an epileptic attack produced; while that, in reality, is the principal symptom in poisoning from absinthe. The dog, as in the human species, suffered from digestive disturbances, the more severe the younger the animal, and finally succumbed, from accidents or complications, resembling, in all points, the human pathological manifestations. The anatomical lesions of alcoholism, in the dog, consist of various degrees of fatty, etc., degenerations (liver, kidneys, and heart), and also in the tendency to various chronic irritations of the meninges, the cord, and the pericardium.

In small doses, absinthe caused vertigo and convulsive movements of the head and anterior portions of the body; large doses caused epileptic attacks and delirium. Absinthe produces delirium from the first day, while alcohol requires a certain time to prepare the ground for its hallucinations and delirium.

Animals deprived of the cerebral lobes, as well as those which had suffered no mutilation, exhibited the epileptic seizures; but after the division of the cord below the bulb, the attack became complete twice as quickly. First were produced tonic and clonic convulsions of the head, with foam on the lips (*attaque bulbaire*); then tonic and clonic convulsions, with expulsion of the urine and fecal matters (*attaque spinale*). Each portion of the cord contributed its part in the production of the assemblage of symptoms which constitute the complete seizure; further, the loss of consciousness during the attack, and the existence of delirium before and after, show an agency of the brain exercised simultaneously with those of the medulla and cord, but each acting in its own way. From these facts, it is seen that the complete attack depends on a general cause, and not on the exclusive action of any one of the nervous centres.

Ophthalmoscopic examination of an animal poisoned with absinthe, shows an active congestion of the retina, from the commencement of the attack to the first (tetanic) stage; at the same time, the pupil is dilated. The association of these two phenomena—retinal congestion and dilation of the pupil—does not accord with what is usually stated by authors.

If, after trepanning the skull, absinthe is injected into the femoral vein of a dog, there is, in the first stage of the symptoms, which correspond to a simple epileptic seizure, a tetanic rigidity and loss of consciousness, accompanied by an intense congestion of the encephalon. There is no order of succession of these phenomena; they are simultaneous—tonic convulsion and cerebral congestion producing, at the same time, dilatation of the pupil and injection of the papilla and base of the eye-ball—a group of symptoms which cannot be easily explained by the theories of epilepsy at present entertained, and which suggest a direct agency, acting simultaneously on the whole cerebro-spinal axis.

The study "On the disturbances of intelligence of the senses in alcoholism," shows the clinical value of physiological experiments. After having followed, in the dog, through phases neatly defined, the gradual progress of the intellectual troubles, one can better understand the succession of phenomena in man, in whom, also, absinthe provokes a premature delirium. Of the two active elements in liquor of absinthe, alcohol and absinthe, the latter produces delirium and hallucinations before the former has had time to affect the nervous centres sufficiently to produce tremor, as we see, for example, in certain individuals, after excesses in drink, a delirium without tremor, on account of the ingredients mixed with the alcoholic beverages. The general characters of alcoholic delirium are produced later; and the analysis of the symptoms shows the order of appearance of the diverse forms —maniacal, melancholic, and stupid delirium, which, depending on the cause, are generally only transient.

The troubles of intelligence are examined in the intermediate period between acute and chronic intoxication, and, finally, in that last phase, in which the individual gradually merges toward dementia, or toward general paralysis, according to the predominance, in the nervous centres, of fatty degeneration (atheromatous or steatose), or of chronic irritations (diffuse interstitial sclérosis).

P. Daub ("*Neue Versuche ueber Alkohol und Korperwarme;*" *Centralblatt der Med. Wissensch.*, No. 30, July 5, 1873), from a series of careful experiments, concludes that alcohol has a general lowering effect on the bodily temperature, although two exceptions were observed, both in cases of chronic disease of the bones of the lower extremity. He considers Rabow's observations defective, from the method employed, the thermometer in the axilla being liable to error from various local causes. Daub employed the thermometer in the rectum, though often simultaneously in the axilla, two instruments being employed.

Dr. Binz read a paper on this subject before the British Association for the Advancement of Science (abstr. in *English Mechanic*, October 10), showing that the action of alcohol on the heart had the effect of reducing the temperature of the body 3° or 4° Fahrenheit. The alleged heat, he said, of the organism, from alcohol, did not exist. The subjective impression was, at least partially, the consequence of an irritation of the nerves of the stomach, and of the enlargement of the vessels arising in the skin. When given in small quantities, the thermometer showed no extraordinary increase or decrease of the temperature of the blood. Moderate doses, however, showed a distinct decrease, of about half an hour's duration; while strong inebriating quantities evinced a still more decided lowering, lasting several hours. The decrease, after moderate doses, took place most successfully in warm-blooded animals which had not, for some time previously, had alcohol administered. By injecting a cubic centimetre of ichor under the skin of rabbits or dogs, death was produced in a short time. This, however, was not the case, if, with the ichor, diluted alcohol was administered, either by the stomach or the skin. In the latter case, the animal was quite lively, and relished its food ; so that alcohol, he contended, need not be a narcotic. His experiments, he said, showed a three-fold action, viz.: the diminution of the heat of the body, reduction of the putrid processes, and raising of the action of the heart. Alcohol was more than a simple stimulant ; it was a strong antipyretic, and an equally powerful antiseptic. It was, *a priori*, to be expected that alcohol would not be without its influence on the metamorphosis of tissues. An agent that, consumed in large doses, clearly lowered the combustion, must also be supposed to decrease the urea and the carbonic acid. This was, in reality, the case. Irritation and functional disturbance took place, when, in our moderate climates, we indulged too freely in the use of alcohol.

Dr. James Ross (*Brit. Med. Jour.*, Oct. 5, 1873), "On the Action of Alcohol," maintains that the physiological effects of alcohol are to be explained by its property of consolidating and solidifying structure. By this hypothesis, he explains its effect in checking growth, and in producing the various morbid phenomena caused by its habitual use. The diffusibility of this agent explains its effect on the nervous organisms, whose delicate structure and lack of density permits them to be earlier affected. The stimulant effect of alcohol, so far as it is real, is due to the nervous energy set free by the consolidation of the soft nervous tissues.

In a later issue of the same journal (Oct. 25), Dr. Ross calls attention to the fact that his views had, unknown to himself, been anticipated by Dr. Lionel Beale, in a paper entitled, "On the Deficiency of Vital Power in Disease, and on Support, with Observations upon the Action of Alcohol in Serious Cases of Acute Disease." London : 1863.

Camphor.—Dr. J. Hawley (*Practitioner*, Oct., 1872) gives the following conclusions from his experiments : Camphor, according to him, acts principally on the cerebral lobes, causing depression, giddiness, and somnolence. The corpora striata seem to partake in the general effect of the drug. Later, delirium appears, sometimes attaining considerable intensity. Long continued use of the drug causes muscular weakness and mental lethargy. In strong medicinal doses, it affects none of the organic functions, except that of generation ; and this may be considered as secondary to its general depressing influence on the intellectual and motor centres. There seems to be no evidence that it has any lowering effect on the circulation ; on the contrary, there is sometimes observed a well-marked stimulation, accompanied with a feeling of increased heat of the body, and some little actual elevation of the superficial temperature. When administered in solution, under proper circumstances, there seems to be no doubt that camphor is completely and rapidly absorbed ; it also appears to be decomposed in the system with equal rapidity.

Physiological Action of Ammonia Salts.—M. Ch. Robin presented to the French Academy, at its session, April 7, 1873, a note from M. Rabuteau relative to the toxic effects of the iodides of tetramethylammonium and tetramylammonium. According to this investigator, they appear to be paralyzers of the motor nerves, and to have effects completely analogous to those of curare.

Apropos to this note, a communication was received by the Academy, at its session, June 5, from Messrs. A. Brown and Th. Fraser, calling attention to a memoir read by them before the Royal Society of Edinburgh, entitled, "On the Physiological Action of the Salts of Ammonia, of Trimethylamine, and of Tetramethylammonium." In this they claim to have noted, not only the paralyzing action of the salts of trimethylammonium, and other analogously constituted bodies, but also, from numerous observations, to have arrived at the general conclusion that the paralyzing action on the terminal organs of the motor nerves is a general property of the salts of the quaternary ammonia bases.

Physiological Action of the Salts of Silver.—Prof. Ch. Rouget (*Archives de Phys.*, July, 1873), publishes the details and discussion of a series of experiments, conducted by himself, as to the physiological effects of the absorption of the nitrate and hyposulphite of silver on various animals. His conclusions are as follows:

"Whatever the various accidents which follow, in different species of animals, the introduction into the organism, by *absorption*, of the salts of silver, these accidents are always the direct consequence of the intoxication of the elements of the encephalo-rachidian nervous centres, complicated, in some instances, by the intoxication of the elements of the muscles of animal life.

"The blood, which carries to the tissues the toxic material received through absorption, does not appear altered, either in its elementary constitution or its normal properties."

His conclusions thus differ from those of Krahmer and Rabuteau and Mourier, who maintained that these salts caused death through changes effected in the blood, and agree with those of Orfila and Charcot and Ball.

Caffeine, Theine, etc.—Dr. Alexander Bennett (*Edinburgh Medical Journal*, October, 1873: 323) publishes the result of a series of investigations, comprising over one hundred experiments, seventy-two of which are tabulated in the article, on the physiological actions of theine, caffeine, guaranine, cocoaine, and theobromine. The conclusions arrived at are stated as follows:

1. The physiological actions of tea, coffee, guarana, coca, and cocoa, are nearly, if not entirely, due to their proximate principle.

2. Theine, caffeine, guaranine, cocoaine, and theobromine, are powerful poisons, inducing a series of symptoms affecting the nervous, respiratory, circulatory, vaso-motor, and glandular systems, which terminate, if the dose be large enough, in death.

3. These five principles are, to all appearances, identical in physiological action.

4. In small doses, not ending fatally, these five substances produce, first, cerebral excitement, not succeeded by coma: and, second, partial loss of sensibility.

5. In large doses, they produce, first, cerebral excitement; second, complete paralysis of sensibility; third, tetanic spasms and convulsions; and, fourth, death.

6. They paralyze the entire posterior columns of the spinal cord; also, the entire system of peripheral sensory nerves; but the anterior columns of the cord and the peripheral motor nerves are not paralyzed.

They frequently produce convulsions of a chronic character; but occasionally they cause tetanic spasms, which latter are sometimes so severe as to cause opisthotonus. There is, at first sight, a resemblance between these spasms and those following the administration of strychnia. But in the case of strychnia, the action of the poison is limited to the spinal cord, the reflex function of which is so much excited that the slightest touch causes powerful spasms. A poisonous dose of theine, caffeine, etc., on the other hand, paralyzes the sensory nerves, so that external irritations do not affect the cord; but, notwithstanding, there are strong spontaneous spasms, which are probably caused by the action of the drug on the cord itself, and which spasms are not to be considered as reflex in their nature.

8. They do not produce muscular paralysis.

9. They at first increase, then impede, and, lastly, stop the respirations

10. They at first increase, and, finally, diminish, both the force and frequency of the heart's contractions.

11. They produce, at first, contraction, and afterward, dilatation, of the capillaries and small blood-vessels, with stasis of the blood, indicating, at first, irritation, and subsequently, paralysis, of the vaso-motor nerves.

12. They affect the temperature by, first, slightly lowering, and, second, increasing it.

13. They usually produce contraction of the pupil.

14. They produce an increase of the salivary secretion.

15. They induce a peculiar form of tenesmus, accompanied by a copious discharge of clear mucus from the bowels.

Seven experiments are given in detail, with comments.

M. Pasteur presented, at the session of the French Academy, August 4, 1873 (reported in the *Gaz. Med. de Paris*, August 23), a note from M. E. Roux on the action of tea and coffee on the excretion of urea. The experiments were made by the author on himself. Under a careful and regular system of diet, work, etc., the amount of urea excreted daily was systematically noted for a period of five months, and the following conclusions reached:

Coffee and tea, contrary to the usual opinion, have the effect, at first, to increase the daily amount of excretion of urea.

With the continued use of these substances, the quantity of urea daily excreted returns to the normal figure but does not descend below it.

The author concludes that tea and coffee do not decrease the waste of tissue. He proposes to seek the cause of the apparent diminution after their continued use; whether it be that their first action is to hurry the elimination of urea accumulated in the tissue, and this filtration having been completed, the normal conditions are established; or, whether we are to understand the phenomenon as the results of a habit to which the organism has adapted itself. These questions are not answered by his researches.

In commenting on the above, Dr. R. Lepine states that the conclusions of M. Roux accord with those of Voit, drawn from some remarkable experiments on dogs. Voit denied that coffee lessened the excretion of urea, and explained its remarkable effects by an action on the nerves. Squarey also made experiments on three persons, and came to the same result: that it did not sensibly influence the excretion of urea.

The various authors who have held the contrary view will now have to bring forward new experimental proofs to establish their views.

Contribution to the Physiology of the Vagus.—MM. Arloing and Tripier, *Archives de Phys.*, Nos. 4, 5, and 6, 1872, and No. 2, 1873 (abstr. by A. Joffroy, in *Rev. des Sci. Méd.*).—The conclusions formulated by MM. Arloing and Tripier are as follows:

1. The section of the cord behind the rachidian bulb considerably diminishes the excitability of the pneumogastric.

2. There exists a notable difference between the two vagi, principally in a functional point of view. The right acts more energetically on the heart than the left.

3. The arrest of the heart is more complete when the galvanization is performed only on the peripheral end of the nerve, instead of acting on the nerve intact.

4. The galvanization of the peripheral end arrests the heart's action in diastole, while that of the central end tends to arrest it in systole. Hence it results that the action of the pneumogastric on the heart is a special, moderating one.

5. The movements of the heart produced during galvanization of the vagi are more feeble than before the excitation; nevertheless, the pulse takes a greater development, because the arterial tension is feebler.

6. The excitation of the vagi by a feeble current does not produce, in the experience of some authors, any acceleration of the respiratory movements (Rosenthal; P. Bert). In reference to the arrest of respiration, in expiration or inspiration, the conclusions arrived at in this work approach more nearly those of Bert than those of Rosenthal.

7. The galvanization of the peripheral end makes itself felt in the respiratory movements, probably because the vagi send towards the periphery recurrent fibres.

8. The section of one vagus is accompanied by the enfeebling of the thoracic movements of the corresponding side.

9 Finally, it does not appear that one of the vagi is specially set over the function of digestion.

Let us compare these conclusions with those of a work by M. Masoin, published a little before, and entitled, "*Contribution a la physiologie des nerfs pneumogastriques. Difference entre le pneumogastrique, droit et le pneumogastrique gauche, pour leur action suspensive sur le cœur.*" (Extracted from the Bulletin of the Royal Belgian Academy, VI., 3d series, No. 4.) M. Masoin establishes:

1. That the two vagi, or still more, the two accessory nerves of Willis, do not act in the same manner on each side of the heart. The right nerve possesses a much greater power than the left, to slacken or arrest the action of that organ.

2. That the restraint on the action of the heart, emanating from the myelencephalon, operates as a whole, without any special exclusive action of either the right or left pneumogastric.

The independence of the researches of M. Masoin, and those of MM. Arloing and Tripier, only gives them still greater importance.

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b.—PATHOLOGY OF THE NERVOUS SYSTEM AND MIND, AND PATHOLOGICAL ANATOMY.

Pathology of Tetanus.—In a discussion in the Surgical Society of Paris, M. Verneuil made some interesting remarks on the Pathology of Tetanus. They are reported in the *Bulletin de Therapeutique*, vol. 84, p. 41. The following is a resume: He remarked that, patients attacked with tetanus, presented a relatively satisfactory condition, so long as the thermometer did not show any considerable augmentation of temperature. The appearance, however, of this last symptom, it was, that rendered the prognosis grave. He asked the question, What was the cause of this elevation of temperature? It was his opinion, it was not to be "attributed either to myelitis of the superior part of the cord, nor to the muscular contractions, nor to asphyxia." But he does not appear to have given any solution of the question, as to what is the cause of the sudden rise in temperature. The cause of death, however, he attributes, and not without some reason, to inflammation of the lungs, developed with extraordinary rapidity, under the influence of a reflex excitation from the superior part of the spinal cord, through the agency of the pulmonary and bronchial vaso-motor nerves. In conformity with this view, M. Verneuil had found more or less extended pulmonary and bronchial inflammatory troubles, in *post-mortem* examinations. (*Bulletin de Therapeutique*, vol. 84, p. 41.)

M. A. Muron (*Gaz. Med. de Paris*, 1873, Nos. 26, 28 and 29) gives the results of researches made by himself in the laboratory of Prof. Beclard, and read before the Societie de Biologie, at its *seance* of June 14th, on the cause of the elevation of temperature in tetanus. From the results of his experiments, excluding all hypotheses, and basing his conclusions on the facts which are apparently established, he believes it safe to say that, "The essential cause of the heat produced in tetanus, is the muscular contraction."

SLEEPING SICKNESS (MALADIE DU SOMMEIL).—The *Gaz. Medicale de Paris*, of Nov. 29, 1873, contains a review of a paper presented by Dr. Manuel F. Ribeiro to the *Sociedade das Sciencias Medicas da Lisboa*, on a curious disease observed by him in the Island of Principe. The principal symptom, and almost the only one, is an irresistible desire to sleep. It seemed confined to subjects of African, or mixed blood, and, as far as observed, was always fatal.

A similar disease is mentioned by Dutrouleau, as indigenous to the coast of Africa; and the English Naval Surgeon, Davis, stationed at Lisbon, furnished further particulars of its occurrence in Africa and elsewhere, and gave several hypotheses which had been suggested for its cause, among which were the use of various vegetable products, malaria, etc.

Dr. Amado de Silva, reporting on the communication of Dr. Ribeiro, was disposed to consider melanæmia as the cause of the affection.

INVOLUNTARY MOVEMENTS OF PARALYZED MEMBERS.—Prof. Westphal (Berlin), describes (*Sitzungber. der med.-psychology Gesellsch.;* 17 Dec., 1872—abstr. in *Allg. Med. Central Zeitung*, 1873, No. 55) a phenomenon observed by him in adult hemiplegics, whose paralysis had existed since earliest infancy. The peculiarity is, that when movement of the corresponding sound member is made, the other, altogether incapable of independent voluntary motion, follows suit, making the same motion, so far as the contractions and stiffening of the joints, etc., will permit. These involuntary movements are most noticeable in the most completely paralyzed members, while those which are still capable of voluntary movement to a very limited degree show them only partly, or not at all. Passive movements of the sound extremity, as well as the flexion and extension produced through faradization, caused no corresponding motion of the paralyzed side. In one case, the sensibility of the skin of the paralyzed side was slightly diminished, the consciousness of the position of the joint abolished, and the sensation of muscular fatigue appeared also to be wanting. At least, if the sound hand was fatigued by frequent and rapidly repeated motions, accompanied by corresponding involuntary movements of the other hand, the latter, notwithstanding its much weaker muscular power, suffered no sensible weariness. Reflex movements of the sound side (excited by pricking) were followed by corresponding ones on the other side of the body. In one case, in which there yet remained slight voluntary power on the hemiplegic side, its movements were accompanied by corresponding ones on the sound side.

As to the signification of this phenomenon, Westphal holds that, with present knowledge, the view must be adopted that, originally, the will-impulse which, for instance, acts through the right hemisphere on the left half of the body, also passed over through commissural fibres to the left hemisphere, and, by its agency, acted on the right extremities; but, through habit, we have learned to suppress the movements thus caused. A trace of this tendency to symmetrical motions is still to be seen in certain motions; for instance, the difficulty we experience when we attempt to move both arms in circles in opposite directions. If we now suppose that, in consequence of a lesion in infancy, the locality for the restraint of the voluntary impulse in one, say the right half of the cerebrum, be destroyed, while the motor centre in the nucleus lenticularis, etc., of the same side remains intact, then it is clear that there would be, during the voluntary motions of the left extremities, no cause to restrain the corresponding action of those of the right side excited through the commissural fibres between the two hemispheres. It is, of course, not easy to establish this theory as absolutely true. It would be interesting to observe in the autopsy of such a case whether the location of the lesion were in the hemisphere itself, or in the motor ganglia.

ASTHMA.—J. B. Berkart (*Brit. Med. Jour.*, Nov. 8, 1873) combats the prevailing opinion that asthma is of nervous origin, and maintains that it is due to a lack of elasticity of the pulmonary tissue, and that it holds an intermediate place between bronchial affections and emphysema. In consequence of this deficient elasticity, the respiratory action is embarrassed, and only effected after increased efforts. He refers to the failure, in some of Bert's experiments to prove the contractility of the lungs, on account of over-insufflation, as proving the absence of nervous agency in cases of asthma occurring during the existence of emphysema, and as an integral part of that disease.

ANOREXIA HYSTERICA.—Sir W. Gull, at the session of the Clinical Society, Oct. 24, 1873 (reported in *Brit. Med. Jour.* Nov. 1), read a paper on an affection, first referred to by himself in 1868 under the name of *apepsia hysterica*. In the paper read he had preferred the name *anorexia hysterica*, which had been applied to the condition by Dr. Lasegue, of Paris, as more consonant with the facts; as lack of appetite for food, and not failure of digestion, characterized the disease. The disease occurs, mostly, in young women between the ages of fifteen and twenty-three, and was characterized by extreme emaciation, and was often considered to be due to latent tubercle or abdominal disease, or to the so-called atrophy. In the one fatal case, in which death was due to starvation and thrombosis of the femoral artery, the *post-mortem* showed no tubercular or other lesion. He was disposed to consider the want of appetite as due to a morbid mental state, and hysterical condition of the pneumogastrics of central origin. He might use the term hysterical, in naming the disease, though without regarding the strict etymology of the word, or assuming that the subjects of this affection presented the ordinary symptoms of hysteria. In regard to the treatment, he advised moral control and feeding; regarding medication by drugs as of only secondary importance.

In the discussion that followed Dr. Quain differed from the author of the paper in regarding the disease as due to some real morbid condition of the digestive organs, and not to nervous influences; the cause being peripheral, rather than central.

At the same meeting, another paper was read by Dr. Gull, in which he described a cretinoid state, supervening in the adult, coming on at the age of forty and upward. The morbid change consisted in a general and gradual increase of bulk throughout the frame, with an especial change in the face and expression, and corresponding change in locomotion and mental activity. In four cases, the thyroid body was not enlarged; but he was unable, from the fullness of the tissues of the neck, to ascertain whether it was wasted or not, as it was found to be by Dr. Flagge, in cases of sporadic congenital cretinism. He had not observed the disease at all in males. As far as his experience allowed him to judge, treatment seemed to fail in producing marked improvement.

VASO-MOTOR CEPHALALGIA.—Dr. A. Eulenberg (*Berl. Klin. Wochenschr.*, 1873, No. 15) has proposed this name for an affection, observed by him in a young man, characterized by headache, coming on in the earlier part of the day, with a feeling of heat in the face, and intense redness of the cheeks and ears, of which the temperature rose, during the attack, to its maximum, that of the right ear being generally a little the highest. In the idea that he had to do with a periodic atony of the walls of the vessels, depending on a disturbance of their innervation, he prescribed ergotine, and had the satisfaction to see a decided amelioration of the symptoms.

In the same paper is described the case of another young man, suffering from mydriasis of the left eye, together with difficulty of accommodation. The near point of the sound eye was four and a half inches; that of the left, twelve inches. The patient had suffered, some time previous, a swelling of the left side of the neck, and, for a still longer time, a periodic pain in the left temple, with paleness of visage, and a sensation of greater coolness in the left ear than in the right, which was confirmed by the thermometer, in the outer auditory passage. On the inner side of the left sterno-mastoid were swollen glands, which were very sensitive to pressure, especially at the height of the upper cervical ganglion.

Eulenberg reçognized in these symptoms an irritation of the cervical sympathetic, through which the pupil became dilated, the vaso-motor nerves irritated, and the intra-ocular pressure increased, from which the resistance to the physiological lense modifications was magnified, and the power of accommodation diminished.

TASTE.—J. Jacubowitsch, *Meditsinsky Wiestnik*, 1872, No. 52 (abstr. in *Hoffman & Schwalbe's Jahresbericht*), gives an account of a leprous Cossack in whom there existed a remarkable condition of this special sense. If a bitter substance (quinine), or a sour one (dilute sulphuric acid), was applied to the tongue, the man was conscious of no sensation of taste; but, with the last-named substance, he felt a sensation of cold. He however readily distinguished sweet (sugar) and salty (table-salt) substances. If a mixture of salt and quinine, or of sugar and sulphuric acid, was employed as a test, he distinguished only a bitter, and in two cases, only a sweet, taste. This alienation of the sense of taste continued during the whole duration of the disease.

PULMONARY LESIONS IN CASES OF CEREBRAL APOPLEXY.—Prof. J. Hughlings Jackson, *Brit. Med. Jour.*, Oct. 25, 1873, calls attention to the hæmorrhagic infarctions observed in the pulmonary tissues in the *post-mortems* of certain cases of fatal comatosing lesions of the brain. The explanation of these appearances favored by the author is, that they are due to embolism of the pulmonary arteries, and therefore do not arise directly from the nervous lesions, but indirectly, and by a quasi-mechanical process, not due to any vaso-motor or nervous influence on the lungs themselves. He suggests that close attention be paid to the mode of death in cases of apoplexy, and especially to the alterations of circulation and respiration in cases of lesions, large or small, differently placed.

ABNORMAL IRRITATION OF OLFACTORY NERVE.—Dr. W. Sander (*Archiv für Psychiatrie*, Bd. IV., *Hft* 1, 234) describes the case of a man thirty-eight years old, who complained of a horrible (schrecklicher) smell coming on at the commencement of an epileptic seizure, and continuing throughout the attack. Consciousness appeared not to be entirely lost; he was sensible, at least, of the nature of the seizure. After his death, which occurred about a year after the onset of his malady, the *post-mortem* revealed, in the anterior part of the left hemisphere, a glioma, which involved and destroyed the left olfactory tract. Similar cases are quoted from the reports of other observers. This subjective sensation, occurring, as in this case, only at the beginning of, and during, the epileptic attack, is to be considered as an aura in the region of the olfactory nerve. Similar auras in the other nerves of special sense, those of taste, hearing, etc., are mentioned by the author.

Another instance of abnormal irritation of the olfactory nerve is reported by Dr. C. A. Robertson (*Bost. Med. and Surg. Jour.*, Sept. 18, 1873), in the case of a patient suffering from irido-choroiditis, subsequent to an operation. The irritation was evidently of a reflex nature. The patient complained of being constantly troubled by an indescribably disagreeable smell; but received permanent relief from a hypodermic injection of morphia in the arm.

SPINAL IRRITATION.—We copy from the *Gaz. des Hopitaux*, Nov. 23, 1873, the following resume of a *brochure* by Dr. Armaingaud, entitled "*Du point apophysaire dans les Neuralgies et de l' irritation Spinale.*" Paris, 1872. A. Delahaye.

"In this work the author offers some observations on the neuralgias, presenting among their symptoms the apophysary point of Trousseau. He next attempts to establish the existence, as a separate disease, of the affection described by English and American physicians under the name of spinal irritation, and shows the analogies presented by this latter affection to the neuralgias with the painful apophysary point, on the one hand, and with the affections described by Dr. Cahen under the name of vaso-motor neuroses, on the other. He also offers some considerations on intermittent fever and exophthalmic goitre, considered as vaso-motor neuroses of spinal origin.

"Finally, in a practical point of view, he calls attention to a method of treatment applicable to a great number of these affections, consisting in local revulsive applications along the spine. The following are the conclusions of the work:

"*Practical Conclusions.*—1. A large number of neuralgias present, independently of the painful points determined by Valleix, a sensitive locality, not described by that author, over the extremity of one or more vertebral spinous processes. This can be determined by pressure over each spinous process, successively, commencing at the first cervical.

"This painful point is altogether distinct from the dorsal point of intercostal neuralgia, which is located in the spinal groove, while this is over the extremity of the spinous process.

"2. This apophysary point is met with more especially in neuralgias of long standing, rebellious to treatment, or reappearing.

"3. When this apophysary point exists, revulsive applications (leeches, blisters, antimonial ointment, etc.) are indicated, and give relief when other means have failed.

"4. Consequently it is as useful, in a practical point of view, to seek the existence of this apophysary point in cases of neuralgia, as it is interesting scientifically to search for its signification: and I cannot too much impress upon my associates the importance of the exploration of the spinal column in all cases of neuralgia offered to their observation.

"*Theoretical Conclusions.*—1. Spinal irritation is a distinct disease, comprising, in its perfect form, four separate morbid conditions: Painful points along the spinal apophyses; neuralgic irradiations of this rachidian pain, especially under provocation; vaso-motor disturbances; and troubles of secretion, localized in one or many regions of the body.

"2. The neuralgias with apophysary painful points, on the one hand, and certain vaso-motor and secretory disturbances on the other, appear to be only incomplete forms, different degrees, or variations, of this same affection.

"In the first of these, the symptom of pain alone exists (neuralgia, with painful spinal point). In the second, the vaso-motor or secretory disturbances alone exist (or, at least, they suppress the other symptoms).

"Finally, in the complete or more advanced form, the three kinds of phenomena are united.

"There are, therefore, three forms or degrees of spinal irritation:

"A. *Spinal irritation, exclusively hyperæsthetic* (neuralgia with apophysary point).

"B. *Spinal irritation exclusively vaso-motor or secretory; or vaso-motor* *neuroses of spinal origin.* Examples: exophthalmic goitre, and idiopathic ptyalism.

" C. *Spinal irritation*, at once neuralgic and vaso-motor.

"3. There can be established between the vaso-motor neuroses a distinction analogous to that between the neuralgias of peripheral and central origin.

"4. From the success obtained with spinal applications in neuralgia with apophysary pain, and in spinal irritation, it is reasonable to think that similar applications would be of great service in certain vaso-motor neuroses, such as exophthalmic goitre, migraine, and also in intermittent fever.

"5. The observations of Malone and Stilling, while they justify the preceding conclusions, come to the support of the theory which localizes the initial vaso-motor determination of fever in the cord, and not in the great sympathetic."

EMOTIONAL TREMOR OF THE INSANE.—Dr. Berthier (*Gaz. des Hopitaux*, November 20, 1873) calls attention to tremor as a symptom of mental pathology. He gives it the name *tremblement emotif*, as best representing its character, and considers it as not lacking in importance in indicating the emotivity of the insane. The following marks of diagnosis are given for this symptom:

1. It is momentary, and only appears under the impulse of irritation or fear.

2. It is slight, superficial, and may be unnoticed in rapid or infrequent examinations.

3. It disappears, or diminishes, as the patient becomes accustomed to the immediate surroundings which have caused it, being in this unlike the tremor of alcoholism, or of paralysis, which is permanent.

4. It is met with in all kinds of mental disease, but is more especially marked in those kinds of alienation characterized by a fixed idea, particularly the insanity of which distrust, or the idea of persecution, is a special symptom.

INSANITY AND INTEMPERANCE.—Dr. D. Yellowlees, in a paper published in the *British Medical Journal*, October 4, 1873, discusses the relation of insanity to intemperance, either as a cause, a symptom, or as a result. He recognizes six forms of insanity, which are, or may be, caused by intemperance, as follows:

1. *Delirium tremens.*

2. Transient acute mania—the true *mania a potu*, which seems to occur in some persons instead of the ordinary form of *delirium tremens*.

3. The insanity of intemperance, of Dr. Skaes' classification, in two phases—acute and chronic.

4. The insanity which occurs in persons previously addicted to drinking, but who have become abstainers; the melancholia which is the result of direct moral causes acting on a brain weakened by previous excesses.

5. Chronic alcoholism.

6. General paralysis; which, however, is due also to other excesses acting conjointly with that in drinking.

Indirectly, intemperance produces insanity by establishing and transmitting a proclivity to nervous disease, or calling into action such tendencies already existing. Adding to these the insanity due to the poverty, disease, and violence produced by intemperance, Dr. Yellowlees considers it within the truth to assert that half the existing cases of insanity are due to this one cause.

Intemperance is, however, occasionally adduced as the cause of insanity, when it really is only one of its early symptoms; that is, it is often one of the first indications of the loss of the normal self-control. It may, also, be the result of insanity, as in the case of dipsomania, which the author asserts to be habitually associated with some inherited vice of the nervous organization —so constantly so, that the intemperance should be regarded, ultimately at least, as the result, rather than the cause. The inherited tendency may itself prompt the habits of intemperance; or they may be induced in the subject by some direct exciting cause, affecting the unstable nervous organization, such as injury, sunstroke, etc.

Insanity in Turkey (*Gaz. Med. de Paris*, November 27, 1873).—Everything in the Orient bears a different aspect from that it presents in Europe; and mental alienation is no exception to the rule. The causes of mental disease do not depend, as with us, on social troubles, but more especially to religious influences.

The statistics of the special hospital of Suleimanie give the following figures for the years 1820 to 1864:

During this period, 673 insane patients were received in that establishment—532 males and 141 females. The Caucasian race was represented by 566; the Ethiopian, by 81; the Mongolian, by 26; 116 patients could read and write; 50 could read only; and 501 were totally illiterate.

In regard to etiology, religious fanaticism was considered to be the cause in 254 cases; 7 cases were attributed to a religious idea of another kind, which the author diagnosticates under the denomination of religious scruples. The abuse of opium counted 20 victims; hasheesh, 94; alcoholic drinks, 88; domestic troubles, 127; disappointments of various kinds, 15; hereditary, 26; insolation, 61.

Simple Vertigo.—Dr. Clifford Allbutt (*British Medical Journal*, July 26, 1873) gives brief accounts of ten cases of simple vertigo from a much larger number occurring in his own practice. The affection is constant, or intermittent, and its connection with any other neuroses, if such existed, was obscure, to say the least. He considers it to be due to some affection of the cerebellum, or of the adjacent basal ganglia, and cannot trace in it any connection with dyspepsia, as was supposed by Trousseau, nor, in women, with that due to menstrual disorders. Indeed, the symptoms occur, for the most part, only in males.

Like migraine, it is very resistent to treatment. The most potent remedies, according to Dr. Allbutt, are, first, change of scene, and removal of all causes of nervous depression; and, second, Turkish baths. The first of these is the most efficient. Of drugs, strychnine is the only one which has been of utility in the majority of cases, and it rarely failed to do some good.

The tendency of the disease appeared to be to wear itself out after a variable period, sometimes, however, extending over several years.

Degeneration and Regeneration of Divided Nerves. — By M. Ranvier, *Comptes Rendes*, December 30, 1872, and February 24, 1873 (abstr. in *Rev. des Sci. Med.*, by M. Duval).

M. Ranvier first recalls his researches on the histology and physiology of the nerves (*Archives de Phys.*, 1872: 129), from which it was discovered that the nervous tube is formed of segments placed end to end (*segments interannulares*), constituted of an enveloping membrane, a layer of protoplasm, with a lenticular core near the middle of the length of the segment, nearer the centre, the envelope of myeline, and, finally, the axis cylinder.

When a nerve is cut, the following successive changes are observed: The cores of the interannular segments swell and augment into masses, which encroach upon the myeline sheath. After about seventy-two hours, the nucleus and increased protoplasm have reduced the myeline to a very fine, smooth filament, which shows an appearance of segmentation. About the third day, the axis cylinder is seen to be divided, opposite the nuclei. On the sixth day, the myeline is reduced to small fragments, and the protoplasm has greatly increased, and contains numerous fatty granulations. The nuclei have also become numerous. On the twentieth day, the fragments of myeline form, in places, fusiform masses, in the interior of which appear oval nuclei, and which are separated from each other by filaments, sometimes extremely slender. In the central portion, on the contrary, the myeline, instead of being separated into segments by an increase of the protoplasm and nucleus, suffers a decomposition into fine granulations, which form themselves into oval masses. The protoplasm, indeed, augments as the nuclei multiply; but they flatten themselves, and never come to touch or segmentate the axis cylinder, which remains intact as far as the point of division of the nervous tube, and, therefore, throughout its whole extent, preserves the connection unimpaired with the nervous centres.

Studying, next, the regeneration of the nervous tubes, M. Ranvier observed, in the interior of degenerated nervous fibres, the appearance of one or two tubes, as had been already described by Remak and Neumann; but these tubes are of new formation, as they each possess a proper membrane of Schwann, and are well characterized by the existence of annular strangulations and nuclei occupying the middle of the segments. * * * In all cases, the new tubes seem to form, first, without myeline (fibres of Remak), and afterwards to add this element. * * * The peripheral division of the divided nerve is united to the central by a cicatricial filament constituted of an innumerable quantity of little nerve tubes, of new formation, nearly all without myeline. Finally, the bud which is found at the end of the central division, presents a peculiar phenomenon; some of the larger non-degenerated nervous tubes give birth to a greater or lesser number of nervous fibres, of new formation, which form a little bundle, having its origin in a single tube. The envelope of this little bundle of nerve fibres is the membrane of Schwann, of the ancient tube, preserved.

M. Ranvier thinks that it is the cylinder axis of the old tube—which, in the central division, is preserved, and. for some days after the section, shows a well-marked hypertrophy — which is the point of departure of the new formation of nerve elements. The new fibres, which form in so great number, grouped in little bundles, constitute the filament uniting the divided ends, and, very probably, they penetrate into the inferior portions, to insinuate themselves, it may be, into the old degenerated fibres, or, perhaps, into the interstitial conjunctive tissue.

Slips of the Tongue.—Wm. M. Ord, M. B. (Session of British Med. Association, Aug. 6, 1873.) Slips of the tongue were investigated with special reference to their sensorial origin. They might be due to fault of the brain, coming then clearly under the head of aphasia. Such faults might be actual disease, or weariness, or disorder of attention, such as absence of mind, excitement, intentness, or confusion. They might be, secondly, of sensorial origin, morbid conditions of the parts concerned in speech interfering with the guiding sensations upon which fluent speech depends, distracting the higher brain, and throwing thought and articulation out of step with each other. Thirdly, they might be of motorial origin, where any circumstances hindered free movement of the lips, tongue or fauces.—*Brit. Med. Jour.*, Sept. 6, 1873.

Morbid Changes in the Cord.—M. G. Hayem gives (*Archives de Phys.*, September, 1873) a paper on the alterations produced in the cord by artificial rupture of the sciatic nerve. His experiments were performed on rabbits.

In one rabbit, killed a couple of months after the operation, the cord presented a striking difference between the two sides, the left side, that of the injured nerve, being atrophied, especially in its posterior column; the nervous cells atrophied, and the lesion extending several centimetres above the limits of the origin of the nerve. The atrophy of the nervous cells in the gray substance was general, but most complete and interesting in the "tractus intermedio lateralis;" it was a simple atrophy, no special degeneration or pigmentation being seen.

The second rabbit was operated upon on the right side: but the injury was less complete. The animal was sacrificed after the same lapse of time as in the other case, and the appearances were much the same, but less extensive and pronounced.

The suppression of function does not suffice to explain the rapid atrophy of the nervous cells, which must, in this case, be produced by a kind of irri tation, apparently propagated along the track of the posterior roots, nearly to the anterior gray substance. M. Hayem concludes that the only precise conclusion he would formulate from the facts is, that the cicatricial atrophy produced in the posterior bundle of the cord, outside of the cordon of Goll, by the rupture of a nerve, causes a very rapid atrophy of the nervous cells, which bears especially on the group of the "tractus intermedio lateralis."

M. Hayem also communicated to the Societe de Biologie, Nov. 15, 1873 (*Gaz. de Paris*, Nov. 29), two cases of hæmorrhagic spinal pachy-meningitis, observed by him in rabbits, after injury to the sciatic nerve.

M. A. Joffroy (*Gaz. Med. de Paris*, Sept. 6, 1873), in a paper read by him at the session of the Societie de Biologie, July 19, gives the results of microscopical examinations of the spinal cord of dogs, in which paralysis had been artificially produced. Although the results were incomplete, they are yet interesting, as giving suggestive facts.

Both above and below the local softening and alteration artificially produced, there was observed a tumefication of the cylinder axis to the extent of several times the normal diameter: with, also, in the first two, a dilatation of the central canal, which was filled with granular substance. In the third case, the animal died only five days after the operation; the lesion was observed for only a few centimetres above the *foyer* of softening.

The author believes that this lesion is due to an irritative process, a kind of parenchymatous inflammation. It should, at least, be separated entirely from the fatty degeneration of the myeline, as the two conditions are not found to co-exist, and, in his opinion, are due to totally different processes.

In the gray substance, a noticeable fact was the appearance of bodies furnished with numerous elongated processes, which are believed to be the cells of Deiters.

In the first two experiments, in which the animals lived for a considerable period after having been operated upon, the symptoms during life bore a considerable resemblance to those characteristic of locomotor-ataxy; and the examination of the cord showed that, in the immediate vicinity of the irritated point, the posterior pillars contained a number of tumefied cylinder axes, exactly limited to the external radical bundles; and M. Joffroy, therefore, considers this as an experimental confirmation of the theory of Charcot, that the principal lesion in this disease is confined to these bundles.

The Causes of Idiocy and Imbecility.—At the session of the Psychological section of the British Medical Association, held August 7, 1873, Dr. Langdon Down read a paper on the subject of the causes of idiocy and imbecility, the abstract of which, as reported in the *British Med. Journal*, Oct. 11, is as follows:

Following up his inquiries into the history of two thousand cases of idiocy which had come under his observation, Dr. Down found that twenty-four per cent. were first-born children. He regarded two causes as potential in these cases: 1. Pressure on the cranium; 2. Suspended animation from retarded labor. He also attributed something to the more exalted emotional life of women during their first pregnancies. Among primiparious idiots, one-fourth had been born with suspended animation. While the ratio of sex among idiot primiparæ was three males to one female, the ratio of those born with suspended animation was five males to one female, indicating the influence of the increased size of the male cranium over that of the female. He pointed out that there is less danger to the mental future of the child in the timely use of forceps, than in allowing a lingering labor. Three per cent. of the idiots had been delivered by forceps, and two out of the three per cent. were born with suspended animation, probably from the use of the forceps having been too long delayed. Disparity in the ages of the father and mother did not appear to be very productive of idiocy. In seven per cent., only, was there a disparity exceeding ten years In all the cases the father was the senior. Four per cent. of the issue were males, and three per cent. were females. Five per cent. of idiots had fathers who were above fifty at the time of their birth; and the male progeny were the most influenced, four per cent. being male, and one per cent. female. A much more potent cause was found to be the neurotic condition of the progenitors. In forty-five per cent. there were well-marked neuroses in one or both families. If the neuroses were marked on the maternal side, the first children were the most affected. If the neuroses on the other hand were paternal, he found that it was the later-born children that were affected. He related several cases that had lately come under his notice, where the father had died from locomotor-ataxy and general paresis. In these cases, the early members of the family were mentally vigorous, while the last-born were idiotic. The result was shown to be more perilous to the offspring of those where the neurotic tendency was on both sides of the progenitors; and it was this circumstance that caused the danger in marriages of consanguinity.

ALTERATIONS OF NERVES IN CONSTITUTIONAL SYPHILIS. — Dr. P. Petrow (*Virchow's Archiv*, Bd. VII., Heft I., 121, "*Ueber die veranderungen des sympathische Nervensystems bei constitutioneller Syphilis*)," reports the results of a series of histological researches on the sympathetic, in cases of constitutional syphilis, undertaken by himself at the Pathological Institute of the Academy of Medicine of St. Petersburgh. In the protoplasm of the nerve-cells he found the following alterations from the normal conditions: In cases in which the disease was comparatively recent, he observed in the cells otherwise normal, little brown or dark brown pigment granules, clustered or scattering, and strongly shining in reflected light. In more advanced cases, their number multiplied till they filled the cell, and even completely hid the nucleus. By the addition of concentrated nitric acid or caustic potash, a partial solution of the pigment was effected; but in no case did they entirely disappear. The origin of these pigment granules can be sought, the author thinks, in the hæmatin of the blood.

With these alterations of the nerve-cells, the surrounding endothelium frequently remained altogether unaltered, its cells appearing even more distinct; or, if they suffered any change, it consisted in swelling and multiplication of their number, so that the nerve cell appeared encircled with these increased elements.

In some preparations with chloride of gold, and also in some unprepared fresh specimens, there were observed on the inner nerve capsule, a narrow layer of compressed polygonal cells, more or less sharp in contour, and with round nuclei.

In many of the pigment containing nerve cells, the protoplasm was found changed in a peculiar manner, appearing as a homogeneous, shining, strongly-refracting, mass; while the cell contour remained unaltered; and the nucleus was either invisible or pushed to one side; the latter being the case when this change had taken place in only a part of the cell contents. These altered cells were easily to be mistaken for the light, transparent vacuoles, which also occur in the normal condition, but could be distinguished by their behavior under re-agents. The same change, which the author regards as colloid, also took place in the endothelial cells, making it at times impossible to distinguish them from the altered nervous elements.

The changes in this connective tissue of the sympathetic, Dr. P. found to consist in hyperplasia of the fibres, coarse-fibred bundles appearing, instead of the normal, fine, scarcely noticeable fibres of uncertain contour. The cell elements (endothelium) surrounding the nerve fibres, partake in the change; they become turbid, finely granular, indistinctlv outlined, with the nucleus only here and there observable.

The nerve fibres in this abnormal connective tissue appear somewhat compressed, their sheaths thickened, and, apparently, the number of the granules of the sheaths is much greater; the myeline substance, instead of being homogeneous, appears finely granular, etc.

In the fibrous interstitial tissue, the white fibres of Remak cannot be distinguished.

The author sums up the results of his examinations as follows:

1. The general disorder of the organism in syphilis, causes alterations of nerve elements, and interstitial tissue in the system of the sympathetic.

2. The nerve elements suffer, independently of the interstitial alterations, a pigmentose and colloid change (most frequently the former).

3. With the development of the interstitial processes in various organs, similar alterations also occur in the connective tissue of the sympathetic, whereby atrophy of the nerve fibres and cells takes place.

4. The endothelium which surrounds the nerve cells likewise partakes in the disorder. Here the metamorphoses progress as follows: In the beginning, with increase and proliferation of cells; later, passing into a retrograde (fatty) metamorphosis.

Hysteria.—Dr. Alb. Puech, of Nimes, France, gives the case of a female subject to typical attacks of hysteria, in whom he could detect no appearance of uterus or ovaries, thus supporting the views of those who deny that hysteria always depends upon functional or organic alterations of the uterus. Similar cases have been observed by other authors.—*Gaz. Obstet. de Paris*, quoted in *Gaz. des. Hopitaux*, Aug. 23, 1873.

Congenital Blindness.—Geo. C. Harlan, M.D., "Report of an Examination of the Eyes of 167 Inmates of the Penn. Institute for the Instruction of the Blind" (*Hay's Jour.*, Jan., 1873, Art. IX.), gives some remarks in regard to congenital blindness. Of thirty-five cases examined, in ten the blindness is given as due to cataract; nine to retinitis pigmentosa; nine to deformities; three probably to atrophy of the optic nerve; three to atrophy of the choroid; and one, cause unknown. Of these thirty-five cases, six are the children of blind, or partially blind, parents, all being subjects of retinitis pigmentosa, or congenital deformities; and in only two cases out of the whole number can consanguinity of the parents be traced.

From each of four marriages which have occurred among the inmates of the Institution, one or more children have resulted, only one of which is blind, and, in this case only, was the blindness of either parent congenital. As Dr. Harlan remarks, the subject is worthy of study.

General Paralysis.—Dr. Victor Hanot concludes, in the *Gaz. Med. de Paris*, 1873, No. 39, a series of articles on the apoplectic accidents of general paralysis, with special reference to the thermometric variations and the conjugated rotation of the head and eyes, observed in these attacks. He gives the following resume of his conclusions:

1. "The apoplectic attacks which occur in the course of general paralysis, give a thermometric trace identical with that of some simple phlegmasiæ—simple pneumonia, for example:

"By this they are to be distinguished from analogous attacks due to limited local lesions of the encephalon (hæmorrhage, softening, etc.), and come under the law established by Charcot, relative to the generality of the apoplectic or epileptiform states, not immediately due to local lesions.

"In my observations, the special evolution of the temperature cannot be explained by any concomitant inflammatory malady of the thoraçic or abdominal organs.

2. "During the apoplectic attacks of general paralysis, there can be observed the conjugate rotation of the head and eyes. In five cases, this appeared to have for its cause the unilateral predominance of the morbid condition in one hemisphere, that corresponding to the side toward which the rotation takes place.

3. "As regards the conjugate rotation of the head and eyes, in the apoplectic seizures of general paralysis, as in those due to local lesions, *en foyer* of the brain, the symptom has not invariably an absolutely grave significance. The apoplectic attacks of general paralysis, accompanied with this peculiar symptom, do not always have a fatal termination."

MM. Magnan and Mierzejewsky, *Arch. de Phys.*, January and March, 1873 (abst. in *Rev. des Sci. Medicales*), give an account of *post-mortem* appearances in the ventricular walls and subjacent parts in various cases of general paralysis. They find in nine cases reported, independent of other lesions of the brain and meninges, the walls of the fourth ventricle, and to a less degree of the lateral ventricles, also, thickened, and more or less covered with small, rounded granulations, consisting of connective tissue.

Dr. Lolliot, "*De l'alcoolisme comme cause de Paralysie generale.*" *Gaz. des Hopitaux*, Sept. 6, 1873. An account, with comments, of two cases in which alcoholism is considered to be the direct cause of the general paralysis.

Diagnosis of General Paresis and Locomotor-Ataxy.—Dr. W. H. O. Sankey contributes to the *Brit. Med. Jour.*, Sept. 20, a portion of a lecture on the diagnosis between the two diseases, general paralysis and locomotor-ataxy. He inclines to the belief that many cases of locomotor-ataxy, complicated by the presence of cerebral symptoms, have been called general paralysis. He gives the characteristics of each affection in the following tabulated form:

PARESIS.	ATAXY.
Runs its course in a few years.	Is usually much slower, and may last ten or twenty years.
Commences with mental symptoms.	Commences with pain in a distal nerve.
Is attended with libidinous ideas.	Is attended with absence of sexual feeling.
The motor symptoms are secondary in the order of time.	The motor symptoms are the primary phenomena.
Is only rarely complicated with pelvic difficulties.	Pelvic symptoms are a prominent feature.
There often is great violence.	The mental phenomena are imbecility and impaired memory.

Locomotor–Ataxy.—Dr. G. C. Lawrence (*Chicago Med. Exam.*, March 15, 1873) suggests that the motion of railroad trains is a frequent cause of locomotor-ataxy among railroad employes. His attention was first directed to the subject by the disproportionate and increasing number of railroad men afflicted with this trouble, among his patients at the Hot Springs of Arkansas; and the review of his practice has satisfied him that the occupation holds a causative relation to the disease.

Acute Primary Mania.—W. H. O. Sankey (*Brit. Med. Jour.*, Oct. 25, 1873) contends that there is no such disease as acute primary mania, and that those cases which have been so called are either cases of paresis before the motor symptoms are developed, or secondary attacks of recurrent insanity, in which the premonitory symptoms are marked, or of very short duration. The name *mania* should, if this opinion is correct, be expunged from the list of mental diseases, and only be retained as characterizing a group of symptoms occurring in the course of certain forms of insanity.

c.—THERAPEUTICS OF THE NERVOUS SYSTEM AND MIND.

Aconite.—A. Leslie Mease (*Med. Times and Gaz.*, March 29, 1873) has obtained beneficial effects from the tincture of aconite, in facial neuralgia from carious teeth and gastric disturbance; also, in facial neuralgia connected with hysterical symptoms; in neuralgia of the testicles; and in headache, with pain in the back and limbs, with the vomiting of pregnancy. He advises, in cases where constipation or gastric disturbance exists, the withholding of this drug, or the simultaneous employment of remedies for those conditions. The effect of aconite is very different on different constitutions, some persons being unable to take even small doses without annoying symptoms. He prefers to commence with small doses—four to ten drops of the tincture—and considers it to act best combined with chloroform. In some of his cases he has also added bromide of potassium.

Atropia.—Dr. Giquel has studied, in his *Thesis de Doctorat*, the physiological action of atropia. His work is divided into three parts.

In the first part, the author relates ten observations which prove that atropia, introduced into the skin around the orbit, produces a dilatation of the pupil on the corresponding side. In some cases, when the solution was concentrated (one-tenth), this effect also appeared in the other eye; but, with a weaker solution (one-hundredth), it was never so observed.

Examining, next, the diverse theories which have been proposed to explain the mode of action of atropia, Dr. Giquel adopted the following: The unilateral mydriasis which follows the peri-orbital vaccinations, is due to a reflex action, the point of departure of which is the numbing of the cutaneous extremities of the trigeminus. This was also the opinion of his teacher, M. Potain.

When the phenomenon is presented on both sides, it is necessary to admit an absorption of the liquid, and consequently a direct action on the nervous centres, or else a veritable synergic or reflex action on the other pupil, especially if the contraction there is only slight.

The hypothesis of this benumbing action of atropia being admitted, the author concludes by showing that anatomy and physiology may be invoked for the support of this explanation.

In his conclusions, Dr. Giquel urges the employment of atropia vaccinations, and advises, correctly, the application of the benumbing agent as nearly as possible to the affected nerve.—*Rev. des Sci. Medicales*, T. II., No. 1; abstr. *Bulletin Generale de Therapeutique*, Sept. 30, 1873.

H. C. Wood ("A Contribution to our Knowledge of the Physiological Action of Atropia:" *Hay's Jour.*, Apr., 1873, Art. III.) gives details of experiments as to the action of atropia on the circulation, and as to the reported antagonism between it and morphia. In regard to the first of these points, he infers that the alkaloid possesses a stimulating power over the accelerator nerves of the heart, or, more properly, over their centres; and that the enormous increase of the pulse, in cases of atropia poisoning, is not entirely due to paralysis of the vagi, but also to direct action upon the antagonists of the latter. Dr. Wood considers the medical value of atropia, in opium poisoning, as a completely established fact, without, however, considering them as absolutely antagonistic, in the usual sense of the word. In their general influence on the respiratory centres, however, they are opposed to each other; and upon this fact the value of atropia rests.

S. Buckley reports (*Edinb. Med. Jour.*, Sept., 1873) a case of poisoning by strychnia, successfully treated by subcutaneous injections of atropia. The patient, a melancholic woman, had taken, purposely, a quantity of insect poison containing the alkaloid, some of which was removed by the stomach-pump, about half an hour afterwards. The treatment, besides external warmth, and chloroform to relieve the spasms, consisted in, at first, the subcutaneous injection of twenty minims of the liquor atropiæ (B.P.), at intervals of ten minutes. After three injections of this quantity had been made, a marked alleviation being noticed at each, the interval was lengthened to twenty minutes, and the dose reduced to one-twelfth of a grain each time. In all, not less than one and one-sixth grains were thus administered. The patient made a good recovery, and left the hospital the ninth day after admission, having been detained several days on account of an accidental burn. The atropine, alone, seemed to alleviate the symptoms; and the case is otherwise of interest on account of the remarkable quantity administered, and the tolerance of the system, in this case, to the remedy, even under the circumstances in which it was administered.

Dr. Roberts Bartholow (*Clinic*, Aug. 9, 1873) discusses the antagonism between atropia and physostigmia, and gives a report of the experiments performed by himself. The results are summed up as follows:

"Atropia and physostigmia are not antagonistic, as regards their action upon the muscular system of animal life, paralysis being induced by both. Atropia produces paralysis, by destroying the muscular irritability and excitability of the motor nerves; physostigmia, by paralyzing the spinal cord.

"Atropia and physostigmia are antagonistic, as regards their action on the sensory nerves—atropia destroying, and physostigmia heightening, the sensibility of these nerves.

"They are antagonistic, as to their influence over the respiratory movements—atropia increasing, and physostigmia retarding them.

"They are antagonistic, in their action on the heart—atropia producing excitation of the cardiac ganglia, and physostigmia paralyzing these ganglia.

"They are opposed, in respect to their action on the sympathetic—atropia producing increased action of the sympathetic, physostigmia paralyzing this system.

"They have opposite effects on the pupil, in virtue of opposite effects on the sympathetic—atropia dilating the pupil, by its action on the radiating fibres of the iris, physostigmia contracting the pupil, by paralyzing the radiating fibres.

"A very singular effect, which I was not prepared to find, is the peculiar exaltation of the reflex faculty, produced in frogs, when these agents are administered together—a sudden irritation of the surface causing tetanic rigidity, like electric shocks, the muscles, immediately afterward, resuming their very relaxed and flaccid condition. Atropia sensibly weakens, although it does not abolish, entirely, the reflex faculty; physostigmia destroys the reflex faculty; yet the combination of the two agents produces effects not unlike those of strychnia. The analogy is preserved, even after death; for *post-mortem* rigidity sets in at once, and is very decided. The tetanic spasms must not be confounded with the tremors which are characteristic of physostigmia. These tetanic spasms are less marked in warm-blooded animals, but they nevertheless occur, to a limited extent; and, after death, a marked degree of rigidity exists, the head and neck being curved back, and the feet turned in."

Bromide of Potassium.—Dr. Aug. Voisin, *Arch. de Med.*, January and February, 1873 (abstr. in *Rev. des Sci. Med.*), gives the following conclusions, in regard to the therapeutic action of bromide of potassium:

In epilepsy, the medicine has acted sufficiently on the upper part of the cord, when nausea is not produced by the introduction of a spoon to the base of the tongue, or sneezing, and flowing of tears, by titillation of the nares and nasal fossæ. When the reflex nausea is produced, it is, according to M. Voisin, an indication of the insufficiency of the dose. Over-medication is indicated by discoloration of the skin, rapid loss of flesh, general depression of vital forces, dryness of the buccal cavity, and the diminution of appetite. Iron and arsenic should be administered, concurrently, to prevent the anæmia consequent to the bromide medication. The duration of this medication in epilepsy, may be fixed at less than ten years, as relapses have often occurred after six years.

The employment of this drug may cause the accident of bromism, which may develop itself either rapidly or slowly. The rapid form is characterized by the following symptoms: titubation, somnolence, dull look, stupor, cephalalgia, inability to express one's self; at the same time the handwriting is tremulous, badly traced, the invalids forget words and phrases, and write incorrectly; the tongue is dry, and thirst excessive.

The slow form commences in two different forms: in the one, the sufferer loses strength; his complexion is a dirty yellow; his eyes hollow; and his visage thin; his expression stupid; his sight feeble; and hearing poor; he hesitates in his words; memory is obscured; the gums are tender, and sometimes red and tumefied; the tongue and hands are tremulous in movement; and the walk is titubating. When the condition becomes aggravated, the patient falls into coma, some fever is present, and sometimes a pulmonary catarrh intervenes, to which he finally succumbs.

The second form is characterized by cerebro-spinal complications, general delirium, accompanied with hallucinations, ideas of persecution and violence, ataxia of the members and of the tongue, and troubles of speech.

With the accidents of bromism, M. Voisin describes those which he attributes to the *cachexie bromique:* discoloration of the skin and mucous membranes, general debility, loss of flesh, and vascular murmurs. The affections which have been observed, following this cachexia, have always presented the typhoid and adynamic character. M. Voisin has observed a peculiar dry cough produced, which was followed by vomiting of mucous and alimentary matter, and which resembled, somewhat, the whooping-cough. Among the eruptions produced by the bromide, he notices acne, one slightly resembling rupia, another comparable to nettle-rash, eczema, and pityriasis.

Bromide of potassium, from its sedative action on the bulb and spinal cord, is employed with advantage in epilepsy, chorea, tetanus, spinal irritability of hysterical and anæmic persons, and pains and cramps of spinal origin; and, by its constrictive action on the muscular fibres of the capillary vessels, it acts in simple visceral congestive affections, in cerebro-spinal congestions, and in spermatorrhœa; it also diminishes the buccal, pharyngeal, and probably, also, the gastric secretions. Two or three doses, of one gramme each, have sometimes caused the disappearance of leucorrhœa. It fails in epileptics, with those under the influence of hereditary tuberculosis, who have malformations of the cranium, who have given themselves up to onanism, or alcoholism, who are affected with tubercles of the brain, who are demented, hæmiplegic, or paraplegic, because these complications are due to plastic formations, to partial sclerosis, etc., etc.

Vertigo is little modified by the bromide of potassium. It succeeds in epileptiform attacks, connected with congenital cerebral lesions, imbecility, idiocy, cerebral softening, in chorea, in the series of medullary peripheral nervous troubles, tetanus, etc., and, finally, in all such phenomena of spinal origin.

Dr. D. N. Kinsman (*Clinic*, April 12, 1873) advises the use of bromide of potassium in cases of sick headache, thirty grains to one drachm, dissolved in water, every hour until relieved. Keep the patient quiet, and, according to this authority, a couple of doses will generally be found effective in relieving the attack. The essential point is to commence early, as soon as there are any premonitory symptoms.

Caffeine.—T. Curtis Smith ("Caffeine:" *The Clinic*, Nov. 1, 1873) recommends its use in neuralgic pains of the head and face, and migraine, when there is a lack of tone in the nervous system, upon which the pain seems to depend, in permitting sleep, in wakefulness dependent on nervous debility, and in intermittents—in fact, wherever a simple nerve tonic or stimulant is needed, without a subsequent soporific effect.

Cherry Laurel Water as a Vehicle for Hypodermic Injections.—M. Luton recommends (*Repertoire de Pharmacie*, 1873, No. 11) the use of cherry laurel water as a vehicle for the hypodermic use of narcotic medicines. Its advantages are, according to him, as follows: "Beyond that it adds its own slight hypnotic action to that of the principal drug, it opposes, better than any other aromatic distilled water, the formation of the cryptogamic forms which so readily invade these preparations. Its efficacy, in this last respect, at least, equals, if it does not surpass, that of the distilled water of eucalyptus, recommended by Prof. Gubler. The contact of cherry laurel water with the tissues is not more painful than that of ordinary distilled water; and I have never seen any inconvenience follow its employment. As to its physiological effects, when pure, they are the same as when taken by the stomach, only more pronounced, as is usually the case with medicines applied hypodermically."

CHLOROFORM.—Prof. Roberts Bartholow (*Clinic*, Sept. 27, 1873) gives a report on the use of deep injections of chloroform for the relief of tic douloureux. His method of operating is as follows: The needle is inserted under the upper lip, and passed up until its point rests near the infra-orbital foramen when the chloroform is slowly injected. At its withdrawal, pressure is applied over the locality, to insure diffusion of the chloroform. The unpleasant effects are usually but temporary; and, in the two cases reported, one appeared to be permanently cured, and the other was very decidedly relieved. In another case, in the practice of Dr. J. T. Whittaker, mentioned by Prof. Bartholow, and afterwards, in the same number, by Dr. W., a tolerance of the remedy seemed to become established, so that it no longer produced an effect.

CHLORAL AND CHLORALISM.—M. Gubler, in a remarkable lecture on chloral, informs us that the metamorphosis of chloral into chloroform, in the system, according to Liebrich's theory, generally received by chemists, only takes place in a very small part, and that nearly all the chloral acts in its own quality of chloral. This is to-day scientifically demonstrated, both by observation and experiments in the laboratory. There exists a fundamental difference between the pharmaco-dynamic actions of the two substances. Chloroform is the most powerful of anæsthetics, and a sleep-producer of but little value. Chloral, on the contrary, possesses a hypnotic power comparable to that of opium, and only becomes anæsthetic when it threatens the existence as a poison of the heart. M. Gubler recognized, among the effects of chloral, a certain set of symptoms, to which he gave the name chloralism. He distinguishes two kinds of chloralism, an acute and a chronic. The first, sometimes mild, sometimes grave, and even mortal, is characterized, in the mild form, by vomiting, vertigo, hebetude, loss of force, and then by various eruptions. The grave form is attended with paleness, troubles of vision, cold sweats, weakness of the pulse, stupor, coma, tetaniform convulsions, and, perhaps, death, which may be attributed to depression or paralysis of the heart. Chronic chloralism is characterized by symptoms analogous to those of ergotism—that is to say, by hyperæsthesia, general malaise, desquamation of digital epidermis, superficial ulceration around the nails, anasarca, albuminuria, weakness of the heart, and embarrassment of respiration. These symptoms often terminate in death. The dangerous effects of the medicine should be avoided by care not to let its administration be too long continued. The severe form of acute chloralism may be combated by warmth, stimulating odors, artificial respiration, and the inhalation of oxygen.—*Jour. de Pharm. Gaz. des Hopitaux*, Aug. 30, 1873.

Prof. DeRenzie (*La Nuova Ligur. Med.*, 1873, No. 11) combats the efficacy of curare and chloral in tetanus. He recommends, instead, the employment of prolonged warm baths, together with the removal of all cause of irritation from light and sound—placing the patient in a darkened room.

In the autopsy of one fatal case, he found the gray substance of the cord reddened through the general hyperæmia, but found no trace of the granular degeneration described by Clarke.

S. Baruch (*Richm. and Louisv. Med. Jour.*, Jan., 1873) reports a case of trumatic tetanus, treated successfully by chloral and bromide of potassium, and gives statistics of forty-eight cases in which chloral was used as the principal remedy, of which thirty-two recovered and sixteen died.

Sargenti describes (*Gaz. Med. Ital.*, 1873, No. 22) a case of epileptic attack, due to helminthiasis, which ceased after the cause was removed, but was immediately followed by traumatic tetanus. This was, at first, attributed solely to the laceration of the tongue in the seizures; but a careful examination found, also, an imbedded splinter in the sole of the left foot. The tetanus was combated with success by means of warm baths and chloral.

In vol. 84, p. 128, of the *Bulletin de Therapeutique*, M. Bouchut gives a highly favorable account of the use of the hydrate of chloral in chorea. He seems to have employed it quite extensively, and with excellent results. It needs only to be remarked, as regards its administration, that it must be given in doses sufficiently large, and with sufficient frequency, to control the convulsive movements. The opinion of M. Bouchut is, that the chloral is changed into chloroform in the blood, and in this way is efficacious.

Use of Chloral in Uremic Convulsions.—In a well-marked case of uremic convulsions, occurring after parturition, the convulsions, and also the albuminuria, disappeared speedily under the use of hydrate of chloral, in full doses, in the service of M. Daim, in one of the Paris hospitals.—*Gaz. des Hopitaux*, 1872, No. 112; *Bull. de Therap.*, vol. 84, p. 42.

Chloral in Delirium Tremens.—It is reported on, in a highly favorable manner, in the following journals: *Bulletin de Therapeutique; Gazette Medicale; Lo Sperimentale.*

Croton Chloral Hydrate.—Dr. Benson Baker gives, in the *Brit. Med. Jour.*, Oct. 25, 1873, mention of several cases of neuralgic pains relieved by this drug, which he recommends as a valuable therapeutic agent in such cases. He gives it in one or two grain doses, every hour; and, in thirteen patients thus treated, has been unable to observe a single unfavorable symptom from its use. In one grain doses, it relieves pain quickly, causes natural sleep, with no subsequent headache or furred tongue. In several cases, it acted as a gentle laxative.

Electricity.—Dr. Bouchaud publishes (*Bull. Gen. de Therap.*, July 15, 1873) his experiences with the use of the constant current in odontalgia. He obtained, generally, very good results with a current from ten elements (Caillaud), sometimes more or less than this number, the positive pole being applied on the cheek, and the negative on the antero-lateral region of the neck, thus bringing the affected nerve, under the influence of the former, into a condition of analectrotonus. The excitation of the vaso-motors, and consequent diminution of the determination of blood to the affected part, by the centripetal current, may, the author suggests, have a part with the analectrotonus in producing the result. The duration of the application lasted about half an hour, though the relief from pain commenced in ten or fifteen minutes.

A few cases proved rebellious to this method of treatment; and, in others, the relief was temporary, lasting a few hours or days only, in some of which a new application of the current produced a permanent cure. Dr. Bouchaud deems that a lasting cure might have been attained in all cases, had it been practicable to have renewed the applications in each.

Dr. Julius Althaus (*Brit. Med. Jour.*, Nov. 1, 1873) also recommends the use of the constant current in toothache After numerous trials of various modes, he concludes that the induction of analectrotonus of the dental nerves, with the complete avoidance of catalectrotonus, answers best. His method of procedure is as follows: The large anode, armed with a moist sponge, is placed on the suffering cheek and jaw, and the cathode to the palm or back of the hand. In this way, both the second and third branches of the fifth nerve are brought into the condition of analectrotonus, while the influence of the cathode is neutralized by its distance. One application of a gentle but plainly perceptible current, for five minutes, is generally sufficient; but, in some bad cases, a second application may be required to bring about the desired result. This application can be made the same day, if practicable.

The same principles, the Doctor adds, apply to the treatment of the different forms of neuralgia, which yield to the induction of analectrotonus, if practiced sufficiently early In the later stages, the treatment becomes more complicated and less readily successful.

Galvanism in Tetanus.—M. Leon Lefort communicated to the *Soc. de Chirurgie*, at the seance of October 30, 1872 (reported in *Bull. Gen. de Ther.*, 1873: 44) a case of tetanus following a crushing wound of the hand, in which the application of the constant current was followed by instantaneous cessation of the tetanic contractions. In spite of the fact of a fatal termination in this case, M. Lefort was struck with the promptness of the action of the current in relieving the spasms. The ascending current appeared to him to be the most advantageous, instead of the descending, to which MM. Onimus and others had given the preference.

"*Central Galvanization.*"—Dr. A. D. Rockwell (*N. Y. Med. Jour.*, May 1873, Art. II.) discusses the effect of electricity on the nervous centres, and gives cases illustrative of his method and its success.

Central galvanization, in its most thorough form, is practiced by placing the positive pole on the vertex, and the negative on the epigastrium. The former is then passed downward, over the occiput, to the spine, and along it to a point opposite the position of the other electrode. The strength of current and duration of the application are to be varied according to the needs in each case. As far as any positive directions can be given, the application to the head seldom ought to be continued over two or three minutes, and, often, not more than fifteen to thirty seconds. In some cases, any use whatever of galvanism is contra-indicated. As a rule, the metallic taste which is noted by the patient, with even very weak currents, is a sufficient guide for the first application. The current, if well borne, and it is considered advisable, may be increased, even until its effects are felt in a burning sensation at the pit of the stomach. In all cases, Dr. Rockwell considers the galvanometer an indispensable appliance in central galvanization, no other means answering to give information as to the strength and constancy of the current.

Dr. Rockwell ("On the Application of Electricity to the Central Nervous System; A Reply to the Objections of Anstie, Brown-Sequard, Cyon, and Others:" *N. Y. Med. Jour.*, Sept., 1873, Art. IV.) in this paper, replies to the objections of those who have spoken against the electrization of the nervous centres, and maintains that electro-therapeutics, based on clinical experience, is independent of, and in advance of, electro-physiology; and that the latter is, in its present condition, an uncertain science, unreliable as far as it bears upon the practical employment of electricity in the treatment of disease.

Electricity as a Tonic.—Dr. Victor Revillout (*Gaz. des Hopitaux*, Aug. 20, 1873) recommends the use of feeble currents, on the region of the dorsal spine, to influence the vaso-motor and trophic nerves, and stimulate the nutrition of the tissues.

Emetine.—Dr. Antonio Evaristo D' Ornellas, in a memoir read before the Society de Therapeutique, May 14, 1873 (reported in the *Gazette Med. de Paris*, Oct. 25), gives the following as the conclusions arrived at in his experiments to ascertain the physiological action of emetine:

1. Emetine is the active principle of ipecacuanha, which owes to it its powerful physiological and therapeutic action.

2. The odorous, nauseous principle of ipecacuanha is only accessory to the emetine in its action.

3. In medical practice, excluding rare and exceptional cases, it is not advisable to substitute emetine for ipecacuanha, as it is not a sufficiently perfect medicine, and does not offer any advantages in respect to the size of dose, etc.

4. Emetine has an irritant action when topically applied to the mucous

membrane, or when endermically administered, but none at all on the uninjured epidermis.

5. When injected into the subcutaneous cellular tissue it produces vomiting, but more slowly and under the impulse of stronger doses than when administered by the stomach.

6. After the drug is once introduced into the system, it is eliminated by way of the gastro-intestinal mucous membrane and the liver, and causes vomiting at the moment of its elimination.

It may be mentioned here that in our article, "Du Vomissement" it was shown that it is on the mucous membrane of the stomach and duodenum that emetine acts in provoking the act of vomiting, by exciting the peripheral expansions of the nerves which terminate there; and that emesis in this case, as with all other emetics, is a reflex act on the part of the stomach, having for its centripetal agent the gastric portion of the pneumogastric.

The emetine, at the time of its elimination *via* the intestinal mucous membrane, causes stools, the more numerous as the attacks of vomiting have been less energetic. Injections of emetine by simple topical action causes, also, an increased secretion of the great intestine, and gives rise to the discharges which relieve the rectal tenesmus.

8. In divided doses, emetine retards the respiration, and, slightly, the circulation; it lowers the animal temperature, but does not directly change the vascular tension. In nauseating or emetic doses its action is the same, but much more energetic.

9. It retards the respiratory movements most probably by reflex action, which acts from the gastric portion of the pneumogastric and its branches, by way of the medulla on the pulmonary division of the same nerve, and by way of the cord in the expiratory muscles. The revulsion caused by the agent in the stomach and intestines also influences the respiration.

10. In a therapeutic point of view, this action in the respiration may be advantageous, if it comes on gradually and is preceded by an increased secretion in the gastro-intestinal mucous membrane; it then produces an ischæmia of the pulmonary tissue. It will have a bad effect if it takes place too rapidly, and the action on the expiratory muscles pass to the state of paralysis before the intestinal revulsion has sufficiently emptied the pulmonary tissue; it then causes hyperæmia, ecchymoses, and hepatization of the lung. If the effort on the respiration be frequently repeated, and for many days consecutively, without much revulsion, it then will very probably cause hepatization of the lung.

11. The circulation is less rapidly and less constantly affected by emetine than the respiration. The retardation of the pulse is also probably due to reflex action, but on the cardiac portion of the vagus.

12. Emetine has scarcely any effect on the vascular tension; it neither augments or diminishes it.

13. The animal temperature is profoundly affected by this agent. At the surface of the body it is lowered, but in the rectum, instead of continually decreasing, it soon commences to rise, and constantly continues to do so. This is the double effect of the fluxion produced towards the gastro-intestinal canal and the work of elimination of the emetine.

14. Emetine causes a profound alteration of innervation, and operates in producing reflex acts. In fact, it acts on the peripheral extremities of the nerves, and especially upon those of the gastric portion of the pneumogastric, whence it, by reflex action, excites the medulla and the nerves which arise from it.

By the relaxation it produces in the voluntary muscles, it advantageously counteracts the tetanic convulsions produced by phenic acid and by strychnia; it possesses an anti-convulsive action.

Saponin.—H. Kohler, in a *brochure*, "*Die locale anæsthesirung durch Saponin*" (*Halle*, 1873; abstr. in *Centralblatt der Med. Wissensch.*, No. 26), treats of the physiological action of this agent with special reference to the idea first suggested by Pelikan, from its paralyzing the sensible nerves as a local anæsthetic. The local and general physiological phenomena from injection, both under the skin and directly into the circulation, were noted. By the former method of administration, it acts as a powerful poison to the muscles, causing them to finally stiffen, with complete loss of irritability. The paralysis of the nerves progresses independently, commencing at the periphery and extending as the poison enters the circulation, even to the cord. In all cases, a paralysis of the heart occurred sooner or later, at first through its peculiar nervous apparatus, and was finally perfected by the rigidity of its muscle. This, also, occurred when 6 cgms. of saponin were introduced into the abdominal cavity of a rabbit; and, in this case, the intestines were also paralyzed. When the poison was introduced directly into the spinal cord of a frog, there followed, first, tetanus, then paralysis of sensibility and motion of the hinder limbs, independently of the brain. The paralysis progressed from the center to the periphery; and here, also, the peripheral nerves were affected before the muscles.

When saponin is introduced directly into a vein or artery the general symptoms are those of disturbance of the respiration and circulation; after a primary increase there occurs diminished frequency of the pulse and respiratory movements, lowering of the blood pressure and temperature, and, very quickly, death. This diminished frequency of the pulse is independent of the action of the vagus, of which the cardial terminations are also paralyzed. The diminished respiration and pulse are themselves sufficient to account for the lowering of the bodily temperature. The author did not succeed in producing an immediate general effect on the nervous centres or peripheral nerves, by the injection of saponin into a vein; only here and there were staggering, muscular tremor and tetanus observed—perhaps on account of the disturbance of the circulation; the sensibility of the peripheral nerves and the muscular irritability remained intact. The administration by the mouth only altered symptoms in their intensity.

In conclusion, the author remarks that, with the probable dose—judging from the experiments with rabbits—required for local anæsthesia in men, the danger from its possible entry into the circulation should be well considered.

The author also publishes, *Archiv f. Exper. Path. u. Pharmakol*, I., 2: 138

(abstr. in *Cbl.*), experiments as to the antagonistic action of saponin and digitalin which had been suggested. According to him, this exists to a limited degree. The latter only retards, and does not counteract the effects of the former, and cannot be considered, at least in the rabbit, as an antidote.

NITRITE OF AMYL.—Dr. Amez-Droz publishes (*Archiv de Physiologie*, Sept., 1873) an account of his researches on this substance. Together with the details of his experiments, he gives a historical review of the subject, and discusses its probable method of action. Without pretending to decide the question absolutely, the author rather favors the hypothesis, that it operates by causing an irritation of the peripheral extremities of the nerves (perhaps the vagus), which, in turn, produces a reflex irritation of the vaso-motor centre, causing a diminution of the tonicity of the walls of the capillary vessels. In regard to the possibility of its being the muscular fibres of the capillaries which are affected, instead of the ultimate ramifications of the vaso-motor nerves, he brings forward the fact that the pupils do not show a constant dilatation under the influence of nitrite of amyl, as having a bearing against the theory of Brunton that it affects the non-striped muscular fibres generally. The fact, observed in nearly all his experiments, that a momentary contraction of the capillaries of a frog occurred whenever the animal made any violent movements, speaks strongly against any absolute paralysis of the vessels. The author explains the phenomenon by admitting two kinds of muscular fibres in the coats of the capillaries, one set contracting, and the other dilating the vessels, and explaining the dilatation produced by nitrite of amyl, as an irritation of these latter, the others remaining unaffected, and reacting, to a greater or lesser extent, under excitation.

The probable primary cause of irritation by nitrite of amyl is the non-oxydation of the blood under its influence. The acceleration of the heart's action, and lowering of blood-pressure which follow its use, are due to the dilatation of the capillaries; but when the acceleration becomes very considerable, as after injection of the nitrite into a vein, it is possible that the nervous ganglia which give the heart its automatic movements, may also play some part in producing the effect.

The method by which the substance is introduced into the system is of much importance in producing its peculiar effects. The speediest and surest way, according to Dr. Amez-Droz, is by injecting it into a vein; the next most reliable method is by inhalation. It would seem, however, to be much more rapidly eliminated when introduced directly into the blood, than when only inhaled; and larger doses could, in some instances, be employed without fatal effect, than when it was only inhaled into the lungs. By the other two methods by which it can be used, its injection into the subcutaneous tissue, and its introduction into the digestive canal, it is, according to Dr. Droz, comparatively slow and uncertain in action.

The author concludes that, physiologically, the nitrite of amyl will have to take its place as a remedial agent by the side of digitalis. Further investigations are, however, needed to fully determine its therapeutic value.

LIST OF FOREIGN PERIODICALS EMPLOYED IN MAKING UP THE JOURNAL.

COMPLETE *files of the more important, especially among those bearing directly on the subjects embraced in the plan of* THE JOURNAL, *are on hand:*

Allgemeine Medicinische Central-Zeitung.
Allgemeine Zeitschrift fur Psychiatrie und Psychisch. Gerichtl. Medicin.
Annales Medico-Psychologiques.
Annales d'Hygiene Publique et de Medicine Legale.
Archiv fur Anatomie, Physiologie, und Wissenschaftl. Medicin.
Archiv fur Mikroscop. Anatomie.
Archiv fur Pathol. Anatomie, und Physiologie und fur Klin. Medicin.
Archiv fur die Gesammte Physiologie der Menschen u. Thiere.
Archiv der Heilkunde: Leipzig.
Archiv fur Psychiatrie und Nervenkrankheiten.
Archives Generales de Medicine.
Archives de Physiologie, Normale et Pathologique.
Archivio Italiano, per le Malattie Nervosi.
Berliner Klin. Wochenschrift.
British Medical Journal.
British and Foreign Medico-Chirurgical Review.
Bulletin Generale de Therapeutique Medicale et Chirurgicale.
Bulletin de l'Academie de Medicine.
Centralblatt f. d. Medicinische Wissenschaften.
Der Irernfreund.
Deutsches Archiv f. Klinische Medicin.
Deutsche Klinik.
Dublin Journal of Medical Sciences.
Edinburgh Medical Journal.
Dorpater Medicinische Zeitschrift.
Friedrich's Blatter f. Gerichtl. Medicin.
Gazette Medicale de Paris.
Gazette des Hopitaux.
Gazette Hebdomadaire.
Jahrbuch fur Kinderheilkunde u. Physische Erziehung.
Jahresbericht u. d. Leistungen u. Fortschritte in der Gesammten Medicin.
Jahresbericht u. d. Fortschritte der Anatomie u. Physiologie.
Jenaische Zeitschrift fur Medicin u. Naturwissenschaft.
Journal of Anatomy and Physiology.
Journal de l'Anatomie, et de la Physiologie Normales, et Pathologiques de l'Homme, et des Animaux.
Lo Sperimentale.
L' Union Medicale.

Medicinisches Correspondenzblatt des Wurttemburgisches Arztliches Vereins.
Medicinische Jahrbucher.
Nuova Ligur. Medica.
Psychiatrisches Centralblatt.
Rivista Clinica.
Revue des Sciences Medicales.
Revue Scientifique.
Schmidt's Jahrbucher der In- und Auslandischen Gesammten Medicin.
The Practitioner.
Vierteljahreschrift fur die Prakt. Heilkunde.
Wiener Medicinische Presse.
Wiener Medicinische Wochenschrift.
Zeitschrift fur Biologie.
Zeitschrift der k. k. Gesellschaft der Arzte in Wien.

NOTE.—In regard to American medical journals, the editors would state that, besides their own resources in this department, they have been kindly favored by several private parties; and, through the courtesy of Drs. N. S. and F. H. Davis, they have been permitted to examine the files of exchanges of the *Chicago Medical Examiner*.

The following list of works comprises those already on hand, to be reviewed during the current year. Arrangements of the most complete kind have been made for receiving, as soon as published, all foreign works that fall within the scope of THE JOURNAL. It is the aim of the editors to spare no reasonable pains to render this department of their journal full and practical:

Untersuchungen ueber den Fieberhaften Process und Seine Behandlung. Von Dr. H. Senator. Pages, 208. Berlin: 1873.
Allgemeine Pathologie der Krankheiten des Nervensystems. Ein Lehrbuch fur Aerzte und Studirende. Von Dr. Gustave Huguenin. 1 Theil. Anatomische Einleitung, mit 130 Holtzschnitten. Pages, 296. Zurich: 1873.
Alimentation du Cerveaux et des Nerfs. Par le Dr. O. Tamin Despalles. Pages, 260. Paris: 1873.
Etudes Cliniques et Thermometriques sur les Maladies du Systeme Nerveux. Par Bourneville, 4 Fascicules. Paris: 1872–3–4.
Recherches sur l'Anatomie, la Physiologie, et la Pathologie du Systeme Nerveux. Par J. Baillarger, avec 3 Planches. Paris: 1872.
Lehrbuch der Functionellen Nervenkrankheiten auf Physiologischer Basis. Bearbeitet von Dr. Albert Eulenberg. Berlin: 1872.
Die Pathologie des Sympathicus auf Physiologischer Grundlage. Bearbeitet von Dr. Albert Eulenburg und Paul Guttmann. Berlin: 1873.

Traite d' Electricite Medicale. Recherches Physiologique et Clinique. Par les Docteurs E. Onimus et Ch. Legros. Paris: 1873.

Die Electrotherapie und Deren Besondere Verwerthung in Nerven und Muskelkrankheiten. Ein Handbuch fur praktische Arzte. Von Dr. Moritz Rosenthal. Wien: 1873.

Grundzuge der Criminal-Psychologie auf Grundlage des Strafgesetz Buchs des Deutschen Reichs, fur Aerzte und Juristen. Von Dr. R. Von Kraft-Ebbing. Erlangen: 1872.

Die Zweifelhaften Geisteszustande vor dem Civilrichter, fur Aerzte und Juristen. Von Prof. Dr. R. Von Kraft-Ebing. Erangen: 1873.

Des Nerfs Vaso-Motor. These pour le Concours d' Agregation (Anatomie et Physiologie). Par le Dr. Ch. Legros. Paris: 1873.

Handbuch der Systematischen Anatomie des Menschen. Von J. Henle. Dritter Band, Zweite Abtheilung, Nervenlehre. Braunschweig: 1873.

Ueber Progressive Muskelatrophie; ueber Wahre und Falsche Muskelhypertrophie. Von N. Friedrich. Mit II. tafeln. 4to. Berlln: 1873.

Considerations sur l' Atrophie Aigue des Cellules Motrices. (Paralysie infantile spinale. Paralysie spinale aigue de l' adulte.) Par Alfred Petitfils. Paris: 1873.

Recherches Experimentales sur le Fonctionnment du Cerveau. Par le Dr. Edouard Fournie. Paris: 1873.

Grundzuge der Physiologischen Psychologie. Von Wilhelm Wundt. Erste Heft. Bogen 1–29, mit circa 150 Holzschnitten. Leipzig: 1873.

Des Accidents Convulsifs, dans les Maladies, de la Moelle Epiniere. Par le Dr. H. Hallopeau. Paris: 1871.

Nouveaux Elements de Physiologie Humaine. Par W. Wundt. Traduit de l'Allemand, sur le deuxieme edition, et Augmentes de Notes. Par le Dr. Bouchard. Pages, 621. Paris: 1872.

Anatomie et Physiologie Cellulaires ou des Cellules Animales et Vegetales du Protoplasma, et des Elements Normaux, et Pathologique' qui en Derivent. Par Ch. Robin, avec, 82 figures, intercalees dans le texte. Pages, 650. Paris: 1833.

Physiologie du Systeme Nerveux, Cerebro-Spinal. D'Apres' l'Analyse Physiologique des Mouvements de la vie. Par le Dr. Edouard Fournie. Pages, 832. Paris: 1872.

Etude Medico-Legale, sur la Folie. Par Ambroise Tardieu. Pages, 610. Paris: 1872.

Etudie Medico-Legale, sur la Simulation de la Folie, etc. Par le Dr. Armand Laurent. Pages, 383. Paris: 1866.

F. Lussana. e. a. Lemoigne. Fisiologia dei Centri Nervosi Encefalici. Monografia premiata della R. Accademia di. Medicina de Bruxelles. Volume I.; pages, 344. Cervello E. Mesencefalo. Padova: 1871.

De la Regeneration das Organes et des Tissues en Physiologie, et en Chirurgie. Par J. N. Demarquay, etc., avec Quatre Planches. Quarto; 328 pages. Paris: 1874.

Du Dynamisme Compare des Hemispheres Cerebraux chez l'Homme. Par le Dr. Armand de Fleury, avec Planches et Gravures. 8vo; pages, 240. Paris: 1873.

Nervous Exhaustion, and the Diseases Induced by It, with Observations on the Origin and Nature of Nervous Force. By Hugh Campbell, M.D. 8vo; pages, 195. London: 1873.

Traite Theorique et Pratique d'Hydrotherapie Comprenant les Applications de la Methode Hydrotherapique, au Traitement des Maladies Nerveuses, et des Chroniques. Par le Dr. Beni-Barde. Pages, 1039. Paris: 1874.

Des Lesion des Nerfs et de leurs Consequences. Par le Docteur S. Weir Mitchell, M.D. Traduit et annote avec l'autorisation de l'auteur par M. Dastre, et precede d'un preface par M. Le Professor Vulpian. Pages, 408. Paris: 1874.

Das Myophysische Gesetz. Von W. Preyer. 8vo; 144 pages. Jena: 1874.

Traitement des Maladies Nerveuses, et des Affections Rhumatismales, par l'Electricite Statique. Dr. A. Arthuis. 12mo; 164 pages. Paris: 1873.

Etudes sur les Faculties Mentales des Animaux, Compare a celles de l'homme. Par J. C. Houzeau. Volume I.; pages 364—Volume II.; pages, 644. Bruxelles et Leipzig: 1872.

Nouveaux Dict. de Med. et de Chirurg, Pratiques, etc. Tome 15th, 1872. Articles: Goitre Exophthalmique, page 493. Tomes 16 and 17. Articles: Froid, Folie (two articles), Hallucination, Heredite.

Dictionnaire Encyclopedique des Sciences Medicales. Paris, Premiere serie, 1873; Volume XIV Articles: Cerveau, Cervelet, Cephalalgie, Cerebro-Cardiaque (nevropathie), Liquide Cephalo-Spinal. Deuxieme serie, 1873; Volume VI. and VII. Articles: Nerf Median, Melancholie, Memoire, Mendicite, Meninges, Meningite, Mesmerisme, Meurtre, Miasme, Microaphalie, Migraine.

Injuries of Nerves, and their Consequences. By S. Weir Mitchell, M.D. Philadelphia: 1872.

Lectures on Madness, in its Medical, Legal, and Social Aspects. By Edgar Sheppard, M.D. Philadelphia: 1873.

Insanity in its Relations to Crime; a Text and a Commentary. By W. A. Hammond, M.D. New York: Appleton & Co. 1873.

Contributions to Mental Pathology. By I. Ray, M.D. Pages, 558. Boston: 1873.

Principes d' Electrotherapie. Par le Dr. E. Cyon. 8vo; pages, 277. Paris: 1873.

Die Histiologie und Histiogenese der Nervosen Centralorgane. Von Dr. Franz Boll. 8vo; 138 pages. Berlin: 1873.

THE

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Original Articles, Selections and Translations.

ART. I.—PATHOLOGY OF THE VASO-MOTOR NERVOUS SYSTEM.

LECTURE II.

Vaso-Motor Centres in the Spinal Cord and Brain.—Reflex Vaso-Motor Action.—Relation of Vaso-Motor Centres in the Cord to Sympathetic.—Action on the Vessels Indirect.—Vascular *Tonus;* its Nature and Source.—Inhibitory Nervous Action; its Nature; Examples Nervous Apparatus of Heart and Vessels.—Theory of Action of Vaso-Motor Nervous System on Vessels.—Theory of Vaso-Dilator Nerves.—Functions of Muscular Vessels, etc.

GENTLEMEN: In my former lecture I was chiefly engaged in laying before you a few general facts in regard to the anatomy and physiology of the vaso-motor nervous system. I terminated what I then had to say by referring briefly to vaso-motor centres in the cord and brain. I will begin this lecture by an account of these centres.

Perhaps the first experimental observations of importance made in relation to them, were those of Budge and Waller. In their researches on the spinal cord of living animals, they found that sections, more particularly of its lower cervical and upper dorsal portions, led to the same phenomena, so far as the pupil of the eye is concerned, as when the cervical sympathetic is divided. This region of the spinal cord they named the *cilio-spinal*. The conclusion drawn from this experiment was, that the portion of the sympathetic which controls the movements of the iris, has its origin in the lower cervical region of the spinal cord.

Subsequently, M. Claude Bernard observed that the vessels of the head were innervated through the cervical sympathetic, from that portion of the spinal cord which is situated between the sixth cervical and fourth dorsal vertebræ. He admitted, in fact, two centres in this region, a cilio-spinal, and, below it, a vaso-motor centre.

Brown-Sequard, in the course of his experiments, ascertained that sections of the spinal cord, much lower down, had an influence on the unstriped muscular fibre of the head.

Subsequently, Budge and Schiff discovered another centre, the *genito-spinal*, extending from about the sixth dorsal vertebra down to the lower end of the cord, near which another centre, the *ano-spinal*, has been fixed, by M. Masius, of Liege. But, without entering into details, I may say that there is reason to think that there are vaso-motor centres scattered all along the substance of the cord, for different parts of the trunk: perhaps a continuous column of such centres, as M. Luys has supposed.

For my own part, I am strongly inclined to this opinion. But it is with this as it is with all mere opinions that I hold, I am ready to modify, or even abandon it, any day, if good reasons should appear for doing so.

Have vaso-motor centres been found in the cerebro-spinal axis, above the cilio-spinal? Yes. According to the researches of Schiff, Salskowsky, and Ludwig, and Thiry, the vaso-motor centre for the whole body lies in the medulla oblongata. Another investigator, Tscheschichin by name, has fixed it at the point of junction of the medulla oblongata, and pons varolii. Brown-Sequard would extend the vaso-motor tract up to the cerebellum and brain proper. But quite recently, Kronecker, at Leipsic, seems to have pretty definitely fixed the seat of a general vaso-motor centre for the whole body in the floor of the fourth ventricle.* That this is the true seat of a general vaso-motor centre seems probable, for it would then be near that wonderful constellation of special sensory and motor centres which lie in the medulla oblongata, pons varolii, and in the base of the brain. When we consider how suddenly a visual or auditory impression, carried back to the region in question, may be flashed out by reflex action to the whole voluntary muscular apparatus, or out on the vaso-motor system of nerves, giving rise to the almost instantaneous paleness and coldness of the cutaneous surface which we see in fear, or, on the contrary, the redness and heat of the surface which we see in shame, or under sudden and exciting passion, we seem obliged, in explaining such phenomena, to admit the existence of such a centre at the base of the brain; or, if you please, where Kronecker has fixed it, in the floor of the fourth ventricle, from which the whole vascular system may be controlled, and its action co-ordinated, or harmonized. But I must guard you against supposing that the actions of the whole vascular system are ordinarily controlled from this centre, any more than I would have you understand that all the movements we see in the limbs are excited from the corpus striatum. My belief is, that there are, at least in the lower animals, special vaso-motor centres in the whole length of the cord; but that these may, all of them, be excited from the general vaso-motor centre at the base of the brain.

Are these facts, obtained by experimentation on animals, also true for man? I believe they are. One reason for thinking so is, that the higher animals, and man, have been found to agree in so many other respects, in the physiological phenomena they present, as to lend a strong presumption in favor of essential identity in such a case as this. But there are other reasons Of course, we cannot experiment on man as we do on animals; but it sometimes happens pathological cases furnish us proofs as much to the point as if they had been the result of direct experiment. Rendu,† among other cases,

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* *Vulpian: Experiences pour rechercher si tous les Nerfs Vasculaires ont leur foyer d' Origine, leur Centre, Vaso-Motor, dans le Bulbe Rachidien.—Rev. Scientifique*, 1874, No. 35, p. 834.

† *Des troubles fonctionnels des grand sympathetique, observes dans les plaies de la moelle epiniere.—Arch. gen de Med.* 1869; p. 286–297.

has reported several bearing on this point. In one case there was a luxation of the sixth cervical vertebra, and compression, to say the least, of the spinal cord in that region. Both pupils were extremely small; the face and neck were congested, and of a bluish red: and the color of the ears was that of an intense red; while other parts of the body below the head were unchanged in color. In this case, the condition of the cord was much the same as if it had been divided in the lower cervical region, as it has been in animals; and the effects are such as we would be led to expect beforehand. I could cite other observations, but, for my present purpose, one is as good as many. Besides such evidence as the case I have just mentioned, for the existence of vaso-motor centres in the cord and brain of man, as in the lower animals, there is another kind already referred to: it comes under the head of reflex action. Sensorial impressions are conveyed to the spinal cord and brain, along the course of cerebro-spinal nerves, such as the auditory or optic nerves, or the cutaneous nerves, as those of tact, or those which take cognizance of changes in temperature; and they are often reflected outward, not along cerebro-spinal, but vaso-motor nerves: sometimes so as to affect suddenly the action of the whole vascular system. Such phenomena, and they are very common, show there are vaso-motor centres in the cerebro-spinal axis.

Let it then be regarded, as provisionally established, at least, that there is a line of vaso-motor nervous centres, in the spinal cord, extending its whole length up to the brain. This, if true, is an important fact. It will show us how it is possible for sensorial impressions—whether healthy or morbid when conveyed into the spinal cord—to be reflected out from closely related vaso-motor centres in the cord, so as to produce vaso-motor disorder in corresponding parts of the body. One of the best examples of this kind of action I can cite you to is congestion of the respiratory mucous membrane, following an exposure to cold. In this instance, an impression is made by the cold air, we will suppose, on the periphery of a cerebro-spinal nerve: the impression is conveyed to the spinal cord or medulla oblongata, and is reflected out from a vaso-motor centre, along vaso-motor nerves, to the vessels of the mucous membrane of the nose, or larynx, or trachea, and a congestion results, certain consequences follow, and we say the person has taken a "cold."

But the vaso-motor disturbance may be in many other parts of the body as well as the respiratory mucous membrane. It may be in the uterus, or mucous membrane of the bladder, or bowels; or it may be the lungs, or liver, or kidneys, or other parts, as a muscle, for example; but in either case, a vaso-motor disturbance arises in the organ, that may lead, according to its location, to a pneumonia, or a colitis, or hepatitis, or to uterine or vaginal, or vesical congestion, or inflammation; and the sensorial impression which leads in the way I have supposed, to reflex vaso-motor disorder, need not be made on the cutaneous surface. It may be made on any of the mucous surfaces, or in any of the solid organs of the body, and lead, finally to vaso-motor disorder, even in remote parts. Moreover, the impression which leads to the vaso-motor disorder may arise in the brain, or even in the spinal cord itself, as a result of mental, or, more likely, various *emotional* states, which affect the brain in certain ways, which, in turn, sends an influence down to the vaso-motor centres in the spinal cord, that may produce essentially the same effects, as, for example, the peripheral impression of cold. These injuries and diseases of the spinal cord may, and do, become, the causes of a disturbance in the action of vaso-motor centres that may be involved in the disease; and thus, in all these ways, give rise to vascular disturbances in various parts of the body. But I have said enough to show you that the discovery and fixation of vaso-motor centres in the cerebro-spinal axis is a matter of no small importance in pathology. I will, hereafter, have frequent occasion to apply the principles I have been seeking to establish.

Hoping you will bear in mind clearly what has been just said, I wish to inquire briefly by what routes vaso-motor fibres which arise in the vaso-motor centres of the cord and brain, escape from the spinal cord. They do so in two ways: by the *rami communicantes*, which pass between the spinal nerves and the sympathetic ganglia, and also by the spinal nerves themselves, especially those which pass out to the extremities.

Another question of importance is, whether the outgoing fibres in question extend outward, until they are finally distributed to the vessels? Do these fibres, which arise in the spinal cord, act *directly*, or *indirectly*, on the muscular vessels of the body?

This is a question of no small importance, as I hope soon to show you. For my own part, I have but little hesitation in saying, their action is indirect. If it were direct, it would be necessary, in many instances, for the vaso-motor fibres which arise in the cord to extend from the points of their origin to remote parts of the body. But there is not the slightest proof of this; neither is it proved, positively, that they do not continue uninterruptedly between the spinal cord and the vessels. We have to rely on indirect evidence in this case. But it all seems to me, to speak in favor of the view, that the spinal vaso-motor centres do not act directly on the muscular vessels. What the indirect proof is to which I refer, I will presently show you.

But, suppose the action in question to be indirect, can we so state the process or mode of it as to explain the facts of the case? Let us see. If the vaso-motor centres in the cord and brain do not act directly on the vessels, it must be by means of an intermediate nervous apparatus. In this view, the vaso-motor fibres, which take their origin in the cerebro-spinal axis, pass outward, and terminate at their peripheral ends—not in the muscular coat of the vessels, but in the ganglia of the sympathetic—and it may be isolated ganglion cells of the same that I have already mentioned as thickly strewn in different parts of the body. According to this view, the vaso-motor fibres originating in the cord and brain, excite to increased action, or, on the other hand, depress the action of the subordinate ganglia of the sympathetic.

If from the vaso-motor centres in the spinal cord, the sympathetic ganglia are excited to increased action, the muscular vessels—the action of which these ganglia directly control in their turn—contract, as they do when the nerves leading to them are excited by the galvanic current. If the sympathetic ganglia are depressed or arrested in their action by the spinal centres, the vessels relax in the same way as when the nerves leading to them are paralyzed.

Before proceeding farther, I must explain what is meant by *vascular tonus*. By this is meant an enduring state of contraction of the muscular walls of the blood-vessels, by means of which they embrace, with a varying degree of force, the column of blood they normally contain. How is this state of vascular tension maintained? What affords such a constant stimulus to the muscular tissue of the vessel? This stimulus comes, I have no doubt, from the vaso-motor nervous system. But what excites this nervous system so that it emits such a steady tide of motor influence to the muscular wall of the vessels? Is the action in question direct, or reflex? It may be either; but in my opinion it is, as a rule, reflex, as is the case with the heart, which is usually excited to action by the blood which fills it—so with the small muscular vessels.

They are, in health, unlike the heart, constantly filled with blood. There is the continuous impression of the blood, not only on the lining membrane of the vessel, but, possibly by constant expansive pressure, the muscular fibres are provoked to as constant contraction. From one or both these sources an enduring sensory or centripetal impression is sent back to the related vaso-motor centre or centres; and this leads to a reflex motor impulse, which is conveyed by motor nerve fibres to the muscular coat of the vessels, which, of course, contracts. Inasmuch as the sensorial impression is continuous, so is the resulting reflex contraction; and this is what is called *vascular tonus*.

The state of the vessels I have described may not be wholly due to special tonic centres, but may depend on any vaso-motor centre, whether in the cord or out of it, though, ordinarily, there can be little doubt it depends much on the larger or more central ganglia, which command wide vascular areas.

But what proof have we that the action of the spinal vaso-motor centres on the vessels is indirect? I will now call your attention, at some little length, to what has been called "inhibitory" nervous action, and in so doing answer my question. I can cite but few examples of this kind of action; my intention is, rather, to explain it to you. It is very common, especially in the cerebro-spinal nervous system.

The tendency in the spinal cord and medulla oblongata, is naturally to reflex or independent action. Coughing, sneezing, winking, breathing, and involuntary movements of the limbs, when the skin covering them is irritated, are examples of such movements. But, in spite of this natural tendency to reflex action, carried on through the special motor centres of the cord and medulla, we can, and in health, ordinarily we do, control, or even entirely arrest, such action, by an effort of the will.

The will acts from the cerebral centres, above in the brain, which centres send down commands that arrest or inhibit certain actions that otherwise would inevitably occur. A higher centre excites or arrests, at pleasure, as it were, the action of a lower centre, while the state is a normal one. This is *inhibitory nervous action.*

It is easy to give examples of it. At this point I would be glad to speak of various interesting experiments which have been made, and facts which have been collected, by such writers as Setschenow, Bezold, Pflueger, Hermann, Goltz, Schiff, Brown-Sequard, Handfield Jones, and others; but I have no time for this now.

There is one example of inhibitory nervous action, however, so complete, and so much to my purpose, I will describe it with some detail. I now refer to the action of the nervous mechanism of the heart. I will describe, first, the nervous apparatus of the heart, briefly; and this is all the more important, because the heart, like the small muscular vessels, the action of which it is one of our chief purposes to consider in these lectures, forms a segment of the vascular circle.

The heart forms a singular compromise, in respect to its structure and nervous mechanism. In its *structure*, it is allied to the voluntary; in its *action*, to the involuntary, muscles. On the one side, it is dependent on the sympathetic, and, on the other, it depends on the cerebro-spinal nervous system.

The nervous apparatus of the heart, has been investigated by many physiologists, but by none more thoroughly or successfully than by Bezold, of Jena, and the Cyon brothers, of St. Petersburgh.

The result of investigation is to the effect that the heart has, in its own structure, a special nervous system consisting, of course, of ganglia and nerve fibres. These cardiac ganglia have been divided into two classes, *motor* and *regulator*. The motor ganglia of the heart give off, as well as receive, nerve fibres, the former being distributed to the muscular tissue of the heart. The fibres from the regulator ganglia do not go to the muscular tissue of the heart, but to the motor ganglia. They may restrain, or altogether stop, the action of the motor ganglia, and, by consequence, the action of the heart. The action of the regulator ganglia is, therefore, of the kind I am trying to describe to you, *inhibitory*, as well as exciting.

But this is not all that is to be said in regard to the nervous mechanism of the heart. The heart is supplied, *ab extra*, not only by sensory but motor nerves. The latter are from two sources, the cerebro-spinal and the sympathetic nervous systems. The one, the vagus or pneumogastric, is an inhibitory nerve, the motor nerves from the sympathetic being the so-called "accelerator" nerves discovered by the Cyon brothers. The vagus does not terminate in the muscular tissue of the heart, but in the regulator ganglia. By influencing these, the action of the heart can be depressed, or even completely arrested. On the contrary, the "accelerator" nerves, terminating, it may be, both in the regulator and motor ganglia, if irritated, lead to accelerated cardiac action.

This is an excellent instance of inhibitory action. We see certain ganglia controlling the action of others. In my opinion, this is the way the larger ganglia of the sympathetic, but more particularly the vaso-motor centres in the spinal cord, act on the smaller sympathetic ganglia and ganglion cells that are so thickly disseminated through the body, and which, in their turn, act directly on the vessels. The one class of these ganglia bears the same relation to the other as the regulator do to the motor ganglia in the heart.

But as this is an important point, I must be explicit. My opinion, more fully stated, as to the action of the vaso-motor nerves on the vessels, is this: Each muscular vessel, or small vascular territory, has its ganglion, or group of ganglion cells, on which it is directly dependent. This nervous apparatus is quite sufficient for ordinary purposes. But it needs control, and, in view of thousands of similar mechanisms, co-ordination, so as to maintain an equilibrium of action in the whole body. This must be done by ganglia that have a wider sweep of relations; and as you retreat from the remoter parts of the body towards the centre, we find such ganglia larger in mass, and more extensive in connections—representative ganglia. Such ganglia we find in the chain of these bodies, on either side of the spinal column, and in the semilunar ganglia of the solar plexus. For the mere purposes of vegetative life, this is sufficient. For the sympathetic nervous system, especially its peripheral portion, is developed before the cord and brain, as is seen more particularly in the case of those monsters which at birth have no brains, or even no perfect spinal cord, yet the vaso-motor system of nerves is well developed in its peripheral portions, and the vegetative processes go on.

But in the case of vertebrate animals, there is no commanding centre, outside of the brain and cord, from whence the whole vaso-motor nervous system may be acted on at once, and in this way equilibrium in action secured, as between widely separated vascular areas. Then again, the vaso-motor nervous system must be placed in close relation with the central nervous system, so that there may be close intercommunication between what is purely physiological and what is purely mental in the lives of animals, but especially man. Accordingly, we have a chain of vaso-motor centres in the cord extending up toward the brain, the whole system culminating in a supreme centre, in the floor of the fourth ventricle, perhaps, towards which all lines of centripetal vaso-motor impulse tend, and from which all general centrifugal impressions, destined to affect the action of the whole vaso-motor system, must proceed. This chief centre forms a worthy member of that wonderful society or constellation of special centres massed at the base of the brain, within easy reach of each other.

In this view, the centre for ordinary action of a vessel, would be the nearest ganglion or ganglion cell. Nerve fibres, sensory and motor, pass between the ganglion and vessel. If all other nerve centres were cut off from this, it might still act independently; but in the observed order of things, all these ganglia are arranged with reference to associated action, so if one ganglion suffers or is destroyed, there is, at least, temporary disorder produced in the action of related ganglia. This may be called the *proximate* or *primary*, or *direct motor* class of ganglia. They are peripheral, exceedingly numerous, and very small, and are always situated very near, or even *in* the structure of the organs they are expected to influence.

If we retreat toward the centre of the body, we encounter a second class of ganglia, fewer in number, more massive, and better isolated, than those of the first class. They may be compared to the *regulator* ganglia of the heart. They may constitute the *secondary* or *regulator* class, if you please. I would not have you understand, however, that they might not perform the office of the first class, also, in regard to vascular areas in their neighborhood.

As examples of this class, I would point to all the larger, more central, sympathetic ganglia. If they should be destroyed, and they often are, still, the ganglia of the first class, if they maintain their integrity, are able, after a fashion, to act alone. As already intimated, though the ganglia of the second class are associated by intervening cords, yet no one of them can be considered as representative or chief in rank. They need, in their turn, controlling and co-ordinating centres; hence, we have a tertiary class, fewer in number, more central, and less independent of each other, than is true of either of the other classes, and these are found in the cerebro-spinal axis; and the nerve fibres which proceed from these spinal centres may be compared to the *vagus*, which acts on the regulator ganglia in the heart, which regulator ganglia of the heart belong to the second class of vaso-motor ganglia that I have described.

Finally, at the cephalic end of the chain of this third class of vaso-motor centres, and in perfect analogy with the mode of arrangement and action in the cerebro-spinal nervous system, we find a supreme vaso-motor centre, from which the whole system of vaso-motor nerves *may* be controlled, if the exigency requiring it should arise.

If what I have been saying to you is true, there is a close analogy as regards the nervous mechanisms which control the two muscular segments, the cardiac and the arterial, in the great vascular circle of the body. I see no reason to doubt the truth of the analogy I have traced. Before I pass on, I ought to tell you the analogy I have mentioned may not hold good altogether. I think the nervous apparatus of the vessels may be less complex than that of the heart. For example, it may not be necessary to admit, for the vaso-motor mechanism of the vessels, a system of nerves similar to the *accelerators* of the heart. It may suffice to admit, as going from the spinal centres, one set of nerves, to be compared to the vagus in its action on the regulator ganglia of the heart.

But I am simply giving you my opinion in many of these matters; and you know opinions often have to be altered, or even abandoned. On the other hand, it may be true we have two kinds of nerves proceeding from the spinal vaso-motor centres, one of which, by exciting the ganglia of the second class to action, would, in turn, through those of the first or proximate class, excite the vessels to increased action or vascular tonus, that is, to more energetic contraction, and so in this way diminish their calibre; or, by depressing the action of the regulator ganglia lead, through the same channel as before, to a diminution of vascular tonus, and so permitting a vessel to relax and expand. In this latter way, congestions of blood may be explained, as we may see in the next lecture.

Such is the probable way in which, it seems to me, the vaso-motor system acts on the muscular vessels. But there is another way of explaining vaso-motor phenomena, which differs from the one I have given you in some particulars. It has had some eminent physiologists as its supporters in times past, such as Schiff and Bernard. But few are inclined to accept it now; and of its two former supporters I have just mentioned, Bernard certainly appears to have abandoned it for another view, which seems to me scarcely more tenable than the one for which it was exchanged. It differs from the above mode of explanation in that it admits two kinds of vaso-motor fibres, which *act directly on the vessels.* I am inclined to admit two kinds of fibres which act on the regulator ganglia, but *not directly on the vessels.* Of these two classes of fibres which are said to act directly on the vessels, the first are called *vasor-constrictors.* If they are irritated, they lead to contractions of the muscular walls of the vessels. We may admit this kind of action, for up to this point the two modes of explanation can agree. The other class, when irritated, lead, as is supposed, singularly enough, to *dilatation* of the vessels, which is in this view just as *active* a process as the contraction is by all admitted to be. In the view I am inclined to adopt, the dilatation of the vessel is not an active process, so far as the wall of the vessel is concerned, but a *passive* one. The point at which *action* terminates is not in the *vessel*, but in the *motor ganglia* of the first or proximate class, on which the vessel is directly dependent. I see no more reason for admitting a *dilator* nerve in this case than I do for admitting a *relaxing* nerve for a voluntary muscle, when it relaxes upon the withdrawal of its nervous stimulus. Certainly, no one would think of a relaxing nerve leading to the muscle, for the voluntary muscles. The point of *positive* action in the latter case is at the spinal cord; of *negative* action, at the muscle. And so I believe it is for the involuntary muscle of the vessel. But, having admitted vaso-dilator nerves, the mechanics of active dilatation require explanation; but that is not possible without supposing a peculiar disposition of muscular fibres in the walls of the vessels, which supposition not only lacks the confirmation of actual observation, but even of analogy.

The phenomena which at first induced Schiff, Bernard, and others, to adopt this view, are very interesting, and I cannot pass them without mention and comment. They refer to certain vascular and related secretory phenomena observed during experiments on the submaxillary gland and its nerves. It has been found, in regard to the circulation in this gland, that if the sympathetic nerve fibres leading to it are destroyed, there is the usual enlargement of the blood-vessels, or congestion, that is always observed when the sympathetic leading to the vessels of a part are divided so as to separate them from their controlling centres. But there is no increase in the salivary secretions corresponding to the increased supply of blood. But now, if, in the case of another animal, the sympathetic remaining intact, the *chorda tympani* nerve is irritated, say by means of the galvanic current, the blood-vessels immediately dilate, but with more appearance of activity than was observed in the dilatation which took place after the sympathetic was divided. Moreover, the secretion from the salivary duct is vastly increased, which was not true after division of the sympathetic. There are other phenomena accompanying those already mentioned worthy of notice; but I pass them by now. What we wish to have explained is, the dilatation which follows irritation of the chorda tympani nerve. It is in the attempt to explain such cases as I have just described, that certain physiologists have been led to admit the existence of *vaso-dilator* nerves. The chorda tympani was said to be a nerve of this class. The irritation of no other nerve leading to the submaxillary gland led to any such phenomena, but the contrary. How shall we explain the phenomena I have described? They certainly are curious. There are two ways, so far as I know, that have been proposed: one is, to admit dilator nerves; the other is, to suppose, as Bernard has more lately done (if I understand him correctly), that by irritating the *chorda tympani* nerve, you do not act directly on the vessels, but on the secretory structure and nutrition of the gland, creating, in this way, a demand for more blood, which, by a species of attraction supposed to exist between the tissues and blood, creates a tide or flux of blood in the direction of the gland, and in this way leads to vascular dilatation. Of this latter purely hypothetical mode of explaining the phenomena in question, I cannot now speak. I will do so hereafter, fully, when speaking of congestion. But of the two hypotheses, I must confess, if I had to choose one or the other, I would adopt the first; and I do not think it tenable. But you will ask me, perhaps, if I reject the above explanations of the phenomena as untrue, how I would explain them myself? I would answer, that I do not know of any certain way of explaining them. Besides, the rejection of one hypothesis does not have coupled with it the necessity of substituting another in its stead. But what seems to me a better way of explaining the phenomena in question, is this: When the chorda tympani is irritated, it does not act on the vessels of the gland at all, but on the secretory structure, exciting it to increased activity. This increased activity requires, as its correlate, more blood. Now, how is it obtained? I take it to be as follows:

The excitement of this secretory structure, by irritation of the chorda tympani, gives rise to a sensorial impression in the gland, which is carried along certain nerves to the appropriate vaso-motor centres of the glands, and so influences them as to diminish the vascular tonus of the vessels they supply, at the same time, and in the degree required, by the excitement of the secreting structure by the secretory nerves, if you please. This view violates, so far as I know, no principle in the physiology of the nervous system. It requires, it is true, a hypothetical connection between the supposed secretory and vascular nerves. But the supposition violates no analogy, but, on the contrary, is supported by many.

It makes it necessary to have certain nerves whose office it is to excite to action the secreting structure of glands. This is no unfamiliar conception in physiology. In the next place, it makes the excitation of the glandular tissue give rise to a sensorial impression, at the periphery of nerve fibres, which lead, we will suppose, to a regulator vaso-motor centre, which, in turn, by a reflex act, *inhibits* or arrests the action, or capacity for action, of the direct vaso-motor centres for the vessels of the gland, and, as a result, the vascular-tonus of the same is diminished, and the vessels relax so as to admit a greater volume of blood to the gland, as one of the essential conditions of an increased secretion. The difference between this case and the one in which the sympathetic was divided, and more blood, by consequence, was admitted to the gland, consists in this: that in the one case the secretory nerves, viz.: the chorda tympani, is excited, while in the other it is not. The explanation I have offered you, if it is worthy of the name, would make the dilatation of the vessels, following irritation of the chorda tympani, a reflex result, which, as such, has its origin and measure in a sensorial impression arising in the secreting structure of the gland, in consequence of its excitation by the "excito-secretory" nerve of the gland, viz.: the *chorda tympani*. But I need not dwell longer on this case, important as it is. In my judgment, it may be so explained as to make it conform to the theory of vaso-motor nervous action I have adopted, and tried in a plain way to set before you. I would not have spent so much time with this matter as I have done, but it is one of very great importance in the pathology of the nervous system. I have been willing to be a little prolix, even tedious, to place before you what most assuredly you will not find stated with the same fullness, nor exactly in the same way, in any books accessible to you.

I would not have you suppose the views I have advanced are essentially new. The facts I have referred to are well known. If there is anything new in what I have said to you, it consists in the mode of interpretation of the facts. It now remains to me to bring the hypothesis I have set before you, face to face with the observed phenomena in health and disease, as regards the action of the small vessels. This will require me to review the whole subject of vascular action, congestion and inflammation. The questions we are now to open are among the most central and living in the whole field of pathology.

Before I enter on their consideration, however, I must say a few words to you in regard to the physiological office of the small muscular vessels.

There is one office they have, about which all observers are agreed; it is this: by their contraction or dilatation they regulate the quantity of blood that may be admitted to the capillaries of a part. This office is an important one, and cannot be too much dwelt on in its relations to the nervous system.

But I will pass it by now, to speak of another supposed function of the muscular vessels. It is one I have myself, for twelve years or more, maintained as probable.* It is that of their propulsive action, aiding in circulating the blood through the capillaries by a sort of vermicular contraction, varying in its degree. But the discussion of this important subject must be left until my next lecture.

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* See the *Chicago Medical Examiner* for 1864, and the Transactions of the Ill. State Med. Soc. for 1863.

Art. II.—MECHANISM OF REFLEX NERVOUS ACTION IN NORMAL RESPIRATION.

An Address Delivered February 16, 1874, before the New York Society of Neurology and Electrology, by Austin Flint, Jr., M.D., Professor of Physiology in the Bellevue Hospital Medical College, New York.

With Remarks by J. C. Dalton, M.D., Prof. of Physiology in the College of Physicians and Surgeons, New York.

Phonographically Reported by Geo. W. Wells, M.D., of New York.

Mr. PRESIDENT, and Gentlemen of the Society: I shall have the honor, this evening, of making some remarks on the mechanism of nervous reflex action in normal respiration. A great part of the statements that I shall make, and the views advanced upon this subject, are derived from personal experimentation; but they are by no means entirely new, for many of the experiments upon which my views are based were published in the *American Journal of Medical Sciences*, in October, 1861. Still, these experiments, which seem to me to be of considerable importance, have been noticed so little in physiological writings, that I venture to assume that they may be new to many of those who now listen to me.

After Marshall Hall had formularized the ideas of certain of his predecessors, with regard to what he termed reflex action, it was pretty generally understood by physiologists that the movements of respiration were of a purely reflex character, unless they were modified by voluntary acts; and that the ordinary movements of respiration, which take place without the intervention of the will, were entirely reflex.

The experiments that I shall detail this evening were based upon, or, rather, suggested by, an experiment made in 1664, by the celebrated Robert Hooke, and published in the *Philosophical Transactions*, for 1667. This experiment, though it could not be completely understood at the time it was made, in 1664, is very instructive. It consisted in introducing a bellows into the trachea of a dog, making an opening into the chest, cutting off a portion of the lungs, and forcing air through them; and it was found that, so long as air was forced through the lungs in this way, the animal, though sensible, made no efforts at respiration. I may here anticipate enough to say that I shall assume that, in this experiment, while air was supplied to the system, the animal felt no want of it, had no inclination to respire, and consequently did not respire.

In studying the subject of the reflex nervous action in respiration, we are immediately struck with the anatomical relations of the pneumogastric nerves to the respiratory apparatus; and it is all the more important to study the relations of these nerves to the process of respiration, as they arise near that point in the medulla oblongata where the so-called "vital knot," or the respiratory nervous centre, is supposed to be situated.

It might be opportune, perhaps, to rapidly sketch the condition of our knowledge respecting the influence of the pneumogastric nerve upon respiration.

As you all know, the pneumogastric nerve is one of immensely wide distribution, and is connected with various distinct functions. The branches that are distributed to the respiratory organs are the following:

The superior laryngeals, which are distributed to the mucous membrane of the larynx and the membrane covering the top of the larynx, sending off a branch on either side to the crico-thyroid muscle, this branch being a mixed nerve.

Next in order, we have the inferior, or recurrent laryngeal nerves, which are distributed to all the intrinsic muscles of the larynx, except the crico-thyroid. These nerves are composed entirely of motor filaments, and are derived from a variety of sources. The experiments of Bernard, which have been so often repeated by Dr. Dalton, myself, and others, of extirpating the spinal accessory nerves, or the section of the communicating branches to the pneumogastrics, show that this is the nerve of phonation; and the filaments that preside over the voice pass to the larynx through the recurrent laryngeals.

Then we have, distributed to the lungs themselves, the anterior and posterior pulmonary branches, which go, almost exclusively, to the mucous membrane of the pulmonary structure. These branches communicate with the sympathetic; but, according to Sappey, they do not go to the walls of the blood-vessels, being distributed to the mucous membrane lining the air-vesicles.

So much for the distribution, in general terms, of those branches of the pneumogastrics which go to the lungs; and this distribution being so extensive, you can readily see that we can hardly discuss the reflex nervous action in respiration, without taking the action of these nerves into consideration.

The pneumogastric is originally an exclusively sensory nerve. Experiments are somewhat obscure upon this point, on account of the difficulty in irritating the original roots of the pneumogastrics, without involving filaments of other nerves; still, the careful experiments of Longet showed that when the spinal cord of animals is opened, and the roots of the pneumogastrics are carefully isolated and stimulated, no movements follow their irritation. This shows that the original filaments of the pneumogastric are not motor. But the pneumogastric, as it emerges from the cranial cavity, receives numerous communicating motor filaments, and thus, in its course, it is a mixed nerve. Following out the filaments that are distributed to the respiratory apparatus, we find that the filaments from the superior laryngeal going to the crico-thyroid muscle, are almost exclusively motor; the motor filaments of the recurrents go to the intrinsic muscles of the larynx, whereas the true pulmonary branches are distributed to the mucous membrane. Therefore, excluding the movements of the larynx, the action of the pneumogastric in the reflex phenomena of respiration, theoretically, would be that of a sensory nerve, conveying to the respiratory nervous centre an impression, or sensation, which gives rise to the movements of respiration.

If both pneumogastrics, however, be divided, we find that the respiratory movements are very much diminished in frequency; and I have in my mind an experiment in which they were reduced from twenty-four to four or six in a minute; yet, they still continue; and this simple experiment, so often performed as a class-demonstration, is a denial of the proposition that the pneumogastric nerves are the only nerves for the transmission of the so-called *besoin de respirer*, or sense of want of air, to the respiratory nervous centre. If the pneumogastrics were the only nerves which have this function, respiration should cease after their division; but it does not.

I do not think that physiologists are at present able to explain the cause of the great diminution in frequency of the respiratory movements, after the division of both pneumogastric nerves; but this is, nevertheless, an invariable phenomenon. In the experiment to which I have referred, curiously enough, the animal did not die; and when I presented him to the class, some three weeks after the section of the nerves, the number of respirations had returned to the normal standard. I imagine that a reunion of the two ends of the divided nerve had occurred. A post-mortem examination (the animal being sacrificed in another experiment) showed that the nerves, though not, perhaps, completely united, had formed a partial union, at least, between the divided extremities.

The condition of the lungs after the division of the pneumogastrics, that is, in cases where death follows such division, is peculiar, and was, for a long time, unexplained by physiologists. In animals that live for three or four days and subsequently die, the lungs present pretty generally, throughout their entire substance, a carnified condition. They are solid; will sink in water; but still do not present evidences of inflammation. It was thought, at first, that this was due to inflammation; but physiologists failed to find the positive evidence of any such action. Bernard, I think, has given the correct explanation of this peculiar appearance. He observed that, when the respiratory movements are gradually diminished in frequency, they are immensely increased in intensity; that the inspirations are remarkably prolonged and profound; and that the chest, in the inspiratory act, is extraordinarily distended. He advanced the idea that this extreme dilatation of the air-cells induced capillary hæmorrhage in certain parts of the lungs; that, as this extended, the blood coagulated; and, finally, the lungs became almost solid.

Galvanization of both pneumogastrics in the neck arrests the respiratory movements, if it be powerful; and this action is reflex, not direct. If the nerves be divided, galvanism of their peripheral extremities has no effect on respiration, though it arrests the action of the heart; whereas galvanization of the central ends arrests respiration in the same way as galvanization of the nerves before their division. Galvanization of the superior laryngeal nerves arrests respiration, and renders the animal motionless. This effect follows powerful galvanization of any of the sensitive nerves, though not so certainly and promptly as galvanization of the superior laryngeals. If the superior laryngeals be powerfully galvanized, respiration stops immediately, and is arrested at the instant the current is applied, but more easily during inspiration than expiration. This arrest of the respiratory movements is particularly marked as regards the action of the diaphragm. I have made these preliminary remarks to show that there is very little known with regard to the reflex phenomena of respiration, operating through the pneumogastrics.

Although the proposition that I am about to enunciate has been denied by a few physiologists, still, the greater number believe that the medulla oblongata is the respiratory nervous centre. Adopting this view, which is almost universally accepted, the mechanism of the reflex phenomena of respiration may be briefly stated as follows:

These phenomena require three conditions:

1. The physiological integrity of nervous filaments conveying an impression, or sense, to the nervous centre.

2. The existence and physiological integrity of the nervous centre.

3. Finally, the physiological integrity of the motor nerves which convey the stimulus that is generated at this nervous centre to the respiratory muscles.

If we assume that respiration involves a reflex action, we must admit that there are nerves which convey certain impressions to the medulla oblongata. We find that the medulla oblongata is the respiratory centre; for, when this centre is destroyed, the movements of inspiration instantly and permanently cease. A single series of experiments has been published by Dr. Brown-Sequard, which are assumed to prove that respiratory movements may occasionally persist after destruction of the medulla oblongata; they have never been confirmed, and cannot be accepted as demonstrating that the medulla oblongata is not the centre for respiration.

The sensation which we experience of want of air has been called, by the French, the *besoin de respirer*. It might be well enough to call it the sense of the want of air; but, under ordinary circumstances, when respiration is free, when the surrounding air is pure and in abundance, this sensation is not felt, except at the medulla oblongata. This impression, however, at proper intervals, is conveyed to the medulla, and keeps up the respiratory movements, without our knowledge; and it is only when there is a greater deficiency of air than usual, or when there is an obstruction to respiration, that we feel this sense of want of air as a positive sensation, in the form of a sense of suffocation, more or less pronounced. I think that the old experiment of Robert Hooke established this point; and it certainly demonstrates it, when taken in connection with what we have learned of late years.

In Robert Hooke's experiment, the dog was supplied artificially with air, completely and efficiently; and he noticed that, so long as he supplied the respiratory needs, though the animal looked around and was entirely sensible, he made no respiratory efforts. This showed that, during the free passage of air through the lungs, the want of air was not felt by the medulla oblongata, and there was no stimulus to produce respiratory movements. There was no necessity felt for respiratory movements, and none took place. This experiment suggested my own observations, in 1860–61. I put an animal, a dog, completely under the influence of ether; introduced the nozzle of a bellows into the trachea; opened the chest; turned back the anterior walls, by breaking the ribs, so that I exposed the lungs and diaphragm, and then very carefully maintained artificial respiration. I found that while artificial respiration was complete and efficient, the animal remained perfectly quiet, and made no respiratory efforts. I could see, in this experiment, the slightest movement of the diaphragm. I then interrupted the artificial respiration for a moment. Very soon I could see the diaphragm begin to quiver; it contracted, at first, slightly; then more and more powerfully and rhythmically; and the animal finally opened the mouth and made ineffectual efforts to breathe. I then resumed the artificial respiration, and in a short time the animal became quiet, when the respiratory needs were entirely supplied.

I then exposed an artery, and introduced in it a stop-cock, so that I could take blood from the vessel at will. While I kept up artificial respiration, I drew a little blood from the artery upon a white plate. It had all the characters of pure arterial blood. I then had my assistant, who was working the bellows, stop the artificial respiration, and I allowed the blood to flow in a small stream from the artery. I found, always and invariably, that when the blood began to be dark in the artery, and not before, the animal made efforts to respire.

There are several views, which have been advanced by physiologists from time to time, as to the location of the *besoin de respirer.*

Marshall Hall and some others thought that it was due to a want of air in the lungs themselves, and that this want was conveyed by the pneumogastric nerves to the medulla oblongata; but I do not see how, with this supposition, it is possible to explain respiratory movements which occur after division of both pneumogastrics.

Reid thought that the sense of want of air was due to the circulation of venous blood in the medulla oblongata; a view which is entirely theoretical and incapable of positive demonstration.

Berard thought that the sense of want of air, or the *besoin de respirer*, was due to the distension of the left side of the heart by venous blood when respiration was arrested. In support of this view, he brought forward the well-known fact that, in certain cases of disease of the heart, even when the lungs are perfectly normal and completely filled with air, there is frequently a sense of suffocation.

Vierordt thought that the sense of want of air was due to the circulation of venous blood in the substance of the nerves themselves.

Volkmann, in 1842, made the very important observation, that an animal experiences the sense of suffocation, when deprived of air, after division of both pneumogastrics. This fact was well known. Every one who has divided both pneumogastric nerves in a cat must have noted that the animal experiences intense distress from suffocation. In this animal, the cartilages of the larynx are very flexible, and paralysis of both recurrent laryngeal nerves, which follows division of the pneumogastrics in the neck, causes the glottis to close in inspiration, so that the animal is almost immediately deprived of air. Volkmann reasoned, from this fact, which had often been observed before, that the sense of want of air resides in the general system, and is not to be referred to any particular organ or organs.

If I may be permitted, now, to continue the account of my own experiment, I think I can show that it is certain that the sense of want of air resides in the general system; and, farthermore, that it is due to a want of oxygen in the general system.

Here we have an animal with the heart and lungs exposed; a bellows placed in the trachea, and artificial respiration maintained; but there are no efforts at breathing, so long as air is supplied in sufficient quantity. We put a stop-cock in the artery, and, while artificial respiration is continued, there is the natural red color to the blood. But we stop the respiration, and we find that, just so soon, and no sooner, as the blood becomes markedly dark in the arteries, the animal begins to make efforts at respiration, and feels the sense of want of air. I think this experiment shows that the sense of want of air is due to the circulation in the system of blood more or less venous in its character. I say I think it shows this fact; I am sure it shows it, in connection with the other facts bearing upon the question.

What is the cause of this sense of want of air, and what are the conditions of the blood that are different from the conditions during efficient artificial respiration? Of course, whatever they may be, these two conditions are present: one, a deficiency of oxygen in the blood, that is rendered more or less venous; and another, the presence in the arteries of blood containing an excess of carbonic acid. The question now arises, whether the sense of want of air be due to a deficiency of oxygen in the system, or to the irritating qualities of carbonic acid. How can we separate these two conditions, experimentally, and how can we deprive the tissues of oxygen, without supplying blood charged with carbonic acid? A very simple way is to drain the system of blood; for, if blood get to the system, there is no question but that oxygen will be carried to the tissues, it being always conveyed by the blood, and by the blood alone. Therefore, if the system be deprived of blood, no oxygen can get to the tissues. Again, if we drain the system of blood by cutting out the heart, we answer the question whether or not the sense of want of air be due to the distension of the left side of the heart by venous blood. If you take this same animal, that is not breathing, and in which the respiration is kept up by the bellows, and tie a ligature around the aorta, he begins to breathe, although the lungs are supplied with air, for the reason that the oxygen-carrying blood is cut off from the system. If, now, in this same animal, we suddenly cut out the heart, the system is, of course, almost instantly drained of blood, and the animal always makes violent and repeated respiratory efforts, although the lungs are fully supplied with air. It seems to me that these experiments show conclusively that the sense of want of air is derived from the general system; that it is due to a want of oxygen in the system, and not to the irritating properties of carbonic acid; and that this sense is entirely analogous to the sense of hunger and the sense of thirst. The sensations of hunger and of thirst are subjectively referred to the stomach or to the mouth and fauces; but they really reside in the general system. If a fistula be made in the stomach of a dog, and if the animal be allowed to drink, after having been deprived of water for a day or two, the water will flow out through the fistula as fast as it is taken into the stomach; and, although the animal will continue to drink, the water is not absorbed, and the thirst is not satisfied. I have seen animals drink, in this way, gallons of water, being satisfied with a moderate quantity after the fistula has been closed. Also, if food be taken into the stomach and not absorbed, the sense of hunger is only momentarily appeased; but this sense is referred to the stomach, because food is naturally introduced into the system by the stomach. So the sense of want of air, which I believe to be due to the want of oxygen in the tissues, is referred to the respiratory organs, because it is by filling the thorax that we naturally supply this deficiency in the system. If the sense of want of air be exaggerated, it constitutes the sense of suffocation; and this is one of the most distressing sensations of which we have any knowledge.

It has been observed that convulsions very often follow hæmorrhage; and this fact has been found very difficult of explanation. But hæmorrhage is really suffocation; and convulsions are generally observed in suffocation. It makes very little difference, practically, whether we drain the system of the oxygen-carrying fluid, or whether we prevent oxygen from going to the lungs; we have, in each case, the same result, as far as respiration is concerned; and, in death from profuse or sudden hæmorrhage, it seems to me that the convulsions are, in fact, no more than convulsions due to suffocation. This view seems to offer a satisfactory explanation of the convulsions following hæmorrhage. There is one point, however, in this connection, which is interesting, and which I appreciate as fully as any one who now hears me.

I have assumed that draining the system of blood, by preventing the oxygen from getting to the system without carrying to the tissues carbonic acid, proves that the sense of the want of air is due to a want of oxygen in the tissues, and not to the stimulation of carbonic acid. Carbonic acid does not originate in the blood, and is undoubtedly an excretion. If we take a muscle cut from a living frog, and put it under a bell-glass containing oxygen, even though it contain no blood, this muscle will respire. Again, if we put the same muscle in an atmosphere of hydrogen, we find that a certain amount of carbonic acid is given off. In normal nutrition, carbonic acid is carried away from the tissues, almost as soon as it is formed, by the blood. If, then, the system be drained of blood, what is to prevent the carbonic acid from accumulating in the tissues, and may not this be the cause of the sense of want of air?

I have tried to imagine experiments to meet this objection. I have tried to devise some means of getting rid of the carbonic acid from the tissues, that will not, at the same time, either supply the oxygen, or send through the tissues a fluid like blood, containing carbonic acid. This flaw in my argument I cannot correct experimentally.

One other important point in this connection, which may be of more interest to some of my hearers than those to which I have thus far called your attention, is the cause of the first respiratory effort made by the new-born child.

Many of the ancient writers regarded the placenta as the respiratory organ of the fœtus; and we now know positively, that the fœtus *in utero* gets its oxygen from the blood of the mother through the placental vessels; but when the child is born, this source of supply of oxygen is cut off, and the first act of pulmonary respiration is performed, this being the commencement of the function which continues to the end of life.

What is the exciting cause of this first respiration? It has been shown positively, by experiments upon animals, that the first respiration is due to an arrest of the placental circulation. I have frequently opened the abdomen of dogs and cats, big with young, and taken the young from the uterus, when they had hardly attained one-fourth of their size at term; have laid them on the table, and respiratory movements have always occurred in a very short time after they were separated from the mother. Experiments have been made upon animals, by opening the abdomen and pressing upon the umbilical cord, and in a short time, respiratory movements have occurred.

It is well known to gynæcologists and obstetricians that respiratory movements occasionally occur in the human fœtus, *in utero*, as a consequence of some interference with the placental circulation; and the amniotic fluid, and even meconium, have been found in the respiratory passages.

A very thorough exposition of these facts has lately been made by Dr. B. S. Shultze, in a work published at Jena, in 1871, entitled *Der Scheintod Neugeborener*, in which the points I have stated are so fully set forth that there can be no doubt upon the subject. It seems to me that the respiratory efforts before birth constitute a very strong argument in favor of the view that I have stated; and it seems to me certain that the first respiratory movements after birth are due to the following conditions: The placental circulation is arrested; the new being feels the sense of the want of air; and the impression is conveyed to the medulla oblongata, where a stimulus is generated which is carried by motor nerves to the respiratory muscles. The respiratory muscles then contract, and thus the lungs are, for the first time, distended with air.

The general results of the experiments that I have detailed this evening, and which, I may say, I have performed over and over again, are the following:

Respiration is a reflex phenomenon. The movements of respiration are reflex. There is a special respiratory nerve centre, which is situated in the medulla oblongata. When this nervous centre is destroyed, no respiratory movements can take place, because there is no centre to receive the impression of want of air. Respiratory movements are due to an impression made upon the centripetal nerves: and this impression is due to a want of oxygen in the general system. The sympathetic system may possibly be involved in this action, but this point has not been determined. The sense of the want of air, conveyed to the medulla oblongata, gives rise, under ordinary conditions, to respiratory movements, which take place without the consciousness of the individual. Under ordinary conditions, respiration is carried on by the medulla oblongata, and does not involve the action of the brain. Whenever there is any difficulty in respiration, the sense of want of air is exaggerated, until it constitutes a sense of suffocation, which involves voluntary efforts on the part of the individual to supply this want of air.

[Prof. Flint was followed by Prof. J. C. Dalton, Professor of Physiology in the College of Physicians and Surgeons.]

REMARKS OF PROF. DALTON.

I have been very much interested, Mr. President, in the question of the location of the sense of the want of air, and reflex nervous action in respiration, and am pleased that Professor Flint has given us a paper on this subject. It has been a sort of puzzle to physiologists ever since the discussion began. We have all known that the stimulus to respiration, as the Doctor says, consists in some deficiency of air supplied to the lungs: but, of course, the question is, whether that deficiency, or that disagreeable sensation or impression consists, properly, in a want of oxygen, or in the irritation produced by accumulation of carbonic acid. I think that nearly everybody is inclined, at first, to think that it is due to a collection of carbonic acid; and more especially because we all know the bad effects of CO_2 when present in an unusual quantity, as when an animal or man is subjected to the respiration of an atmosphere constituted mainly, or entirely, of this gas.

But we have found that there are two things to think of when an experiment is tried of subjecting an animal to the respiration of such a gas as CO_2. If you place an animal in an atmosphere of this gas, it dies, of course; but not necessarily because it is breathing CO_2. This effect may be due to the absence of oxygen. It does not take a great deal of discussion to prove that the last is really the cause of death. I think the frequency of accidents which have happened to the human subject, from inhalation of CO_2, has been the principal reason why physiologists are disposed to attribute to the irritation of carbonic acid the stimulus of respiration.

Suppose a man descends into an old beer-vat, where fermentation has been going on: of course, as soon as the gas begins to be inhaled, the man feels an unpleasant sensation, and in a short time he falls insensible. When the next man goes down to his rescue, he also falls, in the same manner; and this is repeated at each new trial, until the bystanders become convinced that to enter the place is death. In such a case as that, it seems as though the individual had been poisoned by the deleterious effects of the carbonic acid. But we know the same thing would happen if the gas were nitrogen or hydrogen, instead of CO_2; and as soon as this double interpretation of such a phenomenon as that is fully appreciated, everybody understands that the question is not so simple as it first appears; and that when death takes place, or the peculiar sensation of suffocation is produced, it may be due to either of these two causes, namely: the effects of CO_2, or the absence of oxygen.

In regard to the cause of the normal stimulus to respiration which exists originally in the mucous membrane of the lungs, and is conveyed to the medulla by the pneumogastric nerve, there is room for the same double interpretation. I must say that I have been, heretofore, disposed to attribute it to the accumulation of CO_2. The experiments which Dr. Flint has detailed to-night, certainly go a very long way in favor of the opposite theory. They are especially instructive and important in connection with the experiments performed a few years ago, with a similar object, by Pflueger. They were intended to ascertain whether the stimulus to respiration was due to the absence of oxygen, or to the presence of carbonic acid.

In regard to the respiration of the fœtus, there is not the same difference in color between the venous and arterial blood in it that there is in the adult. There is no perceptible difference in the color of the blood in the umbilical arteries and veins. And what is more, both the two kinds of blood in the fœtus are venous in color, and not arterial. The blood of the fœtus is all of a dark color. That shows that there is no very large amount of oxygen in the blood. Of course the fœtus cannot get along without oxygen in some form. But it directly does not absorb free oxygen in any large amount from the placenta of the mother.

The other difficulty which still remains depends on the phenomena manifested when an animal breathes a mixed gas containing both oxygen and carbonic acid. If we place an animal in an atmosphere of pure CO^2, we are not surprised that it dies. If we put him in an atmosphere of pure nitrogen, he also dies. Consequently, the absence of oxygen alone is sufficient to cause death. But it is certain that the CO_2 also has some effect. If we consider the constitution of the atmosphere to be $O\ 20 \times N\ 80$, and we enclose a sparrow in such a normal atmosphere of sufficient quantity, it will live indefinitely Twenty per cent. of oxygen, therefore, is sufficient to maintain life. But now suppose that we shut up the same sparrow in an atmosphere consisting of O 20 and CO_2 80. It will die in two and one-half minutes, with all the symptoms of suffocation. But we know that it is not at all necessary to make a preparation of CO_2 as great in order to cause death. A much smaller quantity, in an artificial, mixed atmosphere, will produce the same effect.

What we want to get at, however, is what it is in such an atmosphere that produces dyspnœa. When we know that, we shall know what is the cause of the natural stimulus to respiration. Suppose we take an atmosphere which consists of O 20 N 74 and CO_2 6. Such an atmosphere produces dyspnœa. Now, the quantity of CO_2 is so slight here, and at the same time we still have the normal proportion of O, that it seems to me it presents a very strong case for those who believe carbonic acid to be the normal stimulus to respiration.

An experiment which I have frequently performed consists in placing a pigeon under a bell-glass, with an open top, and then slowly introducing a quantity of CO_2. The CO_2 is passed into the upper part of the bell-glass, just within its open mouth, so as to mix gradually with the air. The animal is thus gradually made to breathe an atmosphere which becomes more and more loaded with CO_2; and just in proportion as this gas becomes more abundant in the mixture, the signs of dyspnœa become more manifest, and at the end of two or three minutes the bird becomes paralyzed and insensible.

These are the points in regard to producing this mixture of gases that seem to me not fully explained, on the ground that the want of ogygen is the only cause by which the medulla oblongata is stimulated to the act of respiration.

What I wish, Mr. President, more particularly to speak of before closing, is the effect of respiration of various gases, as I have observed it upon myself. I do not know whether these experiments have ever been performed by any one else.

Some few years ago, I wished to see what was the result, by personal experimentation, of breathing pure carbonic acid gas, as well as mixtures of carbonic acid gas and atmospheric air. For this purpose I had a small gasometer constructed, with the weights so accurately adjusted that the movements of respiration would be entirely unimpeded. The cavity of this gasometer was connected with a flexible tube, and mouth-piece. The gasometer being first filled with pure CO_2, I expelled the air from the lungs as fully as possible, by a long expiration, and then inhaled the CO_2 through the tube and mouth-piece.

The first thing noticeable in attempting to breathe pure CO_2 is, that it is difficult to do so, since by coming in contact with the mucous membrane of the larynx, it produces a strong local stimulus and, at first, a spasmodic closing of the glottis. The gas tastes warm and pungent. I should not say that it is actually painful, but only highly stimulating; but with a little patience, this irritating effect passes off. The glottis becomes less sensitive, and carbonic gas and CO_2 enters the larynx and passes down the trachea. When it has reached this point, the lungs make no further resistance, and experience only a sensation of warmth as the gas penetrates into them. I have succeeded in breathing in this way from 50 to 100 cubic inches of pure CO_2. At first, no very marked effects are perceptible, but at the end of three seconds, or between three and four, there comes on, very imperceptibly, an intense feeling of suffocation; at least, it would be a feeling of suffocation if it were more powerful: and I do not know any other term to use for it; but it is a most imperative and altogether irresistible desire to breathe; and that it is absolutely necessary to get the greatest amount of breath in the least possible time. It is an excessive sensation; and yet it is not exactly as distressing as actual suffocation would be. It is a feeling that, however rapidly you may breathe, you cannot satisfy the extraordinary desire for breath that exists in the lungs at that time. At the same time, there is a little flushing of the face, with projection of the eyeballs, and some dimness of vision, and confusion of mind. These effects, however, are but momentary, and pass off in a few minutes, leaving only a sense of quietude somewhat like that from the inhalation of an anæsthetic.

I have also breathed a mixture of atmosphere and carbonic acid gas. A mixture of one part CO_2 and three parts atmospheric air, can be breathed without any immediate difficulty; but the same sense of suffocation is produced, only not so excessive as when the pure gas is used. These results may not have been unexpected, but they are very marked. What makes them of some importance, however, in this connection, is that the respiration of pure hydrogen, or pure nitrogen, does not produce the same effects. There is no immediate or decisive sensation of suffocation, when breathing these gases, at all to be compared with that caused by CO_2. I have been able to breathe from 100 to 400 cubic inches of nitrogen at a time, without any difficulty; and the same thing is true of hydrogen.

Now, I do not imagine, by any means, that these facts settle the question, or prove that CO_2 is the only normal stimulus to respiration; but I am sure that there is something in the contact of CO_2 with the mucous membrane, which produces an effect entirely different from that caused by other indifferent gases with the same membrane.

Art. III. — SPEECH AS A REFLEX ACT. — THE PHONO-MOTOR NERVOUS CENTRE.

By Dr. E. Onimus.

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THE importance of reflex action in all the phenomena of the nervous system, is to-day universally recognized. It may be said that it presides over all the functional manifestations. It is not necessary, in fact, to limit the designation reflex to those movements which the spinal cord can produce without the intervention of the brain; but in a general way we may consider those as reflex acts which are due to the transformation of a centripetal impression into a centrifugal reaction; no matter whether that change takes place in the cord or in the brain; whether the impression be central or peripheral; whether it proceed from a nerve of special sense, or a spinal sensory nerve; from ganglion cells, or from centres presiding over psychological phenomena. In a word, reflex acts of the most indisputable character may be recognized in the intellectual functions themselves; and we aim to show in this article the influence of this automatism on the function of speech.

It is necessary to first remark, at this point, that some reflex acts are very complex, and that they produce an assemblage of co-ordinated movements corresponding to a fixed purpose. We may, therefore, at this point, recall certain fundamental facts which we insisted upon in our paper on the "*Phenomena Following the Removal of the Brain.*" (*Journal d' Anatomie et de Physiologie*, Nov. 1870–1.)

Reflex acts take place from all sections of the cord, from all groups of nerve cells, from a ganglion, for example; but in proportion as the central nerve mass is large, they take on special characters.

When the cord is severed low down, only a movement limited to the limbs can be brought about: but little by little, as it is cut higher up towards the encephalon, the motions become more extensive. Finally, when the cerebral lobes themselves are alone destroyed, no reflex act limited to one member is seen; and any excitation whatever causes a general movement, which in animals is exhibited in the movements of locomotion, leaping, swimming, or flight.

When the cord is separated from the brain at a point below the medulla oblongata, general movements (leaping or natation of the frog, for example) are impossible; and in this case the reflex movements are always proportional to the energy of the excitation. But when the cerebral lobes only are removed, the case is different: whether the excitation is powerful, or the reverse, the movements which follow it are always movements of the whole body. We may remark here, that the removal of the cerebral lobes does not cause any of the motions of the uninjured animal to cease: these occur with even more regularity than before; we might almost say that they take place more normally than in the normal condition.

On the other hand, these movements follow inevitably; it seems to be impossible to avoid them. It is as if the frog, deprived of its cerebral lobes, and put in the water, is compelled to swim; or the pigeon, without its brain, and thrown into the air, is obliged to fly.

We may conclude from these facts that the general movements of the whole body are produced by certain special nervous centres, situated outside of the brain, although in the encephalic region; centres independent of the will as regards mechanism, but which are essentially passive, and which do not enter into action except as incited from the brain, or by peripheral irritation.

We ought not, therefore, to recognize in all the habitual and voluntary movements a direct action from the brain on each muscle; and we are compelled to acknowledge that there exists, outside of the brain, centres for movements which serve as intermediators between the will and external acts. The brain sends, so to speak, the general order for the execution of such and such motions; and it is these co-ordinating centres and directors of muscular action that are charged with the details of the functional act.

Further, as we have already stated above, these automatic movements have a precision, a perfection, that those over which the will has a constant and direct supervision do not invariably possess.

We have distinguished two classes of these general movements produced by these locomotor centres, and apart from any direct influence of the brain; the first comprises the instinctive acts, such as those of flight in birds, and of swimming in ducks (we have observed that young ducks, raised by a hen, which have never before been in the water, and from which we had taken the cerebral hemispheres, immediately began to swim when placed in the water). The second class includes all those movements of the body which are only met with in adult animals; which we have called reflexes of habit or education; such, for example, is the habit of the pigeon in putting its head under its wing when it sleeps, or the motion of picking and smoothing its plumage. We have never observed these acts in young pigeons after removal of the brain, while in old birds this habitual act continued to be performed subsequent to the operation. We see in this instance, therefore, a striking exemplification of the influence of habit on the automatic acts.

Finally, organic or physiological modifications of the system, when they are not merely temporary ones, act equally on the general reflex movements. For instance, if a male frog is decapitated in its season of copulation, and is then excited, movements of the anterior limbs, such as are made when embracing the female, are provoked.

It is true, then, that the co-ordinated and automatic movements of instinct or education, and those coincident with a temporarily exaggerated function, exist unimpaired after the removal of the brain. It is then correct to say that, in the normal condition, they are effected by the same mechanism, and that the will has only an excitant and directing influence over them.

That which is true of the general movements belonging to the other principal functions, appears to us equally true for the function of language, for which we believe we can establish a centre which presides, independently of the will, over the acts of phonation. To be sure, the question in this case becomes more complex, because the nerves producing the impression are no longer sensitive nerves; and it is impossible to establish, *experimentally*, the independence of this centre, to which we propose to give the name *phono-motor.*

It is necessary, nevertheless, to remark that the locomotor centres themselves do not invariably require a peripheral excitation, or the action of spinal sensory nerves, to cause them to enter into activity.

Also, it would be difficult to specify what sensory nerves act in the case, when an animal without a brain seeks to maintain its equilibrium; or by what peripheral action a pigeon, in like condition, invariably exerts itself to fly when thrown into the air. There is to be seen, also, in many cases where the reflex action is incontestable, a mechanism which is located in the central regions of the nervous centres, and where the existing impression comes from a kind of memory or reminiscence of previous ones. The word memory, in this sense, evidently signifies an impregnation of anterior impressions; and the phenomenon is in itself the same for the sensory cellules as that which takes place in the cells of the brain.

The general movements, and particularly those of education, have, as an element of co-ordination, that unconscious memory which is nothing else than an oft-repeated impression, which reacts on the motor cells, regulating and proportioning the muscular activity. Having been excited an incalculable number of times, in a certain manner, the same cell reacts again in a similar fashion to a like impression, even when the will and the intelligence are abolished. It is by this mechanism that we can explain the more complicated reflex acts, and those which take place without peripheral excitation.

Among the superior animals the reflex movements of instinct are much less numerous than among the lower ones; but those of habit or education, on the other hand, exist in great number: and it is a mistake that they have not been for so long a time attributed to the same mechanism as the others; for, if the automatism does not predominate in all, we may be assured that it exists as a fundamental and necessary fact.

As regards speech, in which we believe we have discovered all the characters of the reflexes of education, the combined actions only become developed little by little, and by a long and difficult education; but it is absolutely the same method of procedure as for other more simple complex movements: such as those of locomotion. The first acts are simple, limited, and still inco-ordinated; and gradually, in proportion as they are often repeated, they become more regular and extended, and it is only after each simple movement has become perfect that the more complex ones begin, at first indecisive, then co-ordinate and regular.

Plutarch has made the remark that the first articulate sound pronounced by man is that of the letter *a*, because the *souffle* which leaves the mouth by the mere opening of the lips is the first simple sound, and needs no other instrument, not even requiring the use of the tongue for its production. After this, follow words a little more complex, and requiring the use of many different muscles. President De Brosse, in his *Traite de la Formation Mecanique des Langues*, 1765, is one of the first who has correctly analyzed the physiological development of language. "The germs of speech," says he, "or the inflexions of the human voice, which include all the words of a language, are physical and necessary effects, resulting absolutely, such as they are, from the construction of the vocal organs. Human speech, and the forms of the names of objects, are not, as much as they seem, the arbitrary product of the human will. In the first formation of language and of radical terms, the form is the necessary effect of the sensations derived from external objects; and the will exercises scarcely any influence."

It is a noteworthy fact that, in all languages, the syllables *ab, pa, am, ma,* are the first pronounced by children: that is to say, they commence by using their lips, all these syllables being labials. We think that the explanation of this fact is to be found in the great facility that they have of controlling the movements of their lips, since they contain the voluntary muscles which are the first and most frequently put in action by the act of sucking.

We see, therefore, that the phenomena of the exterior movements of speech are ruled by the same laws as those of locomotion, of swimming, or of those which need the play of any instrument whatever.

The education is accomplished in proceeding from the simple to the complex at the time when, by this series of efforts, it forms, through habit, a kind of memory which regulates and co-ordinates the muscular action, and causes it to take instantly the energy and direction desired.

In the adult, the phenomena which attend the function of speech are so complicated that it is difficult to recognize the automatic actions; so much the more as speech, serving to manifest our ideas, appears constantly to be the effect of a voluntary and intellectual act. But in certain pathological conditions, and even in some normal ones, we find plain proofs of a purely reflex action, at least a kind of automatic mechanism, through which the sensations perceived have a more direct influence, and often a more powerful action, than the will.

Let us first examine some of the instances which occur normally, and especially the influence that can be exerted on the memory of words, and consequently on the function of speech, by the sense of hearing, that one, of all the special senses, which most acts to create that function.

When we observe what takes place during an oral recitation, especially by children, we can very well see, in nearly all cases, that it is not the expression of the idea which causes the words, but rather the habitude, from having repeated many times the same sounds in the same order. One word causes another, because the aim has been to retain one after another by the succession of sounds. It is for this reason that we are able to retain a more exact verbal memory of verse than of prose: because we have in this case, besides the order of the words, the rhythm of the poetry. So with most persons when they have stopped short in the middle of a paragraph, or especially of a verse of poetry, it is often necessary, in order to recall the part forgotten, to go back to the beginning of the verse, or even to repeat the preceding series, and to profit by the cadence and the rhyme.

Often, again, when the word which the mind endeavors to recall is not distinctly understood, the vague, unintelligible sound will recall it to the memory. This phenomenon takes place nearly always when the prompter is a little distance off, and is understood, so to speak, only by the person reciting, when others near at hand only hear a confused sound. There is certainly in this case an unconscious action, in which the intellect has no part, and in which the sense of hearing acts in an automatic manner.

Laplace, in his *Essai Philosophique sur les Probabilities*, cites the following fact: "One day, as a merchant of Paris was walking the streets of St. Germain, reflecting on some important matters, he found himself unable to refrain from humming, in a very low tone, as he walked the street, the air of an old song which he had forgotten for many years. Some two hundred steps farther on, he began to hear an old blind man playing, in the public place, the same air on his violin, and he imagined that it was a slight perception, a *semi*-perception, of the sound of the instrument, enfeebled by the distance, that had come to his organs with the strain, in a manner insensible to himself.

"He declared that, since that time, he had often given himself the pleasure to suggest such airs as he pleased to a roomful of working girls, without their being able to hear him. Whenever he ceased to hear them sing for a moment, he began to hum, in a very low tone, the tune he wished them to sing; and it scarcely ever failed to reach them without their sensibly hearing him, or any suspecting the cause of its suggestion to them."

So, from the intonation of the first part of a word or paragraph, we often recall the whole. In this respect children are very interesting subjects for study, for as the exact word is recalled by this procedure, they pronounce it very rapidly, as if it might again escape them; as in certain aphasic persons a word is uttered with energy, and, an instant later, they are often unable to pronounce it.

In the case of an aphasic patient whom we have had occasion to observe, we have often tried this experiment of intonation, in order to make him name an object. Nearly always, by giving him the first part he would be able to pronounce the entire word. One day I showed him a statuette, and asked him the name of that object. As he was unable to recall the right word, I thought I could make him do so by pronouncing the syllable *sta*, the first part of the word statuette. I repeated *sta* three or four times; but he could not recall the word; and it was only when his wife, who accompanied and took care of him, said to him, "It is an es - - -," that he found the name, and said, "It is an estatue." I had forgotten that, in common language, it is usual to say, *une estatue*, and not *une statue;* and I only cite this instance because it indicates so well how the first intonation recalls the forgotten word.

One of the facts which prove the automatism which exists in the function of language, is the difficulty we often meet with in refraining from habitual expressions. So, in joy, in anger, or in sudden pain, while the intelligence fails to control our speech, and the volition is distracted, there escape, in spite of us, oaths and ill-sounding words: at least, this happens, almost constantly, among those who are in the habit of employing such expressions when under no social restraint. We can almost compare the *tics* of speech to muscular *tics*, in the case of those who are in the habit of swearing, or of using certain expressions.

On the other hand, how as regards common expressions, from which we seek to refrain when they express the contrary of our thought? How many persons are there who have found themselves automatically responding that they are well, to the usual question, "How do you do?" when, on the contrary, they are not well, and are obliged to take back and rectify their response.

These phenomena are often very noticeable among the subjects of aphasia; and M. Moreau (of Tours) has observed that some of them are still able to reply to a question, and yet are immediately after incapable of repeating the same words. You give them a word and they cannot repeat it; but you ask them a question, the answer to which requires that word, and they reply readily. For example, they are asked, "How goes it?" and they reply, "It goes very well." Re peat the words, "It goes very well." They endeavor in vain to pronounce that phrase; they become impatient and irritated at their failure, but without result.

M. Broca has also remarked that certain subjects of aphasia who are unable to pronounce substantives, do so readily when they are intercalated with other words, or in the middle of a rapid speech, or an oath.

All these facts seem to me to demonstrate very evidently that there is in the function of language, in certain normal conditions, a true automatism, which is produced by education, and which in its processes resembles the automatic functions of other organs.

If, from the normal condition we pass to those which arise from pathological alterations of the organs of phonation, we observe much more clearly the resemblance which we seek to establish between the locomotor centres and a phono-motor centre.

Those who have in part lost the faculty of speech, afford us some remarkable examples in regard to this point, as has been already stated by M. Luys; that it seems, in certain patients, the way being all marked out across their brains, the word reaches it, and leaves automatically, under the least excitation. Allow us to give, in this place, one of our own observations bearing on this point:

Victor Prince, an employe of the Suez Canal Company, aged fifty-eight years, following a short loss of consciousness, was temporarily paralyzed in the right arm, and lost the use of words. The arm recovered its movements the same day, while speech was completely lost for eight days. The first word he was able to say, was *oui*, followed by oaths. Gradually he began to pronounce some words, without being able to carry on a connected conversation; and it was very seldom that he was able to find the exact word he wanted.

Intelligence was perfectly preserved, and he very evidently retained the memory of places and facts. He sang readily a couplet of the *Chant du Depart*, and the song of the *Bœufs*, of Pierre Dupont; but when I wished to make him repeat the verse slowly, he failed, and could not recover the words.

If I interrupted him after repeating two or three lines, he could not continue, but was obliged to recommence the couplet. He could not pronounce separately the words he sang. As regards figures, he very readily repeated, one, two, three, four, etc., in their natural succession; but if asked to pronounce them in pairs, he found it impossible. After two, or after four, he always gave three or five, although conscious of his error. The same happened in reading the numbers: he read them very well in their regular order, as, for instance, four, five, six; but could not do so when they were otherwise arranged, as four, one, nine. He could make additions mentally, but when he came to write down the figures he did it wrong.

Sometimes, after he had been compelled to exert himself for a long time to recall the exact word or figure which he found it difficult to enunciate, he afterward repeated it very often, and in the place of other words or figures. We might say that the effort had left, for a time, its impression.

He could only read a few words aloud; but he read mentally with facility. When he read aloud, he often substituted for the word before him another commencing with the same syllable. For instance: instead of the word *medical*, he would read *meridional;* in the place of *physiologie*, he read *physique*. He could not write that which he was able to speak—not even the couplets of verse which he recited—but he could copy them. The only words he could write without having the copy before him were *Paris*, and his own name. When he tried to write a couplet of the *Chant du Depart*, out of all the fifteen times that I made him attempt it, in only four did he make any exact approach to the first words; but generally, afterwards, the words were badly written, and finally completely indecipherable. In the different specimens of the handwriting of aphasic patients, collected by Dr. Proust

(*Archives Generales de Medicine*, 1872), we also observe that only the first words and the monosyllables are correctly written.

We give here an example of the writing of our patient:

> " *La vitore en tanfant nous hour la bariliere*
> *La vicerte la limite nige non* pas
> *Et du nours four nasei la frandite* fergiere
> *La nonterie gourere la saurne lur fictus du* coupat
> *Tranto nendimi de la* Fances "

We may notice in these first lines some traces of the stanza:

> " *La victoire en chantant nous ouvre la carriere,*
> *La Liberte guide nos pas*
> *Et du nord au midi la trompette guerriere,*
> *A sonne l' heure du combat.*
> *Tremblez, ennemis de la France,*" *etc.*

That which strikes us in these lines, is that the intonation of the rhymes still exists in the disfigured words; and this only confirms that which we have said already in regard to the memory of poetry. We see, also, in many of the words, a similarity of sound which approaches the exact word.

We would remark, still, that when we insisted that he should correct a word wrongly written, he much more readily wrote it right by commencing to write the whole word in one trace, than by trying to correct it as written.

Do not we often find, among healthy persons, that if they are unable to recollect the exact orthography of a word, they can write it mechanically, and, when done, it is rather the result of mechanism than of an orthography controlled by reason

We know that, among aphasics, one word often necessitates another, when this last is ordinarily used with the other. The same holds good in their writing; at least in the case we report.

One stroke necessitates another, so to speak; for when they are made to write the same letters many times, they have the tendency to always put them one after another. For instance: when I made the aphasic patient whom I have described to write the word *chose* a number of times, and then told him to write *coquin*, he wrote *cho . . .;* and in this case *ch* only formed, so to speak, one letter. An instant afterwards, I made him write the word *concorde* a number of times, and then told him to write *coquin:* he wrote, correctly, *co.* I then made him write *chose:* on the first trial he made it *cohse;* and on the second, after I had called his attention to his mistake, he wrote it *chonse.* We observe, in this last orthography, the *n* of the first syllable of *concorde.*

We see, in these instances, how an act often repeated previously can influence the writing; and how, in all these phenomena, there exists a reflex action as manifest as in certain movements of the limbs.

In studying the majority of the observations on aphasia, we find related phenomena of the same kind as those we have cited above, which also confirm the position we have taken.

For instance: what more striking example of the automatic succession of sounds or syllables is to be found than that given by Trousseau, in the case of a patient who often repeated *consisi*, but could never pronounce the syllables *si*, *si*, and only with great difficulty, *con?* We notice, in this case, that the patient, in giving the first part, had great difficulty in limiting his speech, and was, in a manner, compelled to give the whole word; while he was absolutely unable to pronounce the last syllable, except as a reflex effect ot the first, the prior impression being a necessity. Another aphasic patient could say *bon-jour*, but could never utter the word *bon.*

We equally observe this automatic influence in an observation of Franck, who gives the example of a Polish lady, who, whenever she wished to say "*Yaka dobra*," (How good you are) said "*Yaka durna*," which signifies, How *bad* you are.* In this case we discover, further, what is quite frequent among aphasics, a marked tendency to say or write, regularly, the first word or first syllable, while the following ones are incoherent. †

The facts we have given suffice, we believe, to demonstrate that there exists in the function of language, reflex action, as well defined as in any of the other functions.

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* See, for further details, the excellent paper of Dr. Proust (*Archives Generales de Medicine*, 1872.)

† As we write these lines, we have under observation the case of an aphasic who can repeat, with sufficient ease, the words *Champs Elysees*, but

If we now apply to this function the laws which we know exist, in a certain manner, for the other nervous centres, we shall admit that there exists, for the function of speech, a co-ordinating centre, which presides over the complicated movements involved in the act. The will only gives the order, and this phono-motor centre carries it out according to its means and conditions. The movements are co-ordinated and complete if the organ is in a healthy condition, and inco-ordinate and incomplete if it is otherwise.

In a word, as we are obliged to distinguish for each movement of a member (1. The simple and limited reflex action; 2. The more complex reflex actions, producing a general movement; 3. The influence of the will), so, in the case of the motions involved in the utterance of words, we have to distinguish the isolated movements of each separate part, the movement of the whole, and the influence exercised by volition. One can, in this manner, understand how loss of speech may result from diverse lesions.

In pathological conditions of the ordinary movements we find different phenomena, as the lesion bears on one or the other of these systems. If the lesion is situated in the spinal canal, voluntary motion is abolished; but certain simple reflex acts are still produced. If it is in the locomotor centre, the movements are ataxic, choreic, incomplete, and are constantly and forcibly produced. Finally, if the lesion is situated in the brain, the general movements may be regular, but the will does not act any more to produce them. As the locomotor centres belong to the essentially passive organs, the movements can only be induced by some peripheral excitation, or, in rare cases, by the will, when the brain still has power to transmit a part of its action. In this respect, nothing appears to us to correspond better with the function of speech than the play and the movements of the fingers of a musician (a pianist, for example); and this comparison seems to us all the more true, as the fingers of the musician act to display his feelings

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cannot say *Elysees;* he also repeats the words, *Bois de Boulogne*, while he says *Boulogne* with great difficulty, and only when, instead of being asked to pronounce the last word of *Bois de Boulogne*, he is requested to give the name of the city of Boulogne.

and his musical memories, in the same manner as the vocal organs express our thoughts and recollections to those about us. In the case of the musician, also, the habitude of the fingers is only acquired slowly, by education; and the finger becomes merely a mechanism, with which the mind does not occupy itself. The playing appears the more natural, as the player leaves its direction to his feelings and habits.

Then, when a musical note escapes his memory, he recovers it by going over the preceding ones. In this case of the memory of the notes, as in that of the recollection of words, it is recalled by a kind of excitation of continuity and impulse.

The loss of the faculty of musical expression may depend on various causes; but they all are included under the three heads we have given above. It may be due to a lesion of the peripheral nerves, or of the spinal centre, producing a difficulty or a complete loss of power to execute the necessary movements. In the second place, there may be a disturbance in the locomotor centre, which may cause inco-ordination, or complete abolition of movement. In these two cases, the musical feeling still exists, but its external manifestation is abolished; the intelligence remains intact, but its instrument is defective. When, on the other hand, the brain only is injured, the instrument is perfect, but cannot be used.

The loss of speech also comes under these three categories, because it follows, 1. The alterations in the external organs; 2. To alterations of the phono-motor centre; and 3. To intellectual disturbances. The first and third cases are so evident, and so easy to understand, that we need not dwell on them here; but we will occupy ourselves a little more fully with the alterations of the phono-motor centre, which presents absolutely the same phenomena as those to be observed in the locomotor centres.* Therefore, as the pathological modifica-

* All the divisions which have been proposed for the different cases of loss of speech are included in the classification which we adopt; it has, moreover, the great advantage of at the same time allowing the alterations of this function to be brought into the divisions admitted for the alterations of the other functions. In fact, for the functions of language, as for all the others, there is functional trouble when the external organs are affected, when its motor centre is disturbed, or, finally, when the brain itself is altered.

tions of the different combined movements can be divided into two great classes, the ataxies and the paralyses, we can also include in these two divisions all the alterations of speech which do not depend on an intellectual disturbance or a lesion of the external organs. In fact, in all the observations of aphasics which have been cited, one or the other of these kinds of symptoms is presented.

For example, as the musician who is troubled with choreic movements, and who wishes to touch a certain key, strikes that by its side, so the aphasic person uses one word for another, and cannot ennunciate the exact one which expresses his idea. In the one case, as in the other, the alteration may be slight, and the first phrases are often perfect; but soon the inco-ordination appears, and augments as the patient makes

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The classification which we propose has yet the advantage of corresponding with the three successive acts which, together, form speech: 1. The idea is conceived; 2. It is reduced to the desired form to be transmitted to the exterior; 3. The vocal apparatus is put in motion, and the muscles of the larynx and the mouth produce our thought aloud.

M. Jaccoud has proposed the following classification:

1. Alalia by hebetude.
2. Alalia by verbal amnesia.
3. Alalia by interruption of voluntary transmission.
4. Alalia by default of co-ordination in the motor centre.
5. Alalia by paralysis of the tongue.

MM. Broca and Proust have classed the different losses of speech in the following mnaner: 1. Alogia; 2. Verbal amnesia; 3. Aphasia, or aphemia; 4. Mechanical alalia.

We would remark that, in this division, it is often quite difficult to separate, satisfactorily, verbal amnesia from aphasia; while, on the other hand, very different cases of aphasia are included in one class.

The classification which we would propose, as according with the anatomical and physiological facts, is as follows:

1. Cerebral alteration.
2. Alteration of the phono-motor centre, comprising two great classes: the one with ataxic, and the other with paralytic, symptoms.
3. Alteration of the external organs.

The first division comprehends alogia (Broca and Proust), and alalia by hebetude (Jaccoud).

The second includes verbal amnesia and aphasia (Broca and Proust), and the alalia by verbal amnesia—that by interruption of voluntary transmission, and that by default of co-ordination in the motor centre (Jaccoud).

Finally, the third class is the same with all the authors; and the name, *mechanical alalia*, is certainly well chosen, and ought to be kept.

the effort to avoid it. Often, also, as in the case of the choreic subject, who at the moment when he wishes to seize an object, lets it go, and it falls, the aphasic patient, likewise, after having found, for an instant, the word he wishes, loses it again, and cannot pronounce it, though he makes many vain efforts; or rather, still resembling the choreic patient, he utters brusquely a few words, and then stops short.

In all these cases there is to be found no lesion of a paralytic nature, but rather a defect of co-ordination. There is a disturbance of the phono-motor centre, which causes the ataxia of the vocal phenomena, analogous to that which a lesion, itself slight, of the locomotor centres causes in the movements.

In the second category we find the aphasias of the paralytic form. Some are, indeed, completely deprived of all power to emit articulate sounds; others can still pronounce some syllables, or some words, but always the same ones. Observations of this kind are numerous, and M. Broca, especially, has recorded many cases. One man could only say *tan*, *tan;* another, only yes and no; a patient of M. Charcot could only articulate the monosyllables *ta*, *ta;* others employed *bizarre* words without any signification. We have known the case of a very intelligent man who could only say *chaussette*, and who scandalized his confessor, to all of whose questions he responded by this single word.

M. Broca has compared these aphasics to infants who have not yet acquired, after infinite gropings, the very complex art of causing, by the combined action of a great number of muscles, the production and articulation of sounds; and in whom the faculty of speech, still in its budding state, only reveals itself by a series of monosyllables. We would prefer to make the comparison with certain cases of hemiplegia in which only very limited movements can be produced, always of the same kind.

Frequently, after a cerebral hæmorrhage, the only possible motion is the flexing of one or two fingers; and this takes place when the patient desires, on the contrary, to extend the hand. It is still this mere flexion that is produced whenever the patient desires to put his whole arm into motion, either by raising or by lowering it. In a word, the will has no further elective action; and to each effort, or to every intellectual excitation, there follows only this very limited and unvaried movement, no matter what the will had intended to produce.

So, in the case of the aphasic according to the extent of the lesion, he can only utter one, or a certain number, of words, invariably the same. He desires to express his ideas, but forcibly and inevitably, he always makes the same vocal movement; there is no inco-ordination, but a true paralysis, more or less complete, according to the case.

Nothing appears to us more logical than this assimilation of the vocal acts to the other complex muscular movements; and in admitting the existence of a phono-motor centre, as well as of locomotor centres, we believe that we need only review the theories so far proposed, adding some new physiological facts.

We would not say, therefore, that the will and the intelligence do not constantly intervene in the act of speech; but we have sought to show that there exists, in this function, besides the voluntary action, a kind of automatism which is the result of education; that this automatism possesses all the characters of those which exist in the other functions—characters which consist, principally, in a perfect condition, in a reflex and almost inevitable succession of phenomena which are associated from habit, and in a functional activity which derives from the will only a general excitation. In short, we believe that it is, in this respect, the same with language as with the movements of walking or dancing, or playing on an instrument.

The details of execution, their co-ordination, and their functional adaptation, are the result of the action of a special centre, which is only dependent on the will in a general manner, and which presides directly over all the phenomena—it might be called a superintendent or a register, who is charged with the overseeing that the orders are regularly carried out by all.

A great number of facts, well known to all, serve to confirm our view. Under many circumstances, in fact, words continue to be uttered while the mind is occupied with other ideas than those which are spoken at the time. Every one has spoken or answered mechanically when the thought was otherwise occupied. The actor, after a series of representations, scarcely occupies himself with that which he declaims; the child, who recites a fable for the hundredth time, plays and engages itself with other things. Outside of certain cases of cerebral affections, in which there is a true excitation of the phono-motor centre, are there not many who speak almost in a dormant state, as there are others who have continued to march while sleeping?

The orator speaks from intention, without doubt, but, by an abstraction which well indicates the division we have made, there are, so to speak, two persons combined in him—the one mechanical, which utters what he has before thought, and the other who at the same moment thinks what he is going to say next.

In the man who speaks in public, talent consists chiefly in his ability, either from natural aptitude or from education, to abstract himself from his language, and not to be disturbed in regard to the action of his phono-motor centre.

Following out the comparison given a little above, he has the advantage of a good superintendent, whom he may have found naturally to hand, or may have educated. He is, therefore, as if freed from all care of details; and while he speaks, he can employ himself, from the beginning, in giving his ideas, regulating and classifying them; while in the case of the man who is compelled to occupy himself both with the thoughts he wishes to express, and with the manner of their expression—who is forced, as they say, to seek for words—he is not master of his speech, and becomes embarrassed at the end of a very short time.

We might cite still other facts of this nature; but we believe that those we have produced will suffice to show how important a part the reflex acts play in the function of language. This automatism, created by habit and education, is to be found, moreover, in all the nervous centres; and we can, indeed, almost recognize them in the purely intellectual manifestations.

We find, in fact, in the intellectual acts, reflexes of instinct, of education, and of momentary excitation, of the same kind as the reflex actions of the exterior movements. We believe ourselves to be the masters of our thoughts and ideas; but they are often only the result of a kind of automatism of heredity, of education, or of momentary passion.

Habit exercises a special influence over our judgments, and they are often, like a reflex act, inevitable consequences of anterior impressions. One can predict, in persons the intellectual state of whom is known, as far as it is due to habit, the whole series of sentiments and volitions.

The processes of the functions of the nervous system are throughout the same; for even when they are of different orders and infinite variety, they are yet subject to the same laws; and among these, no one is of more importance than that of the reflex acts, which might be called the law of the physiological fatalities.

There might be added an important chapter on the automatism of the intellectual acts, which often controls us more than we imagine. Montaigne has said, "Custom is a violent and faithless school-mistress;" and Pascal has developed the idea well in a chapter in his *Pensees*, and we cannot afford better proofs than by quoting it. In this chapter, of which the title alone is to be remarked—"*It is difficult to demonstrate the existence of God by natural evidences: but it is most sure to faith*"—he expresses himself as addressing a sceptic. "You wish to come to the faith, and you do not know the road; you wish to be cured of your infidelity, and you demand a remedy. Learn it from those who have been such as you are, and who at present are free from doubts. They know the road you wish to follow; they have been cured of the disease of which you wish to be healed. Follow the way by which they began. *Imitate their external actions*, if you cannot yet enter into their interior dispositions.

"Leave off these vain amusements with which you are now wholly occupied. I would sooner leave them off, say you, if I had the faith. And I say to you that you would sooner have the faith if you would abandon these pleasures.

"It is not necessary to disown one's self; we are body as well as spirit; and from this it comes that the instrument by which the persuasion makes itself is not the only demonstration. How few things are demonstrated! The proofs only convince the mind; custom renders our proofs more strong. *It inclines the sense, which draws the spirit with it unconsciously.*

"It is necessary to acquire a more ready faith, which is that of habit; which, without violence, without art, without argument, makes us to believe the facts, and to devote all our powers to that belief, in such a manner as our mind *naturally takes.*"

We have only a word to add. If habit has such a powerful action on acts purely intellectual, it without a doubt has an incontestable influence on the phenomena in which intelligence less constantly intervenes, such as those of the vocal acts; and this is the point we have endeavored to prove in this memoir.

IV.—SOME REMARKS ON THE THEORY OF INHIBITORY OR REFLEX PARALYSIS.

By C. Hanfield Jones, M.B., Cantab., F.R.S.

From the Practitioner.

IN the *British Medical Journal*, 1874, I., p. 40, the following passage occurs in a highly interesting lecture on diabetes, by Dr. L. Brunton: "The second way in which the hepatic vessels may be dilated is by reflex paralysis, or inhibition, as it is generally termed, of their vaso-motor nerves. Every one knows that when a sensory nerve is irritated, the impression is transmitted to the vaso-motor centre, and arrests its usual action over the vessels of the part to which the sensory nerve is distributed. Thus, when a grain of sand falls into the eye, the irritation which it occasions to the sensory nerves of the conjunctiva is conveyed by them to the vaso-motor centre, and arrests the action of that part of it which regulates the contraction of the conjunctival vessels. In consequence of this, they become dilated and full of blood, and continue so while the irritation continues; but so soon as it is removed the vaso-motor centre again regains its wonted power, and the vessels return to their normal size. The same is the case with the liver, and its sensory nerve is the pneumogastric." Loven's experiment on the auricular nerve of the rabbit, Rutherford's demonstration of the inhibitory action of the vagi on the gastric blood-vessels, Bernard's of that of the gustatory nerve on the vessel of the submaxillary gland, and Cyon's of the action of the depressor nerve of the heart on the intestinal arteries, through the splanchnics, constitute, I suppose, the chief experimental evidence in favor of the view above stated, which now seems to be reckoned among the accepted doctrines of physiology. As Dr. Brunton takes his illustration of the mechanism of the process from pathology, it may be concluded that he considers the same view to be applicable to morbid as well as to normal actions.

Admitting this, it may, however, be well to point out in what respects pathological differ from physiological inhibitory actions: 1. The immediate motors of the former are not normal and appropriate stimuli, but injurious irritants, or depressants. 2. The paralyses are not confined to vaso-motor nerves, but affect also musculo-motor, common and special sensory, and even the hemispheres themselves. 3. The districts affected are often non-coterminous with, perhaps remote from, that occupied by the incident nerve and its ramifications. 4. The occurrence of pathological inhibition may depend not on an absolutely excessive or injurious irritation having been applied, but on the incident nerve, or the recipient centre, being previously in a morbid state, hyperæsthetic, or hyperexcitable. 5. The same irritation may affect (pathologically) different parts in different persons. 6. Physiological inhibition is essentially transitory; pathological continues as long as the irritation persists. 7. Physiological inhibition is requisite for the performance of some function; pathological conditionates disease, disorder of function.

Sufficient heed has not always been given to the difference which may exist between the effect of different kinds of excitants. Just as we know it is with mental influences, some of which exalt and intensify nerve-force, to a high degree, while others as powerfully depress it, so it is, probably, with physical. One which is appropriate in kind and degree may be a very beneficial stimulant; another which is too powerful or altogether pernicious will have the most opposite effect. Thus it certainly is with wine and electricity: both in suitable doses are recreative; both in excessive, are ruinous to healthy life. A carious tooth and a warm condiment are, no doubt, both excitants of the same sensory nerve; but how very different are the impressions made on the nerve centre in the two cases! All impressions on sensory nerves certainly do not paralyze the associated vaso-motor. Vasal nerve centres appear to be more readily paralyzed than any other; which may depend partly on their being smaller, and therefore more easily deranged than larger. It may also be the case that the resulting hyperæmia is more easily appreciated than diminutions in the force of muscular masses, or in the tactile faculty of the integument. Some morbid phenomena of inhibition produced by disease are almost as precise and significant as the results of experimentation. It may be well to enumerate a few for the sake of those to whom the idea is not familiar:

1. Crimson flushing of head, face, and neck, from gastric irritation, itself perhaps depending on gastric hyperæsthesia, unilateral flushing and heat, sweating of face from loaded bowels, face hyperæmic, and covered with sweat when prepuce was irritated in a case of preputial neuroma. (*Vide* Verneuil, *Year-Book Syd. Soc.*, 1862, p. 239.)

2. Salivary flux from uterine irritation in pregnancy, or from neuralgia of fifth, or from irritation of filaments of the vagi distributed to the œsophagus. (*Vide* Dr. Fussell's case in *Lancet*, 1873, II., p. 625.)

3. Paralysis of heart by gastric, intestinal, cutaneous, or urethral irritation, as in gastralgia, peritonitis, burns, and operations on the urethra.

4. Paralysis of vasal nerve centres from stricture-splitting, with coma; temperature 107°·6. (*Vide Medical Times and Gazette*, 1873, II., p. 121.)

5. Sensory paralysis, as in Roche's case (*vide* Brown-Se-

quard's Phys. of Central Nervous System, p. 131), and Sir Thomas Watson's case (Lectures, last edition, Vol. I., p. 538.)

6. Motor paralysis, *vide* instances of reflex paraplegia given by Brown-Sequard; a case, cited from Mr. Morgan, of stone in bladder *vide F. N. D.*, p. 140; a case of my own, at p. 115, of left hemiplegia cured by an emetic; case of same ceasing after delivery, *vide* p. 119; cases of palsy from exposure to cold; cases of paralysis of muscles of eye, in neuralgia of fifth.

7. Cases of amaurosis from gastric or dental irritation.

Those who hold with me that pain is a mode of sensory paralysis, will see in the common instances of reflex, or remote pain, a phenomenon closely analogous to, if not identical with, inhibitory paralysis.

The first observer who entertained the idea of reflex paralysis of blood-vessels was, I believe, Henle, who proposed a theory to this effect: That the nerves of the vessels are in antagonism with the nerves of animal life, especially with the centripetal, so that in proportion as the latter are excited, excitement ceases in the former.* This view was adopted by Mr. Simon in his admirable lectures on pathology, where he says (p. 80) that "reflex relaxation" (as he calls it) appears to him "the only plausible explanation of the condition of the larger blood-vessels in active hyperæmia, whether inflammatory or hypertrophic." Subsequently, the views developed by Weber, Pflueger, Rosenthal, and Nasse, as to the existence of certain systems of inhibitory nerves, contributed to familiarize the minds of inquirers with the idea that one nerve might diminish the action of another. Lister's researches confirmed and corrected these views, showing that the same afferent nerve might enhance or inhibit, exalt or depress, the functions of the nervous centre on which it acts, according as the stimulus applied to it was mild or potent. His paper was published in 1858. Bernard's essay on the influence of paralyzing reflex agencies is dated September, 1864; Loven's paper appeared in 1867; Rutherford's in 1870. My views were first published in February, 1859, in the *British Medical*

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* *Traite d'Anat.*, Vol. II., p. 58.

Journal, and subsequently developed more completely in my Lumleian Lectures, *Medical Times and Gazette*, 1865, and in *F. N. D.*, 1864 and 1870. They have been well appreciated by Anstie, but are scarcely mentioned by anyone else. Brown-Sequard, in his lectures published in the *Lancet*, 1860 and 1861, laid great stress on the production of paralysis, either in the cord or in the brain, by remote irritation; ascribing it, however, rather to anæmiating spasm of the vessels of the paralyzed centre than to a direct action of the afferent nerve on the nerve cells. Though I differ from him on this point, I think he has contributed very highly, perhaps more than anyone else, to establish the doctrine of reflex or inhibitory paralysis. His extension of this view to those cases where head symptoms—paralysis, etc.—cannot be explained by any discoverable destruction of the organ of the will, or of the conductors between it and the muscles, seems to me quite correct, and a step of no mean importance. Henle's original view, important and suggestive though it be, and supported by Loven's experiment, does not seem to be substantiated. All excitements of afferent nerves do not dilate blood-vessels. Cold operating reflexly certainly does not. Nor does heat invariably, for Trousseau found hot water a more efficient styptic in epistaxis. Neuralgic perturbation does not induce hyperæmia in the majority of cases. Local irritants generally confine the resultant hyperæmia to the area on which they act, and produce no general flushing of the adjacent surface. Their action seems to be rather on the tissue than on the vessels. When the skin on being scratched with the finger-nail presents the *tache meningitique*, it is difficult to think that the lines of redness can be produced by any paresis of vaso-motor nerves. If such occurred in a reflex manner, it surely could not be so limited in extent, but would appear as a more or less wide-spread flush, as in instances cited in my Lumleian lectures. In five instances where I faradized the peroneal nerve with moist rheophores, just below the head of the fibula, for five minutes, no redness of the skin of the parts below was produced, and only in two or three some filling of one or two superficial veins. Galvanization is known to be much more effective than faradization in causing augmented blood-flow in the district traversed by the current.

On the whole, it appears, I think, that my original view is correct; that it is for the most part *morbid* excitation—*irritation* as opposed to *stimulation*—which produces reflex or inhibitory paralysis in any part. Some few instances there are of physiological inhibition, but these seem only to render it more probable that a similar effect can be produced pathologically. Loven's experiment, on which much stress is laid, seems to me rather an instance of pathological than of physiological inhibition. If we think what a difference there must be between our rude experimental excitation of a nerve and the normal, we must admit that the conclusion arrived at by Mr. Lister, respecting the different effects of gentle and strong stimulation, is highly rational and probable.

Art. V.—NOTES OF SOME RECENT CASES OF DEAFNESS, FOLLOWING CEREBRO-SPINAL MENINGITIS.

By Samuel J. Jones, A.M., M.D., Professor of Ophthalmology and Otology in Chicago Medical College.

THAT many cases of deafness, following cerebro-spinal meningitis, give evidence that the lesion is in the perceptive apparatus of the ear, instead of in the conducting, is well known. It is, however, often difficult to ascertain, in these cases, the extent of the lesion and its exact location; to determine if it be in the brain, at the origin of the auditory nerve, in the length of the nerve, or in its termination.

The ordinary tuning-fork of musicians will generally afford a means of deciding whether the conducting or perceptive apparatus alone be affected; but where both are involved, diagnosis is more difficult. Since nervous deafness is a far more serious calamity than impaired hearing, dependent upon some defect of the conducting apparatus, it is important to distinguish between the two cases, as prognosis and treatment will be materially influenced thereby.

Notwithstanding the recognized frequency with which cerebro-spinal meningitis is followed by deafness, the literature of otology shows how little the pathology of these cases is understood. If physicians would but note and report the manifestations of ear trouble in the progress of cases of this disease, and examine the labyrinth in fatal cases of it, much valuable information would doubtless be obtained. The aural surgeon frequently sees the cases only after all the change has occurred, and he is consulted only when hearing is lost and an effort is being made for its recovery.

With a desire to contribute some facts regarding a few of these peculiar cases, the following notes are given:

Case I.—Miss S., age seventeen, at present in good general health, suffered several months since from cerebro-spinal meningitis, which has left her deaf. Her friends report that she does, at times, hear certain particular sounds, but does not hear conversation.

In examination, the tuning-fork was either heard, or the vibrations were felt, but it was difficult to decide which was the result. No marked evidence of hearing was manifested. Subsequently, whilst I was engaged in conversation with her friends, and she was looking at pictures on the walls, I struck the table in my consulting room very lightly, when there was no chance that she saw me do so, nor could she possibly have felt any vibration from so slight a movement, and yet she turned immediately and smilingly intimated that she heard the sound made.

No further attention was given it; and as the conversation progressed her attention was again attracted by something. The knock on the table was repeated, when she turned again and in a similar manner indicated that she heard the sound. Her friends stated that, under some similar circumstances only, she gave evidence of hearing an occasional sound at home.

The only explanation of these phenomena at all satisfactory to myself, would be in the acceptance of the theory of Professor

Helmholtz, that in the distribution of the auditory nerve different filaments are attuned to different notes, and are excited to action by the vibrations produced by waves of sound of certain pitch and volume, resulting in hearing; and that the death or obstruction of these different filaments destroys or impairs the power of hearing those notes.

These phenomena would indicate that only a comparatively few filaments might retain vitality, or mobility, sufficient for hearing their corresponding notes, whilst the vitality of all others is destroyed; just as a few keys of a piano may continue to sound when struck, whilst all the other keys of the same instrument are so obstructed as to produce no sound when similarly struck.

This patient came merely for consultation; and as the prognosis was so unfavorable she returned home, and nothing has since been learned of the further history of the case, though inquiry has been made for it.

Case II.—F. F. B., age nine; a bright, active boy; had cerebro-spinal meningitis one year ago. Within an hour or two after the disease began to show itself he heard certain subjective sounds, which caused him to inquire if some one was not "popping corn" in an adjoining room; and at the same time he complained of great and increasing pain in his ears; and he has never heard since then. He *occasionally* recognizes the tick of a watch placed between his teeth; and the vibrations of a tuning-fork placed against his teeth, on the forehead, or over the mastoid processes, are either heard or felt. He recognizes scratching on a small rubber tube, one end of which is placed in the external meatus. In some of these trials the vibrations have been so slight as to seem to make it doubtful, if not impossible, that he felt them, instead of hearing the sound. There is in his case, as in some of the similar ones in which I have been consulted, a seemingly unnatural mental activity, as if some cerebral irritation existed, and produced excessive nervous action. This patient has just come under my care, and no result can be given.

Case III.—M. E. C., age seven years; unusually fine physical development; is said to have had cerebro-spinal meningitis five years ago, which greatly impaired his hearing. One year later he had measles, and since then has been wholly deaf. Each membrane of the drum is slightly opaque, and the Eustachian tubes are diminished in calibre. In examination it could not be determined whether he heard the tuning-fork, or felt its vibrations. There is the same irritable condition of the nervous system as in the preceding case, with unusual keenness of perception. The prognosis was also unfavorable, and the patient was not seen again after the consultation.

Case IV.—A. H. R., age three; had cerebro-spinal meningitis three months since. He is now apparently quite healthy again, fat, and very large, for his age. The right membranum tympani is slightly inflamed, and the left one nearly normal. His power of speech has been impaired, but is not entirely lost. There is an unsteadiness of gait in walking, which is suggestive of the existence of Meniere's disease of the labyrinth as its cause. An alterative and tonic course of treatment was advised, and the little patient was taken home after consultation, and no subsequent history has been received. In this case there is less indication than usual of cerebral irritation.

Case V.—P. C., age three; three months since he was supposed to have had typhoid fever. Careful inquiry as to the symptoms of his disease at that time renders it probable that he suffered from cerebro-spinal meningitis. Since then he has heard scarcely anything, and has almost entirely lost the power of speech. There is no marked evidence of cerebral irritation, but there is great unsteadiness of gait in walking, the only noticeable indication of physical impairment. Under an alterative and tonic course of treatment, he has so far improved that he has learned the names of all the members of the family with whom his parents are staying, and whom he never saw before; and he calls them by name, showing a returning power of speech.

Case VI.—G. E. H., age two years and seven months; does not speak, and gives no evidence that he hears. Parents state that, within the first two months after his birth, there was some disease affecting him which caused him to throw his head back a great deal, and at the same time made him keep his spine curved backward, which symptoms manifested themselves for several months. At present there is no evidence of any impairment of his physical health. There is, however, more indication of some cerebral irritation, if it may be correctly termed such, than in any of the preceding cases. It is certainly quite the reverse of the mental obtuseness seen in many cases of deafness. Prognosis was unfavorable, and the case was not seen again after the consultation.

ART. VI.—A CASE OF CHOREA.—A NEW METHOD OF TREATMENT SUGGESTED

By RANSOM DEXTER, A.M., M.D., CHICAGO.

IN the early part of January, of the past winter, my attention was called to the case of Miss D., aged thirteen, who had been suffering from impaired health for the previous six or eight weeks. This was attributed to hard study at school, and an effort was made to avert any further serious consequences from that cause, but too late, as the result proved; in less than forty-eight hours after her withdrawal from school, she presented decided symptoms of chorea.

On the 14th of January the disease had fairly made its invasion; and the first prescriptions I thought best suited for the existing conditions were as follows: Extract of valerian and cimicifuga, twice a day; and the elixir of pepsin, bismuth, and strychnia, before each meal; and bromide of potassium and cannabis indica at bed-time.

In a few days I noticed some malarial periodicity, but no cardiac or rheumatic troubles, though my little patient was growing worse rapidly. I now prescribed Fowler's solution of arsenic; but the symptoms appeared obstinate. I then solicited the advice of one of our most competent physicians, who approved of the adjustment of the agents to the peculiarities of the case; but the course of treatment to be instituted for the periodicity was a complicated question. After canvassing the *pros* and *cons*, we decided to try, cautiously of course, small doses of quinine and iron.

I accordingly did as we thought best; but as soon as the slightest effect of quinine was observable, the following symptoms ensued: Hemiplegia of the left side; dilatation of the pupils of both eyes; some choreic movements during sleep, increasing so much when awake that the patient had to be held down on the bed or lounge; she could not sit up, but, in attempting to do so, would be jerked down, instantly and violently, by the muscular contraction.

But four grains of the quinine had been given when the peculiar symptoms of this complaint became general, and I despaired of saving my patient, apprehending a general failure and wearing out of the vital powers.

Only ten days had now elapsed, and during that time I had reviewed the writings of several of the most able modern writers on the subject, and was unable to see wherein I could improve upon my first system of treatment. This stage of the disease, with its outlook, was unpromising.

Not being satisfied with either the pathology or the therapeutics of the disease in question, I began to review the facts and phenomena, and the following queries suggested themselves to my mind:

1. Why are choreic patients quiet during sleep?
2. Why do noise and excitement aggravate the condition?
3. Why are the symptoms aggravated by the entrance of neighbors or strangers?
4. Why was the condition so unfavorably affected when the piano was played?
5. Why did so small an amount of quinine have such an injurious effect?
6. Finally, what are the unquestionable physiological interpretations of the phenomena under all these conditions and circumstances?

To me, the whole problem seemed solved in an instant, as follows:

1. That all the sensorial ganglia, or the centres for the nerves of the separate senses, were more or less affected;

and that the sensori-motor centres were the special seat of the disease.

2. That the pathological condition could not be other than an asthenic irritation of the sensori-motor organs.

3. That these organs, and especially the sensori-motor guiding ones, must have physiological and therapeutical rest, with the additional treatment by such medicinal agents as will also contribute to that end.

4. If the *physiological* and *therapeutical* rest be the *sine qua non* of treatment, then what is to be done?

At first, I essayed to carry out these ideas by blindfolding my patient, filling the ears with cotton, excluding all company, and keeping her in one room, where everything was familiar. The blindfolding, and cotton in her ears, she could not tolerate, from her sensitive condition. I then ordered the blinds closed, kept her in a middle room of the house, and enjoined the most strict quietude in every particular, even to lying as perfectly still as possible upon a lounge, that the nerves of touch might not be wrought upon; also in every other particular pertaining to the five senses, and muscular motions.

5. Continued the extract of valerian, cannabis indica, and bromide of potassium, with an occasional cathartic to act as a revulsive. Her attendant was her mother, most of the time, but relieved by the patient's father. But little was said in the room; the patient could not speak; and the parents spoke as little as possible, the room being kept in a twilight condition.

In eight hours after this treatment was instituted, we were impressed that a noticeable improvement had ensued; but, within twenty-four hours thereafter, the improvement was a decided one. From that time the improvement continued rapidly; and in six days all choreic movements had subsided.

I do not believe the patient's general health suffered from want of light, but, on the contrary, was much better off without it, as long as she suffered from chorea.

It would be difficult to make me believe that this rapid recovery was a mere coincidence; therefore, I feel at liberty to express my views, and ask the profession to try them, or be governed by the principle that the *sensorial centres in chorea need physiological and therapeutical rest.*

Art. VII.—NERVOUS SORE THROAT

THE following article is introduced into the Journal less on account of its intrinsic merit than as a means of calling attention to a form of disease not usually considered as neurotic in character.

It is an abstract of an article contained in the *Deutsch Klinik*, No. 6, 1874. It had been our intention to have prepared an article on a kindred topic, in which various cases we have observed would have been detailed; but we will content ourselves by referring to one of them briefly.

Mr. D., of Howarden, Wales, had, ten years before I saw him, what was called, and from the history of the case may have been, severe inflammation of the spinal cord and its membranes, in the upper and middle dorsal regions. He was confined to his bed and room for over nine months, and made a slow and painful recovery; but since then he has had, at times, more or less pain in the back in the region described, and pains and neuralgic shoots along the nerves which terminate in that part of the cord, and has been greatly inclined to headache, and to changes in the vascularity of the head. He can hardly remember when it began, but there has been, for several years, a tendency to painful disorder in the throat.

The mucous membrane lining it is reddened, but not apparently thickened or abraded. There is a little induration of submucous structures, but nothing marked in this respect. Tonsils slightly enlarged. But there is often, for weeks at a time, a scalded or burnt feeling, with a sense of stiffness and disagreeable soreness in the whole throat. He also feels, at such times, as if there is something clinging to the mucous membrane, which he tries to remove, but cannot. There is a throbbing sensation in the throat, that seems at least, often, to be synchronous with the cardiac impulse. The sense of soreness is aggravated by movements of the throat, as in swallowing or much speaking. There is dryness rather than increased secretion from the mucous membrane of the affected part.

Externally, there is often flushing of the face and neck, especially on the left side, on which the throat disorder is most manifest; and also an unnatural elevation of temperature at times. The eyes are prominent, more or less injected, somewhat dry, and a little sensitive to light; pupils slightly contracted; heart a little irritable in its action, with occasional palpitation; tongue clean, with a distinct but very thin white, velvety fur; digestive system in fair order; general health moderately good; bowels slightly constipated; sleeps moderately well.

But it is not our intention to describe fully this case. The point of immediate interest was the throat disorder, which resisted, at first, all plans of treatment adopted, whether local or general. Upon careful consideration, we had but little doubt the disorder in the throat was neurotic in origin, and that the several local symptoms, at least in part, could only be explained by a reference to the nervous centres, from whence the nerves proceed that innervate the mucous membrane of the throat and parts beneath it.

It seemed highly probable, as the spinal vaso-motor centre for the head is situated in the upper dorsal region of the cord, and inasmuch as that was the seat of the former inflammation of the cord, that there still remained some traces of disease, which so acted on the vaso-motor nerves proceeding from the part of the cord in question as to give rise to the vascular disturbance so often seen about the head and throat in this patient. Whether the phenomena in this case did not point, moreover, to some more central disturbance higher up in the cerebro-spinal axis, even so high as the origin of the glosso-pharyngeal nerve, we will not now stop to inquire. But, acting on the view of the pathology of the case just expressed, we employed, occasionally, the continuous current (descending) of moderate strength, one electrode being placed on the back of the neck, or sometimes held in the hand, while the other was either placed over the dorsal region of the spine, or at the feet. Beside this, he was placed on a preparation of ergot and one of the bromides, with suitable medical and hygienic regulation as to his general health, and occasional local applications to the throat of nitrate of silver solution; and on this course the patient slowly improved.

Before he had recovered he removed to another part of the country, where his improvement continues. It will be seen that this case of sore throat differs from those referred to below, in regard to its condition or cause. Dr. Klemm attributes the sore throat in his cases to nervous disorder, produced by unhealthy mental or emotional states, in its turn, doubtless, arising from some cerebral disorder. In the case we have cited, the sore throat was presumed to be, at least in part, the outgrowth of vaso-motor disorder, arising out of long standing disease of the spinal cord; but both kinds of cases agree in having a supposed nervous origin. But we now introduce the promised abstract of the article of Dr. Klemm, in No. 6 of the *Deutsch Klinik.*— Ed.

" A very large number of sufferers from so-called sore throat, complain of a constant painful sensation, without showing any other than the most trifling morbid alterations in the region affected. Such patients, who cannot on other considerations be accounted as nervous cases, often occupy, more than any others, the time of the physician, and frequently have to be accounted as absolutely incurable.

" In such cases, even the closest examination affords no satisfactory explanation why the abnormal sensation should appear in the perfectly normal mucous membrane. The search for an anatomical cause has, in this matter, afforded us no satisfactory explanation; on the contrary, it frequently occurs that altogether unimportant and secondary alterations are mistaken for the *corpus delicti*, and, in consequence, a false system of treatment is decided upon, and the stronger caustic agents are, especially, misemployed. Quite often, very trifling alterations of the mucous membrane of the throat are taken to be the source of the numerous and often very different sensations experienced in the throat and larynx (in drinking, speaking, or in rest, etc.): such as the well known small, isolated, hypertrophic, warts or band-like excrescences, or even the almost constant thickening of the lining membrane with increased secretion of mucus in old persons, which, if it occurs together with any nervous complaint, is usually believed to be the cause: so that the whole is considered as a chronic throat catarrh. In very many cases these alterations are present without producing any uncomfortable symptoms of pricking, pressure, constriction, choking, etc., and they are very often wanting when these troubles are present in a very pronounced and rebellious form.

"In many such cases of sore throat we find, to be sure, some swelling of the mucous membrane, the submucous cellular tissue, and the glands, especially in the throat; and in these cases the pain is relieved by caustic applications. But the author excepts these cases of true chronic pharyngitis, and confines himself more exclusively to those in which the pharyngitis is either extremely slight or altogether lacking. In like manner, many cases of pulmonary disease, with normal mucous membrane, would be erroneously diagnosed as chronic pharyngitis, and treated with caustics, without profit, if we take into account only the pain in the throat and the feeling of pressure, etc.

"The results of treatment show, moreover, that the caustics have either a very slight effect, or none at all; and that only one thing proves beneficial, that is, change of air, and the milk-cure, with the employment of some mineral waters, which are here often of great service.

"The essential symptom of these various forms of hyperæsthesia of the pharynx is the difficulty experienced in swallowing or in speech. Swallowing is always accompanied with an abnormal sensation; the patient complains of pressure, pricking, or sensation of constriction, or the feeling of some foreign body, sometimes as if a hair was lodged in the throat. The painful sensation either is felt on both sides, or it may be confined to a single point, accurately designated by the sufferer. Sometimes it is constantly present; but it is generally periodically milder, often lacking entirely, and then again severe. In the evening it is always more severe than in the forenoon, and, in many cases, returns daily, in the afternoon. Emotions of all kinds have a bad influence, especially upon those who have lost relatives from consumption, or who are particularly fearful and nervous. Many of the patients complain of a dryness of the throat, without any such appearance to the mucous membrane, an especially troublesome and constant symptom; or they affirm in the most confident manner, that there must be a foreign body lodged there, causing them to attempt to swallow, or hawk, and cough, while really no collection of thick mucus, as in actual pharyngitis, is really present. Speech is affected in sympathy; it is not hoarse, but almost inaudible, and the patients complain that it soon fatigues them and causes pain. Finally, we have the *globus hystericus*, but this is met with much more rarely than the other symptoms. Sometimes again the painful sensation extends to the ear, and hearing is affected.

"The individuals who are especially liable to this nervous affection are by no means always of a nervous or hypochondriacal disposition; it attacks frequently the female sex, and not merely hysterical or irritable women, and those in the higher walks of life, but among others, strong and healthy women, and particularly those of the lower classes, who have nothing but their throats to complain of. This hyperæsthesia is rather common among men; and according to the author's observation, it affects the cultured more than the working class, and is not at all rare among those who are in the custom of public speaking or singing, or who have often suffered from catarrh. In both sexes, he found the fear of consumption, which had caused the death of a cousin, or a brother, etc., to be an indubitable cause of the affection; and frequently a recent loss of this kind throws the patient into great agitation and establishes the disease.

"Very often the psychic origin may be detected when there also exists an ordinary nasal or bronchial catarrh, without any participation of the membranes of the throat; and in this case, also, fear is the principal cause of the disease. A third cause is yet to be mentioned, the persistent excitability often remaining after an acute pharyngitis or laryngitis, similar to the lasting irritability of the tonsils, without hypertrophy, after an acute amygdalitis. This is the case not only after acute but also after subacute inflammations of these parts, which are readily re-incited, and which leave the throat for a considerable period in quite an irritable condition. Finally, we may enumerate among its exciting causes, external irritation from wind, dust, indulgence in stimulants (even coffee), which very easily produce hyperæsthesia in sensitive subjects, without any corresponding alteration in the mucous membrane. It is sometimes very difficult to decide, in cases where there are slight alterations, whether the actual very insignificant and habitual abnormal appearances are really the cause of the trouble or not; and only by prolonged observation can a correct opinion be given. Whether the affection is ever hereditary is doubtful; but the patients will sometimes so assert.

"The participation of the vocal organs is specially noticeable in this form of hyperæsthesia; the voice is either inaudible or harsh, although nothing abnormal can be detected in the larynx; the patients unintentionally aggravate the symptoms, either because fear and imagination co-operate with the disease, or because the activity of the motor fibres is diminished. The inconstancy of the phenomena, the rapid onset of the disease after emotional disturbances, and its quick departure, prove that its cause is not a catarrhal trouble, but a purely nervous affection. Another peculiarity is in the fact that such invalids feel free from their difficulty in the open air, while they suffer in-doors; and correspondingly, we find this purely nervous hyperæsthesia much more rarely among dwellers in the country than among towns-people, although they are often enough the subjects of chronic pharyngitis, and are made worse by raw air or draughts.

"The cure of this affection is one of the most difficult tasks of the physician; the patients often engage his attention for years without obtaining relief; and even if a cure seems to be obtained, they again readily relapse. Here, also, appears the difference between the nervous and the catarrhal form; the latter is altogether more yielding to local remedies, while in the other case they very often are of no use, or are merely of transient effect, and their employment seems to be very little indicated, as they only afford a momentary alleviation. Frequently they are even injurious, since by their use the hyperæsthesia is increased.

"The inexperienced physician is readily inclined to consider the disease altogether imaginary; but this is not the fact; it really exists, and is much more important than many others with visible alterations for a cause, and which are suitable for treatment with nitrate of silver in substance.

"The treatment is based on very slight foundations. If material alterations of the mucous membrane present themselves, it is always justifiable to apply local applications; and if it is desired to remove red fleshy excrescences, the caustics in substance are preferable to weak solutions. But if these are lacking, the action of weak solutions (0.3 to 15 or 30 water), is indicated; or we may pencil the parts with chloroform and glycerine, which is sometimes of service. If there is no chronic catarrh, we may try electricity, which sometimes causes a rapid improvement in rebellious cases; but it is needful that one electrode, armed with a sponge, be placed directly upon the mucous surface, while the other is applied at different points of the external surface. If nitrate of silver is used, after other treatment has failed, strong solutions should at all events be avoided, and the weak solution should be applied over the whole surface of the pharynx, and especially over that portion below and behind the tongue. Dr. Klemm has used, instead of glycerine, a solution of morphia with mucilage, and has found that this means has a better effect than the astringents.

"The most effectual treatment in this, as in other disorders of nervous activity, is, according to the author, a change of air; and mountain air is, by all means, the most beneficial; after it comes the sea air. Among mountain localities, those must be chosen which are moderately high and well protected; and those elevated situations which are recommended for lung complaints are unsuitable, as the raw, dry atmosphere only aggravates the evil. If it is not possible for the patient to visit the mountains or the sea-shore he should be sent into the country, and treated by the milk-cure and mineral waters. The atmospheric change is still the principal point, and the good results attainable by residence at watering and bathing places are doubtless due not to the high-priced mineral waters, but to the favorable situation and climate. The fact that in the various localities the most different agents are employed with equal results, shows plainly that the cure does not depend upon these, but on the effect of the atmosphere on the nerves and mucous membranes."

Reviews and Bibliographical Notices.

I.—INJURIES TO THE NERVES, AND THEIR CONSEQUENCES.

I. Des Lesions des Nerfs et des leurs Consequences. Par le Docteur S. Weir Mitchell. Traduit et annote avec l' autorisation de l' auteur, par M. Dastre, et precede d' un preface par M. le Professeur Vulpian. Paris: 1874; 408 pages. *(Injuries to Nerves, and their Consequences, etc.)*

II. Traite des Sections Nerveuses. Physiologie Pathologique. Indications. Procedes Operatoires. Par E. Letievant. Paris: 1873; 548 pages. *(Treatise on Nervous Sections, etc.)*

III. De la Regeneration des Organes et des Tissues en Physiologie. Par J. N. Demarquay, etc. Paris: 1874; 328 pages. *(The Regeneration of Organs and Tissues in Physiology.)*

IV. Ueber Nerven Degeneration und Nerven Regeneration. Von Dr. Herm. Eichhorst. Virchow's Archiv, LIX., I. *(On Nerve Degeneration and Nerve Regeneration.)*

The tide of medical literature for a long time—and for reasons not at all to the discredit of a new country, such as ours—has been from Europe to America. But now, as time and opportunity for original research are afforded, contributions begin to flow the other way. The book of Dr. Mitchell's, the title of which stands at the head of our article, is one of the best instances of this fact. It has been widely and favorably noticed, both in Great Britain and on the Continent, but especially the latter. We congratulate the author, on account not only of the compliment done to himself, but to American medical literature, by the present translation, introduced as it is by so eminent an authority in all that pertains to the nervous system as is Professor Vulpian.

We had not proceeded far with a review of the present work for the pages of the Journal, including the preface by Professor Vulpian, when it seemed best, instead of giving a review of our own, to translate bodily the latter, as being not only in the main a full and satisfactory estimate of the work, but also as containing the latest results of his own rare studies in regard to the subject in question. This we do, not to avoid the labor of a notice such as the work merits, but because we think we can perform no greater service for our readers, in this connection, than in giving place to the generous and critical paper of Professor Vulpian. Lengthy as it is, we earnestly commend it to all who feel an interest in the physiology and pathology of the nervous system. It is as follows:

"The nerves form a system of organs altogether peculiar, under all its relations, texture, structure, course and distribution, physiological properties and functions. The injuries which may occur to them must necessarily, therefore, give rise to special changes, and make themselves apparent by symptoms of a peculiar kind. These accidents and these symptoms are almost invariable, at least in their general features, when the mixed nerves are the ones attacked, but are somewhat modified when other nerves are affected; when the lesions, for example, are in the nerves of special sense, or in those of the sympathetic system.

"The idea of combining in one work all the scattered observations on nerve lesions, would naturally suggest itself at some time, to a pathologist, accomplished in physiological researches, and rich in an extended clinical experience of these injuries. No one was better fitted than Weir Mitchell, for such an undertaking. As he explains himself, there was organized in America, during the war of secession, a special military hospital for nervous affections, the direction of which was confided to MM. Weir Mitchell, G. Morehouse, and Keen. To this point were sent, from different parts of the theatre of war, the wounded who suffered from lesions of the nervous system. These physicians had, therefore, under their eyes, the most varied cases of traumatic alterations of the nerves; and they have published their principal observations in a well-known work: 'Gunshot Wounds and other Injuries to Nerves.' Philadelphia, 1864.

"Since then M. Weir Mitchell has not ceased to study this subject, and to collect new material. He has published, more recently, another work, on 'Nervous Maladies Resulting from Lesions of Nerves,' which is included in the medical reports of the National Sanitary Commission. Moreover, he has attempted to test, by numerous experiments on animals, certain results of observations which he has made on the human subject. He is also in possession of the greater part of the needful resources for the attainment of the object in view, that is, to compose a 'Treatise on the Injuries to the Nerves.'

"Yet, in this Treatise, while lesions of the nerves are viewed from a general stand-point, traumatic lesions have furnished the principal contributions. The spontaneous affections of the nerves, while not neglected, are relegated to a secondary position. But far be it from me to reproach the author on this account; I would rather commend him. He speaks especially of that which he has himself observed, and observed well.

"Moreover, among all nervous lesions, those of traumatic origin are the most simple and clear to be understood, and their interpretation is the least the subject of dispute. The idiopathic lesions in general are very different in this respect. If the neuromas up to a certain point have a general resemblance to the traumatic lesions, the case is different with the other affections, the neuralgias, for example. To what kind of nervous lesions are these due? Where are these lesions located? Does it act by a congestion, or an inflammatory irritation of the affected nerve? Is it the nerve trunk itself which is altered? Is it its peripheral or central extremities, or is it the nervous centre?

"While, according to various pathologists, the neuralgias have their usual seat at the periphery of the nerves, or in a part more or less distant from the nervous columns, I believe that, in many cases, the alteration which causes these affections is situated near the central extremities; very often, perhaps, in the spinal cord, or in its membranes. And even in those cases where the neuralgia has clearly for its primary cause a peripheral nervous lesion; in those, for instance, where a decayed tooth, an alteration, perhaps, of the alveolo-dental periosteum, or of the maxillary bones themselves, etc., has given rise to the painful affection, we ought, I think, to admit that, frequently, a little while after the beginning of the trouble, it produces in the nervous centre, or, speaking more strictly, in the focus of origin of the trigeminus nerve, a morbid modification, which exalts to a very high degree the excitability of the anatomical elements of the gray substance. This exaggeration of excitability may extend itself to the elements of the gray substance situated next to those which are directly in relation with the nervous fibres of which the peripheral extremities are affected. The excitation transmitted by these fibres to their origin, propagate themselves to neighboring *foci* of origin; but because of the morbid erethism of these *foci*, the modification which is there determined shows itself by a painful sensation, reported by the sensorium to the periphery of the fibres which take rise in the mass of the gray substance; and it is in this manner that we are enabled to explain the radiation of a dental neuralgia, for example, over the whole corresponding half of the face.*

"It is doubtless because this modification of the nervous centres

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*For this explanation to be admissible, it is necessary to suppose that the various cellular groups of the origin of the trigeminus, selecting this for an example, communicate, the one with the other, by anastomosing conductors. In the normal condition, when one group of cells receives an excitation from the periphery of the corresponding nervous branch, these conductors do not permit a sufficient stimulation of the other groups to make them enter into activity; but it is no longer so when a morbid irritative condition produced in one group invades those adjoining, and their anastomotic conducting elements. These new conditions once established, the excitations produced at the periphery of one branch of the trigeminus can pass, preserving their intensity, from the origin of that branch to the *foci* of origin of the other branches; and, on the other hand, can cause in the one as well as the other, those modifications of functional activity, which we become conscious of as pain.

exists in so many cases of neuralgia, that the treatment by the section of the nerve so often fails. This explanation appears to me at least as acceptable as the one admitted by the majority of surgeons, and which M. Weir Mitchell appears to accept. According to them, the persistence, or the reappearance of the disease, after section of the nerves, in a great number of cases of neuralgia, indicates, perhaps, that the nerve is diseased for a considerable portion of its length, so that after the division an altered portion remains in connection with the nervous centres; or, perhaps, that the alteration commences anew above the section.

"I certainly do not deny the possibility of the existence of a neuritis occupying a part of the course of a nerve in certain cases of neuralgia; but direct search for that morbid condition has too often failed for us to say that a neuritis is the ordinary lesion in neuralgias which are rebellious to surgical treatment. The hypothesis which I have indicated, and which I have developed many times in my lectures, seems to me, on the other hand, to account sufficiently well for all the facts, and may be held provisionally.

"When the neuralgia has for its exclusive cause a central lesion, whatever it may be, it is easy to understand that the section of the nerve which goes to the parts where the pain seems to be, cannot, in general, immediately cure the affection. In fact, the cause exists, and may give rise to painful sensations which will still be felt, by a well-known illusion, as though they resulted from impressions arising in the extremities of the divided nerve. Nevertheless, we should say that, in cases of this kind, the section of the nerve might cause a considerable diminution of the pain, or even make it cease. The section, by interrupting or modifying the physiological relations which previously existed between the central region affected and the periphery of the body, might produce in that region a functional change sufficient to cause the cure, or a more or less marked alleviation of the neuralgia. We will readily admit that matters may so happen, at least occasionally, if we call to mind the instantaneous relief which we so often observe to follow in cases of neuralgia, from the application of agents, either irritant or sedative, over the parts of the surface where the pain seems located. I need only cite one example, that of the terrific pains, more or less continuous, differing from lancinating pains, which are often suffered in the course of locomotor-ataxia, sometimes in one, sometimes in another, part of the body of the patient. These pains, due to a morbid irritative action in the cord, frequently disappear entirely in a few minutes under the influence of topical applications, such as a compress saturated with chloroform, over the part which the patient indicates as the seat of his sufferings.

"If the neuralgia is dependent on a lesion of the extremities, or any other points whatever in the course of a nerve, there are, as I have said, at a certain moment of the development of the affection, two principal factors which concur in its maintenance, the peripheral irritation and the morbid excitability of the central elements for the reception of impressions. Sometimes the division of the affected nerve, by suppressing completely the influence of the irritation of its peripheral extremities on its centra origin, abolishes, immediately and definitely, every symptomatic manifestation, and the morbid excitability of this central *foyer* does not delay to disappear without any question. In certain cases, the nervous section is without any effect, even a temporary one, on the neuralgia. It is presumable, in these, that the section has not intercepted all the routes by which the morbid excitability of this central origin may be put in play. It may be, even, that in certain circumstances, this excitability having continued in spite of the operation, the neuralgic symptoms can still show themselves under the influence, for example, of circulatory disturbances. In other cases, finally, the section only produces a transient cessation of the symptoms; we may then suppose that new irritations, taking rise perhaps from other branches of the same nerve, perhaps from other nerves, it may be from the peripheral extremity of the central portion of the divided nerve, have aroused the morbid excitability of the central elements having relations with the divided nervous fibres. In all probability, the diffuse neuritis plays only exceptionally an important part in cases of this kind.

"We perceive by these considerations, that grave discussions may be raised in regard to the location of the morbid process which gives rise to neuralgias. We can understand, therefore, that we will meet with much that is doubtful, if we attempt to establish the pathology of nervous lesions by the aid of studies made upon the neuralgias.

"M. Weir Mitchell has not found himself confronted by such great difficulties. The locality and the character of the traumatic injuries to nerves are naturally known, and he has, therefore, a solid basis for his deductions. The results furnished by the study of these lesions, are, moreover, not exclusively applicable to themselves. They have a general character, which allows us to use them in the history of other nervous affections.

"M. Weir Mitchell has been enabled to describe, better than any of his predecessors, certain alterations of nerves, in regard to which we have hitherto had only very imperfect ideas. I do not here speak of the congestions of nerves, because that which he has here said on this subject reduces itself, in fact, to the *expose* of experiments made upon animals—experiments that are certainly of great interest, but which are insufficient to make us admit that such a morbid condition can engender, in man, well-defined symptoms. I do not dare to say, like the author, 'that many diseases which we are compelled to call *functional affections*, are allied to this pathological condition, of which we find no traces after death.' I am much more disposed to believe that these affections are due to modifications of the nervous centres; and inasmuch as the material proof of the habitual coincidence be tween congestion of the nerves and certain neuralgias has not been given, we ought, it seems to me, to consider this opinion as an unfounded hypothesis. And yet, even when we have proved the reality of this coincidence, it will be necessary to show that the affections concerned, the *functional nervous disorders*, are really due to the congestion, and not, on the contrary, its determining cause; hyperæmia is so often a result of the suffering of the organ in which it makes its appearance.

"But M. Weir Mitchell has studied neuritis in its principal forms, the acute and chronic; and this study is so much the more valuable as respects human pathology, as it has been made entirely by the aid of clinical observations. Experimental pathology has been, and can be here, only a very feeble aid, since it is extremely difficult to provoke, in animals, a true neuritis of progressive march. M. Weir Mitchell has succeeded but once, out of numerous fruitless attempts, in producing an inflammation of a nerve in the rabbit. How many experiments have I not also made in this direction? I have included the nerve in ligatures drawn more or less tightly; or I have pressed it between the point of a forceps; have bruised it, by striking it between two hard bodies; cauterized it with various substances—the essence of cantharides, liquid ammonia, acetic acid. I have pierced it in all directions, with needles, etc., without ever obtaining a true suppurative neuritis extending beyond the points submitted to experimental violence. I have never seen a single case of ascending neuritis of any form in these experiments. As to the lesions recognized in the peripheral extremity, and which offer the characters of interstitial neuritis, that is to say, a multiplication of the elements of the neurilemma and the sheaths, they are in no respect different from the lesions which follow a simple section of the nerve.

"According to the description of M. Weir Mitchell, we see that the neuritis in man has a great tendency to propagate itself from the periphery toward the nervous centre, and to implicate other branches of the same trunk or plexus. This tendency is especially manifest in chronic neuritis. But it is necessary to say that certain cases of chronic neuritis are not mentioned by M. Weir Mitchell. He says nothing, in fact, of the remarkable ascending neuritis noticed by Remak, and well studied by M. Charcot and his students, and which I have often had occasion to verify [I speak here of the neuritis which occurs in cases of descending sclerosis of the cord, following lesions of certain parts of the encephalon]. We are aware that, in these cases, there is a genuine interstitial neuritis, which does not at all necessarily cause the atrophy and destruction of nervous fibres, but which gives rise to an augmentation, more or less considerable, of the volume of the nerve, and is often accompanied with spontaneous pains along its course, with, at the same time, a very lively sensibility to pressure. This lesion shows itself very readily in the nerves of the members. I have many times observed, in the nerves so altered, that mechanical alterations, such as friction or compression of the nerves, not only produce severe pains, but cause, even much more readily than in the healthy state, contractions of the muscles supplied by them. This can be well observed by rubbing the cubital nerve in the olecranial channel in the upper members of a hemiplegic subject. In case the hemiplegia is some weeks old, if there is a certain degree of contraction in the members of the paralyzed side, the friction of the cubital nerve of this side causes not only the painful sensation of numbness extending as far as the ends of the ring and little fingers, but also a rather severe local pain, and, further, contractions in all the muscles innervated by this nerve. On the sound side, on the contrary, the effect is limited in general to the feeling of numbness we have mentioned. I will add, that we can obtain the same muscular contractions, though perhaps to a lesser degree, by rubbing the cubital nerve of the paralyzed side in certain cases of hemiplegia with flaccidity of the members. Ought we to consider that in these different cases there is only an exaltation of the motricity of the nerves? Are the muscular contractions caused by the friction of the cubital nerve exclusively due to a centrifugal excitation, extending from the irritated part of the nerve to the muscles? We will not affirm this. These contractions may, in fact, also result from a reflex stimulation of the muscles innervated by the cubital nerve. We will understand this when we take into account, the exaggerated state of excitability in which we always find the half of the spinal cord on the paralyzed side in hemiplegia; that is to say, the side opposite the seat of the lesion.

"A very interesting chapter is devoted by M. Weir Mitchell to the study of the various forms of traumatic injuries of nerves. He successively passes in review sections (cuts), punctures, contusions, lesions due to luxations, or the operation of the reduction of luxations, those which take place from fracture, and those which are produced by compression.

"Next, he gives the symptomatology of nervous lesions. This chapter is one of the most important in the whole work. After indicating the phenomena manifested at the moment the lesion is produced, he studies the general symptoms which show themselves at the first moment after the wound is received. In a certain number of the cases observed by M. Weir Mitchell, there was immediately a shock or commotion of the nervous system; the wounded person fell, with, sometimes, a loss of consciousness, and sometimes consciousness retained. In some there were immediate intellectual troubles. In all these cases there was evidently a violent shock to the nervous centres; but while in some of the subjects it was, as M. Weir Mitchell says, the cardio-motor centre which suffered the commotion, in others it was the cerebral hemispheres that were affected. The instances of sudden delirium reported by the author, which were produced under the influence of nerve lesions in the superior member, are among the most striking. Other facts ought also to be specially noted, such as those in which we see a wound involving the nerves of one region producing a paralysis in a part more or less distant; a gunshot wound, for example, in the right thigh, involving the crural nerve (observ. 27, p. 161), caused a paralysis of movement of the right arm. This case, and the others which M. Weir Mitchell cites, are examples of *reflex paralysis;* a morbid phenomenon which the author has proposed to designate under the evidently more appropriate name of *paralysis by peripheral irritation.* The theory of these paralyses, as I have myself often remarked, shows, in fact, that no reflex phenomenon is produced in this case. A group of anatomical elements of the cord is struck into inaction by a lesion of a part of the body which appears to have no direct relation with them; hence, paralysis of the muscles, innervated by the nerves, whose functional activity requires the physiological integrity of these elements. There is, in this case, no transmission from the centre to the periphery; everything is confined to an influence toward the centre, reaching and modifying a certain part of the nervous centres; there is nothing of reflex nature. The expression, *reflex paralysis*, should, in the actual state of science, be expunged from the medical vocabulary.

"One of the points of symptomatology on which M. Weir Mitchell most insists, is that relative to troubles of nutrition caused by nervous lesions. The numerous opportunities which he has had for this kind of study, have led him even to give a very detailed description of these accidents, well known to-day in France, thanks to the labors of M. Charcot and his students. He passes in review, also, the alterations produced in the muscles, the skin and its appendages, the connective tissue, and the articulations.

"*A propos* to the alterations which the nails undergo under the influence of nervous lesions, M. Weir Mitchell makes a remark which struck me when I first met with it some months since: 'The nails,' says he, 'continue to grow after section of the nerves; while, in the case of cerebral paralysis, their growth is entirely suspended.' We are well aware that when the nerves of a member are divided the nails may still grow on in a more or less normal manner. Every physiologist has been able to convince himself of this fact; but up to the present we have not observed the arrest of growth of the nails in cases of hemiplegia of cerebral origin. It is a phenomenon difficult to understand, since, in a general way, we see the influence of nervous lesions on the nutrition of parts diminish as they bear less directly on the nerves of these parts, or on the regions of the spinal cord in relation with the origin of these nerves.

"On the other hand, we should say that, if there is no cause of error in the observations of M. Weir Mitchell, it is very probable that he has had under his eyes cases altogether exceptional. I have, in fact, sought myself to test the assertions of the author.

I have blackened, with nitrate of silver, the nails of both hands, as far as they were exposed, of three women in my hospital service, who suffered from hemiplegia, produced by hæmorrhage or softening of the brain: but have observed that their growth continued on the paralyzed as well as on the healthy side. I did not recognize any difference in the nails on the side of the hemiplegia, either in the form of parts turned back or in their growth.

"The observations of M. Weir Mitchell relative to the alterations of the nutrition of the different tissues under the influence of nervous lesions, are very instructive, and serve to call attention to that interesting subject. As regards the mechanism of this influence of the nervous system on the nutrition of tissues, the author speaks only with reserve, and thus gives evidence that he is well acquainted with all the difficulties of the question.

"He examines the two principal theories proposed for the explanation of this mechanism, and adopts neither. Like most physiologists and actual pathologists, he does not imagine that we can account for the disorders of nutrition by making them depend exclusively on modifications of the vaso-motor fibres comprehended in the injured nerves; nor does he any more believe that we are authorized to admit the existence of the *trophic nerves* imagined by M. Samuel. As long as we have been furnished with no proofs of the existence of these nerves, it is best not to base any theories on this hypothesis.

"M. Weir Mitchell is disposed, rather, to regard the trophic influence of the nervous centres as exercised on the tissues through the sensory, or excito-motor, or the motor fibres themselves.

"As regards the influence of the nervous centres on the muscles, it is very probable that this is the case. If the muscular atrophy due to a lesion of a nerve were caused by a physiological modification of the vaso-motor fibres contained in the nerves, and particularly to a *paralysis* of these fibres, the atrophy ought to be so much the less, as the lesions affect the nerves nearer their origin. They receive, in fact, by anastomosis, in various parts of their course, fibres from the sympathetic, containing, according to all appearance, vaso-motor filaments; and experiments confirming this presumption afforded by anatomy, show that paralysis of the vessels, in the parts which receive the ramifications of a nerve, is more pronounced when we divide the trunk of this nerve than when we cut its roots. But it is easy to show that muscular atrophy is quite as rapid and pronounced when we divide the roots of a nerve, as when we divide its trunk. I have demonstrated this in the facial nerve, and, undoubtedly, the result would be the same with any other. I have shown that the section of the facial nerve, made at the level of the floor of the fourth cerebral ventricle, in the immediate neighborhood of the origin of this nerve causes in the muscles of the face an atrophy altogether comparable, in all respects, with that which is produced when we cut the facial at the level of the masseter muscle.

Besides, lesions which rapidly destroy the cells of the anterior cornua of the cord, produce as rapid and profound an atrophy of the muscles innervated by the nerves in relation with these cells, as would a traumatic lesion interrupting the continuity of these nerves at any point whatever of their course. It is this which we have identified, for example, in cases of infantile atrophic paralysis, or in those atrophic paralyses in youth or adult age consecutive to various affections, eruptive diseases among others.

"It is not the paralysis of the vaso-motors, therefore, that causes the atrophy of the muscles following lesions of the nerves distributed to these organs. And this proposition hardly needs special proof, since innumerable physiological observations have shown that paralysis of vaso-motor fibres does not cause atrophy of the muscles in relations with them; for example, I have never seen an atrophy of the muscles of the head produced in animals after division of the cervical sympathetic, or laceration of the superior cervical ganglion.

"It seems to me, that the well-known facts that I relate may also be invoked to show that the muscular atrophy dependent on sections of nerves, does not have for its cause an *excitation* of the vaso-motor fibres, with constriction of the vessels, diminution of the afflux of nutritive material, and destruction of muscular elements. Further, if we study in each case of nervous section the state of the circulation in the parts which the nerves supply, we easily recognize that the vessels, at least in the first period, dating from the moment of the lesion, are dilated, rather than constricted. There are seen, to a certain degree at least, the same phenomena as those which we observe over one whole side of the head, when we have divided the cervical sympathetic of that side; and *a priori* these are then the conditions of an exaggeration, rather than a diminution of nutrition.

"Therefore, the muscular atrophy produced by nerve lesions cannot be attributed to modifications of the vaso-motor influence of the nervous centre on the muscles; and consequently, we have to admit that this atrophy is due to modifications of some other influence exercised on the muscles by the nervous centres and nerves.

"But great difficulties yet remain. We are compelled, in fact, to demand if the muscular atrophy is produced in these cases by an enfeeblement or by an exaltation of the influence in question. Let us see if it is possible to explain the muscular atrophy by admitting that, when the nerve is completely divided, it depends on an irritative action provoked in the tissues of the muscles by a traumatic excitation of the peripheral extremities of the severed nerves.

"First, how are we to suppose that a traumatic, inflammatory excitation of the divided nerves, allowing that a phlegmasic excitation, with all its characters, does there exist, can yet produce and maintain any irritation whatever in the muscles, when these nerves, at the end of a few days, have incontestably lost their physiological properties with their normal structure? And then it is needful to note that the atrophic alterations of the muscles which take place when their nerves are cut, presents only imperfectly the characters of irritative lesions. It shows, it is true, a multiplication of the nuclei, or cells of the primitive muscular bundles; a thickening of the internal perimysium and of the sarcolemma; and these are of the character of irritative lesions; but the principal modification, the progressive shrinking of the muscular bundles, is purely atrophic. As this shrinking also occurs in the bundles the nuclei of which are least multiplied, and which are not surrounded by noticeably thickened sheaths of connective tissue, we cannot consider it, in general, as the result of a crowding out of these elements by inflammatory neoplastic products. The vessels of the muscles undergoing this atrophy do not present, in these cases, constant and obvious lesions, so that the atrophic alteration cannot be considered as due to modifications of the vascular walls embarrassing the nutritive changes that ought to take place between the anatomical elements of the muscles and the blood of these vessels. When atrophy of the muscles takes place, after the complete section of its nerves, this alteration does not result, therefore, from any irritative inflammatory lesion, either of the muscular elements themselves, the interfascicular tissue, or the muscular vessels. Further, I repeat, this morbid action, whatever its nature may be, cannot be provoked at a distance in the muscles by irritation of the divided nervous fibres. All that I have said of the cases where the nerves were cut completely across, necessarily applies to those in which only the medullary centre is fully destroyed; for, as we know, the nerves are altered in the same manner as if they had been completely divided.

"But the conditions are not identical, we will say, when the muscular nerves suffer an injury which does not entirely interrupt their continuity, or when their nucleus of origin is only partially affected. Can it not, in this case, through irritation of the nerves, or their origin, produce, either in a direct or reflex manner, and by the intermediation of fibres remaining intact, a morbid super-excitation of the muscles, there producing an irritative action, which results in muscular atrophy?

"Where are the proofs establishing the possibility of such a mechanism? What would be the morbid super-excitation which we are to suppose may enter into action in these cases? It would evidently not be a super-excitation of the muscular contractility. In the immense majority of cases we see nothing like it. The muscles, or certain of their bundles, are paralyzed, and not contracted, either during the first period of the atrophy, or during its whole action; and in those cases where spasmodic contractions are exceptionally produced, under the influence of partial lesions of the nerves or the nervous centres, this condition cannot be considered as a cause of the atrophy. Some muscles may be kept contracted for many weeks, on account of lesions which cause a lasting excitation of their nerves, without, at the end of that time, any manifest signs of atrophy. We have frequently had occasion to observe this in hysterical patients, and sometimes in those not hysterical, but resembling in their general appearance cases of tetanus. I have recently had under my observation in my service at La Pitie a case of this kind. In an adult and vigorous man, a permanent contraction of the muscles of the two lower limbs lasted over three months, without their showing any appreciable diminution of volume.

"If we cannot explain by a super-excitation of the muscular contractility, in the cases of partial nervous lesions, the atrophic influence of the supposed irritation of the nerves and the nervous centres, is it to be explained by a morbid superactivity, more or less perverted, of the nutrition? But the modifications of the structure of the muscles do not present in these cases, any more than in the others, at the moment of their evolution, the characters of an actual irritative lesion; and, moreover, there is a perfect identity between the two categories of cases, as regards the form and progress of the muscular alteration. It is, therefore, correct to assume that this alteration is due to one and the same mechanism in both; and that in these cases the muscular atrophy does not have for its cause an irritation of the nervous elements in relation with the muscles.

"If the atrophic alteration of the muscles is not caused in these different cases by the irritation of the injured parts of the nervous system, it is produced, apparently, in consequence of a diminution or an abolition of the physiological activity of the nervous elements in relation with the muscles. It is in this way that we are obliged to explain not only the atrophy produced in the muscles to which the affected nerves are distributed, but also the so-called reflex muscular atrophies which appear in the course of neuralgias, and even, as I shall explain farther on, those which appear at a distance, in muscles not in any direct relation with the nerves originally affected, it may be in the injured limb, perhaps even in another.

"But what is the mode of influence of the nervous system the suppression or enfeeblement of which involves the production of atrophic alterations of the muscles? Is it the motricity of the nervous elements, or some other physiological aptitude pertaining to them?

"Various pathologists have denied that the functional inertia of the muscles, caused by the section of their motor nerves, or by the destruction of the intra-medullary origin of these nerves, was the cause of the muscular atrophy in these conditions. We may object to this supposition that the functional inertia of the muscles exists, also, in cases of hemiplegia in which the paralysis is complete; and that it also exists in paraplegia, where a compression of the upper part of the dorsal cord prevents all voluntary movements of the lower limbs; and that, nevertheless, we never observe in these cases any atrophy comparable to that produced by division of the nerves or destruction of the cells of the anterior cornua. This objection, it is necessary to say, has only a relative value; because, in these cases, reflex movements can still take place in the paralyzed parts, the immobility of the muscles is, therefore, not as complete as when their nerves are cut, or when the *foci* of origin of these nerves are destroyed.

"It is, therefore, impossible, as regards the muscles, to completely refute the opinion which attributes to their functional inertia a principal, if not an exclusive, part, in the production of muscular atrophy, under the influence of nervous lesions.

"But we may apply to the muscles, by analogy, the conclusion at which we arrive, when we examine the influence of this functional inertia on the alterations of nerves separated from their centres. We know that we have desired to attribute, also, to this cause, the atrophy of nervous fibres which takes place under these conditions; but, as I have already remarked, this hypothesis cannot be sustained.

"First, as regards the motor nerves, we see the peripheral portion of certain ones among them recovering completely its normal structure and properties, after previous alteration, even when its function had been completely abolished. It is this which happens, for example, in the hypoglossal in the dog, when, having cut the nerve across, we completely tear out its central segment with its medullary roots.

"On the other hand, as regards the sensory nerves, we can invoke the same argument, drawn from experiments of the same kind made on certain of them, on the lingual nerve, for example; but we can support ourselves on at least one other fact quite as decisive.

"When we cut across a sensitive nerve, its peripheral segment undergoes, like that of a severed motor nerve, a process of atrophy along its whole length; this is the case, as Waller has shown, when the posterior root of a mixed nerve is cut between the corresponding rachidian ganglion and the point where it joins the anterior root to form the nerve; the portion of the root comprised in the space between the section and the junction of the two roots becomes atrophied, and the alteration extends along the sensitive fibres of the mixed nerve to their extremities. We may admit that, in such conditions, the sensitive fibres are, from the instant of the section, in a state of complete functional inertia, although that may be disputable, since peripheral impressions may yet, inasmuch as these fibres are unaltered, cause an ascending modification. The functional inertia being conceded, we can attribute it to the atrophy of the nervous fibres detected in these experiments. But what of this explanation when the section of the mixed nerve is made between the rachidian ganglion and the spinal cord?

"Waller has shown that when the experiment is so performed, the peripheral segment of the posterior root, that which is yet in connection with the rachidian ganglion, remains intact, and that the same is the case with the fibres beyond the ganglion, as far as their peripheral extremities; while, on the other hand, the central segment of the root, that which is yet in connection with the cord, is altered, and undergoes an atrophic degeneration; and yet the sensitive fibres in their whole course between the point of division and their peripheral extremities, are in the same condition, as regards their functions, as when the section is made between the rachidian ganglion and these extremities.

"As we are aware, Waller, led by these experiments, considered the ganglion of the posterior roots as the trophic centre for the sensitive fibres; while, according to his researches, the gray substance of the cord is the trophic nucleus for the motor fibres.

"These experiments are of the utmost value for our discussion; they prove that functional inertia cannot be invoked to explain the atrophic alterations of divided nervous fibres, since they can exist, in the same degree, in fibres that are atrophied, and in others which are not.

"If we are not authorized to attribute to functional inertia the atrophy of the nerve fibres which takes place in the peripheral segment of a nerve separated from the nervous centres, why shall we adopt that theory for the muscles? Does not the rapid march of the alterations in the muscles, that are cut off from the influence of the nuclei of origin of their nerves, disagree with such an explanation? Fifteen days suffices to produce a very recognizable atrophy in that half of the tongue, the corresponding hypoglossal nerve of which has been cut, and this atrophy is quite considerable at the end of five or six weeks.

"We can, therefore, conclude, without being able to always prove it as clearly as in the case of the nerves, that muscular atrophy does not depend directly on the functional inertia of the fibres in connection with these organs. It is therefore to the suppression of some other influence exercised in the normal condition by the nervous centres on the nerves and muscles, that we have to attribute the atrophic alterations which are developed in these organs after they are separated from these centres. What is this influence? It is impossible, in the present state of our knowledge, to convey a clear idea as to its nature; and we are compelled to designate it by a particular name. The word *trophic*, already employed by Waller, seems to us sufficient. We say, therefore, that the nervous centres exercise over the tissues, and particularly over the nerves and muscles, a *trophic influence*; and that it is the enfeeblement or abolition of this influence that causes the atrophic alterations of nerves and muscles when the connection of these organs with the nervous centres are broken or simply diminished. But we do not consider that this influence is exercised by the intermediation of particular nervous fibres, having a special functional *role;* it is not transmitted to the nerves and muscles by distinct *trophic* nervous fibres, constituting the *trophic nerves* of M. Samuel.

"This *role* is not one of the physiological attributes of the sympathetic nerve fibres. Among the reasons that I have given for rejecting the intervention of the vaso-motor fibres, are two which I will repeat in this place: on the one hand, is the fact that the section of the cervical sympathetic of one side, and the laceration of the corresponding superior cervical ganglion, never causes an atrophy of the deep or the superficial muscles of the corresponding half of the head; and, on the other hand, the second fact, that the division of the root of the facial nerve in the medulla itself produces, in the face, an atrophy exactly like that which follows the division of the same nerve in front of the parotid gland, although the nerve between these two points receives numerous anastomotic filaments from the sympathetic.

"We are therefore led to think that it is the musculo-motor fibres themselves that carry to the muscles the trophic influence of the nervous centres; and this, as we have said, is the opinion of M. Weir Mitchell.

"Injuries of nerves cause not only the so-called reflex paralysis, they can also provoke muscular atrophies at a distance; and these accidents, studied already by various authorities, have also been designated by the name reflex. M. Weir Mitchell speaks of these atrophies in many passages of his book, and inclines to consider them as produced by an ascending propagation of an inflammation of the nerves, the irritation passing from the nervous cord directly affected to other branches of the trunk from which this cord arises, or even to other nerves of the plexus which it aids to form. I believe that, in the cases where the atrophy at a distance is considerable, it depends, ordinarily, not on the cause supposed by the author, but on a modification produced, under the influence of the injured nerve in a more or less distant portion of the gray substance of the spinal cord. And it is not impossible, even, that, in certain cases, this modification may show itself in a part of the gray matter very distant from that which gives rise to the nerves primarily affected. Experimental physiology shows, in fact, that lesions of the nerves may influence regions of the spinal cord very distant from that which contains the focus of origin of these organs. As M. Brown-Sequard has shown, the division of the sciatic nerve in the guinea-pig can cause, in the medulla oblongata, and, probably also in the upper portion of the cervical spinal cord, morbid modifications, which engender epilepsy, on one hand, and, on the other, give rise, under the same conditions, to the appearance of the cutaneous epileptogenic zone on the postero-inferior region of the face, and the superior lateral portion of the neck. A fact like this, so well established, authorizes us, it appears, to admit that, in certain cases, there can also be produced, under the influence of injury to the nerves of a member in the gray substance of a medullary region far removed from that which gives rise to these nerves, a more or less profound alteration, resulting in the atrophy of the muscles whose nerves are supplied from the region last attacked.

"When the muscles which are secondarily atrophied are innervated by nerves forming part of a trunk or plexus whence is derived the nerve directly injured, it will be the modification produced in the seat of origin of this nerve, which extends itself to the neighboring parts of the gray substance; it next produces a centrifugal alteration in the whole, or, more frequently, in one portion of the trunk or plexus of which the nerve made part, and, at last, an atrophy of the muscles supplied by these nervous fibres so attacked. In some cases, where I have cut, in guinea-pigs, the great sciatic nerve in the middle of the thigh, I have seen the small sciatic nerve, which had not been touched, undergo, at the end of a certain time, a quite pronounced simple atrophy; and M. Brown-Sequard has shown, in cases of the same kind, that muscles corresponding also to uninjured nerves, may become atrophied in a very remarkable manner. He would evidently not have the least difficulty in admitting that these phenomena may occur, under these conditions, in man as well as in the lower animals.

"The alterations which the cutaneous integuments may present, in cases of injury of nerves, have been studied with especial care by M. Weir Mitchell. The mechanism of their production appears to me even more obscure, if possible, than that of the muscular alterations. It is difficult, here, to reproduce in animals the results which we observe in man. It is rarely that we are able, by means of injuries to the nerves or the nerve centres, to produce on the usual subjects of vivisection (dogs, cats, rabbits, and guinea-pigs) cutaneous affections similar to those we see appear in man, following lesions of the nervous system. The most of the eruptions we have observed on animals that have undergone the operation of dividing certain nerves, the sciatic nerve, for example, were developed beyond the limits of the region supplied by the injured nerve. As to myself, I have never seen any true eruption on the member corresponding to the cut sciatic nerve. Among all the numerous animals in which I have divided this nerve, I have caused, in some cases, the falling out of the hair; and very often, the only lesion of the skin—a very inconstant one, moreover—has been the formation of eschars from undergoing pressure, either from within or from without. Light on this question, from experimental pathology, is, therefore, almost absolutely lacking.

"Is it, then, to the paralysis of the nerve fibres going to the integument, that we are to attribute the development of the diverse affections of the skin and its appendages, observed in consequence of injuries to the nerves? Or do they take place, on the other hand, under the influence of an irritation of these fibres? M. Weir Mitchell adopts this last view; and, supporting himself by the fact that the cutaneous alterations are, in general, more considerable in cases of incomplete than in complete section, he thinks that, most frequently, they are the result of a reflex irritation of the integument. One other consideration pleads, also, according to him, in favor of this hypothesis: the alteration of the cutaneous surface ordinarily appears but slowly.

"Nevertheless, it is necessary, I believe, not to go too far, and, without making serious reservations, to attribute the alterations of the skin and its appendages, in cases of nerve lesions, to a state of neuritis or of sclerosis in the affected nerves. Besides, M. Weir Mitchell only regards these morbid conditions of the nerves as the *more frequent causes* of the alterations, and considers that, in certain instances, they may be due to an interruption of the nervous relations between the integuments and the nerve centres.

"For my own part, I believe that it is, on the other hand, to this interruption, more or less complete, that we must usually attribute the production of these alterations. They appear to me to be due, at least in most cases, to an enfeeblement or an abolition of an influence exercised by the nervous centres on the nutrition of the skin, an influence analogous to, but less powerful than, that which these centres exercise on the muscles and nerves.

"This influence is transmitted, without doubt, to the skin, by the sensitive fibres themselves; and there is no need of admitting, here, the existence of special trophic nervous fibres, after we have already rejected them in the case of the muscles. It is necessary to see that there is, then, no special difficulty. That the sensitive nerves transmit the *experimental* excitations only in the centripetal sense, or in both centripetal and centrifugal, or that the exclusive route of the transmission of the excitations, *in the normal conditions of the functional activity* of these nerves, may be centripetal, is of small importance. It is certain that the trophic influence of the nervous centres may be carried centrifugally by the sensitive nerves; and a better demonstration of this than we can give is furnished by observing the atrophy which shows itself in the sensitive fibres themselves, when they are separated from their trophic centres; that is, between their rachidian ganglia and the periphery. It is the peripheral portion that is altered in this case; it is, therefore, that portion that is withheld from the trophic influence of the ganglion, this influence being centrifugally transmitted to that part. If the trophic influence of the ganglia of the posterior roots is carried to sensitive fibres associated with motor ones in a mixed nerve, following a centrifugal course, it may, following the same direction, propagate itself as far as the cutaneous integuments.

"Therefore, in my opinion, the alterations of the skin and of its appendages, the subcutaneous cellular tissue, etc., originating under the influence of those of the nerve centres, rachidian ganglia, and bulbo-spinal axis, depend, most frequently, if not always, on a diminution or a default of trophic action of the nervous centres.

"There can be no doubt, it seems to me, in regard to the atrophic and necrosial alterations of the tissues in question. These alterations are most plainly produced when the tissues are entirely cut off from the trophic action of the centres; and vivisections prove that, when they take place under these conditions, they show themselves so much the more promptly, as the cessation of this action is sudden and complete. In fact, among all the lesions that the nerves can be made to undergo, it is the section or the laceration—the rupture, in short—of these cords, that causes the most rapid and extensive alterations of the skin and subjacent tissues (œdema, sphacelus).

"As to the other alterations, they comply better with the hypothesis of an exaltation of the trophic action of the nervous centres. I would speak here of the various herpetic, vesicular, and bulbular eruptions, which we observe in these conditions; of the cutaneous ulcerations, whitlows, and one form of alteration which MM. Mitchell, Morehouse, and Keen, have made better known to us than their predecessors, describing it under the name of glistening aspect of the skin. In most cases, all the relations between the localities of the cutaneous affections, on the one hand, and the central parts of the nervous system, on the other, are not absolutely broken. We may, therefore, suppose that the morbid excitations provoked by the inflammatory action in the part of the nervous system which is subjected to an injury, traumatic or otherwise, causes in the trophic centres of the sensitive nerve-fibres, that is, the ganglia of the posterior roots, an irritation that is reflected by these fibres toward the periphery. We may suppose, also, with M. Weir Mitchell, that, in a certain number of cases, the excitation originating at the point of the nervous system, or the locality of the primary lesion, acts directly, in a centrifugal sense, on the nutrition of the skin and other tissues, by irritating them, and so causing the development of the alterations in question. But these hypotheses are far from being yet proven. It is possible that there is, in these cases, simply a disorder of nutrition (dystrophia) caused by a diminution of the central trophic influence. I am well aware that the appearance of these cutaneous alterations is often preceded, or accompanied, by symptomatic phenomena, pain, for example, indicating a more or less intense irritation of those parts of the nervous system suffering injury. But cannot we admit, simultaneously with these phenomena, and perhaps from the same cause, an enfeeblement of the trophic action of the centres?

"We may, therefore, if this method of reviewing the question be correct, embrace in one general formula all the pathogenetic conditions of the alterations which are brought about in the muscles, the skin, and the other tissues, under the influence of injuries to the nervous system. We can say that these alterations are the result of disorders of nutrition, due to the abolition or diminution of the trophic action of the nervous centres on the different tissues.

"M. Weir Mitchell has not neglected any part of his subject; therefore, we find in his work valuable information in regard to the influence of nerve lesions on the sudoriparous glands, on the subcutaneous connective tissue, on the articulations, and on the modifications of temperature that are produced in parts of the body supplied by the affected nerves. He describes the effects of injuries of nerves on the functional action of the muscles; next, he studies the disorders of sensibility induced by these lesions. This study is full of interest. All the modifications of sensibility which present themselves in cases of nerve injury are passed in review and subjected to a very delicate physiological analysis.

"M. Weir Mitchell has met with cases analogous to those long known, in which a traumatic violence acting on the nerves of a member, destroys the power of motion in a certain number of muscles, leaving more or less intact the cutaneous sensibility of the corresponding region. The experiments of MM. Arloing and Tripier* put us in a position to-day to explain these facts, by showing us that if a mixed nerve, the cubital, for example, is divided, the region of the skin to which this nerve is distributed does not completely lose its sensibility on account of the anastomoses, very variable, moreover, that are furnished to the extremities of this nerve by the other sensitive nerves of the arm; the muscles supplied by the fibres of this nerve, on the contrary, are necessarily paralyzed, because the arrangement that exists for the cutaneous nerves is not, also, common to the muscular nerves.

"It is in this way, also, that we can explain, at least in certain cases, the more rapid return of the sensibility than the motility. It is probable that the cutaneous anastomoses of the fibres of a nerve, of the arm, for example, permit, in some cases, the prompt re-establishment of a certain degree of sensibility, if there has been complete anæsthesia and paralysis of motion on account of an injury to the nerve which supplies this part. But there is still another condition which accounts for this peculiarity, when the sensibility remains abolished for some weeks. When a mixed nerve has been cut transversely, we know that the muscles innervated by this nerve undergo atrophic alterations, which may become extensive if the reunion of the two parts of the divided nerve does not soon take place. The regeneration of the muscles only commences when the continuity of the nerve is re-established, and its peripheral portion has recovered its structure and functions. This muscular regeneration demands for its accomplishment a considerable period, the longer in proportion to the length of time that the division has existed.

"We understand, therefore, what should take place at the time when the reunion of the two segments of the nerve, and the restoration of its peripheral portion are sufficiently advanced to allow the passage of excitations throughout its whole extent. The sensibility will readily be re-established in the cu-

* I have repeated the experiments of these physiologists for the commission of the Institute, who were charged with the examination of their work, and have proved that they give the results these authors claim.

taneous region supplied by the severed nerve, because the skin, on the one hand, and the spinal cord, on the other, have undergone only slight modifications, while voluntary motion is still for some time difficult and feeble, because the work of muscular regeneration only really commences at the time of the reunion of the parts, and is perfected but slowly. These muscles, therefore, remain for some time in a more or less marked state of atrophy, and respond to the excitations of the will only by feeble contractions. The difficulty of voluntary motion depends, also, on the modifications that are produced in the fibrous or tendinous parts of a member, when the paralysis caused by the division of its nerves has been of long duration.

"When nerves suffer a powerful compression, or a violent contusion, that portion lying between the point compressed or contused and the periphery, undergoes an atrophic alteration quite as complete as in the case of their section. But in this case, inasmuch as the continuity of the fibres is interrupted, without the nervous cord itself being divided, this continuity may be rapidly re-established. The muscles are still very little altered when the transmission of the excitations again become possible; the power of voluntary motion may, therefore, promptly regain its normal condition. It is plain, moreover, that if, from any cause, the restoration of the peripheral segment is hindered, in cases of contusion or compression, the conditions approach those produced by the complete division of the nerves, and the restoration of voluntary movement will be quite as gradual in the one case as in the other.

"I have already developed these considerations in other works, and need not dwell on them farther. M. Weir Mitchell calls them in, also, to explain the difference which exists, as regards rapidity, between the return of sensibility and that of motility, in certain cases of injuries to nerves.

"According to him, this difference finds, also, a *raison d' etre* in the different conditions of the skin and muscles, as regards their natural excitants. The skin undergoing, without cessation, the diverse excitations produced by external agents, will always preserve its active excitability, and re-assume its functions as soon as the transmission of impressions from the centres again becomes possible; the muscles, on the other hand, when deprived of their physiological excitants, that is to say, the motor excitement emanating from the cerebro spinal axis, become less and less excitable, and answer to that incitation only with great difficulty when the continuity of their nervous fibres is re-established.

"This hypothesis, ingenious as it is, seems to me hardly acceptable; besides, it is perhaps needless, when we are able to explain the facts in question very clearly by means of the physiological facts I have given.

"The chapters that M. Weir Mitchell devotes to the treatment of injuries to the nerves, merit all the closest attention of the reader. The various indications furnished by the circumstances of the lesion, by its nature, by its primary or secondary symptoms, and by the later accidents to which it may give rise, are all successively examined with the greatest care, and the author gives a very judicious statement of the therapeutical resources, which the physician may employ to meet each of these indications.

"The author next examines the lesions of certain spinal nerves: the great sympathetic, the facial, the motor oculi, and the trigeminus. Finally, in the last chapter, he traces the history of the nervous affection of stumps after having shown the functional and other modifications which take place after amputations.'

"We find in this chapter an extremely interesting study of the illusions and sensorial hallucinations in regard to the amputated part. M. Weir Mitchell has had frequent occasions to observe these phenomena; he has noted various remarkable peculiarities, among which we cite the two following: In a case of amputation of the leg, in which the illusory sensations of the absent foot had almost entirely disappeared, he saw a second amputation, performed higher up on the same limb, cause them to be renewed. On the other hand, in subjects who no longer experienced the sensation of the amputated limb, he was able to make them reappear by electrizing the nerves which supplied the stump.

"The principal affections of stumps, indicated by M. Weir Mitchell are, neuralgias and chorea of the stump. The author attributes neuralgia of the stumps to an ascending neuritis. I have already explained my position in regard to this theory. If we can really refer these neuralgias to neuritis in some cases, it is yet very probable that they often have another cause, at least, in part. As I have said, according to all appearance, there is produced in these cases a morbid modification of the gray substance of the regions of the spinal cord, which give rise to the divided nerves. This morbid modification, in certain cases of amputation, brings on neuralgia, even when the inflammatory irritation of the extremities of the nerves of the stump has disappeared; and this is the reason why the section of these nerves, performed in order to remedy this neuralgia, produces no lasting cure, and may not even procure a temporary relief. As to the *chorea of the stump* (the author designates under this name an affection which is described by no one before him), I have no doubt but that it also depends on a morbid modification of the gray matter of the spinal cord; only this modification is different, either in nature or location from that which I have mentioned in regard to the neuralgia; but, in certain cases, the two can coexist, as prove the cases in which M. Weir Mitchell observed the choreic spasms of the stump at the same time with a more or less violent neuralgia.

"In what do these morbid modifications of the spinal cord consist that give rise to either neuralgias or chorea of the stump? We cannot tell. Perhaps the wasting, which in all cases of amputation takes place in the region of the cord, in connection with the nerves of the member operated on, creates a predisposition favorable to these modifications.

"This superficial examination of the work of M. Weir Mitchell, cannot give any correct idea of its value. I have allowed myself to bring into special prominence certain points of physiology and experimental medicine, and these are assuredly the less important points of this work. All questions bearing on pathology are treated in the most instructive manner. The various results which are established are supported by numerous observations, nearly all collected by M. Weir Mitchell, and given either in brief or *in extenso.*

"The book, which is very easy reading, as is usually the case when the authors speak of what they have themselves observed, will be consulted with the greatest profit by the physiologist, the physician, and the surgeon.

"The editor, M. G. Masson, has done us a real service in publishing a well-made translation of this remarkable memoir."

With the views expressed in this admirable paper we heartily agree, especially in the stress that is laid on changes in the nerve centres, rather than the nerve trunks, as the pathological basis or essential condition, not only of most neuralgias, but many other affections involving the nervous system. We had intended to speak of certain points in Dr. Mitchell's work not adequately noticed by M. Vulpian, but the length of this notice already, induces us to postpone any further remarks until a future number of the JOURNAL.

Very appropriately following a notice of Dr. Mitchell's work, we may pass to an examination of that of M. Letievant. It is divided into three parts. Part first is devoted to the "Physiological pathology of nerve sections." In the second part, the "Surgical indications of nervous sections in man," are considered; and in the third, or final part, "Operative procedures" are described. This last, falling within the limits of operative surgery, it is not our intention to examine; nor, indeed, does a review of the second part come within our plan.

The work opens with a brief historical sketch of the literature bearing on the subject of nerve sections; then follows a statement of the plan of the work, and a full sketch of the bibliography of the subject, the latter occupying almost seven pages. Then our author enters abruptly on his work. He says:

"I will expose, first, the physiologico-pathological phenomena consecutive to division of the median nerve. This study will permit me to establish firmly certain points of the theory of motor and sensitive substitution, or vicarious supply (*suppleances motrices—et sensitive*), a new theory, destined to throw a strong light on the study of other nervous sections. The doctrine once established, I shall describe, successively, the phenomena which result from section of the ulnar and radial nerves, the principal nerves of the inferior extremities, and those of the face."

After this statement, the author begins to relate cases as a basis for his theory. They are twenty-five in number, and are drawn from a great variety of sources, some of them being cases observed by M. Letievant himself. They are, all of them, instances of real or supposed complete division of the median nerve. The results of a careful analysis of these twenty-five cases are summed up as follows: "Section of the median nerve does not involve either profound change in the tissues, nor an absolute paralysis of sensibility, nor of motility, in the parts to which it is distributed;" and, we may add, to which it alone is distributed. Now, the question is, how it comes to pass, if the median nerve has been completely divided, that parts that seem to be supplied by it alone, retain or recover, notwithstanding, a measure of sensibility and motility. There are just three ways, according to M. Letievant, in which such phenomena may be explained. They may be explained by supposing the nerve to have been repaired or *regenerated;* or we may explain them by supposing the nerve not fully divided. Some fibres belonging to the median must be left as conductors of motor and sensory impressions. But a consideration of M. Letievant's cases, we think, will leave no doubt that both these modes of explanation are inadequate. The third mode of explaining the phenomena in question, is that proposed by M. Letievant himself, and which he calls the theory of *substitution* or *vicarious supply.* It is this:

In case of the sensibility which remains, after division of the median nerve, for example, in parts innervated by it, the route for sensory impressions is not the median, but through anastomotic fibres, which extend between the median and the ulnar and radial nerves. Among other proofs that such an anastomosis exists, he cites a rather striking experiment of MM. Arloing and Tripier. He says: "Those two authors divided, successively, three collateral nerves on the toe of a dog. Sensibility to pain persisted on all parts of the toe. They then divided the fourth collateral nerve. The analgesia became absolute. They were then able to prick or tear the toe of the animal, without the slightest evidence of pain. Before the section, the fourth collateral was sufficient to establish a relation with the nervous centres. On it alone, depended as well the parts of the member to which it was not distributed as those to which it was. This is an incontestable example of sensibility by anastomotic substitution." (P. 48.)

Moreover, our author, besides giving experimental and pathological proofs of his theory of substitution, refers to the anatomical proofs of anastomosis, between the median and adjacent nerves. We suppose impressions made on the *trunk* of the median to be conveyed by these large anastomotic branches, which pass between the median and the ulnar and radial nerves. But, without following into details, we must say his theory of substitution does not satisfy us entirely. We believe the connections of sensory nerves in the skin may be such as to enable the radial or ulnar nerves, for example, to convey, in an imperfect manner, the sensory impressions, the direct and normal routes of which, to the nerve centres, would be the median nerve. In this respect, we conjecture, the arrangement at the periphery of a sensory nerve is much the same as at its central termination, where a nerve fibre connects with a nerve cell; and this, in its turn, with many others: so, that if the direct route of transmission is interrupted in the centre, provision is made for a more circuitous and less perfect one; and an opinion not far from this seems, at least, to constitute the main feature of M. Letievant's theory of nervous substitution.

Though we do not remember to have met with the idea of substitution, in the action of the peripheral nervous system, so definitely stated as it is by M. Letievant, yet the idea is not a novel one in the physiology in the nervous system, and we cannot share fully in the enthusiastic expectations of its author as to its value in nervous pathology.

As to the remainder of this first part of M. Letievant's book, it consists in a detailed account of the effects following division of the nerves of the face and of the inferior members. They relate to changes in the motility, sensibility and nutrition of parts dependent on the nerves in question, which are essentially the same as those related as following division of the median nerve.

The work is especially of interest to the surgeon, but contains many facts relating to the effects of nerve sections on the nutrition of parts which are of interest to the neural pathologist. But phenomena of this latter class it is our design fully to discuss in a future number of the JOURNAL, and so we pass them by for the present.

As regards the last two monographs, they relate to purely intrinsic nerve changes, rather than to the extrinsic effects of nerve lesions. They are highly important. But the present article has so much exceeded the limits intended at first that we must postpone a consideration of them until a future number of the JOURNAL.

II.—RECENT WORKS ON MEDICAL ELECTRICITY.

I. PRINCIPES D' ELECTROTHERAPIE. Par Dr. E. Cyon. Paris: 1873. 274 pages. *(Principles of Electro-therapy.)*

II. NERVENPATHOLOGIE UND ELEKTROTHERAPIE. ZWEITE AUFLAGE DER ELEKTROTHERAPIE. Von Dr. Moriz Benedikt. I. Abtheilung. Leipsic: 1874. 395 pages. *(Nerve Pathology and Electro-therapy. Second Edition of the Electro-therapy. Part I.)*

III. Dr. A. Arthuis. Traitement des Maladies Nerveuses et des Affections Rheumatismales par l' Electricite Statique. Paris: 1873. 162 pages. *(Dr. A. Arthuis. Treatment of Nervous Diseases and Rheumatic Affections by Static Electricity.)*

IV. Die Behandlung der Nervenkrankheiten mit Elektricitat; Eine Uebersicht des Gegenwaertigen Umfanges der Elektrischen Behandlung, und der Anzeigen fuer Dieselbe. Von Dr. Friederich Fieber. Wien: 1873. 66 pages. *(The Treatment of Nervous Diseases with Electricity; A Survey of the Present Scope of Electrical Treatment, and the Indications for its Employment.)*

V. Galvano-therapeutics; A Revised Reprint of a Report made to the Illinois State Medical Society for 1873. By David Prince, M.D. Philadelphia: 1873. 63 pages.

Of the works whose titles appear at the head of this chapter, the first two are in all respects the most noteworthy. The second, however, the treatise of Benedikt, though included here in our rubric, will, for the most part, receive but passing mention, as we hope to review the completed work at a future time. We can, indeed, hardly expect, within the limits allowed us, to more than briefly go over the principal points of the subject as it is treated in the other works; and must, therefore, content ourselves with perhaps an inadequate and partial notice.

Electro-therapeutics, as a recognized weapon against disease, in the hands of the regular physician, is of comparatively recent introduction. Notwithstanding this fact, the popularity which it has already attained is extraordinary; and at the present time there is no department of therapeutics that is looked forward to, by the progressive members of the medical profession, with more interest, as promising to develop a truly rational and scientific treatment of morbid processes and conditions, than is this. Certainly, no other seems to offer more from the alliance of physical and physiological laws, or is more suggestive in view of the important questions of psycho-physics and biology, which are occupying the attention of the scientific world at the present time. Nor have its cultivators been inactive. Since the first appearance of the classic works of Du Bois Reymond, and Matteucci, on electro-physiology, and those of Remak and Duchenne on the application of electricity as a healing agent in disease, many works have appeared, some scientific and accurate, others less so, but practical, and containing valuable clinical facts and suggestions; and, in the medical press of the present day, the literature of the subject is more than ever voluminous. Still, the practice of electro-therapeutics is yet largely empirical; and the amount of professional ignorance on the subject, even among otherwise well-informed practitioners, is something astonishing. And not only this, but this empiricism is even commended as the only true route of progress in this direction; and a distinguished authority has recently advised that the practitioner should make no attempt to master the principles of electrophysiology, and has expressed the opinion that practical electrotherapy is independent of, and in advance of, physiology. We always regret such utterances on the part of educated physicians. They are too much like the cant of narrow men, who would deny the value of the very labors the results of which they utilize, and without which any other than the merest haphazard progress is impossible. We would not wish to be understood as undervaluing the results of practical experience. They are often our only sure guide; but inquiry must not be satisfied with them alone. The charge of pure empiricism is too often made against the practice of medicine, to make us reconciled to anything which might appear to give it justice.

We have been led into this line of remark by reading the introduction of M. Cyon's memoir, in which he laments and condemns this neglect and ignorance of physiology among electrotherapeutists. We cannot do better than quote his own words. After speaking of the advance, in recent times, in the department of ophthalmology, and in the diagnosis and treatment of the affections of the abdomen and chest, he continues as follows: "It is certain that a similar transformation may be produced in neuro-pathology, and, consequently, in the science of the application of electricity to medicine. It is certain that there can yet be given to that branch of medicine a basis rigorously scientific, founded only in physics and physiology. I am convinced, for my own part, that, with the exception of ophthalmic medicine, no department of pathology can comply as well with the application of the laws of physiology, or can put to as immediate profit every acquisition made by it, as the pathology of the nervous system. The question now is, whether, in the actual state of neuro-pathology, it is yet possible to base the application of electricity to the cure of nervous diseases on veritable scientific truths? The response will be either affirmative or negative, according to the limits we assign to this scientific basis.

"At the present day, the reply would certainly be in the negative, if we were to demand the scientific and rational indications for the treatment of each particular case of nervous disease, or if we ask why electrical treatment is sometimes useful, sometimes completely inefficacious. But if we ask if it is possible, at the present time, to elaborate the general principles on which we may base a rational application of electricity for the healing of diseases, then our answer will be in the affirmative. We are now in a condition to seek out what effects the electric current may cause when applied to the human body, its nerves and muscles; and to say what modifications these effects undergo by the use of different sources of electricity; by alterations of the intensity, the duration, and the direction of the current; to indicate, finally, the parts of the nervous and muscular systems which are accessible to these electric agents, and those which are not.

"It is useless to insist at length on the proofs which show the necessity of first establishing the general principles before undertaking a rational application of electricity for the cure of nervous and muscular maladies."

The last paragraph of this quotation expresses the task to which M. Cyon has applied himself. His work is confined to the general principles of electro-therapy. He gives no clinical histories or tables of cases, nor treats of any special indications for the different ailments. His whole book is filled with solid matter pertaining to the general subject in all its bearings. He evidently aims to supply a treatise which shall be free from the errors which have characterized the works of so many of his predecessors, and which he unsparingly criticises. He brings to this somewhat difficult task an eminent reputation as a physiologist and an investigator; and his competency for the work will scarcely be doubted. Neither can it be questioned that he has, in a great measure, succeeded in giving us the best short epitome of the general scientific principles which underlie all rational electro-therapeutics, that has yet appeared. If we have any criticism to make on his work, it will be, that he falls, perhaps, too much into the opposite error from that we have mentioned; that he has too little respect for deductions made from the experience of the profession, and not absolutely based upon received scientific principles. His language may also, at times, appear too harsh; as, for example, when he speaks of the present state of electro-therapeutics as presenting, in fact, only a "chaos of statements, in great part false, of extravagant hypotheses, and theories often void of sense." It is no doubt true that, as he says, electro-therapeutists have too much neglected the true scientific methods, and have too exclusively depended upon the experience gained from purely empirical essays. On the other hand, there may perhaps be some room for doubting whether we are to follow even so able a physiologist and clinicist as M. Cyon, in all his views, when they are at variance with the practical experience of the great mass of those who have treated of the subject of electro-therapy. Until theory and scientific experimentation have entirely cleared the ground, and shown us all the relations in which electricity may act upon the system, in health and disease, practical experience, unscientific though it be, is yet our guide in very many cases.

M. Cyon divides his treatise into six principal chapters. In the first of these, he gives a general view of the present state of electrical physics; explaining, in a clear and readily comprehensible manner, the general laws of electricity; showing the practical application of the law of Ohm as to electrical resistances; and giving, what is usually left out of works of this kind, but which is of the highest importance for the understanding of the action of the agent in the tissues of the body, a full exposition of the laws of the ramifications of currents in different conductors. This part of the chapter is the most valuable. The remaining portion, in which he describes and discusses the different kinds of electrical apparatus for medical purposes, is necessarily much less complete; and much of the matter can also be found in other works. As in nearly all European works, most of the arrangements described are different from those in general use in this country. We will merely mention, that the author recommends, after the Siemens cell, the battery of Grove for medical purposes, and considers the Stohrer modification of the Bunsen element as too unreliable and otherwise inconvenient; an opinion which, as far as regards the comparative merits of these two forms, we will venture to say, is not in accordance with that held by the majority of practitioners in this country.

The second and third chapters are devoted to the consideration of the general principles of electro-physiology and their applications to the human subject. The subject of electrotonus and the laws of nerve and muscle currents are clearly explained in the second chapter; and, in the third, the author gives an account of his own researches on the phenomena of electrotonus in man, which prove the applicability of Pflueger's law in all essential particulars. Next he gives the results of a series of experiments as to the relation which exists between the excitant force and the degree of irritation, made by means of a simple myograph of his own devising, and which indicate that all the laws of nervous irritation deduced from experiments on animals, are valid also for man. The other modifications of the condition of the nerve by electricity, the alteration of its irritability, etc., are briefly mentioned; and in regard to its effects on the nerves of special sense, the author cites Helmholtz's observations on the electrical excitation of the optic nerve, as being perfectly in accord with the laws of electrotonus. As regards the direct irritation of the auditory nerve, he holds, with Erman and Weber, that no direct acoustic phenomena can be produced, thus rejecting, completely, the views and therapeutical theories of Brenner, which have excited so much attention. In a note at the end of the third chapter, M. Cyon alludes to the experiments of Wreden and Loewenburg, which seem to indicate that the sounds described by Brenner and his followers are due to excitation of the tympanic muscles, and not of the auditory nerve; and states that he has satisfied himself of the exactness of the experiments of the first-named observer.

In the fourth chapter, the author treats of the effects of electricity on the different organs of the body. The distinction is carefully dwelt upon between the effects of currents on the nerves themselves, producing molecular change, and those due to the special physiological modifications produced by this excitation. The failure to appreciate this difference is, according to him, the principal cause of the confusion which exists among therapeutists in regard to the subject; and in this light, the emphasis which he places upon it in the commencement of the chapter is fully justified. The great principles which apply to all localized electrization, are fully explained, and the details of the application to different organs are given in a general way. M. Cyon disagrees with Remak, Benedikt, and others, in rejecting, entirely, all galvanic treatment of the brain, considering that currents feeble enough to be safe have too little intensity to overcome the resistance offered by the bony envelopes. The cases related are discredited as being due to faulty diagnosis and hasty assumption. Galvanization of the sympathetic is also discouraged; and anatomical and physiological reasons are given why it should not be practiced. It is impossible, according to the author, to act on the sympathetic in the manner usually practiced, without also affecting the pneumogastric and depressor nerves, the excitation of which has a counteracting effect to that of the sympathetic. The effects desired, he thinks, may be better obtained by other methods of applying the agent. If, for instance, the object is to produce an excitation of the vascular nerves of the extremities, the points corresponding to the location of the dorsal sympathetic, governing the vascular supply of those parts, should be sought, and the electrodes so placed that the current may traverse them. Although M. Cyon is at variance, in this regard, with the great majority of electro-therapeutists, yet it seems that his points are well made; and that, especially in the light of recent utterances by so eminent a neurologist as Dr. Brown-Sequard, as to the danger of this procedure, great caution should always be exercised in the application of electricity to the cervical sympathetic.

The author discards the hypothesis that the excitation of the special sensory nerves is due to reflex action, and maintains that the laws of the ramification of the current, given in the first chapter, are sufficient to prove that the luminous phenomena sometimes perceived are due to direct irritation of the optic nerve, and not to any reflex influence transmitted from the trigeminus. The question as to the duration and intensity required for the currents, is discussed at the close of the chapter, and general directions given. As a rule, M. Cyon recommends applications of the constant current of ten to twenty minutes' duration, to the spinal cord, or to affect the vaso-motor system.

In his preliminary remarks in the fifth chapter, which treats of the subject of electrical diagnosis, M. Cyon calls especial attention to the distinction between muscular excitability and contractility, the latter depending entirely upon structure, and not at all on the exciting agent. The same is the case with muscular sensibility, which may be resolved into two factors—one the sensation of the contraction, and the other the pain due to the direct irritation of the sensible nerves—neither of them in any way dependent on any special action of the electric current. In other words, properly speaking, there are no such things as specific electro-muscular contractility and sensibility; and the erroneous use of these terms has, according to the author, hindered the profession from fully appreciating the true value of electricity as a diagnostic agent.

The electric examination of the sympathetic as a method of diagnosis, is rejected as altogether unfounded on any true physiological understanding of the real conditions and functions of the nerve; and the phenomena supposed to be due to its electrical irritation are attributed to reflex action from the neighboring sensitive nerves, the vagus, laryngeal, etc. The author is especially severe on the view, which has been held by some, of the pathological sensibility of the sympathetic. To quote his own words: "The doctrine of the pathological sensibility of the sympathetic nerve, is one of the saddest evidences of the scientific methods of certain electro-therapeutists; this nerve, of all the rest, is the one which has been most maltreated in recent times."

In the sixth chapter, the differences between the physiological effects of the constant and induced currents are discussed at length, and shown to be due to the differences in their intensity and duration. The indications for the employment of the two kinds are given at considerable length; and, at the close of the chapter, a few pages are devoted to the theory of Duchenne as to the different effects of the induced currents from the primary and secondary spirals on the muscles and nerves, which the author explains, in the usual way, by the difference in the length and fineness of the two wires.

If M. Cyon is, perhaps, too little inclined to give his assent to any methods not based on the most rigorous scientific principles, as much cannot be said of the author whose work we have next to notice. Dr. Arthuis pays very little attention to scientific principles; and the treatment which he here exposes does not appear to be at all dependent upon them. The title of his book is in a measure deceptive. He does not employ static electricity in the same manner as such eminent practitioners as Russell Reynolds, Schwanda, and others; but his method consists, rather, in a kind of homœopathic medication by various substances, which, he assumes, he is able to introduce into the system through the "pores of the skin," by means of the electric current; his ideas being a modification of those of Beckensteiner, more fanciful, even, than the original.

A few quotations will give a better idea of the character of the book than can be otherwise conveyed. The following gives the author's idea of the manner in which his medical agents enter the system: "It is, therefore, by these pores that the electric fluid passes into the interior of the various organs, and to the smallest organic parts. It is by this route, also, that the infinitely minute molecules which escape from the surface of the exciters, and for which the electricity serves as a vehicle, are carried into our organism." The exciters are not usually applied, even to the surface of the body, but are held in the hand of the operator, at some little distance. Not only the different metals, to each of which he gives different virtues, are employed, but also vegetable substances are made use of in the following manner. M. Arthuis says: "When a plant has been designated to be employed in the electrical treatment of a nervous disease, nothing is more easy than to administer it. If the stem or root is sufficiently large, we make of it an exciter, which is employed in the same manner as the metallic exciters. We have so used exciters of a great number of vegetables. If the plant is minute, we reduce it to powder, which we place in a glass tube, closed at one end, presenting, at the other end, a cork pierced with a hole, through which is allowed to pass a minute root of the vegetable, which fills the tube, and in the powder of which it is plunged. With this apparatus we obtain no sparks, but have a sufficiently strong current. By the aid of this simple instrument, there is no vegetable which cannot be used in electro-therapy."

It is unnecessary to comment on the above extracts. The work is noticed here merely because it offers, better than anything else that we have recently seen, an illustration of the unscientific or empirical method carried to extremes.

Dr. Fieber's little pamphlet is what its title indicates, a brief survey of the various affections in which rational electrical treatment has been found to be of benefit, with some judicious general remarks as to the value of electricity as a healing agent in nervous affections.

Dr. Prince's report is confined, mainly, to the subject of the uses of the constant or galvanic current. A number of useful practical hints are given; and the book will doubtless be found very useful and suggestive to those who, from want of time, or from other reasons, have been unable to consult the larger works on the subject. The author gives a list of some of the principal American treatises and reprints, with some judicious recommendations as to a course of reading on the subject. In only one point would we offer criticism. From the accounts of the cases given for illustration, it appears that Dr. Prince is accustomed to frequently employ the full power of his battery of one hundred and twenty Hill cells, or a very large proportion of it, in the treatment of very many nervous affections. While this is good evidence of the comparative harmlessness of galvanism, even of very great intensity, in experienced hands, it yet appears to us as rather heroic treatment, and is in striking contrast with the views of so high an authority as Benedikt, who considers a well-furnished battery of twenty Siemens elements as amply sufficient for all the needs of the physician, excepting only when powerful electrolytic or cauterizing effects are required. From our own experience with galvanism, we should hesitate to recommend, even by implication, such methods to the general practitioner previously unaccustomed to the employment of the constant current in disease; and we have some doubts as to its absolute necessity in any but very exceptional cases.

III.—NERVOUS EXHAUSTION.

NERVOUS EXHAUSTION, AND THE DISEASES INDUCED BY IT; WITH OBSERVATIONS ON THE ORIGIN AND NATURE OF NERVE FORCE. By Hugh Campbell, M.D. London: Longman & Co., 1873. Pages, 195.

Clinically speaking, no single fact in nervous disease is more important than nervous exhaustion. Yet, like many other important facts, it is seldom remembered and acted on in practice as it deserves to be.

Impressed with this thought, we give considerable space to a notice of this plain, practical little book, which is not so much warranted by the book itself as its subject.

In discussing "nervous exhaustion," it is hardly possible to avoid some reference to the "nature and origin of nervous force." Accordingly, the author devotes a chapter to this subject, some of the views in which we cannot pass without notice. The discussion of but few points in the physiology of the nervous system is better calculated to exhibit a writer's possession, or want of, clearness of conception, and penetration, and steadiness of thought, than is the discussion of the subject of "nervous force." It has not only a positive, but a speculative side.

As regards the *nature* of the nervous force, Dr. Campbell says: "There is no question in physiology more definitely settled than that nervous force is as certainly a *substance*, and as clearly demonstrable to the senses, as light, heat, electricity, or any other *imponderable body*," etc. (Page 23.)

But, is it settled that nervous force is a "*substance?*" or that light, for example, is a "*body*," whether imponderable or otherwise? By no means. The fact is, if anything is settled in regard to light, heat, electricity, etc., it is that they are *not* "substances," at least in the accepted sense of the term.

Dr. Campbell gives a short sketch of the progress of nervous physiology, and with particular reference to Du Bois Reymond's experiments; and concludes that the electrical currents that have been discovered in nerve fibres, not to speak of other structures, *are the nerve forces.* But what real proof is there of this position, so often maintained by different writers? None that is conclusive, that we have ever been able to find.

While we recognize the value of the researches made during the last few years, on the electrical phenomena of nerve, muscle, etc., we deny, as in great measure gratuitous, the assumption that nervous force, whatever it may be, is identical with electricity. In a future number of THE JOURNAL, we expect to enter at length into this subject, and only mention it now to protest against the loose habit, of which this is an example, of baptizing an assumption in the name of an established principle or fact.

As regards the mode of generating the electrical currents in the body, or, in other words, the nervous force, he says: "They can be shown to be the natural and inevitable consequence of the processes of *nutrition and de-nutrition*." In the next paragraph he proceeds to show how the work is done. As this ought to be an important part of the book, we quote it:

"In assimilation, or nutrition, the nutrient material, containing many different elements, prepared from the food by the process of digestion, and carried through the circulation, in the form of blood, to the minute structures which it is intended to repair and renew, bears within itself, as almost everything in nature does, natural or latent electricity. This is given off at the moment of assimilation; and as it is a compound substance, consisting of two distinct varieties, named negative and positive, it is decomposed, the negative going to the poles of the nerve molecule, while the positive attaches itself to the equatorial zone; these again combine, as the occasion requires, forming free electricity, which, always flowing in one direction, becomes the true nerve currents," etc.

This passage is a curious medley of facts and fancy. He glides easily along in other passages, as well as this, toward his ill-defined object, without any apparent sense of insecurity in his premises or modes of reasoning. Let us examine this passage briefly and see what it contains. It proceeds on these assumptions:

1. That food bears into the body "natural or latent electricity."
2. That "this is given off at the moment of assimilation."
3. That electricity is a "compound substance, consisting of two distinct varieties, named positive and negative."
4. That this "is decomposed, the negative force going to the poles of the negative molecule, and the positive attaching itself to the equatorial zone" of the molecule.
5. That "these again combine, as occasion requires, forming free electricity, which, always flowing in one direction, becomes the true nerve currents," etc.

Now, have all these points been established? Not at all. And yet, if they have not been, how do such statements show, by "natural and inevitable consequence," the mode of generation of nerve currents, or nervous force? No one doubts nerve force depends on, and is related to, nutrition. But when one proceeds to place by the side of a recognized fact of this kind, and as of equal certainty, such assumptions as the above passage contains, striking evidence is given of a want of knowledge, of the real conditions of proof, and of loose habits of thought, of which there are so many examples to be met with in medical, not to mention other, writings.

Throughout its theoretical parts, where clearness and comprehensiveness of thought, and precision in the definition and use of words, is demanded, we find the book faulty and unsatisfactory, to use no stronger terms.

In the conclusion of his chapter on nervous force, he quotes a passage from Dr. Radcliffe's "*Dynamics of nerve and muscle,*" which begins as follows: "What is called electricity, is only a one-sided aspect of a *law*," etc.

How can this be said, in scientific strictness and propriety? What is electricity? If it is anything, it is a form of physical force. What is a law? Simply the uniform *mode* in which an event happens, or an agent acts. A law *is* nothing, and can *do* nothing. When we speak of the "laws of electricity," we do not mean the electricity *itself*, only its fixed or uniform conditions, or modes of existing or acting. There is no more common nor greater abuse of terms than that which would make the words *law* and *force*, for example, interchangeable.

Then, again, we do not like to see prefacing each literary reference throughout the book, or any book, the Latin word *vide.* It is a small matter; but we never could see either the utility or propriety of such a practice.

Under the head of "Diseases induced by" nervous exhaustion, Dr. Campbell ranges dyspepsia, hypochondriasis, melancholia, spinal irritation, locomotor-ataxy, hysteria, neuralgia, angina pectoris, chlorosis, chorea, epilepsy, asthma, diabetes, paralysis, dyspnœa, etc. The plain meaning is, that the above-mentioned diseases are *caused* by nervous exhaustion.

A little reflection will show, however, that the assumed causal relation does not exist in the cases given; or only in an indirect manner. But this is only another instance of the too common looseness in tracing the relations between effects and their causes.

Under the heads of "General principles of treatment," and "Remedies," there are many highly suggestive and practical remarks, which, while they can hardly be condensed without impairing their value, are too lengthy to be extracted in a book notice such as this. Under the head of "General principles of treatment," he directs especial attention to two cardinal points, viz.: a loss of balance between waste and repair in the nervous system, and loss of sleep; the latter being at once a condition and a consequence of the former.

These points should be steadily kept in view, in clinical observation and experience. We are persuaded, plain and simple as they are, they do not have that stress laid on them which they demand. Under the head of *remedies*, he gives, perhaps, the first rank to electricity, more particularly, "general electrization," which Beard and Rockwell have done so much to bring into notice.

But it is not possible, in the space we have, to do more than call attention to a work which, whatever its faults may be, will be useful to the profession, in serving to call attention to an important fact in the pathology of the nervous system.

IV.—BEARD: RESPONSIBILITY OF OLD AGE.

LEGAL RESPONSIBILITY IN OLD AGE, *based on Researches into the Relation of Age to Work.* By Geo. M. Beard, A.M., M.D. Republished, with Notes and Additions, from the Transactions of the Medico-Legal Society of New York city. New York: 1874. Pages 42.

As indicated by its title, the aim in this little work is to investigate how far old age affects legal responsibility. It is one of the fruits of a protracted study of the relations of age to work in its various aspects, the mature results of which are promised in the near future.

The author himself, in the beginning, analyzes his subject under three heads, as follows:

"1. What is the average effect of old age on the mental faculties? In other words, What is the law of the relation of age to work?

"2. To what extent is the average responsibility of men impaired by the change that the mental faculties undergo in old age?

"3. How shall the effects of age on the mental faculties be best brought to the attention of our courts of law?"

Of these three propositions we will call attention only to the first two, but especially to the first, since, if the questions raised under it are answered definitely, the answers to the others may be settled with comparative ease, on principles already recognized. Because the responsibility, either legal or moral, of persons in old age, will depend on the actual "effects of old age on the mental faculties," as determined by observation. Here lies, indeed, the *nodus* of the question involved in the title of this essay.

As regards the word "responsibility," which, for the purposes of discussion, should receive definition, Dr. Beard says: "It is impossible, in the state of science that now obtains, to fix any mathematical limits for human responsibility. Responsibility is itself a vague term, and our means for determining the degree of it in any individual are more indefinite than the term itself." (Page 29.)

This remark we believe to be true, especially its latter part. The point on which the eye of the medical jurist must be steadily fixed is the "*means* for determining the degree of it (responsibility) in any individual." This done, we can define our term by declaring its contents and limits, and the way will be opened for the solution of the other questions proposed. Let us look over Dr. Beard's essay, to see what help he offers at this important point at once scientific and practical.

The paper opens with a statement as to the "method of ascertaining the law of the relation of age to work," and concludes that "the true and only way by which the subject can be approached is by studying the history of human achievements, and comparing the age at which has been done the best work in the world." Or, again, he says: "The method by which I sought to learn the law of the relation of age to work, was to study, in detail, the biographies of distinguished men and women of every age. * * * * From these data, which, though not absolutely exhaustive, are sufficiently so for a final and convincing settlement of the questions involved, I have derived the period, the decade, and the year of maximum productiveness, and the various grades between this and the period, the decade, and the years of the least productiveness."

The range of our author's investigations include the period between twenty and eighty years of age. The period most productive—the "golden decade"—is between thirty and forty years, while the least so is from seventy to eighty.

Our author then draws a distinction between "original and routine work," and states the exceptions to his laws or averages, and then shows that they apply to animals and plants as well as man. He also shows that the moral faculties are subject to the same law as the more strictly intellectual, and follows with a large number of "illustrative cases." In the next place, the author states, and endeavors to answer, certain objections to his views, and admits certain "qualifying considerations," which show he is alive to the difficulties of his theme. The discussion under the first head is terminated by paragraphs intended to show that "childhood, youth, and old age, are but imitators" of what is done, of course, in the more favored, and strictly productive, periods in life; and that "the art of writing may improve with age."

The outcome of the paper up to this point is well stated in the following language: "The significance of the law of the relation of age to work, in its bearings on legal responsibility, in cases of crime or contested wills, is that it demonstrates the *presumption* of more or less intellectual or moral decline in old age." It demonstrates, or definitely fixes, a truth that was known before; but up to this time we have not been put in possession of any new criteria for practical purposes, or, in other words, more definite "means for determining the degree of" responsibility in individual cases, which our courts of law look to the scientific medical expert to furnish.

But to proceed. Dr. Beard shows, again, there are many exceptions to the rule that, as age advances, the moral and intellectual faculties decline. Moreover, he shows "that very great mental decline, and grave cerebral disease, are yet consistent with average responsibility;" "and that a man in the decline of old age may be irresponsible in one or two directions, while perfectly responsible in others." Still farther, our author says that, "after we have exhausted all our methods of physical examination, and, perchance, have made clear our diagnosis of senile disease of the brain—sclerosis of some sort, it may be, or hæmorrhage, or congestion, or anæmia, merely—we have done a little, and but a little, towards answering this question, Whether *the man is, or is not, responsible for all his actions?*" If this is so, and we believe it is so, how shall we, in any given case, try to determine the degree of responsibility? It is not enough, so it seems, to show that the person is old, or has brain disease.

Dr. Beard simply relegates us to the old mode of proceeding in our courts, by telling us "each case must be studied by itself in the light of the law I have adduced, and the several principles that I have indicated." But even the law of Dr. Beard is not new, as a fact. His studies serve to give it prominence, and tend toward giving it a fixed value—nothing more, so far as its legal aspects are concerned; so we are still left without any better criteria than we had before for determining, in any given case, how far old age may have impaired the moral and legal responsibility through the decline purely incident to old age. We are still in the dark as to modes of distinguishing as between the degenerating effects of old age and the effects of brain diseases that may and do occur at any period in life. But we would not be understood as censuring Dr. Beard for not reaching more definite results. In the present state of our knowledge, we do not deem it possible to reach such results as will dissipate the real difficulties which beset the path of the medical expert and jurist, where the responsibility of persons is called in question on the score of old age. However barren this well written paper may be of results, in regard to the main point, it is highly suggestive in various other important relations, under which it is no part of our present purpose to examine it. We earnestly hope the accomplished author may be able to complete his studies, that are so attractively foreshadowed in this as well as other works.

V.—SHEPPARD: LECTURES ON MADNESS.

Lectures on Madness, in its Medical, Legal, and Social Aspects. By Edgar Sheppard, M.D. Pages 186. Philadelphia: Lindsay & Blakiston, 1873.

We have found it difficult to see why this little book should have been issued, especially when it was preceded by the much more full, and, in many respects, admirable lectures, of Dr. Blandford, and by the "Manual of Psychological Medicine," by Drs. Bucknill and Tuke, and the work on the Physiology and Pathology of the Mind, by Dr. Maudsley, so long and favorably known to the English medical public. To justify the appearance of a new work—say on "Madness"—it should be able to lay fair claims to either something new in matter or in method. But so far as this work is concerned, it can lay claim to neither. It is published because the author desired it should be, and because he could find a publisher.

But while we acknowledge the right of an author to publish anything he pleases, this *right* is inevitably connected with the *duty* of taking the consequences, be they good or bad.

The very title of the work we dislike: "Lectures on *Madness*." The use of the term, "madness," instead of "insanity," while it serves no good purpose, serves to convey a false notion of the author's, under the transparent guise of plainness and candor.

The work, so far as we are able to see, contains nothing of importance that is new. Yet it contains much, very much, valuable matter, found, however, at least most of it, in a more acceptable form elsewhere.

The first thing that struck us in reading the work, is the vanity or self-conceit of its author. There is a constant and conscious display of the paraphernalia of the new professor, which might be excused, possibly admired, in the class-room, but strikingly violates a healthy sense of propriety when deployed before the less indulgent eye of the profession at large. The author seems to be the incumbent of a new chair of Psychological Medicine in King's College, London. The first lecture has, as its opening sentence, the following: "Gentlemen, this is a new Chair, and I am a new Professor." The same spirit of enthusiastic self-congratulation bursts out in phrases like this: "The Visiting Committee of Colney Hatch Asylum, whose physician and servant I am." "I say it as a layman and as an alienist physician," etc. But it is to be seen less in such phrases than in the general but unmistakable spirit which pervades the book.

Then, there is unusual license taken in descriptive, and, we may say, figurative, parts of the lectures. For example: "By the noise and bustle and everlasting spin of this fast, six-mile-cannon age," "they find it hard to keep the driving-wheel of their moral locomotive on the rail." And we were about to say, in hundreds of passages like this, the same toying, or even trifling, with his subject, is manifest. Then again, there are the frequently-recurring quotations from Shakspeare and the rest, a habit among certain writers we should feel greatly relieved to see, at least partly, abandoned.

The only striking merit the book has is its brevity. We hope the time will be long before the enterprising American publishers whose imprint it bears will republish a work the faults of which are so conspicuous and whose merits are so small.

VI.—DUNGLISON: DICTIONARY OF MEDICAL SCIENCE.

A DICTIONARY OF MEDICAL SCIENCE, ETC., WITH THE ACCENTUATION AND ETYMOLOGY OF THE TERMS, AND THE FRENCH AND OTHER SYNONYMS. BY Robley Dunglison, M.D., LL.D. A new edition, enlarged and thoroughly revised, by Richard J. Dunglison, M.D. Philadelphia: Henry C. Lea, 1874. Chicago: W. B. Keen & Cooke.

This new edition of Dunglison's Medical Dictionary, which has since its first appearance been recognized as the standard work of its kind in England and America, will be welcomed by the profession. Its editor has added over six thousand new terms, and one hundred pages, to the book, bringing it up, as fully as possible, to the medical standard of the time. In its present form it is almost a Cyclopædia; the name Dictionary is scarcely applicable, as many of the subjects are treated at length, and are short articles, rather than mere definitions. Every reading physician should have this work, the best of its kind in the English language, and not surpassed in any other.

Editorial Department.

THE editors have reason to feel gratified, in view of the way their JOURNAL has been received.

For the kindly notice it has had from the medical press, and especially for the expressions of approval and encouragement we have received by letter from eminent members of the profession in different parts of the United States, we are grateful.

The enterprise involves no small expense of time and money, but both are cheerfully given, and were fully counted in the beginning. Our expectations have in no sense been disappointed, but rather exceeded.

The first number of the JOURNAL by no means filled the conception of its editors as to what it should be, on account of the peculiar circumstances under which it was issued. It should have appeared early in January, but the panic in the financial world had led the editors almost definitely to delay its publication one year. But the symptoms of returning confidence in monetary matters, after the first of the year, led us to reconsider our determination; and hence the first issue was not only behind time, but was prepared and published so hastily as to render it unsatisfactory to us. We make these explanations for obvious reasons, simply in justice to ourselves. But we point with more satisfaction to the present number of the JOURNAL, as nearer what we would have it be in matter and style.

The readers of our first number will notice, perhaps, that the type of the first department has been changed; it is larger and clearer. The paper is thicker; and this issue of the JOURNAL contains about forty more pages than did the first. We feel confident, if our readers were inclined to be satisfied with our first number, they will be more than satisfied with this.

We will take this occasion to say a final word as to the scope of the JOURNAL. The field we have especially in view is that of the nervous system, not only on its own account, but that of its relations to the body, of which it forms a part, on the one hand, and to the mind and mental action on the other. It is not our intention to produce a Journal of Insanity. We leave this more restricted and important field to other journals. In our own country it is already occupied, with marked ability, by the *American Journal of Insanity*, now so well known, not alone to the profession in the United States but also in other lands. But we do not expect to neglect mental diseases, clearly arising out of organic nervous disease.

Everything of real interest, especially if new, that will throw any light on the anatomy, physiology, pathology, or therapeutics of the nervous system, in its various relations, whether medical or legal, is to be included within the scope of our JOURNAL.

THE ALCOHOL QUESTION.—Whatever may be thought of some of the methods recently employed to further the cause of temperance in the use of alcoholic drinks, there can be no question that the time has fully come for the thorough discussion, not alone of the social evils arising out of an intemperate use of alcoholic beverages, but of their true physiological action, both in health and disease.

There can be no question but intemperance, in the sense of this term as ordinarily employed, is one of the most, if not the most, prolific sources of disease of various kinds, especially nervous and mental, whether hereditary or acquired, known among civilized men. And at present the evil seems to be on the increase.

It is peculiarly incumbent on the medical profession to discuss the questions involved, from scientific as well as practical standpoints, because no class in the community sustains such relations to this whole matter of intemperance as the medical profession. As a class, they alone have the means and opportunity of a scientific study of its real effects on the body and mind, and hence, not only of determining how much value it has as a beverage and medicinal agent, but in what way, and to what extent, it is injurious. The results of such a study are not only required to guide rational reform, whether legislative or otherwise, but to guide physicians themselves in prescribing the use of alcoholics as medicinal agents; because, however useful as a pharmaceutical or remedial agent alcohol may be, we are deeply impressed with a belief that incalculable remote, if not immediate, harm, is often done by carelessly prescribing its use in many cases; and the profession, as a whole, seems to be awakening to a sense of its responsibility in this direction. The question as to its real action and therapeutic value, is now, almost for a wonder, beginning to attract great general attention in Great Britain and on the Continent, especially in France. Symptoms of this same awakening begin to appear in our own country; but we cannot discuss any of the questions involved now.

At an early period in the progress of our Journal we propose to discuss especially those phases of the action of alcohol which relate to the nervous system and mind, whether for good or ill; and if we mistake not, future investigations will develope that a larger share of nervous and mental diseases than many have suspected must be at least attributed to the abuse of alcoholics.

Necrology.—We have to notice, in this issue of the Journal, the death of the distinguished English alienist physician, Dr. Forbes Winslow, which occurred at Brighton, England, on the third of March last. The cause of his death was granular disease of the kidneys, the symptoms of which developed very suddenly and carried him off, after altogether less than a month's sickness.

Dr. Winslow is known to our readers by his numerous writings on subjects connected with psychological medicine, the department which he specially cultivated, and in which he attained his eminence. His best known work, "On Obscure Diseases of the Brain, and Disorders of the Mind," London, 1860, passed through four editions in the space of eight years. In 1848 he founded the *Journal of Psychological Medicine*, a quarterly journal, and for sixteen years was its sole editor and proprietor. He was the first English physician who introduced the plea of insanity in criminal cases, and was summoned as an important witness in a number of noted criminal trials, where his opinion carried much weight.

It is, perhaps, of interest to us as Americans, to note the fact that Dr. Winslow was a lineal descendant of Edward Winslow, one of the pilgrim fathers of the Mayflower, and one of the early governors of Plymouth Colony.

Professor Jean Cruveilhier, the eminent French anatomist and pathologist, died March 11th, at Limoges, France, aged eighty-three years. Since 1865 he had been retired from active work and spent his time mainly in works of charity and benevolence. It is unnecessary here to enumerate his numerous works; they are among the classics of the profession; and the title given him by Virchow, "the father and patriarch of pathological anatomy," best indicates his merits, and the estimate which posterity will place upon him.

Prof. Max Schultze, the distinguished microscopist and histologist, died very suddenly, at Bonn, Germany, on the 16th of January last. As a discoverer and investigator he stood in the foremost rank, and his death, at this time, is a serious loss to science.

Dr. Richard Hertwig, of Bonn, has undertaken the completion of the present volume of the *Archiv fur Mikroskopische Anatomie*, the forthcoming number of which will contain a portrait of its late editor and founder, with a biographical notice from the pen of Prof. Schwalbe, of Jena. We hope to be advised of its further continuance.

Psychische Studien.—We have received the first number of a new monthly journal, bearing the above name, published in Leipsic, and devoted, as its title-page indicates, "to the investigation of the less known phenomena of the '*seelenlebens*,' the psychic life." Its editor and publisher is Alexander Aksakow, Imperial Counsellor of St. Petersburgh. The peculiar field which it proposes to occupy is that of modern spiritualism, in its recent developments, as championed in Great Britain by Wallace and others, and in this country by numerous writers, none, however, at the present day, of any particular scientific reputation. The contents of this first number are, translations of the Report of the Committee of the Dialectical Society on Spiritualism; Crookes on Spiritualism, in the light of Modern Science; Alfred Russell Wallace on Mesmerism, Clairvoyance, and Spiritualism; and an article by Prof. Butlerow, of the University of St. Petersburgh, on the Reality of Mediumistic Phenomena, which, with reviews and notices, fill the number.

We have, also, received the prospectus of another publication, allied in its character to the one above mentioned, the *Revue de Psychologie Experimentale*, published by T. Puel, Doctor of Medicine, Laureate of the Imperial Academy of Sciences, etc.

The issue of these two publications at the same time, and the scientific standing of some of its adherents abroad, gives evidence that spiritualism, which, in this country, has been relegated almost entirely to the itinerant lecturer and the Woodhull faction of social extremists, has, at least in some of its phases, obtained a firmer footing in Europe. It may, perhaps, be the reaction from a too harshly materialistic scientific method, carrying a class of thinkers even to the opposite extreme; or it may be only one of the manifold developments of modern mental activity. At all events, it is a phenomenon worthy of study, not exactly in the manner in which these publications propose to treat it, but as a psychological manifestation of a certain sort.

The Neurological Society, of New York, re-organized April 6th, with about fifty members, and the following officers: President, W. A. Hammond; Vice-Presidents, J. C. Peters, and J. Marion Sims; Recording Secretary, Geo. W. Wells;

Corresponding Secretary, Max Hertzog; Treasurer, Alexander Murray. This makes two active societies in New York city devoted to the advance of neurological medicine.

As a further indication of increasing activity in this department of medical science, we can state we are informed that it is the intention of Dr. Hammond to revive the *Psychological Journal* at an early date, and to continue its publication, as formerly, in the city of New York.

ERRATUM.—In the hurry of going to press, some errors in the first two or three signatures of this number escaped correction. Among them is one we wish to rectify here: in the note on page 130, the dates should both be 1866, instead of 1863 and 1864.

Periscope.

a.—ANATOMY AND PHYSIOLOGY OF THE NERVOUS SYSTEM.

FUNCTION OF THE BRAIN.—Dr. H. Nothnagel (*Virchow's Archiv*, LVIII., 420, Nov., 1873) continues the publication of his researches on the functions of the brain. A number of experiments are detailed, and some very peculiar phenomena described. He found that by the simple puncture, with a fine needle, in a circumscribed region of the posterior portion of the brain, on either the right or the left side, he produced the most violent springing movements of the rabbit, which generally either entirely ceased in two or three minutes, or left merely a slight medianwards deviation of the limbs and curvature of the spinal column. In some sixty different experiments, positive phenomena of this kind were produced in twenty-three; that is, the circumscribed portion of the brain, the wounding of which caused these remarkable movements, was punctured. No disturbance of sensibility of any part of the body was detected in these experiments. The region of the brain which, by puncture, caused these phenomena, is on the inner side of the posterior portion of either hemisphere, over the anterior part of the corpus quadrigeminum, and most commonly in a white, fibrous mass, surrounded by gray matter, which must be penetrated to the depth of one-half a millimetre by the needle in order to produce the effects described. The author does not attempt to explain these appearances, but merely gives the description.

Altogether negative results followed similar experiments on the cornu ammonis.

Experiments were also made on the optic thalami. Slight wounding of the upper surface with the needle produced no very marked results. Deeper puncture, in the posterior portion, caused deviation of the head and anterior limbs; and a horizontal cut brought about the phenomena already described by Schiff, in his *Lehrbuch der Muskel und Nervenphysiologie*, with some few additional peculiarities, which the author mentions; such as a momentary movement of the animal toward the wounded side, the hinder extremities remaining undisturbed, etc. As regards disturbances of sensibility, he thinks that he sometimes detected a slight hyperalgesia, more particularly on the injured side; but it was not very decided.

Dr. Nothnagel concludes his paper with a sharp criticism and analysis of some of the experiments of Fournie, which were noticed in the review department of the January number of this journal. He, himself, abstains from offering any general conclusions, deeming the subject as yet too little known, and that hasty generalizations can only have the result to bring the facts already won by careful experiment into discredit.

A communication from Dr. Nothnagel also appears in the *Centralblatt der Med. Wissenschaften*, No. 56, Dec. 13, 1873, in which he states that in the rabbit precisely similar effects are produced by the simultaneous extirpation of both nuclei lenticulares as by the removal of the two cerebral hemispheres. All voluntary motion ceases, though reflex movements continue to be produced.

M. Carville presented, on behalf of himself and M. Duret, at the *seance* of the *Soc. de Biologie*, Jan. 3 (report in *Gaz. Med. de Paris*, Jan. 24), a communication relative to the excitability of the cerebral hemispheres by faradic currents, in which he criticised the recent investigations and conclusions of Dr. Ferrier, in regard to the location of the functions of the brain. The aim of these authors is to show that the phenomena described by Ferrier and others are not due to localized excitation of cortical centres, but to transmitted irritation of the cerebral ganglia and peduncles. They repeated the experiments of Dr. Ferrier, and then, afterwards, endeavored to obtain the same results when the animal was completely under the influence of anæsthetics, but found them altogether different. The following are the principal points of his communication:

1. When the anæsthesia was imperfect, it was possible, by applying the electrodes successively to the surface of the convolutions, to reproduce some of the movements described by Ferrier in his memoir.

For example, the excitation of the anterior portion of the external superior convolution, caused the raising of the fore paw, the flexion of the toes, the elevation of the shoulder, etc., of the opposite side; while, on the other hand, the excitation of the second and third external convolutions produced a rotation of the head toward the opposite side, etc., etc.

Some of these movements the authors attribute to conduction of the electric excitation to the muscles by the liquids which bathe the surface; others by the direct diffusion of the excitation through the substance of the cerebrum; and test experiments appeared to demonstrate the correctness of both of these suppositions.

2. When anæsthesia was complete, no effects were obtained by the excitation of the convolutions with any intensity of the current. Neither the centres described by Ferrier as regulators of the associated movements of the fore paw, nor those of the posterior paw, or the lips, or the eyelids, were manifested by electric irritation.

The authors ask, Why do anæsthetics prevent the movements caused by exciting the hemispheres? and review the hypotheses which present themselves to explain the fact. The first of these—that the anæsthetic produces such a change in the cortex as to prevent the electricity from acting on the muscular centres—is disposed of in a few words as inadmissible. The anæsthetic has no power to prevent the diffusion of the current; and the theory as to alterations of the cells is equally untenable.

The second hypothesis is that proposed by Schiff and Dupuy, that these movements, thus produced, are of a reflex nature; and that the anæsthesia hinders or prevents these. MM. Carville and Duret do not admit this theory, as the point of departure for a reflex movement is wanting, the convolutions not being excitable.

The third hypothesis is the one adopted, and is simply that the currents applied to the surface only act by exciting the corpus striatum and the peduncles; or, in short, those portions of the cerebrum which are known to be excitable. Anæsthetics affect the medulla, and through it gradually suppress the excitability of the various organs. When, therefore, the animal experimented upon is under their influence, the diminished excitability requires stronger stimulus; until, when the loss of sensibility is complete, the reaction ceases altogether.

M. Carville sums up his opinions in the following conclusions:

The peripheral layer of the hemisphere is unexcitable, insensible, and contains no motor centres.

The effects obtained by faradization, which penetrates to the corpora striata and peduncles, are due to the direct excitation of those organs.

These effects cannot be attributed to any reflex action.

Complete anæsthesia, which hinders these effects, does not at all change the conditions of the peripheral stratum of the hemispheres, but acts merely by more or less diminishing the excitability of those parts of the hemisphere which are universally recognized as excitable.

The following are the conclusions of a note presented to the *Soc. de Biology*, Jan. 3, by M. Dupuy:

1. The conclusions of M. Ferrier's memoir cannot be accepted, because that observer has never troubled himself to find out whether the electric currents (faradic, as he terms them), which he used in his experiments, diffuse themselves, or not, through the encephalon. By the aid of a galvanoscopic frog, I have been able to prove that, in fact, they are so diffused; and that consequently we cannot claim, any more than before, that the cortical layer of the brain is excitable by electricity.

2. We cannot localize the nutritive or functional centre of any nervous conductor in a particular cerebral convolution, as Ferrier proposes, because, when the animal operated on is completely anæsthetized, electrical irritation of the cortex causes no contractions; while in the same animal, and with the same electrical current, the direct irritation of the sciatic nerve, previously laid bare, causes a contraction of the muscle to which it is distributed.

Dr. Edward Hitzig publishes, in the *Berliner Klin. Wochenschrift*, No. 6, Feb. 6, an account of the electrical exploration of the brain of an ape (*Innuus rhesus*). Both the constant and the induction currents were employed, and the following general results claimed: The anterior central convolution is the general centre for the muscles of the body; and the special centres for the different sets of muscles were found distributed in it as follows: Close to the great longitudinal fissure, separated from it about three millimetres, was the centre for the hinder extremities. That for the anterior limbs was found about three millimetres more to one side; and still seven millimetres farther in the same direction was the centre for the parts innervated by the facial nerve. Finally, close to the sylvian fissure, he found the centre for the muscles of the tongue, mouth, and jaws. This last is interesting, from the fact that lesions in this same neighborhood are so frequently connected with aphasia in the human subject.

A comparison is made of the anatomical relations in this case and those of the dogs' brains experimented upon by the author, and the homologies of the convolutions pointed out.

We extract the following from the Cincinnati *Enquirer* of March 24th. Although not coming to us directly from a medical source, the main facts are well accredited; and we are informed that a full account of the experiments will appear in the *American Journal of Medical Sciences* for April.

"An *Enquirer* reporter went to the Academy of Medicine last night to hear the report of the Committee on Ethics on the case of Dr. Maley, in causing the arrest of a physician in an alleged abortion case. The Committee was not ready to report, and the Academy adjourned, but was, upon the arrival of Dr. Roberts Bartholow, one of its members, called together to listen to that gentleman explain the recent wonderful experiments made by him upon the brain of a patient at the Good Samaritan Hospital with the galvanic battery. The patient was a woman inflicted with an incurable disease, and almost in *articulo mortis* during the three weeks that the experiments continued. An accident of her childhood, that of falling into the fire and burning the back part of the head, so that instead of a scalp only a cicatrix formed over the brain, had brought her to the hospital. This cicatrix became the seat of a cancerous disease, which ate away the skull and left the brain exposed, making her a fit subject for experiment.

"Dr. Bartholow had the brain of the subject hardened by chromic acid for exhibition ; also, photographs of the face of the patient and of the exposed portion of the brain experimented upon. In the first place, he ran over the history of experiments upon the brain of living animals.

"In the case of the Good Samaritan Hospital, the patient was a woman about thirty-two years old. A large part of both hemispheres of the brain was exposed by the eating away of the bones by a long-standing cancer. The Doctor said that, as the brain had frequently been penetrated by the surgeon's knife, and portions lost by accident, without obvious detriment to the patient, he assumed that he might, with the same impunity, introduce extremely fine needles for electrical experiments. This he did. These needles were insulated to near the point, so that the electrical current, when applied, could be confined to a small extent of brain substance. These needles, during the three weeks of experimenting, were introduced about ten times in each hemisphere, to the depth of from a twelfth part of an inch to an inch and a half, and at distances apart varying from a quarter of an inch to an inch. What he termed a Faradic current, which we suppose scientific readers will understand, was applied—the very weakest that could be used.

"The first point demonstrated was the insensibility of the dura mater, and of the brain substance itself. The patient was absolutely unconscious of the presence of the needle when introduced, though in the entire possession of all her faculties.

"The presence of the needle of itself, when it penetrated to a certain depth, was felt by the patient, not where the needle was, but in the extremities on the side of the body opposite to that of the hemisphere of the brain penetrated, chiefly in the hand and arm. For instance, when the needle penetrated the left hemisphere of the brain, pain and tingling were felt in the right arm and hand; and when it penetrated the right hemisphere of the brain, a like pain and tingling were felt in the left arm and hand.

"When the needles were connected with the galvanic battery, distinct muscular movements occurred on the side opposite to the hemisphere of the brain in which the needles were. The pain experienced in the extremities was also much more decided.

"Another phenomenon was what the Doctor termed the 'choreic movements,' which, being explained, are a nodding of the head and moving of the hands up and down. These were on the side of the body opposite to that of the brain in which the experiment was going on. A stronger electrical current applied to the right hemisphere, caused, the Doctor said, 'an unilateral epilepsy of the opposite side, of great violence.'

"Dr. Bartholow stated that upon a *post-mortem* examination he had found that the traces of the paths of the needle were distinctly visible in the brain substance, and that brain substance had been destroyed to make a path for the needle. After he had concluded his very brief but lucid statement, the matter was opened for remark and discussion.

"Dr. Kearney said he would like to hear opinions as to how far such experiments should be permitted, in view of their possible effect of causing the death or shortening the life of the patient.

"Dr. Bartholow said the insertion of the needles caused injury. He was emboldened to make the experiment by the fact that surgeons had penetrated the brain with the knife to let out ulcerous matter, and that portions of the brain had been removed by accidents, without being followed by loss of life or serious injury. Since, in strangulated hernia, puncturing by fine needles was employed with success, and also in many cases the bladder was punctured with fine needles, he felt that the experiment might be safely made upon the brain, especially in a case like this, which was necessarily hopeless.

"Dr. Kearney said that the using of the knife to penetrate the brain, by Dr. Detmold, was a different case; then it was necessary to save the patient's life. He wanted to know whether in an inevitably hopeless case a doctor was justified in making an experiment that might shorten the patient's life?

"Dr. Ludlow wanted to know if in this case the acceleration of the disease by the experiment did not probably hasten the patient's death?

"Dr. Bartholow said that, in regard to the question of humanity, he felt, before the experiment, that fine needles might be used with impunity. Now, with the knowledge of that experiment and the *post-mortem*, that the needles did inflict injury, he would never again repeat that experiment.

"Dr. Carson had some doubts whether there was not a complication of conditions that might vitiate the conclusions.

"Dr. Muscroft was glad the experiment had been made, and he did not see why such experiments should not be made in the future, where opportunity offered, in the cases in which death was inevitable, and the patient consented to have the experiment made."

[We know of no *a priori* reasons why, in a case like this, the experiments should have any influence whatever on the prognosis, and have little doubt but that the patient lived as long under these circumstances as under any other conditions. Although altogether exceptional among recent physiological investigations, the experiments are of the highest importance, and we shall look for the complete report of them with great interest.—Eds.]

The Function of the Semicircular Canals.—Prof. E. Cyon, in a paper published in *Pfluegers Archiv*, 1873, 306, in which he gives, at length, descriptions, etc., of experiments performed on animals by himself and others, concludes that the function of the semicircular canals is to assist in maintaining the equilibrium "by a series of unconscious (auditory?) sensations," informing the animal of the position of its head in space; and that each semicircular canal has a sufficiently defined connection with one of the dimensions of space.

The disturbances of movements which are produced by the section of these canals, he divides into three classes: 1st, Those due to disturbances of equilibrium, directly following the injury; 2d, Forced movements, caused by abnormal auditory sensations; and, 3d, Subsequent phenomena, appearing some days after the operation, and due to the consequent inflammation of the cerebellum.

Reflex Innervation of the Vessels.—E. Pick (*Reichert u. DuBois Reymond's Archiv*, 1873, 1) publishes an account of his observations on the reflex innervation of the smaller vessels of the natatory membrane of frogs. The animals were prepared by being put moderately under the influence of curare; and the membrane of the second digital interspace was selected for the observations. The irritation was applied by means of the induction stream, or by dilute acetic acid. The smaller the vessels, and the stronger the irritation, the quicker and more pronounced was the corresponding reflex contraction. In the larger vessels, a dilatation followed the contraction, which, as the arteries thus dilated responded but slowly, and to a slight degree, to subsequent excitation, the author considers as an evidence of fatigue. The irritation of different regions of the cutaneous surface acted differently in producing these reflex effects. The skin of the face was the least sensitive, and that of the back the most so. The route of the vasomotor nervous influence was exclusively in the sciatic nerve.

Connective Tissue of Nervous Organs.—M. Louis Ranvier, in a note communicated to the Acad. des Sciences by M. Cl. Bernard, Dec. 1, 1873 (*Gaz. Med. de Paris*, Jan. 3), gives it as his conclusion that the cells of Deiters, described by that author, Golgi, Boll, and others, in the nervous centres, have no real existence. He says if these cells do exist, as described, and form the fibrillary stroma of the nervous centres, there is then an important morphological difference between the conjunctive tissue of the central nervous system and that of other organs. He is convinced that this difference does not, in fact, exist; and that the conjunctive tissue of the spinal cord, and that of the peripheral nerves, for example, are constructed on the same type. Other competent persons, who have examined his preparations, are in accord with him on this point. He concludes by saying: "*En resume*, the conjunctive tissue of the spinal cord is composed of connective fibres, in bundles and flat cells. It appears with the same characters in all the organs I have so far studied, and particularly in the peripheral nervous cords; but in the nervous centres, the arrangement of the fibres and cells is such that the figures thus formed have deceived histologists into believing them to be ramified cellules."

THE STRUCTURE OF THE TACTILE CORPUSCLES.—Geo. Thin, M.D. (*Jour. of Anat. and Phys.*), gives the results of his microscopic investigations on the structure of the tactile corpuscles of Wagner, carried on by him in Prof. Stricker's laboratory in Vienna. He found, by means of osmic acid preparations, that these corpuscles were both simple and compound; that is, one or more separate corpuscles may be inclosed in a common capsule. The nerve fibres were carefully followed after they entered the capsule; and the conclusion arrived at is that "each single corpuscle and each member of a compound corpuscle, represents the termination of a single medullated nerve fibre."

The transverse elements are declared to be the nuclei of oblong cells, which anastomose with each other by means of prolongations of elastic tissue fibres.

The division of the papillæ of the skin into vascular and nervous, is not borne out by these investigations. A majority of the papillæ which contain touch-corpuscles, contain vessels also; and nerve fibres are found in a proportion of those which contain vessels and no touch-corpuscles.

INHIBITION OF REFLEX ACTION BY HEATED BLOOD.—M. Foster (*Jour. of Anat. and Phys.*, Nov., 1873) discusses the phenomena, first observed by Goltz, that a frog deprived of its cerebrum, when placed in water, the temperature of which is gradually raised, dies without struggles; while, in the uninjured animal, the reverse is the case. From his investigations, he concludes that the absence of reflex action, in Goltz's experiment, and the other modifications of it, are due primarily and chiefly to the depressing influence of heated blood, carried from the skin to the spinal cord, which comes into play by virtue of the gradual character of the stimulation. When the stimulation is sudden, violent movements are produced; but in the other case, the sensibility of the cord is already blunted before the temperature has become sufficiently high to actively stimulate the peripheral sensory nerves; and when this point is reached, the spinal cord is so deadened as to require a still stronger impulse for the production of reflex movements; and so on, stage by stage, till the animal is fairly boiled without having made a sign.

The reason why an uninjured frog does not act in the same manner, is given, that a much less intense sensory impulse is required to call forth a movement of volition than a simple reflex action; that is, one produced by the spinal cord alone, without the intervention of the brain.

SYMPATHETIC.—G. Gianuzzi (*Ricerche eseguite nel gabinetto di Fisiologia della R. Universita di Siena;* abstr. in *Centralbl. der Med. Wissensch.*) has tested the relations existing between the spinal cord and the system of the grand sympathetic, by the Wallerian method. He opened the spinal canal of dogs, and cut the nerve-roots on either side of the spinal ganglia, and then, after a time, examined the rami communicantes of the sympathetic. The results are summed up as follows:

1. The rami communicantes, in mammals, consist almost entirely in fibres, which have their centres of nutrition in the cord and spinal ganglia; still, there are a few fibres (altogether lacking in some communicating branches) which are dependent, in this respect, on the sympathetic ganglia.

2. These fibres, which are always distinguishable through their fineness, and generally, also, by their lack of double contour, go from the sympathetic ganglia to the spinal roots, through the communicating branches.

3. The number of the motor fibres sent from the spinal cord to the sympathetic is variable, but is always greater than the number of sensible fibres. The number of these last varies very much in the different rami communicantes.

4. The "actio nutritiva" of the cord and spinal ganglia extends its influence on the nerve fibres, after they have penetrated the ganglia of the sympathetic.

The same animals which had served for these researches were also experimented upon to test the irritability of the posterior columns. These, after the section and degeneration of their sensible roots, gave a positive reaction, and are, therefore, to be considered as irritable.

Microscopic examination showed that the degeneration of the sensory roots frequently extended into the substance of the cord.

C. Eckhard (*Centralblatt der Med. Wissensch.*, No. 35, 1873) contradicts the statement of Sinitzin (*Cbl.*, 1871, 361), that the extirpation of the superior cervical ganglion of the sympathetic prevents the occurrence of the usual alterations of nutrition, etc., in the eyes and lips, following the section of the trigeminus; or that, if these are already exhibited, after a previous section of this nerve, it renders them less pronounced. His experiments, made with great care, appeared to show that when the section of the trigeminus is complete, that of the sympathetic has no influence on the consequent alterations.

RESEARCHES ON THE CHORDA TYMPANI.—M. J. L. Prevost (*Comptes Rendus*, Dec. 30, 1872), and M. A. Vulpian (*Comptes Rendus*, Jan. 3 and March 10, 1873; abstracts in *Revue des Sci. Medicales*)—M. Prevost discovered that, after destruction of the chorda tympani, either by section of the facial, or in the ear, there always followed, in nine or ten days' time, degeneration of the terminal branches of the lingual nerve, as far as the fibres of the deep submucous layer. After the destruction of the spheno-palatine ganglion, or section of the Vidian nerve, the chorda tympani remained uninjured.

In 1863, MM. Vulpian and Philipeaux showed that in the dog, after section of the hypoglossal nerve, the lingual nerve of the same side acquired motor powers not previously belonging to it. According to recent researches of M. Vulpian, it is the chorda tympani, and not the lingual, that presents this singular physiological modification; *i. e.*, the nervous fibres which join the lingual from the chorda tympani acquire this property, and not those of the lingual itself. It yet remains to be seen why these fibres do not exhibit this peculiarity until some time after the section of the hypoglossal.

M. Vulpian also sought to determine the vaso-motor supply of the tongue and sub-maxillary gland; and, from his experiments, concluded that the dilator nerves also are solely derived from the chorda tympani. He electrized this nerve, with the lingual intact, and with it divided; and in both cases produced the symptoms of hyperæmia of the tongue. Then, after cutting the chorda tympani and awaiting its degeneration, he electrized the lingual without producing any signs of increased vascularity whatever. With regard to the constrictor fibres for the vessels, his experiments seemed to prove that they are mainly derived from the mixed lingual, though, in the dog and rabbit, the hypoglossal seemed to contain a few. The dilating fibres predominated in the lingual; for, on its section, and the electrization of its cut peripheral portion, the permanent redness of the tongue was notably augmented.

It can now be easily seen how facial paralysis may disturb the sense of taste; for the function of the lingual mucous membrane must be more or less affected by its vascularity.

M. J. L. Prevost (*Arch. de Phys.*, July, 1873) concludes his article on the gustative functions of the chorda tympani,. narrating and discussing several additional experiments on cats, rats, and guinea-pigs. The following are given as the complete *resume* and conclusions:

1. The section of the two spheno-palatine ganglia does not induce, in the dog and cat, sensible modifications relative to the gustatory functions of those parts of the tongue supplied by the lingual nerve.

2. After the section of the two chordæ tympani, made in dogs and cats deprived of the glosso-pharyngeal nerves, the taste was slightly modified in certain cases, notably diminished in others, and altogether abolished in one. Our results do not permit us to specify the part which the chorda tympani takes in the function of taste. Meanwhile, we are inclined to accord to it only an accessory *role.*

3. Contrary to the former observations of Prof. Vulpian, and conformably to his more recent researches, we have found that the chorda tympani sends fibres to the terminal branches of the lingual, as also to the sub-maxillary gland. We have found, after the section of the chorda tympani in the cat, the dog, the rat, the rabbit, and the guinea-pig, degenerated nervous fibres, either in the terminal branches of the lingual, the mucous layers of the tongue, or in the sub-maxillary gland.

4. The chorda tympani has no trophic centre in the papillæ of the tongue; and if the sub-maxillary gland acts on it as a trophic centre, its influence must, at least, be only limited. After the section of the chorda tympani in the ear, the central end of this nerve, on the side of its facial emergence, remains sound.

M. Vulpian publishes (*Archives de Physiologie*, 1873, 598) new researches on the motor supply of the tongue. In a number of dogs, he united the peripheral portion of the severed hypoglossal to the central end of the lingual, after it had been divided. In three of the animals, which survived the operation long enough to allow the regeneration of the fibres along the course of the (so to speak) compound nerve, a period of several months, he cut the chorda tympani in the ear, and then submitted the central end of the lingual to galvanic irritation. In one case, where subsequent microscopic examination showed that the chorda tympani had been only partially severed, strong contractions of the corresponding half of the tongue followed the application of the stimulus. In the others, on the contrary, this branch being completely divided, not the least contraction was observed. The continuity of the motor fibres derived from the facial nerve, through the chorda tympani, was necessary for contraction to ensue.

These results seem to bear against the author's former hypothesis in general physiology, drawn from experiments by himself and M. Philipeaux, that the nerve fibres conduct sensory or motor impulses indifferently, as they are in connection with the central or peripheral organs of either function.

M. Vulpian, in a communication to the Academie des Sciences, Feb. 2, 1874, repeats the statement that the contractions failed in galvanization of the lingual, after the complete section of the chorda tympani, and admits that his former hypothesis cannot be sustained.

Development of Nerve Cells. — Dr. A. Lubimoff (*Centralbl. f. d. Med. Wissensch.*, 1873, No. 41) gives the results of investigations on the embryonal development of nerve cells, undertaken by him in the course of a study of the pathological processes in the sympathetic system. His examinations were chiefly made on human embryos, ranging from the middle of the third month to birth. He ranges the order of development of the ganglion cells of the different parts of the nervous system as follows, in the descending order:

1. The sympathetic nerve cells of the Gasserian ganglion; the ganglion trunci inferins nervi vagi; and the intervertebral ganglia.
2. The sympathetic nerve cells of superior cervical ganglion; the thoracic ganglia of the sympathetic cords; and the cœliac ganglion.
3. The nerve cells of the spinal cord.
4. The nerve cells of the cerebrum and cerebellum.

According to these conclusions, the cells of the sympathetic are developed earlier than those of the cerebro-spinal centres; and in the latter those of the spinal cord (especially those of the anterior cornua, according to one author), than of the brain.

To an abstract of this paper, in the *Psychiatrisches Centralblatt*, Dec., 1873, Dr. Obersteiner adds, in reference to the fourth of these conclusions, that the cells of the cerebellum, at least, the large Purkinje cells, appear to be developed at an earlier period than those of the cortex cerebri; while in the cerebellum itself, again, the cells of the nucleus dentatus reach a very high degree of development at a relatively very early period.—*Beitr. z. Kenntniss v. fein. Bau der Kleinhirnrinde. Sitzungber. d. k. Akad. der Wissensch.*, IX. Bd., 1869.

Chemical Reaction of Nervous Organs.—R. Gscheidlen (*Pflueger's Archiv.*, VIII., 1873, 171) gives an account of experiments as to the chemical reaction of the central nervous organs. He employed for this purpose gypsum or clay plates, prepared with litmus.

The gray substance of the brain gave always an acid reaction; the white a neutral or weak alkaline one. The results were identical, no matter whether the animal had been killed or was yet alive, as was the case in some of his experiments. The gray and white matter of the cord, and the ganglia and nerve fibres reacted in the same manner; but heat changed the reaction, in the white substance, to acid, in all parts of the nerve centres.

By analysis, measurable quantities of lactate of lime were obtained from the gray substance of the brain of animals, and traces of the same in the medullary substance.

Reaction of the Sensory Nerves.—A. Heinzmann (*Pflueger's Archiv.*, 1873, 222) discusses the question as to whether the sensible nerves react to very gradual alterations in the strength of the irritation. From his experiments with thermal irritants, it would seem that the sensible nerves, like the motor, fail to react under very gradual alterations of the exciting force.

b.—PATHOLOGY OF THE NERVOUS SYSTEM AND MIND, AND PATHOLOGICAL ANATOMY.

Paralysis of Tuberculous Meningitis.—The *Gaz. des Hopitaux*, Jan. 15, 1873, contains the following conclusions of a work on the paralysis of tuberculous meningitis, by Dr. H. Rendu. Paris: A. Delahaye.

1. The paralyses which occur in the course of tuberculous meningitis are nearly always among the later accidents of the second period, and frequently the ultimate complications. In a clinical point of view, they may be divided into transient and permanent paralyses.

2. The transient paralyses are nearly always preceded by violent convulsions. In their locality, progress and duration they are governed by no fixed rule.

3. The permanent paralyses, on the other hand, whether complete or incomplete (the latter are the most frequent), are sometimes preceded by slight convulsive twitchings, generally of no great violence, and sometimes supervene gradually in the midst of a progressive coma. They occur usually on one side of the body; they may be either general or partial, invading at the same time the members and several cranial nerves. There frequently exist relations of succession and coincidence between these paralyses and other troubles of motility, such as convulsions and contractions.

4. The sensibility is nearly always simultaneously affected—rarely under

the form of hyperæsthesia—ordinarily as anæsthesia, more or less pronounced. The alterations of sensibility, however, do not exactly correspond with those of movement; and the reflex sensibility is very little modified.

5. The *post-mortem* examination only shows constant lesions where the paralysis was of the most permanent form; the transient and the ultimate paralyses are not characterized by any particular alteration.

6. Neither the presence of disseminated granulations in the meninges, nor the existence of a liquid effusion in the ventricles, are sufficient to produce the paralysis.

7. There is, on the contrary, a constant relation between the paralysis and the amount of the exudation which takes place at the base of the brain, especially towards the origin of the fissures of Sylvius; but this alone cannot directly provoke it.

8. There is nearly always to be found, in the nervous centres of paralytic subjects, either *foyers* of softening, or of capillary apoplexy, or cerebral tubercles. These lesions are located indifferently in all parts of the brain, but are particularly grouped at the level of the corpora striata, the optic thalami, and the cerebral peduncles.

9. The *foyers* of softening are themselves the consequence of the obliteration of the arteries by fibrinous exudation. They present rather the characters of necrobiosis than of encephalitis; they seem therefore to be allied to the patches of white softening, consecutive to arterial atheroma.

Renal Lesions following Cerebral Hæmorrhages.—Dr. A. Ollivier (*Archiv Generales de Med.*, Feb., 1874) publishes a series of clinical observations and physiological experiments in regard to the dependence of renal congestions and apoplexies on cerebral hæmorrhages. In his experiments, which were made on rabbits, he endeavored to reproduce the conditions which he found in his observations of his human patients; they are therefore the complement to the latter. He was able, by lacerating the cerebral substance in one hemisphere, to produce congestion and albuminuria in the kidney of the corresponding side; and by causing a meningeal hæmorrhage of the superior longitudinal sinus, to establish a bilateral congestion with albuminuria. By making a puncture in the lateral half of the floor of the fourth ventricle, either an unilateral congestion was produced or a bilateral one, but most pronounced in the kidney of the side corresponding to the cerebral lesion.

Dr. Ollivier rapidly reviews his clinical observations as follows:

"The hæmorrhage, with only a single exception, had always for its point of departure the left hemisphere, and generally the parts adjoining the fissure of Sylvius. It was constantly accompanied by a slight subarachnoid sanguine effusion over the surface of the neighboring convolutions.

"In all the cases, the corpus opto-striata was either destroyed or almost separated from the encephalon by the clot. The clot was always of considerable size, its anterior border either extended to the peduncles or the superior part of the protuberance, or with a circumscribed clot in the corpus opto-striata there existed disseminated *foyers* in the peduncles, or the protuberance. There was found, moreover, in all cases, either a laceration of the septum lucidum, a distention or laceration of the ventricular walls, or the aqueduct of Sylvius was found filled with blood.

"It will be observed therefore, from these facts, that the albuminuria may follow not only hæmorrhage in the protuberance, but also that in other parts of the encephalon.

"Albuminuria of cerebral origin is much more frequent than is at present believed; and I do not doubt that new researches will reveal many examples. In the present actual state of our knowledge it is not yet possible to fix with precision, from the symptoms, the location of a cerebral hæmorrhage. Nevertheless, in the cases where the signs of a lesion of the protuberance are wanting, we may say that the presence of albumen in the urine indicates, perhaps, a *foyer* situated at the base of the brain, or perhaps an extensive hæmorrhage compressing the base. In all cases it seems to be, from the facts I relate, a prognostic sign of very grave significance."

Morbid Changes of the Cord.—M. G. Hayem, in a note presented at the session of the French Academy, Jan. 26, by M. Cl. Bernard (reported in *Gaz. Med. de Paris*, Feb. 7), thus sums up the conclusions drawn from his experiments on the changes of the spinal cord in rabbits, produced by rupture, or resection of the sciatic nerve.

1. The rupture of the sciatic nerve in the rabbit is followed by a cicatricial myelitis, which may be the point of departure for a kind of general central myelitis.

2. The principal character of this alteration of the gray substance of the cord consists in an atrophic degeneration of the nervous cells.

3. This kind of myelitis, which appears to be the rule when the animal is allowed to survive after rupture of the nerve, may likewise follow in a case of simple resection.

These experimental facts may be applied to human pathology, and important conclusions drawn from them. I can only indicate here the principal ones:

1. The propagation to all the gray matter of an irritation affecting, primarily, only a limited portion of the cord (rupture of the nerve), enables us to understand the numerous clinical observations in which a wound, or a contusion of the cord, or any limited lesion whatever, has been the point of departure of an acute subacute, or chronic central myelitis. [Some of my animals experimented upon died at the end of five or six days, with perhaps a generalized central myelitis. Unfortunately they were not examined with this point in view; but a cat in which I ruptured a cervical nerve, died in a few days of an acute myelitis.]

2. The possibility of causing at will, so to speak, a central myelitis in animals, shows that this kind of alteration, characterized especially by a more or less rapid atrophy of the nervous cells, is truly of an irritative nature; and that there really exists, besides the interstitial forms, a parenchymatous myelitis, which, in the gray substance, attacks in a special manner the nervous element itself. One may therefore study at leisure the degener-

ation and atrophy of nervous cells. [I may here say that, in my experiments, I did not obtain the pigmentary degeneration of cells, which, according to the published observations, particularly those of Lockhart Clark, and M. Charcot, appears to be the most frequent alteration in man.]

3. From these experiments we see that the central myelitis has an invincible tendency to generalize itself, that from a single point of the gray matter it extends to the corresponding point on the opposite side, and through the whole cord as far as the nuclei in the medulla oblongata. These peculiarities are in accord with the invading, progressive march of central diseases of the cord, such as is seen in clinical observations.

4. The rapid atrophy of the muscles observed in animals suffering from lesions of the cord, shows, in a manner altogether new, the trophic influence of the cells of the spinal cord. These facts also appear to me to be of very great importance in the point of view of the atrophic paralysis of infancy and age, and of progressive muscular atrophy.

5. The experiment relative to the resection of the sciatic nerve, by establishing the fact that a traumatic irritation of a nerve may be propagated into the gray matter of the cord, producing a parenchymatous myelitis with cellular atrophy, explains very clearly the observation which M. Dumenil has published under the name of "ascending neuritis," and in which that distinguished observer has erroneously inferred, in order to explain a progressive muscular atrophy consecutive to contusion of the sciatic, multiple lesions of peripheral nerves converging separately toward the special centre.

6. Finally, these experiments, as a whole, establish, in a general manner, that irritations of the white portions of the nervous system (bundles, roots, and nerves), may extend to the gray matter, and there give rise to generalized and diffuse lesions, and so explain, experimentally, the clinical relations between the greater part of the chronic affections of the nervous system, particularly the fascicular scleroses and progressive muscular atrophy, relations which have especially been brought forward before the world by M. Charcot and his assistants.

New Form of Progressive Muscular Atrophy.—M. Charcot gave at the session of the *Soc. de Biologie*, Jan. 3 (reported in *Rev. Scientifique*), an account of a new form of disease. It is a sclerosis, primitive and symmetrical, which affects a portion of the lateral bundles of the spinal cord. It may at the same time affect the medulla, or the cord; is characterized especially by trophic troubles; by its symptoms it is allied to the provisional group of progressive muscular atrophies, and has their usual clinical history.

When the medulla is affected, there is a glosso-labio-laryngeal paralysis; and the increase of the pulse and the respiratory troubles indicate that the origin of the pneumogastric is involved.

A differential diagnosis of this affection from the other forms of progressive muscular atrophy, is difficult; but in them the weakness is generally proportional to the damage to the muscular tissue, while in this disease of Charcot paralysis is among the first symptoms, the fibrillary contractions, the emaciation, and the deformities, follow later.

As regards the pathogeny of this affection, M. Charcot has established it by observing, at the same time with the primitive sclerosis of a part of the lateral bundles, the destruction of the cells of the anterior gray horns. M. Charcot explains the violence of the affection by considering the inflammation to be propagated along the nervous fibres leading out from the anterior cornua.

Aphasia.—M. Troisier, at a recent *seance* of the *Soc. de Biologie* (Jan. 3), reported in *Rev. Scientifique*, offered some remarks on a case of aphasia, which are of interest. At a previous meeting he had exhibited the brain of an aphasic woman who had died of cerebral softening. The lesion had destroyed parts of the sphenoidal and occipital lobes, while the frontal convolutions, and those of the Island of Reil remained intact. M. Bouchard demanded at the time that a section should be made to determine whether or not the fibres from the third convolution remained unaltered. M. Troisier, therefore, had made the examination, and the fibres were found intact.

In the discussion which followed, M. Magnan recalled the fact that he had, in 1864, published an account of a case of aphasia due to cerebral hæmorrhage, taking place in the left sphenoidal lobe.

Softening of the Brain in Infants.—M. J. Parrot (*Arch. de Phys.;* abstr. in *Gaz. Med. de Paris*, Dec. 13, 1873)—In the new-born infant, noncadaveric softening of the encephalon is only the last stage of cerebral steatosis. The *foyers* of softening are multiple, and are almost exclusively confined to the hemispheric centres near the latter ventricles, more especially in the posterior portion. Only in one instance did M. Parrot observe the alteration to extend to the convolutions, and once in the lenticular core of the corpus striatum The softened portion, in this case, instead of being white, presented a brownish appearance, due to a mixture of blood or of hæmatosine. Histologically, the altered tissues differ from those which have undergone a steatose degeneration only by the separation of a great number of granular particles. This softening may be met with in the fœtus, and also in infants of several months, or years, of age. In these last cases, the commencement of the morbid process may be dated at very near the time of birth.

M. Parrot is disposed to consider, as the *reliquiæ* of softening, certain alterations which have, up to the present time, been variously interpreted, certain hydrocephali for example : the nervous matter disappearing, its place is filled by liquid, the secretion of which, continuing, causes dilatation of the ventricles. He explains, also, by an anterior softening, the lesions which Cazanvielh has classed among the cases of cerebral *agenesis*, and which Duges, Lallemand, and Cotard, have regarded as the product of encephalitis.

When the lesion is old and extensive, it may cause a secondary degeneration of the protuberance of the medulla, and of the cord.

This softening of the brain in the new-born, does not generally manifest itself by symptoms which can be recognized during life.

CONGESTION OF KIDNEYS FROM CEREBRAL LESIONS.—M. Carville communicated to the *Societie de Biologie*, Jan. 31 (reported in *Rev. Scientifique*), that in injuring the white fibres situated just outside of the extra ventricular nucleus of the corpus striatum, he had produced a decided congestion of the kidney of the corresponding side, and in some cases, a hæmorrhage ensued, revealed by the sanguinolent tinge of the urine. These experiments confirm the remarkable results of M. Ollivier, as to the determination of symmetrical lesions of the kidneys by injuries to the encephalon.

O. KOHTS (*Berliner Klin. Wochenschr.*, 1873, Nos. 24–27; abstr. in *Cbl.*) gives the observations made by himself on the influence of fear in the production of disease, during the bombardment of Strasburg. A great variety of diseases were evidently either produced, or greatly aggravated, by sudden fright from various causes during the siege. He reports, among the affections of the central nervous system, three cases of paralysis agitans, two women and one man; three cases of spinal paralysis; and the case of a man, who, though deformed by a spinal curvature since his youth, was yet in fair health, till, from a sudden fright, a paralysis of the left leg ensued, lasting until his death, three years later, from a lung affection. There were also noticed affections of the genital system, suppression of the menses, and abortions; and one case of angina pectoris, following the shock of sudden fright in a healthy person.

Affections of the respiratory apparatus were notably aggravated; and the first appearance of hæmopthisis in consumptives was often dated at the occurrence of a sudden terror from the events of the bombardment.

Among a great many cases of stomachal and intestinal catarrh, the author observed three cases of icterus catarrhalis, which were all due to this cause, following the shock almost immediately.

One case of affection of the joints is given, and is of interest. The patient, hitherto a sound man, was, by the explosion of a shell near by, rendered speechless, and trembling for several hours, and immediately noticed a painful swelling of the hand and knee joints, with stiffness of the right index finger, which lasted for a considerable period, though somewhat bettered. A similar case is mentioned of a man who, through similar causes, in Paris, in 1848, became paralyzed on the right side, with swelling of the joints of the hand and feet. Twenty-five years subsequently (1873), there still existed tremor of the upper extremity, with swelling of the right finger and wrist joints, which gave the feeling of crepitation on pressure.

INSANITY FOLLOWING ACUTE DISEASES.—Dr. J. Christian (*Arch. de Med.*, Sept. and Oct., 1873; abstr. in *Rev. des Sci. Med.*)—Insanity may succeed acute diseases in two different ways:

Directly: When it comes on after a disease, without the intermediation of any new pathological accident.

Indirectly: When it only appears as due to irremediable lesions of the brain or meninges, supervening as complications of the primary disease (meningitis, encephalitis, tumors, hæmorrhage, etc).

The author sets to one side the last class of cases, and, among others, the general paralysis which sometimes follows an acute malady.

The forms of mental alienation which succeed acute diseases are very variable; mania and stupor are most frequent; still, we may see in the same patient, agitation, alternating with stupor or with hypochondriac delirium, etc. Sometimes there is nothing more than a trace of insanity, characterized only by a few insane ideas, or some isolated hallucinations.

Dr. Christian calls attention, principally, to a series of facts scarcely noticed, and which are only observed after acute maladies; those where the mental disturbance is accompanied with various troubles of motility. He cites a certain number of observations taken from different works.

These intellectual disorders, produced at the same time with disorders of motility, may cause a diagnosis of general paralysis, or some other severe affection, when only a transitory disturbance really exists. Ataxy of the movements, ambitious delirium, and intellectual weakness, are, in fact, phenomena common to general paralysis, and to the mental and locomotor disorders which may supervene after an acute disease. But in this last case, these accidents appear in a secondary manner, have a rapid progress, and follow an acute disease. It will be equally easy to distinguish the delirium of alcohol, traumatic causes, or of toxic influences, by the cause which has produced them.

As to the diagnosis between the vesanic and the sympathetic delirium, it is sometimes difficult to distinguish them on their external characters alone. For example, the symptomatic delirium is systematized as in the insane: Sometimes a typhoid fever may be accompanied with religious delirium and hallucinations of sight and hearing; and certain such fevers may be taken for and treated as insanity; the stupor of this disease may be confounded with that due to mental alienation (*Ann. Med. Psych.*, 1872, *Dagonet*); nevertheless, we may say, that the vesanic delirium is recognizable when its appearance coincides with the amendment of the febrile symptoms, and the general condition.

As to the delirium supervening in convalescence, it is ordinarily easy to be distinguished from that due to any complication whatever (meningitis, etc).

The prognosis of insanity following acute maladies cannot be made with absolute certainty; death is an exceptional termination; but the passage into the chronic and incurable state has been observed. Sometimes, says Griesinger, the delirium following a typhoid fever changes into a mania and profound dementia. M. Behier has seen in these circumstances the dementia prolonged for a year and over; in a general way, the prognosis should be reserved, since a first attack of insanity always establishes a predisposition to further attacks.

The delirium ordinarily comes on suddenly and unattended; but sometimes it is preceded by certain prodomata (inquietude, change of character and irritability, insomnia, etc). In eighty-one patients, the duration of the delirium was from thirty-seven hours to fifteen days; in six, from fifteen days to one month; and in nineteen, from one to three months. Recovery is the usual termination, and is commonly sudden and rapid.

Like the paralysis after acute disease, the mental disturbance is either precocious or tardy; that is, it appears during the evolution of the malady, or is developed only after convalescence.

To explain the cause of these mental disorders, there have alternately been suggested congestion and anæmia of the brain, the influence of alterations of the blood, etc. According to the author, all the acute maladies have a common point of resemblance, and are translated by a peripheral irritation. This acting on the brain may produce insanity by reflex action (?) It is the same with it as in the reflex paralyses, which, according to Brown-Sequard, are produced by the reflexion of peripheral impressions on the spinal vaso-motor nerves, and the consequent abolition of the physiological properties of the cord. The peripheral impression may similarly be reflected on the intra-cerebral, vaso-motor nerves, and cause circulatory disturbances, giving rise to the cerebral disorders subsequent to acute maladies.

The treatment does not differ from that for other forms of insanity, and only includes, as a special indication, the care of the accessory lesions, produced by the acute disease (congestion, anæmia, etc).

PRODROMAL STAGE IN CHOREA. — Dr. Aug. Schmitt (*Memorabilien* XVIII., 3 hft.; abstr. in *Rev. des Sci. Medicales*) — This period, says the author, often escapes the observation of the physician, who is only consulted, in most cases, when the disease is confirmed. The period is characterized by disturbances confirmatory of the opinion of Dr. Betz, who sees in chorea an affection of the central nervous system, particularly of the cord and its envelopes. These disturbances are chiefly those of spinal irritation. Pain may be caused by pressure of the spinous apophyses, especially in the dorsal and lumbar regions, and at the level of a varying number of vertebræ. The patient also suffers from rheumatic pains in the shoulder and the nucha, and from headaches, always much less pronounced; from itchings at the arms, and the orifices of the nasal fossa, which may suggest a suspicion of worms. There are signs of irritation of the nerves of the heart; a general lassitude; a kind of uncertainty in progression. Sometimes the patient sees flashes of light. It is impossible to read, or to look long at objects. The nights are passed without satisfactory sleep, and are disturbed by painful dreams. Sometimes, during the day, the patients are seized with violent terrors, without any reason.

In one case, this prodromic period lasted sixteen hours.

These prodromata are certainly due to anæmia, which itself may be caused by tuberculosis, scrofula, lack of alimentation, or the establishment of menstruation.

Dr. Schmitt finds in the part played by anæmia, the indication for the treatment to be applied in this period. He does not content himself with ordering frictions on the back, with an ointment of opium and oxide of zinc, but he particularly prescribes the preparations of iron, and a tonic regimen.

STAMMERING.—Dr. H. Folet (*Ann. de la. Soc. Med. Chir. de Liege*, 1873, pp. 229 and 309; abstr. in *Revue des Sci. Med.*)—The author seeks to find, in the examination of the symptom of stammering, what is the nature of the vocal trouble, and concludes that it is due to convulsions, which happen only at the moment of phonation. He compares these functional convulsions to others of the same order, such as writer s cramp.

The vocal muscles, endowed with all their power, only become inapt to exercise it when the will puts them in action. The cause, therefore, does not reside in the muscles, but in that point of the nervous centres corresponding to the origin of the nerves which supply the affected groups of muscles; that is, there is an original nervous trouble, very probably in the central part of the medulla. The intimate nature of the nervous lesion is unknown; the author believes it to be only temporary and transient; analogous to the unknown lesions of hysteria, chorea, etc.

In an appendix the author states that there are functional paralyses, as well as functional convulsions; he is inclined to think that glosso labio-laryngeal paralysis is the one which corresponds to the convulsion which produces stammering.

EPILEPSY.—*Remarks on Colored Vision preceding the Seizures.* By Dr. Hughlings Jackson. It is said that, in cases of color-blindness from disease, red is, in most cases, the first color to go; and that the further progress in loss of color-sight is towards the violet end of the spectrum. [In the great majority of cases of congenital color-blindness, red is the fundamental color not seen. All people are red-blind in the most peripheral parts of the retina, and more extensively so to its nasal side.] Loss of power to see colors is one of the sensory analogues of palsy of muscles (motor nerves). Now, just as palsies have their mobile opposite in spasm, so, in opposition to loss of color-sight, there are cases of development of colored vision. Of course, the physiological comparison is, strictly speaking, betwixt excitations in motor and sensory nerves. Thus, occasionally a patient, who is subject to epileptic or epileptiform seizures, may have, as a first symptom (so-called aura), a color, or "all manner of colors," before his eyes. It is well, when the patient is intelligent, to ask which color is first developed, and the order in which they come. Theoretically, one would expect that the first color to be developed would be red, because it is the one first lost in cases of color-blindness. For, returning to paralytic symptoms for an analogy, we find that those very movements which are first lost in destruction of nervous organs, are those which are first developed in epileptic discharges of nervous organs. Dr. Hughlings Jackson thinks, so far as limited and recent inquiries enable him to judge, that red is usually the color first developed when color-development is a "warning" of an epileptic seizure. It is not always so; one of his patients has blue vision before severe epileptic fits; and she has had attacks of the blue vision, followed by temporary and complete darkness, without anything further. [Blue, according to Maxwell, is the fundamental color most removed from red. Helmholtz adopts the theory of Thomas Young, that the three fundamental colors are red, green, and violet]. To ask patients to note the order of development of colors, would, however, avail little in the majority of cases; probably there is, in most cases, a development of color, rapidly becoming complex (" rainbow").—*Brit. Med. Jour.*, Feb. 7.

After-Effects of Epileptic Discharges.—After epileptic spasm of muscles, if the spasm be severe enough, there is paralysis; as, for instance, in epileptic hemiplegia. Is there loss of power to see a color, after strong and continued development of that color? Dr. Hughlings Jackson has had no opportunity of testing this. It is important to note the after-effects—the "paralyzing effects"—of strong epileptic discharges. The presumption is, that strong discharges temporarily paralyze much of the centre in or through which the discharge spreads. For example, epileptic hemiplegia is probably the result of temporary paralysis of the corpus striatum, the centre discharged through.

There is no subject in the inquiry into epilepsy more important than the after-effects of epileptic discharges. Dr. Hughlings Jackson believes that, in epileptic mania, the maniacal phenomena occur when the discharge has ceased; the discharge leaves the highest sensori-motor processes *hors du combat* (there is loss of consciousness). The mania he attributes to action of processes more automatic; they act uncontrolled; they are not "inhibited." By an epileptic discharge the patient is "reduced" to a more automatic condition of mind; just as in hemiplegia the patient is reduced to a more automatic condition of gross movement; and as in epileptic aphasia the patient is reduced to a more automatic condition of speech. In so-called masked epilepsy, he believes there is, at the outset, a transitory and unobserved fit. He does not believe that an attack of mania replaces an epileptic paroxysm. —*Ibid.*

Systemic Sensations in Epilepsies.—The order of frequency in which the higher senses suffer in epilepsies is, Dr. Hughlings Jackson believes, sight, smell, hearing. An aura of taste is very rare; a "sting," or other non-gustatory aura, from the tongue, is not so uncommon. It is not easy to say where touch comes.

In our investigation of epilepsies, we must not pass over those sensations which Lewes calls systemic sensations, and which Bain calls organic sensations. Speaking of the error of restricting sensations to the reactions of the five senses, Lewes says: "Physiology teaches us that there is another, and, indeed, far more important class of sensations, arising from what I have proposed to call the systemic senses, because, distributed through the system at large, instead of being localized in the eye, ear, tongue, etc., they make up the greater part of that continuous stream of sentience on which each external stimulus raises a ripple." It is probable that the aura from the neighborhood of the epigastrium (sensation referred there, that is) is a crude and excessive development of visceral and other systemic sensations. However, if so, it seems strange that these sensations should, as is most common, occur in those cases of epilepsy in which loss of consciousness is, next to such warning, the first event in the paroxysm. For it implies that systemic sensations are first and most represented in the highest processes. Epilepsy, in which loss of consciousness is the first, or one of the first, events, is often preceded not only by development of systemic sensations, but is attended by pallor of the face. Indeed, the experiments of disease seem to show that the very highest processes (those underlying consciousness), sum up and represent all lower processes of the body. The epigastric sensation, so-called, "aura," is variously described by patients. Some speak of it as a "fear." A woman, nineteen years of age, said it was "a frightened feeling: as if I had done something wrong." Another patient said, it was "an indescribable feeling of horror." Women at the change of life, and other persons, will complain that they feel depressed, and as if they had done something wrong; and when asked the seemingly ludicrous question, "Where do you feel it?" will put the hand over the epigastrium. The local physical sensation is usually described as a "sinking." These are, probably, in most cases, referred sensations; but, probably, organic changes in the abdominal viscera, will provoke mental depression in the predisposed. It is, indeed, almost proverbial, that dyspepsia goes with melancholy, and sometimes with uncertainty of temper.

For what is called the physiology of the mind, the development of all kinds of sensations in cases of epilepsy, from the most impersonal (as of sight) to the most personal, the systemic, deserves serious consideration.

To show the importance of the systemic sensations, we will quote again from Lewes's "Problems of Life and Mind," p. 134. After remarking that "their immense superiority as *motors* has been singularly overlooked," he writes: "They make up by far the larger portion of our sentient material, since from them issue the emotions, sentiments, etc.; combined, indeed with the objective sensations, but subordinating these as means to their ends, inasmuch as we only see what interests us".—*Ibid.*

Destruction of Brain Substance without Functional Lesion.—Prof. Porta, of Pavia, gives an account (*Archivio Italiano*, Nov., 1873; abstr. in *Psychiatr. Centralblatt*) of the case of a man who had received an injury of the skull, causing, as nearly as could be estimated, the complete disorganization of the upper right hemisphere. In spite of this extensive lesion, no measurable psychic or sensorial disturbance was observed; and at the end of eighteen months a partial hemiplegia of the left side only, remained. This was apparently somewhat improved by electrical treatment.

The same author reports another case of the *post-mortem* of a woman who had died of fever, without stupor, somnolence, or delirium, in whom the whole right side of the brain was found disorganized by suppuration, the only parts remaining intact being the cerebellum, the pores, the crus cerebelli, and the intraventricular portion.

From these facts, Prof. Porta holds that the brain is a double organ, consisting of two similar halves, one of which can do the duty of both; that is, that it is physiologically, as well as anatomically, double.

Reflex Paralysis.—G. H. Roessingh (*Jour. de Med.*, Oct., 1873; abstr. in *Rev. des Sci. Med.*)—"Among the paralyses of the motor nerves, one is distinguished, which has received the name of reflex paralysis, because it was believed to have for its cause a morbid irritation of the sensory nerves.

In the majority of cases a rigorous anatomical examination always discloses pathological alterations, perhaps in the nervous centres — may be in the course of the nerve corresponding to the paralyzed parts—so that the name reflex paralysis is not applicable to it. For some cases, nevertheless, it has been impossible to account for the paralysis by any histological alterations; the source of the trouble, therefore, has to be sought by another way, in demonstrating that the disease in question might be produced by an irritation of the sensory nerves alone, and that such an irritation may cause an inflammation of the spinal cord, without its being necessary that its traces be discovered in the track of the nerve. It is therefore evident that the cause of the paralysis should be sought for in the primitive irritation of the sensory nerves."

Roessingh, with the collaboration of Prof. Rosenstein, repeated the experiments of Lewisson and Feinberg, on account of their pathological importance; but as their results differed totally from those of their predecessors, they felt compelled to publish them.

The author first gives the results of his experiments, made according to the method of Lewisson. The latter had always observed a paralysis consecutive to contusing or compressing various organs in the rabbit and the frog. In the majority of cases the posterior extremities were affected, whence he concluded that he had to do with a veritable reflex paralysis. In nine experiments, of which we pass by the details, made on rabbits and frogs, Roessingh did not once detect this pretended paralysis.

He then undertook a new series of experiments, according to the method of Feinberg. This investigator cauterized the sciatic nerve of rabbits, and found that the animals died, with a paralysis of the posterior extremities, with incontinence, cramps, hebetude, etc. As the cause of these symptoms, he found, at the autopsy, a myelitis, especially in the gray matter, slightly less marked in the white. None of the animals whose sciatic nerves were submitted, by Roessingh, to the action of various caustic agents, such as caustic potash, nitric acid, etc., were affected with the least trace of reflex paralysis. Microscopic examination revealed no lesion of the cerebrum, cerebellum, or of the spinal cord; and, further, different sections of the cord, subjected to the most careful examination, and compared with similar ones from a healthy animal, appeared perfectly sound.

Alcoholism.—M. Magnan (*Gaz. Hebdom. Med. et Chir.*, Nos. 46, 47, 1873; abstr. in *Rev. des Sci. Med.*)—It is known that the prolonged influence of alcohol can develop in the system a duplex morbid action: 1. A tendency to fatty degenerations of the organs; 2. A tendency to chronic diffuse irritations, associated with the steatosis, but sometimes capable of an existence alone. According to the predominance of one or the other of these lesions, chronic alcoholism may lead to dementia (steatosis, or atheroma), or to general paralysis (diffuse interstitial sclerosis).

Among the troubles of motility and sensibility, there are to be remarked paralyses of one side of the body, with diminution, or abolition, of general and special sensibility.

In consequence of a brusque apoplectic attack, or little by little, without other phenomena than headache, dizziness, and numbness, with formication of one side of the body, the patients become aware of a muscular weakness: they drag their limbs, let objects drop from their hands; they may have embarrassment of speech, and change of features. The paralyzed arm generally presents a more marked tremor than the other; and, in one case, M. Magnan observed rhythmic movements, having an analogy to those of paralysis agitans. The whole of the paralyzed side presents an anæsthesia of the skin, the mucous membranes, and the deeper parts. The loss of sensibility is sometimes so complete that it extends to the touch, to tickling, pricking, temperature, and to the action of constant and induced currents. Sometimes it is incomplete, and can only be determined by the application of the compass of Weber. But it is necessary to know that the extension of the two points of the æsthesiometer is greater when the points of the compass are placed according to the axis of the limb—that is, according to the direction of the nerves—than when applied perpendicularly to this axis, or when the points are placed on distinct nervous branches, answering to two separate *foyers* of innervation in the cord.

The anæsthesia reaches the deeper-lying portions, the muscles; the muscular sense is enfeebled, and sometimes abolished. When the eyes are shut, the patient is not conscious of his own motions; he is able to walk in a rather straight line, but is easily drawn from it, without consciousness of the deviation, into a circular movement, when the insensible side is gently restrained. If an organ is touched, on the sound side—the nose, or the ear—the patient believes that he has himself executed the movement.

The anæsthetic members are, ordinarily, colder than the corresponding healthy ones; and in some cases the invalid is conscious of the chilliness, which may reach two, or even three, degrees centigrade. The anæsthesia is not limited to the skin; it extends to the mucous membranes, the conjunction of the eye-lids, the sclerotic, and even of the cornea; to the pituitary membrane, the mucous membrane of the tongue, the velum of the palate, the uvula; to the mucous membrane of the glans penis, the meatus urinarius; and to the margin of the anus, on the affected side.

Special sensibility is also affected in the sense of sight; a manifest enfeeblement is noted on the side attacked; the passage of a continued current through the head, or in its vicinity, produces no phosphenes in the eye of the anæsthetic side, while the other eye perceives them at the opening and closing of the current.

Besides the amblyopia, there exists, in some patients, an unilateral dyschromatopia; the sound eye can easily distinguish all colors, while the other confounds the composite colors, and even the different shades of the same color. Ophthalmoscopic examination reveals no lesions, except a little venous stasis, and peripapillary, and perivascular infiltration.

Enfeeblement or abolition of the senses of hearing, taste, and smell, are also observed.

Among hysterical individuals, hemianæsthesia is much more frequently observed on the left than on the right side. In alcoholism it occupies either side indifferently.

In that hemiplegia of sensibility and motion, the disturbances of motility may pass off rapidly, while the anæsthesia persists; at other times the two kinds of trouble may cease simultaneously.

The patients, like the subjects of chronic alcoholism, suffer from headache dizziness, buzzing in the ears, chills, and cramps in the members, more pronounced on the paralyzed side. The mental faculties are also ordinarily enfeebled.

These troubles of sensibility show themselves not only in alcoholism, they may be produced by a hæmorrhage, a softening, or a sclerosis, following a material alteration. In hysteria, on the contrary, this assemblage of symptoms can only be a simple functional trouble, transient, without any appreciable material lesion.

But what are the regions of the nervous centres which preside over the free exercise of the general and special sensibility? According to Longet and Vulpian, the centre of the perception of sensory impressions is in the annular protuberance, which, according to the last-named experimenter, seems also to preside over the gustatory and auditory sensations. But the mere lesion of this perceptive centre does not suffice to explain the loss of smell and of sight observed in our subjects of chronic alcoholism.

According to the theory of Todd and Carpenter, the centre for the perception of tactile impressions resides in the optic thalamus; it is there that the sensory impressions are transformed into sensations.

According to the author of this paper, it is the external and superior portion of the optic thalamus, the nucleus lenticularis, and the radiant crown, which are the parts involved in these cases of hemianæsthesia.

Chronic Alcoholism, Ending in General Paralysis.—Dr. Gambus (*These de Paris*, 1878; abstr. in *Rev. des Sci. Med.*)—Chronic alcoholism may terminate either in dementia, or in general paralysis. It is this latter method of termination that M. Gambus has attempted to make known. He first gives the opinion of the greater number of his predecessors, who, in certain cases, accord to excesses in drinking a principal part in the production of general paralysis; then, supporting his position with the facts already known in regard to the pathological physiology of alcoholism, and with new clinical facts detailed in his thesis, the author proves that, under the influence of alcoholic drinks, two orders of lesions have a tendency to occur: 1. Fatty degeneration of the organs and vascular atheroma; 2. Chronic diffuse interstitial inflammations, which may occur in various organs (the liver, the kidneys, the nervous centres), but which, when it reaches the brain, gives birth to the usual lesions of general paralysis.

The author reports many cases, in which he follows, step by step, the progressive march of alcoholism toward general paralysis. These observations, collected with care, are of interest not only in the special aspect which the author gives them, but also for the study of certain points relative to the pulse, to temperature, and to anatomical lesions in the patients.

Dr. J. F. Troyon (*These de Paris*, 1878; abstr. in *Rev. des Sci. Med.*) devotes the first part of his Thesis to a study of the pathological physiology of alcoholism. After examining the diverse theories which have been proposed as to the action of alcohol, he relates comparative experiments on the action of alcohol and absinthe. As a clinical demonstration, agreeing with experimental physiology, he cites two cases of alcoholism with absinthism, in men in which, besides the usual accidents of alcoholism, there were added epileptic attacks, under the influence of the abuse of absinthe.

The author, after broadly sketching the general characters of alcoholism, gives a report of eight cases observed at the Bicetre. *A propos* to these, he discusses the degree of responsibility of the subjects of disease from the use of alcohol, and as all the patients in question were subject to hallucinations and delirium, the author naturally concludes that they are irresponsible. In these conditions the medico-legal question is, generally, easily answered; but the difficulty resides, especially, in the estimation of the acts committed by the individuals in a state of drunkenness.

Hydrophobia.—*L' Union Medicale*, of Feb. 14, reproduces from the *Recueil de Med. Veterinaire*, a letter from M. Fitte, Veterinary Surgeon at Villa Bigorre, on the spontaneous production of hydrophobia in the dog. M. Fitte's observation seems to prove that unsatisfied sexual excitement may be the cause of spontaneous hydrophobia, as the case related by him occurred under his own eye. The disease was immediately consecutive to the cause; the dog had not been bitten, and no case of madness had been known in the place for more than a year.

Handwriting of Ataxics.—At the *Seance* of the *Societe de Biologie*, Feb. 7, M. Onimus made the following communication (reported in the *Gaz. Med. de Paris*, Feb. 21):

"We have observed, in examining the handwriting of ataxic patients, phenomena analogous to those exhibited in the lower limbs.

"When the arms are slightly affected, the chirography, with the eyes open, scarcely differs from that of persons in health; but when the eyes are closed we have observed a great uncertainty in the writing.

"It is true that, with every one, the handwriting is less correct and plain when it is done with the eyes closed; but the special characteristic of the ataxies, is the difficulty in making the rounded parts of the words—the c, the a, the o, the l, the e, are angular, and formed by straight lines, instead of curves more or less extended.

"We might say that there no longer existed in the movements of the fingers, the synergy of the normal state.

"The handwriting varied in other respects, according to the degree of the ailment. In the first period, nothing distinctive can be noticed when the eyes are open; and it is only when they are closed that we remark a kind of inco-ordination in the formation of the letters.

"In the more advanced stages these characters exist, even with the eyes open, and are specially increased when the eyes are shut.

"In these cases we notice, further, that each letter is made, so to speak, with a jerk, and that the hand has difficulty in limiting the trace. In the p, for example, the descending line never ends in a neat termination; and we may say that there is a kind of impulse to continue. Also, at this period, the patients are hardly ever able to use the pen, but make use of the pencil, because the point of the pen catches every moment, and sputters, to use a common expression.

"At last, when the ataxy of the arms is very pronounced, the writing of a single word with the eyes closed, becomes impossible, and we obtain only a set of traces, unformed and without order.

"These different characteristics are seen in a series of *fac similes* of handwriting, which we have collected from various ataxic persons.

"We see that the modifications in the movements of the arms are analogous to those of the limbs; for one sees in the lower limbs, according to the development of the disease, first a slight titubation, and then a complete failure; and in the upper members, first a slightly modified penmanship, and afterwards a complete inability to form a single word.

"We would still remark that these facts come under the same rules as those we have shown to hold good for the formation of language and writing, as in almost every case the signature is correct, even when the eyes are shut.

"Further, when a person is in the habit of writing without stopping, and placing the dot over the i when the word is complete, the writing is much more altered when he attempts to dot the i's as they are written.

"On the other hand, and for the same reason, there is no trouble with those persons who put the dots on the i's only when they are made to write the words without stopping.

"So, for the word *electricity*, which contains two i's, we notice that the c and the t are badly formed if we ask the patient to write the word without dotting the letter. The changes in this case appear even with the eyes open, but become much more marked when they are shut."

c.—THERAPEUTICS OF THE NERVOUS SYSTEM AND MIND.

Physostigmia.—Dr. J. Q. A. Hudson (*Southern Med. Record*, December, 1873) gives a paper on The Physiological Action and Therapeutic Uses of the Calabar Bean. His summary of its physiological action on man is as follows:

1. "It lessens the reflex action of the spinal cord, diminishing or destroying this function, according to the dose given. It is a perfect spinal paralyzer."

2. It acts in a slight degree to lessen the excitability of the motor nerves.
3. Muscular irritability is not affected.
4. The excitability of the afferent nerves is not affected; their sensibility is sometimes increased.
5. In small doses the action of the heart is weakened. This is shown either by a lessening of the number of beats, or by an increase in the number of beats, with feeble action. In large lethal doses the action of the heart is at once destroyed, and death results from cardiac syncope. The action of the heart ceases in diastole.
6. It sometimes contracts the pupil. This action, probably, frequently occurs with large doses, but when moderate doses are administered it is rarely observed; a dilatation of the pupil is an exceptional phenomenon.
7. Catharsis is sometimes observed; and the same may be said of vomiting and diaphoresis.

Dr. Hudson gives the following rules for the use of physostigmia in tetanus:

1. Commence the treatment by the subcutaneous injection of one-fourth or one-third of a grain of the extract, dissolved in water, repeating and increasing the dose, according to its effect, every hour, or two hours, till the disease yields, or the action of the remedy is exhibited.
2. If the case is severe, and the spasms violent, continue the hypodermic method.
3. If not severe, after the use of the hypodermic method in the start, give the remedy by the stomach, every two or three hours, in treble the quantity, and with the syringe.
4. The patient must have a liberal supply of nourishing food and stimulation (if symptoms of debility arise), during the whole course of the disease.
5. Should there be manifestations of increased excitability of the sensory nerves, the exhibition of morphine, hydrate of chloral, or bromide of potassium, will aid in controlling this state of the sensory nerves.

The author also reviews the use of this agent in strychnia and atropia poisoning, chorea and epilepsy, in some of which (chorea, strychnia poisoning) he considers it of value. In cases of poisoning by atropia, he deems opium a preferable remedy. In epilepsy, a further trial is deemed desirable.

The local action of the drug upon the eye, is also discussed, and it is pronounced to be of value in cases of prolapsus of the iris and idiopathic mydriasis; also, to counteract the action of atropia, and to contract the pupil, previous to the operation of endectomy.

HYOSCYAMIA.—M. Hellman (*Inaug. Dissert, Jena*, 1873; abstr. *in Centralblatt der Med. Wissensch.*) gives the results of a series of researches instituted by himself on hyoscyamine and its allied products. The experiments were made upon frogs and rabbits. The mydriatic effect of pure hyoscyamia is about the same as that of atropine, a quantity, inappreciable by chemical tests, was detected by this physiological reaction. A dose of one centigramme demonstrated the reflex irritability, apparently by paralysis of terminations of the cutaneous nerves. With atropine this effect is observed with only four millegrammes. The paralysis of the cardiac inhibitory centre, characteristic of atropine, was not observed in frogs poisoned with from five to twenty millegrammes of hyoscyamine; but the frequency of the heart-pulse decreased to one-half or one-third the normal number; while, on the contrary, in warm-blooded animals (dogs) which had received from five to thirty millegrammes, there was an increase of the frequency of the pulse, and a perfect paralysis of the vagus to electric irritation. The respiration is slowed in frogs by hyoscyamine; and in mammals, it is only in the later stages of the poisoning that it is quickened (as is always the case with atropine). This similarity in the physiological actions of hyoscyamine and atropine, caused Preyer to employ the former also, as an antidote in poisoning by Prussic acid; indeed, guinea-pigs were restored after receiving fatal doses of this poison.

The basic product of the common alkaloid, hyoscine, is an oily fluid, one-half to one drop of which, administered to frogs, produced, not the action of hyoscyamine, but powerful disturbances of the respiration, from which they sank in a short time. In rabbits, on the contrary, after the subcutaneous injection of three drops, there followed a paralysis of the cardial termination of the vagus, as in the case of the use of the alkaloid itself, while the pulse frequency diminished but very little. No effect on the respiration was observed; and the action on the pupil was entirely lacking. Hyoscinic acid, like tropic acid, is indifferent. The author describes the influence of tropine differently from Frazer. While the latter considered its action identical with that of atropine, except in its mydriatic power, the former finds it to possess no mentionable influence on the reflex activity or heart's action of frogs and rabbits, and to produce only a very slight decrease in the frequency of the respiration.

Phosphorus.—Willis E. Ford, M.D. (*Am. Jour. of Insanity*, Jan. 1874) gives a paper on the use of phosphorus in insanity, in patients dementing, or passing from the acute stage, either toward the more chronic form, or toward recovery. In this stage, he used the following formula, which is essentially the same as that of Anstie, Radcliffe, and others:

℞.—Phosphori		grs. xxxij.
Pulv. acaciæ Glycerinæ	aa	℥ ss.
Aquæ		ʒ vj.
Pulv. ext. glycirrhizæ Pulv. rad. glycirrhizæ	aa	℥ jss.

Melt the first three ingredients in a closed porcelain vessel, and stir until the phosphorus is finely divided; then add the other ingredients, and divide into 960 pills; these are afterwards coated with collodion. The principal thing to be observed is, that the phosphorus be very finely subdivided, so that it may not cauterize the walls of the stomach.

These pills were administered three times a day, one after each meal, with very favorable results, in most cases. The temperature was brought nearer the normal stage; the pulse became more regular and full; the excretion of phosphorus approached the standard of health, and there were marked indications of mental improvement.

None of the disagreeable symptoms which are mentioned by authors, were observed, such as albumen, blood, or casts in the urine; and the author concludes that it can be safely and advantageously administered.

Opium and its Alkaloids.—Dr. J. V. Laborde, in an article in the *Bull. Gen. de Therap.*, Dec. 15, 1873, gives the following conclusions in regard to the use of opium and its alkaloids:

1. The officinal preparations of opium in general use, offer more serious and real dangers than is usually believed, on account of the possible, and, indeed, frequent, predominance of the toxic and convulsive action of the principles contained and mixed in the raw substance.
2. The alkaloids should be substituted, as much as possible, for the crude opium, in practice.
3. Among the alkaloids, narceia and morphia should be preferred to the others, as much on account of their relatively inferior degree of toxic power as for the reliability of their action.
4. Finally, codeia should be employed only with great caution, if not entirely discarded, by reason of the insidiousness of its toxic action.

In the following number of the same periodical (Dec. 30), M. Laborde continues the subject. He gives at length the advantages of narceia, which he characterizes as the least toxic and most soporific of the opium alkaloids, not producing the inconvenient and disagreeable effects habitual to morphine. In regard to the method of administration, he prefers some of the liquid preparations to the pilular form, and recommends the administration of the morphia and narceia, by the intestine, either in injections or in suppositories. According to the author, narceia is especially adapted to the treatment of infancy; and he particularly mentions whooping-cough as among the affections to which it is most applicable. The disadvantages which have thus far prevented its more general use, are the difficulty of obtaining it pure, and its high price; these, however, may, perhaps, be both done away with, were the demand sufficient to justify its manufacture on an extended scale.

Joseph Parish, in an address on Opium Intoxication (published in the *Med. and Surg. Reporter*, Nov. 15 and 22), sums up as follows:

1. Opium is a poison.
2. Men take it not for social enjoyment, but for a physical necessity.
3. Taken thus, in proper doses, and in corresponding conditions, it is a valuable remedy. When it relieves physical or mental distress it fulfills its physiological purpose, and its use is legitimate.
4. It is a fascinating drug in its influence upon both mind and body, with persons who have opposing idiosyncracy.
5. There is a constitutional condition which is in harmony with its hypnotic quality; and a constitutional condition which is in harmony with its excitant quality.
6. It will affect persons differently, as they are inclined by nature, or by the pathological condition which demands its use.
7. Taken in excess, it produces a diseased condition, certain symptoms of which are decided and specific.

8. To relieve the symptoms, it is desirable to avoid the shock, as it is desirable to avoid it in surgical operations.

9. For this purpose, the practitioner should immediately reduce the accustomed supply to the minimum dose which will meet this condition.

10. When the minimum is reached, the suffering of the patient begins, and then the practice should be to give tone to the nervous system as the opium stimulus is withdrawn. The reduction should be in minute quantities, and the tonic doses full and persistent.

11. The moral sentiment, the confidence and courage, of the patient, should at all times be kept up to the highest attainable degree.

12. Such a course will almost always secure the desired result.

Bromide of Potassium.—The *Practitioner*, January, 1874, contains a paper on the therapeutic action of bromide of potassium, by Professor Binz, of Bonn, translated from the *Deutsche Klinik* by the editor, Dr. Anstie. The ground taken in this article is unfavorable to the prevailing opinion as to the therapeutic value of this medicinal agent. The author reviews the various researches in regard to the subject, and suggests that the favorable effects may be due to the potassium, and that the assumed nervine action has no other basis than this, with, perhaps, the aid of mental influence as suggested by Amburger. He concludes with the following words: "Without wishing wholly to deny the justice of the prevailing bromo-therapy, it may be well expected, from the manifold contradictory facts, that, of the many hundred weights of this salt which are now yearly expended, a few pounds will soon again be found sufficient for the purposes of scientific clinical medicine." This article is immediately followed by another from the pen of Dr. Anstie himself, in which the prevailing English, and, we may also say, American, opinion as to the actual value of this remedy, is set forth. He admits, however, that certain observations by eminent authorities have a bearing on the question of bromide medication, which fully justifies the scepticism which would demand further investigation of the subject.

Dr. J. Drouet (*Ann. Med. Psychologiques*, Sept., 1873) also gives an exceedingly sceptical article on the therapeutic value of bromide of potassium, in which he, without denying it a certain degree of efficacy in some cases, considers that it will, with oxide of zinc, and several other medicinal agents, which he assumes have had their day in the favor of the profession, be relegated to a much less prominent position among the remedies for nervous disorders than it holds at present.

Dr. Lunier, one of the editors of the *Ann. Med. Psychologiques*, appends a note at the close of the paper, in which, while he agrees with Drouet as to the overestimation of bromide of potassium, he holds that the oxide of zinc is undervalued in the connection in which it is put in the article.

A propos to this note, we may here state, also, that the author of the notice of Dr. Drouet's articles in the *Revue des Sci. Medicales*, does not, by any means, fully agree with him in his valuation of the bromide in epilepsy, and allied affections, and holds nearer to the average opinion of English and American authorities, based on their own clinical experience.

A letter from Dr. J. Warburton Begbie, called out by the papers of Drs. Binz and Anstie, is published in *The Practitioner* for February. The writer bears testimony as to the value of the bromide in epilepsy, insomnia from various causes, either alone, or as an adjunct to other remedies, in spasmodic diseases, etc., etc.

Immediately following Dr. Begbie's communication, in the same journal, is published an article by Dr. John W. Bligh, of Montreal, on the use of the bromide of potassium in the treatment of gonorrhœa. He considers that there is no drug in the pharmacopœia which, *prima facie*, promises such happy results in the treatment of this disease. He claims that it has the power not only to diminish the secretions, and relieve the pain of all mucous membranes, but also to produce a special sedative effect upon the organs of generation.

NITRITE OF AMYL.—Dr. C. Steketec (*These of Utrecht;* abstr. in *Revue des Sci. Med.*)—The author commences with a description of the chemical papers on this drug, from its discovery by M. Balard, in 1844, down to the latest researches.

In the second place he gives a historical *resume* of its physiological action, and therapeutic employment, in which he cites French, English, and German observations, the experiments which have been made on animals, and the results of its administration in certain diseases, such as angina pectoris, hemicrania, trismus, melancholia, and epilepsy.

The results of the different experiments may be resumed as follows:

Nitrite of amyl diminishes the tension of the blood in the arteries.

This diminution is equally observed in cases of paralysis of the vaso-motor nerves in cutting the spinal cord below the first cervical vertebra, as in the contrary case.

Nitrite of amyl does not diminish the labor of the heart in the unit of time, although the number of contractions increases.

It has no influence on the nerves, but has on the contractile elements of the blood-vessels, because it diminishes the arterial tension in diminishing the resistance.

As to diseases, the author reviews particularly the effects obtained in melancholia, and shows that it is a palliative of transient influence. The patients find themselves much better in the evening, and sleep well during the night. Some shortly recover completely, while others fall back sooner or later into their habitual apathy.

In the third place, he publishes his own observations, likewise made by means of physiological experimentation, on himself and in animals, particularly on rabbits. From the first, he gives traces obtained by the sphygmograph; for the second, he gives graphic tables to indicate the tension in the auricular vessels. He adds to this study that of the administration of the medicament in cases of disease observed in the clinic of Utrecht: four of melancholia; six of epilepsy, or epileptic attacks.

In the cases of melancholia, he says that he has never seen the satisfactory results that others pretend they have obtained; and that in one case of active melancholia the effect was really injurious.

In regard to epilepsy he draws the following conclusions:

a.—This agent exerts an important influence in all cases of epilepsy in which the attacks are due to, or are accompanied by, cerebral anæmia.

1. Because it anticipates the attack when there are prodromata.
2. Because it cuts off the attack when it appears.
3. Because it relieves the symptoms due to interrupted innervation after the attack.
4. Because the attacks become less frequent (?). (Interrogation point put in by the author.)

b.—It is productive of injurious effects in all cases of epilepsy, where the attack is due to, or accompanied by, hyperæmia of the brain.

1. Because the attacks last longer, and become more frequent.
2. Because the attacks, either maniacal or convulsive, increase in intensity.

R. Pick (*Centralblatt der Med. Wissensch.*, 1873, No. 55) gives the results of his researches and observations on this agent. After stating the general symptoms following the inhalation of the vapor, in which he states that it produced no perceptible dilatation of the retinal vessels, he gives an account of a peculiar phenomenon which may be observed after the system is fully under the influence of the drug. Fixing the eye on one point of a bare wall, there is perceived a circular, intensely yellow space, surrounded by a violet blue ring, and still outside of this, numerous wavy lines. This appearance, according to the author, is nothing else than a projection of the macula lutea, to which the size of the yellow space corresponds. The outer circle of violet is the complementary color to the yellow, and the sinuous lines are the retinal vessels.

In regard to its action on the heart, he states that there follows a notable relaxation of its muscular walls. The increase of frequency of the pulse was noted by simply counting it in a narcotized dog. In respect to the respiration, he states that neither its frequency nor the capacity of the lungs were at all altered.

Nitrite of amyl is to be considered as a direct muscle-poison. This was proved by experiments on protozoa, which consists merely of contractile substance, without nerve elements, and by experiments on curarized frogs. The electric irritability of the muscles of the frogs poisoned with curare, was first determined, then the sartorii and gastroenemii were prepared separately, and the one placed under a bell-glass filled simply with atmospheric air, and the other under a similar one, in which some wadding moistened with this substance was fastened. Ten minutes afterwards the latter was found perfectly uncontractible, while the other reacted to the electric current with nearly its original strength.

In regard to the question whether this agent, in acting on the vessels, did so from a centre, the author holds, with Brunton, as against Bernheim, the negative

The cases which the author has collected show that nitrite of amyl is a very useful medicament in various disorders: hemicrania, epilepsy, asthma, etc. He has also used it in a case of trismus and traumatic tetanus, and even then it produced a better muscular relaxation than curare.

Electricity.—Prof. Schwanda, in his general report of the Electro-therapeutical department of the *Wiener Allgemeiner Poliklinik*, for 1873 (reported in *Wiener Med. Presse* No. 2), gives the following particulars as to the results of electrical treatment under his supervision:

Out of seven hundred and forty-six cases thus treated, there were three hundred and sixty-four of neuralgia, fifty of hyperæsthesia, two hundred and thirty-four of paralysis, and ninety-eight of convulsive affections. Of these, a central disorder was diagnosticated in three hundred and five cases; in the remainder, the disturbance was presumed to be only peripheral. A complete cure was attained in three hundred and three cases; decided improvement in two hundred and twelve; in the remaining one hundred and thirty-six no alteration for the better was obtained.

The electrical treatment was isolated as completely as was practicable from all treatment by medication, so as to render it possible to form a judgment as to its value as a method of curing disease. Very few prescriptions were written for patients in Dr. Schwanda's department in the course of the year, and those for ailments which had no direct connection with that against which the electrical treatment was specially directed.

Neuralgia of the Testicle cured by Electrization.—This observation, given at length, and with a tone of gaiety peculiarly Italian, may be briefly stated. A young màn, not suffering from any venereal trouble, was tormented with a neuralgia of the testicle, in the degree that he demanded instant castration, since the usual means of relief had proved unavailing. Dr. Felippi conceived the idea of employing the constant current; and after five applications the patient was cured. Unhappily, the author is less prolix as to his method of application. He simply informs us that he employed a feeble, constant current. A careful examination showed that the neuralgia was essential, that is, it did not depend on an affection of the gland, nor on an accumulation of fæcal matter.

We have no reason to believe that the cure was not permanent; and undoubtedly electrization is a means of cure for neuralgias of the testicle, when they are essential; but we believe that the affection is more frequently only symptomatic: and then electrization fails. We have recently had a proof of this in two cases. The repeated use of purgatives in one, and the application of a bandage in the other, with which there co-existed an inguinal hernia, succeeded after the ordinary means, including galvanization, had been of no avail.—(*Gaz. Hebdomadaire* fr. *L'Imparziale*) *Bulletin Gen. de Therapeutique*, Dec. 30, 1873.

Physiological Effects of Induction Currents.—M. Onimus, in a note communicated to the *Acad. des. Sciences*, Dec. 1, 1873, through M. Becquerel (*Gaz. Med. de Paris*, Jan. 3), gives the result of his experiments in regard to the difference of the physiological action of induction currents, according to the composition of the wire forming the helix. He had constructed helices of copper, lead and argentan, exactly similar, in every respect, except as to composition, and influenced by the inducing current in precisely the same manner.

The effects on healthy nerves and muscles were found to be different, according to the metal used; generally, when the wire of the helix was a comparatively poor conductor, the contraction was stronger, and the impression on the cutaneous nerves was less vivid, than when a good conductor, copper, for example, was used. These effects are more marked as the exterior resistance is greater.

General Electrization.—Dr. R. Vaeter, of the University of Prague, in the preface to his translation of Beard and Rockwell's Medical and Surgical Electricity, thus speaks of the method of general electrization:

"Although general electrization had been used before by some electro-therapeutists, yet to Beard and Rockwell belongs the credit of giving it a scientific basis, and reducing it to a system. Of the advantage of this method, I have thoroughly and abundantly convinced myself by actual experience, and I can, therefore, conscientiously recommend its employment in very many cases."

Benedikt, in the recent second edition of his Elektrotherapie, also speaks with commendation of this method, and states that he can, in general, agree with the views of the American authors, and that he has attained excellent results in cases of sleeplessness of the insane, migraine, and general nervousness. He adds, a little farther on, that general electrization has the recommendation that, when all parts are electrized, the particular seat of disease is certainly not missed in the application.

CHLORAL.—Dr. Anstie, in his journal, the *Practitioner*, for February, gives an account of a rather curious case of chloral poisoning, with comments. The patient, a medical man, had for some time been in the habit of taking chloral in large amounts, and had suffered from severe pains about the joints, dryness of the skin, and a peculiar intolerance of alcoholic stimulants. The pains were intensely aggravated by an accidental over-dose, which was the immediate cause of his coming under Dr. Anstie's observation. The use of chloral was completely stopped, the patient put under proper treatment, and recovery rapidly succeeded.

Dr. Anstie calls attention to the case as suggestive of new precautions in diagnosis and treatment. The prominent feature of pain in the joints is specially referred to, and the fact that similar pains may be produced (though rarely) by chronic alcoholism is referred to; similar ones also have been caused by prolonged abuse of chloroform inhalations; thus affording support to the theory that chloral is decomposed in the blood with the effect of producing free chloroform.

The other feature of intolerance of alcohol is mentioned, with the statement of Ludwig Kern, that it is dependent upon a weakened condition of the vaso-motor nerves of the head and face. In this case, as in others which have come under the author's observation, there co-existed with this condition in the head an opposite one (spasm) of the vessels of the lower extremities.

Still another consequence of the injurious action of chloral, which was seen in another patient, is mentioned by Dr. Anstie. It consisted in a partial paraplegia, which disappeared with the abandonment of the use of chloral. The author concludes with the following words:

"It is to be hoped that, before long, some one will give us a complete account of the toxicological position of chloral; for there is certainly no drug introduced of late years, concerning which more erroneous rubbish has been taught. It is exceedingly useful, beyond doubt; but it is far enough from being harmless; and it would be well for us to get a complete view of the extent of its powers of mischief."

THE MADAGASCAR ORDEAL POISON. — Andrew Davidson (*Jour. Anat. and Phys.*, Nov. 1873) publishes an account of the tanghinia or ordeal poison of Madagascar, in use in that island previous to the adoption of the Christian religion, and the laws and customs of civilization. This poison is derived from the kernel of the drupe of a large tree (*Tanghinia venenifera*) of the natural order Apocynaceæ, which grows abundantly on the eastern coast of the island. Two crystalline principles are reported to have been obtained from the tanghinia: one the bitter principle, tanghinin, and the other the poisonous principle, tanghicine. The author, however, in his experiments, used the emulsion obtained from the oily kernels, or the extracts made with ether and alcohol, and these were employed on warm-blooded animals (lemurs, cats, civets), and on frogs. The following are his general conclusions:

1. The tanghinia must be classed among the cardiac poisons. It uniformly causes death by arresting the actions of the heart.

2. It does not act on the heart through the vagus nerves. When applied to the exposed heart, its rapidity of action is remarkable. The fact that it arrests the pulsations of the excised heart of the frog, is conclusive proof that its influence, when topically applied, is direct, either on the muscular substance, or the muscular substance *and* cardiac ganglia.

3. There is sufficient reason to believe that the tanghinia acts on the spinal cord, producing paralysis, and diminishing reflex action.

4. Voluntary motion is abolished, and the irritability of the motor nerves lessened by the poison. When it acts through the circulation in mammalia, sensation is not remarkably affected; muscular contractility is very much diminished. More exact knowledge of the degree and order in which these various functions are affected, can only be obtained by carefully performed experiments made in Europe, where the more delicate instruments can be had.

5. It is exceedingly fatal to man, in doses of thirty grains of the kernel, if not promptly rejected.

6. It causes a numb, tingling sensation, in the part with which it comes in contact, and also throughout the body.

7. It is powerfully emetic and purgative; produces great nausea and debility, paralysis of motion, occasionally delirium, narcotism, and perhaps vertigo.

8. It may be inferred to cause death in man, as in all other animals, by tetanizing the heart.

IODO-BROMIDE OF CALCIUM.—C. H. Guptill (*Hay's Jour.*, Jan., 1874) reports the case of a woman, aged forty-eight, who was suffering from the characteristic symptoms of exophthalmic goitre, enlarged thyroid body, protrusion of the eyeballs, violent action of the heart, headaches, and nervous disturbances, which proved resistant to treatment with tonic remedies, digitalis, nervines, etc. About three and a half months after he was called, new features —induration of the abdominal muscles, and the muscles of the thighs and lower legs—appeared. At this period, he prescribed the iodo-bromide of calcium in solution, one-half a teaspoonful in water, morning, noon, and night; also, bathing the indurated portions with the same. Improvement at once followed this treatment; the nervous symptoms ceased; appetite and strength returned; and in six months the thyroid body was again its natural size. The patient regained good health, but is still, at the date of writing, compelled to keep up the use of the remedy: the omission of its use for a few weeks only, sufficing to cause a return of some of the symptoms. Dr. Guptill concludes as follows:

"I regard the iodo-bromide of calcium as a very valuable addition to our therapeutical list. It is a decided sedative, as I have seen in other nervous cases. It is an efficient alterative, and is well calculated to meet many conditions where a sedative, alterative, and tonic treatment is required. The effect is very satisfactory where nervous irritability and debility are conjoined with a peculiar cachexia, as seen, for instance, in exophthalmic goitre."

THE FOLLOWING FOREIGN PERIODICALS HAVE BEEN RECEIVED SINCE OUR LAST ISSUE.

COMPLETE *files of the more important, especially among those bearing directly on the subjects embraced in the plan of the* JOURNAL, *are on hand:*

Allgemeine Medicinische Central-Zeitung.
Allgemeine Zeitschrift fur Psychiatrie und Psychisch. Gerichtl. Medicin.
Annales Medico-Psychologiques.
Annales d'Hygiene Publique et de Medicine Legale.
Archiv fur Anatomie, Physiologie, und Wissenschaftl. Medicin.
Archiv fur Mikroscop. Anatomie.
Archiv fur Pathol. Anatomie, und Physiologie und fur Klin. Medicin.
Archiv fur die Gesammte Physiologie der Menschen u. Thiere.
Archiv der Heilkunde: Leipzig.
Archives Generales de Medicine.
Archives de Physiologie, Normale et Pathologique.
Archivio Italiano, per le Malattie Nervosi.
Berliner Klin. Wochenschrift.
British Medical Journal.
British and Foreign Medico-Chirurgical Review.
Bulletin Generale de Therapeutique Medicale et Chirurgicale.
Centralblatt f. d. Medicinische Wissenschaften.
Der Irrenfreund.
Deutsches Archiv f. Klinische Medicin.
Deutsche Klinik.
Dublin Journal of Medical Sciences.
Dorpater Medicinische Zeitschrift.
Edinburgh Medical Journal.
Friedrich's Blaetter f. Gerichtl. Medicin.
Gazette Medicale de Paris.
Gazette des Hopitaux.
Gazette Hebdomadaire.
Jahrbuch fur Kinderheilkunde u. Physische Erziehung.
Jahresbericht u. d. Leistungen u. Fortschritte in der Gesammten Medicin.
Journal of Anatomy and Physiology.
Journal de l'Anatomie, et de la Physiologie Normales, et Pathologiques de l'Homme, et des Animaux.
Journal of Mental Science.
Lo Sperimentale.
L'Union Medicale.

Medicinisches Correspondenzblatt des Wurttemburgisches Aerztliches Vereins.
Medicinische Jahrbucher.
La Nuova Ligur. Medica.
Psychiatrisches Centralblatt.
Psychische Studien.
Rivista Clinica.
Revue des Sciences Medicales.
Revue Scientifique.
Schmidt's Jahrbucher der In- und Auslandischen Gesammten Medicin.
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Cincinnati Lancet and Observer.
Detroit Review of Medicine and Pharmacy.
Medical Examiner.
Medical News and Library.
Medical and Surgical Reporter.
Peninsular Journal of Medicine.
Pacific Medical and Surgical Journal.
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NOTE.—The works marked with an asterisk will be noticed in the Review department of the July number.

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* Illustrations of the Influence of the Mind upon the Body, in Health and Disease. Designed to elucidate the Action of the Imagination. By Daniel Hack Tuke, M.D., M.R.C.P. 8vo; pages, 410. Philadelphia: Henry C. Lea, 1873. Chicago: W. B. Keen & Cooke.

* Principles of Mental Physiology, with their application to the Training and Discipline of the Mind, and the Study of its Morbid Conditions. By Wm. B. Carpenter, M.D., LL.D., etc. London: 1874.

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De la Neuropathie Cerebro-Cardiaque. Par le Docteur M. Krishaber. Paris: 1873. Pages, 259.

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* Die Sensibilitaets Verhaltnisse der Haut. Fuer die Untersuchung am Krankbette. Uebersichtlich Dargestellt. Von Dr. M. Bernhardt. Berlin: 1874. Pages, 25.

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Original Articles, Selections and Translations.

ART. I.—PATHOLOGY OF THE VASO-MOTOR NERVOUS SYSTEM.

LECTURE III.

Active Congestion—What it is—Different modes of explaining how it arises—Critical examination of the modes of explaining the phenomena in question—Does not depend on paralysis of muscular vessels—State of the vessels an active one—Peristaltic action of the small muscular vessels—Proofs of the existence of such a kind of action—Direct—Indirect—How it explains active congestion—Objections to this hypothesis considered.

GENTLEMEN: I am now to discuss questions of the greatest importance, as I think, in a rational pathology. I am to endeavor to explain to you, how *active idiopathic* congestions are produced. By *active* congestions, I do not mean those that are produced by physical obstructions *in* the vessels, such as we see in embolism or plugging of an artery, or in thrombosis, or the coagulation of the blood in the vessels in such way as to plug them, and prevent a flow of blood through them; neither do I mean those congestions that are produced by pressure *on* the veins from without, in such way as to interrupt the flow of blood through them, as often happens; neither do I refer to those congestions which result so often from severe mechanical injuries, such as cuts, lacerations, contusions, or from the destructive action of corrosive chemical agents. In all such cases we can easily see how it comes to pass that the current of the blood in the vessels is interrupted, and the way paved for that unnatural *accumulation of blood* in a part, that we call *congestion*. I mean by the term *active*, those congestions only, which occur suddenly, and do not depend, so far as we can see, on any physical obstruction whatever, to the progress of the blood current. The congestions I refer to, are such as happen while the vessels are yet open and the way free. or even more free or open than is true in the natural state, so far as appearances are concerned. I will give you a few instances of what I mean: Take the act of blushing deeply, or the congestion of the mucous membrane of the nose, or throat, or larynx, or the bronchial mucous membrane, or the congestions that happen in a secreting gland sometimes, or that we see in the skin in many skin affections, and so on. In these and very many other cases met with daily, we have evidence of congestion arising suddenly, and without any apparent cause—certainly, so far as we can see, without any *immediate physical* cause. It is with the *process* of such congestions I desire to deal in this lecture. I have been speaking to you in preceding lectures in regard to the mechanism of the vaso-motor nervous system, and the mode of its action on the muscular vessels. Now I wish to speak of the actions of the vessels themselves, as they are related to the circulation of the blood, whether in health or disease. I do this with the hope that we may be able to develop some rational mode of explaining what we have called active congestion; and surely if we can do this, it will be no small matter, when we come to see what a common and important process congestion is.

But what *is* congestion? It may seem useless to ask this question, for it is generally assumed that most persons know what the word means with sufficient clearness for practical purposes. But though I am persuaded to the contrary, I will not now undertake to define it formally, but will as we pass along. Neither can I refer to the works of various investigators in regard to the circulation and its disorders. It would require almost a lecture in which to give a list of their works and papers. No part of the literature of Pathology is more full.

Before stating the positive facts in regard to congestion on which I desire to fix your attention, I would remark that it is not my purpose in this lecture to speak of inflammation.

It is true that inflammation is congestion — but it is something more. Inflammation without congestion, it would seem, is impossible. An active congestion is the most important factor, and is probably the initial one, in the inflammatory process. After the congestion is fully established, and the case is ready to take on the further or ultimate features in the inflammatory process, other important changes occur in the part which is the seat of the morbid action, such as the extravasation or *diapedesis* of certain of the contents of the congested vessels, etc. But these later changes it is no part of my purpose to try to explain in this lecture. They do not belong, necessarily, to active congestion. I am particular, as you will see, to say just what I mean *not* to include under the head active congestion. There are a few features, however, it does include, which it will be my aim to explain to you if I can. They are:

1. The fact that there is more blood in the congested part than is natural it should have, and the further fact that more passes through it in a given time than should pass.

2. The fact of certain peculiarities in the motion of the blood in the vessels of the congested part, especially such as backward and forward or oscillatory motions, and in some cases, a temporary quickening of the stream, in vessels which are *above their normal size.*

3. The fact that the blood finally ceases to move in some vessels, which, so far as can be seen, are free from physical obstruction, and the further fact, that, in many cases, the circulation of the blood having been stopped, for minutes or even hours, is once again put in motion, in this way leading to what has been called *resolution.*

There are many other important particulars belonging to the history or progress of congestion, about which I will speak in due time. All I wish now to do, is to speak of the *move-*

ments of the blood, and the corresponding *actions* of the blood vessels, and try to account for the same if I can. As to the massing of the blood corpuscles, along next the walls of the congested vessels, and their passage through the walls of the vessels, or *diapedesis*, so well described by Cohnheim and others, and also the increased heat of congested, more particularly, inflamed parts, I will lay them aside now, to speak of them, if at all, in my next lecture. Having stated certain facts in regard to active congestion, it is my purpose to see how they are to be explained. There are several modes of doing this, that have found more or less favor. They may be enumerated as follows:

1. That active congestion is caused by an abnormal attraction as between the tissues and the blood. They attract each other more strongly than in health, and since the tissues are fixed, and the blood movable, the latter is accordingly attracted, or by a sort of *suction*, drawn into the tissues, in greater proportion than should be in health. This is one way of accounting for congestion.

2. Again, it is said by others to depend on physical obstruction to the blood current. This may be either outside of the vessel so as to induce passive constriction, as Henle appears to have supposed, or *internal*, as in case of embolism, or thrombosis, in consequence of which the vessels are plugged.

3. Or, by others it is said to depend on paralysis, and dilatation of the small vessels, and hence the congestion.

4. Or, finally, it has been ascribed to loss or perversion of *propulsive* or *peristaltic* action on the part of the small muscular vessels.

These are the principal modes of explaining the phenomena of active congestion. I will now bring them in the order I have named them, face to face with the facts of congestion:

1. *That active congestion is caused by an abnormal increase of attraction between the tissues and the blood.*

That there is some kind of attraction, exerted between the tissues and certain elements in the blood suited to their needs, I have no doubt. Any kind of attractive power, mutually enjoyed by the blood and tissues, must act, it seems probable, if it acts at all, at practically insensible distances. It would seem to be, in view of such a fact, that the capillaries are so small and their walls so thin, and hence the diffusion of blood so minute and perfect. Such a power as the one in question, can only act between the blood already in a part, and the corresponding tissues. It can have nothing to do with blood coming or gone. It can have nothing—so it seems to me—to do with the *supply* of blood to a part, only with the blood *supplied.* It might tend to retard or delay the current, but could do nothing toward quickening or augmenting it—least of all in turning it backward, as is often observed to be done in the progress of active congestion.

It is required, besides, of this power, that it shall greatly vary in health and disease, becoming so strong as not only to attract an abnormal quantity of blood into the congested part, but actually increase the quantity which may pass through the vessels of an inflamed part, in a given length of time, as compared with other parts around, or the same part in a state of health. How can such a power produce such effects? We can only explain it in this way: by supposing *arterial* blood to be attracted or sucked into the part, while *venous* blood is repelled by this discriminating power. But what proof is there of the correctness of this explanation? What proof have we, indeed, that any such power really exists, as suits the exigencies of the case? I do not know of any. So far as I can see, it is a pure assumption. The only real title to favor it can have, it borrows from its use and perfection, as a means for explaining the phenomena under consideration. But does it explain them? Not at all, or only in part.

What necessity is there for assuming such a cause? None, so long as there are simpler modes of explanation open to us. It explains how blood may be *detained* in a part, but not why more than is natural *goes* to a part; nor how the circulation is increased; nor how an oscillating or to-and-fro-movement of the blood occurs; nor how blood, when once stopped, is put in motion again, as is so often seen in experimental investigations. Evidently we must take, beside this, some other element into our explanation, to embrace all the observed facts. I am willing to admit it may figure somewhat in the arrest of blood in the vessels, and I will admit, also, if such a power as the case requires can be shown to exist, that it may, indirectly, have something to do in determining a flow of blood to a part, on the same principle as that I called your attention to in my last lecture, when speaking of the action of the so-called vaso-dilator nerves, in increasing the flow of blood in the submaxillary gland. The same explanation made then, I would be inclined to make now, if it is proved, as it has not been, that such a *kind* of attractive force as the case requires truly exists.

These considerations seem to me to teach, that we must turn in some other direction for an explanation that will embrace all the phenomena of congestion. Let us now turn to the second mode of accounting for them :

2. *Active congestion is caused by some kind of physical obstruction, either within or without the affected vessels.*

The first objection, and the only one that needs to be made against this mode of explanation, is, that it is inadequate. It does not explain the enlargements of the vessels, nor the peculiar motions of the blood, and so far as observation teaches, does not even explain, in many cases, the arrest of the blood. In many instances of active congestion, there is reason to think, there is no physical obstruction of the kind now contemplated, from the beginning to the end of the congestion. It certainly cannot explain how it comes to pass that the circulation is once more restored in vessels in which it has been for some time arrested. I doubt not, obstruction of the vessels by means of clots, or in some similar way, plays an important part, sometimes, in the circulation, as we find it in congestion. But it is inadequate to explain all the phenomena, if it actually explains any. Let us now turn to the third mode of explaining the facts of congestion :

3. *The phenomena of active congestion are to be explained by admitting a paralysis, and consequent simple relaxation of the muscular vessels, so that they admit more blood than they should naturally do.*

The type of this kind of congestion is seen in experimental physiology, in cases in which the cervical sympathetic is divided in the neck, and immediately afterward the blood-vessels in the corresponding half of the head are enlarged, and contain more blood than they should do, naturally. Under such circumstances the vital activities of the parts seem to be increased, as is evidenced by the increased heat, increased sensibility, and the like. In this case the muscular walls of the vessels are, in a certain sense, paralyzed, relaxed, inactive.

But the congestion that is produced in this way, seldom, perhaps never alone, leads to inflammation, and is more or less permanent—depends simply on the loss of what I described to you in my last lecture as *vascular tonus.* But in the kind of congestion I have called *active*, there is no proof, so far as I know, that there is a *paralysis* of the muscular coat of the affected vessels. But unless this is proved, or at least rendered probable, how can we admit this mode of explanation as the true one? Moreover, if the view I am now discussing be accepted, it must still be objected that it is inadequate to explain the facts. It will explain the dilatation of vessels, and by consequence, a mere congestion, without any reference to its character. But it cannot explain the oscillations of the blood-current, nor how the blood is quickened in the enlarged vessels, as sometimes it is. Nor why, when it has once ceased to move, it is set in motion again. The active congestions I am trying to find an explanation for, in my judgment do not depend on paralysis of the muscular wall of the vessels at all. I rather—at least at this present moment—believe them to be produced through a nervous mechanism, such as I described to you in my last lecture,* when speaking of the submaxillary gland—the phenomena in relation to the nervous supply of which has attracted so much attention. In that case the congestion was certainly not the result of a paralysis of the vaso-motor nerves of the gland. It seemed to be—but was probably not—caused *immediately* by the irritation of the chorda tympani nerve, which was at once followed by the congestion, and an active state of the secretion of the gland. In the congestions that have so often been observed to follow after division of the sympathetic nerves leading to a part, there is but little or no disposition to tissue change, even after months of time have passed. There is an enormous supply of blood, the vessels are greatly enlarged that permeate the part, but no noticeable increase in bulk of tissue, nor of degeneration of the

* April No. of this Journal.

same. This is the passive simple congestion of *paralysis.* Such a congestion has no noticeable tendency to pass into irritative change or degeneration, so long as the animal continues in other respects normal. But in the congestions of which I now speak, the supply of blood is no greater than in the case just mentioned, but from the very first, there is unmistakable activity of tissue change, going hand in hand with the extra supply of blood. But a few hours, and sometimes only a few minutes, after the congestion has made its appearance, the signs of trophic or tissue change become apparent. To illustrate the case, I will recall the phenomena that were mentioned at length in my last lecture, in regard to the submaxillary gland. In that case, if the sympathetic nerves leading to the gland are divided, immediately thereafter the blood-vessels of the gland enlarge greatly, and there is congestion of the gland. But there is no corresponding increase in secretion of the gland, as we might suppose beforehand there would be, since the supply of blood is increased. This I take to be exactly the case with any part in which a simple passive congestion arises, on account of paralysis, either partial or complete, of the corresponding vaso-motor nerves. The vessels of such a part are cut off from their *tonic* centre or centres, by dividing the nerve fibres that connect the vessels and centres in question. The blood continues to go through these vessels as before, only more slowly, perhaps. There is no stasis. This is the uniform experience in experimental physiology. The vessels have simply lost their *tone—vascular tonus.* This is what happens in the side of the head of the animal operated on, when the sympathetic is divided in the neck, as well as in the case of the submaxillary gland referred to.

But if instead of dividing the sympathetic, as was done at first, we irritate the chorda-tympani nerve leading to the submaxillary gland, immediately a congestion arises, quite as pronounced as after division of the sympathetic, but with this difference, at least, viz.: That there was no increase in the secretion from the gland following the one congestion, while in the case of that following irritation, the increase is remarkable. This is a singular and significant difference to my mind. In my last lecture you will remember I endeavored to explain this difference. I will not repeat now what I said then. But I wish to say that this last case is what I mean by *active idiopathic congestion.* I suppose all *active* congestions of the kind of which I am speaking, to be caused in a *similar* though not the *same* way. If this is so, you will see that the mode under discussion of explaining the phenomena of active congestion is, to say the least, inadequate. By just so far as the passive congestion of the submaxillary gland that followed a division of the sympathetic fibres leading to the same, failed to produce an increased secretion, corresponding to the increase in the volume of the blood, by just so much does such a state fail to explain the phenomena of *active* congestion. Besides the mere increase in the volume of the blood, which is as far as this mode of explanation, if we accept it, can carry us, we must introduce some other and different factor or factors into it, to make it adequate.

In the one case, that in which you have divided the sympathetic fibres, you have vascular dilatation, because the vessels are cut off from their *tonic centres*—there is a paralysis or loss of *vascular tonus.* In the latter case — say that in which the chorda tympani is irritated, and vascular dilatation is a result, there is no separation from tonic centres, nor *loss* or *paralysis* of *vascular tonus* — there is simply an *arrest* or *inhibition* of the action of those centres, similar to that which I tried to develop in my last lecture. The two cases are not the same at all, though the results are in appearance partly, and only partly, identical.

If these remarks have any real foundation, you will see that the theory of a vaso-motor paralysis, such as we observe in experimental physiology, as an explanation of *active* congestion, is inadmissible, because not true of them.

All I have endeavored to do, in these remarks, is to show that this third mode of explanation is inadequate. I am desirous of laying particular stress on this, because it has been put forward by some as the true neurotic theory of active congestion and inflammation, and by others it has been combated as an evidence of the impossibility of explaining the morbid processes in question, by a reference to the nervous system. In my judgment, both these parties may be wrong.

But after what has been said, we seem justified in turning to our fourth and last mode of explanation:

4. *Active congestion depends in part, at least, on arrested or perverted peristaltic, or propulsive action on the part of the small muscular vessels, probably both arteries and veins.*

If the small muscular vessels have such a function as the one alluded to, I think it will be conceded that by it we could explain many of the phenomena of active congestion, at least so far as the *movements* of the blood are concerned. But it is seriously questioned whether the vessels referred to have any such function. This being so, it is necessary, first of all, to establish the mode of explanation itself, as probable or true, before we can be permitted to use it as a means of explaining something else. For there is a valuable rule in philosophizing, at least in matters of science, which forbids the use of one hypothesis to support another hypothesis or hypothetical case. The question before us, then, is whether the muscular vessels do really have such a function or not, as the mode of explanation assumes them to have. The majority of physiologists certainly do not admit it. But in regard to such a question as the one we have under consideration, it is not sufficient to shut the door in the face of honest inquiry, to know that some or even many eminent men, have held certain opinions in relation to a matter that may yet be looked upon as *sub judice.* I would have you combine, with a due respect for authority, an abiding sense of independence, in looking at things for yourselves. A well-tempered habit of this kind will be of incalculable value to you in the work of the profession, and in no department of human effort is such a habit more necessary or valuable, because cases in scientific medicine are not settled, as a rule, either by creeds or precedents.

But now to the proofs for and against the possession of such a function for the muscular vessels, and, first of all, those in favor of this view.*

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* Since this lecture was delivered, I have found in the March and April numbers of the *Journal de l'Anatomie et de la Physiologie, etc.*, for 1874, (in a memoir by M. Onimus, on the life and work of Dr. Charles Legros, who died on the 25th of December, 1873,) a statement to the effect that M. Legros, so early as 1866 or 1867, had maintained, that the small muscular arteries have,

The proofs are of two kinds—the *indirect* and the *direct.* The first kind of proof shows that, under the circumstances, a peristaltic action of the arteries *ought* to be true—the second kind should prove it *is* true.

But before considering the proofs to which I have referred, I must make a few preliminary remarks on the circulatory system:

The vascular circle may be divided into five segments, three of which are *muscular* or *contractile*, and the other two are *elastic.*

The first is the *cardiac* segment. It is remarkable for the volume and strength of its muscular walls. It imparts the initial impulse to the blood, in the case of such animals as have it.

It would seem that no one could regard the heart—its strong muscular walls, its valves, and, in particular, its actions, in various animals—without admitting the importance of its agency in the circulation of the blood. Though this is true, yet, so good a physiologist as Dr. J. W. Draper, of New York, has contended that the heart exists only to impart *rhythm* to the movement of the blood, when the fact seems to be that its chief office is a very different one.

The next segment is the *elastic.* It consists of the arteries, both systemic and pulmonary, of the larger class. The middle coat of these vessels, while it contains some muscular

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as one of their functions, that of a *peristaltic* action, so exercised as to aid in circulating the blood. He seems to have been chiefly led to this view by his researches into the structure and mode of action of the erectile tissues of various animals. M. Onimus gives M. Legros the credit of having introduced this view into physiology. I only mention these matters now to say that in this country similar views were expressed by Dr. Hartshorne, of Philadelphia, in an article published in the *American Journal of Medical Sciences*, for 1868. Moreover, I published an article myself in the *Chicago Medical Examiner*, for August, 1866, on the *Functions of the Minute Arteries*, of which my present lecture in the part which follows this note is little more than a repetition. The same views were set forth in a report of mine on *Cerebro-Spinal Meningitis*, made to the *Illinois State Medical Society* in 1866, and published the same year in its transactions. I had already taught a similar doctrine as probable long before the time of publication of the articles mentioned. I only refer to them now as matters of history in this question, and not to set up any claim of priority in the case.

fibre of the unstriped or involuntary kind, which increase in quantity as you pass toward the capillaries, yet it is chiefly composed of elastic tissue, which will stretch and return on itself, as India rubber will do. This elastic tissue could be of no service, seeing that it is essentially passive, unless it is in some way put on the stretch. This is accordingly done by the heart, which fills the elastic arteries not only, but stretches them, and keeps them in this state, with varying degrees of force, during the life of the animal. By this means the elastic arteries are made to store up or *conserve* part of the energy of the heart's action, with which force the arteries in question re-act on the column of blood they contain, so as to force it along. Inasmuch as it cannot pass back into the heart, by reason of the semi-lunar valves which are situated in the great arteries at the point where they spring from the ventricles, it can only pass in the direction of the capillaries. The elastic arteries thus become, in a certain way, efficient in the circulation of the blood. They impart steadiness and continuity to the blood flow, which it would not have from the heart alone. But let it be remembered that nearly all the force these vessels exert, they have borrowed from the heart.

The next segment is muscular, and consists of the small muscular arteries just before they terminate in the capillaries. I have already described them and their relations to the nervous system in an earlier lecture. That they are muscular, and hence contractile, and that they are under the control of the vaso-motor nervous system, I have already tried to show, and all admit. The only question is as to the character of their functions. But this will be the subject of the latter part of this lecture.

The next segment is *elastic*, if anything. It is the *capillary* segment. The walls of the capillary vessels contain neither muscular nor elastic tissue, but still they are, in a certain sense, elastic. They may be regarded as having nothing to do in circulating the blood. They are mere blood-channels. So we may dismiss them from further consideration.

The next, or final segment, is also muscular, and comprises the veins from the point at which the capillaries terminate in them, to the heart. Throughout they are distinctly muscular.

They are furnished, as you will remember, with somewhat imperfect valves, which open toward and close from the heart. The question arises now, for what purpose are the veins so freely furnished with muscular fibre? It is easy to see why the heart should be furnished with muscular walls, and many think it equally easy to see why the small arteries should be. They are said to be furnished with a muscular coat, that, by its contraction or relaxation, it may be the means of regulating the quantity of blood a part shall receive in a given time. But why should the veins receive a muscular coat? It cannot be for the same purpose as that for which the arteries are said to receive their muscular coat, viz.: To enable them to control the amount of blood which a part shall receive. This subject I will shortly return to. I will set before you briefly the proofs that the muscular vessels possess a vermicular or peristaltic action, which may be efficient either in aiding or retarding the circulation of the blood in the small vessels.

I. *Indirect Proofs.*—We know very well how the esophagus, which is a simple muscular tube, is made to propel, even against considerable resistance, its contents, in the process of swallowing, or the opposite one of eructation. It will even convey in this manner a solid object. The same is true for the small and large intestines, the peristaltic motion of which is well understood, and is an example of the kind of action about which I am speaking to you. You have a muscular tube, supplied rhythmically, or otherwise, with fluid or semi-fluid contents, the chief agent in moving which is the muscular wall of the intestine acting in a vermicular or peristaltic manner. This action may be arrested, quickened, or it may be reversed, as in cases of stercoraceous vomiting, in each case modifying the velocity, or even the *course* of the movement of the contents of the tube. There are other cases, such as that of the Fallopian tubes—the ejaculatory ducts, in the expulsion of the semen, or the action of the urethra in expelling the last of the urine, and of muscular tubes in the case of many lower animals, especially in those animals that do not have a heart proper. In all these cases, and many more I might mention, there is unmistakable peristaltic action, with the effect to circulate or propel the contents of a tubular vessel, whatever they may be.

The case of the small muscular blood vessels seems to me perfectly analogous. You have a muscular tube somewhat rhythmically supplied by the heart with its contents, and, if so, we may well inquire why, in the one case, the peristaltic action occurs, and not in the other? Unless it is certainly known, as a matter of fact, that the muscular vessels do not act as we have supposed, what good reason can be given for refusing to admit it in one case, when it is known to happen in many that are parallel with it?

Beside such indirect evidence as I have mentioned, I would refer you to other cases, as, for example, the portal circulation. Here you have the blood transmitted to the intestines by means of the mesenteric arteries. After passing through the capillaries in the wall of the intestines, the blood enters the corresponding veins, which converge so as to form the portal *vein.* So far all is clear. But how is the blood made to pass through the second set of capillaries at the hepatic end of the portal vein? I have never been able to see just how the circulation through the second set of capillaries, in a solid organ like the liver, is accomplished, if not in some such way as I have supposed. Surely it cannot be said the heart propels it, or that it is done by the pressure of elastic tissue, as in the case of the large arteries. My own opinion is that the portal circulation is carried on, in no unimportant sense, by the peristaltic action of the muscular veins, which, in the case of the portal veins, are very muscular, as compared with some others; a fact that does not seem to have been as much considered as it should have been.

If in cases exactly parallel, so far as we can see, such a kind of action does exist, as I will shortly show you, and is sufficient to circulate a fluid, what good reason exists for refusing to admit such a kind of action in this case? Is it because it is unreasonable or improbable, or because a better mode of explanation has been found? Neither, so far as I know. But again, take the placental circulation. How is the blood circulated from the fœtus through the long umbilical arteries *to* the placenta, and then *through* the placenta by the vein to the child again? The first time you get an umbilical cord in your hand, please notice the powerful impulse with which the blood in it is sent along. Do you say it is caused by the beating of the fœtal heart? I do not believe it for two reasons:

First. Because the fœtal heart often beats twice as fast as the cord does. If the pulsations of the fœtal heart are the source of the pulsations of the umbilical vessels, they should be synchronous, but they are not, most certainly. Then the pulse in the umbilical vessels must be won from some other source. But what is that source? The only one I can find is that of the powerful muscular vessels of the umbilical cord, but especially the placenta itself. The muscularity of its vessels is simply remarkable. But why are they so much more muscular than the corresponding vessels *in* the child? I can see very well why they are so. That they may do the work of a heart by powerful peristaltic action, which, in my judgment, is the origin of the impulse we feel when we take the cord between the fingers.

Second. But I have a better reason yet, if possible, than the one I have given you. It is the case of a full-grown fœtus born *without a heart.* I will mention one such case which was recorded by Sir Benjamin Brodie, and is to be found in the volume of transactions of the Royal Society of London, for the year 1809. The fœtus was one of twins, and received the blood from the placenta. It was tolerably well developed, brain and all, except the heart, thymus gland and pleura, all of which were absent. The lungs were very imperfectly developed. The aorta was well developed, but it started from the umbilical artery in the left groin, and extended upward along the front of the spine into the upper part of the thorax where it gave off the two subclavian and afterwards divided into the two carotid arteries without forming an arch. The external and internal iliac arteries of the left side came from this artery in the left groin, immediately after it left the umbilical, and the common iliac of the right side was given off from it after it had gained the normal situation of the aorta. This is not the only case of the kind I could relate. But one is as good as a thousand. The inquiry arises, how was the circulation maintained, not in part, but altogether? I know not how others would explain it, but I would explain it just as I would explain the circulation of the lymph, or the portal circulation. I can see no way but that of a peristaltic movement.

Now, if such a kind of action is competent to carry on the complicated circulation of the *fœtus in utero*, is it unreasonable or improbable such a kind of action should be predicated to explain certain circulatory disorders, such as it is perfectly fitted to explain? Shall we seek some mysterious mode of explaining the anomalies of movement of the blood current in the vessels of an inflamed part, rather than adopt the explanation I am trying to call to your attention? For these various reasons am I inclined to admit, a disordered or perverted, or lost, peristaltic action, as a means for explaining certain phenomena observed in active congestion. But I have other proofs yet, and of the direct kind.

II. *Direct Proofs.*—I will call attention first of all to certain facts, chiefly to be observed among the lower animals. I believe, in the first place, it is a fact that in proportion as the action of the heart becomes inefficient as we descend the animal scale, just in that proportion do the vessels become more decidedly muscular. Why is this so? Is it to regulate the supply of blood to the parts to which the arteries lead? I think not; and I cannot imagine besides this any other purpose this muscular tissue should fulfill, if not to supplement, by peristaltic, a deficiency in cardiac, action. The kind of action I am now speaking about may be seen, and I have repeatedly seen it, in the first blood vessels that appear in the embryos of birds, more particularly chickens. An action that appears to be peristaltic may be seen in the young of frogs, and has been seen either in the arteries or veins by multitudes of observers in various lower animals, from the mollusks on upwards to the highest class of animals. I could cite specific observations of Thomson, Hastings, Henle, Tiedmann, Nysten, Koelliker, Gubler, Wharton Jones, Colin, Vulpian, Onimus, Legros, Schiff, Lœven, Riegel, Bezold, Eulenberg, Landois, Rœver, Saviotti, and many others, all of which show the kind of action of the vessels in various animals I am speaking of. In some of the lower animals — the *annelids*, for example — it is by such action of the vessels alone, that the circulation seems to be carried on, seeing that they have no heart. There can, to say the least, then, be no doubt that there is a kind of peristaltic action of the vessels, the purpose of which is to aid in circulating certain fluids. To the statements already made, I will just add a few passages from the thesis of M. Legros, on the vaso-motor nerves, in which are recounted some of the instances I have been mentioning with others.*

He says: "In the annelids we may see this contraction (peristaltic) perfectly; and we can easily distinguish the dilatations and the contractions which succeed each other in the same point in an artery. These contractions are not only seen in the dorsal vessel of the annelids, which we may liken to a heart, but in all their vessels, the valves in which facilitate the movement of the blood, and impose on it a centrifugal direction. These animals, in reality, have no heart, but have, instead, arteries endowed with the capacity of rhythmical contractions like the heart. M. Faivre, in examining a fragment of the dorsal vessel of the *Dytiscus marginalis*, observed rhythmical contractions continue for more than half an hour. We have often observed these same contractions in the *Nais filiformis*. When in a frog we examine the circulation of the interdigital membrane, we observe, at the moment when the heart is stopped in its motion, that the blood, though diminished in its swiftness, continues for a little time to progress regularly, then we see distinctly a jerking motion, and certain oscillations of the blood current, which are due to the contractions of the arterioles. At the beginning of inflammations, and whenever there is an obstacle in the capillaries, we see these contractions augment.

"In man, when the central artery of the retina is obstructed by a clot, we see, by the aid of an ophthalmoscope, in the arteries through which a collateral circulation must be established, very marked peristaltic movements. These movements are observed also in the healthy eye, if, at the time of the observation, we act on the superior cervical ganglion with the con-

* *Des Nerfs Vaso-Moteurs. These pour le concours de l' aggregation, Par le Dr. Ch. Legros; p.* 112. *Paris*, 1873.

tinuous current, from 14 to 16 elements. We see then perfectly a succession of dilatations and contractions of the arteries at the bottom of the eye. In an observation of Poiseuille, he found that when an artery, yet contractile, is dilated by the injection of a liquid, it re-acts with a force much greater than that employed to pass the injection, which would not have occurred if the elastic tissue alone had re-acted. The same thing which happened in this experiment, must occur with still more energy in the living animal.

"Finally, I have seen with the naked eye, in an animal, the peristaltic contractions of the vessels, such as I have described. In the case of a female dog in the season of heat, I have laid bare an artery of the perineum. In the ordinary state, the arterial pulsations were feeble. But if a male dog should be brought near, and if erection in the female should come on, the artery in question is seen to be at once agitated with pulsations of unusual force, and presenting some well-marked movements of constriction and dilatation.

"I have already said, that in examining attentively that which happens in some canals, the texture of which presents a great analogy with that of the arteries, we observe constantly peristaltic contractions. These rhythmical contractions have been seen in the ureters, biliary ducts, etc. I shall insist only on the action of the lymphatic vessels. Here we do not have the impulsion of a central organ, like the heart, to complicate the phenomena, and direct observation is able to establish the fact. Heller, in 1869, and afterward M. Philippeaux, observed such phenomena, in which there were ten to twenty pulsations in a minute. Similar observations have been made by M. Chaveau. It is true, the rhythmical contractions of the lymphatics are not regular, for the frequency of the pulsations depends on the abundance of the supply of lymph, or of chyle."

It seems to me the facts to which I have referred serve to show that to claim that the muscular vessels act in a peristaltic manner is not to be looked upon as a mere assumption. If it is considered a sufficient answer to the question as to the purpose of the muscular coat of the small arteries, to say that it exists for regulating the quantity of blood a part may receive in a certain time, surely such an answer cannot apply to the veins, that are also muscular, as well as the arteries.

But suppose that we admit that the vessels in question may, and, in some cases, do, act in a peristaltic manner (and this much has been proved by observation), it may be a question whether such kind of action plays any part in active congestion. We know but very little about the class of congestions of which I am speaking, so far as direct observation by the microscope is concerned. Though we meet with them daily in clinical experience, they seldom can be made the subject of direct and prolonged observation. The studies that have been made thus far, have been made in cases produced by the application of local irritants, which differ often according to the character of the irritant in the phenomena evoked. Then, in the cases that have been studied, attention has been given less to the actions of the vessels than the motions of the blood, and the disposition of its corpuscles, etc. The vessels have been noticed, in some few cases, to have a distinct peristaltic action, both arteries and veins, especially the latter. They have always been seen to contract and dilate, either one or the other, but whether in a peristaltic manner, but few observers, comparatively, seem to have tried to determine. So far as direct observations teach, it must be confessed, if taken as they stand, that if they do not militate against, they do not confirm, the view, that in active congestion a peristaltic motion of the vessels is an important factor. I do not myself regard the observations that have been made public up to this time sufficiently extended and pertinent to decide the question as to whether a peristaltic action of the vessels should be admitted to play a part or not.

But while the direct proof, for various reasons, may be meagre or conflicting, we are permitted to apply the mode of explanation I have just been discussing, to the phenomena of active congestion, to see if it can rationally account for them.

By it I think we can explain why so much more blood passes through a part that is actively congested and inflamed, than is natural to it. I know it may be questioned by some whether more blood than is natural does pass through a part. But, for my own part, I have no doubt on this point. But, if true as a fact, the admission of a peristaltic action will, most certainly, and, as it seems to me, naturally, explain it. It also will explain the to and fro movements of the blood, just as truly as a similar kind of action on the part of the œsophagus or intestine will explain the ordinary or the reversed movements of the contents of the intestine. It will explain the failure of the circulation in some cases, such cases as those in which this kind of action is lost, and it certainly will explain how the blood current is started once again in vessels in which it has been for a time stopped, as in those congestions and inflammations that terminate by *resolution.* In explaining such phenomena by this means no analogies are violated, and the means itself is not a hypothetical one, but has extensive actual application as a means in the circulation of the fluids of animals.

But I would not have you suppose that I attach undue importance to this mode of explaining the disorders of the circulation, comprehended under the title of active congestion. Nor would I have you suppose that the other modes of explanation may not play a part in producing the phenomena I have been trying to account for. But I would have you understand that I consider it as highly probable that a disordered peristaltic action is one of the chief agencies in the production of active congestion.

I know this view has often been objected to as untenable. But I have only met with one objection that seemed to me well founded, and that is, that actual observation has failed to establish this kind of action as true. This objection is certainly a formidable one, more so in appearance than reality. Its force is diminished when we remember that such action has really been witnessed in a few cases. Moreover, I do not know that it should always be visible, or even always present. I feel sure it need not be, as some have supposed it must be, synchronous with the heart's action, but the contrary. It need not always be manifested in different cases in the same degree, or even in the same case, and may be absent or lost altogether, as I assume it to be in many cases. If these things are so, we would not expect the views of observers to agree. Then I have reason to think but very few observers, comparatively, have ever given it that determinate attention it requires to be clearly recognized. So, on the whole, various admissible considerations diminish but do not wholly remove the force of this objection. There are some other objections of a more speculative nature, to the view I have set before you, and I may notice some of them in the near future. So much, then, for the chief objections to admitting that the vessels I have described have, as one of their functions, a peristaltic action, and which is supplementary to the heart's action, and that of the elastic arteries. But I put these views forward as partly speculative, and therefore awaiting positive proof of their correctness in a certain measure. I would also say to you, that I do not hold to peristaltic action of the vessels so much as an aid to the *normal* circulation, but rather as a means for producing some of the phenomena of that form of unhealthy circulation we have called active congestion.*

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* Since the above lecture was delivered, I have received by the courtesy of their author, Dr. John J. Mason, of New York City, three papers, the first, entitled "*The Pathogeny of Infarctions, or Congested Patches, which follow Embolism*" (Psychological Journal, April, 1872). Second, "*Experiments on the Tonicity of the Arterioles*" (Psychological Journal, Oct., 1872). And a third, entitled "*Peristaltic Arterial Action, Objections to this Theory.*"

Though they all have some reference to the subject in hand, yet, only the last will be noticed here. Dr. Mason combats the views of Onimus and Legros, as set forth in their work on *Medical Electricity*, and in the thesis of the latter already quoted, in reference to the effect of electrical currents on the arteries, when passed in different directions. These authors maintained that if the current passes along the artery in a peripheral direction, the circulation in it is increased, by reason of an increased peristaltic action of the muscular vessels. Dr. Mason admits the fact of an increased circulation, but denies the validity of the explanation. In order to elucidate the mode of action of the arteries, Dr. Mason performed certain experiments, an account of which follows in the author's own words:

"For injecting liquid into the arteries a spring enema syringe was used, attached to a fine brass canula by a caoutchouc tube, two feet in length, and two millimetres in calibre. The liquid was returned from the vein by a similar canula and tube, from which a deep, graduated glass vessel was filled. Intervals of time were measured by a metronome. In order to produce a wave-like current in the arteries it was only necessary to press suddenly with a hard substance upon the first caoutchouc tube, at intervals corresponding closely with the beats of the animal's heart, and regulated by the metronome. By using two glass vessels, graduated exactly alike, the stream could be directed from one to the other, without stopping to empty the first one filled. The liquid used was milk in all cases, warmed to about the central

But you may now ask me what all this has to do with the vaso-motor nervous system? If it were established that the muscular vessels have the function we have supposed them to enjoy, what relation, if any, has this to the nervous system, especially in active congestion and inflammatory processes? This will be the subject of my next lecture, which will also include an account of nervous trophic action in general.

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temperature of the animal, and the pressure was regulated by the stop-cock of the syringe.

"In order to arrive at the influence of an intermission of the current upon the quantity of liquid flowing from the vein, it became necessary first to establish a constant ratio between the time required to fill the vessel with an intermittent, and that with a continuous stream. Before vivisection, therefore, I found that, with a continuous flow from the first caoutchouc tube, a time elapsed represented by seventeen beats of the metronome, and with the intermittent, thirty beats. The ratio, after several trials, was placed at $\frac{\text{continuous } 17}{\text{intermittent } 30}$. The abdomen of the animal was now opened, the canulæ tied in the vessels, and the quantity of liquid timed as before. The ratio now became $\frac{\text{continuous } 44}{\text{intermittent } 84}$. By the rule of three we should expect less than 77.65 for our denominator, while, in reality, we waited for 84 beats of the metronome before the vessel was filled by the intermittent stream (17 : 30 = 44 : 77.65). With another rabbit the ratio was $\frac{\text{continuous } 75}{\text{intermittent } 152}$ (17 : 30 = 75 : 132.35); 152 beats against 132.35. In some of the experiments woorara was given, while in others no poison was employed. In all cases the animals were unusually vigorous, and the peristaltic movements of the intestines were always to be observed. Practically the result was invariable:

"*The quantity of liquid flowing from the vein, when coming from an intermittent source, was always proportionally less than that coming from a continuous source with or without woorara, and irrespective of the order in which the two quantities were measured.*

"It seems fair to claim, therefore, that something in the vessels of the animal alters the ratio between the quantities coming from the two currents. This something is not elasticity, for, if it were, the figures would be reversed. It is in no way of the nature of an accelerator of the flow. On the contrary, it seems to oppose an obstacle to the stream coming from an intermittent source, which it does not oppose to one coming from a steady, continuous source."

These observations show that the means for rousing the tonic contraction of the muscular tissue of the vessels, is not simply the contact of a liquid, but the mechanical shock, such as is transmitted in case of the intermittent stream, in which the action of the heart is imitated. But these observations do not meet the view I take of vermicular contraction fully. In this case it is held they must be synchronous with the action of the heart, while my own opinion is they need not be, as they are shown by observation not to be, as in the ear of the rabbit, for example. We have not space to discuss the subject farther, but ingenious as the experiments are, which Dr. Mason has performed, I do not see that they constitute an insuperable objection to the views expressed in this lecture.

Art. II. — A CASE OF FACIAL NEURALGIA, TREATED BY REPEATED SECTIONS OF THE BRANCHES OF THE TRIGEMINAL NERVE.

By Edmund Andrews, M.D., Prof. of Surgery in Chicago Medical College.

THE patient referred to in the title of this paper, came from the interior of Illinois, suffering fearfully with facial neuralgia, principally affecting the right inferior maxillary branch. His general health was fair, but he had suffered for some seven years such horrible torture, that once or more times he had made preparations for suicide. He had caused all his right lower molars and bicuspids to be extracted, in the vain hope of alleviating the pain referred to there, and was wholly disabled from business. His daily experience was this: About every fifteen minutes a violent paroxysm of pain would come on, forcing him to desist from conversation and everything else, until it was over. The duration of each attack was, perhaps, two minutes, and during it he attempted to relieve the sensation by violently rubbing the side of his face.

On being carefully questioned, he referred his chief suffering to the distribution of the right inferior maxillary, though he felt secondary pains radiating through other branches of the trigeminus.

As he had long ago exhausted medical resources in the hands of excellent physicians, I proceeded, without much delay, to perform neurotomy. Having anæsthetized him, I raised the masseter muscle, by a horse-shoe flap, from the ramus of the jaw, and, applying a trephine over the course of the nerve, removed a button of bone from the external table of the jaw. The nerve and its vessels lay across the centre of the cavity, and hooking it up, about half an inch of the cord was snipped away with the scissors, and the flap was replaced.

On awaking from his anæsthesia, the patient felt, instead of the paroxysmal pain, a steady aching, referred to the old region, which continued for some time, but the sense of touch in the part was wholly abolished. After some time this pain of the operation subsided, leaving him comfortable for almost the first time in seven years. Such was the established habit, however, of expecting the paroxysms, that it was many days before he began to feel secure in his improved condition. The wound being healed, he seemed a new man, resumed his business, and lived in comfort for one or two years. He then began to have renewed pains of some severity, and a physician living near him, acting on the suggestion of Prof. Gross, (see Gross Surgery,) passed in a tenotome, and cut off the nerve where it emerges from the anterior mental foramen. As might be expected, this produced no effect, and he subsequently returned to me for further treatment. Again carefully questioning the patient, I found that while secondary pains were felt in nearly all parts of the face, the chief one was not in the distribution of the inferior maxillary (which I had destroyed), but in the infra-orbital. I therefore exposed that nerve by raising a horse-shoe flap from the cheek, and opened the front of the antrum with a trephine. I then took a strong pair of dissecting forceps and some other instruments, and picked away the floor of the infra-orbital canal, so as to allow the nerve to hang down like a string into the opened antrum. Carrying this process backward until I had traced the nerve into the orbit behind the eye, I inserted into the orbital cavity a slender scissors, and cut away the loosened nerve. (I was not at that time acquainted with the neater method of working along the floor of the orbit above the antrum.) The wound being healed, the patient again found himself cured, and continued in great comfort and enjoyment for, perhaps, three years. He then re-appeared with a pain, which, though moderate, he desired to have removed. A new examination showed, that the suffering was referred to the distribution of the myloid branch of the inferior maxillary, which leaves that nerve just as it is entering the jaw. The myloid branch runs at first along a groove on the inner surface of the ramus, and then is distributed to the myloid muscles. As the books classed it among the motor nerves, I hesitated, but finally determined to operate on it. I therefore again exposed the ramus of the jaw, and, setting the trephine a little higher, and further back than before, removed a button of bone of the entire thickness of the ramus, and having the posterior mental foramen in its centre, and the groove of the myloid nerve on its inner surface. I now cut away the old stump of the inferior maxillary, and also a segment of the myloid branch. This cured the patient a third time. Some two or three years later, his physician wrote that there was a partial return of his troubles, and that he desired another operation. Not seeing, exactly, what further operations could be performed, I wrote, advising that a full trial of arsenic be made. This was done, with the effect of curing him for a year or two more, but at the end of that time the pain returned, somewhat, and would not yield to the arsenical treatment, and the patient begged for another operation. The pain was referred to the distribution of the same inferior maxillary nerve, which I had operated on years before, suggesting the idea, that the organ had been reproduced across the gap made by the operation. An examination, however, showed that the paralysis of sensation still remained in the parts, notwithstanding the referred pain. Further investigation showed that the paroxysms were excited by movements of the muscles of deglutition, among which the stump of the old exsected nerve still remained, so that efforts at swallowing, etc., were often painful. I concluded that the trouble consisted in an adhesion of the stump of the divided nerve to the muscles of the region, and, though the pain had nothing like the original severity, I consented to make another effort for its relief. I, for the third time, exposed that ramus of the jaw, and, going through it with the trephine, laid bare the tissues beneath. I soon found the stump of the nerve, and, following it upward as far as possible, I snipped off all I could reach. This cured the patient for the fifth time. Some six years have now elapsed without my seeing anything more of him, but I have occasionally heard that he continued well. I should state that, during the last operation, I determined to ascertain the condition of that part of the nerve which lay in the dental canal on the distal side of the gap made by the operations. I therefore extended my incision along the body of the jaw, and, exposing the bone, I removed the external table forward to the anterior foramen. Thorough exploration showed that no trace of the nerve nor of its canal remained, the whole space, even to the inner table, having become filled with cancelli or bone, proving that, in this case, at least, the return of the pain was not due to the restoration of the continuity of the nerve across the gap made by the operation.

One important observation in this class of desperate cases is, that when the difficulty returns after a thorough operation, it is not generally in the same branch of the nerve, and, when it seems to be so, it is, at least sometimes, due to the entanglement of the stump of the cut nerve in a cicatrix subjected to traction by the adjacent muscles.

Another lesson is the old one, that the true seat of irritation in facial neuralgia, is usually in some of the numerous bony canals through which the branches of the trigemini pass, and that the operation must always reach the nerve on the proximal side of these bony foramina, whose rigid walls, in a state of disease, may compress the nerve and its accompanying artery, and thus, perhaps, in some cases, produce the pain.

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Art. III.—On the Pathogenesis of Hemicrania.

By Dr. Oscar Berger, Docent in Breslau.

Translated from Virchow's Archiv, LIX, III and V, 315, by H. Gradle, M.D.

It is well known that Du Bois Reymond* has, on the basis of observations made upon himself, emitted the hypothesis that the painful paroxysms in hemicrania, in many cases, depend on a tetanus of the muscular fibres of the vessels of the affected side, or a tetanus in the region of the cervical sympathetic of the corresponding side. The pressure which the convulsive contraction of the muscular coats of the vessels produces on their sensory nerves may be the most immediate cause of the

* *Archiv. Anat. Physiol.*, 1860, *p.* 461.

pain, as in the case of the painful sensations in the striated muscles for example, in cramps of the limbs, or in electrical tetanization, and in the unstriped fibres of the uterus and the intestines in labor, and in colic. The phenomena during the attack which favor this theory are comprised in the condition of the temporal artery, which is felt as a hard cord on the affected side, while on the other it retains its normal condition, in the bleached appearance of the visage, the sunken eye, and especially in the dilatation of the pupil on the painful side of the head. While these phenomena plainly point to a lasting contraction of the vascular muscles of the affected half of the head, that is to an irritated condition of the cervical sympathetic, the reddening of the conjunctiva, and the lively sensation of warmth and reddening of the ear with an elevation of its temperature perceptible to the hand, indicate a secondary relaxation of the vessels. The variations of the blood pressure in the brain, which would be the result of an intermittent tonic convulsion, now increasing and then again relaxing the muscular coats, can explain, according to Du Bois Reymond — as in Wollaston's theory of sea-sickness — the nausea accompanying migraine, and perhaps, also, the frequent flashes before the eyes. Eulenberg and Guttman* have moreover observed in several perfectly typical cases, towards the close of the attack, in connection with the flushing and rise of temperature, a decided contraction of the pupil of the affected half of the head, corresponding to the secondary paralytic condition of the cervical sympathetic. Brunner† found in his own case, besides the symptoms given by Du Bois Reymond, also a pronounced sensitiveness to pressure in the region of the upper, and sometimes, also, the middle cervical ganglion. Du Bois ‡ also adds that he has found the spinous processes corresponding to the cilio-spinal region, painful to pressure during and after the attack. Such cases, for which Du Bois has proposed the name, "hemicrania sympathico-tonica," are not to be considered as neuralgias in the usual sense of the

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* *Die Pathologie des Sympathicus. Berlin*, 1873, *p.* 20.

† *Zur Casuistik der Pathol. des Sympath. Petersburger Medic. Zeitschr. N. F. Bd. II*, *p.* 260.

‡ *l. c.*, *p.* 466.

word, either of a peripheral sensory nerve (of the first branch of the trigeminus), or of a sensory tract of the brain, but as a disease of the cervical portion of the sympathetic or its spinal centre. I myself have observed several cases of hemicrania, which offered in the most striking manner a complicated series of symptoms that perfectly filled all the indications of Du Bois, and for these cases I first recommended the nitrite of amyl as a palliative*: Later, Vogel and Holst,† the latter on himself, have also employed the same agent with good results. I have, however, observed a dilatation of the pupil only twice in six such cases when I had the opportunity to see the attack myself; on the other hand I have in no case seen the secondary contraction.

Some eight years after Du Bois Reymond's communication, Mœllendorff ‡ offered a theory of hemicrania, acccording to which it is due to "a partially typical, partially atypical debility of the vaso-motor nervès governing the carotid arteries whereby the artery is relaxed and an arterial fluxion to the cerebrum is established." Mœllendorff also considers an arterial hyperæmia, a dilatation of the vessels in the region of the cervical sympathetic, in opposition to Du Bois Reymond's observation of the phenomena of vascular contraction, to be the cause of the hemicrania, and brings forward in favor of his views, the following statements: the compression of the carotid of the affected side causes, "as if by magic," a temporary cessation of the pain, which returns again, it is true, the moment the pressure is withdrawn. On the other hand, the compression of the carotid of the sound side increases the pain. The ophthalmoscope affords a striking proof of the increase of blood-pressure by dilatation of the vessels. Ophthalmoscopic examination of the eye on the side of the hemicrania, during the attack, shows a dilatation of the central as well as of the choroidal vessels, so that the fundus of the eye appears of a bright scarlet color, while the other eye shows the normal dark brownish-red background and the usual conditions of the central arteries and veins. Frequently,

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* *Berliner Klin. Wochenschr.*, 1871. *No.* 2.

† *Dorpater Medic. Zeitschr.*, 1871. *Bd. II*, 261.

‡ *Ueber Hemicrania, Arch. f. Path. Anat. Bd.* 41, 385.

moreover, there was to be remarked a strong injection of the episcleral vessels around the corneal margin during the attack. Mœllendorff noticed, in addition, during the attack, a marked slackening of the heart's pulsations (a pulse of 48–56 in a minute), the radial artery appearing small and contracted, while the carotids remained soft and full. The narrowed, contracted arteries of the extremities explain the icy coldness of the hands and feet during the attack, often not to be relieved by any attainable means, and the frequent shiverings, together with a subjective feeling of increased temperature in the affected side of the head, and of warmth in the interior parts. According to Mœllendorff, the pupils are both equally contracted during the attack. We must, therefore, with the hemicrania sympathico-tonica, admit a hemicrania sympathico-paralytica, for those cases in which we observe the phenomena of vascular paralysis instead of vascular contraction. The slackening of the pulse, related by Mœllendorff, would, then, according to the experiments of Landois, who observed cerebral hyperæmia artificially produced by stoppage of the venous outflow, be due to a direct irritation of the medulla and the vagi.* By an irritation of the medulla, as the chief vaso-motor centre, all the above briefly given phenomena of the attack of hemicrania may be explained, and the increase of salivary and urinary secretions which takes place, according to Mœllendorff, as well as the plethora of the abdominal organs in persons suffering from migraine, would be owing to a secondary relaxation of the vessels. In the cases of hemicrania sympathico-paralytica, we must seek for the decision of the question as to the peculiar cause of the pain, another explanation than the one given by Du Bois for his case. It is certainly not hazardous to admit as the cause of the irritation the hyperæmia itself, and the consequent pressure of the extended vessels on the nerve fibres and ganglion cells. Eulenberg and Guttmann consider it, and I think correctly, as a sufficient explanation of the cases of hemicrania sympathico-

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* Mœllendorff himself states that the slowing of the heart-beat finds its explanation in the "stroke experiment of Goltz, according to which inhibition of tonus in a vascular tract produces as its consequence, diminution of the activity of the heart."

tonica, that in the variations of the arterial blood supply, especially in the temporary anæmia of the side of the head concerned, we find the cause of an irritation of sensory-cranial nerves, either in the skin, the pericranium, or the brain itself, and thus the starting point of the attacks of pain. In a similar manner they also suppose the transient increase of blood pressure in hemicrania sympathico-paralytica to produce the pain, so that the "the local anomalies of the circulation are to be considered as the essential and general movement in the etiology of migraine." *

I have to thank the courtesy of my colleague, E. Stern, for the observation of a case of hemicrania sympathico-paralytica, which leaves nothing to be desired in the matter of clearness and completeness of its symptoms. If any doubt yet remains as to the general correctness of the description of Mœllendorff, our observations prove beyond question that cases of migraine occur in which the cause of the affection is a primary paralysis in the region of the cervical sympathetic of the affected side.

Frau Apotheker H., of this place, aged forty-five years, gives the following history: Her father died at the age of seventy-four from old age; her mother at twenty-six, of consumption; several sisters also died of the same disease; no history of nervous disorder in the family. The patient was, as a child, quite delicate, and suffered from the various symptoms of scrofula, till her fourteenth year; commenced to menstruate at thirteen, the monthly period always appearing regularly; at this time she was chlorotic for several months. At twenty-six she married, and has been confined three times, the last time only two years ago. Since the last confinement the catamenia have occurred very irregularly—every six or eight weeks. In her seventeenth year she suffered, it is stated, from fright, from a severe attack

* Even in the older authors we find descriptions given of a series of the phenomena of hemicrania which are now so important in the study of the pathogenesis of the complaint. Fordyce (*Historia febris miliaris et de hemicrania dissertatio, London*, 1758) shows that the eyes during the severe paroxysms are sunken in their orbits and appear smaller. Monro states (*Prelect. med. ex Cronii Instituti*, 1771, 1755, *London*, *p.* 59) that the eye of the affected side in hemicrania is commonly much reddened and suffused, and often, as it were, contracted. Tissot (*Die Nervenkrh.*, *Uebers. von Ackermann*, 1782, *Bd. III. s.* 509) says that the temporal and frontal arteries become extraordinarily tense, and the face, which toward the end of the paroxysm becomes sometimes bloated, is intensely hot. Josef Frank says, among other things, "the temporal artery is seen as a tense cord, and here and there are seen ecchymoses on the eyelids, the forehead, and the cheeks." (*Nervenkrh. Uebers. von Voigt*, 1843, *I*, *s.* 129). Both of the last named authors mention also local disorders of perspiration. Tissot also reports the case of a woman who suffered from an intense migraine on the right side of the head, and never perspired on that side of her face.

of fits of convulsive laughter, which was afterwards repeated, as well as also attacks of convulsive crying, and obstinate singultus. Since her eighteenth year she complains of frequent headaches, which previously had only occurred after bodily or mental over-exertion or excitement. At these times the constant prodromatic vaso-motor symptoms which preceded the peculiar pain were very striking; the patient turned always very red over the whole face and on both ears, experiencing also a lively sensation of heat, whereupon she first began to feel a sensation of dull, steady pressure over the whole head, but especially on the frontal region. The symptoms were always palliated by cold applications. The statement of the patient is characteristic, that she could not attend balls, because in a little time these vaso-motor symptoms appeared in a manner intensely embarrassing to her. From her youth she had possessed, moreover, the peculiarity of the skin that a slight touch or pressure, or anything of the kind, on any part whatever, produced a redness lasting often for an hour or more. For about ten years, now, these phenomena of "vaso-motor cephalalgia"* had been appreciably altered. While the headache formerly only occurred from some special provocation, it now became much more frequent and intense, with all the clinical symptoms of true migraine, while the vaso-motor disturbances now became limited to the right half of the head, and were accompanied with oculo-pupillary symptoms on the affected sides. The slightest cause, especially any light bodily exertion, as, for example, when the patient walked two hundred steps (even very slowly), or when a draft of air or direct sunlight struck the anterior part of the face, or after a short stop by a warm stove, or the drinking of any warm fluid, and also from the slightest irritation, and always without exception at the monthly period, with special intensity, produces the following appearances: Above the right eye-brow there appeared a somewhat rounded red spot about the size of a small coin, in a few seconds extending inwards; soon after, or simultaneously, a similar red spot shows itself on the right cheek (under the lower border of the malar bone), the right ear also becoming flushed, and in from two to four minutes this redness, at the beginning so limited, extends itself over the entire half of the face, accurately limited at the median line. The patient feels an intense heat in the reddened parts, and the right ear especially is "glowing hot." In rare instances these symptoms disappear in ten or fifteen minutes, and then comes on a kind of fullness of the head and flashes before the right eye, but in most cases they are followed by an attack of hemicrania, lasting several hours, in which, with a continuous very painful "dull boring" in the right side of the head, especially in the forehead and temple, and a sensation of intense warmth inside the skull, severe lancinating pains are felt, and always nausea, sometimes vomiting occurs, and the patient is compelled to lie down and remain perfectly quiet, every noise, even loud conversation, etc., causing so much irritation that she is obliged to cry. After the attack she is very much exhausted, and when the flushing of the right side of the face has gradually disappeared with the departure of the pain, the countenance is noticed to be unusually pale. During the paroxysm there exists, to a high

* Alb. Eulenberg proposes this designation for the form of headache dependent on vaso-motor disturbances.—*Berlin Klin. Wochenschr.*, 1873, No. 15.

degree, hyperalgesia of the whole right side of the head and face, so that the slightest contact is painful. Sometimes precordial pain, and the sensation of irregular pulsation of the heart accompanies the attack; the patient often complains of a feeling "as if the blood must come into the throat," and "as if a hot blast passed over the whole body." Coldness and shivering are never present, and the flushing of the right side of the face is never preceded by the stage of pallor. The patient further stated of her own accord, that with the vaso-motor symptoms, which she had often watched herself in the glass, there were always to be remarked certain alterations in the right eye, which appeared in the paroxysm as if "pinched together"—contracted—and the right pupil narrowed. Cold applications now and then mitigated the pain, but often she believed they aggravated the burning. During pregnancy, and in the puerperal state, the attacks were more frequent and severe than at other times. The patient is a moderately powerfully built woman, and when well has a rather blooming appearance, with both cheeks equally rosy. I have had repeated opportunities to examine her satisfactorily during the attack, when she offers a strikingly peculiar appearance. The whole right side of the face is of a uniform scarlet red hue, sharply bordered at the median line, and gradually decreasing, the tinge extends in lesser degree over the right side of the neck, in front as far as the upper border of the third rib, and behind to a point one or two inches above the spine of the scapula; the right ear is intensely reddened. A certain amount, also, of *ephidrosis unilateralis* is regularly present, and in the right frontal and maxillary regions some scattered drops of perspiration are noticed, with perfect dryness of the left side. The temperature of the right side of the face and the right ear appears to the hand perceptibly heightened; with two similar thermometers applied in the same manner, the difference in favor of the right side, on different points of the cheeks, was from 3—4.5 C. (= 5.4—8.1 Fahr.) in the external ear, 2—4 C. (= 3.6—7.2 Fahr.), and in the mouth from 0.5—1 C. (= 0.9—1.8 Fahr.). The right temporal artery is plainly dilated, and its pulsations are noticeably quickened; in a lesser ratio the right carotid pulse is fuller than that of the left. The right conjunctiva is likewise more injected than the left, and the lachrymal secretion increased, while the nasal mucous membrane and salivary glands appear to be unaffected. The urinary secretion shows nothing of special interest; the urine is free from sugar and albumen, its specific gravity 1.025. In all cases of the intensity just described, the statement of the patient in relation to the oculo-pupillary symptoms is seen at once to be perfectly correct. The space between the lids of the right eye is strikingly narrowed; the eyeballs seem retracted; the right pupil is contracted. The greatest vertical diameter of the right inter-palpebral slit, when opened without exertion, is about seven millimetres; the left, ten. When opened as much as possible, the upper border of the cornea is covered by the lid. Although no difference can be detected by the compasses in the horizontal diameter of the inter-palpebral spaces, still the right eye appears somewhat diminished in size, since its outer canthus is slightly drawn downward. The difference between the pupils varied with the different attacks; some times the right pupil was more than one-half smaller than the left; sometimes only ½—1 line less; still *myosis* of the right side could always be distinctly seen, and simultaneous shading of both eyes made the difference still more apparent. The right pupil was tolerably round and its re-action to light almost destroyed. An attempt to dilate it with atropine failed. I could determine no disorder of sight, particularly no alteration of accommodation in any instance. The co-ordination of the upper lids with the visual axis was perfect. Herr Privat Docent Dr. Hermann Cohn has had the kindness to make an ophthalmoscopic examination, and I give here in brief the results of his, in all points, very thorough examination. Still I must first state that at the time this examination was made, the condition of the patient had considerably improved by some weeks' careful treatment, and that the vaso-motor symptoms were not as severe as they had been, though still quite pronounced. The examination showed a perfectly normal condition of the retina and optic nerve, and no difference on either side as to the appearance and fullness of the vessels. Median of both sides absolutely clear; both eyes emmetropic; vision left, 30-30; right, 30-40; the right eye about one-fourth line less open than the left. Ordinarily the upper lid encroached upon the border of the cornea, on the right side about one line; on the left, three-fourths of a line; opened to the fullest extent the right upper lid was about one-fourth of a line lower than the left; right pupil one-fourth of a line narrower than the left; round; both pupils not easily changed by daylight. It appeared remarkable that the re-action of either pupil by lamp-light was much worse than by daylight, so that scarcely any contraction ensued; field of vision and appreciation of color normal. Examination, also, with Foerster's apparatus for measuring the perception of light, gave no difference in the two eyes. Just as little was there any mentionable difference in the range of accommodation. The pulse was occasionally somewhat decreased during the attack (60–64 in a minute); toward the close it was quickened; usually it was normal. The radial pulse seemed to be of normal volume and tension; there was no difference in that of the two sides. During the paroxysms, as I myself had the opportunity to observe, I could never produce, by compression of the carotids on either side, any noticeable alteration in the symptoms. Besides, it was impossible to maintain compression on the right carotid for more than a few seconds, because pressure at the point on the inner margin of the sterno-mastoid corresponding to the artery, is very painful, a most extremely painful sensation being always produced by deep pressure in the auriculo-mastoid fossa of the right side, corresponding to the upper cervical ganglion of the sympathetic (between the ascending ramus of the jaw and the mastoid process); even moderate pressure on this point was declared painful by the patient, while no trace of any such sensibility is observed on the opposite side. The locality of this sensitiveness is to be sought deep in the tissues, since the skin showed no such abnormal sensibility. In like manner the spinous processes of the seventh cervical and first dorsal vertebræ are exceedingly sensitive to pressure and percussion, while the strongest pressure on the upper cervical and lower dorsal vertebræ produces no pain. Some of the lumbar vertebræ are, it is true, sensitive to pressure, but to a much less degree than those above mentioned. Also, in the intervals between the attacks all these localities were sensitive to pressure, but in all cases less so than during the paroxysms themselves.

There is no doubt but that we must refer the above described phenomena, in connection with the vaso-motor and oculo-pupillary symptoms of the attack, to the right cervical sympathetic, and we are perfectly justified—as exceptionally in other cases —in considering in our observations, the sensitiveness to pressure of the cervical sympathetic as an important and interesting nosological symptom. The condition of the cutaneous sensibility during the attack seems to be of general interest. Repeated methodical examination of the patient gave the constant astonishing result that the right side of the face in the described condition of intense fluxionary hyperæmia, showed a notable increase of its cutaneous sensibility, and, indeed, not merely a cutaneous hyperalgesia, that is, increase of common cutaneous sensibility, but we always found the most certain evidence of an exaltation of the special sense of tact in all its qualities. This hyperpselaphesia (from Ψελαφάω, to touch), as Albert Eulenburg has denominated the pathological exaltation of the tactile sense, is an extraordinarily rare symptom to be determined with certainty. Among many hundred cases of the most various affections of the nervous system in which I have made careful tests of the cutaneous sensibility, I have never before observed it.

Without going into details I will only add, that the comparative examination of both sides of the face also gave an unusual sensibility of the sound side, but always only slightly over the empirically established limits, so that it must yet be considered within the physiological extremes; thus, the examination of corresponding spots on both sides yielded the following figures:

	Right.	Left.
Diameter of tactile circles	1 line.	4 lines.
Perception of differences in temperature	0.4° C. 0.72 F.	0.8° C. 1.44 F.
Electro-cutaneous sensibility (minimum)	160 mm.	120 mm.
	(Polar distance.)	

Though examinations at different times yielded a different absolute value, an augmentation of cutaneous sensibility of the morbid side was an unexceptional result. Even with coarser modes of procedure, such as contact of the skin with a hot or cold object, pricking with a needle, etc., was a more delicate perception observed by the patient on the affected side, which was complicated with an excessive hyperalgesia during the paroxysms. The electro-muscular contractility of the facial muscles was not altered on either side at any time. The same could be said of the special senses of sight, taste and hearing. Neither could any difference in the cutaneous sensibility of other parts be found. During the intervals the patient's health is fair, though she complains of frequent backache and general weakness. The pulse is normal in frequency and volume; the heart healthy; the thyroid gland of normal dimensions. No difference can be detected in the state of nutrition of either half of the face.

The patient, whose complaint had not materially changed in ten years, was subjected by me to electrical treatment. During 2—3 seances per week, each of but 1—2 minutes duration, I galvanized the right sympathetic, placing the cathode on the inner margin of the sterno-cleido-mastoid muscle in the auriculo-mastoid fossa (corresponding to the superior ganglion of the cervical sympathetic), the anode on the first dorsal vertebra, and alternately closing the current and changing its direction, for which purpose the battery consisted of 10—15 Siemen's elements. The result of the treatment was surprising as well as interesting. After 6—8 sittings the patient testified to a marked improvement, as the attacks had diminished in intensity, while the violent migraine proper had disappeared almost completely. The redness of the right side of the head at first still came on frequently, but was never as intense, and always without cephalalgia. It is now four months since the commencement of this treatment, and even the menstrual period, previously never passing without migraine, has lately spared the patient her agony. The redness of the right side, though not yet entirely absent, is observed only on special excitement, and is always moderate and confined strictly to the right half of the forehead. The oculo-pupillar symptoms have disappeared up to a faint trace. The patient has repeatedly, even on very warm afternoons, taken with impunity walks of one-fourth of a mile and more, while previously a walk of scarcely 200 paces would bring on a severe attack, especially in warm weather. On getting warm at present, the entire face becomes uniformly red and is covered with perspiration, in the production of which the right side still bears the greater part, while formerly there was always torpidity of the sudoriparous glands.

It was noticed that, notwithstanding the short duration of the current, an erythematous rash was developed on and around the spots corresponding to the poles, lasting for some hours. This high vulnerability of the cutaneous vaso-motor nerves, which, as the history showed, was not of late origin, could be easily demonstrated on any part of the skin by any mechanical, electric, or thermic irritation, though the phenomenon was most marked on the right half of the face and neck; the hyperæmia was not preceded by any pallor indicative of vaso-motor spasm. An immediate influence, however, of galvanization, especially on the pupil, was never observed definitely.

An examination undertaken together with my colleague, Dr. Stern, a few days ago, confirmed the total absence (for months) of the migraine; at the same time the patient stated that her period is now recurring every two or three weeks, while formerly only once in twice that time. Within the last eight days she has suffered from frequent pains in the occiput, and from vertigo and vomiting, without, however, any vaso-motor symptoms. Finally, she affirms, that within the last few weeks, thin copious stools appear at a definite hour in the morning; while, during the day they have ceased entirely, not even being provoked by errors in diet. Dr. Stern also found a slight tumefaction of the portio vaginalis uteri, especially the anterior labium.

The above case ought certainly to excite interest, for several reasons: The symptoms described are in such harmony with the results of experimental research, that no doubt can be entertained as to their interpretation. Evidently a paroxysmal disturbance of function of the right cervical sympathetic, corresponding in all details to section of that nerve, is the cause of the appearances. The redness and augmentation of temperature, as well as the heightened activity of the lachrymal and sudoriparous glands, is due to a paralysis of the vaso-motor fibres in the course of the cervical sympathetic, and the partial closure of the lids and contraction of the pupil to paralysis of the oculo-pupillary filaments, as experiments of Bernard, twenty years ago,* (and previously those of Pourfour du Petit and Biffi, on pupillary contraction,) have shown. It is well known that in the tract of the cervical sympathetic the fibres innervating the dilator pupillæ are found, while the diminution in size of the interpalpebral space can be traced to the paralysis of certain external ocular muscles (Mueller's musc. orbitalis, and the unstriped muscular fibres in the lids, likewise discovered by the same), innervated by the sympathetic nerve. The slackening of the pulse, occasionally observed during a paroxysm, can be explained by an irritation of the (cardiac) inhibitory centre, by the intercranial vascularity. In what way, and by what causes, the condition of the sympathetic producing the above symptoms was induced, I will not attempt to demonstrate theoretically; but certainly the *constant* tenderness of the cervical sympathetic pointed to a pathological state of the same. Disease of the cervical sympathetic, with a similar train of symptoms, sometimes from compression of the nerve, rarely from traumatic lesions, has been, recorded in a few instances.† Thus, cases of myosis paralytica have been observed, when caused by glandular swelling on the neck (Willebrandt), aneurism of the aorta, the carotid and the innominate artery (Gairdner, Coates). Special interest is connected with a case of paralysis of the cer-

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* In the orbital aponeurosis, Sappey, (*Archives General*, 1868, Jan., p. 104,) has also described unstriped muscles aiding in the protrusion of the ball of the eye, and supplied by the sympathetic.

† Eulenburg and Guttman, l. c., p. 1–16.

vical sympathetic, lately reported by Ogle,* in which, after the healing of a deep cervical abscess, vaso-motor and oculo-pupillary symptoms indicative of sympathetic paralysis appear, occasionally accompanied with a slight headache of the right side. Ogle believes that the abscess destroyed a large portion of the nerve; besides, he cites a case of section of the right sympathetic during an operation, followed, the next day, by unilateral facial congestion and contraction of the corresponding pupil.†

A very interesting case of gun-shot injury of the right sympathetic is also mentioned in the well-known monograph of Mitchell, Morehouse and Keen, on injuries to nerves. Injuries of the cervical cord and brachial plexus have also been observed to occasion similar disturbances.‡

Our case, therefore, with its symptoms in the region of the sympathetic, is not a special curiosity, even if such an intensity and extension of the vaso-motor phenomena is of rare occurrence; its importance depends mainly on its connection with the hemicrania, the pathogenesis of which affection—so definitely clinically illustrated—is as clear and well marked as in any other cases that have come to my knowledge. The objective appearances of vascular dilatation were constantly first developed, and only after these had reached a high degree did the hemicranial paroxysm come on, the intensity of which was, *cæteris paribus* directly proportional to the heighth of the vaso-motor disturbances; in case these were but slight, merely a dull headache and no true migraine followed. Never was the attack preceded by a stage of vascular spasm.§ With a show of probability, amounting almost to certainty, we can claim as

* Med.-chir., transact. 411, p. 151, 1869; and Schmidt's Jahrb., vol. 145, p. 149.

† *Gaz. des. Hop.*, No. 64, 1868.

‡ Hutchinson, *Med. Times and Gaz.*, 1868, p. 584; Seeligmueller, *Berl. Klin. Wochensch.*, 1870, No. 26; 1872, No. 4. Bernhardt, *Berl. Klin. Wochensch.*, 1872, No. 47, 48.

§ Mœllendorff (l. c.) speaks of a hyperenergy, of short duration, of the vascular musculature, preceding the anenergy. According to him, the excitation of the muscular tonus of the vessels, by alcohol, offers a remarkable analogy to hemicrania, since cephalalgia never appears during the drunkenness, but only after its effects have ceased and the pleasant stimulation has become replaced by disagreeable relaxation.

the cause of the pain an intercranial active hyperæmia, corresponding to the external visible vascular paralysis, although it is uncertain to which special part of the sensitive nervous system the irritation is directed. If, therefore, our observations confirm that migraine may be dependent on unilateral intercranial fluxionary congestion, (corresponding to primary paralysis of the cervical sympathetic,) such a conclusion is still more justified by the effect of the treatment directed against the diseased sympathetic. This is my first case amongst a large number of diseased states, in which galvanization of the cervical sympathetic, empirically employed, has been followed by marked success. Some years ago I pointed out the negative results obtained by me from this therapeutic procedure in hemicrania, and only by this late case has my former opinion been corrected; it is true, however, that amongst the previous observations, there was no instance with such marked symptoms of sympathetic paralysis.*

Holst was the first to introduce—on the strength of about 30 cases—a galvano-therapeutic procedure in methodic manner (analogous to Brenner's polar method), adapted to the different varieties of migraine. In hemicrania sympathico-tonica he recommends us to apply the anode to the sympathetic, the cathode being seized by the hands, and to diminish the current gradually during about 2–3 minutes; on the contrary, he advises (in the paralytic form) to place the cathode on the sympathetic, and to induce an energetic excitation of the nerve by frequent interruption and change of direction, while the application of the anode is intended to diminish a pathological irritation. Since Holst assumes as the cause of migraine, a morbidly increased irritability of certain nervous sections, he employs in most instances the first-mentioned treatment, and, as his observations show, mostly with good success.†

At any rate we are not to forget that a depressing action of the anode on the cervical sympathetic is yet without physiological proof; in fact, galvanization of the human sympathetic is as yet a procedure so imperfectly understood that we must

* *Berl. Klin. Wochensch.*, 1871, No. 2.

† *Ueber das Wesen der Hemicranie und ihre electro-therapeutische Behandlung nach der polaren methode. Dorpater Med. Zeitschrift*, 1871. *Bd. II, p.* 261.

be very cautious in applying to its elucidation certain deductions from electro-physiology, and the more so as an experimental basis for the distinction between the action of currents of different directions does not exist. Certainly a primary electric excitation of the sympathetic is decidedly more probable than an immediate paralytic influence of the current. However, I wish to discourage further attempts in the manner of Holst, as the physician is justified in using means (not injurious), even if their *modus operandi* has yet escaped clear definition. The tonic form of hemicrania has not as yet been treated successfully by me with galvanism. Eulenburg and Guttman* recommend in cases coming under the angio-paralytic division, the use of extract secalis cornu. aquos. (ergot), which exercises a contracting influence on the vessels (according to Vogt, through the intervention of the vaso-motor centre), and which Woakes† has employed with good results in migraine. Durable and progressive improvement was observed by Eulenburg in a case lately reported by him of paroxysmal vaso-motor disturbances —periodical atony of cranial vessels, attended with cephalalgia —after large doses of the drug—0.6—0.9 gramme [9-14 gr.] per day—had been continued for some time.‡ I also treat at present two ladies suffering from violent hemicrania, with angio-paralytic symptoms—by subcutaneous injections of ergotin—0.12—0.18 gramme [2–3 gr.] extr. ergot. aquos. per dose—and, as it seems, with good success. Also, in an inveterate case of tic-douloureux, and several of neuralgia brachialis, complicated with vaso-motor paralyses, has this medication served so well, that I do not hesitate to recommend it under the circumstances.§

In my present case of hemicrania sympathico-paralytica the application of the galvanic current—with the intention

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* *Loc. cit.* p. 27.

† *British Med. Journ.*, 1868, Vol. II, p. 360.

‡ *Berlin Klin. Wochensch.*, 1873, No. 15.

§ I usually employ the following solution: Extract secal. corn. aquos. 5.0 [75 gr.] Aq. dest., Glycerin *aa* 15.0 [4 ʒ.] The ordinary addition of spirit vin. rect. is to be avoided as the thus prepared solution of Bonjean's extr. secal. corn. aquos. is by no means clearer than the purely aqueous preparation, while more painful as a subcutaneous injection. Swidersky maintains that the rapidity of action is proportionate to the amount of alcohol the solution contains. (*Berlin Klin. Wochenschrift*, 1870. No. 50.)

of exciting, though not over-exciting the affected nerve—was attended with remarkable success, which is of so much greater moment, as the affection had existed for years, and an error in the estimation of the therapeutic efficacy was thus excluded. Whether the greater frequency of the menses, which had lately increased without definite cause, was dependent on the treatment rather than on the general alteration of the patient's condition, I would not dare to decide, though it seems probable. The watery discharges, appearing at a definite hour without apparent cause, might likewise be traced to a paralytic state of the intestinal vaso-motor nerves. A rather strange fact was that, notwithstanding the intensity and duration of the unilateral vaso-motor paralyses, neither inflammatory processes nor, indeed, any trophic trouble were ever noticed.

General interest can certainly be claimed for the augmented acuteness of cutaneous sensibility of the right half of the face, both of tactile sense and general sensitiveness, during the arterial hyperæmia intensification; apart from the rarity of such an occurrence it is in contradiction to all previous statements, according to which hyperæmia reduces both the cutaneous tactile power and the sensibility of temperature. Methodic experiments on the influence of vascularity on sensibility have, as far as I know, been performed by Alsberg only, who is the sole authority of physiological works. Alsberg* studied these variations on the palm of the hand and the sole of the foot, producing anæmia by raising the extremity, hyperæmia artificially by application of a tight bandage (as in venesection), and came to the conclusion that the tactile sense is diminished in both conditions, as he believes, from the alteration in cutaneous tension. The sense of temperature, however, showed an augmented delicacy of 0.1 — 0.3° C. during anæmia, while the opposite condition reduced it 0.2—0.3° C. Eulenburg (*l. c.*) lays stress upon the fact, that the heightened sensibility to differences in temperature by anæmia is in gross contradiction to the well known reduction of cutaneous sensibility by a high degree of anæmia as from occlusion of arteries [ischæmic anæsthesia]. The distinct augmentation

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* Dissert. Marburg, 1863. I am not in possession of the original, and cite according to Eulenburg. (*Jahrb. der funct. Nervenkr., p.* 28 *and* 238.

of sensibility which our patient showed during her paroxysm proves that: Active cutaneous hyperæmia increases sensibility in its different qualities, both the tactile sense and the sense of temperature, as well as the general sensibility.

Probably the contradiction of this fact, with the statement of Alsberg, may be explained by his method of producing hyperæmia, as the application of a tight bandage must result in a *passive* congestion, the influence of which cannot be identical with the active supply of oxygen during active hyperæmia. In what way the augmentation of sensibility is brought about I will not try to decide; it seems justifiable, however, to assume an increased irritability, or perhaps conductibility, which is aided in the perception of peripheral impressions by the moist condition of the epidermis, etc.; perhaps the augmented temperature is of some influence, as experiments on the motor nerves of the frog have shown that while a temperature above 45° C. destroys, below that point it first augments and then reduces excitability. An alteration of cutaneous tension cannot well be the cause of the changes in sensibility, as its augmentation would only *stretch* the tactile circles, thus blunting the tactile perception, unless we admit that a heightened irritability of the nerve filaments is produced by the stretching.

In connection with this explicitly detailed case, I beg permission to make a few remarks on two other interesting cases of hemicrania:

The first patient was a robust, previously healthy coachman, 39 years of age, who consulted me in November, 1870. He had been suffering for one and a half years from a violent migraine of the left side, originating from *insolation*. On a very warm summer day he had been driving a distance of four miles exposed to the burning sun. During the ride he experienced pain in the left half of the head, while towards evening a violent pinching pain was felt in the abdomen, especially in the epigastric and mesogastric regions, and was soon followed by intense cephalalgia of the left side. The attack lasted during the whole night, towards the end of which time repeated bilious vomiting and copious watery discharges occurred. From this day intense paroxysms of pain set in every 14—28 days, while previously the patient had never complained of anything similar except a headache of several weeks' duration while a child. The attacks are started with intense *enteralgia*, with sense of oppression, marked pallor of the face, cold sweat, etc. After these have abated the pain rises to the head, and an intensely painful hemicrania of the left side, especially the temporal region, remains for many hours. At the acme of the attack repeated vomiting, and before or after it *copious watery discharges* are of regular occurrence. During the paroxysm the face appears pale, without any difference between the two sides, nor between the two eyes. The interval leaves the patient in a healthy state; he does not suffer with indigestion nor diarrhœa; examination proves the abdominal organs normal. *The left cervical sympathetic is tender to pressure.* Arsenic seemed to mitigate the attacks.

In this case the causative relation of insolation, as well as the coincidence of the enteralgic and hemicranial paroxysms, with acute disturbance of intestinal secretion, is of particular interest. Though we cannot trace with absolute certainty the enteralgia to the abdominal sympathetic plexuses, the sudden diarrhœa strongly points to a paralysis of vaso-motor nerves of the alimentary tract—similar to the effects of violent emotions —with consecutive increase of secretion. Our physiological knowledge on the point is, at the best, deficient, even contradictory. Sections and extirpations of the abdominal sympathetic plexuses are causative partly of anomalies of local circulation (hyperæmia, extravasation), partly of important disturbances of nutrition (ulcerations) of the gastric and enteric mucous lining (Pincus, Adrian). After extirpation of the cœliac plexus, Budge* and Samuel† noticed diarrhœa, so that the large intestines were filled with fluid fæces, in contradiction to the statement of Pincus, that vaso-motor paralysis produces a diminution rather than an augmentation of intestinal secretion. Brunner also reported that some cases of migraine, under his observation, ended with vomiting and diarrhœa, and concludes that, besides the cervical sympathetic, the solar plexus is probably also affected. Moreover, it is well-known that intermittent diarrhœas, caused by malarial influence, with other manifestations of malarial poisoning, occasionally occur, which are traceable to an intermitting paralysis of the vaso-motor nerves along the alimentary tract.‡

The second case which I am about to report is that of a colleague of mine, *æt.* 40 years. This gentleman has been suffering since his sixth year with violent migraine—a disease also affecting his father, two aunts and one sister. The paroxysms were rare during his childhood, increasing to 1—2 times per week until his 17th year, when up to 22 he had a free interval, after which they came on again with a frequency corresponding to the amount of mental occupation, affecting him at present on the average once

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* *Comptes rend*, 1856, *II.*, *No.* 15, *Nova acta acad. Leopold, Carol, XXVII.*, *p.* 255.

† *Wiener Med. Wochenschr.*, 1856, *No.* 30.

‡ Schramm, *Aerztl. Intellig.-Blatt*, 33–34, 1867.

a fortnight. Since his infancy the patient has complained of a certain "functional debility of the spinal cord," apparent in the inability to undertake long walks and subsequently in the extreme debility induced by even very moderate sexual indulgence. In 1852, at the time of the State examination, symptoms of a grave affection of the cord made their appearance, as: easily produced fatigue of the extremities, twitching of the legs, formication, rheumatoid pains, violent rhachialgia, frequent involuntary emissions, constipation, etc., which gradually disappeared again. From time to time, especially after mental or physical exertion, the symptoms have transiently re-appeared. No objective anomalies are observed. The hemicrania attacks at present alternately the left and the right half of the head, the latter in a more intense degree.* The paroxysms are sometimes of the tonic variety (unilateral pallor, followed by *secondary* redness and heat of the affected side especially the ear, prominence of the temporal artery and increased pulsation,) and sometimes of a paralytic aspect (*primary* redness of the ear and face, excessive injection of the conjunctiva, tumefaction of half of the face). The attacks of angio-paralytic symptoms are usually milder than the sympathico-tonic form, and the accompanying vomiting is not as intense. A few times ecchymoses occurred in the affected conjunctiva, but the patient does not recollect to which description the paroxysm belonged. The attacks of the tonic variety were attended with a very troublesome salivation, so excessive that above two pounds of a viscid saliva were ejected, besides large quantities during vomiting. Alternating rigor and sense of warmth are never absent in a violent paroxysm. The pulse is at first slow, gradually surpassing in frequency the normal. About oculo-pupillary symptoms the patient made no definite statements. Occasionally a violent gastralgia complicates the migraine, sometimes even precedes it, commencing on the previous evening.† During the attack the urine is copious and clear; the next day cloudy. The right half of the face is distinguished, also, in the interval, by an unusual vascular excitability; a slight pressure; exposure to heat or cold produces a redness of half an hour's duration; ephidrosis, also, is easily produced in the right temporal region, while the left remains dry. *Both cervical sympathetic nerves are very sensitive to pressure;* in a minor degree also the spinous processes corresponding to the cilio-spinal region.

The particular interest connected with this case of migraine, that disabled my colleague from attending to his duties, consists in the combination of both forms of the affection in the same patient, a coincidence never previously recorded—at least to my knowledge — besides in the copious salivation,‡ easily explained by the assumption of sympathetic irritation, as in the tract of the cervical sympathetic are found secre-

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* Tissot (*l.c.*) reports the case of a lady in whom the pain alternated regularly. Wepfer has seen this also in a nun. (*Observationis med. pract. de affect. cap. Observat.* 49.)

† Tissot reports similar instances. Willis speaks of a young lady with whom an unusual hunger in the evening always predicted the violent headache of the next morning.

‡ A slight increase of salivary secretion is not of rare occurrence in hemicrania.

tory fibres going to the salivary glands; the viscid consistency of the saliva is also a proof of its origin in sympathetic irritation. The ecchymosis in the affected conjunctiva is probably dependent upon a predisposition to rupture of the vessels, caused by the abnormal state of tension, which tendency is aided by vomiting as the exciting cause.

APPENDIX.

As five months have elapsed since the above article has left my pen, I avail myself of the opportunity to complete the clinical history of Frau Apotheker H. Since the middle of July the galvanic treatment has been suspended, and *no trace* of an attack has re-appeared. Only on special provocation are *slight* vaso-motor disturbances noticed, while all oculo-pupillary symptoms have ceased.

I will now take the liberty to speak of another case which has come under my observation only in the last few days (from the practice of Dr. Schmeidler). The lady, *æt.* 49, of a cultured family, bears a sad inheritance. Her mother died in a lunatic asylum; one sister is melancholic; her oldest son suffers since his eighth year of bilateral *tic convulsif* without assignable cause. Suffering since her youth from various nervous disturbances, the anæmic debilitated patient became affected this spring with torturing paroxysms of pain in the left frontal and temporal regions, coming on several times a week, and lasting usually a number of hours, constantly increasing in intensity, especially during summer heat. Neither nausea nor vomiting, nor disturbances of vision in any manner have been observed, nor are anomalies of cutaneous circulation and secretion ever witnessed. Dr. Schmeidler, however, noticed in October a diminution in size of the left interpalpebral space and pupil, which has continued—with the cephalalgia—ever since. Examination shows at once a marked contraction of the left lids and pupil, which latter, though round, is only half the size of the right one; on shading the eyes the difference is still greater. The re-action to light is normal. No abnormal vascular injection of the conjunctiva; no lachrymation of the left eye. Privat docent Dr.

Hermann Cohn gives the following result of an examination of the eyes:

L. Eye.—With +10: Sn. I_{II}^{I} at 4—6″. Sn. III up to 9½″. M. $\frac{1}{30}$ V. $\frac{15}{15}$.

R. Eye—With +10: Sn. I_{II}^{I} at 5½—8″. Sn. III up to 9″ E. S. $\frac{15}{20}$.

Right pupil, 1¼‴; left, 1‴. Both pupils re-act well. The left upper lid sinks down ordinarily to the upper margin of the pupil, and can be raised by great exertion about one mm., but no further. Fundus of both eyes alike—no difference in vascularity. On the left side there is, therefore, myopia, and the *proximate point* is nearer than in the other eye. Temporal and carotid arteries beat alike on both sides, and no difference could be observed in the vascularity and temperature of the two sides of the face; however, on the last three days of examination, at which time violent paroxysms existed, a slight redness and augmentation of temperature could be found in the left malar region, the difference amounting to 1° C. on repeated trials, while the mercury stood at an equal height in both meati. The congested parts were endowed with a slightly, but *distinctly*, increased electro-cutaneous sensibility, while the tactile sense and perception of temperature were not altered; no disturbance of facial symmetry; pulse normal; thyroid gland the same; pressure along the inner margin of the sterno-cleido-mastoid is no more painful on the left side than on the right, while a marked tenderness to percussion and electric irritation exists over the spinal processes of the first three dorsal vertebræ only. Tender points in the region of the left frontal nerve are not to be found. Evidently there exists in this case a paralysis of the oculo-pupillary filaments of the left cervical sympathetic, the seat of which is perhaps to be sought—as it seems from the tenderness of the upper dorsal vertebræ—in the medullary centre, though no other symptoms of a central involvement are present. The vaso-motor fibres participate in but a slight degree; still the attending pains must be referred to their diseased condition.

Cases of isolated affection of vaso-motor fibres as causes of "cephalalgia vaso-motoria" (Eulenburg) seem to be of frequent occurrence, so I conclude from my observations; of less frequency is the isolated affection of oculo-pupillary filaments—or of both. A physiological proof of the fact that either vaso-motor or oculo-pupillary fibres of the sympathetic may be diseased alone, is found in the well-known experiments of Bernard,* according to which experimental production of vaso-motor symptoms (by section of the ascending filament of the dorsal sympathetic, laterally from the spine, between the second and fourth ribs), as well as of oculo-pupillary symptoms (by section of the anterior roots of the first two spinal nerves), is possible. Even in case of localization of the disease in the trunk of the cervical sympathetic, is the solitary affection of one set of fibres quite possible, as we occasionally see in the train of isolated symptoms in lesions of mixed nerves. Finally, a word on "ptosis" in paralysis of oculo-pupillary filaments. In the last case it was mentioned that with no exertion could the lid be raised more than one millimetre; are we therefore to assume a paralysis of the motor oculi (levator palp. sup. muscle), or can the condition be explained by the state of the sympathetic? As is well known, H. Mueller† has discovered a system of unstriated muscular fibres which are inserted in a vertical direction on the dorsal cartilages, and serve to open the lids. Wagner‡ and Mueller§ have shown experimentally that these muscles are supplied by the cervical sympathetic, on electric irritation of which (on executed criminals) the lids opened. These retractors of Mueller are supposed to possess a certain "muscular tonus" like the antagonistic muscles of the iris, and their centre of automatic innervation has been assumed to be in the cervical, respectively the upper dorsal part of the spinal cord, the same as the centre for the pupil-dilating fibres (Budge's "cilio-spinal"—Bernard's oculo-pupillary centre ‖). If we assume, therefore, that for a complete elevation of the upper lid the action of these involuntary muscles is needed, as well as voluntary contraction of the orbicularis, an incomplete ptosis is evident on their inactivity by paralysis. A critical

* *Arch. general.* 1862, p. 495.

† *Verhandl. d. phys. med. Gesellsch. in Wuerzburg*, 1859, Vol. 9, p. 244.

‡ *Verhandl. der Wuerzb. Ges.*, Vol. 10, 1860, p. 11.

§ *Verhandl. der Wuerzb. Ges.*, Vol. 10, p. 49.

‖ *Salkowsky Centralblatt*, 1867, No. 31.

examination in cases of ptosis paralytica with simultaneous myosis would, therefore, probably show also other symptoms of disease of the sympathetic, while ptosis, with dilatation of the pupil, would be traced to paralysis of the motor oculi, in the branches going to the levator palp. sup. and sphincter iris muscles.

Breslau, December 5, 1873.

Art. IV.—REPORT OF THE RESULTS OF ELECTRICAL TREATMENT, AS ADMINISTERED IN THE DEPARTMENT FOR NERVOUS DISEASES AND ELECTRO-THERAPEUTICS, OF THE GENERAL HOSPITAL OF VIENNA.

By Drs. Max Gumplowicz and Emerich Klotzberg.

Translated from the Wiener Medizinische Presse, Nos. 14, 17 *and* 19, 1874.

[Note.—This article is inserted as affording a brief and concise statement of the results of a rather extended trial of electrical treatment, isolated from all other agencies, in a large number of diseases. The candor and reliability of the statements, apart from the internal evidence which they bear, are fully guaranteed by the high character of Prof. Schwanda and his assistants, as well as that of the institution in which the observations were made. As a thoroughly fair and candid testimony of the value of electricity as a remedial agent in many forms of disease, we commend it to the attention of our readers.—Eds.]

In the year 1873, six hundred and eighty-two new cases, mostly of diseases of the nervous system, were treated in this department by electricity, principally, and, as far as practicable, to the exclusion of all other treatment. We started on the principle of local treatment of the disease, and in each case have endeavored to apply the electric current as far as possible to the diagnosticated, or, at least, the presumed seat of the ailment, although we are aware that in many cases the locality, and also the exact effect of the remedy, cannot be, *a priori*, determined.

In accordance with this anatomical and therapeutical standpoint, we have made the division of the materials at our command in this report, first, on anatomical, and, secondly, on physiological grounds; still, we must first state, that we adopt this method only as an approximately practical one, without denying its difficulties, nor in any way to the prejudice of any other division.

We will, therefore, narrate the cases treated by us in the past year, in the following order:

(1) Cerebral affections, (2) spinal diseases, (3) affections of peripheral nerves, (4) general neuroses, (5) diseases of the locomotor organs, (6) disorders of various other organs.

CEREBRAL DISORDERS.

a. *Cerebral Paralysis.*—In all, seven cases of hemiplegia following apoplectic attacks were treated, mostly in the later stages, when the morbid process in the brain was supposed to have run its course, and, therefore, the paralysis of the extremities was considered as the immediate object of the treatment. We employed, in part, the faradic current, and in part the galvanic, and partly both, changing from one to the other. The electro-muscular contractility was diminished in none of our cases—in some it seemed to be heightened. Where no irritative symptoms were present, the result of the treatment was good, in some cases eminently so. We consider that, from our experience, the alternating faradic and galvanic currents, with the avoidance of too great an intensity and all shocks, should be given the preference.

In one case, where irritative phenomena (contraction, pain) were present, this method, with experimental galvanization of the sympathetic and head, gave no results.

In one case of complicated paralytic symptoms, the provisional diagnosis of tumor cerebri [in the left basal region (?)] was made; treatment was not attempted.

One case of aphasia, due to a fall from a considerable height, underwent a decided improvement during treatment by galvanization of the hypoglossus, and through the head.

b. *Cerebral Convulsions.*—We treated twenty-two cases of epilepsy, more for the purpose of observation than with the hope of success. In none of these cases was there any indication for local treatment (scars, etc.); in most, psychic impressions were given as the cause. Since epilepsy cannot well be considered as anything else than a vaso-motor neurosis of the brain, we believed ourselves justified in employing galvanization of the cervical sympathetic. This method, as has been incontestably proven by experiments recently made in Paris, with the ophthalmo-microscope of Nachet, exercises a momentary influence on the conditions of the cerebral circulation. Unfortunately, we cannot convince ourselves that this influence has any result, as regards the treatment of epilepsy, in spite of the fact that we continued the applications for a considerable time. The apparent improvement appeared to us to be within the limits of the variation, which may follow any treatment, or none at all. Nevertheless, we concluded still to continue the experiment of the electrical treatment of epilepsy.

Among the cases of cerebral convulsions, we include that of a man fifty-one years old, who, in consequence of a wound of the skull, suffered from time to time with clonic cramps of the right upper and lower extremities. No results from the treatment.

c. Cerebral Hyperæsthesias.—In a series of seventy-eight cases, headache, vertigo, insomnia, or slight psychic disorder, were the most striking symptoms. The great majority of these symptoms affected anæmic, cachectic, or hysterical individuals; in some there was typical hemicrania, in others, hyperæmia of the brain had to be considered as the cause; in others, again, the nature of the disorder was obscure.

Without going further into the causal relations of the trouble, and only considering its location, we employed, in all these cases, galvanization of the sympathetic, or the head.

The results, especially in the first group, (in anæmic and hysteric individuals,) were remarkably favorable. In most cases, after each sitting a temporary improvement was observed, which, after several applications, became permanent.

In typical hemicrania we often saw improvement, but never a complete cure.

One case of long standing sleeplessness, in a woman forty years of age, was cured by a few applications, if we can trust the patient's own testimony.

On the other hand, the treatment appeared to us to be without results, in conditions of cerebral congestion.

Before leaving the cerebral affections, we will briefly mention a case of glosso-labio-laryngeal paralysis. Juliana B., aged thirty years, peasant, had suffered for some five months from weakness in the arms and difficulty of swallowing, without known cause. At her admission, May 25, 1873, she showed the following conditions: Patient, of medium size, well nourished, strength of both arms much diminished; noticeable pharyngeal trouble; only semi-fluid substances can be swallowed; articulation slow, irregular and awkward; tone of voice somewhat nasal; tongue and lips movable with difficulty, the former tremulous when extended; at times, difficulty of respiration. No other anomalies, no hereditary cause determinable. After two months of daily treatment, (galvanization of the sympathetic, and excitation of the movements of swallowing,) no objective results were obtained; only, according to the opinion of the sufferer, the act of swallowing was made somewhat easier. We cannot, therefore, offer other than an unfavorable prognosis. (Duchenne, who first described this disease in 1852, has had, in the course of twenty years since then, thirty-nine cases, and yet has not observed a cure in a single one. On the other hand, Prof. Benedikt, among sixteen cases which have come under his observation, reports a failure in only two; in nine a perfect cure, and in three, improvement.)

In one case of diabetes-mellitus, which we include here, as apparently consisting in an affection of a vaso-motor centre in the medulla oblongata, the patient had to be remanded to hospital treatment for advanced phthisis.

SPINAL AFFECTIONS.

We had a single case of myelitis in a five-year-old boy, which was soon withdrawn from treatment.

In a case where paraparesis and cutaneous anæsthesia of the lower extremities remained as a sequel of typhus, the faradic treatment produced a notable improvement.

Tabes dorsalis was diagnosed in fourteen cases, mostly belonging to the later stages of the disease. We employed, preferably, the interrupted galvanic spinal nerve and plexus nerve currents, and cutaneous faradization for local anæsthesia. The result of this treatment was usually favorable in palliating symptoms, the sensorial symptoms (pain and anæsthesia), especially, were sometimes greatly relieved; we have, however, failed to perceive any lasting influence on the course of the disease, and just as little from the much-praised spinal galvanization.

Nine cases of infantile spinal paralysis came under our notice, and these, also, generally in the later stages—four months to two years after the commencement of the affection.

We found the electro-muscular contractility generally diminished, and usually corresponding, more or less, to the degree of the atrophy. Starting with the view that we were not in a condition to act directly on the seat of the disease, the motor cells of the anterior columns, we concluded to apply the treatment directly on the inactive muscles separately. We employed for this purpose the direct faradization of the different muscles, and obtained in all cases favorable, and sometimes very striking results. In one case, only, in which the atrophy had already reached its most advanced stage, did we fail to see good results from this method.

In one of these cases where the disease had, a year before, made its appearance with paralysis of all four extremities and the muscles of the neck, we found, on its admission, only a well developed paresis of the right upper and left lower extremities. As much as we are inclined to refer the general symptoms in these cases to one central locality, which we must locate in region of the inferior (motor) crossing of the fibres in the pyramids, still, it seems to us impossible, in the face of the facts of the pathological anatomy of infantile spinal paralysis at our command, not to accept for this case two locations of disease, one in the right cervical anterior column, and the other in the left motor tract of the lumbar enlargement.

Progressive Muscular Atrophy.—Of three cases of this disease, only one could be watched for any length of time and be treated. We, following the views of Duchenne, faradized the separate muscles with moderate currents. During about three months of treatment, with daily applications, the condition of the patient remained the same.

In reference to the question as to the neuropathic or myopathic nature of this affection, one circumstance in our case seems to us to favor the first of these theories: the extension of the atrophy corresponded to the ramifications of single nerves, but not to the contiguity of the muscles. On this last condition Friedrich, in his latest monograph (*Ueber progressive Muskelatrophie*), by the maintaining of the opposed view, lays some weight. More concerning this case will be communicated in another place.

AFFECTIONS OF PERIPHERAL NERVES.

Of isolated disorders, single nerves of sense, we have had only two cases of "nervous" deafness, in which no material organic changes could be detected by the specialist. The reaction formulas were not determined with satisfactory precision. In one of these cases the accompanying very severe subjective sensation of sound was relieved in a very few sittings.

We treated altogether twelve cases of disease of the orbital muscular nerves, — part isolated oculo-motor or abducens paralyses, part complex forms. Among them were eleven recent rheumatic cases, which were generally healed within a few weeks by local galvanization. Faradization was employed but rarely, on account of its greater painfulness.

Of diseases in the region of the facial nerve, we had thirteen cases of recent rheumatic facial paralysis, which healed quickly, and in the same manner as the analogous ocular troubles.

One case of paresis of the lower facial branch, combined with local paræsthesia, apparently the residuum of an old central process, appeared resistant to all treatment.

Two cases of convulsive tic, one male and one female, were improved by the use of voltaic alternatives.

One case of bilateral blepharo-spasm without known cause, its origin being perfectly obscure, remained unmodified by various methods of treatment. Two other cases of blepharospasm, following conjunctival blennorrhœa, were considerably improved.

Of twenty-nine cases of disease in the region of the trigeminus, ten implicated its common trunk on one side; twelve the first, three the first and second, three the third, and one case the second and third branches. The majority were rheumatic troubles; a smaller number, typical neuralgia. Local galvanization proved an excellent remedy for the first kind, the second was palliated by cutaneous faradization. One case of very severe neuralgia, which had lasted many years, withstood every method of treatment.

Of neuroses in the sphere of the vagus, we treated two cases of palpitation of the heart, without any apparent organic disease, one of which was considerably improved. Two out of three cases of cardialgia were bettered by energetic cutaneous faradization in the epigastric region. One case of paralysis of the vocal cords (after diphtheritis?), and one of unilateral spasm of the glottis in an adult,—in which the laryngoscopic examination revealed nothing abnormal, except clonic contractions of the vocal cords,—were treated without result.

Of neuroses in the region of the accessorius we had under observation, one case of torticollis from contraction of the sterno-cleido-mastoid. The patient withdrew from the treatment. Another case, lasting since birth in a child eighteen months old, of torticollis, resulting from paralysis of the right and consequent contraction of the left sterno-mastoid (sent to us by Dr. Rob't Bernhard), was treated by faradization of the paretic muscles with good results, and is now progressing towards recovery.

Neuroses of the Phrenic.—One case of singultus, in a child thirteen years old, of a year's duration and resistant to the most various methods, was perfectly cured in two sittings. Two other cases in older individuals were improved; one case of paresis of the diaphragm remains unhealed.

Cutaneous faradization was twice employed for neuralgic affections in the track of the major and minor occipital nerves, with very striking results.

We treated brachial and cervico-brachial neuralgias by the method of voltaic alternatives (plexus nerve current) frequently with favorable, but still only palliative, results.

In nineteen of our patients we found, as sequelæ of traumatic affections of the upper extremities, sensory, motor, or trophic disorders, either one form alone, or combined with others. In the majority of these cases the faradic current proved to be an excellent remedy, as far as our experience gave evidence. In a few cases only, in which severe pain formed the chief symptom, were we obliged to have recourse to the constant current. The direction of the current in these, as in all cases where we have employed the constant current, seemed, as regards therapeutic effect, of very little consequence.

In six cases of functional convulsion, we employed the faradic current with perfect success.

Against saturnine paresis and paralysis we used, alternately, the faradic and galvanic currents: out of ten cases eight were cured.

In a series of cases, three were paresis in the region of single branches of the brachial plexus, the exact nature of which could not be determined. We treated these, mostly, by voltaic alternatives, with varying success. If it is allowed for us to diagnose from our successes, we might declare the cases which were cured to be of rheumatic nature.

Intercostal neuralgia was frequently treated by cutaneous faradization, with good success as a palliative.

In regard to affections in the region of the lumbar and sacral plexuses and their branches, of which, altogether, eighty-four cases came under our treatment, we may state, in brief, to avoid repetitions, what has been already said in regard to similar affections: the traumatic and rheumatic cases gave the best results, the former generally to the faradic, the latter to the galvanic treatment; in typical neuralgias, however, we could, in general, only afford a palliative relief. This difference was most noticeable in sciatica, the rheumatic form of which gave very good, and sometimes truly astonishing results, while the rarer, purely neuralgic form, resisted all treatment, as regards its continuance.

GENERAL NEUROSES.

Hysteria.—Thirty-eight cases gave us opportunity to test the value of electrical treatment in the different forms and symptoms of hysteria. Since we were not in the condition to act on the cause of the disease, it is evident that we could scarcely expect to effect an absolute cure. Still, we were able, in nearly every case, to alleviate, temporarily, single, particularly troublesome symptoms. Hysterical complications, which resemble severe forms of nervous disease, and are readily relieved by electrical treatment, are not uncommon, and to those must be attributed many wonderful accounts of cures in electro-therapeutics. Numerous experiments which we have undertaken, with "electrisation par action reflexe" in hysteria, encourage us to give this method a still further trial.

While we sought, in cases of hysteria, to apply the therapeutical measures to separate symptoms, we have aimed, in cases of hypochondria and nervous weakness, to improve, first of all, the general condition of the patient. Under these and other similar, partly somewhat vague diagnoses, we treated fifty-three individuals. We had, in some cases, to deal with hypochondriacs of the most unmistakable type, among them several medical men; in others, with individuals depressed and weakened by various physical and psychic causes. We made it a rule to apply galvanization along the spine, in cases where irritative phenomena were present, and energetic faradization of the skin and dorsal muscles, as a powerful and easily managed revulsive, in those where the symptoms were those of depression. These last named applications sometimes gave us altogether striking results, especially in persons weakened by sexual excesses, with simultaneous psychic depression. How much of this effect is to be attributed to mental action, we do not attempt to say. We ought to include, also, in this catalogue, nineteen cases of pollution and impotence, in so far as there was present with the local functional disorder a modification of the general condition. Therefore, we joined with the local treatment the general applications according to the method given above, and obtained like results.

Of chorea minor, which we count among the general neuroses, by the extension of the irritative motor symptoms, and by their combination with psychic disorders, we had ten cases. This small number, and the fact that recovery from chorea may take place in a short time without treatment, allows us to draw no conclusion on the efficacy of electricity, and the various methods of its application in this disease.

Of other general material functional disorders, we have observed:

Two exquisite cases of paralysis agitans (weakness, tremor, peculiar staring look, and propulsion movements);

One case of mercurial tremor;

One case of delirium potatorum;

Nine cases of tremor from various depressing influences.

The stable galvanic current appeared to us of utility in certain of these cases, but we are not in condition to give positive conclusions in this regard.

AFFECTIONS OF THE LOCOMOTOR ORGANS.

The relatively most numerous and most satisfactory contingent of our practice, were the cases of rheumatic affection of the muscles and joints. Of the first of these, we treated, during the past year, fifty, and of the last, sixty-seven cases. With the greatest cautiousness in estimating therapeutic facts, in view of the success—sometimes truly astonishing—which we have attained, we can say with perfect confidence, that electricity is a sovereign remedy in most rheumatic affections. It sometimes happened that patients who had been treated with various methods for months and years, without success, were healed in a few sittings. Especially is this true of such affections as are limited to a circumscribed portion of the body, and combined with paresis of the affected muscle. We are of the opinion that, in such, the interrupted galvanic current should be chosen before all others. We obtained less favorable results in chronic articular rheumatism with changing location, and were compelled, in the majority of such cases, to have recourse to cutaneous faradization.

AFFECTIONS OF OTHER ORGANS.

Of the various other affections to which we applied the electro-therapeutical treatment, partly according to the views of other physicians, partly for the purpose of experiment, we will only briefly mention two—amenorrhœa and chronic enlargement of the lymphatic glands.

Of amenorrhœa we treated six cases, in which no other uterine disorder could be detected, by local faradization. In three of these we were able to cause the appearance of the menses in a short time.

Out of four cases of chronic glandular enlargement, three remained unhealed; in the remaining one, on the other hand, we obtained a brilliant success: an enlarged gland, as large as an apple, in the inferior maxillary region, of a girl nineteen years old, which had remained the same for a number of years, after it had resisted treatment some three months, in the three succeeding ones was almost entirely reduced by local galvanization, leaving only a slight swelling and redness of the skin.

The other cases afforded nothing of interest.

We have, in this report, abstained from the so common ostentatious "statistical" form, with tables and figures, because, with so small an amount of material, which, moreover, is so heterogeneous and so insufficient, as regards the present state of science, it seemed to us that to adopt it would be the merest trifling. We have also refrained from relating interesting electro-therapeutical anecdotes.

We have accordingly confined ourselves to repeating, as far as possible objectively, the remarks which the treatment of numerous cases has suggested; we consider this the only justifiable style in the presentation of a report like the present.

In conclusion, we have to acknowledge our indebtedness to our honored chief, Prof. Schwanda, for the liberality with which he has granted the use of the material of the department under his control.

Reviews and Bibliographical Notices.

LEYDEN: SPINAL CORD.

Klinik der Rueckenmarks-Krankheiten. Von Dr. E. Leyden, Strassburg. Erster Band. Mit 8 zum Theil farbigen Tafeln. Berlin: 1874; pages, 478. (*Clinic on Diseases of the Spinal Cord, etc.*)

It would seem to be known to but comparatively few physicians, even at the present time, that the spinal cord, close as the relations are which it sustains to the parts of the cerebro-spinal axis that lie above it, has yet a sphere, both in health and disease, altogether its own. Its subordination to the medulla oblongata and the brain are too frequently so understood as to reduce it merely to the rank of a conductor of impressions to and from the brain. It is only recently that a physician of no small eminence in certain walks of the profession, expressed to the writer surprise that the cord was ascertained to have any higher office than that of a mere conductor of impressions. But there is no longer any excuse for such ignorance on the part of those who keep an open eye to the current medical literature of to-day.

But, it should be remembered, it is only recently that works like the one at the head of this article have been possible. It has been but a few years since anatomical researches like those of Stilling, Henle, Clarke, Luys, Dean, Van Der Kolk, and physiological researches like those of Longet, Brown-Sequard, Vulpian, and others, have been made and have become to such a degree the property of the profession, as to make them fruitful in the domain of pathology. But now the literature of medicine is becoming all at once rich in works, produced with the aim of carrying out the principles won from the domains of anatomical and physiological research, to the solution of the practical problems of pathology.

The work, the title of which we have placed at the head of this article, is, to say the least, a noteworthy example of this latter class. It is written by a painstaking, laborious and cautious investigator, with whose name all readers of German medical literature must have become already in some degree acquainted. Our author is evidently a plain, matter-of-fact, candid investigator. In research, whether literary or original, he appears industrious and methodical, but lacks originality and inventiveness. He recites mechanically, rather than critically, the opinions of others. But, in spite of any general faults inherent in our author's method or his mental make-up, his work is a valuable one, and we now proceed without farther delay to make our readers acquainted with its contents.

It consists of two parts—a *general* and a *special*. In the former, the author sets forth those principles which, on the one hand, are the generalized results of a study of particular facts and cases, and, on the other, are useful when they are comprehended, in aiding to construe or classify the phenomena observed in special cases. The second, or *special* part, is by far the largest, and is devoted to a study of special forms of disease of the spinal cord. The *general* part will alone claim our attention now. The *special* part we may notice when the second volume appears, if it ever does.

Very appropriately the work opens with what must constitute the foundation of any good work on diseases of the nervous system, viz.: An anatomical outline, as a convenience to the reader, and as an exhibition of the author's view of the nervous mechanism. This occupies the first thirty-nine pages. The first division of this chapter is devoted to a few brief observations on the bony membranous and fluid investments of the spinal cord. A few words are said concerning the arachnoid, but we fail to find either here, or in any other work, that stress laid on its peculiarities of structure which for a long time has appeared to us should be. We often see diseases of the arachnoid, especially inflammations, spoken of with a freedom, the facts as we view them do not appear to warrant. It seems to be forgotten that the arachnoid is comparatively devoid of vessels and nerves, and that just in proportion as this is true of a part, just in that proportion is it likely to be free from acute diseases. Many of the morbid changes which the arachnoid is said to undergo, do not originate in the arachnoid at all. It is often mentioned that it is discolored, or opaque, or covered with lymph, or pus, etc., as if these substances had been the result of disease of the arachnoid. But in most such cases, to say the least, we believe that disease of the arachnoid itself had very little to do in producing such morbid changes. In most such instances we believe them to have been the result of disease of neighboring parts, especially the *pia mater*. Our author also speaks of the cerebro-spinal fluid, and mentions the researches of Magendie, Jolly, Pagenstecker, Quincke, Althann and others, as well as his own, on its origin and use. In regard to the former, no decided opinion is expressed. But it has always appeared to us pretty clear, that it consists chiefly, if not exclusively, in an exhalation from the *pia mater*, or the vessels which meander in it. Certainly it cannot be from the arachnoid. We now speak of the fluid between the pia mater and what is called the visceral or internal layer of the arachnoid. As for the *cavity* of the arachnoid, or the space between its so-called parietal and visceral layers, there is usually but little fluid there. The opposed surfaces of these two layers of the arachnoid, are, like the free surfaces of all serous membranes, lined by fine pavement epithelium. This implies the two surfaces were intended to be in contact, like the opposed serous surfaces of the lung and costal-pleura, or of the intestine and peritoneum. The cerebro-spinal or *subarachnoidean* fluid, which occupies the space between the pia mater and arachnoid, we have no doubt is exhaled from the vascular surface of the brain and spinal cord. So much in relation to the *origin* of this fluid. As regards its *use*, it seems to us there should be little room for doubt. It exists for two purposes. First, as a means for the protection of parts so delicate as the cord and brain, but especially the former, lodged as it is in a flexible column, exposed but for such a delicate and instantly adjustable medium, to the disturbing influence of innumerable mechanical shocks. The spinal cord is anchored in the axis of a column of fluid, for protection. This is its most important office in man. The *second* use of this fluid, is that of exercising an equable pressure on all parts of the surface of the cerebro-spinal axis, regulating in some degree the amount of blood which may be admitted into the cerebro-spinal cavity. That this is one use of this fluid is shown, on the one hand, by diminishing its quantity. Immediately thereafter motor and mental disorders follow in many, perhaps, all cases, and congestions, or even haemorrhages, into the nervous substances, have been found, all pointing to too great vascular pressure in the cord and brain. Or, on the other hand, by the effects of its increase, as we sometimes see in cases where rapid "effusion" at the base of the brain has occurred, and under which circumstances the face of the patient becomes pale, the pupils enlarged, and coma comes on, and finally death. The *post mortem* in such cases usually reveals an essentially anaemic condition of the parts, say, of the brain, with an excess of the subarachnoidean fluid. Such facts as we have related, occurring in connection with a decrease or an increase in the normal quantity of the fluid, point to its use as a means of equalizing and antagonizing expansive vascular pressure within the nervous masses of the cerebro-spinal axis.

We have been led into this train of remarks because we deem this subject an important one, and because it has seemed to us, in a rather wide reading, that erroneous views in regard to this point have prevailed, and are yet extant.

But to proceed. After describing the investments of the cord, he next proceeds to a statement of his views concerning the anatomy or mechanism of the same. Of course the author gives but a mere outline of the subject, not to be compared in fullness to the descriptions of purely anatomical works, such as the admirable productions of Luys, Stilling, Henle, or even Huguenin, for example.

The account of the anatomical configuration and intimate structure of the cord and medulla oblongata, though brief, is clear and judicious, especially in describing the disposition of nerve cells in various regions of the cord, and also in the account of the vascular supply of the brain and cord, and of the so-called "peri-vascular spaces."

In speaking of the destination or use of the caudate processes of nerve cells, he appears to consider as a mere hypothesis the opinion that the nerve fibres connect with these processes, and does not mention the later observations, more especially of Ranvier, in regard to the non-existence of the so-called Deiter's or peculiar connective tissue cells. He gives a pretty full resume of the discussions had in regard to the destination of certain caudate processes of nerve cells, especially of the cells contained in the vesicular column of Clarke, as described by Gerlach, who believes them to divide and subdivide, and then terminate by becoming continuous, not with nerve fibres, nor directly in the caudate processes of other cells, but in a fine network of protoplasmic substance, in the midst of which the cells are supposed to lie, and which protoplasmic substance in this view would become a medium for the conduction or diffusion of nerve impressions, or nerve force in the nerve centres. But we will find occasion for mentioning this point more fully in noticing the work of Prof. Poincare. He next mentions as probably correct, the views emitted, especially by certain French writers, that the large multipolar cells contained in the anterior horns of gray matter in the spinal cord, are not motor cells, as Owsjannikow, Jacubowitsch and others, have supposed, but *trophic* or nutritive cells, because, that in progressive muscular atrophy they are found diseased or even destroyed, though not so in paralysis without muscular atrophy.

It would be hardly useful in this notice to follow our author through his summary of researches in regard to the state of our knowledge as to the intimate disposition and connection of nerve cells and nerve fibres in the spinal cord, and the real mode of re-distribution of the elements of the cord after they have been sent into the medulla oblongata.

He adds nothing himself to our stock of knowledge in this respect, and evidently feels that, thus far, research has done more to raise questions than to answer them.

He next endeavors to determine what particular regions of the body are supplied by particular regions in the cord. It would certainly be a matter of great practical value to determine exactly what region of the body the nerves, given off from a given part of the cord, acted on or were distributed to. He mentions with approval the law of Schrœder Van der Kolk, also studied by Eckhard, and which is to this effect: "If a nerve of motion gives branches to a muscle, then the corresponding nerve of sensibility is distributed to the skin of that part which it is the office of the muscle to move," and the further law of Eckhard, "that nerves of motion which innervate groups of muscles destined to act together, arise from very limited regions in the spinal cord."

After mentioning the observations of Peyer, on the distribution of the motor nerves in rabbits, our author cites with approval, the results of certain pathological observations of Schutzenberger, to the effect, "that the point in the spinal cord from whence the motor nerves for the extensors of the hand have their origin, lies deeper in the cord than the point from whence the flexor nerves proceed. That in disorders of the deeper parts of the lumbar enlargement of the cord, the motor nerves to the leg, below the knee, may be affected, while the muscles of the upper leg may remain free, etc." "The motor nerves for the peronei muscles are from the deepest part of the lumbar enlargment of the cord."

Next, the chief results are given of the investigations of Hein and Koschewnikoff, and also of Masius and Vanlairs, on the particular regions of the cord to which each pair of spinal nerves is related. This is a matter of no small importance in nervous physiology and pathology. Hence we give a translation of some of the conclusions reached, but especially those of Masius and Vanlairs, in certain lower animals :

1. "The origin of the roots of the 7th pair lies in the spinal cord, at the level of the joint between the 5th and 6th cervical vertebræ, and sometimes corresponds to the whole height of the body of the 5th cervical vertebra, but seldom farther.

2. "The 8th spinal nerve corresponds in its central termination or origin, exclusively to the body of the 6th cervical vertebra, sometimes extending to the lower part of the 5th, but seldom so low as the joint between the 6th and 7th cervical vertebræ.

3. "The origin of the 9th pair of spinal nerves extend from the middle of the body of the 6th vertebra to the middle of the body of the 7th.

4. "The place of insertion of the 10th spinal nerve is one of the most variable. It lies between the 6th cervical and first dorsal vertebræ, but as a rule is included within a portion of the cord limited by the body of the 7th cervical vertebra.

"The reflex centres of the four nerves of the ischiatic plexus are contained in a segment of the spinal cord which lies close behind the point of insertion of the roots of the 10th (dorsal ?) spinal nerve. This segment may be divided into distinct reflex centres, corresponding to each pair of nerves contributing to the plexus.

"The centre for the 8th and 9th nerves cannot be separated. The centre for each root begins immediately after the one before it, and extends backward or downward to the next below, and is in an important sense independent of those contiguous to it. The dimensions of a section of the cord corresponding to a pair of nerves is about 2 to 2½ mm.

"The inferior part of the 10th root contains no reflex centre.(?)

"As regards the anterior extremities their reflex centre begins 1 to 1½ mm before the insertion of the 2d root (?) and extend downwards from thence 3 to 3½ mm." (Pages 43 and 44.)

After this anatomical part comes a brief but interesting chapter on the Physiology of the Spinal Cord, opening with a judicious summary of the history of progress in this respect. He gives, as do most others, the chief credit of opening the path of discovery, to Sir Charles Bell, instead of Magendie.

He calls attention to the crossing of the motor fibres in the cord and medulla oblongata, and agrees with some other observers, that it is improbable that all motor fibres supplying the trunk and extremities, cross from one side to the other in the medulla oblongata. Dr. Leyden not only adopts the opinion that they may, in some degree, cross both above and below the medulla oblongata, but doubts whether they all cross over—especially such fibres as supply the trunk, muscles and the larynx, not to mention the cranial motor nerves, which there is no good ground for supposing cross over in any such way. Upon the whole he adopts, and we think wisely, Brown-Sequard's views, in regard to the decussation and paths of sensory and motor fibres in the cord.

Under the head of Reflex Activity of the Cord, he mentions Pflueger's laws of reflex action, now so well known to the student of nervous physiology.

In a section on Inhibitory Nervous Action, the diminution of activity is ascribed not so much to the controlling influence of arrest of one centre over another, as to a diminution of sensibility in the inhibited centre.

The subject of the co-ordination of movements in the cord receives careful attention, and the view is adopted that the posterior columns of the cord are the seats of co-ordination of the muscular movements of the limbs.

In a section on the "Trophic Influence of the Spinal Cord on the Peripheral Nerves," he esteems it as probably true that the trophic cells for the motor nerves consist in the large multipolar cells in the anterior horns of gray matter in the spinal cord, while those of the sensory nerves are in the ganglia or the sensory roots of the spinal nerves.

He next calls attention to the "Influence of the Spinal Cord on the Involuntary Muscles," and gives a resume of the most important observations of such investigators as Budge, Pflueger, Schiff, Bezold, Bernard, Goltz, Salkowsky, Ludwig and Thiry, Samuel, Gianuzzi, etc. As to the seat of vaso-motor centres in the cord and brain, not much is said. But this subject was fully discussed in the last issue of the JOURNAL. This, with a few remarks, too brief to be of much service, on the influence of the spinal cord, and on the secretions, closes the anatomical and physiological part of the volume. Brief as it is, a step in the right direction is taken, in introducing such prefatory matter.

The third chapter is devoted to the general pathological anatomy of the spinal cord. This is one of the most important in the book. Dr. Leyden notes the immense progress made in the last few years in this respect, especially in consequence of the improvements in methods of investigation—such as the use of chromic acids and the chromates, to harden the brain and cord, first brought into use by Hannover, and also Clarke's method of rendering the sections transparent.

The method of examination of our author is as follows: Take a portion of the cord and immerse it in alcohol for twenty-four hours; then for six to forty weeks in a solution of chromate of potassa, or chromate of ammonia, or chromic acid, at first frequently changing it; then color with carmine, and then with a razor make very thin slices, to be cleared by alcohol, benzine, or creosote, and then sealed upon the plate of glass—marked and studied.

After relating various "macroscopic" changes, or those observed by the unaided eye, he gives attention to the microscopic changes observed. He calls attention to atrophies and hypertrophies of the nerve fibres, and recites the usual appearances, especially of morbid changes in the axis cylinder. The changes in the nerve fibre that become appreciable, it should be observed, appear last in the axis cylinder. The account of the microscopic changes in nerve cells, and of fatty degeneration, the occurrence and significance of amyloid bodies, and of morbid changes in the *neuroglia* or connective tissue, and of the remoter results of the same, is drawn in view of the latest investigations, with which the author seems quite familiar. In this connection he discusses the question as to how far the substance of the cord and brain may be regenerated when once destroyed. He cites the remark of Mueller, made in his *Handbuch der Physiologie*, 1851, p. 335, in which doubt is thrown on the cases of Flourens and Arnemann. But he then cites the later observations of Demme on the brain, and especially those of Voit on the brains of pigeons. He calls especial attention to the microscopical investigations of Robin, as showing union of the two opposed cut surfaces in sections of the cord, and those of Masius and Vanlairs, which latter show a high capacity for regeneration in the cord. This is of importance to remember in case of lesions of the nervous centers. The regenerated fibres in the last case were found to be of the pale gray kind, and the cells were either wholly devoid of caudal processes, or had but few, comparatively, while many of the cells had begun to undergo granular degeneration. These experiments certainly show a remarkable power of regeneration of the cord and nerve centres. In the return of motility to the legs of animals in which segments of the spinal cord have been removed, the muscles first to recover their normal action were those of the thigh, then of the lower leg, and lastly those of the foot. Our author gathers from such sources, and from the improvement following some severe organic diseases of the cord and brain, a favorable point in prognosis.

This leads us to the fourth chapter on "General Symptomatology of Diseases of the Spinal Cord." This is one of the most important in the whole book. Our author first calls attention to the symptoms of paralysis in general. This class of symptoms occupies a front rank in point of frequency, and, first of all, very properly in paraplegias, which as a rule are true spinal paralyses, as is not the case with hemiplegias. He notices that all paraplegias do not have their seat in the spinal cord, and divides paraplegias into spinal, cerebral, and peripheral, and divides the latter into myopathic (or those depending on organic disease of the muscles), neuritic (or those depending on disease of nerve trunks), and *vaso-motor* paraplegias.

Under the head of *cerebral paraplegias*, it is observed that they seldom have their organic seat in the cerebrum proper. In this class he ranges paraplegias having their seat in the pons varolii, cerebellum, etc. But such forms as the latter cannot be properly called *cerebral.* The difficulty of a differential diagnosis, in case the organic seat of the paraplegia is above the spinal cord, is noted, and also the aid at this point which may be obtained from the presence or absence of disorders of speech, of the pupil, of the trigeminus, or of co-ordination, etc. Under the head of myopathic paraplegia, it is mentioned how rare such a case must be, and that such cases, if they exist, probably are not accompanied by corresponding loss of sensibility, and that the sphincters escape. The bearing of the action of the woorara poison on the existence or possibility of a pure myopathic paralysis is discussed. (H. Friedberg. *Pathologie und Therapie der Muskellaehmung*, Weimar, 1858.) Our author is not disposed to admit this as a class, aside from those instances that depend on some specific cause, such as woorara, phosphorus, lead, etc. Next in order come the paraplegias dependent on inflammation, or neuritis, or other disease of the nerve trunks. The author is inclined to the opinion that neuritis is quite common, leading to isolated paralysis of the parts to which the affected nerve trunks lead, or in case of extension of the neuritis into the spinal cord, to a complete paraplegia. In this opinion he is supported by Dr. S. Weir Mitchell, not to mention others, and is combated by M. Vulpian, of Paris. In a review of Dr. Mitchell's book, *Injuries to Nerves and Their Consequences*, we have had occasion to set forth the views of Professor Vulpian, and briefly express our own on this subject. To that paper we would refer the reader for a full discussion of this subject. We cannot, however, refrain from again expressing the opinion that *neuritis*, or inflammation of the nerve trunks, is far less common than Dr. Leyden, Dr. Mitchell, and others, seem to suppose. Many, at least, of the morbid phenomena *neuritis* is invoked to explain, it seems to us may be explained in some better way.

Then come vaso-motor paralyses. Our author regards their existence as questionable. By vaso-motor paralysis, the author appears to mean, if we understand him, paralysis caused by vaso-motor disturbances, not in the cord, but in the paralyzed parts. We would certainly agree with him if this is his meaning—that their existence is doubtful. But we do not so regard vaso-motor paralysis depending on vaso-motor disturbances in the nerve centres. We conceive such paralyses may and do occur.

He calls attention to Cruveilhier's and Charcot's distinction of paraplegias into painful and the not painful, the former supposed to result from gradual compression of the cord, the latter not. He next refers to the well-known fact that according to the completeness of the compression at any given point, so is the increase in reflex irritability, and that the capacity for voluntary motion is usually lost before the sensibility of the parts below the compressed point.

The so-called reflex paraplegias, arising on account of disease of the urinary organs (Stanley), or in certain dysenteries, etc., our author considers to belong to those cases depending on an ascending neuritis, which finally reaches the cord, so as to generate a myelitis. But we cannot agree with him here. He also mentions hysterical and lead paralysis. Then comes paralysis of the arms, or *diplegia brachialis*, and he discusses the question how it can happen that the arms may be paralyzed and not the legs. This, however, in case of limited lesions, need not be a matter of surprise. He attributes it most frequently to hæmorrhage in the gray substance and to ascending neuritis. But we do not believe this latter can be a frequent cause of such troubles. As to the former, it may be it often happens. A centre that is used so much as that is, from whence the nerves to the arms are given off, must often suffer, for the liability of a part to disease is in proportion, as a rule, to its activity. Hence the comparatively more frequent disease of the lumbar and brachial enlargements of the cord. Then follow a few remarks on the seat of general spinal paralysis, and also Duchenne's *Paralysie Generale Spinale Anterieur*, which he described as having its seat in the anterior columns of the cord, or rather in the anterior horn of gray matter in the cord. Its more characteristic symptoms are very properly described as: 1, Weakening and progressive paralysis of voluntary motion, first in the lower, then in the upper extremities. 2, Loss of muscular irritability in the muscles of the paralyzed members. 3, Atrophy and fatty degeneration of the paralyzed muscles. In its progress it is painless, and, except, perhaps, in its later stages, it leads to no disorders of intelligence, nor, except in the paralyzed muscles, to any nutritive disorders.

Then the acute ascending paralysis of Grandry is described, and its general characters dwelt on, more particularly its rapid ascent of the cord, and the continuance of electrical irritability in the muscles, in which it differs from Duchenne's paralysis. But in Grandry's form of paralysis, there seems to be no characteristic morbid change discoverable after death.

Dr. Leyden next mentions *paralysis cruciata* or *alternans*—*crossed* or *alternate* paralysis. A common form of this kind of paralysis is that in which there is paralysis of the facial nerve on one side, and of the arm and leg on the other, and which has its probable seat, according to Gubler and our author, in the upper part of the medulla oblongata or pons varolii, of the same side as that of the paralyzed facial. In case of a paralysis of the leg on one side, and of the arm on the other, our author thinks the seat may be in the interior of the anterior pyramid of the same side as the paralyzed arm.

Our author mentions and discusses the two forms of muscular atrophy, and especially the rapid, progressive muscular atrophy and appears to adopt the conclusion, that the true seat of the disorder is in the supposed trophic cells, in the anterior horns of gray matter, in the spinal cord, and with which the fibres of the motor roots of the spinal nerves are believed to be connected. But we notice he does not seem to have made himself acquainted with the masterly researches of Friedrich, of Heidelberg, on this subject, some of the results of whose studies have been published from time to time in Virchow's Archives, and whose elaborate monograph has but recently appeared.*

Our author next passes to a study of convulsive disorders, beginning wlth tetanic affections. The chief pathological element in such cases the author takes to be a morbid increase of reflex irritability, which makes the slightest in-going or excitor impressions give rise to a reflex contraction of muscles. He refers to the observations of Schrœder Van der Kolk, as to the hyperæmia of the gray substance of the cord, with small clots in the same, and the tetanic cramps of cerebro-spinal meningitis, and of certain forms of myelitis, &c. He also mentions the now well-known fact that the reflex excitability of the cord is increased by such substances as alcohol, ether, chloroform, opium and its preparations, picrotoxin, nicotin, and, most remarkably of all, strychnia. But it seems, that in explaining the action of some of these agents, enough stress is not laid on the diminution of the inhibitory power of the centres, in the medulla and brain, as regards the spinal cord.

Our author next describes the seat of *epilepsia spinalis*, or spinal epilepsy. Under this head, among other points, he calls attention to Nothnagel's attempt to fix the centre for the co-ordination of muscular movements, and hence the general centre for convulsions; or the *nodus epilepticus*,† which, according to him, is in the posterior surface of the upper end of the medulla oblongata, and, perhaps also, of the corpora quadrigemina.

He calls in question, but, as it seems to us, on insufficient grounds, the trustworthiness of Schrœder Van der Kolk's views as to the pathological basis of epilepsy, in which great stress is laid on the enlarged state of the vessels of the medulla oblongata. We believe in the essential soundness of Van der Kolk's views, than whom few, if any, have done more careful or reliable work in the whole domain of nervous pathology.

He then briefly discusses Brown-Sequard's, Van der Kolk's, and Westphal's experiments, in producing artificial and transmissible epilepsy in Guinea pigs, and especially the results of the microscopical investigation of the latter, which showed that all

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* *Ueber progressive muskelatrophie, ueber wahre und falsche muskelhypertrophie. Von N. Friedrich. Mit* 11 *Tafeln.* 4*-to. Berlin:* 1873.

† *Die Enstehung allgemeiner Convulsionen. Von Pons u. der Med. Ob. Aus. Virch. Arch.*, 1868; 34 *Bd.*, *p.* 1–12.

parts above the medulla oblongata were normal, but that in it and the cervical, and even the dorsal portions of the spinal cord, the vessels were not only enlarged, but many small hæmorrhagic clots were found in the same parts,—or even outside of the cord, or at the base of the brain—as a testimony to the severity of the congestion that had existed. But we cannot now stop to consider the full significance of these facts observed by Westphal. Our author, singularly enough, does not seem to be aware of their pathological importance.

Our author then relates a case in detail from Virchow's Archives, Bd. 55, p. 1–12, in which the appearances so perfectly correspond to those recited by Westphal, as to mutually confirm and support each other. But we cannot dwell on this now. He also calls attention to the cases of Brown-Sequard, in which sections of the spinal cord low down, or even of the sciatic nerve, produced a disorder like epilepsy, if the skin was irritated at or about the angle of the jaw. He concludes, especially in view of the last experiments, "that an epileptiform affection may have its primary seat, or *fons et origo*, in either the spinal cord, or in a peripheral nerve." He criticises Brown-Sequard's list of cases, drawn from various sources, in which wounds or diseases of the cord led to epileptiform disorders, the latter forgetting, apparently, that hundreds of cases of disease and wounds of the cord, do *not* lead to epilepsy, and that many cases do not arise in consequence of injuries, so far as we can tell. But his criticism appears, to us, open to objections.

In paralysis-agitans, our author appears to agree with Charcot and Vulpian, in making it depend on sclerosis of the cord, especially its upper portion. Two phenomena are worthy of especial study in this disease: 1. Paralysis, generally partial, but always in some degree, of the affected muscles. 2. Tremulous action—wholly involuntary. The first is not so difficult to account for as the last. What maintains such steady, but at the same time, intermittent, action of the affected muscles and centres? The action ceases, generally, during sleep, as in chorea. What can excite the affected centres so rhythmically? Dr. Leyden mentions Jouffroy's *post mortem* investigations, in which there was present, enlargement of the central canal of the cord, with proliferation of its lining epithelium, and sclerotic patches in the cord and medulla oblongata. He also compares with paralysis-agitans, tremor and the subsultus tendinum of fevers, and the mercurial and saturnine and alcohol tremors, but the comparison is so brief as to throw no light on the pathology of the morbid condition on which the tremors depend.

Then follows a consideration of that important class of disorders known as ataxies, or disorders of locomotion. In spinal ataxies, our author thinks the seat of the disorder may be exclusively in the pons, cerebellum, or corpora quadrigemina, on the one hand, or, on the other, in the posterior columns of the spinal cord. He mentions the chronic loss of co-ordination in drunkards, and in hysteria and following certain acute diseases, but does not discuss these phenomena. In discussing the symptomatic differences between cases of loss of co-ordination by reason of disease of the cerebellum, and of the spinal cord, he seeks to draw a diagnostic distinction. He calls attention to, and discusses at some length, Duchenne's distinction, viz.: that in the former there is dizziness, in the latter, not. He then gives an account of a highly interesting case of well-marked ataxy, in which the lesion was found in the pons, on disease of which, it is the author's opinion, many cases of ataxy depend.

Our author endeavors to fix the organic seat of chorea, and mentions briefly the opinions especially of Arndt, L. Meyer, and others, that its seat is chiefly in the brain. He dismisses this view somewhat abruptly, but as it seems to us, not justly, especially in view of the recent researches into the physiology of the brain, which leave room for supposing the organic seat of a muscular disorder may be in the cortex of the cerebral hemispheres.

The experiments of Chaveau on choreic dogs are cited, where the cord was divided between the atlas and occipital bone, and yet the chorea continued; and also the conclusion reached in the experiments of Onimus and Legros, that the "seat of chorea is in the nerve cells of the posterior columns of the cord, or in the nerve fibres connecting these with the corresponding motor cells of the cord." The effect of the induced and constant currents is also noticed when they are applied to the cord. (Page 119.)

Then follows an analysis of separate or single symptoms to determine their value, or to ascertain what interior conditions they depend on. And first, those in the sphere of the *motor apparatus.*

He first directs attention to the methods of determining the degree of contractile force a muscle or group of muscles have, either approximately or by the use of a dynamograph, by which the same group of muscles on the two sides of the body may be compared. He then considers the capacity for *immediate* action, and for *continuance* of action of a muscle, and says they should be clearly distinguished. A muscle may be able to act perfectly well for a short time, but soon becomes exhausted or unsteady in action, while another muscle may have its capacity for action greatly diminished, but its endurance or persistence of capacity for action remarkable. Under these two relations, Dr. Leyden considers the various diseases in which one or other or both states exist, such as muscular atrophies and pseudo-muscular hypertrophy, etc. But we cannot follow farther in this direction. He then considers the degree and significance of muscular atrophies. The principal point is to observe, not so much the *degree* of the atrophy as its *rapidity.* He notices the now pretty well-known fact, that in cerebral paralysis, for example, in which the spinal cord remains intact, the atrophy is slow and depends on disuse. But in true spinal paralyses, or those in which the trophic centres in related portions of the cord are involved, the atrophy is rapid and often extreme. In the former case, the electrical irritability of the muscles continues good, in the other it is diminished or lost.

Next, our attention is called to muscular hypertrophy, especially false, lipomatous, or fatty hypertrophy. In these cases, notwithstanding the *appearance* of hypertrophy, there is, in *reality*, atrophy of the muscular tissue, as was shown by W. Mueller. The pathology of this and other forms of muscle change, in connection with, or in consequence of, nervous disease, will be fully noticed in the next number of the JOURNAL, in a review of N. Friedrich's work. In connection with false, he also mentions true hypertrophy of the muscular fibre without corresponding increase of the connective and fatty tissues, as observed by Auerbach, Berger, and others. He recites the opinions of various observers as to the nature and relations of true muscular hypertrophy, some holding it to be a disease *sui generis*, as Hitzig; others that it constitutes the beginning stage of fatty degeneration with hypertrophy, as Auerbach, Berger, and others, in particular, citing a case of *Charcot*, in which all stages from hypertrophy (true) to fatty degeneration and true atrophy of the muscular fibre, were observed in one and the same case.

He then discusses the electrical irritability of muscles, and the condition on which it depends, especially as it relates to the nervous system. Mention is made of the original observations of Marshall Hall, and the criticisms to which Duchenne subjected them, and also the part taken in the discussion by Althaus, and others, and the conclusion which the most recent observations seem to confirm, that the irritability of the muscular fibre depends simply on its structural integrity. In case of true atrophy, especially if accompanied by fatty degeneration, the electrical excitability is diminished or lost, or the contrary if this is not so. This state of the muscles follows if they are cut off from their trophic centres, or if those centres are destroyed. These centres are in the anterior gray horns of the cord. Hence, rapid atrophy with loss of electric irritability indicates either lesion of the nerve trunks extending to the muscles and cords, or disease in the trophic(?) cells in the cord.

An observation of Duchenne's is referred to, that in disorders of the lower parts of the cord, there is more likely to be rapid atrophy of the muscles, with loss of their electrical excitability, than if the disorder is situated higher in the cord. But this is what we might expect from what has been already said, especially in case of paraplegias, for there would be a greater likelihood that the trophic cells of the nerves leading to the muscles would become involved.

After noticing the different effects of the two currents—galvanic and induced, on the muscles, and the diplegic contractions first observed by Remak, and the various explanations given of them, he considers at some length, increased reflex irritability of the nervous centres, but leaves wholly out of the account, diminished inhibitory action of the higher centres, in which the increased reflex excitability may be only relative.

Our author describes a form of muscular disorder sometimes observed, which consists in a peculiar muscular rigidity or stiffness, in which the muscles are unnaturally slow either to act, or to cease action under an impulse from the will, and are often arrested in action in a tetanic way. In the case he cites at length, the muscles appeared well developed. Our author seems inclined to fix the seat of morbid change in the muscle itself, in a change similar to that of *rigor mortis*. But whether it is a disease of the muscles, *sui generis*, or one consequent on disease of the nervous system, our author does not undertake to determine. Indeed, he very seldom expresses an opinion of his own, but contents himself generally with a summary of those of others. Without dwelling longer on what is said under the head of motorial disorders, we turn to the symptoms and conditions of disease in the domain of the senses.

Pain and *hyperæsthesia* are first considered. Mention is made of various morbid sensorial phenomena, such as formication, sense of constriction about the head in some cases of cerebral congestion, and certain perversions of the sense of temperature, etc., but in few or none of the cases are discussions given, or opinions offered, as to the conditions on which they severally depend. An imperfect discussion of the muscular sense, as to its reality, etc., is given, in relation to which our author would appear to have made quite extended researches, but without very clear results, so far as appears in this work.

In regard to disorders in the sphere of the sympathetic, Dr. Leyden seems to be aware of the origin of the sympathetic nerves in the cord, and has directed his attention to disorders in the sympathetic consequent on disorders of the spinal cord. He speaks of changes of color, on account of contraction or enlargement of the muscular vessels through influences conveyed to them by the vaso-motor nerves, which, in turn, are affected by diseases of the cord, and of changes in temperature, in parts produced in a similar way, and more especially if, at the same time, there is a cyanotic hue. He explains these phenomena by reference to a failure of action in the muscular vessels on account of disorders of the sympathetic, and, as it seems to us, correctly. He mentions that in paralysis, as a rule, perhaps, a paralyzed member has a lower temperature than the other. He gives, also, a summary of the experiments of Weinhold, Wilson Philip, Chosat, Legallois, etc., and refers to the well-known case reported by Sir Benjamin Brodie, of a man in whom a penetrating wound in the cervical portion of the vertebral canal, led to the slowing of the respirations, five to six a minute, and to a slow, small pulse, and a temperature of 111° F. in one leg. He also refers to the observations of Wunderlich on temperature, in diseases having their seat in the spinal cord, which observations, with others, show that the regulation of the bodily temperature depends much on the spinal cord and its varying conditions. But we expect to treat this question separately hereafter.

Trophic Disorders.—Our author devotes considerable space to this subject, and very properly. We will simply give an outline of the author's views, for it is expected that the fourth or last of the series of lectures on the pathology of the vaso-motor nervous system in this journal will contain a full discussion of this subject. He especially mentions muscular atrophies and false hypertrophy, and reaches no decision as to their pathology, for he says, "Whether we deal with a primary muscular disorder or with a disorder having its beginning in, and its existence as a consequence of, a nervous disorder, are questions which have not, as yet, been settled."

He also notices the symptoms of disorder in the actions and conditions of the alimentary canal, thought to be consequent on disease of the spinal cord, but there is nothing new, or even suggestive, in this part.

His section on the influence of the diseased cord on the urinary secretion, is little more than a discussion of the effects of retention of urine on the urinary passages, which should not be discussed in this place. But many paraplegics that die soon, go down on account of a secondary bladder affection induced, apparently, by the paralytic disorder.

Our author asserts again that not only disease of the cord, but of the brain, may be caused by an ascending neuritis. But we still see no reason for admitting this as a fact of frequent occurrence. The brain may, by sympathy, suffer from disease of the cord, or *vice versa.* The latter is to be seen in the secondary degeneration of Tuerck, and in progressive paralysis of the insane, and, according to Westphal, in hæmorrhages, meningitis, tubercles, etc. This part is instructive, but does not contain general truths of so much importance as to be of surpassing benefit to the reader. Many disorders of the special senses, or the outer apparatus of the same, are mentioned, but nothing new or of importance is noted.

Our author passes next to a consideration of the symptomatology and etiology of diseases of the spinal cord. He notices the fact that the cord and brain, being situated as they are, cannot be studied in the same manner as other organs, like the lungs or liver. We cannot do more than study them in secondary symptoms. Clinically, these are the most important for study. He notices in what respect we have gained in the progress of the anatomy and physiology of the nervous system by connecting certain symptoms with definite lesions of the interior.

Under the head of etiology he gives a list of the causes of spinal cord diseases. The list is full and instructive, but can hardly be condensed, and contains too few generalities to enable extracts to be made. The course of most such diseases is long, unless they lead rapidly to death. They give rise to trains of symptoms and disorders as various as can be, and, on the whole, open a most remarkable field for observation and study.

Then follow condensed remarks on "general therapeutics," in which it is admitted some substantial progress has been made. He briefly mentions the "antiphlogistic method," by means of blood-letting, and the use of cold, the latter applied by means of Chapman's icebags, or some similar contrivance, in acute diseases of the cord. Then follows derivation on counter-irritation, on which he is inclined to place small reliance.

We next have a few remarks on the use of mercurials, iodine, strychnine, quinine, iron, ergotin, belladonna, arsenic, nitrate of silver, phosphorus, the nervines, such as musk, valerian, bromide of potassium, cannabis indica, morphine, chloral, etc. Liniments are useless, he thinks, except to amuse people. Diet, baths and hydrotherapy, medical gymnastics, and medical electricity induced and constant, are discussed, in regard to which latter a middle position is taken, but nothing worthy of note is found which is not more fully stated in accessible works on the medical uses of electricity. This section closes with a few remarks on the use of statical electricity, especially by means of the Holtz machine, referring to the experience more particularly of Schwanda and Fieber. He says, himself, that he has no experience with this latter method. Then follow a few remarks on the therapeutics of bed sores and cystitis, etc., as these affections occur in paraplegias, and this ends the general part of the book, which it is alone our intention to examine in this notice. It is the work of a conscientious and faithful laborer, who can collect, and, without a fine sense of discrimination, investigate facts. He lacks in the power of generalization and of lucid statement, but has succeeded in producing a highly useful work, so far as it has gone. In this notice it has been our aim to give only a running commentary on the general part of Dr. Leyden's work, that our readers might be able to see the course that is taken and the character of the materials that have been gathered by a laborious and prominent worker in the domain of neurological medicine. When the second volume appears we will review the special part for the pages of the JOURNAL.

CARPENTER—MENTAL PHYSIOLOGY.*

PRINCIPLES OF MENTAL PHYSIOLOGY, WITH THEIR APPLICATIONS TO THE TRAINING AND DISCIPLINE OF THE MIND, AND THE STUDY OF ITS MORBID CONDITIONS. By William B. Carpenter, M.D., LL.D., F.R.S., F.L.S., F.G.S., Etc. London: 1874. 737 pages.

We have no hesitation in declaring that for the physician and scientific reader this is the most practical, not to say most novel work of its class that has appeared for many years. It is the latest, and is in some respects, most important of the now rather large list of works from the pen of Dr. Carpenter.

In it we discern, and without abatement, the same unaffected precision in analysis, naturalness in method, clearness and facility in language and exposition, sound judgment and practical spirit, that has characterized his other admirable writings, with which every physician must have become more or less familiar.

That, taken altogether, no one living is better qualified than Dr. Carpenter to write a work on psychology, from the standpoint of the physiologist, few, we presume, would question. For nearly half a century has he been engaged in scientific pursuits, and but few scientifie men have been more highly favored in their opportunities than he has been, at the British capital, and but few have turned them to better account. His success has been due, it is probable, not more to his fortunate associations and surroundings, than to the impulses of his early training and the possession, naturally of generous sympathies, and a well balanced and eminently practical mind.

Dr. Carpenter is one of the very few scientific men of the present time, or any time, who has been able to maintain a fair equilibrium in culture, as between scientific and speculative modes of thought. He is apparently equally at home, on the one hand, with the scientific investigator, pure and simple, who looks on speculative movements only to contemn them, or, on the other, can turn about and enjoy, not only the personal friendship, but an intelligent sympathy with the speculative labors of Mr. Mill, or better yet, with those of that first among living English philosophical critics and metaphysicians—James Martineau.

Thus, not only by the range of his scientific studies, but by a very considerable degree of acquaintance and sympathy with

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* NOTE.—We had intended to have taken into this review the recent work of Professor Wundt, of Heidelberg, entitled "*Grundzuge der Physiologischen Psychologie;*" but the last volume came to hand too late for a notice in this number.

psychological science, as expounded by such masters as Sir William Hamilton, Mr. Mill, or Mr. Martineau, is Dr. Carpenter fitted to produce a work that should command the respect of both of the reigning schools of psychology. Such a work is the one before us.

We now invite our readers, who may not have read the work itself, to follow us, while we endeavor, with a practical aim, to make our way through it. As the author himself tells us, it contains essentially the same views as he has long since set forth in the fourteenth chapter of his work on the *Principles of Human Physiology*.

We will follow in the main, the path our author has marked out in the development of his subject.

It may not be needless to observe, that in approaching the study of mental phenomena from the standpoint of the physiologist, we must do so through a study of the mechanism and modes of action of the nervous system. Accordingly it will be our first care, to ascertain what are our author's views in regard to the same—at least, in so far as may be necessary to the understanding of his psychological views.

Losing sight, for the moment, of the cerebral and spinal nerves, which serve simply the purpose of conductors of nerve impression to and from the nerve centres, and also of the sympathetic nervous system, which Dr. Carpenter, in common with others, considers to have but few, if any, *direct* relations to *mental* actions, to say the least, we are limited in our view to the spinal cord and brain, and the intermediate parts connecting these extremities, viz.: the pons varolii and medulla oblongata. These, taken altogether, constitute one continuous nervous mass—the *cerebro-spinal axis*. This axis, according to Dr. Carpenter, may be divided into three parts. They are:

1. *The Spinal Cord.* This includes the whole cord and part of the medulla oblongata. It contains the nervous centres or mechanisms on which the movements of the voluntary muscles, at least those of the trunk and extremities, are dependent. All sensory—or as Dr. Carpenter prefers to call them "*excitor*"—impressions sent into the cord in any part of its length, if not transmitted higher up, we do not become conscious of them. They either come to naught, or excite the related motor centres in the cord to action, and by consequence an outgoing motor impulse is the result. This outgoing or reflex impulse may be conducted outward, along cerebro-spinal motor nerves to the muscles, thus leading to reflex muscular action. Or the outgoing motor impulse may be reflected from vaso-motor centres, out along vaso-motor nerves, either to the vessels or to glandular structures—modifying their action. But all these actions may, and often do, happen, without the subject being conscious of them. In other words, the seat of consciousness, wherever that may be, is not in the spinal cord. It is the seat of a vast number of reflex actions, and, for such purposes, it is

perfect in itself. Hence we may consider the spinal cord, as has often been done, as a part of the nervous system, having important relations of subordination to the brain above, it is true, but still possessing an important sphere of its own. It is that part of the central nervous system which is directly connected with nearly all parts of the body, and the influence between the spinal cord and those parts of the body with which it is connected, is reciprocal.

It receives, in some part of its course, the greater number of centripetal impressions sent in from all parts of the body, along the sensory nerves, and out from it, most motor impulses are emitted.

Dr. Carpenter objects to the application of the terms "sensitive" or "sensory" to those centripetal or ingoing impressions, which we never become conscious of, but which excite to reflex acts of which we are, in a sense, conscious. He would prefer to call such impressions simply "*excitor.*" But we can see no good purpose, at this late day, that would be subserved by such a change in names. In a certain sense such impressions are "*sensory*" or "sensitive." There is some sort of capacity for *feeling* them. And on the other hand those impressions which we become conscious of, are "excitors" to action, motor or mental. That there are two fundamental forms of sensibility—conscious and unconscious—has been clearly recognized since the time of Bichat, to go no further back. We see no sufficient reason for a departure from the use of the term to which objection is made, unless it be shown more marked advantage would result than can at present be seen.

2. The second division of the cerebro-spinal axis comprises the upper portion of the medulla oblongata, pons varolii, and the great sensory ganglia at the base of the brain. This remarkable group of ganglia constitutes, taken as a whole, the *sensorium* of Dr. Carpenter, or that part of the nervous system which is the seat of true *sensation* and of *consciousness.* Any nervous action that takes place outside of this region, whether in the spinal cord below or cortical substance of the brain above, we are not conscious of. Dr. Carpenter holds, in common with others, that the *thalamus opticus* is the probable seat of common or general sensibility, as that of tact or touch, etc., while near it lie the centres for the more special senses, such as those of sight, smell, hearing, etc.

Every sensory or "excitor" nervous impression, no matter from what part of the body it comes, must be transmitted to the appropriate sensory ganglia of this region of the central nervous system, before we can become conscious of it as a sensation. This is the so-called *sensorium commune.* It is also the general seat of the emotions, which are so closely allied to the sensations.

3. The third and final division of the central nervous system, comprises at least the cerebral hemispheres, excluding the gan-

glia imbedded in their base, and including more particularly the gray matter of their surface—the "cortical substance," so largely composed of nerve cells. This division may also be regarded as comprising the white or so-called medullary substance filling the interior of the two hemispheres, and which consists mainly of vast numbers of white nerve fibres, that pass both ways between the cells of the cortical substance of the hemispheres, and the cells of the ganglia that are comprised in the *sensorium* below, and also of fibres that connect different parts of opposite hemispheres together, or even different parts of the same hemisphere.

The cortical substance of the hemispheres (or the cells that are found in it) is held to be the seat of our intellectual operations, though not the seat of consciousness. The latter, according to Dr. Carpenter, is the *sensorium* or great group of sensory ganglia at the base of the brain, already referred to. Up to this point we have been on the ordinary or beaten track. But now we come to certain views in relation to the functions of the hemispheres and of the *sensorium*, and of their relations to each other, which, though not peculiar to Dr. Carpenter, he has yet used with peculiar clearness and practical effect in the domain of cerebral physiology. Dr. Carpenter holds that the operations which take place in the cortical substance of the hemispheres, if they are confined to it, we are as truly unconscious of as if they occurred in, and were limited to, the spinal cord. It is only when they are repeated in, or, as it were, reflected on, the ganglia of the *sensorium* below, that we become conscious of them. This is done —when it is done—by means of the fibres which connect the cells of the cortical substance with the cells of the sensory ganglia, which, taken together, constitute the sensorium. Impressions sent up to the sensorium from the spinal cord are recognized as *sensations*. Impressions sent down from the cortical substance of the hemispheres to the *sensorium* are recognized in our consciousness as mental states or operations. The one connects the sensorium with the physical world as represented in our bodily organism, the other with the mind, as it acts on the cortical substance of our hemispheres.

Thus it appears, that the central axis of the nervous system consists of two poles or extremes, the activities of which we alone become conscious of, when they or their results are communicated to the *sensorium*, that lies between and connects them. As many of the actions of which the spinal cord is the instrument are confined to it, and are never made known in the sensorium, so for the hemispheres above.

At this point we will drop the spinal cord out of sight, and devote attention chiefly to the hemispheres and their relations to the sensorium. And here comes in, perhaps, the capital point of Dr. Carpenter's plan of the nervous system and its mode of action. We now refer to what he has called from the physiological side "*unconscious cerebration*," and that Sir William Hamilton has described from the psychological point of view as "unconscious mental action." By the phrase "unconscious cerebration," Dr. Carpenter, as is well known to many, refers to those actions of the cortex of the brain, of which we are not conscious, and which involve frequently, as there is reason to think, long trains of mental action, even when we are nominally asleep, the results of which action are afterwards made known in consciousness. The unconscious, as well as conscious action of the hemispheres, may be excited either by impressions sent up to them from the sensorium, or possibly by impulses communicated directly to the cortical substance by the free activity of the mind. As to the *sensorium* itself, it may be aroused into an active state either by impressions sent down from the hemispheres above, or that reach it by way of the spinal cord below. But for this representation in the sensorium, by means of impressions conveyed along nerve fibres, we would not be conscious of what is going on in the world without, or the world within us. To accomplish these ends, the animal, more particularly man, is provided with a nervous mechanism or veritable piece of machinery, that it is of the highest importance to all concerned to understand. One of the most remarkable things about this mechanism, consists in its capacity for being added to, developed, or perfected, in accordance with its customary activity. It seems possible to shape and train this living mechanism permanently, so it shall acquire new aptitudes and powers, or, as Dr. Carpenter calls it, "grows to" them.

In the unconscious and independent action of the hemispheres and the actions and re-actions between them and the sensorium, and the play between *thought* here, and *emotion* and *sensibility* or *feeling* there, and in the capacity of the nervous system to be developed and perfected, in accordance with its customary uses and activities, within these limits, we say, will be found most that is peculiar in Dr. Carpenter's work. Though Dr. Carpenter, of necessity from the point of view he has chosen, speaks much of the nervous organization, yet he does not, as some have done, lose himself in it, and fancy there is nothing beside a mere automatic mechanism, which creates the forces expended in its own activities. He maintains an active equilibrium between mere materialistic and spiritualistic hypotheses. But, having given an outline of Dr. Carpenter's mode of looking at the nervous system and its workings, let us see briefly how he fills up that outline. In doing this we shall in the main follow the path the author has chosen himself:

Chapter first is concerned with the "*General Relations of Mind and Body.*" In the beginning, Dr. Carpenter contrasts, in fair and comprehensive terms, the "materialistic" and "spiritualistic" hypotheses, in regard to the "relations between mind and body."

On the one hand, he makes plain the necessity of recognizing the *organic conditions* of mental actions, in which conditions some are inclined to rest, while others are inclined to neglect or disparage them. On the other hand, he calls attention to the "*self-directing* power," of the possession of which, every well-regulated mind is conscious, and which does not seem to be directly dependent on known organic conditions. He concludes the brief contrast to which we have alluded, with reflections so judicious, we will quote a paragraph containing some of them:

"This combination of two distinct agencies in the mental constitution of each individual, is recognized in the whole theory and practice of education. For whilst, in its earlier stages, the educator aims to call forth and train the intellectual faculties of his pupil, and to form his moral character, by bringing appropriate *external* influences to bear on him, every one who really understands his profession will make it his special object to foster the development, and to promote the right exercise of that *internal* power, by the exertion of which each individual becomes the director of his own conduct, and, *so far*, the arbiter of his own destinies. This power is exercised by the will, in virtue of its domination over the *automatic* operations of the mind, as over the *automatic* operations of the body; the real *self* formation of the ego, commencing with his consciousness of his ability to determine *his own* course of thought and action. Until this self-directing power has been acquired, the character *is* the resultant of the individual's original constitution, and of the circumstances in which he may have been placed; and so long as the circumstances are unfavorable to its development, and to the operation of those higher tendencies, which should furnish the best motives to its exercise, so long the character of the individual is formed *for* him rather than *by* him. A being entirely governed by the lower passions and instincts, whose higher moral sense has been repressed from its earliest dawn, by the degrading influence of the conditions in which he is placed, who has never learned to exercise any kind of self-restraint, (or, if he has learned it, has only been trained to use it for the lowest purposes,) who has never heard of a God, of immortality, or of the worth of the soul—such a being, one of those heathen outcasts, of whom all our great towns are, unhappily, but too productive,—can surely be no more morally responsible for his actions, than the lunatic who has lost whatever self-control he once possessed, and whose moral sense has been altogether perverted by bodily disorder. But let the former be subjected to the training of one of those benevolent individuals, who knows how to find out 'the holy spot in every child's heart;' let patient kindness, continually appealing to the highest motives, which the child *can* understand, progressively raise his moral standard, and awaken within him the dormant susceptibilities which enable him to feel that he has a conscience and a duty, that he has a power within himself of controlling and directing his thoughts and actions, and that the highest happiness is to be found in the determinate pursuit of the *true* and the *good*,—then, but not till then, can he be justly considered *responsible* for his actions, either morally or religiously; then only does he rise above the level of the brute, and begin to show that he is indeed made in the image of his Creator.

"Thus we see," he continues, "that the materialistic and the spiritualistic doctrines alike recognize, and alike ignore, certain great truths of Human Nature; and the question returns upon us, whether any general expression *can* be formed, which may be in harmony alike with the results of scientific inquiry into the relation of mental to physiological action, and with those simple teachings of our own consciousness, which must be recognized as affording the ultimate test of the truth of all physiological doctrine. Towards such an expression we may make a step, as it appears to the writer, in strict accordance with true philosophy, by withdrawing ourselves entirely from the futile attempt to bring matter and mind into the same category, and by fixing our attention exclusively on the relation between *mind* and *force*." (Pages 9 and 10.)

This passage may serve, in some sort, at once as an example of the comprehensive and judicial character of the author's views, and of the purpose and animus of his book.

In choosing to direct attention to the relations of *mind* and *force*, rather than to those of *mind* and *matter*, Dr. Carpenter appears, to us, with true philosophical insight, to have placed in view the two fundamental elements of the problem involved in his work.

Contrary to the views of those who belong to the *realistic* school, of which Sir William Hamilton was the type, he holds, and we think, correctly, that we do not know matter directly or intuitively, or by direct contact, as it were, but that we know it *mediately*, through the *forces* with which matter is endowed. These act and re-act on mind, in their reciprocal activities, and so force becomes the subtle medium of interplay between these two great substantial realities. But, much as we are tempted to do so, we must refrain from these speculative matters, for the purpose of developing practical ones. Before leaving this subject, however, we must notice a few points that appear in Dr. Carpenter's analogies of the relations of *mind* and *force.*

In contrasting the *activity* of mind with the *passivity* of matter, Dr. Carpenter says: "but *mind*, like *force*, is *essentially active*," etc. This means that force,—*physical* force—like mind, is *self*-active, capable of begetting *its own* activity. This we regard as fundamentally erroneous. Electricity is a form of physical force. Can it be maintained of it, that it is *self-active?* It is foreign to the scope and purpose of this review to enter on this question, but we will confidently await the result if Dr. Carpenter will look this question steadily in the face for awhile. In the possession or want of capacity for self-action or *essential activity*, lies, if anywhere, the fundamental distinction between mind, as a force or power, and the various known forms of *physical* force.

Again, Dr. Carpenter, in speaking of the physical changes in the brain and nerves, that correspond to, if they do not constitute, physical changes, such as when we feel or think, says: "There is just the same evidence of what has been termed *correlation* between *nerve-force*, and that primary state of mental activity, which we call *sensation*, that there is between *light* and *nerve-force*, each antecedent, when the physiological mechanism is in working order, being invariably followed by its corresponding consequent." (P. 13.) Again, "The like correlation may be shown to exist between mental states, and the form of nerve-force which calls forth *motion*, through the muscular apparatus." Or, again: "The *correlation* between *mind*-force and *nerve*-force, is shown to be complete *both ways.*"

We have not cited the whole of the passages from which these extracts are made, the only point in them to which we would call attention, being that which affirms *correlation*, as between *light* and nerve-force, and between *mind* and nerve-force. Now, what does the word "correlation" mean? We hear of the "*correlation* of the physical forces"—what is meant by this phrase? Says Mr. Grove, in his "*Correlation of Physical Forces*," the word "*correlation* means a necessary or reciprocal dependence of two ideas, inseparable, even, in mental conception." Or, again, "it means *necessary reciprocal reproduction*," and so on for many other definitions.

Forces said to be correlated are understood to be capable of complete conversion, the one into the other and *vice versa*—this wholly into that, that wholly into this. Forces, or portions of them, if we may so speak, between which such interchange cannot take place, are not *correlated*. As Mr. Grove says, it means "*necessary* mutual reproduction," and nothing less. Consequently, when we speak of the *physical* and *chemical* forces on this hand, and the "vital" and "mental" on that, as being *correlated*, we mean that *heat*, for example, may be converted into *life-force* or *mind-force*, and that *mind-force* and *life-force* are wholly convertible into *heat*.

Now, if these remarks are just, can it be said that, "nerve force" and "*sensation*" or, that *light* and *nerve-force* are *correlated*, or "mutually convertible," or that "the correlation between *mind*-force and *nerve*-force is shown to be complete *both ways?*" Not at all. We have no space in this notice to enter on this question fully, but we have no hesitation in declaring, there is no real warrant, as yet, for any such statements as are made above. Either Dr. Carpenter means something different from what other good writers do, when he uses the word "correlation," or his statements are wholly untenable. Such is our conclusion after a rather protracted study of this question.

In speaking of the "automatic activity of the mind," Dr. Carpenter says: "that there are a great number of mental phenomena, which cannot be accounted for in any other way, than as resulting from the operation of a physiological mechanism, which may go on, not only *automatically*, but even unconsciously. That we are not always conscious of the working of this mechanism, is simply because our sensorium is otherwise engaged: for, just as we may not see things which are passing before our eyes, or be conscious of the movements of our legs in walking, if our attention be wholly engrossed by our cerebral 'train of thought,' so may we be not conscious of what is going on in our cerebrum, whilst our attention is wholly concentrated on what is passing before our eyes. But, the physiological mechanism has this peculiarity, that it *forms itself*, according to the mode in which it is *habitually* exercised; and thus, not only its *automatic*, but even its *unconscious* action comes to be indirectly modified by the controlling power of the will."

Dr. Carpenter then goes on to say, that "it may serve to promote a right understanding of the general doctrine as to the relation of Will to Thought, which it is the chief object of this treatise to set forth, if we briefly enquire into the relation of the will to bodily movements." (Page 15-16.)

We have quoted the above passages, so as to have before us and our readers, in our author's words, the chief purpose of his work.

Inasmuch as it will be impossible in the limits assigned this notice to review in detail Dr. Carpenter's book, we will state the main propositions it is his purpose to establish and apply in the domain of "mental physiology." They may be stated as relating to:

1. The capacity which the brain has in common with other parts of the nervous system, of being *developed*, in conformity with its modes of action, especially those that are habitual or customary, whether for good or ill.

2. The mode of action of the cortical substance of the hemispheres and its relations to the *sensorium*, or mass of sensory ganglia, that lie at the base of the brain.

These propositions, and others, are worked out with so much clearness and fullness, and such varied and apposite illustration, as to make a satisfactory review of the work a difficult task.

But we will endeavor to set before our readers the above propositions in as fair a light as we can, in so brief a space.

1. *The capacity which the brain has, as well as other parts of the nervous system, of being developed in conformity with its modes of action, especially those that are habitual or customary.*

In many of the lower animals, the nervous system is nearly, or quite complete in its *development* at birth. It is—physiologically speaking—as a mechanism, adapted to the performance of all the actions of which the animal is ever capable. This is the case with all those actions that are instinctive or originally automatic, such as sucking or breathing. But as you ascend the animal scale, the class of purely instinctive actions becomes relatively smaller, and by consequence that class of actions that are *learned by experience* becomes relatively larger, until we come to man, in whose case there are comparatively few originally automatic actions, and an almost unlimited *capacity* for learning various kinds of action,—a capacity that becomes progressively less and less marked as we descend the animal series, until among the very lowest animals it almost fades out. Nearly all the various actions performed by animals are accomplished through the agency of the nervous system, and have a peculiar mechanism therein adapted to their performance. In the case of the originally automatic or *instinctive* actions, the appropriate mechanism is complete at birth, while the contrary is true of those actions that have to be learned. They are perfected, or even *developed* by repeated attempts to perform the actions appropriate to them. Good instances of this kind of acts are found in walking or learning to speak, or to play on a musical instrument. There is every reason to think, in the case of the child, that that part of the spinal cord from whence the nerves proceed, that excite and control the movements of the lower limbs in walking, is in a highly rudimentary state, at first, and would never be perfected, unless, at the instance of prolonged effort by constant and long-continued use the part becomes perfected, according to a well-known physiological law. But for this exercise, the part of the spinal cord in question would never attain to perfection as a part of the mechanism of walking. And how long and difficult the process of learning to walk is. So it is for the nervous mechanism of speech. The real point of difficulty in learning to talk is the development and training, by repeated, even numberless efforts, of the nervous mechanism, in the perfection of which, from the physiological standpoint, will be found to lie all niceties of speech, which, in its highest state, is only to be acquired after almost incredible effort. And so for all the actions that depend on experience or education, such as all kinds of manual operations, and complicated muscular actions having determinate ends—all training of the senses, and all mental operations that are acquired, and not instinctive. They all, whether physical or mental, depend in an important sense on the appropriate nervous mechanism with which man is born, in most cases, in a rudimentary state, with an almost unlimited capacity for developement and improvement by well directed and persistent use. Herein lies the possibility, and, in general, the process of education, speaking in physiological terms, by which man is so widely, not to say completely, separated from the animals below him. Thus we see, that while broadly speaking, the originally automatic or *unlearned actions* predominate in lower animals, the reverse is true of man, under the best circumstances. We also see that the educability of a human being does not lie wholly in purely mental capacities or aptitudes, but, also, in the state of the material instrument with which the mind is connected.

At first, in learning to walk, for example, constant attention is necessary to the execution of each step, which is accomplished only with great difficulty and many mistakes. But as time passes, and experience becomes wider, the child walks better, and with less attention to the motions of the limbs, and the maintenance of equilibrium, until at last the individual may walk without being conscious of performing the complicated series of muscular movements involved in such a case. At first, it required a constant strain of attention to execute a step under favorable circumstances—at last, it is done with no more attention being given than if they were originally automatic. In this way do we come to have two classes of automatic actions—*original*, such as breathing, and *acquired*, such as walking. And it is the end of *education*, in the physiological sense, to so develope and train those multifarious nervous mechanisms, to be devoted to various operations, physical or mental, that we are born with in a rudimentary state, that they may be placed on a par in perfection of structure and working with those mechanisms that are the instruments of instinctive acts. In this way all kinds of actions, whether physical or mental, are performed with less and less effort, until the degree of development and training arrives at such a pass, that the actions in question are performed without effort, or even without attention, or, as we sometimes say, "*naturally.*" This is not only true of physical acts, of which walking may serve as an example, but also in the training of the senses, and of all our faculties of knowledge, whose *instrument* in one way or other is the brain. In each and every case the nervous mechanism devoted to the acquired functions is gradually formed and perfected, in conformity to the end it subserves, or, as Dr. Carpenter says, "*grows to*" it. In this way, that which is acquired by laborious effort or experience is *conserved.* Each effort increases the efficiency of the mechanism through which it is made, until all the conscious effort that is necessary, is confined to setting the mechanism in motion—as in walking in some cases—and it runs on in exactly the same way as those do which are devoted to the instinctive actions. Just in proportion as an individual reaches this automatic perfection in his acquired actions, and in proportion to their range and elevation, as regards the ends they subserve, just in the same proportion, all else being equal, is he to be considered as *educated.*

These remarks are true for the nervous mechanism, not only in health, but in disease; not only for good, but for evil. When the nervous system has in some part "grown to" a particular kind of action, so it performs—or ministers to—it *automatically,* then we have what may be called a *habit,* whether good or bad, physical or mental, voluntary or involuntary.

But we have pursued these remarks far enough to show the nature, and in general the practical use of the truth, that the nervous system, more perhaps than any other in the body, "grows to" its customary mode of action.

Dr. Carpenter carries out this truth in a very instructive way in various relations. He shows how in morbid, as well as healthy actions, we have either to hope or fear, that the organic conditions involved in the nervous mechanism may become fixed so as to fasten a *habit* on the individual. He shows that such changes as are contemplated may be transmitted hereditarily. In this way the changes induced in the nervous mechanism, say in epilepsy, may be, as is well known, transmitted hereditarily. In the same way a peculiar acquired type of nerve mechanism favorable to insanity may be transmitted. The same truth is applied to education, whether physical, mental or moral, and in each case is shown to have interesting relations. But we cannot trace farther, even in general terms, this line of thought. It is not mentioned in this place as novel, but because it is a matter of high scientific interest in physiology, and because we do not believe it is looked upon with the interest it deserves. We must confess it never struck us as having so much importance as it has since reading Dr. Carpenter's book. We commend it to the earnest attention, especially of our professional and scientific readers.

2. *The mode of action of the cortical substance of the cerebral hemispheres, and its relations to the sensorium, or mass of sensory ganglia that lie beneath it, at the base of the brain.*

As already seen in the outline we have given of Dr. Carpenter's views in regard to the mechanism of the nervous system, it is his opinion that the action of the cortical substance of the hemispheres is *unconscious*, unless it is made known through the medium of the *sensorium* below it. Collectively, the cortical layer of the hemispheres constitutes the "*ideational*" centres, of Dr. Carpenter, or those whose office it is to form and retain *ideas*, so far as the brain may be said to be concerned in such matters. In conformity with the principle already laid down, that the nervous system "grows to" its customary modes of action, or is more or less permanently modified by even transient activity, so it may be said of the cortical substance of the brain, which is assumed to be the seat of the purely intellectual actions. At first its structure is highly imperfect, but as the senses are played on, in the young subject, and the *sensorium* is tremulous with sensibility, and is rapidly receiving impressions, many of these are transmitted from the sensorium upward, to the cortical substance of the brain, which in turn is excited to action, and to a corresponding development of structure. This is the *elaborative* part of the nervous system, where thinking, judging, and similar intellectual operations are conducted. But they cannot go on without a corresponding development of the *structure* of the cortical substance of the brain, and this increases the aptitude of the part of the brain in question for such action ever afterward, unless there is change or disorganization. At first this part of the brain depends on the sensorium for starting it into action. All sensations felt in the latter, with certain exceptions, are reported up to the "*ideational*" centres, in the cortical substance. During the whole period, especially of infancy and childhood, numberless impressions from the outer world are transmitted, through the avenues of the senses, to the sensorium, where they are felt with varying degrees of intensity. The wonderful activity of the sensorium leads to the development and perfection of its structures, or various nervous mechanisms, which are the seats of the special and other senses. This activity of the sensorium is constantly repeated in or transmitted to the *ideational* centres above, so that what was merely a matter of *feeling* becomes a matter of *knowledge*. In this way, with varying degrees of rapidity and perfection, the *nervous* mechanisms of thought and feeling are developed, the former lodged in the sensorium, the latter in the cortical substance of the hemispheres. The more perfectly they are developed, the more easily do they work. By repetition of the process of feeling or thinking the nervous mechanisms concerned in them become so perfectly adapted to their purposes, that their action may become automatic, not only working without the immediate superintendence of the will, but in some cases in spite of it. But this is not all. The cortical substance or *ideational* centres may act without our being *conscious* of the action. This may happen even during sleep. Such action is called, by Dr. Carpenter, "unconscious cerebration," and was described before, by Sir William Hamilton, as already observed, as "unconscious mental action." In this case the action of the "ideational" centres is either not reported in the sensorium, which, according to Dr. Carpenter, is probably the seat of consciousness, or, if so, is neglected or forgotten. The nervous mechanisms, gradually perfected in the cortex of the cerebrum, may, when once excited to action, go on of themselves, as a mechanism may do that is moved by a spring. One point of importance in regard to those mechanisms of thought and feeling, is that when they are once formed, they may be excited to action in various ways, whether in health or disease. Those in the cortex of the brain, for example, may be excited by something in the blood, such as opium, or some morbid matter peculiar to certain diseases, as in fevers attended by delirium, or by simple congestion of blood in the part in question, etc. In such ways may the mechanisms of thought, when they are once formed, and the *habit* of their working has been once established, be excited either by impulses from the sensorium below, or by direct impulses communicated from the mind. In these ways "trains of ideas" may be excited without any suggestion or direct excitation from the sensorium below. But this is not all. This independent "ideational" action or its results may be transmitted down to the sensorium in such a way as to "revive" sensations or emotions corresponding to the ideas. This may happen either in health, as when the *thought* of something nauseating will produce the *feeling* or *sensation* of nausea, or in disease, as in a wide range of cases of insanity, or in diseases attended by delirium, and in many of the peculiar mental phenomena of hypnotism and spiritualism. In fact, the sensorium and the "ideational" centres may mutually excite each other, both in health and disease. Dr. Carpenter carries out this conception in a very instructive and, we may say, satisfactory manner. He applies it in endeavoring to explain the phenomena of mesmerism, spiritualistic phenomena, delusions and hallucinations, and other phenomena, either healthy or morbid, in the action of the mind.

But here we must terminate our remarks, without noticing many points of interest, in this plain and, we may say, popular book of Dr. Carpenter's. The style is not easy nor simple, though clear. The work abounds, more than any other with which we are acquainted, in italicised words. But this can hardly be looked upon as a blemish, in a work so evidently destined for the non-professional as well as the professional reader.

Taken altogether we do not know, in the whole range of the literature of physiological psychology, of a more interesting and suggestive work. In an appendix, Dr. Carpenter gives a rather full account of Dr. Ferrier's experiments on the brain, and on the supposition that they are reliable, shows how they accord with the scheme of the nervous system set forth in his book.

We have endeavored in this notice to set forth, mostly in our own language, the general outlines of Dr. Carpenter's mode of regarding the structure and mode of action of the nervous system, rather than enter into detailed, especially adverse criticism, which the first and last chapters in the book more especially invite. But we heartily recommend all our readers to procure and carefully read this book.

TUKE: INFLUENCE OF THE MIND ON THE BODY.

Illustrations of the Influence of the Mind upon the Body in Health and Disease. Designed to elucidate the action of the Imagination. By Daniel Hack Tuke, M.D., M.R.C.P. 8vo. 410 pages. Philadelphia: Henry C. Lea, 1873. Chicago: W. B. Keen, Cooke & Co.

Dr. Tuke's work on the Influence of the Mind on the Body has been before the medical world for some time in its English edition, and we have now before us the first American reprint. We shall endeavor to briefly mention, in these pages, a few of the principal features of the book.

The title of the work, "Illustrations of the Influence of the Mind upon the Body," will afford, in some measure, a clew to its character, and will also show the difficulties in following closely and in detail all the lines of thought which are suggested. We shall, therefore, not attempt to more than notice a few of its more salient features, and convey the more general impressions which its perusal has made upon us.

The principal objects of the work, as given by the author in his preface, are, first, to collect together authentic illustrations of the influence which the mind exerts upon the body, and to show its power in producing and altering morbid phenomena, to ascertain the method and channels of this influence, and to arrange these instances on a definite physiological basis; and, second to elucidate the nature and action of what is generally understood as the imagination.

The first of these desiderata, as far as illustrating this influence and its power is concerned, is amply met; indeed, it could not well be otherwise. Although Dr. Tuke has evidently not had command of all the literature extant bearing on this special theme, he has fully illustrated his subject in all its bearings with well authenticated examples, supplementing the facts drawn from numerous sources by observations of his own. In this labor he shows, moreover, a scholarly acquaintance with classical and general literature. The frequently recurring quotations, so tempting to many writers on the subjects of psychological medicine are freely used, it may be not unduly so, but still noticeably as illustrating the various points of the author's subject. We would not say that a rather ornate literary style, embellished with quotations from the poets and the Greek and Latin classics is a blemish in a scientific medical treatise, but we prefer not to see too much labor bestowed in this direction. A criticsm of this kind in the present work is hardly a just one, but there is enough of a suggestion of it in certain parts to remind us of other works in which the feature we mention is particularly prominent.

One other point which impressed us in reading the book, and which we may perhaps be allowed to mention, is its arrangement. The author divides his work into four parts, in which he treats of the influence of the intellect, the emotions, the will, and the curative power of each and all of these in cases of disease. Each of these sections opens with a chapter of general principles, and in the first three the subsequent arrangement is also identical, the impression of a somewhat mechanical and monotonous treatment being given to the mind of the reader. This is a minor matter, not at all affecting the value of the book as a contribution to medical literature, and the arrangement is, perhaps, as good as could have been devised, but we wish, nevertheless, that the genius of the author had suggested a variation upon it.

In each of the three divisions treating respectively of the influence of the intellect, the emotions, and the volitions, their action upon sensation, the voluntary and involuntary muscles, and the organic functions are discussed in separate chapters or sections. Although, as Dr. Tuke states, the object is rather to furnish illustrations of these actions than to enter into the metaphysical questions that are suggested in their connection, he still devotes a considerable portion of his pages to the consideration of some of these points, at least, in their physiological applications. The subject could, indeed, hardly be handled without more or less attention to this side of the question, and in this the author shows the same extensive reading and careful study as is exhibited in the other portions of his work. He does not seem to have, or at least does not officiously obtrude the poor opinion of metaphysical studies that is held, more or less avowedly, by certain other workers in this and allied fields, and in this, we think, he occupies a better stand-point as a writer and investigator of psycho-physical phenomena. The greatest disadvantage under which he labors is a lack of acquaintance with German literature, which is evident, and as good as honestly avowed. In the note on page 308 he states, after quoting the opinions of Zeller, Domrich, and others, that he is indebted to the theologian Delitsch, whose work on Biblical Psychology has been translated into English.

In the division of his work which treats of the influence of the emotions, and which takes up fully one-half of the contents of the book, he rather favors the theory which would locate these feelings in the medulla instead of the hemispheres, or even the great ganglia of the sensorium, as was supposed by Carpenter.

The distinction, of course, between emotional and intellectual states and their respective manifestations, is difficult, in many cases, and the author frankly admits this, and states that under the head of emotions, employed in a wide sense, he includes compound states, comprising both intellectual and effective elements. In the main, however, his lines are as closely drawn as the inevitable circumstances will admit, and we cannot but testify that he has discriminated with much care in his illustrations and remarks.

Of course, automatic or unconscious reflex cerebral action plays a very important part in the results produced by the mind on the bodily functions and movements, and putting a broad construction on the term, we may, perhaps, thus account for each and every bodily manifestation produced through these influences. This is recognized by Dr. Tuke, and is referred to in many places, especially when he treats of the influence of the intellect, but it appears to us that it might be made more prominent in that part where he discusses the action of the emotions. A large part of this action might, it seems to us, be put among the reflexes. He follows Claude Bernard in admitting two kinds of vaso-motor nerves, the constrictors and the dilators, though he does not fully accept his theory as to the origin of these two kinds, the former from the sympathetic, and the latter from the cerebro-spinal system. On this he speculates in regard to the action of the emotions on the organic functions through the vaso-motor nerves, but we cannot follow him at length in a notice like the present.

The influence of the will is treated more briefly than that of the emotions or the intellect, only about twenty pages being devoted to it altogether. Within this space, however, the subject appears to be well handled, and a number of interesting instances are collected of the influence of the volition over the sensations, organic functions, etc.

The fourth part of Dr. Tuke's work, that which treats of the influence of the mind on the body in the cure of disease, is of general interest. There is probably no practitioner who has not had under observation instances of this influence, and, if all cases could be properly interpreted, the record would be very largely increased. A just appreciation of this fact, combined with a moderate degree of intelligent skepticism, throughout the medical profession, would, we think, go far to give us more correct ideas as to the value of many drugs, and would certainly aid to raise the tone of the current medical literature of the day. We need say nothing of the non-medical public; while their simple faith sometimes comes to the help of the educated and conscientious physician, it still more often supports quackery and humbug in some one of the various forms in which it manifests itself. The subject is, therefore, worthy of all the attention which it receives from our author. Although he states that the illustrations may seem to some as of no use, they are, as he says, of great importance, especially in regard to the question whether the psychical cures by mesmerism, etc., are due to a force proceeding from one individual to another, or to the simple excitation of a particular mental state. The phenomena of mesmerism and hypnotism are reviewed in this connection, and their advantages in psycho-therapeutics discussed and emphasized. The author's conclusion that the "effects accidentally produced upon the body by mental impressions in disease, can be imitated, and the arts employed by the empiric can be divested of the non-essentials, and systematically utilized," is worthy of the consideration of all who wish to aid suffering humanity, and he states that, in his opinion, the method which offers the most advantages for this purpose is that of Braidism or hypnotism. At present this forms no part whatever of the recognized therapeutic armament against disease.

The book before us deserves a fuller review than we have here given it. In this short notice, which is all that time and space allows us, we have spoken freely of a few points which have impressed us in its perusal. It is not a pretentious work, although its scope is so great; the only reason that it at all disappoints expectation is from the suggestiveness of its topic, which would make almost any treatment of it whatever seem incomplete. Within the limits which he has assigned himself, Dr. Tuke has handled his subject well, and, we think, has rendered a service to the profession and the public by the production of this work.

IV.—McCLELLAND: CIVIL MALPRACTICE.

Civil Malpractice. *A Report Presented to the Military Tract Medical Society, Jan.* 14, 1873. By M. A. McClelland, M.D. Published by W. B. Keen, Cooke & Co., Chicago.

This thin octavo consists of a statement of the opinions and decisions of courts and of surgeons on the subject indicated in its title. The first few pages are occupied in stating the legal obligations of the surgeon, as given by the courts, the substance of which is, that the surgeon is bound to possess a reasonable ordinary amount of skill and knowledge, such as is usual among the surgeons of the region where he practices, and to give the patient reasonably diligent attention; that is, such an amount of care and attention as is ordinarily given to such cases by other surgeons. The legal obligation does not call for extraordinary skill nor care. If the surgeon does not possess such knowledge and skill, or, if he neglects to use them for the benefit of the patient, he is liable for whatever damage results from such inferiority of his skill or care.

On the other hand, the courts hold that the surgeon "is not a warranter of a cure, unless he makes a special contract to that effect. If he is shown to possess the qualifications stated" (ordinary knowledge and skill), "to authorize and justify him in offering his services as a physician, then, if he exercises his best skill and judgment, with care and careful observation of the case, he is not responsible for an honest mistake as to the nature of the disease, or as to the best mode of treatment, when there was reasonable ground for doubt or uncertainty."

The courts also hold that if there are different sects or schools of physicians, such as the Botanic, the Homœopathic, etc., the practitioner is bound to treat the case according to the usages of the sect to which he professes to belong, and is not responsible for neglecting to use those resources which are not customary in his class.

As to the question of fees, in the appeal of the case, Lowe *vs.* McNevins, 40 Ill., 209, Mr. Justice Lawrence delivered the opinion of the court, that the responsibility of the physician or surgeon was the same, whether he received a fee or not; but that a person acting only as a friend, and not professionally, was not liable to malpractice suits.

It would be well to add, here, that some of the courts have held that, at least, in cases of emergency, a lawyer cannot abandon his client, nor a physician his patient, merely on account of a failure to pay. Probably this ruling applies to urgent cases only, where an obvious inhumanity would be committed by suddenly abandoning the patient or client. It would seem absurd in slow chronic cases.

The author of the book before us indulges in a very injudicious remark respecting the use of bandages next to the skin, in fractures. He says (p. 60), that by the use of such a bandage, "the danger of so ligating the limb as to occasion congestion, inflammation, and gangrene, is so great, that the surgeon would hardly be held 'not guilty' who should use it." This is a very sweeping assertion, and calculated to do mischief if quoted in court. True, the initial bandage is of much less use than was formerly supposed, and, for myself, I rarely apply it; yet it is recommended and enjoined by many of the best authors, and it is entirely wrong, therefore, to intimate that a man may properly be convicted of malpractice for following out the directions of our standard text books. There are eminent European surgeons who direct this bandage, even in the fore-arm, and, for obeying this direction from established authority, one surgeon in Illinois was prosecuted. The initial bandage is legally good practice, even on the fore-arm, because the books that recommend it, and the eminent men that use it, are still on the stage. It is one of those points where authorities differ, and, therefore, according to the courts, the surgeon may follow his own judgment.

Surgically speaking, the truth is this: The bandage next to the skin is generally useless, and, in the hands of a careless surgeon, is dangerous; but, at the same time, in the practice of a careful man, who does not apply it too tightly, and who loosens it promptly, as soon as swelling requires, it is innocent. Whether it does any harm or not depends entirely on whether it is too tight. No man should, in this generation, testify that its use is bad practice, unless the excessive tightness is proved.

On the whole, this little book is a very useful one, and will be found worthy of a place in every surgeon's library. E. A.

V.—ROCKWELL AND BEARD: ELECTRO-SURGERY.

Clinical Researches in Electro-Surgery. By A. D. Rockwell, A.M., M.D., and Geo. M. Beard, A.M., M.D. 72 pages, 8vo. New York: 1873, Wm. Wood & Co.

In the first part of this little book, the authors, already so favorably known by their larger joint work on medical electricity, give an account of various cases of the electrical treatment of tumors, malignant or otherwise, from the notes of their own practice. The cases are related with great candor, and the results stated without favor or prejudice, the failures being as frankly stated as the successes.

In the second chapter, the results of the electrical treatment of various forms of cutaneous disease are given in like manner. This part of the work comes more within the special province of this journal, and we shall therefore notice it with a little more detail. In all, five cases of eczema, three of acne, and several others of prurigo, lichen, pityriasis, etc., were treated with, in many of the cases, quite astonishing success. The method used in most of these cases was that of central galvanization, as practiced by the authors, and described in the *N. Y. Med. Journal*, Oct., 1872. Local applications were also employed in a few instances, but the main dependence was upon the more general method. The results of this method are suggestive, in reference to the pathology of some of these forms of skin disease, eczema, for example, which has not been invariably considered to be of nervous origin. The authors call attention to the following points, as illustrated by these cases:

1. "The rapid relief of the itching and pain of eczema, prurigo, and herpes, by local applications.

2. "The relieving and curative effects of *central galvanization*, not only in prurigo, but also and especially in eczema, which has not generally been supposed to be so closely dependent on the nervous system.

3. "The fact that herpes, prurigo, and eczema, yield in some instances very rapidly, while psoriasis and pityriasis are quite slow and obstinate.

4. "The tendency of some cases to relapse even after they have been greatly benefited, while in other cases—notably in eczema and herpes—the cure is more or less permanent."

These points are certainly worthy of the attention of those who have to treat the various skin affections, especially those in which any trace of a neurotic character is to be suspected. Skin diseases, at least some forms of them, have been so long among the *opprobria* of medicine, that every addition to our means of combating them is of the highest interest.

VI.—ARTHIUS: STATIC ELECTRICITY.

TREATMENT OF NERVOUS AND RHEUMATIC AFFECTIONS BY STATIC ELECTRICITY. By Dr. A. Arthius. Translated from the French by J. H. Etheridge, M.D. 144 pages, 8vo. Chicago: 1874. W. B. Keen, Cooke & Co.

We have already noticed this work in the original, in the April number of this journal, and have there said all that seemed needful in regard to its value, etc., to the medical public. The substance of the work seems to be faithfully given by the American translator; we notice, however, the misspelling of the author's name—Arthius, instead of Arthuis, as it should be. This, however, under the circumstances, is an unimportant matter. The publishers have brought out the work in good style, and it presents quite a tasteful appearance.

Editorial Department.

THE MEETING OF THE AMERICAN MEDICAL ASSOCIATION.

THE Annual Meeting of the American Medical Association was held at Detroit, from June 2d to June 5th, 1874.

On many accounts it was one of the most pleasant meetings in its history. It was probably not as numerously attended by leading Eastern members as other meetings have been, and the absence of members from some localities in New England and the Eastern States was not less conpicuous than in former years.

In this connection it may not be out of place to notice a late editorial article in the *Philadelphia Medical Times.* We hope it does not speak the sentiment of any large class of the profession in the East, as its tone would lead us to suspect it does.

It says: "We do not, in any way, wish to disparage our Western brethren, but it is a simple fact, that by far the largest portion of the leading minds of the profession are to be found in our Eastern cities. The most influential periodicals, with a few exceptions, are there issued, the American medical works almost all have such nativity; and the chief medical schools of the country are there situated, and the facilities for higher medical self-education, for study and investigation, do there most abound."

Does the *Times* know why this is? Is it not true that across the Atlantic, in all the respects mentioned, the profession is in a more advanced state than even our brethren in the East? When the profession in the West has had the same time and opportunities as in the East, it will be soon enough to compare them as the *Times* does. Was it the fault of the Western men that Eastern members of the profession did not attend at Detroit?

In common with all, to say the least, candid members of the profession in the West, we may admit all the *Times* says as to the comparative superiority of the profession on the seaboard, for reasons that are easily understood, and that do not require, in making the admission, a sacrifice of self-respect.

The only thing of which we would be inclined to complain is, not that we are so far behind our Eastern brethren, but that the *Times* should have felt it necessary to tell us of it, and that having, as we are left to suppose it has, the open ear of the profession abroad, it should have felt it necessary to relieve its sense of humiliation at the bare thought of being held as accessory to so crude a performance as was enacted at Detroit, by saying "for the sake of our foreign contemporaries, we want to deny emphatically, that the convocation was in any true sense representative of the American profession."

Though we are not a member of the Association, and did not participate in the meeting at Detroit, yet we feel that the best way to reform the Association, or to improve the tone and culture of the profession in the West, is not by making such unfair comparisons, and in spite of the disclaimer, such disparaging implications as are certainly contained in the article in the *Times.*

The address of the President, Dr. J. M. Toner, of Washington, was eminently sensible and practical. Whether the establishment of a physiological laboratory, such as Dr. Toner referred to in his address, is desirable or not, there can be no question that better facilities for scientific physiological research are urgently required, and we hope soon to see, even in the *West*, some more decided steps in this direction than have yet been taken in the United States.

The action of the Association on the "alcohol question," was, to say the least, timely, and we hope may have the effect of arousing the attention of some to the important practical questions involved in the *use*, as well as abuse, of alcoholic drinks.

We notice Dr. Bartholow came in for a share of condemnation, for his experiments on the brain of the patient under his care in Cincinnati, with the details of which our readers are sufficiently familiar. Is it not about time to let this matter drop?

We have regretted to see so much of a disposition—both here and abroad—to condemn, not simply the method, but the *man*, especially after the candid and manly statements about it that he has made from the beginning.

In spite of the feeling that had been rather freely expressed by some, that the Association had outlived its usefulnes, and that its existence should be terminated at no distant day—at least, in its present form—we were gratified to find that the sentiment in favor of its continuance was strong and unmistakable. We earnestly hope the Association will be continued, and its plan of organization and mode of working be so modified, if need be, as to make it more effective as a *scientific* rather than a *social* body, and more useful as a means for summing up the sentiment and power of the whole profession in effecting reforms, or for concentrating the influence of the profession upon questions of public and professional importance. To this end we have no plans to offer, but look hopefully to the efforts in this direction of those who have consecrated time and talent to the interests of the Association, which we hope will speedily take rank in activity and fruitfulness with similiar associations abroad.

THE CARE OF THE INSANE.

WITH all that has been done in the last fifty years toward a humane and considerate treatment of the insane, especially as regards their hygienic and moral management, it is evident, on many accounts, that much remains yet to be done. There are special reasons why not even the helplessness of infancy and old age appeals more strongly for sympathy than the insane. And there are special reasons easy to be understood, which seem at times to justify, if not demand, a form of violence in their management which may easily pass into cruelty, even in judicious hands. There is yet ample scope, much as has been done, for the exercise of the most considerate and watchful benevolence, in securing for this most unfortunate class of our fellow beings that unfailing kindness of treatment their peculiar condition entitles them to receive. We are led into this strain of remark by noting a movement, said to be on foot in Boston, to introduce into the Massachusetts Legislature, a bill for securing more perfect protection to the inmates of lunatic asylums, similar to the one passed in Iowa two or three years ago.

"The bill provides for the association of two women with the Board of State Charities, forming a commission whose duty it shall be to visit all asylums for the insane at least once a month, unexpectedly, if possible, and to make inspection unattended by the officers of the institution. Any three members of this commission are to have the power to investigate a charge of abuse, sending for persons and papers, examining witnesses, and taking the testimony of patients; and the authority is given the full Board to dismiss any officer on sufficient cause, while fine and imprisonment are fixed as the penalty on conviction. Another important feature of the bill is, that it takes the supervision of the correspondence of patients out of the hands of the officers, and places it in the control of the Board. Locked letter-boxes are to be provided, to which the members of the Board alone have access."

So far as such a movement might be regarded as a reflection on the general character of the responsible officers of institutions for the insane, we would deem it wholly unjust. But in so far as it may be an indication of the recognition of the necessity of a closer inspection of such institutions, we believe the movement is in the right direction.

We are also led to these remarks by what seems to us a most flagrant instance of bad management in one of our territories (Washington Terr.). The present law in that territory, which is highly defective, authorizes the Governor and territorial auditor to let out the keeping and clothing of the insane to the lowest bidder, who is generally apt to be some middle man, or small politician, who secures the contract for party services.

In the present case, not only has the keeping and clothing of the insane patients been committed into the hands of the "lowest bidder," but, at the instance of this same person, a really competent physician, of some years' service among the insane of the territory, has been summarily dismissed, and the selection of a *physician* has been left virtually in the hands of the "lowest bidder!" And this is "The Hospital for the Insane" at Steilacoom, Washington Territory! In this day of enlightenment, we consider such provision for the insane but little better than an outrage. And yet the system prevalent in many of our county alms-houses, affords no greater guarantee for really scientific and careful treatment than does the shameful system in Washington Territory.

Homœopathic Psychiatry.—We extract the following from the newspaper report of the proceedings of the State Homœopathic Society in this city, May 20th, and present it for the appreciation of our readers. Its author, we understand, was straightway made Chairman of the Committee on Psychology, ot that body:

"Dr. Fairbanks, of Ottawa, Ill., read the following essay on 'Psychological Therapeutics:'

"It is not only in the conditions of insanity, dementia, monomania, and kindred maladies of grave importance, but in a multitude of minor psychological aberrations, that the beneficence of the mild 'Similia' earns the lifelong gratitude of suffering humanity. 'What can minister to a mind diseased?' has been echoed from the poet's lips, down the long line of 'eminent medical authorities,' whose best reply has been a wise shake of the head, to at last find an affirmative response from the disciples of Hahnemann. He has taught us to think of aconite, when there is fear of impending death; or of anacardium for a profane disregard of such serious matters. Of aurum for a desire to commit suicide, or of hyoscyamus when the desire exists to kill another. He has demonstrated at the bedside, that the pangs of secret grief can be assuaged by ignatia, or that stupid apathy and indifference may be brightened by phosphoric acid. Excited imaginations are quieted by coffee, arrogance and pride are calmed by platinum. Who has not repeatedly observed the boisterous levity of arnica, the silly titter and giggle of nux moschata, or the lachrymose smiles of pulsatilla? Even the fiercest passions may be subdued by these wonderful remedies. Many cases of religious despair can be relieved by lycopodium, or of religious monomania by stramonium. The flood of profanity can be checked by anacardium, and the drunken passions quelled by nux vomica or cimicifuga. The happiness of many households can be promoted by silencing the irritable and contradicting child with nux or chamomilla; or removing the fear of darkness by china or stramonium. The bashful face of the self-enervated son may be made to appear more manly by phosphorus, agnus, gelseminum, and their analogues, while a legion of remedies stand ready to aid the irritable, chlorotic girl to attain to a more perfect womanhood. Fury ceases to 'rave,' at belladonna, or hyoscyamus; jealousy is satisfied with lachesis; rancor softens its tone in the presence of nitric acid.

"Yet there are those who, with the haughtiness of platinum, declare their freedom from such symptomatic trifles; who will first know the pathological conditions, and then the malady will be treated in a truly scientific manner. But, after delving with an industry worthy of digitalis, and summoning to their aid all the philosophy that sulphur can command, they at length discover, with the dissatisfaction of crotalus, that they have only arrived at the original starting-point on the therapeutical ladder, showing that the key-notes oftentimes serve as a short-cut across a long pathological bend.

"Psychological Key-notes.—The following are some of the most important mental symptoms, from the Materia Medica Pura and other reliable sources:

"Aconite: Anxious fear of approaching death; taciturn; sad, at times malicious, or in a fitful mood; fear of being in a crowd. (See argument.) Aconite is said to have produced, in some persons, an opposite condition to ignatia, 'unable to weep, or feel any manifestations of grief, under the most aggravating circumstances.'

"Agnus castus: Fear of death, after a while, but thinks it useless to attempt to avoid it. (See caladium.)

"Alumina: Alternating from grave to gay; weak memory.

"Anacardium: Malicious, cruel, profane, imbecile; great weakness of memory; useful in insanity following self-abuse, and hypochondriasis.

"Argentum nitr.: In a great hurry; minutes seem as hours. (Has cured an anxious smothering sensation when in a crowded room. F.)

"Arsen. alb.: Anxious restlessness; fears death, but desponds of a cure—as in cholera.

"Aurum met.: Deep despair; desire to commit suicide; seems to have no friends.

"Baryta, carb.: Aversion to strangers—in dwarfish children.

"Belladonna: Fantastic illusions, quickly changing; nervous anxiety, restless, desires to escape; madness; talks fast; sees horrible things; runs away.

"Borax: Easily startled; fear of a downward motion. (See bella. and gels.)

"Bryonia: Irritable; wants to be let alone; fear of the future; fear of death, which he thinks is near."

We regret that the reporter failed to give us the rest of the alphabet of "psychological key-notes," which have remained hid from all the eminent medical authorities, to be at last revealed to the disciples of Hahnemann. We never before fully appreciated the virtues of the "mild Similia," but we see from this how far its claims exceed those of our own more modest pharmacopœia,—that it assumes to have control over our moral, as well as our physical ailments. We are surprised that the society did not at once resolve itself into a committee of the whole, on ethics, and advise the establishment of dispensaries of platinum, anacardium, nux vomica, hyoscyamus, and nitric acid. We can imagine the disciple of Hahnemann administering lycopodium, and exhorting his anxious hearers to "think of aconite" and be comforted. The suggestion occurs to us that the cosmic diffusion of the higher dilutions, up to the *n*th, of these "wonderful remedies," may, perhaps, account for all moral depravity and diabolism. But we leave such interesting reflections on this suggestive extract to the reader.

The greatest matter of surprise, however, is that the more intelligent members of this homœopathic society did not require the repressing influences attributed to anacardium or hyoscyamus, or exhibit at least, in the strongest manner, the symptoms here given of crotalus, at this naive exposure of their peculiar doctrines. We hope, for the sake of their patients, that a few of them will still continue, "with the haughtiness of platinum," to pay a little attention to pathology, although, in so doing, they are forced to practically abandon every absurd exclusive dogma of their school.

THE NEW YORK SOCIETY OF NEUROLOGY AND ELECTROLOGY. The following is the list of officers of the New York Society of Neurology and Electrology, which was formally incorporated in January last:

President, Meredith Clymer, M.D.; *Vice President*, Austin Flint, Jr., M.D.; *Recording Secretary and Treasurer*, Alfred L. Carroll, M.D.; *Corresponding Secretary*, John J. Mason, M.D.; *Executive Council*, Austin Flint, Jr., M.D., John C.

Dalton, M.D., D. B. St. John Roosa, M.D., E. G. Loring, Jr., M.D., Geo. M. Beard, M.D. The President and Corresponding Secretary are members *ex officio.*

The Society was organized before the Neurological Society, the roster of which was given in our last number. As will be seen from the above, it includes among its members some of the ablest men in New York and the United States.

The Archives of Electrology and Neurology.—We have received the first number of this new semi-annual, edited by Dr. Beard, of New York, the prospectus of which appears among our advertisements. In all respects it seems to fulfill the promise of its founder. The original articles, in the present issue, number sixteen, and include papers by Dr. Beard, Prof. Bartholow, of Cincinnati, Prof. Cabell, of Virginia, Benedikt, of Vienna, Tripier, and others. These, with book notices, extracts, miscellaneous notes, and editorial matter, make up a handsome volume of one hundred and forty-three pages. We wish this journal long life and full success.

New Journals.—We have received the prospectus of the *Archives of Dermatology*, a new quarterly journal which it is proposed to issue under the auspices of the New York Dermatological Society, and under the editorial charge of Dr. L. D. Bulkley. The first number is announced to appear on the first of October next. Each number will consist of ninety-six octavo pages, and is intended to contain a compendium of progress in Dermatology and Venereal Diseases. Subscription, $3.00 a year; single numbers, $1.00.

We have also received the numbers, as far as issued, of the *Virginia Medical Monthly* and the *American Medical Weekly*, two new Southern medical publications, the former published at Richmond, by Dr. Landon B. Edwards, and the latter at Louisville, by Dr. E. S. Gaillard. We welcome them both as valuable additions to our periodical medical literature.

ANNOUNCEMENT.—We have received the prospectus of a work shortly to be issued by Prof. Joseph Jones, of the University of Louisiana, under the title, "*Medical and Surgical Memoirs*," and containing "investigations on the nature and treatment of various diseases during a period of twenty years." The work is to consist of three volumes, octavo, the price of each volume being five dollars.

Dr. Jones has been well known to the profession in the United States, more particularly in the South, for many years, as the author of many elaborate and exhaustive papers on various medical topics, which bear the marks of uncommon industry and ability, and there is every guarantee, therefore, that his work will not only be completed as promised, but also that it will be a solid contribution to the wide range of subjects of which it treats. Most heartily would we venture to commend in advance this work of Prof. Jones.

THE PSYCHOLOGICAL AND MEDICO-LEGAL JOURNAL.—The first number of this new monthly journal is received as we go to press. It presents a very striking and handsome appearance, and contains Dr. Hammond's inaugural on alcohol, before the Neurological Society, with the discussion which followed; a translation from the German, by Dr. D. F. Lincoln, and a number of notices and reviews. This journal will not contain an editorial department, or abstracts from home and foreign journals, but will be devoted mainly to original articles, reviews and proceedings of societies; its object being to increase and diffuse knowledge, more especially the former. It has our best wishes for its success.

Periscope.

a.—ANATOMY AND PHYSIOLOGY OF THE NERVOUS SYSTEM.

FUNCTIONS OF THE BRAIN.—Dr. H. Nothnagel (*Virchow's Archiv*, 1874, 129,) continues the publication of his investigations on the functions of the brain. First he gives a more detailed description of the results of the extirpation of both nuclei lenticulares, a short notice of which had already appeared in the *Centralblatt f. d. Med. Wissensch.*, and has also been mentioned in the Periscope of our April number. As stated, the animal (rabbit), after the operation, remained motionless, the ears erect, and respiration and cardiac action perfectly normal. If the limbs were moved artificially they remained exactly as they were placed, but, if the tail was pinched, even so slightly that an uninjured rabbit would scarcely notice it at all, the animal seems to come again to life; it withdraws its members from their unnatural positions and it appears as if about to spring forward, but immediately retakes again its listless and sleepy appearance. Sudden noises made behind the animal and a strong light brought before the eyes caused slight reflex movements of drawing its limbs together, contraction of the pupils and closure of the lids, but nothing more. In one instance the rabbit stroked its nose with its fore paw, but as this had also been observed by Schiff after removal of the hemisphere, it cannot be considered as a voluntary act. The reflex excitability was increased, as in the case of the removal of the hemispheres.

When the point in the hemispheres, designated by Nothnagel as the *nodus cursorius*, was irritated after the destruction of the nuclei lenticulares, the animal commenced to run from one-half to two minutes after the irritation, just as when the nuclei lenticulares were intact, but with this difference, that when any obstacle whatever was met with the limbs did not continue to move, as in the other case, but stopped their motion at once, and the animal required some new excitation (pinching the tail, etc.), to make it start anew The limbs seem to be in a state resembling the cataleptic condition, and in one instance Dr. Nothnagel placed one fore paw on the neck; then the tail being pinched, the animal started off on three feet, leaving the other still as it was placed over the neck.

The extirpation of both nuclei caudati was followed by the following phenomena: In from one-quarter of a minute to two minutes the rabbit started to course violently around the room, turning to the right or left, avoiding obstacles. The animal is, in this condition, exceedingly sensitive to sight or hearing, but hardly so to touch, which frequently fails entirely to cause any excitation. In two cases the phenomena varied from this form; in one, the rabbit only ran for an instant when the injury was first made, and in the other, in spite of the fact that the greater part of the corpus striatum was destroyed, the animal showed no morbid appearance whatever. No such deviation of the limbs as happened when only one side was operated on, was observed after the double lesion, nor were there any disturbances of sensibility.

The simultaneous extirpation of both nuclei caudati and both nuclei lenticulares produced no special phenomena other than were observed after the destruction of the latter alone, except, perhaps, that the limbs were even more passive in abnormal positions than before. Experiments were made to test the effect of simultaneous injury to the corpora striata and optic thalami, but owing to the difficulties attending the operation the results were not conclusive. Several times, however, after successful extirpation of both nuclei lenticulares the destruction of the thalamus on one side was attained, with the result that not only the passivity but also the capacity for motion under excitation remained unaltered, excepting only that the spring was made to one side instead of directly forward, as before.

In the summing up Dr. Nothnagel gives the two following general conclusions:

1. That in the nucleus lenticularis are united the general routes of the nervous impulse which convey the voluntary motor (psychomotor) impulse from the cortical substance of the hemispheres to the more outward situated portions of the nervous system, and not merely those of the trunk and extremities, but also the voluntary motor cranial nerves.

2. In regard to the nucleus caudatus the author holds the view that it stands in relation to all those combined movements which receive their primary impulse through a psychic process, but then continue, as it were, automatically, without any new incitation from the will. Such movements are those of the act of laughing, or even of running. The author admits that this view is as yet purely hypothetical, but claims that it is in accordance with the known experimental facts, and for the present we have no other data to prove its correctness or its error.

Dr. Eugene Dupuy read a paper before the N. Y. Society of Neurology and Electrology, last May, in which he reviewed the experiments of Ferrier and Fritsch and Hitzig, and illustrated his remarks by demonstrations on living animals. His conclusions, as reported to us by our New York correspondent, were as follows:

1. That it is possible, by exciting certain points of the cortical substance of the brain to obtain contractions in every limb.

2. That as a rule the anterior limb of the opposite side is the one affected.

3. That the electric current must be propagated to the base of the cerebrum to excite either the nerves arising from it or the base itself, or the pons varolii.

4. That if the dura mater be electrically excited, contractions are obtained in the fore leg, and generally in that of the opposite side.

5. The fact that the galvanoscopic frog is thrown into a state of contrac-

tion when its nerve touched some part of the cerebral mass far from the point excited, confirms the view that the electrical current is propagated.

6. In opposition to the results claimed by Ferrier, Dr. Dupuy has never been able to obtain effects on the tongue, either of projection or retraction.

7. The whole cortical layer of the cerebrum is probably a centre of reflexion for a certain kind of sensibility capable of exciting a reflex action upon motor or sensory nerves, but its preservation is not indispensable for the manifestation of voluntary or even intelligent action.

8. In the animals on which Dr. Dupuy had experimented, contractions of the opposite limbs could still be produced after the ablation of the optic thalami and corpora striata of the side opposed to that on which the irritation was applied.

Dr. Austin Flint, Jr., in a few remarks stated that he had not yet studied the subject as much as he wished, but two points were perfectly clear to him, viz.: 1st, That the currents were certainly diffused; and 2d, That in one of his last experiments, stimulation of the cortical substance of the brain, producing movements of muscles after the facial nerves had been divided, showed the diffusion of the irritation beyond the point of section of the nerves. He was free to say that the current used by Dr. Dupuy, though feeble, was much more powerful than he employed in making experiments upon the nervous system. He found, a number of years ago, when making experiments on dogs, and galvanizing certain parts of the deep substance, as well as of the surface, that a feeble current of electricity was sufficient to produce movements characteristic of those following stimulation of motor portions of nerve centres. Undoubtedly, the experiments of Ferrier and Fritsch and Hitzig, that seem at first somewhat definite, are very much in error. It is very striking, as we can all see, to observe these localized movements following stimulation of certain portions of the brain; but there can be no doubt that, if the observations of Dupuy are correct, the portions thus directly stimulated were not those which gave rise to the movements; that if the galvanic stimulus were rigidly confined to the parts touched by the poles of the battery, we would have no movements; but that the stimulus would be propagated we have for a long time known. The doctor concluded by endorsing in the main the conclusions of Dr. Dupuy.

In our last issue we gave an extract from a Cincinnati paper, containing an account of Dr. Bartholow's experiments on the living human brain. A fuller account appeared, as was announced, in *Hay's Journal* for April, and a review of the case by Dr. Bartholow is contained in the *Archives of Electrology and Neurology* for May. Inasmuch as the main points are as stated in the extract published in our last number, we will here merely give the general conclusions of Dr. Bartholow as stated in the paper in the *Archives*.

"Summary of results obtained by mechanical and electrical stimulation of the right and left parietal lobules.

"These parts of the human brain have to do both with motion and sensation.

"As regards motion, they are chiefly associated with the extensor muscles. (Compare Ferrier's observations on electric excitation of the tubercula quadrigemina.)

"An irritative lesion, mechanical, of these parts, will cause crossed unilateral choreic movements.

"Electrical excitation causes crossed muscular movements in the extensor muscles. Stronger electrical excitation gives rise to unilateral epileptiform convulsions on the opposite side.

"Lesions, merely irritative, mechanical or electrical, give rise to pain on the opposite side of the body, more especially in the hands and forearms."

Dr. Geo. M. Beard, *Archives of Electr. and Neurology*, May, gives an account of experimental researches conducted by himself, with the assistance of Dr. Geo. B. Fowler, with the object of ascertaining how far the results claimed by Hitzig and Ferrier were demonstrable, and to test the validity of the objections raised against them, as well as to carry on the inquiry still farther than had been done by these observers. The animals experimented upon were dogs, cats, rabbits, and pigeons. The method of experimenting is fully explained—both currents were employed. The conclusions reached were as follows:

1. The living brain responds to electrical irritation, faradic and galvanic, by various and more or less defined muscular movements.

2. The brain does not respond to electrical irritation everywhere to ordinary currents equally; some portions do not respond at all, and others respond with reactions that uniformly differ from each other.

The objections of Dupuy and Carville are met by the following statements: First, that the quantity of electricity that is diffused is but a very small fraction of that which appears between the electrodes. The currents so diffused are very feeble, and the special reactions of the brain are produced by currents of considerable strength, directed on certain circumscribed parts, and do not follow when they are applied to other regions from which diffusion might take place just as readily. Secondly, the same reaction may take place from localized injury to the brain, as from the irritation of electrical currents. After referring to Dr. Bartholow's case, Dr. Beard gives an instance which came under his own observation, in which simple pressure on the exposed brain caused unilateral movements of the lower extremities.

3. The centres for the muscles of the mouth, face, neck, and legs, appear to be in the anterior and middle portion of the brain. Superficial irritation of the different parts of the cerebellum causes nystagmus; this is easily shown. Irritation of the tubercula quadrigemina causes opisthotonos, as is easily shown. Irritation of the hippocampi causes no visible reaction. If the brain is sliced off, reaction can be produced by irritation of the portion exposed.

4. The brain, in respect to those centres, is symmetrical, and is indeed a double organ.

5. There is in the brain of the rabbit, in the posterior part of the middle third portion, about a quarter of an inch from the median line, a spot in each hemisphere that seems to be a centre for jumping and leaping movements.

This reaction seems to be the same as that obtained by Nothnagel, but its discovery by Dr. Beard was independent of any knowledge of the former's experiment. Dr. Beard considers that the fact that this reaction is only obtained by the exact localization of the current in a very limited portion of the brain, as a strong point against the theory of diffusion.

Experiments on pigeons produced distinct results in movements of the head, eyes, wings, etc.

The strength of the irritation varied with the animal experimented upon, and the length of time the brain had been exposed. As regards the differential action of the currents and the poles, either current seemed to answer equally well, and the anode seemed more effective than the cathode, but the author is not positive. Other methods of irritation, ice, red-hot needles, were tried, but no reaction followed. From the fact that pressure on the exposed human brain caused movements, Dr. Beard thinks that if the brain were very little exposed and no anæsthetic used these methods might also produce effects.

Dr. Beard proposes the following theory, to which he has been led by these experiments: "That these irritable points are complex in their functions, and either from the surface or deeper portions of the brain send fibres to both sides of the body."

In the first experiment, when the electrode was plunged a little distance into the brain, the movement appeared on the same side of the body. In other cases both sides appeared to be affected, either simultaneously or in quick succession. There seemed to be a difference in the effects of superficial or deep irritation, and Dr. Beard thinks that as Ferrier confined his experiments to superficial irritation, he did not observe the complexity of these centres. He suggests that a majority of the cells in each hemisphere preside over the opposite half of the body, and a small minority over the same side. These latter are affected only by exceptional forms of injury. By this theory the different parts of the brain can, to a certain extent, act vicariously for each other; one hemisphere may do the work of both, and a large loss of cerebral substance may sometimes be not seriously felt.

Dr. Beard noticed frequently that when the brain had become disorganized by long irritation, and particularly when it was decomposed by the galvanic current, the reactions became confused and the centres of movement were difficult to localize.

CONGENITAL ABSENCE OF THE CORPUS CALLOSUM, SEPTUM LUCIDUM, AND GYRUS FORNICATUS, WITH INTEGRITY OF THE INTELLECTUAL FUNCTIONS.—(*Lo Sperimentale*, page 579, Tomo 33, Fas. 5, 1874.) Prof. Malinverni Germano, Prof. of Path. Anat. of the University of Turin, describes a case of a man in which there was found, after death, a *complete want of the corpus callosum, of the septum lucidum, and of the gyrus fornicatus, or convolution of the corpus callosum, without noticeable impairment of the mental faculties, or disorder of sensibility or mobility.* He was a countryman, had always enjoyed good health, had passed as usual his term of military service, and finally died, aged forty years, with a form of gastro-enteric disease at the great hospital of Turin. In other respects the brain was in a normal condition. The case excited unusual interest, and was examined and described with great care. Says Prof. Germano, in his memoir: "In physiology it is commonly understood that the corpus callosum serves to connect, to co-ordinate, to harmonize, to unify or sum up the operations of the two cerebral hemispheres, to serve as the anatomical medium of their actions. But if the action of the cerebral hemispheres, as was observed in this case, may be regularly carried on, apparently in co-ordination, in spite of the absence of the corpus callosum, what other part of the cerebrum has in this case supplied the place of the great cerebral commissure?" Prof. Romano regards this as an instance of arrest of development at probably the fourth month of intra-uterine life, and looks upon the brain in the case he describes as to be compared with that of oviparous vertebrates, in the brains of which animals the corpus callosum would appear to be normally wanting. The case is one of very considerable interest.

THE VASO-MOTOR NERVOUS CENTRES.—At the session of the *Academie des Sciences*, Febr. 16, M. Vulpian offered the following communication, as reported in the *Gaz. Med. de Paris*, March 7:

"We have determined, by means of very delicate vivisections, the regions of the spinal cord from which emerge the vaso-motor fibres destined to the different parts, the members, head, etc. That is to say, the *apparent* origin of these fibres, the *real* origin of the vaso-motor nerves must be sought in the gray substance of the cord, in the vicinity of the point where they have their apparent origin.

A great number of physiologists, especially in Germany, claim that the vaso-motor nerves do not, in reality, originate in the spinal cord. All the nerves have their true origin in the medulla oblongata. Their fibres, consequently, are obliged to follow a longer or shorter track in the cord, according to the level at which they emerge from this part of the nervous centres in order to enter into the various parts of the body. The medulla, is therefore, the single point of origin for all the vaso-motor nerves (with the exception of those of the abdominal viscera, according to M. Schiff); it is the centre for all the reflex vaso-motor actions, the point of departure for that permanent excitation which produces the vascular *tonus*. This is, therefore, the *vaso-motor centre*, as we call it, and although its existence is somewhat doubted its intervention is constantly called in the physiological theories of physicians and experimenters.

I have made a great number of experiments relative to this question. The following appear to me the most significant:

If all the vaso-motor nerves depart from a single centre, situated in the medulla, a transverse section of the cord, made at the level of the superior cervical region, ought to completely paralyze all the vessels in all parts of the body; and no other lesion, either in the dorsal region of the cord or in the vaso-motors themselves, ought to augment that paralysis.

But if we cut the cord transversely at the level of the second cervical vertebra, in a mammal, under the influence of curare, and kept alive by artificial respiration, and if we note the temperature of the posterior members subsequent to the operation, we can see, if we make on the same animal a hemisection of the cord in the middle dorsal region, the temperature rises yet more in the posterior members, and particularly so, in general, in the limb of the corresponding side. Operating in the same way on frogs which have not been curarized, we can make out directly that the vessels of the interdigital membrane on the side of the section of the cord are more dilated than those of the other posterior limb.

I ought to state that this experiment does not give absolutely constant results, at least not in the mammals; but it is not so if we cut one of the sciatic nerves of an animal (dog, rabbit, guinea-pig, frog,) which has undergone a complete transverse section of the cord near the medulla. The vessels of the posterior member, on the side of the cut-nerve, dilate more than those of the other posterior member. This phenomenon has been already noticed, in 1855, by M. Schiff. I have also seen, but not invariably, the section of the cervical cord of the sympathetic performed on mammals, after the spinal cord had been cut transversely in its upper part, to produce a new elevation of temperature in the corresponding ear.

We may conclude from these experiments that the vessels, in spite of the transverse section of the cervical spinal cord, still preserve a certain degree of tonic contraction, and that the tonus is only completely abolished when the vaso-motor nerves are separated from their intra-medullary centres of origin by lesions in the course of their fibres, either in the spinal cord or without that organ. We cannot, therefore, admit that all the vaso-motor nerves have their place of origin in the medulla oblongata.

But I have determined, by various experiments, that we may produce reflex vaso-constrictive actions in the posterior members of an animal, the spinal cord of which has been transversely divided in the anterior (or superior) dorsal region.

The reflex vaso-dilating actions are produced under the same conditions.

Clinical observations also permit us to cause in man reflex reddening of the skin of the lower limbs when they are paralyzed from lesions of the spinal cord.

On the other hand, in all the lesions of the nervous centres which exalt the reflex activity of the cord, we observe that these reflex congestions are produced more rapidly and last longer than in the normal conditions. So that in hemiplegics we provoke the appearance of the reflex blush by mechanical irritations, such as striking the skin with a blunt point, more easily and in a more lasting manner on the paralyzed limbs than on the healthy ones. Also in cases of paraplegia, when the paralysis of movement is more pronounced in one member than in the other, we see equally that mechanical excitations of the cutaneous surface cause more rapid and more permanent redness in the limb which is most paralyzed than in the one of the opposite side.

If we compare all these experimental and clinical results with each other, it is impossible to believe in the existence of a single vaso-motor centre situated in the medulla oblongata. Besides, it is needful to state that this hypothesis appears hardly acceptable on *a priori* ground when we consider that every part of the body may, by the mechanism of reflex vaso-motor action, be the locality of circumscribed vascular constrictions or dilatations.

I conclude, therefore, by saying (1) that it is incorrect to assume a unique vaso-motor centre located in the medulla; and (2) that the vaso-motor nerves have, like the musculo-motor nerves of animal life, special centres of origin and of reflex action, distributed through the gray substance of the spinal cord; each of which centres may act alone on the vaso-motor fibres to which it gives rise, and can separately undergo the various modifying influences which act in varying the vascular *tonus*.

The Vaso-Motor Centre in the Medulla Oblongata.—Dr. Dittmar, *Bericht der Kais. Sachs. Gesell. der Wissensch.*, 26 July, 1873 (Abstr. by Obersteiner in *Psychiatr. Centralblatt*). Starting with the experiments of Owsjannikow, from which a rather circumscribed region in the medulla of rabbits had been located as the vaso-motor centre, Dr. Dittmar undertook to still more accurately fix the limits of this tract. His method consisted in seeking to find what portion of the medulla could be destroyed without preventing the reflex increase of blood pressure in the aorta system which ordinarily ensues after a sensible irritation. In order to make the experiment as accurate as possible, the cut was not made with a free hand, but with a peculiarly constructed lancet with movable screw.

By this method of operating, Dittmar was enabled not only to limit the longitudinal dimensions of this space, in opposition to the views of Owsjannikow, who employed a less cautious method (extending from the upper margin of the corp. trapezoides to 3 mm. above the calamus scriptorius), but also to determine its extent in both its other dimensions.

It follows from his researches that a prismatic space on either side within the above given limits must be intact in order that any reflex increase of blood pressure may be produced.

Microscopic examination of this circumscribed tract showed that it contained a nucleus of gray matter, which had been described by Dean and Kolliker as the lower more diffuse part of the upper olive; this nucleus is 3 mm. in length, 1½ mm. in breadth, and its inner margin is from 2 to 2½ mm. distant from the raphe. In the human subject this nucleus is a collection of gray matter, visible to the naked eye, on the median side of the facial nerve immediately at the level of its exit, and contains rather large, multipolar, ganglion cells.

The tonic centre for the vessels is placed by Dittmar somewhat higher up than their reflex centre, in opposition to the view of Owsjannikow.

Blood Pressure after Section of the Vagi.—J. Moleschott, *Untersuchungen zur Naturlehre, etc.*, 1873. (Abstr. in *Centralbl. f. d. Med. Wissensch.*) The author doubts the correctness of Von Bezold's statement, that, after the section of both vagi, the blood pressure curve rises almost perpendicularly to a considerable height and then remains there. He shows that the curve is not so modified, partly by numerous testimonies drawn from the experiments of earlier authors (Traube, Lenz), partly by his own investigations. These gave the following results: Immediately after the division of both vagi the blood pressure arose almost immediately from one-eighteenth to one-third of its total former height, and then fell again, so that in the course of from two to seventy minutes it had sunk to the normal, or below the normal height. Sometimes the normal height was not again reached; the decrease occasionally is not constant, but is now and then interrupted by insignificant augmentations. The author experimented on deeply narcotized dogs and rabbits, whose respiratory movements were very slight and infrequent. He shows that neither the nearness of death after section of the vagi nor the narcotic had any effect on the sinking of the arterial blood pressure. In accord with his well-known views on the cardiac action of the vagus, Moleschott believes that the increased pressure after division of these nerves is not due to any paralysis of inhibitory fibres, but that it is to be considered as the result of a more or less transient irritative action.

INFLUENCE OF THE EXTIRPATION OF THE SUPERIOR CERVICAL GANGLION ON THE IRIS.—(A. Joffroy, in *Rev. des Sci. Med.*)—Having removed in dogs the cervical ganglion of the left side and the upper part of the cervical sympathetic, M. Vulpian, at the end of fifteen days, curarized the animals and subjected them to artificial respiration. He next irritated the skin of the thighs and the abdomen by strong induced currents. On the right side in these cases a considerable dilatation was usually produced, on the left a very much slighter dilatation, amounting to perhaps a quarter or third of the radius of the pupil.

It results from this experiment that not all the sympathetic nerve fibres, which may act on the iris, pass through the superior cervical ganglion.

EXPERIMENTS ON THE TRIGEMINUS AND THE SUBMAXILLARY GLAND.—M. Claude Bernard, at the session of the *Soc. de Biologie* (Abstr. in *Rev. Scientifique*), March 14, reported the following experiments :

Repeating the experiment of Magendie on the trigeminus, he sought to find whether there existed a direct relation between the alteration of the nervous fibres consecutive to the section of the fifth pair and the trophic disorders which have been observed in the ocular apparatus. By dividing the fifth nerve at its origin, in front of the Gasserian ganglion, the cornea becomes opaque, and the conjunctiva becomes inflamed without the peripheral portion of the fifth pair, of which the ganglion of Gasser is the trophic centre, showing the slightest alteration. At the end of eight days the myeline is yet unsegmented and the nervous tubes are intact. He concludes that the section of the fifth pair causes the disorders of nutrition, because the trigeminus contains the dilator nerves, which, in the normal condition, counteract the influence of the constrictor nerves. The trophic disorders are produced on account of the preponderance of the constrictor nerves.

In applying again the method of Waller to an old experiment, he has been able to show, in the most unquestionable manner, that the reflex actions do not at all require the assistance of the nervous centres, and that the sympathetic ganglia act in regard to them, as Bichat said, the part of little brains. M. Bernard showed that by dividing the lingual nerve and the chorda tympani at the same time, he could, by exciting the peripheral extremity of the lingual, produce an abundant salivary secretion from the submaxillary gland, by reflex action. The submaxillary gland contains the reflex centre, for if it is destroyed the flow is no longer produced. But this action becomes less on the following days, and two or three days after the section of the lingual and the chorda tympani the reflex phenomena fails entirely. But in the dog, whose chorda tympani remains uninjured, and whose trigeminus has been cut forward of the ganglion of Gasser, we can produce an enormous flow of saliva after eight days. The action of the submaxillary gland is, therefore, preserved when the lingual branch of the fifth pair is yet undegenerated, although the trigeminus be divided in front of the Gasserian ganglion.

Uterine and Vaso-Motor Nerve Centres.—W. Schlesinger (*Stricker's Med. Jahrbucher*, 1874, 1,) discusses the centre for the vaso-motor system, together with the innervation of the uterus. He found that strychnia injections caused irritations to be effective on the vaso-motors after complete division of the cervical spinal cord, and the consequent complete separation of the general vaso-motor centre in the medulla, from which he concludes that there exist primary vaso-motor centres likewise in the cord. He sums up his experiments as follows:

I. Strychnia causes, very quickly, an important increase of the blood pressure when the cervical cord has been cut.

II. That in strychnized animals suspended respiration causes, in most animals, either a notable increase of the blood pressure or a rhythmic variation in the sphygmographic trace.

III. That these animals sometimes show, with excitation of the spinal nerves, a coincident increase of blood pressure.

IV. That the muscular arrangement of the vessels in relation to their nerve centres shows many analogies to that of the uterus.

V. That the uterine nerve centres certainly extend down past the floor of the fourth ventricle.

Whence follows the apparent probability that the vascular centres extend down past this region into the cord; that the function of these centres disappears from our observation and their influence on the vascular tonus is lost as soon as the medulla is separated from the cord; and that strychnia can arouse this function temporarily.

Whether the centres situated in the cord below the medulla have any actual influence, in normal conditions, with the cord uninjured, or not, is still an open question. The circumstance that these centres do not noticeably act when the cord is cut, except under the influence of strychnia, can scarcely come under consideration. Nor do we yet know what alterations take place from the severance of the connections between the cord and brain.

Prof. E. Cyon (*Pflueger's Archiv*, 1873, 349,) details the result of experiments made under his direction, by Dr. Scherschewsky, as to the innervation of the uterus. After noticing the various opinions that have been promulgated, and criticising the experiments of some of his predecessors who had used animals with virgin uteri, and describing his own methods, he sums up as follows:

1. The uterine plexus contains the most important, if not the only motor

nerves which can produce actual contraction of the uterus by irritation of their peripheral ends. (Irritation of the central ends only produced violent attacks of vomiting.)

2. Irritation of the central ends of the first two sacral nerves caused, in a reflex manner, powerful movements of the uterus, which disappeared after the previous separation of the uterine plexus. (Irritation of the peripheral nerves only caused strong contraction of the bladder and rectum.)

3. Excitation of the brachial, crural, median, sciatic nerves, etc., caused no peristaltic movements, but only a little rigidity and paleness of the uterus.

4. The consequences of the excitation of these nerves failed to appear when the aorta had been previously ligated. Irritation of the central ends of the sacral nerves, however, brought on the peristaltic movements after this ligation.

5. Suffocation, by temporary stoppage of the respiration, caused powerful peristaltic movements of the uterus, apparently through direct excitation of the smooth muscular fibres by the accumulated carbonic acid of the blood.

Innervation of the Intestines.—J. P. Hongkeest Van Braam, *Pflueger's Archiv*, 1873, VI. 266, has instituted further experiments, following the method of Sanders of opening the abdominal cavity of animals in a solution of common salt at the natural temperature of the body, in order to observe the peristaltic movements under the most favorable conditions. He concludes from these that the vagus may be a motor nerve for the stomach, but that it produces no movements of the small intestine, and when these occur after irritation of the nerve they are produced by the impulse derived from the pressure of the stomach into the intestines. The vagus has no influence on the large intestine or the uterus. The splanchnic nerves are vaso-motor nerves of the intestines, and at the same time inhibitory nerves for the movements of the bowels and stomach. The question whether the inhibitory action of the splanchnics is independent of the vaso-motor, or merely a consequence of the latter, is considered as still undecided by the author.

Von Basch, *Stricker's Jahrbucher*, 1874, 45, publishes an account of some experiments performed by himself on dogs, in order to test the correctness of the view of Pflueger, that the splanchnic nerve acts as an inhibitory nerve on the intestines, like the vagus on the heart. He found that when this nerve was completely divided, and its influence on the movements of the intestines altogether eliminated, an inhibitory influence was still exercised by the vaso-motor nerves when the cervical cord was electrized, and that this influence was increased by the compression of the aorta, thus cutting off the vascular motor impulse. He therefore concludes that the splanchnic nerve is not properly an inhibitory nerve, like the vagus for the heart, but that it possesses this action in its vaso-motor capacity. Stoppage of the respiration also inhibits the abdominal movements, because of the excitation of the vaso-motor centre by the unoxydized blood.

Innervation of the Bladder.—N. Sokownin, in a communication at the meeting of Russian scientists in Kasan, (reported by N. Kowalewsky and C. Arnstein, in *Pflueger's Archiv*, VIII., 600,) discussed the subject of the nerves of the urinary bladder.

His experiments were chiefly directed to the central nervous apparatus which causes contraction of the bladder. The author sought first to establish the existence of the motor centre in the brain, and also to test the objections raised by Afonassief against Budge's experiments. Afonassief stated that, in Budge's experiments with the irritation of the pedunculi cerebri and other parts at the base of the brain, the vaso-motor system played the principal part in producing movements of the bladder. After the author had proved that suffocation, hemorrhage, and compression of the aorta caused contraction of the bladder, even when all its nerves were divided, he found in the previous ligation of the aorta a means by which he could exclude the interference of the vaso-motor system when the cerebral peduncles were excited, and allow the results of this excitation to appear altogether uncomplicated by any other agency. The experiments with the ligation of the aorta and subsequent irritation of the peduncles, favored the existence of a motor center in the brain, since they produced powerful contractions of the bladder, to which no mere circulatory disturbance could give rise. The motor nerves of the bladder leave the cord chiefly by the first, second and third sacral roots, to join the hypogastric plexus. A small part go from the cord to the inferior mesenteric ganglion, and from this to the hypogastric plexus. Reflex contractions of the bladder are incited (1) from the sensible nerves of the body [principally the sciatic, crural and splanchnic], with the exception of the vagus, and (2) from the sensory nerves of the bladder itself, which are contained partly in the sacral nerves and partly in the anastomotic branches between the hypogastric plexus and the mesenteric ganglion. The contractions produced through the first named nerves depend on sensations of pain, and continue even after ablation of the hemispheres or complete section of the cervical spinal cord. The reflex movements which are incited by the sacral sensory nerves of the bladder start from the cord, about the fourth lumber vertebra, while those incited by its sympathetic sensory fibres arise in the lower mesenteric ganglion.

Reflex Movements of Respiration.—M. Knoll reported to the Vienna Academy of Sciences, Dec. 18, 1873 (Abstr. in *Rev. Scientifique*), the conclusions of a series of experiments on the production of these movements. They were produced by the introduction of volatile liquids into the air passages below the larynx. Chloroform introduced by the aid of a tracheal canula, in such a manner, moreover, that it could not act on the nasal mucous membrane, caused an acceleration and a *depression* of the respiratory movements, and in certain cases an arrest in the position of inspiration. The division of the vagi showed that the reflex action was transmitted through them. Ether, benzine and oil of mustard produced the same effect with less intensity.

A weak solution of ammonia caused the same effects as chloroform. A concentrated solution caused profound disorders; an alternation of retardation during expiration, and of acceleration during inspiration, was observed. Pure carbonic acid caused, first, a moderate acceleration, then a retardation, without invoking at all the action of the pneumogastrics, since the phenomena took place, whether they were divided or intact.

CURRENTS OF POLARIZATION.—M. Onimus, in exhibiting a polarization battery, constructed by M. Plante, at the session of the *Soc. de Biologie*, March 21, offered some general remarks on polarization currents, which are reported in the *Gaz. Med. de Paris*, April 4 :

It was especially for the purpose of showing the force of the currents of polarization that M. Onimus presented this apparatus, because, as was seen, they might become much more energetic than the primitive current, and give rise to effects much more important.

But, as M. Onimus has observed, in the animal tissues these currents of polarization are always formed when a current traverses any tissue whatever. They are the stronger the longer the duration of the original current, so that each time when we electrize any region, there is produced, after the cessation of the current, the so-called polarization current in the depths of the tissues, which is invariably in the reverse direction from the original one. After having electrized a nerve, for example, with a descending current, there is formed, when the circuit is opened, an ascending current, the influence of which is so often more considerable than that of the direct current, and which causes contractions.

In fact, in most experiments on nerves, the phenomena of excitation which we cause at the opening of the current are due, not to its cessation, but to the production of this current of polarization which takes place at that instant ; and this explaines the majority of the electro-physiological laws established by Du Bois Reymond, Pflueger, Cyon, and the German school generally.

The greater part of the phenomena which we call the analectrotonic or catalectrotonic state are only the result of electrolytic action, and the irritation due to the formation of these currents of polarization.

Some well-known examples may serve to show how the facts on which Pflueger and his students support themselves receive a natural explanation, when we take account of the formation of these currents of polarization.

We are aware that the descending current causes contractions first and most readily, and if the excitation of a nerve does not provoke a contraction any more, except at the closing of the descending current, and that with the ascending current we obtain a contraction at the opening, that is due, not as they say to the disappearance of analectrotonus, but rather to the action of the current of polarization which is formed at that moment and the direction of which is descending.

So when a nerve has been traversed for a long time by a current in a uniform direction, say a descending one, for example, it loses its excitability for this current, but is excitable for an ascending one, and *vice versa*.

But in this case the following takes place: After having electrized the nerve with a descending current it produces, at the moment of its stoppage, a current of polarization in the opposite direction, that is ascending, which destroys the descending current, while in employing them on an ascending current it adds itself to the current of polarization to cause an excitation.

These facts, which might be multiplied, show the importance of polarization currents, and give a simple explanation of all the phenomena on which are founded the theories of Du Bois Reymond, Pflueger, and other physiologists.

CENTRES FOR THE RESPIRATORY NERVES.—Dr. Procop Rokitansky, *Stricker's Med. Jahrbucher*, 1874, 30, details his experiments on the location of the respiratory centres. He found that rabbits, poisoned with strychnia, continued to exhibit the respiratory movements even after the division of the cervical spinal cord. When the medulla oblongata, however, was cut through at the posterior border of the pons they died from asphyxia, but if in this condition strychnia solutions were injected, they began again to breathe. He gives the respiratory curves taken according to the method of Breuer and Hering.

THE SENSE OF EQUILIBRIUM.—M. Mach communicated to the Vienna Academy of Sciences, Nov. 6, 1873, (notice in *Revue Scientifique*) some experiments on the sense of equilibrium in man made on himself. He explains the phenomena of whirling of Flourens, of the orientation of the equilibrium, the phenomena of vertigo, etc., by assuming that the nerves of the ampullæ in the circumvolutions of the labyrinth of the ear respond by a sensation of rotation to the excitations that are habitually produced by this movement.

TERMINATIONS OF THE NERVES.—MM. Rouget and Jobert, in some recent sessions of the *Soc. de Biologie* of Paris, communicated some interesting observations in regard to the terminations of the nerves, an abstract of which we are able to supply our readers from the *Gaz. Med. de Paris* and the *Progres Medical:*

M. Rouget describes the nervous endings in the glands: In pursuing his studies on the development of the nerves in the tail of batrachians, he discovered minute glands stretching in chains in tissue devoid of vessels. They were formed by a mass of cells and granular protoplasm, without enveloping membrane, in the centre of which he observed a lacuna which opened to the exterior, and represented the excretory canal. Below this he saw a large nerve-tube, of double contour, resembling a motor nerve, which, at the level of each gland, sent to it a branch which is lost in its substance. This branch still retains its envelope of myeline as far as the surface of the gland. Exactly how the fibre terminated in the surface of the gland was not clearly seen, but it was thought probable that the envelope disappeared and the cylinder axis was brought into direct contact with the secreting surface, exciting it into activity. These glands being entirely isolated, it is plain that the nerve terminates within their substance.

M. Jobert, in his communication, announced that he had discovered in the wing of the bat a peculiar tactile apparatus developed around the hair bulbs, and the same, also, in the tail of the rat. This, as we are aware, is formed of a number of rings, graduated towards the extremity, and covered with a kind of imbricated scales. There are scattered on these rings long and coarse hairs; each of these hairs is furnished with sebaceous glands, and with a kind of collar, due to a thickening of the connective tissue. Nerve-tubes from the cord, to the number of from four to six, pass toward this collar. In their course they swell into a nucleated cell, then becoming tubes again they bifurcate, and, preserving their myeline they end in the tissue of these follicles where they become entangled in an exceedingly rich plexus. M. Jobert believes that he has sometimes seen the cylinder axis terminating in slight expansions analogous to the corpuscles of Pacini and Meissner.

The least contact of one of these hairs with anything external is at once communicated to the nervous system, and, according to M. Jobert, this is greatly utilized by the animal in his progression; indeed, the loss of its tail will almost totally disable it. The rat of the fable of La Fontaine may have been wise in counsel, but for action he was of no account whatever.

NERVE TERMINATIONS IN THE JOINTS.—Prof. W. Krause describes (*Centralblatt f. d. Med. Wissenschaften*, No. 14, March 21) the endings of the sensible nerves in the digital articulations. He describes peculiar bodies which he terms "joint-nerve corpuscles," the larger ones of which are visible to the naked eye. They are generally longer than broad, somewhat flattened, and consist of a longitudinally striated connective tissue envelope, containing smooth cells, resembling those of the endothelium, and within we find numerous elongated or ellipsoidal nuclei, a finely granular substance like that of the taste corpuscles, not resolvable by the strongest powers, and a number of non-medullary terminal nerve fibres.

Altogether similar bodies were detected in rabbits, dogs, rats, etc., and are described. Prof. Krause used either fresh preparations with acetic acid, or the clean synovial membrane which had been laid twenty-four hours in a two per cent. solution of acetic acid or strongly diluted hydrochloric acid. Strong acids destroyed these corpuscles.

INFLUENCE OF THE SPLANCHNIC NERVES ON THE KIDNEYS.—M. Vulpian communicated to the *Soc. de Biologie* May 17, 1873, (*Gaz. Med. de Paris*, May 31,) the results of observations as to the influence of the splanchnic nerves on the kidneys. His experiments were performed on dogs poisoned with curare or morphia. After opening the abdomen he cut the left splanchnic, producing a congestion of the kidney, which reddened and apparently increased in size, as did also the renal vein. The urine became more abundant, and albuminuria ensued, but without extravasation of blood globules or desquamation of the tubes.

On electrizing the peripheral end of the nerve it was first observed, as had been already done by Cl. Bernard, that it was extremely sensitive. At the same time it produced, after the lapse of a few seconds, a well-marked pallor of the organ, which took on the chamois tint commonly seen in the cadaver. After decortication of the kidney this phenomenon was still more marked. On ceasing the electrization, the kidney remained still pale for a certain time, then retook its natural red color, the renal vein at the same time diminished in calibre. In the rabbit the phenomena were the same, but less pronounced.

b.—PATHOLOGY OF THE NERVOUS SYSTEM AND MIND, AND PATHOLOGICAL ANATOMY.

Changes in the Cord, following Nerve Lesions—M. Hayem made, at the session of the *Societe de Biologie*, March 28 (reported in the *Gaz. Med. de Paris*), a new communication relative to the alterations observed in the cord consecutive to lesions of the nerves. The laceration of the nerves gives rise to a cicatricial myelitis, which is the point of departure of a diffuse and progressive alteration, causing gradually the atrophy of a great number of cells throughout the whole length of the cord. In this way the rupture of the facial causes an atrophy of the cells of the medulla, a lesion which reaches the medullary nuclei on either side, and then invades the cervical and dorsal regions of the cord.

The rupture of the second cervical nerve in the cat is followed by an analogous alteration, at the same time ascending and descending. The same operation performed on the sciatic likewise produces a cellular atrophy, which gradually invades the cord as far up as the medullary nuclei.

In all these cases the alterations pass from one side to the other, but in a very irregular manner, not attacking always cells on the same level.

The same facts may likewise be observed in the rabbit after a simple resection of the sciatic.

One of these last animals, on which M. Hayem performed resection of this nerve on March 7th, died the 19th, some twelve days after the operation, and at the autopsy he presented already a generalized myelitis, affecting the nervous cells and extending beyond the cervical protuberance. The two ends of the resected nerve were surrounded with a cellular-adipose tissue, œdematous and infiltrated with pus, and there existed, besides, a suppurative peri-meningitis in the lumbar region (purulent infiltration of the cellular adipose tissue which normally envelopes the dura mater).

M. Hayem thinks that this inflammation reaches the rachidian canal in following the course of the nerve. The central extremity of the nerve is itself a little inflamed. It contains some tubes of which the nuclei of the envelope of Schwann are multiplied; some have the cylinder axis slightly tumified. We may, therefore, admit that, in this case at least, the irritation of the cord has been consecutive to that of the nerve. This is, however, an exceptional fact; but, according to M. Hayem, it represents the acute stage of that which took place in the other experiments which he either related to the Society or in a note recently communicated to the Academy of Sciences.

In these various experiments the medullary lesions consisted almost exclusively in a modification of a certain number of nervous cells. In the rabbit, which died March 19, these elements were examined in the first state or after maceration for some hours in the liquid of Mueller.

The protoplasm of the cells was hollowed into larger or smaller cavities, a kind of vacuoli or vesicles. The normal granular contents were either pale or transformed into a refracting substance of vitreous aspect, and the nucleus, at first clearly defined, became obscure and disguised, and then atrophied by the modifications of the protoplasm. At the same time the elements of the neuroglia situated in the gray substance became multiplied, and some of them hollowed out among themselves into a vesicular space.

INFANTILE PARALYSIS.—Dr. Mary Putnam Jacobi, *Am. Jour. of Obst.* April, 1874, contributes a lengthy paper on the subject of infantile paralysis. She gives a very complete review of the literature of the subject, and of the various views of the pathology of the disease, and adopts the theory that it is of central origin and due to irritative alteration of the motor cells. This opinion is reached by a process of reasoning by exclusion too detailed to follow satisfactorily in a notice like the present. We will only add the concluding remarks of Dr. Putnam-Jacobi:

"The immense pathological importance of the study of infantile paralysis may be best appreciated by enumerating its different pathological relations, which the foregoing pages have tried to set in relief.

1st. "It links together the most conspicuous external deformities, involving entire limbs, with lesions of internal microscopic groups of cells so minute as, until recently, to have escaped observation.

2d. "By exquisite localization of pathological lesions it confirms the doctrine of localization of function and independence of morbid processes in special groups of nerve cells.

3d. "It helps to establish a group of diseases bearing various relations of cause or effect to this same group of cells—the anterior spinal—as adult spinal paralysis, progressive muscular atrophy; finally, even bulbar paralysis, where the disease is confined to the groups of motor cells in the medulla.

4th. "With these others it helps to show the immense and peculiar influence exercised upon the nutrition of muscles by the nerve cells influencing their motor nerves. This influence is in both resemblance and contrast with that exercised on the nutrition of the skin and subcutaneous tissues by the groups of cells connected with the posterior roots and sensitive nerves. Lesions of these produce eschars, as of those atrophy, sclerosis, or fatty degeneration.

5th. "As a localized myelitis, certain cases, at least, of infantile paralysis are to be considered in their relations to other forms of myelitis, localized or diffused, parenchymatous or interstitial. They are to be contrasted with cases of tabes dorsalis, in which the myelitis localized in the posterior cornua determines a fasciculated sclerosis of the posterior columns, relatively more frequent and important than the anterior sclerosis; contrasted also with the anterior lesion of wasting palsy, which, from the slow march of the disease, may often depend on an extension of irritation from the periphery; contrasted with acute diffused central myelitis, with equally rapid march, but where the lesion involves both neuroglia and nervous elements.

6th. "As originally confined to the latter, the lesions of infantile paralysis offer one of the best illustrations of the 'parenchymatous inflammation,' long ago described by Virchow.

7th. "By its sudden invasion infantile paralysis is symptomatically allied to such accidents of the vascular system as congestion or hæmorrhage.

But as these are shown to be either absent or rare, or consecutive to an affection of nerve cells, the capacity for independent morbid action possessed by these latter receives another confirmation.

8th. "These affections serve as a link between the so-called reflex or inhibitory paralyses and those dependent on marked lesions of the cord.

9th. "Finally, they trace minutely the successive steps in a morbid process that, beginning in a functional alteration of the cellular nutrition, terminates in organic destruction of tissue, and thus dissect apart the complex phenomena both of inflammation and of general cell life."

Congestions of the Liver Accompanying Cerebral Hæmorrhage.—M. Ollivier reported to the *Soc. de Biologie*, April 11, (*Rev. Scientifique*, April 18,) that he had observed an apoplexy of the liver consecutive to a cerebral hæmorrhage. There existed in the liver, together with an intense congestion, two hæmorrhagic patches, one of which was about the size of a small hen's egg. This observation is important and remarkable. It may explain the glucosuria which we observe to sometimes accompany cerebral hæmorrhages, just as the congestion of the kidneys explains the albumen which we find in the urine.

Pulmonary Hæmorrhage from Wounding of the Cerebrum.—Prof. H. Nothnagel publishes, in the *Centralblatt f. d. Med. Wissenschaften*, No. 14, March 21, the following note :

If a limited region of the upper surface of the cerebrum of the rabbit (in the neighborhood of the sulcus already described) is wounded, with a simple needle puncture, there follow peculiar disorders and especially hæmorrhages in the pulmonary tissues, often to such an extent that almost the whole lung is involved. As is known a similar observation was made by Brown-Sequard, but not after injury to the upper but to the basal surface of the brain.

In the second place we can in a similar manner regularly cause a meningitis, generally on both sides, very rarely on the same side as the injury, and sometimes on the opposite side. This meningitis is not merely accidental. The proofs of this, as well as the more full account of these conditions will be given at a future time.

A New Variety of Acute Myelitis.—At the session of the *Soc. Med. des Hopitaux*, Febr. 27 (reported in *Bull'n. Gen. de Therapeutique*, March 15), M. Martineau communicated the following observation : A young man, twenty-three years of age, entered La Pitie Aug. 19, 1873. He had been ill some fifteen days and his symptoms consisted in an incomplete paralysis of the inferior and superior members, with paralysis of the bladder and rectum. Sensibility and electric contractility were retained, but eschars and congestion soon supervened in the paralyzed members, and death ensued on Aug. 21, three days after the entry into the hospital, with all the symptoms of rapid asphyxia. The autopsy showed, to a very careful examination by M. Troisier, a generalized lesion of the gray matter of the spinal cord, consisting in the almost complete disappearance of the nervous elements of the anterior horns, with granular degeneration of the cells of the posterior ones; the whole of the gray substance seemed altered, while the white substance throughout remained intact.

M. Maurice Raymond asked M. Martineau if he had found no lesion of the heart, as he had observed that there existed a very intimate connection between acute ascending paralysis and articular rheumatism.

M. Hayem remarked that the case narrated by M. Martineau differed from the cases so far known of acute myelitis, in which we always see lesions of the white substance as well as of the gray. The persistence of sensibility and electro-contractility also distinguished this case from those of acute ascending paralysis.

M. Martineau replied that the heart showed no alterations to a very careful scrutiny. As to the sensibility and electro-contractility, he had determined their existence one hour before the death of the patient. He thought that his observations, so far unique, might serve as a basis for the study of a new variety of acute myelitis.

The complete report of this case is given by M. Martineau in *L'Union Medicale*, March 10.

INFLUENCE OF ALCOHOL ON CUTANEOUS AFFECTIONS.—The important works which have been written in later years on the subject of chronic alcoholism are yet far from having exhausted the subject. One point in particular, which has yet scarcely attracted the attention of pathologists, is that relative to the influence exerted on the external integuments by the prolonged use of alcoholic drinks. This subject has been glanced at only in the learned article on alcoholism by M. Lancereaux, in the *Dictionnaire Encyclopedique des Sciences Medicales.* Nevertheless, if we consider the important part taken by the skin in the elimination of alcohol, it seems reasonable to admit that the prolonged abuse of alcohol ought to have *a priori* an action on the integument. Dr. Reucalt has deemed that it might be of interest to verify clinically this physiological conclusion. He has therefore devoted his last year as *interne* at the Hospital St. Louis, in the service of Dr. Hillairet, to this kind of investigation.

The first series of cutaneous affections to which he gave his attention, was that of the so-called spring eruptions, roseola, millet, lichen, urticaria, and pityriasis.

In four patients suffering from eruptions of this class—one case of urticaria, a second of millet rash, the third of herpes iris, and the last of hydroa bullata—two, the third and fourth, showed plainly the relation existing between the eruption and a recent alcoholic excess. In the last, especially, the same eruption was produced twenty times in seven years, and on each occasion after an excess in drinking.

It being characteristic of alcohol to cause congestion of the skin, should we not attribute its phlegmasic irritation to the frequent repetition of this action?

In fourteen subjects of tertiary syphilis, M. Renault proved that all, with only one exception, had been given for years to alcoholic excesses. Analysis of these fourteen cases shows that these accidents were developed in spite of previous mercurial treatment, and that the syphilides took on an ulcerous type under the influence of alcoholism. Alcoholism, therefore, exercised an influence in these cases, on the appearance and progress of the syphilides, analogous to that of scrofula.

Twenty-six cases of psoriasis were divided into four classes: The first comprising the patients either addicted or not to alcoholic drinks, (to the number of ten,) who present from the beginning a confluent eruption of very rapid generalization; the second, those habitually given to alcoholic drinking (number, five cases), in whom the psoriasis, at first discreet, extends itself gradually, and shortly becomes general; the third, the temperate individuals (eight in number), in whom the eruption, discreet at the beginning, remains permanently so; and the fourth, those of temperate habits, in whom the psoriasis only becomes extensive after a long series of years.

The general conclusion derived from this study seems to be, that alcohol has an injurious influence on the cutaneous affections pertaining to the group of the phlegmasias. — *Gaz. des Hopitaux*, Feb. 28.

Circular Insanity.—Ludwig Meyer has, during the past year, come to the opinion that the theory of Falret of *folie circulaire*, *i. e.*, of the existence of a special mental disorder, consisting of a regular and continuous succession of melancholic and maniacal attacks, a theory the correctness of which had been generally doubted, is founded in the main on facts, and that particularly certain trophic processes, that are connected with this cyclic disorder, serve to distinguish it from the common change of mania and melancholia in other cases of mental disease. In Meyer's cases the melancholic phase usually occurred first, and lasted for a long time, frequently a year, and the maniacal stage came afterwards, while in those of Falret and Baillerger the latter was the primary. The initial melancholia exceeded all later attacks in its duration and violence, and was accompanied with what the author considers as peculiar to these attacks: short maniacal periods, often only of an hour's duration, characterized by lively gesticulation, grimaces, and loud laughter. The long period of initial melancholia was followed by a long intermission, during which the patient frequently showed few marked symptoms. We extract two cases from the author's tables, in the first of which the initial melancholia lasted six months, the intermission immediately following, eight months, and the succeeding maniacal period six months, and the second intermission three months; in the second cycle the disease in its melancholic phase occupied three and a half, in the intermission five, in the maniacal stage, four, and the succeeding period two and a half months; while a third cycle gave for these changes respectively, five, six and three months. In a second case the observation extended over seventeen such cycle periods. The development of the melancholic as well as of the maniacal attacks, is, in most cases, only moderate. According to Meyer's experience, the melancholic attack comes on very suddenly; it produces only an expression of sadness and silence; delusions are not always apparent, and very soon there is seen an especially noticeable altered appearance of the patient, and a very pronounced disorder of the appetite, with obstinate constipation, also appears to be characteristic of this phase. In the following intermission the patient is somewhat timid and shy, and a little distrustful. Very gradually, under the disguise of this condition, appears the onset of the maniacal phase, which, in the cases of the author, first showed itself by an inclination for intrigue and gossip; then increased excitement, which sometimes gives the impression of increased mental power, and is also sometimes characterized by the endeavor to make itself as little apparent as possible, and this makes it the more difficult to decide definitely whether delusions and hallucinations are present or not. In some cases they undoubtedly do exist. But the most noticeable feature of all—at least in the three cases observed at sufficient length by Dr. Meyer—was a notable increase in the function of nutrition during the maniacal period, which, unlike all the other phases of insanity, was characterized by a decided addition to the bodily weight, which, in two of the patients, increased, on the average, three pounds a week, and in one four pounds, and there was, besides, a very altered and much more youthful aspect. Folds and wrinkles disappeared, the skin became moist and tense, the hair more shining and thicker, so that the patients looked ten or twenty years younger than they really were; the digestion was active and the bowels regular. The rapidly exalted nutritive conditions cannot altogether be attributed to the increased assimilation of food. The maniacal period of the circular insanity is therefore to be considered as an actual tropho-neurosis, which extends throughout the whole organism and produces a general increase in the nutritive processes. Whether the melancholic phase is to be considered as the opposite condition in this respect, is still uncertain; the more so, as the pulse and heart's impulse are alike, in each period being small and weak.—*Archiv. f. Psychiatrie. Berliner Klin. Wochenschrift.*

Hemianæsthesia of Cerebral Origin.—M. Veyssiere reported at the session of the *Societe de Biologie*, March 14 (abstr. in *Rev. Scientifique*), the results of some experiments made by himself on the production of hemianæsthesia of cerebral origin in the lower animals, which, in man, as is well known, is due to a lesion in the expansion of the cerebral peduncles. After having vainly attempted its production by caustic injections, he at last succeeded in locating the injury by introducing, through the canula of an exploring trocar, a stem ending in a point bent at a right angle, which produced a clearly defined laceration. By this means he caused the hemianæsthesia five times, twice very perfectly, and the lesion produced corresponded exactly to the locality in which it had been found in the autopsies of the various physicians who have reported cases of this kind.

Acute Insanity.—H. R. Bigelow, M.D., (*N. Y. Medical Record*, May 1,) discusses the two phases of acute mania and acute melancholia. After alluding to the opinion of Sankey, that there is no such disorder as acute primary mania, and expressing his dissent from this opinion, he gives the principal diagnostic differences between these two forms. In acute mania the attack is sudden, there is no period of incubation, perhaps no hereditary taint. It affects men more often than women; there is great incoherence and restlessness, but no fixed delusion; apparent unconsciousness of surroundings. There is generally a marked retinal hyperæmia.

In acute melancholia, on the other hand, there is a period of incubation, characterized by change of manner and habits, and close investigation generally reveals some hereditary neurotic taint. It occurs also most frequently in females; is accompanied by fixed delusions, although the patient may be altogether conscious of surroundings. The ophthalmoscope usually reveals a condition of marked anæmia, and the muscular sense is greatly perverted. The prognosis is also more favorable in these cases than in those of acute mania.

As regards the diagnosis of acute mania and general paralysis, which Sankey considered as one of the disorders most frequently called acute mania, Dr. Bigelow states that the sphygmograph will always decide. The tracings in these instances are peculiar, the upward line being short and abrupt, the descending one long and wavy, and lacking the aortic notch.

VASO-MOTOR NEUROSES OF THE EYE.—Dr. Nagel, of Tubingen, publishes (*Klin. Monatsblætter f. Augenheilkunde*, Dec. 1873,) a paper on this subject, of which we insert the following abstract, given in the *Revue des Sci. Medicales:*

"Donders has already called the attention of observers to the disorders of secretion of the ocular fluids. He offers, first, the hypothesis that simple glaucoma is a neurosis of the secretor nerves of the eye. Later, Bowman made some interesting observations on the rapid changes which more or less modify the intraocular tension. Von Graefe signalized, under the name of essential phthisis of the eye, an affection characterized by a sudden and considerable diminution of the ocular tension.

"In the work we analyze, Dr. Nagel gives the clinical researches which he has instituted on the variations of this tension, which are often accompanied by other pathological phenomena. He uses, to designate augmentation or diminution, the expressions "hypertony" and "hypotony."

"Hypotony is very frequent and accompanies, not only parenchymatous keratitis, but also nearly all varieties of keratitis, particularly the phlyctenular form. Sometimes it constitutes the predominant symptom of the disease; sometimes it is only accessory, co-existing with considerable lesions. It is constantly associated with a contraction of the pupil.

"This diminution of ocular tension is now and then accompanied with severe neuralgic pains, comparable to attacks of migraine.

"In certain diseases of the cornea of no apparent gravity, the insomnia, slight febrile symptoms and chills, denote a still more general vaso-motor disturbance. The inaction of atropine on the dilatation of the pupil, even when no synechia or iritic complication exists, is one of the important symptoms of this condition.

"The slighter the tension and the more constricted the pupil, the more intense are the neuralgic pains.

"Dr. Nagel has observed in hypotony, whether allied or not with corneal lesions, a local change of the temperature in the neighborhood of the affected eye.

"It rises from ½° to 1°, sometimes even to 4°. In rare instances it is increased; still, in an acute attack, the temperature at first elevated, afterward descended 2°.

"Other vaso-motor troubles, such as congestion of one-half the face, swelling of the nose, chills, are also sometimes present.

"The refracting power of the eye is increased. But it is difficult to determine whether this modification is due to a true cramp of the ciliary muscle, or a change of position and fixation of the crystalline lens on account of the diminution of the ocular tension.

"Slight contusions of the ocular globe are frequently followed by hypotony. We can establish this fact experimentally on animals.

"According to Dr. Nagel, there is no doubt but that we have here to do with a paralysis of the cervical portion of the great sympathetic.

"By exploring the region of the neck or locality of the affected nerve-trunk, we sometimes discover an exaggerated sensibility to pressure. We might compare with these pathological phenomena, the migraine with spasmodic contraction of the pupil, which Du Bois Reymond considers as a neurosis of the sympathetic. It is not rare to observe the transition to the paralytic from the spasmodic form. In a very limited space of time the phenomena of paralysis of the sympathetic may succeed the phenomena of irritation in the same individual. Dr. Nagel has also observed hypotony in a certain number of general maladies, considered to be neuroses of the great sympathetic—angina pectoris, for example. In the hypotony, accompanied by the limited hyperæmia of one-half of the face, the rise of temperature seems to be the result of a paralysis of the sympathetic; this hypothesis is so much the more acceptable since Pourfour du Petit showed, some one hundred and fifty years back, that the division of the sympathetic in the neck (besides the well-known phenomena which it produces) is followed by a decrease of the intraocular tension; this feature has since then escaped the notice of the greater number of physiologists, because they have operated on rabbits, while Pourfour du Petit experimented on the dog, where it is more marked.

"The disorders of the sympathetic are not the only ones capable of producing hypotony. Donders has already observed that the section of the trigeminus is followed by hypotony; on the other hand, Hippel and Gruenhagen have proved that the irritation of this nerve is followed by hypertony. In zona ophthalmica, when lesions exist in the branches of the trigeminus and in the ganglia of Gasser, we often see a considerable degree of hypotony."

Microcephalism and Cretinism.—Prof. Caesar Lombrosi, *Riv. Clinica*, 1873, 195 and 333, (abstr. in the *Rev. des Sci. Medicales*):

"The author gives the following conclusions of his work:

1. "The anthropometric study of cretinism and microcephalism often gives curious and parallel results. Frequently the anomalies of the cranial vault and of the limbs of microcephalics, remind us of the pithecoid type, and perhaps even further; the structural anomalies of cretins, with the projection of the canines, the horizontality of the basilar bone, the hairy forehead, the flattening of the palate, and the disposition of the limbs, carry us back to the later quadrumana and quadrupeds. Still more frequently cretins offer certain special characters of the black race, such as the obliquity and infundibuliform condition of the orbits, the widening of the

distance between the two orbits, the prognathism, the retraction of the lower jaws, the obliquity of the canine teeth, the deepened color of the skin during life, the coarseness and shortness of the hair, and, what is still more curious, the flattened hair like that of the Hottentot, the eurygnathism of the yellow race, and the analogy between the two sexes.

"It is certain, moreover, that when we only take account of the cranial peculiarities, the pithecoid characters predominate at the base of the cranium in the cretins, and in the vault in the cases of microcephalism.

"These coincidences are, however, not absolute, and as the cretin often possesses the cranial narrowness of the microcephalic, and the latter the prognathism and dental anomalies of the former, so both may lack the pithecoid, negro, or bestial characters, and show peculiarities purely teratological. Such coincidences, however contradictory they may be, are too numerous and too apparent to be denied; they help us to penetrate into the nooks of the past, by allying man with the remoter circles of animal life.

"These coincidences appear also to indicate to us, in the midst of a chaos of contradictory lesions, the place which cretins occupy in nature, and this place appears to me to be between the dark races and the still lower quadrumana.

2. "The knowledge of these frequent, though imperfect reproductions of the negro type, renders more probable the demonstration of the hypothesis given above, that the white race is due to an improvement of the colored races. The greater relations with the quadrupeds and the lower quadrumana than with the primates go to confirm the hypothesis according to which we are derived with the primates, from quadrumana and perhaps from lower vertebrates.

3. "These analogies have still the advantage of explaining in part the genesis of cretinism, by an arrest of development at an epoch of fœtal life, in which we produce precisely the characters of the lower animals."

Diagnostic Characters of Epilepsy and Hystero-Epilepsy.—In his last clinic, at the hospital of the Saltpetriere, M. Charcot narrated a number of symptoms to serve for the diagnosis of the attacks of hystero-epilepsy from those of the true epilepsy. The importance of the subject leads us to give here a short review of these essential characters.

The first of these are taken from the attack considered by itself comparatively in the two diseases. In the hystero-epileptic, says M. Charcot, the attack is nearly always announced by prodromata of considerable duration. In that form of hysteria, which we may call ovarian, these prodromata constitute a special aura, which, leaving the ovarian region, reaches successively the epigastrium, the neck, and at last the head. These curious prodromic phenomena of incontestable diagnostic value, are entirely lacking in epilepsy; in such premonitory signs as are present the sensations occupy an altogether different region, and ordinarily only precede the attack by a few seconds.

The cry which the hystero-epileptics sometimes utter at the time of the fall, is generally prolonged, modulated; it is scarcely possible to confound it with the short cry that the epileptic emits.

At this phase—that is, when the patient falls—the symptoms are much alike in the two diseases; the head and the eyes are turned in the same fashion; the members are thrown into tonic convulsions; the whole body becomes rigid; the face is congested, and a sanguinolent foam runs from the mouth.

But the differences reappear anew at the moment when the epileptic is taken with an attack of snoring, resembling apoplectic stertor; the hystero-epileptic seems, on the contrary, to wake up; sometimes to return to the habitual condition; sometimes to offer a succession of symptoms, forming, in some measure, a new period, which we may designate under the name of the period of contortions. These contortions, so extravagant, so frightful, sometimes, are very variable as to form, and we may say that each patient, despite a few common traits, appears under her own peculiar aspect. During this period the hystero-epileptic appears to be under the influence of delirium, sometimes boisterous, which seems, in great measure, to determine the form of the contortions, the attitudes and the intentional gestures. M. Calmeil, in his remarkable work, has cited many examples of these strange convulsions, especially in the epidemics of London and St. Medard. Many of the patients exhibited by M. Charcot to his hearers offered the symptoms of this period in a most marked form.

To the contortions there succeeds, in the hystero-epileptics, a relatively tranquil delirium, of which one of the characters is the presence of hallucinations of sight and hearing. They hear clocks striking at random, and voices; they see all kinds of animals; butterflies fluttering on the ceiling; lizards crawling on the wall; crows, which peck at their faces; they seek by gestures to keep these imaginary beings off; at other times they imagine they tread on serpents, and make leaps to avoid them.

This delirium, which so far in a measure bears a resemblance to alcoholic delirium, is very different from that of true epilepsy, which consists, particularly, as we are aware, in a maniacal excitement, sometimes of extreme violence, of a sombre character, and which renders the patients very dangerous.

Suppose we have to do with a woman suffering under an attack, the nature of which is dubious? The compression of the ovary may help the diagnosis; of no effect if the case is one of epilepsy; in the hystero-epileptic, little as the ovary may have had to do with the cause, it produces, if not always a complete arrest of the attack, at least a more or less profound modification of the symptoms.

Sometimes, and as well in hystero-epilepsy as in epilepsy, the attacks succeed one another in a series, and produce, in the one case, the state of *mal epileptique*, and in the other, the *mal hystero-epileptique*. In such cases the thermometer is a useful aid; in the epileptic the temperature rises rapidly to a very high figure, 40–41 Cent. (= 104 to 106 Fahr.), or even attains (42° = 107.6 Fahr.), and death soon closes the morbid scene; in the hystero-epileptic, in spite of the almost incessant repetition of attacks of extreme intensity, the thermometer marks 37.5 to 38° and some tenths (= 99.6–100.4 Fahr.). After fifty, one hundred, two hundred, attacks of hystero-epilepsy, the temperature is the same as after one.

Outside of the convulsive accidents, adds M. Charcot, there are other characters which aid in the diagnosis. Epileptics are irascible and subject to impulses which render them dangerous; hystero-epileptics are capricious, fantastic, but altogether scarcely formidable.

The special vertigo, so frequent in epilepsy during the intervals of the attacks, is not observed in the hystero-epileptics.

The mental state, a fact already remarked by many authors, is seen to be far different in the two diseases. The hystero-epileptic preserves such of her original faculties as she originally possessed. A patient, for example, is, in this respect, to-day, the same as she has been this thirty years, in spite of the persistence of the disorder. The epileptic, on the contrary, from the time that the disease has obtained some intensity, is seen each day to fail in intelligence, and at the end of a longer or shorter time, he falls into a kind of stupor that commonly ends in a true dementia. These principal distinctive traits, says M. Charcot, already of utility to establish a nosographic demarcation, suffice, generally, to separate clinically two affections which differ fundamentally as regards prognosis.—*Progres Medical*, January 10.

Melancholia.—Dr. Leidesdorf, in his *Psychiatr. Centralblatt*, No. 2, gives the following abstract of the recently issued work of Dr. Krafft-Ebing on Melancholia (Erlangen, 1874):

The author sees, with many other authorities, in the symptoms of melancholic insanity, a simple emotional depression, a psychic pain, or neuralgia, which he considers as a neurosis of the sensorial centres of the cortical substance of the brain.

This condition of psychic pain reveals itself to the consciousness as a want of tone or temper, which is due, not to outer circumstances, but to inner causes. It produces painful ideas, to such an extent, finally, that even cheerful ideas and the revelations of the senses become sources of psychic discomfort; it becomes a psychic hyperæsthesia. The invalid is at last tormented with the idea that he no longer connects with his ideas the wonted feelings of pleasure or pain; that he has become devoid of feeling. This psychic anæsthesia may even drive him to desperation. By this painful consciousness of defects is constituted an anæsthesia dolorosa. This, developed to its full extent, the symptoms of which are well given by the author, without reaching the point of delusions and hallucinations, becomes the melancholic distemper—the "*Melancholia sine delirio*," a very common initial stage of insanity.

This melancholic disorder may be accompanied with precardial pain, a form under which the melancholia very often appears. The precardial pain, according to the author, is to be sought for in the cardiac nerves, consisting of the exciting fibres from the sympathetic, the inhibitory fibres of the vagus, and the automatic nerve system of the heart itself (comprised in the ganglia included in its substance).

What courses of these nerves and what processes of nervous action are involved in this symptom is uncertain. The author is inclined to consider it as a vaso-motor neurosis of the heart.

Finally, the melancholic disorder may be accompanied with delusions and hallucinations; as *melancholia cum delirio*. Of this form we can clinically distinguish, according to the different participation of the psycho-motor sphere, two sharply defined varieties, *melancholia passiva*, and *melancholia activa* or *agitans*.

For the explanation and the better understanding of these forms, which are described in detail, several clinical histories are given.

In regard to the anatomical and pathological alterations found in those who die while under this disorder, the author mentions the general lack of gross appearances in the brain and its envelopes, and points out that Leidesdorff, and later, Meynert, had proved the existence of decided cerebral anæmia. (The nutritive and formative disturbances, and also the chemical processes which follow this anæmia when of long duration, deserve an attentive study, and are certainly not less important than those due to hyperæmia. Ref.)

As regards prognosis, according to the author, the melancholia activa affords a better prospect for a cure than the passive form, since the condition of psycho-motor inhibition readily passes into imbecility.

As regards treatment, the author holds to the customary methods which hitherto have received most support. We find no mention of the use of morphia by injections, as recommended by Wolf, or of the employment of nitrite of amyl.

In describing the method of compulsory feeding, the author recommends the introduction of the œsophageal tube after the forcible opening of the mouth, and fixing the jaw, without recalling at all the method of feeding through the nostrils. Dr. Leidesdorff, however, is of the opinion that he published in the *Wiener Med. Wochenschrift*, sometime during the year 1866, a statement of the difficulties and disadvantages of the method of forcible feeding by the mouth, and, on the other hand, the invariable ease and harmlessness of the introduction of the nasal tube.

Cutaneous Neuroses Connected with Menstruation.—The *Gaz. des Hopitaux*, May 12, contains the following conclusions of a *brochure* by Dr. Dunlos, entitled *Etude sur la Menstruation, au point de vue de son Influence sur les Maladies Cutanees :*

"There exists a manifest sympathy between the utero-ovarian apparatus and the tegumentary system. This sympathy shows itself either by eruptions which coincide in their beginning with the catamenial epochs or with the development of uterine troubles, or by the disappearance of a morbid condition of the skin at the establishment of puberty or the cessation of dysmenorrheal disorders. To explain, we may say that the cutaneous affections have, by their urticarial mobility, by their hyperæsthesic (herpetoid), neuralgic (zona), or anæsthesic (leproid) characters, a certain analogy with nervous affections, and that, of all the organic systems of the economy, the nervous system is the one which receives the first *contre coup* from uterine disturbances. We are thus led to recognize reflex neuroses in the cutaneous manifestations allied to menstruation or its disorders. We may add that this sympathy is not evident in all patients, and even that we only observe it, at least to a pronounced degree, in a rather limited number of cases.

Pulmonary Sanguineous Exudation in the Insane.—Dr. Jehn, *Centralblatt f. d. Med. Wissensch.*, No. 22, states that in the *post-mortems* of five insane patients (one case of melancholia, one of mania, and three of paralysis), he found in both lungs extravasated clots of bright red arterial blood, irregularly but distinctly bordered, cutting with a dry, tough surface. The color was uniform, and microscopic examination showed them to be made up almost exclusively of red blood corpuscles. No alteration was detected in the tissue of the lungs and vessels.

The pathological discoveries in the central organs were partly negative, and in part showed old, and recent meningeal depositions, pachymeningitis hæmorrhagica, diffuse redness of single gyri, and, in two cases, capillary apoplexy of the cortex.

During life there were afforded no evidences of these pulmonary lesions found after death.

Syphilitic Nervous Affections.—The following are the concluding passages of Dr. W. H. Broadbent's series of Lettsomian lectures on syphilis as a cause of disease of the nervous system, *Brit. Med. Jour.*, March 7:

"A few words remain to be said on the diagnosis, prognosis, and treatment.

"*Diagnosis.*—The considerations involved in the diagnosis of syphilitic disease of the nervous system are too numerous and elaborate to be resumed in the time which remains at my disposal. We have, on the one hand, to guard against the conclusion that whatever happens in a person who has suffered from syphilis is necessarily due to this disease, and, on the other, to avoid being misled by the absence of an acknowledged syphilitic history or of traceable syphilitic antecedents. The period of life at which the nervous affection comes on is a great guide; in old persons—except in very obvious cases—we should arrive at a diagnosis of syphilis only after exclusion of other more common causes of disease of the nerve centres; in young adults syphilis would suggest itself early, unless there were heart disease or disease of the kidneys. The chief aids in the diagnosis, in addition to evidences of syphilitic disease in other parts, which must be carefully looked for, will be the antecedent or associated symptoms, which we have learned by experience to associate with syphilis: headache with nocturnal exacerbation, sleeplessness, and irritability. The gradual and irregular mode of access, except in the case of thrombosis, is again suggestive of syphilitic disease, and convulsions are very common.

"*Prognosis.*—In the prognosis we have always to bear in mind the liability to relapse. Occasionally we see recoveries which are apparently complete and permanent; frequently, I think, when the symptoms have been only epileptiform attacks, and the associated nervous disturbances enumerated in speaking of syphilitic epilepsy; sometimes when there has been evidence of graver mischief; but in a large proportion of the cases, the patients will enjoy immunity from similar or more serious symptoms, only on condition of perseverance in the employment of the remedies.

"The chief considerations which bear on the prognosis, are the duration, nature, and seat of the lesion.

"As to *duration*, the longer the mischief has existed, the more likely are its effects to be permanent; for, although syphilitic exudations and growths are singularly amenable to the influence of remedies, if they be allowed to remain for any length of time they destroy the structures in which they are lodged. This is more particularly important in the spinal cord, in which a very limited lesion will involve the entire segment, and cut off the part below from the cerebrum. It is, however, remarkable how much relief is often afforded, even after a prolonged train of disturbances, by removal of the cause, especially when the symptoms point to an affection of the cerebral hemispheres.

"As to the *nature* of the lesion, supposing it to have been determined that it is of syphilitic origin, the most important point is to distinguish between the effects of syphilitic disease in the membranes or nerve substance, and of thrombosis from syphilitic inflammation in the arteries. As Dr. Hughlings Jackson has often insisted, the results of blocking up of an artery will be independent of the nature of the obstruction. If a collateral supply of blood do not find its way to the part, softening is inevitable; and supposing that treatment could affect the original disease, as when a cerebral artery is included in a gumma, it would probably come too late to obviate the effects. Usually, as has been stated, thrombosis gives rise to sudden attacks without much pain.

"Syphilitic epilepsy, so-called, generally yields to treatment. We have here, in Dr. Jackson's language, only a 'discharging' lesion, not a destructive one. In paralysis, on the other hand, there is frequently destruction; but recovery may be expected if we can exclude thrombosis and softening, and if the duration have not been too prolonged. The tumors which give rise to unilateral convulsion appear to be particularly liable to be attended with optic neuritis, and may wear out the patient's strength; but tumors at any part can sometimes be brought to a state of quiescence, and the effects of disseminated lesions are more serious than those produced by a single growth.

"As to the seat of the lesion, I will only further add that growths from the dura mater are apparently less amenable to treatment than affections of the other membranes or of the nervous substance, probably because they are less easily reached, and less freely acted upon by remedies.

"*Treatment.*—The treatment is simple. The one remedy is iodide of potassium, or, this failing, mercury. I usually begin with doses of six grains, and always combine with it ammonia, the carbonate or aromatic spirit. Having, by one or two days' experience, ascertained that there is no special intolerance of the iodide, it may be rapidly pushed to doses of 12, 18, 24, 30, or even 36 grains three times a day. Occasionally, even larger doses are necessary, and I have given a drachm every four hours. That large doses are often absolutely required, and that they succeed when moderate doses fail, I am convinced by abundant experience; and if iodism be induced, which is very rarely the case in tertiary syphilis, it is almost always before large doses are reached. Large doses are better borne when taken after meals. Of course iodide of potassium is more quickly taken up into the blood from an empty stomach, but it is also quickly out of the blood and in the urine; and when a continuous action on the system is needed, which is what we require in dealing with the effects of tertiary syphilis, the indication is best met by giving so diffusible a remedy as the iodide of potassium after food.

"If the iodide of potassium fails, after a full and fair trial, a resort to mercury is always desirable; and, the more recent the syphilis, the earlier should this be made. In passing from the use of one to the other drug, either a certain interval should be allowed to elapse, or the mercury, if given by the mouth, should be in one of its most soluble and active forms—the bichloride or biniodide. More than once I have seen sudden and profuse salivation where this precaution has been neglected—no doubt from the mercury being converted into the biniodide within the system. Sometimes I have employed mercurial inunction at the same time with the internal administration of iodide of potassium, and have frequently given the biniodide of mercury with iodide of potassium, either in the same mixture or in the form of a pill at night.

"One word as to the *modus operandi* of iodide of potassium. This was the subject of a beautiful explanation by Dr. Odling in his Goulstonian lectures before the College of Physicians—hypothetical at that time, but demonstrated by experiment since. The active agent is the iodide, as is shown by the fact that other salts of potassium have not the same effect, while other combinations of iodide, such as iodide of ammonia or sodium, have. The iodide is permitted to exercise its influence on the seat of disease, in virtue of the comparatively slight affinity by which it is held in union with the base; this being so feeble that, in the presence of certain forms of living protoplasm in active change, the salt is decomposed and the iodide set free to exercise its solvent action on the organic matters. Whether this be direct, or indirect, through the well-known oxidizing effects of free iodine, is not so certain."

RABIES MEPHITICA.—Rev. H. C. Hovey, *Silliman's Jour.*, May, 1874, proposes this name for a form of disease, apparently before unnoticed, closely resembling, if not a variety of, ordinary hydrophobia. It is, according to him, produced by the bite of the common skunk, and one or two allied species; but he suggests the idea that it may be only in an abnormal condition, when the natural defensive secretion is suppressed, that its bite is dangerous. The usual opinion among hunters is stated to be the contrary. Mr. Hovey narrated several instances of the disease, and adduces the testimony of several medical men, army surgeons and others, as to its existence.

The principal differences between this form and the ordinary rabies, are stated to be the absence of all constitutional symptoms during the period of incubation; the lack of local lesions (pustules) in the mouth; the usual symptoms of the laryngeal and pharyngeal regions are often wanting; the cutaneous hyperæsthesia is not present; the perceptions are blunted, and the convulsions finally produce delirium and coma, ending in death. In both the mode of death is by asthenia, but in the new form relief is obtained by the administration of anodynes, morphia, etc. As far as could be ascertained the disease was invariably fatal, no cases of recovery having been recorded.

c.—THERAPEUTICS OF THE NERVOUS SYSTEM AND MIND.

Aconitin and Pseudaconitin.—Constantin Ewers, *Diss.* Dorpat, (abstr. in *Deutsche Klinik*, March 21,) has studied the physiological action of the pseudaconitin obtained from *aconitum ferox* by experiments on frogs, rabbits, and cats, and finds a great correspondence with the effects of aconitin, derived from *aconitum napellus*, on the heart, and respiratory and nervous systems; although there are unmistakable quantitative differences. For example, the irritability of the motor nerves of the frog is destroyed much sooner by poisoning with *aconitum ferox* than by the other. The primary central vagus excitation was, moreover, not observed, as a rule, with the aconitum napellus, but there appeared a noticeable slowing of the pulse (without lowering, or with increase of blood pressure), which is never seen with aconitum ferox.

A characteristic difference between aconitin and pseudaconitin, which, moreover, has been already observed by Schroff, consists in its local action, as pseudaconitin applied externally causes not only a burning sensation and pain, but also reduces the sensations of taste and temperature. Ewers has also proved the much greater degree of toxic property of pseudaconitin, since 0.5 mgm. killed rabbits and cats in from five to ten minutes, while, to produce the same effect, at least ten milligrammes of aconite are necessary.

The researches on the effect of these two agents on the respiration are of especial interest, since both produce dyspnœa, and intoxication by them can be recognized by this symptom. This is relieved by section of the vagus (sometimes of only one side), and also by atropine, which may therefore be regarded as the antidote against aconitin and pseudaconitin poisoning. Another peculiar fact is that aconitin causes a destruction of the nervous irritability in Rana temporaria, but not in Rana esculenta.

Bromide of Potassium.—Dr. Huchard, in his *Revue Therapeutique* in *L' Union Medicale*, February 28, after discussing the use of the bromide in intermittents, speaks thus highly of its efficacy in various forms of cephalalgia:

We will not drop the subject without calling the attention of practitioners to the good effects we have always obtained by the treatment of hemicranias of anæmic origin, or diverse cephalalgias, by the bromide of potassium. Without being acquainted with the memoir of M. Barudel (*Mem. de Med. Militaire*, 1867) on the efficacy of the treatment of migraine, due to anæmia or chlorosis, by the bromide, we have for some time employed this precious medicament with the best success in hemicrania and in headaches of different kinds. We should equally remark in this connection, that the bromide has the same power against headaches of different origin; against those which happen in plethoric, hemorrhagic, or gouty individuals; those which accompany vaso-motor troubles or facial congestion, and those which so often attack anæmic persons. How can we explain the identical action of the medicine in these two so opposed conditions, otherwise than by admitting that in both the cephalalgia is congestive, as we have long believed, and that the local congestion may be due to either the excitation of the dilator vaso-motor nerves, or a paralysis of the constrictor ones? The success obtained in these cases by the bromide of potassium tends, therefore, to justify the adage, so well known and so true: *Naturam morborum ostendunt curationes.*

Chloral.—Dr. Oscar Liebreich has recently published a paper in the *Berliner Klin. Wochenschr.*, in which he calls attention to the important subject of the purity of chloral hydrate, and the effect which its deterioration may produce on the patients to whom it is administered, and on its reputation as a remedy. The case, he says, is different from that of such a substance as quinine, the adulteration of which will only reduce, but will not pervert, the proper action of the drug. With chloral and other substances prepared by analogous chemical processes, the result of the manufacture may be the formation of compounds which, if administered, produce an altogether different result from that intended. The process of manufacture is one which requires great care, and it seems that it is at least difficult to ensure the purity of chloral if made in large quantities. Liebig himself, who discovered it, never attempted to make more than a few grammes at once; and Dr. Liebreich was so convinced, when he brought it into notice as a medicinal agent, that purity was necessary for success, that the first supplies were made under his immediate superintendence. At present it is manufactured in various places, and the result is, that in some parts of the continent, notably in Saxony and Switzerland, it has fallen into disrepute. Dr. Liebreich has made a collection of specimens of the drug used in cases where it has failed to produce its proper action, and possesses, he says, some horrible chemical compounds which he would not venture to give to a human being. He prefers the crystallized form of chloral hydrate as the most stable. It may contain hydrochloric acid; this is no disadvantage if the proportion remain the same; but if it increase, it indicates that the formation of dangerous compounds may be going on. Sometimes the hypnotic action is increased; this he attributes to the production of chlorine compounds, which are more readily changed into chloroform than chloral itself is. An acid reaction, arising from the formation of trichloracetic acid, does not show that the chloral is unfit for use, though it weakens its action. In pure chloral, this action is limited, while impure chloral is liable to the constantly increasing production of acid compounds—not trichloracetic acid—of a deleterious nature. Dr. Liebreich remarks that the German *Pharmacopœia* is in error in fixing the boiling point of chloral hydrate at 95° Cent. (203° Fahr.) This, he says, is correct for anhydrous chloral; but the boiling point of chloral hydrate is not constant.—*Brit. Med. Jour.*, March 21, 1874.

J. Pollak, (*Wiener Med. Wochenschr.*) *Allg. Med. Central Zeitung*, March 7, thus sums up his conclusions in regard to this agent:

1. Chloral is a very good hypnotic; that on account of its soporific effect it is a suitable sedative in all those diseases which consist in an abnormal excitation of the brain or are accompanied with this condition.

2. As it produces sleep, it relieves pain during the sleep, but it has no power to cause a cessation of pain except by the production of sleep, and in cases of very intense pain it has very little hypnotic action. In such cases it may be advantageously combined with morphia.

3. It possesses the power to cause muscular relaxation; of the muscles as a whole as well as single ones; of the voluntary as well as the involuntary, and is an excellent remedy for the various convulsive affections.

4. In affections of the heart and lungs, and of the digestive tract, it is partly inert, and in part productive of unpleasant or even dangerous consequences, and therefore, in such disorders, is either contraindicated or should be employed only with the greatest caution.

5. In general it is not an anæsthetic of sufficient power to be employed in the greater operations.

6. In most cases, even when used for a long time, it has no unpleasant effects, and if such symptoms appear they are without significance; at any rate, it produces no cerebral congestion or disturbance of digestion or nutrition.

7. In most disorders in which it is employed, it is an excellent palliative, but exerts scarcely any influence on the disease itself. We can also say, in short, that chloral is indicated whenever morphia is indicated, and where we cannot or will not give the latter on account of its special unpleasant after effects. It is contraindicated in diseases of the heart, lungs, and digestive organs.

If we compare chloral with morphia and chloroform, we may put down as decided the following:

1. As a soporific it acts with more certainty and more pleasantness than morphia, and as a hypnotic it should and will supplant the latter.

2. It only relieves pain as it produces sleep, but not without sleep, and, as it is unreliable in very severe pain, it cannot completely take the place of morphia as a pain-relieving agent.

3. As an anæsthetic it cannot supply the place of chloroform, as the latter surpasses it in the quickness and intensity of its action, besides being unlimited in its effects.

Injections of Chloral into the Veins.—A note of M. Ore; presented by M. Bouillaud at the session of the *Acad. de Sciences*, Paris, March 2, (reported in *Gaz. Medicale*, March 28,) related a case of traumatic tetanus cured by intravenous injections of chloral. Three conclusions are drawn by the author from this case:

1. "*The Harmlessness of Intravenous Injections of Chloral.*—We have not observed," says he, "the slightest trace of phlebitis in this patient; there was

a slight abcess of a peculiar kind, produced in the lower region of the right fore-arm, on account of the penetration of the chloral into the cellular tissue.

"My experiments on four animals show me that this is always the case when the dose is considerable. This conducts us to the important conclusion to the clinicist, that the subcutaneous method of the administration of chloral is the most defective. If the quantity injected is slight, it is absorbed without producing any local accidents, and also without producing any general effect on the organism. If, on the other hand, it is large, it causes an abscess ; in the first case it is useless ; in the second, injurious.

2. "A second conclusion is drawn from the absolute insensibility, so rapid and so lasting, when this substance is brought *into immediate contact with the blood.* I have already spoken on this point, and will not return to it.

3. "Hydrate of chloral given by the veins, quickly overcomes the tetanic accidents. Three injections of ten grammes, repeated three days at twenty-four hours' interval, caused, with sleep, *a complete paralysis of sensibility and motility.*

"But that which is particularly worthy of mention, and on which I specially insist, is *the small quantity of chloral* needful to be employed to produce a favorable result. It has been objected to the method of intravenous injections, 'that tetanus cured by this agent lasts, on an average, twenty-five days, and that to keep the patient in a state of narcosis, we have to give the chloral five or six times a day; and that it is impracticable to make five or six injections a day for twenty-five days consecutively.' In making this objection, note was not taken of the simple fact that by its direct penetration into the vessels, the physiological action of the chloral is in some way increased tenfold, and that its effects on the system are, in this case, more rapid, more certain, and more durable.

"In order to cure tetanus it is not necessary to keep the patient narcotized for twenty-five days, but to *reduce* the reflex power of the cord, exaggerated by the morbid condition, promptly to its physiological condition, and to prevent, in consequence, the muscular contraction from becoming general. This is what chloral does, *but only when it is administered by way of the veins.*

"In order that chloral, injected into the veins, may hinder the tetanic symptoms, the dose administered should be large enough to *paralyze almost immediately the reflex action of the cord, and to cause momentarily a complete paralysis of movement and of sensibility.* The dose of ten grammes at each injection, appears to me sufficient to produce this result."

Cryptopin.—Immanuel Munk, *Versuche ueber die Wirkung des Cryptopins.* Diss. Berlin (abstr. in *Deutsche Klinik*, March 21), has examined this product of opium which was first isolated by T. and H. Smith, and not by Hesse, as seems to be his idea. He has experimented on frogs and rabbits, and the chief results of his research may be stated as follows:

1. In the frog, medium doses of cryptopin caused a perceptible slowing

of the heart's movements, while by large doses it was completely stilled. The stoppage of the heart is due to a paralysis of its muscular substance.

2. Death produced by large doses in the warm-blooded animals is to be considered as due to stoppage of respiration.

3. Cryptopin reduces the irritability of the nerve centres, and in large doses may paralyze them entirely; hence, (*a*) the centre for respiration, and in consequence the stoppage of that function, (*b*) the spinal centres, whence the extinction of reflex movements and disturbances of motility.

4 In warm-blooded animals, also, the cardiac pulsations are independent of the frequency of the movements of respiration. If, by artificial respiration, death by asphyxia is avoided, we may be able to bring the heart to a stop by the employment of very large doses.

5. In warm-blooded animals, also, the stoppage of the respiration is always accompanied with severe convulsions, which are undoubtedly to be considered as due to suffocation, since where artificial respiration is carefully kept up they never appear.

Codeia.—A. S. Myrtle, M.D., *Brit. Med. Journal*, April 1, describes a case of codeia poisoning which occurred in his own practice. The drug was prescribed in four grain doses for the relief of a probably transient glycosuria, dependent on nervous disturbance. The first dose sufficed to produce all the symptoms, which lasted nearly thirty-six hours, before the patient fully recovered from the effects of the medicine. Dr. Myrtle concludes as follows:

"The symptoms I observed led me to infer that, as a therapeutic agent, it forms a connecting link between morphia and nicotine. First, we had a short period of decided vascular excitement, with consequent increased nervous activity and power, common to both; then followed a period of depression, with symptoms equally common to both—dimness of sight, nausea and sickness, cold, clammy skin, muscular debility, and a manifest and persistent "slowing" of the vital functions. On the other hand, we had no stupor; not even drowsiness; no headache, as with opium; no vomiting; no increased alvine, vesical, or salivary secretion, as with tobacco. In thirty-six hours the patient had completely lost all his disagreeable feelings, and was free from all signs of having been poisoned by codeia."

In commenting on the above in the issue of the same journal of May 2, S. W. Moore calls attention to the distinctness of the crystalline form and chemical composition of morphia and codeia, which serve to distinguish the two substances when pure. He adds, "experiments which have been made on the physiological action of codeia tend to show various results. These, in some measure, are due to the experiments having been conducted on animals of different species, genera and classes. Some, and those most reliable, have been on man; and, as it is for human benefit that we prosecute these researches, it would be better if further secondary experiments were always so conducted.

"Thus, Anderson says, the salts of codeia have a powerful narcotic influence when taken internally. In Dr. Myrtle's case we have not even drowsiness, so that it is possible that Anderson did not use the pure salt.

"Dr. Stocker injected some cats subcutaneously with hydrochlorate of codeia. In each instance, the pupils were dilated; cerebral congestion was present, as determined by ophthalmoscopic examination; there was much reflex excitability; in one case, epileptic convulsions; salivation and purging occurred in two cases; there was no vomiting. The dose used was about a grain and a half.

"In the case quoted, there are several points of disagreement with these results, which are probably due to the difference in the animals—one a man, the other a cat. Thus, the cat's pupils were dilated, the man's contracted; and, while there was no increase of alvine or salivary secretions in the man, two of the cats were salivated and purged."

Electricity.—Dr. Hermann Munk, *Reichert u. Du Bois Reymond's Archiv,* 1873, 505, publishes an account of experiments carried on by himself, with his brother, Immanuel, as to the electrical introduction of various medicinal substances in the fluid form into the uninjured organism. They succeeded in poisoning rabbits with a strychnia solution introduced through the unwounded skin, by means of a rather powerful galvanic current applied through moist clay electrodes from one-half to three-quarters of an hour. In the human subject, the urine was found to exhibit the reaction of the substances introduced, after only fifteen minutes duration of the current. Of course, only such substances as act powerfully on the system in very small doses can ever be utilized as medicines in this manner.

Galvanization of the Sympathetic.—Glax (*Pester Med. Clin. Presse* and *Med. Chir. Rundschau*, March, 1874) states that in thirty cases of abdominal typhus under his care, whenever the temperature in the axilla reached 102.2 Fahr., he placed the positive pole of a constant battery of twenty elements on the third cervical vertebra, and the negative pole on the "upper cervical ganglion of the sympathetic nerve," and found "in nearly all cases a remarkable fall of temperature in the course of some hours." Glax believes that in this way the temperature may be reduced.—*Brit. Med. Journal*, April 1.

Ergot.—Dr. H. C. Wood, *Phila. Med. Times*, May 16, 1874, publishes his experiments on the vaso-motor action of ergot. The aim was to decide whether the contraction of the minute blood-vessels produced by it is due to its action on the nerve centres, or to a direct action on the blood-vessels.

The results of the direct injection of ergotin into the femoral vein, were an immediate, almost instantaneous, fall of pressure, and subsequently an enormous increase; also, when extremely large doses are employed, the arterial pressure is permanently lowered. In order to determine whether this first fall of blood pressure is due, as Dr. Holmes asserts, to a spasm of the pulmonic capillaries, on account of the intense direct action of the drug upon them, or not, Dr. Wood experimented by first cutting the upper spinal cord of dogs, in order to cause paralysis of the vaso-motor nerves, and injected the ergotin. If the action was, as Dr. Holmes supposed, entirely peripheral, this operation ought to be of no effect.

The result of these last experiments, which are given in detail, proved that ergot was ineffective to raise the arterial pressure after the paralysis of the vaso-motors, thus showing that the vaso-motor spasm was entirely of centric origin.

As regards the real cause of the primary fall, Dr. Wood says it is due to a sudden precipitation of an overwhelming dose on something, since he has often demonstrated that it does not occur when the drug is merely injected into the subcutaneous tissue. The experiments of Eberty indicate that ergot, in sufficient doses, is a muscle poison to the heart, and this conclusion is borne out by the final result of one of Dr. Wood's experiments, when, after the movement of the mercury and all arterial pressure were at an end, the respiration still continued with fair regularity, and, the chest being opened, the heart was found not pulsating, but in a state of active vermicular movement. From this he considered it almost certain that the sudden fall is due to the action of the dose on the heart, and the subsequent rise follows when the poison becomes diffused through the circulation. The fact that this fall is in no way dependent on the nerve centres, but happens equally after the paralysis of the vaso-motors, serves to confirm this view.

Ergot was also injected directly into the carotids, in order to determine whether its immediate action on the nerve centres could be detected; that is, whether thus acting it would produce a rise by its influence on the brain, followed by the fall as soon as the heart could feel its action. In each experiment this was observed, but on account of the complications of the question Dr. Wood does not consider the results conclusive.

GUARANA.—Dr. Dasara Cao, *Gaz. Med. Ital.*, No. 50, 1873, (abstr. in *Allg. Med. Central Zeitung*, February 18,) gives the conclusions he has arrived at in regard to the efficacy of this drug in cases of hemicrania, particularly in females. He finds that an effect is produced if it is taken at the commencement of the attack, but fails if administered later. At the beginning of the hemicrania he directs half a gramme (about seven grains) of the medicine to be taken every quarter of an hour, if the pain continues.

In many cases the author observed a mildening, and longer intervals to occur between the attacks. Instead of guarana he more often ordered guaranin, and as this was not always obtainable, he prescribed caffeine, which, in coffee, was always on hand, and which, in chemical composition, approaches guarana, and he gives the preference to the raw coffee over the roasted, which contains less caffeine. But, if the decoction of raw coffee disagree with the patient, he recommended a cup of strong roasted coffee.

Guarana, as well as guaranin, caffeine and coffee, are only to be regarded as palliatives. Still, it is a gain to science and to the sick, if a means is supplied that can hinder and reduce the intensity of the pain in this affection.

GELSEMINUM.—Dr. J. Sawyer, *Brit. Med. Jour.*, May 2, calls attention to the value of the tincture of gelseminum in the treatment of certain forms of odontalgia. He used a tincture made by macerating two ounces of the root in a pint of rectified spirits, given in doses of fifteen minims every six hours. The cases for which he especially recommends it are those forms of neuralgic pain associated with carious teeth, but unconnected with any evident local inflammatory changes. Out of about twenty cases thus treated, in only three or four was the pain not decidedly and lastingly relieved. In most cases, this result was reached with the third or fourth dose.

Edward Mackey, M.B., in the same issue of the *Journal*, gives a case of trifacial neuralgia relieved by this drug. The preparation used is the same as that of Dr. Sawyer, and the dose advised is from five to twenty drops. He says: "My note-book contains now many similar cases, which warrant me in adding my testimony to that of Dr. Legg, and to that of my friend and colleague, Dr. Sawyer. I have ample evidence of the power of gelseminum to relieve pain, especially—I do not say only—in the branches of the fifth nerve; and medicines that relieve pain are the most valuable we can have. * * As the 'therapeutic' corner of our *Journal* is now and usefully occupied concerning tetanus, I will point out that gelseminum, from its paralyzing power, ought, *a priori*, to be of use in that malady."

MORPHIA.—Dr. T. M. B. Cross, in a recent paper, recommends very highly deep injections of morphia, for the cure of sciatica not depending on a specific cause. Three or four or more drops of Magendie's solution, should be injected with a hypodermic syringe, into the nerve or its sheath. Sometimes one application, and at most, a very few, will effect a radical cure.

W

MUSCARINE.—At the meeting of the *Soc. de Biologie*, Apr. 25, M. Prevost narrated the result of his researches on a new poison, recently studied in Germany by Schmidberg, under the name of *muscarine*, and extracted from the false mushroom (*Agaricus muscarius*, or *Ammanita muscaria*). "This new toxic compound does not appear to belong to the class of alkaloids; it seems, rather, to be ranked among the glycosides, bodies characterized, as we are aware, by the property of dividing, by fixation of water, into numerous products, in which we find glycose, or saccharated matter. Muscarine, in a rather high dose, (one milligramme in dogs and rabbits,) causes a complete arrest of the heart in diastole; the auricles are lax and dilated like the ventricles. This arrest in the diastole seems to be due to the excitation of the nerves of arrest of the heart, and not at all to any action whatever on the smooth muscular fibres of the walls of that organ. As Schmidberg has already shown, the heart, in fact, remains excitable after the arrest of its pulsations. Atropine, which paralyzes the inhibitory nerves of the heart, is the antagonist of muscarine. Eserine possesses the same property; curare, morphine, and strychnine, produce no effect.

"The lymphatic hearts undergo no change from the introduction of muscarine. They continue to pulsate; they permit the absorption of the atropine when it is injected, after the arrest of the heart. Besides this influence on the central organ of the circulation, muscarine singularly exaggerates certain of the secretions. Following the absorption of some milligrammes, we observe lachrymation and a considerable salivation. After the section of the lingual and the chorda tympani, and the degeneration of these nerves, the same effects are observed; there is, therefore, a direct action on the salivary glands, and probably an excitation of the minute nerve centres scattered in profusion throughout these glands. Atropine causes this secretory activity to cease. Why, as M. Carville asks, has clinical experience never, in cases of poisoning by mushrooms, verified these symptoms of the exaggeration of the lachrymal and salivary secretions?"

NERVE STRETCHING.—Mr. Callender read before the *Clinical Society of London*, Apr. 10, reported in *Brit. Med. Jour.*, the notes of two cases of neuralgia. In the first, the affection, which involved a stump, seemed to be due to neuritis, connected with symptoms of spinal irritation. Several operations had already been performed, but without relief; and, finally, the median nerve was violently stretched, pulling it down from the brachial plexus. No local trouble followed; the pain and spinal symptoms left completely. In the discussion which followed, Mr. Callender alluded to a somewhat similar case described by Billroth, and stated several other instances of nerve stretching.

PHOSPHORUS.—J. Ashburton Thompson, *Med. Times and Gazette*, Feb. 28, and Mar. 21, gives a paper on the medicinal dose of free phosphorus. After reviewing the subject historically, and quoting numerous cases in which various doses were followed by disastrous consequences, he discusses its method of action upon the system, favoring the view that it acts thus in a state of purity, *i. e.*, in its free state, and draws the following general conclusions: "Firstly, that the more perfect the state of reduction in which phosphorus is ingested, the more actively will it exhibit its powers. The most perfect reduction is obtained by solution. Secondly, that the introduction of free phosphorus to the circulation is the first object to be attained in attempting to procure the characteristic effects of this drug. Thirdly, that the better adapted the solvent is to protect the phosphorus from the action of the oxygen in, or the fluids of, the stomach, the more free phosphorus will enter the circulation. Oil is the solvent which best fulfills these conditions.

The author advises the use of cod-liver oil as a menstruum, instead of olive oil, and claims that it is safer and equally efficient. If, however, the solution in olive oil is used, the dose should probably never exceed one-fortieth of a grain, given twice daily, and watched with the greatest care. Solutions in ether, chloroform and alcohol, are distrusted as tending to rapid oxidation, and consequent inertness of the metalloid. Solid phosphorus should only be given when perfectly reduced (pulverized), a condition which it is difficult to obtain, and then the dose should not exceed one thirty-second of a grain three times a day, immediately after meals. Phosphide of zinc is recommended as a convenient and safe means of procuring the therapeutic effects of free phosphorus. The most active dose is two-thirds of a grain repeated every four hours, but as this sometimes causes inconvenient nausea and vomiting, one-third every two hours is preferable. It may also be given in powders, which is the best form for its administration to children. The author has given one-sixth of a grain every four hours to children of all ages, in acute diseases or in desperate exhaustion, with the apparent result of carrying them over the critical period.

The dose of phosphorus varies, also, with the end to be obtained, as well as with the preparation used. In neuralgia and exhaustion the full dose should be employed. The following are the full doses of the various preparations, as deduced from Dr. T.'s observations: Of phosphorized oil, one-fortieth of a grain twice a day; of solution in ether, chloroform or alcohol, one-twelfth of a grain every four hours; of solid phosphorus, one-thirty-second of a grain three times a day; of phosphide of zinc, one-third of a grain every two hours. But in hysteria or epilepsy, small doses will answer every purpose, and in these cases phosphide of zinc or tincture of phosphorus should be chosen. The rule of continental physicians, of intermitting the administration of phosphorus every fourteen days for a space of seven days, is approved as judicious, but the author does not usually find it necessary to continue its administration for so long a period, as whatever benefit it can produce is evident before that time.

In concluding, he calls attention to the peculiar toxic properties of this agent. It is one of the most insidious known: under certain circumstances its administration may appear beneficial, until all at once it manifests its poisonous effects with explosive suddenness.

Spartein.—J. Fick, *Archiv f. exper. Pathol.* t. 1, 1873 (abstr. by V. Hanot in *Rev. des. Sci. Medicales*).

"Fourteen experiments have led the author to the following conclusions:

1. "In frogs, as well as in mammals, spartein diminishes the cerebral activity, and may, therefore, be considered as a narcotic. But in this respect its action is very slight, and no matter how high the dose, it never causes an entire loss of consciousness.

2. "Spartein notably diminishes the reflex excitability of the cord.

3. "Spartein paralyzes the motor nerves; they lose their electric excitability by a large dose.

4. "In a little time, and with a small dose, spartein destroys the electric excitability of the vagus nerve, in such a way that its excitation does not at all diminish the heart-beats; in large doses it even paralyzes the centres of arrest, in such a way that neither the excitation of the auricles nor the muscarine, produces any diastolic stoppage of the heart.

5. "Spartein appears to kill mammals, by paralyzing the respiratory centres. We can, therefore, keep up vitality in animals poisoned by it, by artificial respiration."

THE FOLLOWING FOREIGN PERIODICALS HAVE BEEN RECEIVED SINCE OUR LAST ISSUE.

COMPLETE *files of the more important, especially among those bearing directly on the subjects embraced in the plan of the* JOURNAL, *are on hand:*

Allgemeine Medicinische Central-Zeitung.
Allgemeine Zeitschrift fur Psychiatrie und Psychisch. Gerichtl. Medicin.
Annales Medico-Psychologiques.
Archiv fur Anatomie, Physiologie, und Wissenschaftl. Medicin.
Archiv fur Path. Anatomie, Physiologie, und fur Klin. Medicin.
Archiv fur die Gesammte Physiologie der Menschen und Thiere.
Archiv der Heilkunde.
Archiv fur Psychiatrie.
Archives Generales de Medicine.
Archives de Physiologie, Normale et Pathologique.
Archivio Italiano, per le Malattie Nervosi.
Berliner Klinische Wochenschrift.
British Medical Journal.
British and Foreign Medico-Chirurgical Review.
Bulletin Generale de Therapeutique.
Centralblatt f. d. Med. Wissenschaften.
Der Irrenfreund.
Deutsches Archiv f. Klinisches Medicin.
Deutsche Klinik.
Dublin Journal of Medical Sciences.
Edinburgh Medical Journal.
Friedrich's Blaetter f. Gerichtl. Medicin.
Gazette Medicale de Paris.
Gazette des Hopitaux.
Jahrbuch f. Kinderheilkunde u. Physische Erziehung.
Jahresbericht u. d. Leistungen u. Fortschritte in der Gesammt. Medicin.
Journal of Anatomy and Physiology.
Journal de l'Anatomie, et de Physiologie, etc.
Journal of Mental Science.
Lo Sperimentale.
L'Union Medicale.
Medicinisches Correspondenzblatt des Wurtemb. Aerztl. Vereins.
Medicinisches Jahrbucher.
La Nuova Liguria Medica.
Progres Medicale.

Psychiatrisches Centralblatt.
Rivista Clinica.
Revue des Sciences Medicales.
Revue Scientifique.
Schmidt's Jahrbucher der In- und Auslandischen Gesammten Medicin.
The Practitioner.
Vierteljahreschrift fur die Prakt. Heilkunde.
Wiener Medicinische Press.
Zeitschrift f. Biologie.

The following domestic exchanges have been received:

American Journal of Insanity.
American Journal of Medical Sciences.
American Journal of Pharmacy.
American Medical Weekly.
American Practitioner.
Atlanta Medical and Surgical Journal.
Boston Medical and Surgical Journal.
Canada Medical Record.
Chicago Medical Journal.
Clinic.
Cincinnati Lancet and Observer.
Cincinnati Medical Times.
Detroit Review of Medicine and Pharmacy.
Indiana Journal of Medicine.
Medical Examiner.
Medical Herald.
Medical News and Library.
Medical Record.
Medical and Surgical Reporter.
New York Medical Journal.
Pennsylvania Journal of Medicine.
Pacific Medical and Surgical Journal.
Philadelphia Medical Times.
Richmond and Louisville Medical Journal.
Sanitarian.
St. Louis Medical and Surgical Journal.
U. S. Medical and Surgical Journal.
Virginia Medical Monthly.

BOOKS, ETC., RECEIVED.

NOTE.—The foreign works in this list may be obtained through Messrs. B. Westermann & Co., No. 524 Broadway, New York.

Dictionnaire Encyclopedique des Sciences Medicales, etc. Directeur A. Dechambre. *Premiere serie.* Tome Quinzieme, Deuxieme Partie. Cha-Cha. *Deuxieme serie.* Tome Huitieme. Moe-Moe. *Troisieme serie.* Tome Deuxieme Premiere Partie. Rad-Rat. Paris: 1874.

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Original Articles, Selections and Translations.

Art. I.—PATHOLOGY OF THE VASO-MOTOR NERVOUS SYSTEM.

Lecture IV.

Recapitulation of former Lecture—Irritation—Its nature and relations to Congestion—Irritants—Modes of producing irritation—Irritation produced by disease of nerves and nerve centres—Proofs of it—Zona as an instance of—How produced—Mode of producing congestion by nervous irritation—Other examples besides zona—Trophic nerves—Do they exist?—Trophic nervous action—It does exist—What nerves are its channel—Objections to nervous trophic action considered—Practical conclusions.

GENTLEMEN: In the last lecture I stated, first of all, some of the phenomena of active congestion; and then mentioned, and briefly discussed, the more common modes of explaining the same. They were as follows: The phenomena of active congestion may be explained—

1. By supposing a peculiar attraction to exist between the blood and tissues of the body, which may be so altered in disease as to become the cause, either directly or indirectly, of the presence of an abnormal quantity of blood in the vessels of a part.

2. By supposing some physical obstruction, either within or without the vessels which are the seat of the congestion, and which may be the cause of the unnatural accumulation of blood in them that we call active congestion.

3. By supposing the muscular walls of the congested vessels to be in some way deprived of their nervous influence, so as to become paralyzed, and hence relaxed so as to admit more blood than they should normally contain. Or, finally,

4. By supposing not only a relaxation of the muscular coats of the congested vessels, but by supposing them to have suffered a loss or perversion of peristaltic action, by which means the blood-current, according to the circumstances of the case, is supposed to be either retarded or quickened, or reversed, or even arrested; or, if arrested, sometimes set in motion again, after a variable period of time.

That the muscular vessels have a peristaltic or vermicular action, we saw was admitted by very few physiologists, and some time was spent in citing the proofs, such as they are, which show that such a kind of action is probable, and, in some cases, does exist; and the belief was expressed, with some reserve, that, whatever share other agencies may have in producing active congestion, we ought to admit peristaltic action as one of them, until farther definite observations shall finally settle the case.

I endeavored to draw a distinction between passive congestions, such as follow division of the sympathetic nerve of the neck, and active congestions, such as seem to follow *irritation* of the chorda tympani nerve, as regards the submaxillary gland, to which gland the chorda tympani is distributed.

After discussing the *actions* or *motions* of the vessels, as observed in active congestion, I began the inquiry as to what share, if any, the nervous system has in producing the kind of congestion to which I have referred. It is my purpose to try to answer this question to-day.

There is one point included in active congestion I must now call to your attention. I purposely omitted to mention it in my last lecture. It is *irritation.* It is impossible to have an active congestion, or an inflammation, without *irritation*; and, on the other hand, I may say it seems equally impossible to maintain a part, that is supplied with vessels and nerves, in a state of irritation for any considerable length of time without having an active congestion. *Ubi irritatio, ibi fluxus.*

If it were possible for me to define clearly what this term means, it would, perhaps, be unnecessary. You all know I shall assume what is meant when we say a part is irritated, though you might be much troubled to say just what is the precise nature and order of the phenomena that make up the state or process.

It is impossible to have the *effects* we collectively term irritation, without some adequate *cause.* That cause we call an *irritant.* We have, to say the least, two classes of irritants —mechanical and chemical. Any of you can name a list of them, I suppose. When applied to the living tissues supplied with blood vessels and nerves, they immediately change the circulation and nutrition of the part on which they act in a remarkable manner. The degree, or even the particular way, in which this is done, depends on the character of the irritant. But if applied mildly, the effects are—taken altogether—to increase the circulation of blood and the activity of nutritional change in the part. This is evidenced in many ways and degrees. If the action of the irritant is intense, not only is the circulation of blood profoundly changed, but the nutritional activity, or even the immediate structure of the part to which the irritant is applied.

Such irritations, viz.: those that are immediately produced by palpable mechanical or chemical agencies, are met with everywhere, even in daily life. They are the common staple of the experience of the physician and surgeon. In most such cases we are able to point out the actual and efficient cause of the irritation, and, by consequence, of the congestion and other phenomena which are the usual accompaniments or consequences of the state in question. But it is not of this class of irritations I desire to speak at this time. I wish to speak of those cases of irritation with which, to say the least, it has not been possible to connect the immediate action of any known mechanical or chemical irritant whatever, whether external or internal.

To come at once to the point, I wish to speak of those irritations, the immediate cause of which seems to lie in the nerves which lead to the irritated part. In that class of irritations, there seems to be no other channel through which the irritative influence can be conveyed.

I must now show you that an irritant influence may be conveyed to, or exercised on, a part by the channel of its nerves. I expect to show you, also, that there is reason to think that irritations produced in this way are far more frequent than is commonly suspected.

To show that local irritations may be produced in this way, is *almost* the same as to show that active congestions are produced in this way. Hence you can see the importance of our present inquiry. If active congestion does not arise simultaneously with the irritation, it follows so closely in its wake that we cannot make a practical distinction as to time in the occurrence of the events.

There is one point that irritation in a part always seems to include. That is, increased if not abnormal tissue change; certainly always the former, and perhaps always the latter. This increased tissue change may be regarded as implying more material: as one of the factors in the process; in other words, more blood. We have thus two questions opened up for discussion:

1. The fact and mode of producing irritation in a part through the agency of its nerves.

2. The way in which the irritation, when once established, leads to a corresponding increase of blood in the affected part; or, in other words, how the irritation causes the accompanying, if not the consequent, congestion.

I. *The fact and mode of producing irritation in a part through the agency of its nerves.*—The kind of action about which I am now to speak, is so well known these last few years as to require but little pains to prove its existence. A large number of observers have reported examples of irritative action produced as a consequence of disease or injury of definite nerve trunks, or of nerve centres. M. Charcot, of the Hospital Salpetriere, in Paris, and his now numerous students, have, perhaps, done more in this way, from the field of pathology, than any others. But it is no part of my purpose to give a history of progress in this respect.

Out of almost hundreds of well-authenticated cases of the kind of action I am seeking to describe, I will select at this moment one. I now refer to a form of skin disease, which some of you may have had a chance to observe. It is called *zona*, or shingles. It frequently appears in the skin of the chest, and is disposed in rude lines or patches, and generally extends forwards or downwards from the point on which it first is noticed. It may be confined to one side, or it may occur on both at once; and it may occur on many—or, indeed, on almost any other parts of the body, as I have often seen it do. It appears rather suddenly, beginning—so far as the local symptoms are concerned—with neuralgic shoots along the course of the nerves distributed to the part, and with soreness in and beneath the skin, and redness. The pain, soreness and redness increase, and, pretty soon, on the patches the most red, the epidermis begins to be raised, by fluid beneath it, as it is when a blister is applied; and soon you have a crop of little hemispherical blebs, or blisters, filled with a fluid almost transparent, while the spots of skin on which the blebs occur are deeply congested and swollen.

Now, what is the source or cause of irritation in such cases? Certainly no external irritant has been applied, of any kind. If the source of irritation is not external, it must be internal. There would seem to be, in view of our present knowledge, only two internal sources in such a case. One would be, that the irritant might be conveyed by the blood; the other, by the nerves. But that it cannot be by the blood is apparent on many accounts. That it is a result of irritative nervous action is a fact which has been comparatively well established, as already remarked; so well, indeed, that I may assume it, for the convenience of the present discussion. This being admitted, I will pass on to discuss the mode of production of the irritated and congested patches in the skin, which characterize zona.

I shall assume, then, that an irritative influence is conveyed along some nerve or nerves to the parts where the eruption occurs. I shall next assume, that this irritative nervous influence is expended chiefly, perhaps exclusively, in exciting to an abnormal degree, and perhaps in an abnormal way, the nutrition of the part—irritating—disturbing—exciting it.

I shall also assume, that this increased and deranged nutritional activity becomes the occasion, in a certain sense the cause, of the accompanying congestion, by means of which the supply of nutritive material, or blood, becomes in a measure commensurate with the increased demand for it. These assumptions I shall endeavor to make good presently. But, admitting them, I would explain our case in the following manner:

In an earlier lecture, I made mention to you of certain phenomena connected with the innervation and action of the submaxillary salivary gland. I said to you, that if a certain small nerve, leading to that gland, was *irritated*, it led to a greatly increased secretion from the gland, and to a corresponding congestion in its blood vessels. In explaining this curious case, I said that there is reason to regard the chorda tympani nerve as a secretory nerve, viz.: that it is distributed to the secreting structure of the gland, just as a motor nerve is distributed to the fibres of a muscle. By irritating the nerve in question you excite the secreting structure to action, just as by irritating the motor muscular nerve you can excite the muscle to action. I farther said, that I thought the secreting structure of the gland gave rise to certain other nerves, which we may call sensory nerves, and which, in an unconscious way, report the state of the secreting structure of the gland, just as certain nerves report the condition of activity of the muscles contributing to the so-called muscular sense. These sensory nerves of the secreting structure of the gland, which are aroused in degree, according to the intensity of action in the secreting structure of the gland, lead back, like all sensory or, as Dr. Carpenter would have us call them, "excitor" nerves, to a nerve centre. The centre or centres for the submaxillary gland I suppose to be in the gland itself. The impression carried back by the sensory nerves in question, rouses the centre to action, which has the effect to arrest or inhibit the normal action of the ganglion, upon which the vessels of the gland *immediately* depend for their *tonus*;

though they *mediately* depend for their tonus, as all similar vessels do, on the so-called *tonic centres*, that lie farther back, perhaps in the spinal cord.

The degree of this inhibitory action is measured by the intensity of the sensory or excitor impressions transmitted into the centre, and the intensity of the sensorial impressions is measured by the degree of action of the secreting structure; the intensity of action of which is measured in turn by the intensity of the irritation exerted on the chorda tympani, or "secretory" nerve.

The diminution of vascular tonus that is effected in this way permits the muscular vessels of the gland to expand, and so admit more blood. And they do this not because they are paralyzed, any more than a voluntary muscle is paralyzed when it is made to relax. So in the round, the degree of vascular relaxation and expansion is measured by the degree of activity of the secreting structure of the gland.

So in the case of zona we have irritation of a nerve trunk. This irritative influence is expended, not in exciting a glandular structure, it is true, but the nutritive action of a part. It is quickened, or even perverted, by the influence exercised by the irritated nerves. An irritation cannot be established and maintained without involving, of necessity, the sensory nerves of the part, whether they are the bearers of impressions destined to break into the sphere of consciousness or not. The sensorial impressions produced, as a result of the exalted or irritated nutritive action, are transmitted by the appropriate nerve fibres back to the *small* or "proximate" vaso-motor tonic nerve centres, which regulate the action of the vessels of the part; and they, by a process of inhibition, or arrest, permit the related vessels to expand, and we have a congestion which corresponds, in its intensity and results, to the vividness of the irritative action. Such is the way in which I would explain all active congestions, whether external or internal, that do not immediately depend on the action of some palpable or actual mechanical or chemical irritant.

Not only zona, but the majority of skin affections that are not caused by some local action, as parasitic skin affections, are, I believe, to be produced in a similar way. I believe that in some such way active congestions and inflammations of any internal organ may be, and often are, produced.

That congestion arises in consequence of irritation, and that irritation includes more or less profound tissue change, will be admitted by all, I suppose. But, whether a local irritation can be excited through the nerves of a part by some cause which acts on the nerves, and if so, through what particular class of nerves the irritation is produced, are questions that require farther discussion.

As regards the first of these questions, the proof of it is chiefly from the domain of Pathology. A large number of such cases have been collected by Brown-Sequard, Charcot, Vulpian, Couyba, Samuel, and others, which show, unmistakably, that disease of nerve trunks, but more particularly nerve centres, have led to decided local irritation at the peripheral termination of certain nerves. Various skin affections, as already mentioned, have been clearly traced to disease of related nerves or nerve centres, such as erythema, zona, pemphigus, the so-called "glossy skin," and, as I believe, all other skin affections, such as herpes, in all its varieties, and some forms of lepra, eczema, urticaria, the eruptive diseases, etc., as of similar origin. I also look upon articular rheumatism, as well as certain painful affections of the joints that simulate rheumatism, as produced in much the same way. Then there are certain affections of the muscles, especially the rapid wasting that occurs in some forms of disease of the spinal cord, especially that disease which has been named *Progressive Muscular Atrophy*, or rapid wasting of the tissue of the muscles, that there is reason to think must be ascribed to a similar origin, viz.: to irritative action of the related nerve centres and nerve trunks. We have, also, the sudden occurrence of bed sores, without the operation of the usual causes of such lesions, such as long confinement to bed from exhausting diseases, leading to extreme emaciation, and by long pressure on certain parts, to sloughs. In the cases I now refer to, the sloughs occur suddenly, and in connection with severe organic disease of the spinal cord, and in such way as to leave no doubt, it would seem, of the connection between the two conditions as cause and effect. I could mention many other classes of cases, in which nutritive or *trophic* disorders have clearly arisen as the effects of disease of the nervous system. I could occupy one or more lectures in detailing cases in point, either from my own observation or that of others. As to the *nature* of the action produced in the way referred to, all that need be said now is, that it consists in unhealthy tissue change of an irritative kind. Whether such kind of disordered trophic action is the same as that which underlies the inflammatory process, it is not necessary for me now to decide. Let it then be regarded as provisionally established, at least, that trophic irritative action may be excited, in a part, through the medium of the nerves that are distributed to it. This conclusion, if true, is a highly important one in many ways, in the domain of pathology. How many hypertrophies, or atrophies, or congestions, or inflammations, etc., may not have to be explained by using this conclusion, as one of the most important factors in the explanations?

But it is no part of my present purpose to consider the irritative trophic action produced by the nerves, any further than may be necessary to enable me to explain *idiopathic active congestion.*

The next question for consideration is, what nerves are the channels for conveying the irritative influence to parts that become seats of irritative trophic changes?

This influence that the nervous system undoubtedly exercises over the nutrition of the body, in some cases, was ascribed by Prof. Samuel, an inventive physiologist, of Kœnigsburg, to the action of a peculiar system of nerves, which he called *Trophic.* He has published many papers on the subject of these nerves and their mode of action, but has gathered the results of all his observations and reading into a very instructive little volume on "*The Trophic Nerves.*"*
But few, probably, are now inclined to admit the existence of a special system of nerves of the kind imagined by Samuel. But very few, on the other hand, can be found, it is probable, who do not admit nervous trophic action as a fact.

**Die Trophischen Nerven. Ein Beitrag zur Physiologie und Pathologie, von Dr. Med. S. Samuel. Leipsic*, 1860, 358.

If we have no special system of trophic nerves, the question arises as to what nerves are the channels of the action in question?

Is it by the sympathetic or cerebro-spinal nerves? The answer, in view of our present knowledge, would seem to be quite clear in favor of the latter instead of the former.

One reason for thinking so lies in the fact that when the sympathetic is divided which leads to a part, a well-marked congestion arises, and often continues for days, or even months, without any noticeable trophic change, as invariably happens in cases of active change, though of far less duration.

Another reason is, that the trophic troubles, ascribed to disordered nervous action, when they have been traced to a nervous origin, have apparently always been found within the confines of the cerebro-spinal nervous system, and there alone. But, though this latter statement should be found universally true, yet it might still be a fact that the morbid trophic action we are discussing could be ascribed to the sympathetic system, seeing that there are probably vaso-motor centres in the cord, and that from them vaso-motor fibres pass out with the cranial and spinal nerves. But the spinal and cranial nerves freely acquire vaso-motor fibres after they have emerged from the nerve centres, and during their course from the cord to the muscles. For example, if the trophic action, that the muscles often display, were due to the action of vaso-motor nerves proper, then the action should have an intensity according to the distance from the centre of the point of lesion of the nerve trunk, because it would then in the same proportion contain vaso-motor nerves. But such is not the case. The action is just as intense, or even more so, when the lesion is at the centre from whence the nerve proceeds.

But without going farther now, there are reasons for thinking the influence directly exerted on the nutrition of the body, is not by the sympathetic nervous system, but the cerebro-spinal nerves, and it may be either by the motor or sensory nerves, especially the latter. It may seem a little singular to you, that it should be by the latter nerves, seeing they are properly called afferent nerves, bearing impressions from the periphery to the centre. But I see no reason why any nerve may not carry impressions either way, under certain circumstances. It may be true that the so-called sensory nerves become the channels of impressions—morbid, if you please—that may profoundly modify the nutrition of a part. And though this be accomplished through the cerebro-spinal nerves, it may sometime be found true, as Samuel and Duchenne have supposed, that in the trunks of the cerebro-spinal nerves, there are included a separate class worthy of being called nutritive or trophic nerves.

But whatever uncertainty there may be as to the particular nerves, proceeding from the cerebro-spinal axis, that have an influence on bodily nutrition, there can be no doubt, as it seems to me, that they have an influence, such, at least, as has been contended for. This being admitted, you will remember I have already endeavored to explain the *mode* of action, and the mechanism through which it is accomplished. But the views I have been setting before you are not free from objections. I will now proceed to state and, perhaps, answer some of them. The following are some of the more prominent objections to special nervous trophic action:

1. It has been objected to because it is said that the nutritive changes observed are really only due to an increased supply of blood, which happens when the sympathetic nerves leading to a part are divided. But that this cannot be the true state of the case will appear from the following considerations:

Many cases of marked congestion, depending on disease or injury of the vaso-motor system of nerves, do not lead at all to appreciable trophic disorder, while in other cases, in which the congestion is no more marked, trophic changes form a decided feature from the beginning. In this last case we must, to explain it, introduce some other element beside mere vaso-motor disorder, because in both cases the congestion seems equal, but in one there is no appreciable nutritive change, and in the other there is.

2. It is objected that the nervous system has nothing to do with nutrition, and hence we do not have *nervous* trophic action. This is said to be apparent, because plants and some of the lower animals live, grow, reproduce, and, in short, have

the whole of the processes of nutrition carried on without a nervous system. If this is so, why admit such action in higher animals? Then, again, some of the parts of higher animals, such as cartilages or epithelium, for example, are devoid of nerves, and yet they live, and have a sort of nutrition without nerves. Then, again, it happens that certain parts of higher animals, as portions of transplanted skin, are made to grow and adhere to other parts, to which they are naturally foreign, and this they do, though cut off completely from the natural nervous supply. Facts like these, and there are many of them it is said, show clearly that nutrition does not depend on the nervous system. To these and similar objections, the following answers may be made:

1. It may be admitted as true, that in plants and the lowest animals, and in some of the simplest parts of the higher animals, nutrition is accomplished without the presence, and hence without the agency of the nervous system. But this does not prove that it may not be influenced by the nervous system in the case of animals and parts of animals that have a nervous system. We should remember that *motion* in the lowest animals, and even in some plants, is accomplished without a nervous system, but does this prove that motion in the higher classes of animals is not, in an important sense, under the control of, or dependent on, the nervous system? Not at all. Observation teaches the contrary. Then, again, it should be remembered, that the true position of those who contend for the so-called trophic nervous action, is not that all nutritive action is dependent on the nervous system, any more than they would contend that all muscular movement is dependent on the nervous system. The muscles may and do act independently of the nervous system. But no one would contend that the motor nerves, for that reason, have nothing to do with muscular action. So with nutritive action. It is true it may be, and as a rule is, carried on independently of the nervous system, but this fact does not prove it *always* is, in animals that have a nervous system. The fact is, I am firmly convinced that though nutrition does not *depend* on the nervous system, it is *influenced* by it in no unimportant way.

In such cases as the above, the part is not cut wholly off from nervous influence, seeing that an elaborate system of peripheral ganglia and ganglion cells exists yet in such parts, that may be quite sufficient, so far as the nervous system is concerned, for the purposes of nutrition.

Then, again, some cases of trophic action, morbid in degree or kind, are said to be due to inaction. There are trophic changes, say in muscles or bones, produced in cases of paralysis,—shall we say by inaction? Of this there can be no doubt. But, as M. Charcot says: "The passive lesions which we find to figure in different paralytic affections, have nothing in common with the special trophic lesions which occupy our attention at this moment. The latter may be distinguished from the former, objectively at least, by certain particular traits. The nervous trophic action is almost always marked, at least at a certain epoch, in its evolution by phlegmasic irritation. From its origin, usually, it assumes the characters of inflammation. It may, as we will see, terminate in ulceration, gangrene, or necrosis. Besides, a character which is common to most such cases is, that they are developed with great rapidity, in consequence of the lesions of the nerves or nerve centres, which have provoked their appearance, sometimes appearing with incredible rapidity.

"It is thus that we frequently see, in certain cases of fracture of the vertebral column, with compression and irritation of the cord, eschars appear on the sacrum the second or third day after the accident. It may in fact be said, as a general rule, that the contrast between *passive lesions*, which result from functional inactivity, and *trophic disorders*, which come on as consequences of certain lesions of nervous centres, is striking. The first are produced slowly, and have not, generally speaking, any inflammatory characters; while the latter appear very suddenly and in a very pronounced manner, at least in the beginning of the process, bear the marks, more or less indubitable, of inflammatory action." *

* *Lecons sur les mal. du syst. nerv. Faites a la Salpetriere. · Par J. M. Charcot (Bourneville). Des troubles trophiques consecutifs aux mal. du cerveaux, et de la moelle epeniere.* P. 7–8.

It has been objected to the view, that disease of the nerves or the nerve centres could give rise to trophic or nutritive disorders, that in experimental physiology, if the nerves leading to a part are divided, no immediate nutritive disorders spring up as a result. In such cases as have seemed to be exceptions to this statement, the nutritive disorders have been ascribed fairly to causes other than a deprivation of nervous influence. Thus, in the experiments performed by Schroeder Van der Kolk, in which rapid nutritive disorder in the hind legs of certain animals followed division of the sciatic nerve, it was shown by Brown-Sequard, that the trophic troubles—such as swelling of the hind feet, ulceration of the toes, and loss of the nails—did not come on, if pains were taken to prevent the paralyzed limb from being physically injured. In this case it was shown, that the ulcerative disorders depended on the physical injury that the animal could not take measures to prevent, by reason of the paralysis. So, also, in relation to the phenomena that follow division of the trifacial nerve within the cranium. They were formerly ascribed to the withdrawal of nervous influence from the eye and face, by dividing the fifth nerve. But it has been shown by Snellen, and more recently by Buettner, that the trophic troubles in the orbit, following division of the fifth cranial nerve, are due to the paralysis, and consequent lack of protection; because, when in such cases the eye was artificially protected, the trophic troubles did not appear.

Even the trophic disorders that are sometimes observed to follow after complete section, or destruction of the spinal cord—if no inflammatory or irritative disorder springs up immediately in the seat of the injury—are directly due to other causes than the deprivation of nervous influence. In fact, to cut a part off from the direct influence of the spinal cord, does not seem to be a cause of irritative trophic change, if, indeed, any kind of nutritive change. But, as M. Charcot says: "*It is not the same with those lesions which determine either in the nerves or nerve centres—an exaltation of their properties—an irritation—an inflammation.*"

In the cases of injury to nerves without direct consequences to the nutrition of the parts, the nervous supply of which had been involved, there was always more or less complete *anæsthesia or loss of sensibility*, which made it impossible for the animal to know when the part or member was in conditions that would be painful or destructive to its integrity, and hence could not protect it as it would have done in health.

As an instance of irritative action produced by disease of a nerve, M. Charcot cites the following experiment, related by Samuel in his work on the "Trophic Nerves." (P. 61.)

In the case of a rabbit, the Gasserian ganglion, which is on the sensory root of the trifacial nerve,—where it lies on the point of the petrous portion of the temporal bone, before the escape of the nerve from the cranium—had introduced into it the small needle electrodes from a battery (induction) and the current passed so as to irritate the part. Immediately the pupil was narrowed, there was slight injection of the conjunctiva, and the secretion of the tears was exaggerated. The sensibility of the lids, conjunctiva, and of the cornea, was exalted. After the operation the narrowing of the pupil persisted, though in a diminished degree, and the hyperæsthesia was increased. The inflammatory process began to develop itself at the end of twenty-four hours; its intensity increased during the second and third days, and diminished progressively from that time onward. Here we have all the degrees of an ophthalmia, from a slight conjunctivitis to a blennorrhœa, the most intense. One of the most noticeable features was that instead of complete anæsthesia of the conjunctiva, which follows division of the fifth nerve within the cranium after the procedure of Magendie, there was *extraordinary hyperæsthesia and irritability*, even so great, that a touch of the eye was sufficient to throw the animal into convulsions. In this case there was opacity of the cornea, and small ulcers of the conjunctiva, and in one case, there was a purulent collection in the anterior chamber of the eye. In these cases there was no division of the fifth pair, and no paralysis, but on the contrary, an extraordinary increase of sensibility. This case does not therefore come under the head of those criticised by Snellen and Buettner, in which the essential feature was *anæsthesia*. There was intense *irritation* of parts supplied by the fifth pair, and of the *sensory root* of this pain. I would explain this case as I did that of *zona*. And now, while I am speaking of the ganglion of Gasser, I will mention the sensory roots of other nerves, more especially spinal nerves. I wish to say, that the functions of these ganglia have never yet, so far as I know, been fully determined. But I am persuaded the office is a highly important one. Can it be for the purpose of ordinary muscular motion? Are the sensory impressions which are sent in toward the spinal cord through the sensory root of a spinal nerve intended to be interrupted by the cells in the course of the sensory fibres, that the impressions may be returned as reflex impulses toward the periphery for ordinary motor purposes? This does not seem probable, seeing that the nervous apparatus for muscular motion is in the cord. Can it be that the cord itself depends on these ganglia in some way? This does not seem probable, especially in view of the histological teachings of Kœlliker, Wagner, Robin and others. They unite in saying that the nerve cells that lie scattered between the fibres of the sensory or posterior root of a spinal nerve, and that are found so abundantly in the ganglion of the same, do not send fibres toward the cord, but toward the periphery. But why toward the periphery? What purpose can they subserve? I cannot undertake to answer this question now, but I think it is a highly important one. In my present opinion, these ganglia may often have something to do with trophic disorders in many parts of the body, and in this respect the experiment of Samuel which I have related is significant. If a simple nerve trunk, say of the fifth pair, or the sciatic or ulnar nerves should be treated in a similar way, there would probably be no such irritative trophic disorder in the parts to which the nerve is distributed, as in this case there was. In the experiment of Samuel, the irritant influence was exerted *in* a ganglion on the root of a sensory nerve, and led to certain results, of which *irritation* was the most marked. I would explain the case in question as I did that of *zona*.

But to return. It is my opinion that irritative action, set up in a ganglion like those on the sensory roots of the nerves, or in the gray matter of certain parts of the spinal cord, or even in the brain, as may be about the seat of a clot, or, possibly, irritative action in a nerve trunk itself (though I am in some doubt as to this), may give rise to irritative action, at the peripheral terminations of the related cerebro-spinal, especially sensory, nerves; which peripheral irritation may produce local results, to be compared with those produced by the immediate action of mechanical and chemical irritants, the chief difference being as to the nature and source of the irritation.

The peripheral irritative action, when once set up, leads at once in the way I have pointed out, to *congestion* in the irritated parts.

And thus I have endeavored to answer the question I raised at the close of my last lecture, when speaking of active congestion, viz.: What share in the process the nervous system has. It has been found impossible to explain the case so far as the nervous system is concerned, without going outside of the vaso-motor nervous system proper. We have been obliged to call into the account the cerebro-spinal nervous system, and especially the sensory nerves of the same, with a possibility that the ganglia on the sensory roots of all the nerves, had in some way much to do, as well as the cord and brain, in inducing trophic changes at the peripheral terminations of nerves.

This leads me through all I can find time to say, at present, on this subject. But what has been said has been sufficient, I hope, to show you the subject is one of importance, little as we may know about it. What I have said may serve to show you that it would not be possible to discuss in a satisfactory manner, diseases of the central nervous system, without first of all considering the vaso-motor nervous system.

Before closing this lecture, it may not be useless for me to sum up the results we seem to have reached, and to anticipate later parts of our course, so far as to enable me to point to a few of the practical applications of the principles it has been my endeavor to set before you.

1. The vaso-motor nervous system has important anatomical relations to the cerebro-spinal nervous system, the fibres of connection passing both ways, to and from the spinal cord. The vaso-motor nervous system is subordinate in rank, and in a certain way dependent on, the cerebro-spinal, but has a

sphere of its own, which it is capable of filling even when cut off from the spinal cord.

2. There are, in all probability, vaso-motor centres in the spinal cord and medulla oblongata, and possibly in the brain. These centres are connected by communicating fibres with the ganglia of the sympathetic, the action of which they probably regulate.

The same centres in the cord are probably in close relation with the motor and sensory tracts belonging to the cord proper, so that impressions sent into the cord along the spinal nerves proper, may be, under certain circumstances, reflected out from the cord, along vaso-motor nerves, in such way as to affect the circulation in remote parts of the body.

3. That corresponding to the double nerve supply of the heart, we probably have the same or a similar nervous supply for the small muscular vessels, one set of nerves leading, in a way already explained, to a contraction, and the other to an enlargement of the small muscular vessels.

4. That in explaining active congestions of the kind already described, we must take into the account the state and influence of the vaso-motor nervous system not only, but of the cerebro-spinal as well.

5. That active congestion includes as its prime element irritative tissue change, and that this irritative change may be induced in a part through the channel of its cerebro-spinal nerves, especially of the sensory class, which action of the nerves depends on irritative or inflammatory processes, set up, either in the course of the nerve trunks in question, or far more likely of the same kind of morbid state in the nerve centres, with which the nervous trunks are related; and that in the way already described, most idiopathic active congestions probably arise.

Such are some of the plainest conclusions to which we seem to have been led by our studies thus far. I now come to a few brief practical remarks, in view of what has been said.

If what has been said in the last few lectures is true, we may easily see—so it appears to me—that the positions taken are important ones, both in pathology and therapeutics. In the first case they would seem to enable us to advance nearer a final explanation of active congestions and inflammations of various degrees, and in various parts of the body, whether external or internal, such as pneumonias, pleurisies, bronchitis, certain ophthalmias, various skin affections, rheumatism, the local inflammations of gout, congestions and inflammations of the stomach, liver, bowels, kidneys, bladder, uterus, and other parts.

In such cases, I suppose that by some means an irritative influence is kindled in some part of the spinal cord, or some nerve centre, or, perhaps, nerve trunk, which irritative influence is transmitted to the periphery of the related nerves, as nervous influence is transmitted to a centre, as to a muscle or to a gland, giving rise to action of various kinds, according to the character of the part in which the nerve terminates—and excites that kind of change or disturbance of the nutritive actions that becomes, in its turn, the cause of the congestion, and in no unimportant sense, of the trains of morbid phenomena to which congestion and inflammation are observed to lead. How else can we explain, not the process, but the occurrence of the so-called idiopathic congestions and inflammations in different parts of the body? I do not see what should cause the lung, or liver, or brain, which are hidden away in the interior of the body, to *begin* with active congestion or inflammation, unless in the way I have described. I have carefully considered the views, for example, of Virchow and his school, and have never been able to see that in accordance with them I could explain the phenomena with which I have dealt.

I cannot now enter on a consideration of the evidence that is constantly accumulating, that the various organs and parts of the body are supplied from limited and definite regions of the nervous system. This kind of knowledge, when it is complete, will form one of the most precious additions to topographical anatomy, and will form one of the most essential elements in a correct pathology of the nervous system.

But the evidence as it now stands, if duly considered, would confirm the views I have maintained in my lectures in the past few days.

Then in regard to therapeutical applications, I will mention that I can understand the usefulness of such an agent as electricity only in the light of the principles I have endeavored to set before you. Take *central* galvanization, if you please, for the relief of certain skin affections, such as eczema. How does this operate except by allaying irritative processes in the centres, which then leads to a decline in the peripheral disorder, that depends for its existence on the central irritative action?

But I leave the practical applications of the principles above laid down to the cases we will so often meet with in the future of our course.

Art. II.—CASES OF HYSTERIA, NEURASTHENIA, SPINAL IRRITATION AND ALLIED AFFECTIONS, WITH REMARKS.

By George M. Beard, M.D.

UNDER this head I propose to detail a few cases of nervous disease—commonly called functional—that are at once exceedingly frequent and exceedingly annoying both to patients and physicians. I shall treat the subject mainly from the clinical and practical stand-point, reserving the discussion of its scientific and philosophic relations for another occasion.

By hysteria and allied affections I mean that large and increasingly numerous class of affections that pass among the people and among the profession by the vague and half-erroneous terms, spinal irritation, nervous exhaustion, general debility, general neuralgia, etc.

In a work on which I have been long engaged, and which is now slowly progressing, I hope to be able to unify these diseases—to show that they have in general a common pathology, a common history, a common group of symptoms, and a common therapeutics. I shall seek to show that these diseases, or symptoms of disease—or, as they might, perhaps, with better justice be called, results of disease—are expressions of a common nervous diathesis; that they are all liable to run into each other, and to act vicariously to each other; that they are a part of the price we pay for civilization, being confined mostly to the enlightened peoples of modern times; and that they are, in all their dreary shapes, most abundant in the northern portions of the United States of America.

I shall not anticipate the details of the systematic arguments by which these propositions will be supported, except so far as certain points of interest may be suggested by the following cases.

Those who are unwilling to give a provisional acceptance of my general theory of these maladies, or even to think favorably of it, may yet, perchance, attend to the recital of the results of treatment in cases, some of which are richly suggestive and quite rare; and from the therapeutics they may, I trust, be led to reason backwards toward the diagnosis and pathology.

GENERAL PRINCIPLES OF TREATMENT.

I treat all these affections, by whatever name known, on the same general principles, varying and adapting the method according to individual need. Of the various methods of using electricity, I depend mainly on *general faradization* and *central galvanization*, using them sometimes in alternation—in some cases finding the former, in others the latter, more beneficial.*

Internally, I use preparations of phosphorus and cod-liver oil, and sometimes arsenic. I make a large use of the cod-liver oil emulsion. I have seen good results from the oxide and phosphide of zinc and chemical food.

* The comparative value of these two American methods of using electricity in medicine is discussed by Dr. Vater, of Prague, Germany, in an elaborate series of articles now being published in the *Wiener Allgemeine Zeitung*. The series began with the May number. Dr. Vater confirms in full detail all that I have ever claimed for these methods.

Externally, I use ice and hot-water bags to the spine with studious caution, and mild and cautious counter-irritation to tender points on the spine. My method of counter-irritating nervous patients is, to take one of Alcock's porous plasters and cut off a piece of about the size and shape of my little finger; along the centre of this I place a little Spanish-fly ointment, and then apply over the tender spot, and let it stay there until it falls off. Counter-irritation thus used is not very annoying, and is quite effective. I use Alcock's porous plaster because it sticks better than anything I can find.

Except when I am experimenting, I use all these remedies, or several of them, simultaneously.

NEURASTHENIA, CEREBRASTHENIA, MYELASTHENIA.

The old and almost forgotten term, *neurasthenia*, I have for several years applied to the condition known in common language as *nervous exhaustion;* and I have recently subdivided this condition into *cerebrasthenia* and *myelasthenia*, according as the exhaustion is chiefly manifested in the brain or in the spinal cord. When the exhaustion shows itself chiefly in the brain, there are the symptoms of insomnia, headache, vertigo, flashes before the eyes, muscæ volitantes, tinnitus, etc. When the exhaustion shows itself chiefly in the spinal cord, there are the symptoms of pain in the back, at any point below the first cervical and last dorsal vertebræ, and mostly between the shoulder and in the lumbar region; spinal tenderness (though not always); weakness of the lower limbs, and sometimes of the arms; flatulence; feeling of oppression on the chest; gastralgia, intercostal and abdominal; neuralgia of the bladder and sexual disturbance; numbness of the extremities, etc. While the term neurasthenia implies both cerebrasthenia and myelasthenia, yet in some cases the exhaustion seems to be almost exclusively confined to the brain alone, or to the spinal cord alone.

The terms cerebral irritation and spinal irritation indicate symptoms that are usually prominent in cerebrasthenia and myelasthenia: the head as well as the spine may be tender, either at points or all over. The terms cerebral irritation and spinal irritation, simply refer to one symptom, and do not rightly express, even in a general way, the nature of the disease.

The meaning of these terms will be made more clear by the following cases:

NEURASTHENIA, CEREBRASTHENIA, AND MYELASTHENIA.

Vertigo; feeling of tingling and pricking all over the body; spinal tenderness; mental depression; improvement under general faradization and central galvanization, combined with internal medication.

Case I.—Mr. L——, aged 36, was referred to me, June 25, 1873, by Dr. Geo. Baker. For several months, since January, 1873, the patient had suffered from vertigo, feeling of tingling, pricking, and stinging over the surface of the body; pain in the back; dyspepsia; constipation; insomnia, and mental depression. The spinal irritation was quite variable in its seat, being sometimes in the lower, sometimes in the upper vertebræ. Sometimes there was tenderness of the cervical vertebræ, with stiffness of the neck.

The diagnosis was neurasthenia, including cerebrasthenia and myelasthenia; and the pretty evident cause was excess in sexual indulgence combined with over work in business.

I gave general faradization alternately with central galvanization, for one month; and, at the same time, used phosphide of zinc pills in doses of 1-10 of a grain, and chemical food. Counter-irritation was also employed over the tender vertebræ. July 5, he was much better. August 1, still better; and September 1 he resumed active business.

In the following case there was profound myelasthenia, and but very little cerebrasthenia.

MYELASTHENIA OF THREE YEARS' STANDING.

Tenderness of the vertebræ; occasional numbness of the arms and legs; peculiar susceptibility to hot water; pain in the back with great weakness; mental irritability and despondency. Relief under central galvanization, general faradization, continued counter-irritation, and internal medication.

Case II.—Mr. W——, aged 27, a merchant of New York, was first seen by me in consultation with Dr. H. W. Thayer, of Brooklyn, in the fall of 1871.

For three years the patient had been afflicted with symptoms of general debility, so called; great weakness in the lumbar region; occasional attacks of numbness in the left leg usually, sometimes in the left arm, and lasting for a few hours or days. The *numbness would be brought on in both sciatic nerves and their branches by long sitting.* All the symptoms of weakness and numbness were aggravated by over physical exertion, especially by long standing or walking, or labor. Whenever the patient put his left hand in warm water, the muscles contracted and the hand felt very disagreeable : very much as when the hand is put in water that is connected with an electric current.

Weakness of the bladder, to a very slight extent, had been observed at times,—a trivial slowness in passing water, and nothing more. These attacks came on only by long intervals, and were scarcely noticed. Lying down for a short period seemed to relieve the symptoms, especially after great exertion ; but attacks of localized numbness had come on while in bed.

Physical examination showed tenderness of the lumbar and dorsal vertebræ between the shoulders. The æsthesiometer showed no anæsthesia of the limb ; electro-muscular contractibility was normal ; except insomnia and mental depression at times, there was no evidence of cerebral disease beyond pure exhaustion. Mental labor, however, when protracted, made him worse. The pulse was slow, but tolerably full.

The back was so weak that riding in the cars caused great distress, and he wished to have something constantly pressed against it to sustain it. For that purpose he sometimes took a pillow with him as he traveled. In order to improve his health he had spent several months traveling in Europe, but was more injured than benefited thereby. *Long standing in picture galleries* seemed to have aggravated all the symptoms.

In the excessive heat of summer the patient was always worse ; Dyspepsia was more or less constant in the system ; also chilliness up and down the back, and coldness of the lower limbs. *Hyperæsthesia* of the nerves of the chest and abdomen was observed; also great susceptibility to tickling around the neck, breast, and stomach.

The sexual power was not specially impaired, the appetite was tolerable, and the nutrition well maintained. The queries that arose in the diagnosis were, whether the case was one of spinal congestion going on to posterior spinal sclerosis, or single *myelasthenia* with *cerebrasthenia*. At first I held my judgment in suspense, but finally concluded that the latter was the true condition : that it was caused by overwork, perhaps the excessive use of the sexual organs acting on a nervous constitution. The results of treatment seemed to confirm this view.

I prescribed and used for several months, by intervals, the following treatment : Central galvanization, general faradization, the internal use of Horsford's acid phosphate, cod-liver oil emulsion, and counter-irritation to the spine by means of various blisters of cantharides and tartar-emetic ointment.

Under this combined treatment the patient slowly improved somewhat ; was soon able to attend to business in a moderate way ; slept better, and could walk farther. Under the influence of excessive physical exertion he relapsed, and afterwards improved slowly. The patient subsequently relapsed, and was again treated, with success, by phosphide of zinc.

The symptoms of *transient numbness* of the extremities on pressure is a very common one in all forms of nervous exhaustion, as I have long observed.

The value of time as a remedy was shown in the following case.

NEURASTHENIA AT CHANGE OF LIFE.

Headache; weariness; extreme exhaustion; mental depression. Slight benefit from general faradization and central galvanization. Subsequent improvement and recovery under time and hygiene.

CASE III.—Mrs. T——, a lady 50 years of age, was referred to me July 26, 1871, by Dr. Belden. The patient was at the change of life, so called, but all her previous life had been unusually strong and free from nervous disorder. Her symptoms were, severe headache, with clavus hystericus occasionally; anorexia, insomnia, great mental depression, and profound nervous exhaustion. She could not read, or walk two blocks without being utterly overcome.

The pulse was good, and the nutrition was well preserved. She had taken various and judicious internal remedies. A course of treatment by central galvanization accomplished something for her, but not much. In the course of a year she recovered, and time seemed to do the work.

The peculiar obstinacy of some cases of neurasthenia to all forms of treatment, electrical, etc., is shown in the following case. Every now and then I meet with a case similarly rebellious.

REMARKABLE CASE OF NEURASTHENIA INDUCED BY OVER-TOIL.

Long standing; great debility and little or no pain; localized burning sensations, complicated with, but probably not dependent on, anteflexion of the uterus and dysmenorrhœa. Great susceptibility to electricity. No marked benefit from all methods of electrization or medication. Improved by time.

CASE IV.—Miss G——, a young lady of about 24 years of age, was first seen, with Dr. C. L. Mitchell, Nov. 24, 1871. The patient was of a very fine organization, and of slight, fragile build. Left an orphan at an early age, she had worked hard as a copyist in a telegraph office, where she toiled many hours a day to support herself and her younger brothers and sisters. For two years or more she had been in a condition of excessive debility, which her physician could control only imperfectly by medication.

She could not walk a single block, or even a part of a block, and so remained constantly indoors. Her appetite was feeble and fickle; sleep was uncertain and disturbed; the circulation unequal. The pulse, though weak and nervous, was yet tolerably strong for a delicate lady, but was very susceptible to mental influences. Careful examinations had been able to detect no disease of the lungs or heart, or of any organ; the uterus had not been examined.

Fainting spells, or spells resembling fainting, came over her after severe exertion; even the *shock* of *hearing* the door open fearfully agitated her, so that she suffered for a number of minutes. She dreaded the coming of a new doctor, and lived in a condition of painful apprehension when she learned that I was to be called in to see her.

To all medication she was extremely susceptible; even a few drops of dilute phosphoric acid seemed to do injury. Similarly all tonics and stimulants were badly borne. Some mental depression accompanied all these symptoms; but the patient had considerable *force of will*, and when in good health was very energetic. There were no fits of laughing or crying.

After a careful and thorough trial of general faradization, central galvanization, and galvanization of the cervical sympathetic, I gave up the case. She bore electricity as she bore everything else—badly, and no amount of treatment succeeded in bringing her to that condition where she could tolerate an average dose of either current.

During the latter part of the treatment the uterus was carefully examined by Dr. Skene, who found a tendency to vaginismus and anteflexion; but these symptoms were regarded merely as accompanying or incidental phenomena, and were not treated.

After electrical treatment was abandoned time came slowly to the rescue, and, under the care of her physician, she so far improved as to be able to walk out, but she subsequently relapsed.

This case illustrates: First, that there are certain temperaments that will not bear electricity; secondly, that in nerve functional disturbances time, rest, and hygiene, may cure or greatly relieve, after medication has failed. In all these cases special pains must be taken to avoid over exertion, mental or muscular. A slight indiscretion may put back the patient for weeks or months. For the nervously exhausted to overdo, even for an hour, is a blunder that is almost a crime.

SPINAL IRRITATION, ASSOCIATED WITH PROLAPSUS UTERI.

Localized crawling sensations; neuralgia; improvement under central galvanization and general faradization.

CASE V.—Madam J., a lady of middle life, was referred to me June 11, 1872, by Dr. A. B. Crosby. The patient was of a good constitution, and had usually been well, but recently there had been symptoms of nervous exhaustion, with spinal irritation, and tenderness of the vertebræ, all associated with a not very severe degree of prolapsus uteri.

Her most distressing sensation was that of *crawling and creeping in the back, arms and legs.*

The sensations were most marked after fatigue, and were very annoying; these sensations were referred to the tender points in the dorsal and lumbar vertebræ.

Turkish baths had given some relief.

I treated her one month by central galvanization and general faradization alternately, and with good results.

HYSTERIA WITH NEURASTHENIA IN A MARRIED WOMAN, FOLLOWING AN ATTACK OF REMITTENT FEVER.

Rolling, heaving, beating motions in all parts of the body; localized flushing; sparks before the eyes; spinal. irritation; mental depression; plethora; good pulse; decided improvement under central galvanization, general faradization, internal medication and counter-irritation.

CASE VI.—Mrs. R——, a married lady, 38 years of age, was first seen by me November 19, 1871. The lady was fleshy, full blooded, and had a very good pulse; but was excessively nervous, and gave an interesting history of hysteria with neurasthenia. During the previous summer and fall, the patient had been prostrated by remittent fever that had left her in a condition of extreme nervous prostration, from which she seemed very slow to rally.

Her debility was so great that she could walk or ride but a short distance, and her despondency was so intense that life seemed hardly worth the struggle. Her most distressing symptoms were rolling, heaving or beating motions, felt all the waking hours, especially in the head and stomach, but more or less in the legs, feet, arms, and in all parts of the body. The limbs would feel as though streams of water were rolling through them.

Vertigo was a symptom that frequently annoyed her, and, as she expressed herself, her "legs felt dizzy."

Localized flushings of heat were felt at various times in the face, legs and arms: these would appear and disappear in a moment.

Flashes and sparks before the eyes were a frequent symptom.

Tenderness of the dorsal and lumbar vertebræ, at the points where tenderness is so often found, was manifest on moderate pressure.

For these symptoms I prescribed central galvanization and general faradization, together with the internal use of Horsford's acid phosphate and cod-liver oil emulsion. Counter-irritation, by means of small blisters of cantharides and tartar-emetic ointment was employed. Strychnine was tried internally, and, as is so often the case, aggravated the symptoms.

Under this treatment the patient slowly but unsteadily improved, and with occasional relapses. February 1 the treatment was discontinued: long before that time the patient had been able to go out to walk and ride.

TRAUMATIC HYSTERIA.

I have applied this term to those cases where symptoms clearly hysterical are caused by concussion or some form of direct or indirect injury to the central nervous system. A severe fall or blow on the head may, through concussion, bring the central nervous system into a state where symptoms precisely similar to those that are found in hysteria, spinal irritation and neurasthenia appear. The symptoms thus excited have all the vagueness, the transitoriness, the protean character of hysterical and neurasthenic symptoms in general. Under this head also I bring those cases where similar symptoms are excited by sunstroke, or cerebro-spinal meningitis.*

In the following case the symptoms—as here narrated by the patient herself, the daughter of a physician—followed a severe attack of cerebro-spinal meningitis. The symptoms are accurately and fully described, and are representative of a certain class of nervous symptoms.

Case VII.—"For many months I had a tremor or chill, when just at the point of going to sleep, or on waking in the morning, and through the middle of the day felt miserably, having but little appetite. Some days I was sleepy and stupid all the morning, and others, uneasy and restless, with a pricking sensation in spots all over the body, more especially the face and arms. The sore spot in the left of my head seemed like an inflamed nerve —to press upon it, caused pain; and any fatigue whatever, gave such a weariness to that part of the brain, that I seemed unable to use it,—a feeling of perfect exhaustion.

"As summer wore on, I could see that I gained in strength of mind considerably, but was a long way off from health. My left foot began to have a slight feeling of numbness in the bottom, and the whole limb was weaker than the right one; also the left arm and hand were occasionally slightly numb.

"When I began to sit up, about three weeks after the first attack, I tried to read, but found I could read but a few words without pain, or a dazzling, blurred feeling back of the eyes. Through the summer my eyes grew worse,—a sort of dimness seemed to rest upon everything I saw, and I had a heavy, compressed feeling in the forehead, which weighed down the lids of my eyes so that I sat with them closed much of the time. I was unable to look at anything above the level of my eyes, or downward, by moving the neck, without causing dizziness. As winter came, I found I could read rather better, but not without the same dazzling sensation, and often it seemed as if the nerves of my eyes were losing their power altogether.

"For months after the first attack, the back of my head and neck pained me very much, and became very weak, so that I was hardly able to sit erect, and generally felt comfortable only when the head was leaned back considerably: something seemed to be drawing it back. My dreams were sometimes fearful during the early part of my sickness. One night I woke with the impression that my head was being slowly *crushed*, and for some time after the nerves of my head felt as if contracted.

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*For details, with cases, I may refer to my paper on *Certain Nervous Sequelæ of Cerebro-Spinal and Thermic Fever*, in Brown-Sequard's *Archives of Scientific and Practical Medicine*, No. 4—1873.

"During the whole year, but chiefly through the first few months, I felt a crawling sensation, as of worms, upon different parts of my body, and dreamed of them as creeping over my head, and sticking their feet in my brain; or of running beneath trees, whose branches scratched the top of my head."

The above case was treated by central galvanization, general faradization, and the phosphoric emulsion,* and with excellent effect. I have recently treated a case with similar symptoms, resulting from sunstroke, with very rapid improvement.

The importance of making a correct diagnosis between structural and functional disease is well shown by the following case. The peculiarity of some exhausted states of the nerve centres, that the patient apparently holds his strength in full, or entirely under control for a short time, until he suddenly gives way, was, in this case, well illustrated. There was, as will be seen, no paraplegia, except of a temporary character after exertion.

WEAKNESS IN KNEES AND LEGS OF AN HYSTERICAL CHARACTER.

Improvement under galvanization of spine; general faradization; counter-irritation, and internal medication.

CASE VIII.—Miss S——, a young lady of about twenty-four years of age, was referred to me June 2, 1871, by Dr. S. J. Holley. For three years and some months the patient had complained of inability to walk, for any considerable distance, without "giving out at the knees." Although her general condition was not remarkably good, she complained of no other distinctive symptom; and this special difficulty in walking, that was referred to the knees, did not seem to be associated with any other symptom of sufficient importance to attract her attention. This weakness, it should be observed, would come on suddenly, almost instantaneously, while she was walking, and at once she would be forced to sit down, or seek some assistance. An attempt to walk even a single block, was sufficient to bring on the strange symptoms. Very naturally the patient had supposed *the knees* themselves were diseased, although no external evidences of disease could be seen on either leg. At one time she had been treated by localized faradization of the knees without benefit.

The history of the case, in spite of its meagreness, at once suggested to me the view that the spinal cord was the true seat of the disease, whatever it

* The method of preparing this most valuable emulsion of cod-liver oil, phosphoric acid, etc., is given in the *Archives of Electrology and Neurology*—May, 1874.

might be. Examination of the spine showed tenderness of the lumbar vertebræ, of a decided, but not unusual, character; this tenderness, with the sudden giving way at the knees in walking, were the only symptoms from which the patient suffered. There was no anæsthesia, no feeling of numbness, no sensory disturbances whatever; no motor paralysis, and no diminution or modification of electro-muscular contractibility; no feeling of a cord about the abdomen or limbs; no difficulty of bladder or rectum, and no evidences of uterine disease. One fact of interest and suggestiveness was that, for a few days, at one time the symptoms seem to leave the knees, and go to the abdomen and pelvic regions: then again return to the knees. By intervals, during the three years she had been afflicted, there would be almost complete recovery. The patient felt better in a recumbent position. Clearly there was no organic disease. The condition of the cord was apparently one of exhaustion, with anæmia. The evidences of anæmia were the debility that was brought on by an attempt to use the cord in walking, the entire absence of symptoms of congestion, or even of transient hyperæmia, such as tingling, pricking, burning and heat in the bottoms of the feet, or neuralgic pains, and finally, the fact that the patient felt better in a recumbent position. A favorable prognosis was given.

The treatment used was general faradization and galvanization of the spine, counter-irritation over the tender vertebræ, and the internal use of Horsford's acid phosphate and phosphide of zinc. Under this treatment her improvement was quite slow. In one month she was somewhat stronger, but yet far from being well: she could walk further, but not more than one-eighth of a mile. The idea that the knees were the diseased parts was so powerfully impressed on the patient's mind that she was allowed to try the effect of elastic caps or stockings, without much benefit. In August the patient went to the country, where she continued to improve, and when we last heard from her she was nearly well.

In another similar case the results were even better.

ASTRAPHOBIA (*αστραπη, lightning, φοβος, fear*).

I have applied this term to those cases where, on the approach of or during a thunder-storm, there is very great fear, with nausea, headache, diarrhœa, excessive debility, and, in some instances, convulsions. The disease is analogous to the *agoraphobia* described by Westphal. It is found in both sexes. In the following case, astraphobia was complicated with other important symptoms:

Hereditary astraphobia; reflex paralysis of left forearm; writer's cramp; anæsthesia associated with neurasthenia; improvement under localized galvanization and faradization and central galvanization, spinal cord nerve current and plexus nerve current, combined with internal medication.

CASE IX.—Mrs. R——, a widow of 39 years of age, was referred to me July 27, 1871, by Dr. A. W. Catlin. Eight weeks before that time she had run a needle into her right forefinger; the needle was removed in three hours, but at once loss of power was experienced in the fingers, and in a week the forearm also had become very weak.

The patient supported herself by copying many hours daily and nightly, and the query arose whether the affection was reflex paralysis, or writer's cramp. The history of the case and the special symptoms seemed to show, clearly enough, that the case was one of paralysis, probably reflex, induced by the injury, but that the exercise of the arm in writing had acted as a predisposing cause. This opinion was strengthened by the fact that the patient had had some symptoms of writer's cramp before the injury.

Electro-diagnosis.—No loss of electro-muscular contractibility, but volitional contractility much diminished and considerable anæsthesia, made evident by examination with the æsthesiometer and the electric brush. There was also analgesia.

Examination with the dynamometer showed loss of force over the muscles, and the difficulty of writing was so great that she had acquired the habit of copying with the left hand. The patient was of a thoroughly nervous constitution, and all her life had suffered from *astraphobia* (fear of lightning). Even in her babyhood the approach of a thunder-storm had markedly disturbed her nervous system, and long before she was old enough to be afraid of lightning, she was a victim to the weakness, the distress, the *malaise* and after unpleasant nervous symptoms that lightning excites. *Her grandmother had been similarly affected.*

Dr. Catlin treated the patient by localized faradization and strychnine, and the patient observed that shortly after the electricity was used, the fear of lightning was removed. This was the more singular from the fact that only localized faradization of the arm was used, and no general faradization or central galvanization.

I recommended the use of the electric brush, central galvanization, and the internal administration of Horsford's acid phosphate. The details of the treatment, with the exception of the central galvanization, were carried out by Dr. Catlin, and the patient somewhat improved. Subsequently the patient was terribly frightened by stumbling over the body of a drunken man, in a dark lane of the city, at midnight, and in an almost unconscious state arrived home, and was next day taken sick. Then followed pain in the *very spot* where, weeks before, the needle had entered the hand, and the muscles of the hand began to improve.

June, 1872, the patient again consulted me. There was still, as before, local anæsthesia, only more profound, anorexia, insomnia, spinal irritation, and various symptoms of the neurasthenic condition.

She was again treated, and again improved; but her recovery was never absolute.

In cases like the above, there is more than simple, ordinary fear. The condition of the atmosphere, before and during a thunder-storm, has been repeatedly shown, by observation, to be very different, electrically, from the usual conditions.* The changes and disturbances in the electrical state are very marked and very rapid, and it is probably on account of these changes that the excessive fear, the headache, vomiting, diarrhœa, etc., are experienced.

One of the worst cases of the disease I ever saw was in a young man who was otherwise well, and apparently strong. The symptoms of diarrhœa, in many, occur during sleep, even when the patient is not conscious of the existence of a thunder-storm. A medical friend tells me that, for several years, he would wake up in the morning with a diarrhœa, when there had been a thunder-storm during the night, even though he had been all the time sound asleep.

Astraphobia is one of the sequelæ of lightning-stroke. A very interesting case of this kind, occurring in a physician of great strength of constitution and great native courage, will be published in detail in the *Archives of Electrology and Neurology* for November, 1874. The disease is clearly subject to the laws of hereditary descent, as is proven by the above case, as well as by others that I have seen.†

AGORAPHOBIA (FEAR OF PLACES).

Of this form of morbid fear I have seen two cases. In these cases, the East River Ferry was the obstacle: for a long time neither of them could be induced to cross it. They seemed to have the same terrible dread of crossing the ferry that some people have of ascending a height. Both cases recovered. It may be queried whether the fear of ascending heights may not, in some cases, indicate a pathological state. It is certainly dependent, to a very appreciable degree, on the physical state. All the kingdoms of this world, and the glory of them, could not induce me to ascend a high ladder, or stand on the edge

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* See my paper on *Atmospheric Electricity and Ozone: their Relation to Health and Disease; in Popular Science Monthly* for Feb., 1874. Also Beard & Rockwell's *Medical and Surgical Electricity*—first edition.

† I should regard it as a great favor if those physcians who read this article, and who have met with any interesting cases of astraphobia, will communicate with me in regard to them.

of a tower or precipice, when I am at all exhausted nervously. At such times, indeed, the very thought of standing on an eminence, the mere fancy that I might be placed in such a position causes an actual spasm in the stomach, with a peculiar sinking sensation, which is not imagination, but is as much a reality as small-pox or a broken leg. I cannot bear to hear my friends tell of their perilous ascents, and I suffer therefrom not only at the time, but subsequently when I think of it. In other matters there is no other earthly thought or imagining that causes me such distress, and I find that, in this respect, much depends on the general state of my nervous system.

Art. III.—A CASE OF HYDROPHOBIA IN A CHILD.

By Addison H. Foster, M.D., Chicago.

THE following notes of a case of hydrophobia in a child seem to me of sufficient interest to warrant their publication. It is to be regretted that they are not more full, but the circumstances of the case, the child's parents living in the extreme outskirts of the city, some distance from my office, with my other duties, prevented any such continuous and lengthened observation as I would have otherwise been glad to have given it.

On Friday morning, August 14th, I was called to see Viletta S., aged four, for what was described as "sickness of the stomach," with "strange breathing spells." On arriving at the house, about 10 A. M., I found the little girl, who had always been strong and healthy, sitting up in her crib and gently rocking herself. Her skin was naturally warm, her pulse about ninety, soft, strong and regular. The pupils were natural, the tongue slightly coated, except along the median line, where it was quite red. There was frequently a little nausea, but no vomiting; no disturbance of the bowels; the urination was natural. The child did not complain of pain and was perfectly conscious and rational.

Her conduct as observed during this visit was as follows: She would remain quiet for a few minutes while her mother gently fanned away the flies; but if one lighted on her face she would start as suddenly as if she had received a shock of electricity, look wildly about in a frightened manner, catch her breath, throw back her head, toss her hands about as if repelling some one, and then again become quiet and natural.

At other times she seemed to imagine that a fly had alighted on her face, and would go through a similar spasm. If the fanning was even gently increased, or if any one passed by her quickly, or a draft from the door or window was allowed to strike her, she would suddenly catch her breath in a kind of suffocating fit. If any one blew in her face the acuteness of the cutaneous hyperæsthesia was instantly shown by her tossing her hands, throwing herself back in a very frightened and agitated manner, and catching her breath in a succession of short hurried convulsive or sobbing inspiratory movements, but always relapsing again, in a few moments, into a quiet and natural condition.

She had eaten nothing since she first began to complain, on the evening of the preceding Wednesday, and had swallowed only two or three teaspoonfuls of tea or water. She had, however, repeatedly called for water, coffee, tea, beef tea, etc., all of which had been prepared and offered, only to be pushed away with the words "I can't take it," followed immediately by the kind of spasmodic attack already described. My merely taking up a glass of water on the table across the room and asking her to drink for me brought on one of these attacks.

On my inquiry as to whether she had ever been bitten by a dog, or had been playing with one, the parents, with unfeigned surprise looked at each other and said that she had been bitten some six weeks previously by a small pet dog, on the left cheek, where the mark was still visible. The bite drew blood but healed readily in four or five days. There was no sign in the cicatrix of the "recrudescence" spoken of by Dr. Watson.

I told the parents that I suspected hydrophobia, and left the fluid extr. belladonnæ to be rubbed along her spine, and some calomel to be placed on her tongue, with instructions to keep her as quiet as possible.

Once during this visit she fell asleep for a few minutes, lying on her side, but suddenly woke up very much agitated, apparently terribly frightened, and immediately went into a convulsion. This was the only kind of sleep she had had since she first went to her parents, Wednesday night, between eleven and twelve o'clock, saying that she "did n't feel good." They supposed that she had been too much exposed to the sun, though, as they stated, she had all day appeared as well as usual, had eaten her supper of bread and milk, and had gone to sleep as on any other night.

The effect of the wind upon her was first noticed Thursday after dinner, when she went out walking with her father.

A breeze striking her she caught her breath, saying "The wind blows too hard, I must go back."

This sensibility gradually increased from this time. Friday afternooon at half past twelve o'clock, Dr. Wm. E. Clarke saw her with me. He confirmed my diagnosis with his experience of three or four cases. He tried the effect of a current of air upon her legs, unknown to her, which produced the same terrible suffocating spasms and general agitation. *These* seemed to grow more intense and prolonged and were accompanied with a more or less livid appearance—from deficient æration of the blood.

When they passed off she resumed her play with her checker-men, and continued to be amused with them between times all day, whereas, whenever she had been ailing before the parents said "no one thing would amuse her five minutes at a time."

At this visit her pulse was faster, about one hundred and twenty, and not so strong and regular.

At a quarter past five in the afternoon Drs. Norman Bridge, Garrett, Damon, Whidden and Harroun, saw her with me. Her general appearance and behavior were much the same as at noon, with the addition of some froth appearing on her tongue, with the nausea and spasms. Her pulse was faster, ranging from one hundred and twenty to one hundred and fifty—weaker and more irregular, the pupils widely dilated, although only one application of belladonna was successfully made, about eleven o'clock, on account of the spasms it provoked. None of the physicians present hesitated to pronounce it a genuine case of hydrophobia.

Ten-grain enemas of chloral hydrate every two hours were suggested and one was given at about half past seven in the evening, but with so much apprehension and spasmodic difficulty on the part of the little sufferer that the next attempt was unsuccessful.

I next saw her a little after nine that evening, or nearly two hours after the chloral injection. It had produced no apparent effect. The pupil was even more widely dilated, the pulse more rapid, weak and irregular; the froth increasing; the surface heat seemed but little increased, though no thermometric observation was taken. She was still perfectly rational and conscious, and played with her checkers between the attacks.

At this visit she complained a few times of severe pains in her left eye, but not from the light.

In the course of the afternoon she succeeded in taking two or three teaspoonfuls of tea, which was the last of anything that she was able to swallow; for, although she often cried till near her last how hungry she was, she always appeared greatly distressed if food was offered her. The sound or sight of pouring fluids never disturbed her.

Between eleven and twelve Friday night the parents state that she grew more violent, and towards morning almost incessant in her sufferings, throwing her arms and legs over the sides and end of the crib, being kept in place with great difficulty. At times in the spasms there was opisthotonos, with throwing her body in that condition. The froth grew more abundant and many times that night, before being entirely out of a spasm, she would seize a cloth, wipe it across her mouth and throw it spitefully away.

My next visit was at nine o'clock Saturday morning.

She was then lying on her back, comparatively quiet, and evidently in a moribund condition. Her limbs were flexed and jerking, but neither rigid nor cramped, nor were her thumbs drawn in.

Her legs were purple and covered with a cold, clammy sweat; no pulse or cardiac impulse perceptible. Pupils very widely dilated and strabismic, intensely injected with a profuse puriform accumulation, especially in the left eye. Her breathing in different parts of the minute ranged from forty-eight to ninety-six respirations per minute. It was still slightly influenced by blowing in her face, but the flies did not disturb her. Her bowels, it was reported, had moved several times during severe spasms. There was frequent vomiting of a viscid froth about the amount of a dessertspoonful each time. All this time she was apparently unconscious and had not spoken, it was said, for over an hour.

She soon grew more quiet in manner and respiration and less responsive to blowing in her face, and was evidently slowly dying, when all at once every muscle of her body seemed to go through a slow convulsive transformation from rigid contraction to utter relaxation, while a deathly purple crept over her features, and that was the last.

No unusual *post mortem* appearances were noticed except some degree of discoloration about the eyes; no frothing or purging in the agony; no tumefaction of the abdomen and no *rigor mortis* for several hours after her death. No *post mortem* section was performed, as none would be allowed.

The points of interest in this case, as it appears to me, apart from the age of the child, are, the apparently utter absence of all mental symptoms, the child retaining its consciousness and its capacity for being amused and diverted till near the last, while, according to the statements of its mother, in all the previous slight ailments from which it had suffered it had been more or less affected in this regard; the absence of all premonitory symptoms; and the rapid course of the disease, the first complaint having been made late Wednesday night and death occurring about sixty hours later. It is possible, indeed, that some premonitory symptoms may have passed unsuspected, but the history of the case, as far as it could be obtained, does not state that any abnormal condition whatever was noticed or complained of before near midnight Wednesday. Unsatisfactory as the report is in many respects it may yet be of use as an account of a case of rabies in the human subject entirely uncomplicated by any moral influences whatever.

Careful questioning of the parents and neighbors brought out the following facts: The child was bitten about a week previous to the fourth of July and the wound was healed by that time. The dog, a cross between a poodle and a Scotch terrier, had always been friendly and playful until within a few days of the time the bite was received. It then became cross and snappish and inclined to hide itself away in holes dug in the garden. Its behavior was attributed to the heat of the weather and partly also to the effects of teasing by the neighbors. Rabies was entirely unsuspected. After the child was bitten the dog was tied with a cord which it soon gnawed apart, and commenced quarreling with another dog, a very unusual thing for it to do. On being tied again it again broke loose and stayed away for a day or more. On its return it was observed that it appeared to be very tired, hungry and thirsty, but it refused to touch either water or food. It was finally killed because, as it was said, "it was cross."

One other person, a man, was bitten by the same animal, in the thumb. He immediately sucked the wound and has expressed no alarm, and has as yet experienced no ill result.

Art. IV.—ON THE ACTION OF CERTAIN MEDICINES ON THE CEREBRAL BLOOD-VESSELS.

By Dr. Max Schueller, Bad Laubach, near Coblenz, A.R., former Assistant at the Phys. Institute at Jena.

Translated from the Berl. Klin. Wochenschrift, Nos. 25 and 26, by Dr. H. Gradle.

TOGETHER with rather extensive researches into the alterations of the cerebral vessels and vascularity, under the influence of certain external agencies (application of water), I have also instituted a series of experiments on the action of some medicinal substances, in order to test to what extent alterations of cerebral vascularity may be demonstrated directly on the vessels of the pia mater.

The following substances have, thus far, formed the subject of my researches: *Mustard, amyl nitrite, ergotine, opium and chloroform.* The experiments were made on rabbits—by reflection of the scalp, careful detachment of the periosteum, and subsequently trepanation of one or both parietal bones, by means of a specially constructed trephine, removing pieces of the size of about a cent; when desired, the openings could be enlarged towards the frontal bone. The attending hemorrhage is only considerable in young animals, possessing a very vascular diploe, but even in them it is easily arrested. Usually, the cervical sympathetic, with its superior cervical ganglion, was laid bare on one side before the trephining, and after the bilateral performance of this, severed. The moderate dilatation of the vessels of the pia mater, which followed in most cases, though not in all, immediately or after a little time on the side of the severed nerve, as was first pointed out by Nothnagel,* need not be the subject of any remarks at present, as I have sufficiently treated of it in connection with other researches referred to above, in which I have also detailed the contrary observations of Riegel and Jolly.† I will only remark that, in some few cases even extirpation of the ganglion produced no results on the meningeal vessels, though the vascularity of the corresponding ear was well marked. In all other cases, however, (and I have made a large number of experiments,) I have been able to determine a sometimes slight, but always distinct dilatation of the vessels of the pia mater on the side of the severed sympathetic, which was, however, never as striking as in the vessels of the ear, but was always marked by a more or less reduced power of reaction to the influence of agencies altering the calibre of the *normal* vessels.

The dura mater was not injured in these experiments, as its transparency permits a distinct view of the vessels beneath,

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* *Virchow's Archiv*, Vol. 40, p. 203.

† *Virchow's Archiv*, Vol. 52.

showing perfectly the difference between the bright, thin arteries, and the thicker, darker veins, both of which sets of vessels can be traced to their finest branches. The brain rises during expiration with, and in consequence of, a hightened venous injection, sinking back in inspiration. Pulsations, however, of its substance are rarely witnessed in rabbits. Immediately after removal of the bone, the vessels of the pia mater are seen moderately dilated and rapidly changing in calibre, which becomes constant only after a time, in a state of medium distension. Hereupon the substances to be tested were administered, and the vessels observed with a pocket-lens, sometimes by artificial light.

1. *Sinapisms.*—As this agency ranks highest amongst the rubefacients, and is strongly recommended even at the present date, in cerebral congestion, and the like, its employment being indeed rewarded by success in many cases, I thought it profitable to study its direct influence on cerebral vascularity in animals. With the expectation—according to the usual assumption—of finding a contraction of the vessels, I was surprised to find that small sinapisms applied to the ear or back of the neck, were of no influence whatever; though the points of application were reddened, injected, and the frequency of respiration slightly increased. Only after a *large* surface of the body, shaven of its hair, had been in contact with freshly prepared mustard-plaster for some time, were characteristic alterations of cerebral vascularity noted.

When the greater part of the abdomen or back was covered with mustard, the frequency of respiration increased at first with the pulse, while the vessels of the pia mater dilated regularly as its first influence began to be felt. Soon the calibre of the vessels began changing rapidly, a rapid contraction generally following the previous dilatation, lasting for a time, while the pulsations of the vessels became more and more indistinct, until an equally sudden dilatation again supervened. These changes continued for about ten minutes, gradually diminishing, leaving at last the vessels in a state of permanent contraction, the brain accordingly depressed. The frequency of respiration sank, (falling, for example, from twenty-three per one-sixth minute, after ten minutes to twenty, after twenty to eighteen, after half hour to sixteen, and lastly to fourteen per one-sixth minute,) the breathing became deeper. The pulse retained its frequency for a rather long time, becoming retarded only after twenty minutes, and reduced but four to five beats per one-sixth minute after three-fourths of an hour. The mustard was kept in contact with the skin for about half an hour, and then washed off with luke-warm water. The integument was reddened and tumefied; on cutting into it the subcutaneous cellular tissue was found œdematous, tumefied into a tough, gelatinous mass; its vessels, and those of the corium, freely bleeding. The cerebral vessels remained contracted after removal of the sinapism, sometimes for one and a half hours; during which period, the effects of irritants, ordinarily dilating these vessels, (amyl nitrite) were produced with difficulty, and but imperfectly.

As to the physiological explanation of the effects of sinapisms on the vessels of the brain, the mode of action seems different according to my researches from what had formerly been supposed.

A reflex excitation of intercranial vaso-motor nerves would not explain the dilatation in the beginning; neither would the assumption of simple depletion of cerebral vessels account for all the facts observed. It seems most reasonable to me to assume in the beginning of the influence of the agency a reflex paralysis of vaso-motor nerves by an irritation of cutaneous sensitive nerves, whence the moderate dilatation. Subsequently, when relaxation of the cutaneous vessels and hyperæmia of the skin has been induced by the "toxic" action of the agent, a depletory influence on the cerebral vessels must be the result of this peripheral congestion, while the vaso-motor paralysis has not yet passed off. Hence, the changes of calibre before the final permanent contraction, are proof that the primary influence of the mustard on the cutaneous nerves has ceased; while the peripheral congestion, reducing the blood-pressure and diminishing the relative amount of blood in the rest of the circulatory channels, cannot but cause a depletion of the cerebral vessels. The correctness of this theory is confirmed by even a superficial comparison of the surface exposed to the sinapism with the rest of the integument, and even more so by a consideration of the enormous hyperæmia and exudation; it being evident that the proceeding must induce a powerful alteration in the ordinary conditions of circulation. In fact the effects on tissue-metamorphosis and exchange of gases, which are both augmented, have been proven by Paalzow,* with the assistance of Pflueger.

Whether reflex action, starting from the irritated cutaneous nerves, is a factor in the contraction of the cerebral vessels, I am not as yet able to decide. My experiments on the relation between cutaneous sensitive nerves and the vessels of the pia mater have not been completed, compelling me to reserve my decision until later. Still, such a possibility must be granted, as such immense local alterations of a large surface cannot but influence the state of nutrition of the nerves included. Besides, it is not unlikely that the reduction of temperature induced by the application of external irritants (as found by O. Nauman), is also active in the contraction of the cerebral vessels. As I might state here, the alterations of cerebral vascularity are similar to those observed from the employment of several kinds of baths which I have published in the paper previously referred to.

The practical deductions from these researches, are, that sinapisms do actually reduce cerebral vascularity after continued application, and that the effects noticed on man are the consequence of this action; however, too much ought not to be expected from small sinapisms, as proportionately-sized plasters had no effect whatever on the vessels of animals. Indications for their employment are of course out of place. Suffice it to say that the previously *assumed* depletion of the brain has been verified by experiment.

2. *Nitrite of amyl.* The reports of the almost instantaneous action of this new remedy, especially in hemicrania sympathico-tonica,† induced me to experiment with it on trephined rabbits. I could find no account of direct observations on the vessels of the pia mater, with the exception of a short

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* *Archiv. f. d. Ges. Physiol.*, vol. iv., p. 492. *Ueber den Einfluss der Hautreize auf den Stoffwechsel.*

† Eulenburg and Guttman, *Path. des Sympath.*, p. 27, &c.

notice of Browne's* in the pamphlet of R. Pick,† stating that he had been able to observe with the lens a distinct dilatation of the vessels of the pia mater, while the rabbit was inhaling nitrite of amyl. Pick, himself, who gives us an excellent compilation of the literature of the subject, adding some interesting physiological details of his own, has not traced this action any further. After from 3—5 inhalations of vapor of nitrite of amyl from a sponge, the animals on which I experimented showed a distinct dilatation of the vessels of the pia mater, both arteries and veins; I noticed repeatedly a lively pulsation in the finer branches; in some places the arteries assumed a serpentine course. In one case I could even observe venous pulsation. The brain rises, while its movements become retarded and finally irregular. After its prolonged influence the brain keeps on expanding, pressing itself into the opening made by the trephine, so that at last the vessels are even compressed by the edge of the bone and thus reduced in size. At the same time the respiration, which was at first retarded (rarely accelerated) and deepened, becomes more and more irregular, amounting to actual dyspnœa. The animal begins to tremble occasionally, becomes even convulsed. But, as a rule, I did not continue the inhalations to that extent.

The pulsations of the vessels of the pia mater are accelerated, and the cardiac contractions increased in vigor. Occasionally at the beginning of the inhalation the veins assume a brighter color; later the bright red hue changes more to a brownish red. I refer to this especially, as H. C. Wood‡ had previously shown the brownish coloration of the blood in dogs poisoned with nitrite of amyl. On the side of the severed sympathetic the effects of this agent were in all cases distinctly visible, though the vaso-motor paralysis made its appearance later and much more slowly. The *relative* increase of calibre was less on the injured than on the normal side. Especially after the influ-

* *Nitrite of Amyl in Epilepsy. Med. Reports*, London, vol. 14, July, 1873.

† *Ueber das Amylnitrit u. seine therap. Anwendung.* Berlin, Hirschwald, 1874, p. 48.

‡ *Experimental Researches on the Physiological action of Nitrite of Amyl.* 1871, *Jahresbericht v. Virchow.*

ence of the drug had passed off was the distinction between the two sides most marked; as on the normal side strong contraction of vessels invariably followed, while the vessel supplied by the severed nerve contracted but moderately. This difference was so characteristic that I employed it frequently as a proof of the actual sympathetic paralysis on the corresponding side.

The dilating effect of nitrite of amyl was not prevented (as I shall report at another time*) by any modes of external application of water, but was modified in a characteristic manner in its intensity by the different kinds of baths. During the vascular contraction in freezing animals, as I have observed it on the vessels of the pia mater, after a long exposure to cold water it required a much longer time to effect a dilatation, which never reached its previous intensity. The same fact was observed in the application of large sinapisms.

Section of the vagus and the action of curare do not prevent the dilatation, but it is of later occurrence and of less intensity, especially after the former operation. Pulsations were not visible under these circumstances, but up to the death of the animal the vascular relaxation could be effected. The respiration was at first remarkably accelerated by the inhalation of nitrite of amyl after the section of the vagus, and seemed, in fact, facilitated, but it subsequently became stertorous. Without going into the details of an explanation of these interesting phenomena I will only remark that this acceleration may probably be accounted for by the dilatation of pulmonary vessels, or rather the increase of blood velocity and relative augmentation of the supply of oxygen to the centre of respiration (relative diminution of the hypothetic resistance in the inhibitory filaments which are excited by the section of the vagus), as the effect of the nitrite of amyl. The paralyzing influence of the agent I could ascertain also (like Pick) on the rabbit's ear, though later than on the cerebral vessels.

Even the strong dilatation of the vessels, caused by extirpation of the cervical ganglion was increased by the action of the drug.

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* In *Deutsches Archiv fur Klinische Medicin.*

These phenomena, as Pick remarks, favor the assumption of a paralyzing influence of nitrite of amyl on the vascular musculature. But, as my experiments show, the local effect is also dependent on the degree of distension of the vessels, as well as on the power of reaction of their muscular wall—in other words, the effect is more difficult to produce, the more the vessels are congested (sympathetic paralysis); or on the other hand the greater the contraction of the muscular fibres (as in rapid cooling, or by the action of ergotine).

My experiments have proven sufficiently that the supposed relaxing effect of nitrite of amyl on the cerebral vessels is an actual fact, and I can only hope that this confirmation of the previous theory will increase the therapeutical employment of our remedy, as its effects on man are equally rapid and marked, passing off again after the usual inhalation of but a few seconds, without any unpleasant consequences. As to the indications for its employment, I must refer to the above-mentioned work of R. Pick, but as far as could be deduced from my experiments, good effects can be expected from the agent in any morbid state depending on a contraction of cerebral vessels—in other words, intracranial anæmia, as in hemicrania sympathico-tonica, epilepsy, syncope, some forms of hysteric convulsions, perhaps also in tonic convulsions; as well as a powerful stimulant in threatening cardiac or respiratory paralysis.

3. *Ergotine.*—The contraction of vessels induced by this agent is a well-known fact, and has lately been repeatedly the subject of clinical and experimental researches. I will only remark here that I could produce a powerful and continued vascular contraction at the place of injection—for example, the rabbit's ear—as well as in both the arteries and veins of the pia mater.

The antagonism between ergotine and nitrite of amyl is a subject of even greater interest. When the vessels of the ear and brain are dilated to the utmost extent by nitrite of amyl, a subcutaneous injection of 0.0125 ' (about $\frac{1}{5}$ gr.) extr. secal cornut., dissolved in 8 drops of water, will still produce, in 10—15 min., a very distinct, permanent contraction of the previously relaxed vessels, lasting several days in those of the ear. On the side of the severed sympathetic this contraction takes place also, though it is of less intensity. If after a well-marked contraction by ergotine nitrite of amyl is caused to be inhaled for even ¼ to ½ hour, no dilatation follows; nevertheless, the poisonous effect of nitrite of amyl is not prevented, though of later occurrence.

The same results were obtained when the injection was made without previous inhalation of nitrite of amyl, this being only administered after the effects of ergotine are pronounced. If, however, less extr. secal cornut. was used, a still strong and permanent contraction took place, soon after previous dilatation by nitrite of amyl, but after the lapse of ¼ hour, the latter agent could produce again its usual dilatation, though in a less degree, this being much more intense on the side of the severed sympathetic. Pulsations, however, of the vessels of the pia mater, could not be observed after continued inhalation of nitrite of amyl.

These facts I deem sufficient to establish definitely an antagonism between ergotine and nitrite of amyl, which had been assumed for some time. But usually the contrary had been supposed—for instance, by T. Jones,* viz.: a relaxation by nitrite of amyl of vessels previously contracted by ergotine. That this, however, is not the case, my researches have plainly proven. The contracting influence of ergotine always predominates over the relaxing effect of nitrite of amyl. The retardation of the effects of the latter, which I had found when cerebral vascularity was reduced by cold or sinapisms, made it evident to me that its relaxing influence was not absolute, but dependent on the power of reaction of the vascular musculature, which view was confirmed by experiments with ergotine.

The contraction on the side of the severed sympathetic was proof that ergotine acts directly on the vascular muscular fibres, and in a higher degree than nitrite of amyl, as the latter is of no influence on the vascularity of either side, after a sufficient dose of ergotine has been given. However, that it should act more energetically on the vessel whose nerve is cut, when the dose of ergotine was insufficient, than on the innervated vessels,

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* Jones, Talford. *Nitrite of Amyl; physiol. action and medicinal uses, with suggestions for its employment in cholera.* *Practitioner*, Oct., p. 318. (*Virch. Jahresbericht*, 1871.)

is easily accounted for, as section of the sympathetic alone lowers the tonus of the vessels which it supplies, in a marked degree. The use of ergotine is increasing daily, and my researches, confirming as they do the clinical observations on its action, can only tend to enlarge still more the field of its usefulness. In congestion of the brain, in chronic cerebral hyperæmia attended with cephalalgia (consequence of hemorrhoids), I have employed it with good success. The symptoms abated in a few days, not reappearing often for months. I have also tried it in a case of marked symptoms of cerebral congestion and compression (probably caused by a tumor), with almost instantaneous effect on the very intense headache, with spasms of some muscles, frequent, full pulse, temporary unconsciousness, vomiting, etc.—the attacks recurring every fortnight and lasting several days. On giving 20 grs. extr. secal cornut. within 2 days, the symptoms—at the time of unusual intensity—ceased on the second day, and on the third the patient could walk about, and the pulse was normal. The disturbances of co-ordination, staggering gait, unsteady tongue, etc., which formerly had troubled him very much after the attacks, had now no greater intensity than during the intervals. Finally the intervals were prolonged.

4. *Opium.*—Both subcutaneous and intravenous injections of tr. opii. caused a primary dilatation of vessels at the place of injection, and of the pia mater. But soon the cerebral vessels undergo a contraction, and the brain collapses. The dura mater is raised moderately by the increased cerebro-spinal fluid. The vascular contraction however is not very intense; never as strong as I have observed it after the use of the wet sheet on animals, the influence of which is also quieting and soporific. Occasionally I noticed alternating contractions and dilatations; after the narcosis a moderate dilatation follows.

5. *Chloroform*, inhaled like nitrite of amyl, causes after 6—12 inspirations, a primary contraction of the arteries of the pia mater, and subsequently of the veins, with retardation of. the pulse; soon after there follows an increasing relaxation of arteries and veins, and finally an excessive venous stasis, amounting to actual "cyanosis." The arteries likewise assume a darker color, from the venous character of the blood. The pulse rises

somewhat after a time, to sink again on continued inhalation but the calibre of the vessels does not undergo any changes. Only when free access of atmospheric air is permitted does the calibre change, while the blood becomes brighter. These changes are the same on the side of the severed sympathetic. The movements of the brain are at first slackened, but intensified, subsequently they become irregular, and are at last again slackened. These phenomena continue a little time after the removal of the chloroform, gradually the vessels return to their normal state, while the frequency of the pulse and respiration increases, and the color of the blood becomes natural.

Nitrite of amyl can remove the effects of chloroform on cerebral vascularity in a short time. The arteries dilate, assume their normal color, the veins likewise; the respiration becomes more frequent and unimpeded. As this fact, apart from its physiological interest, seemed to me of practical importance, I have investigated it more closely, and hope to excuse my remarks on the subject by the universal interest connected with it.

On narcotizing a rabbit (strapped on a board) from a sponge saturated with chloroform, the frequency of respiration sinks in the beginning from 30 to 20 per $\frac{1}{6}$ min., to rise again after a little time, especially if atmospheric air is supplied. After 6—15 min. a considerable reduction of frequency is evident, which is greater as more chloroform and less air is inspired. In a similar way, but within narrower bounds, the frequency of the pulse alternates, so that to a reduction of respiratory frequency to 6 per $\frac{1}{6}$ min., the pulse may sink only to 24 beats in the same time (before 30—32). After a long-continued influence of the chloroform (sometimes only after one hour), the breathing becomes stertorous, dyspnœa sets in, the pulse becomes slow and irregular; the muscles lose their tonus, reflex irritability diminishes and finally disappears, as tested on the eye. Touching the cornea excites a reflex contraction of the orbicularis palpebrarum muscles as long as the narcosis is not perfect, but not after its completion. If now the chloroform is removed and nitrite of amyl substituted, the respiration is immediately facilitated; the dyspnœa disappears; the breathing is accelerated at first but 1—2, later 6 respirations per $\frac{1}{6}$ min. More rapidly, almost instantaneously, the pulse is accelerated, becomes fuller and stronger, so that after the lapse of but one minute, it may have risen 20—35 beats per $\frac{1}{6}$ min. At the same time the changes in the vessels above described are apparent. But the greatest interest centres in the reflex irritability, which, destroyed by the chloroform, now returns in a very short time. After a few seconds the orbicularis contracts on touching the cornea slightly; a few seconds later the animal reacts vigorously on mere pinching, the muscles of the tongue and of deglutition commence to act, and the stupor which for the last 10—15 minutes had been complete, vanishes. Mere removal of the chloroform, with free access of atmospheric air, will only accomplish this in a much longer time.

This same restoration by nitrite of amyl will succeed even after a very prolonged action of the chloroform, though with less vigor. Even after section of the vagus I have still obtained the same results, but while the facilitating effect on the respiration was quite apparent, an acceleration was less marked. The circulatory phenomena were not altered. The duration of the influence of nitrite of amyl must not be prolonged, as the poisonous action of the agent will then come into effect. Chloroform inhalation continued after the restoration by nitrite of amyl, again produces its usual effects.

I will also state that the action of chloroform on the cerebral vessels is not affected by section of the vagus. The respiration is not at first influenced, but later pronounced dyspnœa appears. The pulse, however, accelerated by section of the pneumogastric nerve, is slackened at first by chloroform, regaining its former frequency after a time.

The alteration of cerebral vascularity which I have observed on the pia mater, as well as the general effects of chloroform on animals, are in conformity with the clinical observations on man. The only difference is a prolongation of the various stages, and the necessity of a larger quantity, *i. e.*, proportionate to his weight. A careful comparison of my description of the different stages in animals, with the same in man, for instance, by Nussbaum, in Pitha-Billroth's hand-book of surgery,* will show the similarity of the circulatory and

* Pitha-Billroth. *Hdb. d. allgem. u. spec. Chirurgie.* Vol. 1. Abth. 2, p. 595.

respiratory phenomena. The psychical influence, however, is evident in the first stage in man. But occasionally I have also observed an increase of frequency of pulse and respiration in the beginning in animals, though usually the opposite condition. The psychical phenomena in man can, in my opinion, be accounted for by similar alterations of cerebral vascularity as I have noted in animals, as it is evident that the state of nutrition of the cerebral hemispheres must vary with them. The deficiency of oxygen alone might cause disturbances, but the effects on tissue-metamorphosis, caused by the passage of blood deficient in oxygen, and besides saturated with chloroform through relaxed vessels, are manifest; the results are the psychical disturbances, the phenomena of primary excitation and secondary depression, and at last, total cerebral paralysis.

I will return to the interesting restoration of narcotized animals by nitrite of amyl, which I believe can be traced to the instantaneous augmentation of cardiac vigor, in consequence of which more blood, and that richer in oxygen, is supplied to the brain and respiratory centre. Perhaps a direct action of nitrite of amyl is also a factor. The important question is, however, whether this antagonism can be utilized in threatening asphyxia by chloroform. At any rate my experiments on animals would justify a trial on man. The rapid action of nitrite of amyl would be a high recommendation of its use in threatening paralysis of respiration and circulation, and though I do not advise a reliance on this solely, to the exclusion of other well-tested means, it certainly deserves a fair trial.

Art. V.—ON DILATING VASO-MOTOR NERVES.*

By Prof. Fr. Goltz, Strassburg, assisted by Dr. A. Frensberg.

Translated from Pflueger's Archiv, IX, 475, by Dr. H. Gradle.

THE doctrine of the activity of vaso-motor nerves constitutes, at present, an important part of physiology. To solve the question, however, as to whether nerves exist which dilate the vessels, but little has been attempted. Apart from the researches of Schiff, which were not rewarded by success, but two facts are known, the cause of which is to be sought in the activity of dilating vaso-motor nerves.

The *nervi erigentes* of Eckhard, the irritation of which is followed by injection of the erectile tissue in the penis, are generally supposed to contain such dilating filaments. The chorda tympani is also considered as a nerve of this description, as its stimulation is productive of a strong dilatation of the vessels in the submaxillary gland. All other facts which have been cited occasionally as proofs of the existence of such nerves, have either received too little attention, or have been incorrectly interpreted by observers. On this account, the theory of dilating vaso-motor nerves has, as yet, but an insufficient basis.

The experiment which I related on p. 496, in the eighth volume of this *Archiv*, has induced me to institute a more extended series of researches, with the assistance of Dr. Frensberg—the results of which compelled me to admit the great importance of dilating vaso-motor nerves. Facts formerly explained by vaso-motor paralysis, I shall now attempt to

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* This admirable article, by one of the most skillful experimental physiologists in Germany, is given, partly because of its great merit, and partly because it supports, at all essential points, the views that have been set forth in the lectures on the "Pathology of the Vaso-Motor Nervous System," published in the Journal during the year. We commend the article to the attention of our readers.—Eds.

trace to vaso-motor (dilators) stimulation. I shall cite the experiments, not in their chronological order, but rather in an order most calculated to lead the reader to our conclusions :

1. Very frequently the sciatic nerve has been cut in animals, and, not infrequently, such an accident has been witnessed in man. Long before Cl. Bernard had made his celebrated experiment on the cervical sympathetic, it was well known that the temperature of a leg whose sciatic nerve is severed, may vary considerably from that of its uninjured fellow. Immediately, or soon after the section of the nerve, the temperature of the paralyzed limb rises rapidly. In one case, for instance, the thermometer showed on the right side, corresponding to the severed nerve, 38.4° C.; on the left, only 22.5° C.*

In most cases, it is true, the difference is not so great, but always sufficiently marked to be observed by the unaided touch; besides, an increased arterial pulsation is regularly noticed in the paralyzed limb.

All observers agree that the increase of temperature is owing to the section of vaso-motor filaments in the course of the great sciatic nerve, and the subsequent diminution of vascular tonus. However satisfactory this explanation seems at first, its defects become manifest on consideration of the appearances, observed some weeks after the injury. I have divided the nerve in a large number of dogs, and kept the animals continuously under observation. A few days after the operation, the difference in temperature between the injured and the normal extremity is less decided, the temperature of the injured limb sinking constantly, until it equals that of the other; *in fact, in most cases the paralyzed limb has become cooler than the others after the lapse of a few weeks.* In one case the thermometer showed, sixteen days after the section of the right sciatic nerve, 20.5° C., on the right, 26.3° C. on the *left* side. Immediately after the section of the nerve, the right hind leg of the same animal had been warmer, by 9° C., than the left. The time during which the temperature sinks

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* 1° Centigrade equals 1.8° Fahrenheit, the 0° being the freezing point of water, or 32° Fahr.

to its normal point, is subject to great individual variations,—it usually takes from ten days to four weeks. It seemed to us as if this period was shorter, the more kindly the wound healed. Still, the reduction of the temperature in the paralyzed limb cannot be supposed to depend on the reunion of the divided nerve, as it is observed a long time before the latter event can occur. A reunion can also be prevented by exsection of a part of the nerve, without altering the *thermal* phenomena in the paralyzed extremity.

It is well known to the pathologists that a member deprived of motion and sensation is colder than the normal limbs, and the difficulties have often been considered, which are unavoidable, when we try to reconcile this fact with our present ideas of vaso-motor nerves. But that the attempt has been made to overcome these difficulties by forced assumptions, will be apparent on consideration of the following researches.

2. After I had confirmed, by repeated observation, the reduction of temperature in the paralyzed limb, after the lapse of some time, I undertook the following experiments:

Chloroforming a dog whose hind leg had become decidedly cooler than the other, in consequence of a previous division of the corresponding nerve, I cut the spinal cord between its dorsal and lumbar divisions. The immediate consequence of this operation was an increase of temperature in the uninjured hind leg, while the paralyzed leg became even colder. I have frequently repeated this experiment, and always with a similar result. As an example, I will cite the following case:

The experiment was performed on the animal previously mentioned. After the right sciatic nerve of this dog had been severed on the 13th of December, 1873, the temperature of the right hind leg amounted to 38.4° C., that of the left, 22.5° C. (rectum, 39.4° C.) On the 8th of January, 1874, after the wound had perfectly healed, the difference amounted to but 1.8° C., the mercury rising to 27.5° between the toes of the paralyzed leg,—25.7° C. in the uninjured limb. On section of the cord, the thermometer indicated 34.5° C., in the *left* hind leg, but sunk to 25.5° C. in the right, while the fore paws showed 28.5° C. The effect of section of the cord is accordingly different on the opposite sides. On the right side, corresponding to the divided nerve, it caused a reduction of 2°, on the side of the uninjured sciatic nerve it raised the temperature by 7°.

3. By maintaining a dog alive that has undergone the operation above described the temperature of the limb, corresponding to the uninjured nerve, sinks again slowly. If now the cord is destroyed below the previous section, the cutaneous temperature will rise again in the uninjured limb, but remain stationary in the paralyzed extremity. This can only be observed in animals surviving for some time after the destruction of the lumbar portion of the cord.

4. If in a dog, one sciatic nerve of which has been severed some weeks, and the cord divided some weeks later, the second sciatic nerve is cut, the temperature will rise again in the limb whose nerve was cut last. As an example, I offer the following case:

On the 4th of January, 1874, the right sciatic nerve of a dog was divided; on the 15th of the same month the cord was cut between the dorsal and lumbar divisions. Four days later, on the 19th, the temperature of the right hind leg, with severed nerve, was 29° C.; that of the left, 33° C. Section of the second (the left) sciatic nerve was then performed. Within a few minutes the temperature of the left hind leg rose to 39.1°, while the right limb fell to 24.5°. The section, therefore, reduced the temperature on the right side by 4.5°, and raised it on the left by 6°,—the thermometer showing, at the same time, 41.5° in the rectum.

An attempt to reconcile these facts with the present vasomotor theory, would lead me into conflicting positions. The injuries are perfectly symmetrical. The right sciatic has been severed, the left likewise; the spinal cord has been completely divided, and still the state of vascularity is not the same on both sides. The integument of this white-haired dog's left hind leg is of a bright red hue, the corresponding part on the right leg has a pale, bluish tint. Grasping both legs with my hands, I feel violent arterial pulsation on the left side—none on the other. Placing a thermometer between the toes, I find the immense difference of 15° C. in favor of the left side.

The right hind leg has a normal temperature of 24.5°, though the cord is divided and the corresponding sciatic nerve severed. On what vaso-motor nerves is the arterial tonus dependent in this case? They must indeed follow a strange course to still supply the arteries of the toes after the section of the sciatic nerve; and, if indeed they take such an extraordinary course, why do they seem absent or inactive in the left leg? I am sorely puzzled for an answer, unless I disregard the present ideas on vascular innervation. But, more of this hereafter. At present let us simply answer the question, Why, in the last experiment, the left foot is warmer than the right? *Because the left sciatic nerve has been severed fifteen days later, and hence, the wound of the left nerve is so much more recent.*

5. If, after destruction of the lumbar cord, a dog's sciatic nerve is divided, the corresponding hind leg will be the warmer. This experiment has been related on p. 497, in the eighth volume of this *Archiv*, and has been repeatedly confirmed since.

6. If some time is allowed to pass between the section of the first and second sciatic nerves, the foot corresponding to the more recent injury will be the warmer of the two.

7. The following experiment is also of some interest:

A dog whose spinal cord had been divided, was subjected to a manometric observation for other purposes. On this account, the right crural artery was ligated. After this procedure, the right foot was much cooler, as its blood supply could only be obtained through collateral circulation. On severing the sciatic nerve, the temperature of the right hind leg rapidly rose, soon surpassing that of the left, amounting to 34° C.,—the left being but 31°. Section of the sciatic nerve in an animal with divided cord, consequently produces such a dilatation of the corresponding vessels as more than to neutralize the obstacle which ligation of the femoral artery opposes to the circulation.*

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*This case is similar to one reported by Bernard, but the explanation of Goltz is different from the one given by Bernard, and not so nearly correct, as it seems to us.—Eds.

Let us now derive some deductions from these experiments. We can first assert that section of the cord, or of the sciatic nerve, is followed by dilatation of all vessels in the regions connected by nerves with the place of injury. For example, on dividing the cord in a fresh animal, all parts situated behind the wound become warmer, as they are connected by direct nerve tracts with the point of section. If, however, section of the cord is performed in an animal whose sciatic nerve has been previously divided, the paralyzed limb will be exempt from any increase of temperature, as it is not any more in uninterrupted nervous connection with the wound of the cord.

The second conclusion at which we are justified in arriving, is the following: The dilatation of vessels, following section of the cord or nerve, is the more marked, the more recent the wound. The greater the time that elapses since the injury, the more the vessels return to their normal state, *i. e.*, the more completely will the tonus be re-established. From this it can be deduced that, when several sections of the nerve have been made on an animal, the parts lying behind the most recent wound will have the highest temperature. If, for instance, the right sciatic nerve of a dog is cut, and, a fortnight later the cord, the left leg will appear warmer than the right, being in uninterrupted nervous connection with the most recent wound—that of the cord. The right foot must be the colder, as its nervous connection reaches only to the place of section of the nerve—the *older* wound. The reverse would be true, if the cord had been severed, and, two weeks later the sciatic nerve. The right leg would then be the warmer, as there is direct nervous connection with the most recent wound. By recollecting these principles we are enabled to state at once the relations of temperature in an animal undergoing neurotomy, if we are in possession of the necessary dates. But there is one point I must still refer to: The hyperæmia and increase of temperature consequent upon section of a peripheral nerve is, on the average, greater than when caused by division of the cord. If, for example, I sever the *right* sciatic nerve of a dog to-day, and divide the cord to-morrow, the temperature of the left leg would, of course, rise by the latter operation, but hardly equal that of the right foot. In this case, there would be an exception to the previously mentioned rule. The left foot, the rise of temperature of which depends on the section of the cord, would probably be colder than the right limb, which stands in nervous connection only with the *older* wound. But this apparent exception is easily accounted for, as section of a peripheral nerve exercises a much greater influence on the temperature of the paralyzed parts than an injury of the cord. If it is desired to eliminate this difference, the division of the cord ought to be performed only after the lapse of some days.

I have previously stated that I despaired of reconciling these facts, and their legitimate deductions, with the present vaso-motor theory. If we assume the location of the vaso-motor centre in the medulla, and the descent of the vaso-motor nerves in the cord reaching the foot by way of the sacral plexus, etc., it seems unaccountable how one foot can exceed the other in temperature by 15°, when the nervous connection of both with this centre is severed alike. Even if we suppose the existence of vaso-motor centres in the whole length of the cord, as I have previously done, the puzzle is not solved, for how can we explain the increase of temperature consequent on section of the sciatic nerve severed from its origin—*after destruction of the cord?*

For months I have pondered over this question. The theory at which I was forced to arrive, and which, since then, I have constantly tested, will, no doubt, be received hesitatingly by most readers, as it does not only overthrow our present ideas on the innervation of vessels, but also demands a careful reconfirmation by numerous experiments in nervous physiology.

In the 8th vol. of this *Archiv*, I have expressed the view that the vascular tonus does not depend merely on the vaso-motor centres in the medulla and cord, but also on a set of ganglia, in the walls of the vessels themselves, analogous to the cardiac ganglia. These small peripheral centres are under the control of the great nervous centres, as is the heart, the activity of which is regulated by the medulla. If the nervous connection with the cord of any part of the body—for example the foot—is severed, the vascular tonus is not annihilated forever, for the small centres on which the tonus depends, are situated in the walls of the vessels themselves. But how are we to account for the temporary destruction of the tonus after neurotomy?

In order to answer this question, I must make an assumption which many no doubt will consider untenable.

I am compelled to believe that the simple division of a nerve acts as a powerful irritation, and produces permanent phenomena in the peripheral distribution of that nerve. In other words, I maintain that section of a nerve trunk excites the dilating vaso-motor filaments which it contains. The dilatation of vessels and increase of temperature following neurotomy is therefore an *active* phenomenon, and I consider the dilatation to be produced in a similar way to the inhibition of cardiac activity on irritation of the vagus.

It is evident that this hypothesis, according to which simple nervous section would suffice to excite dilating vaso-motor nerves, is competent to explain many problems. We have seen that the dilatation decreases a short time after the division of the nerve, and gives way afterward to permanent contraction. This fact agrees excellently with the supposition that the dilatation is an active, self-limited process. If different sections of the nerve have been performed on the same animal at different times, the vessels of that region will be the most dilated which corresponds to the most recent nerve wound, as there the irritation to which the nerves are subjected is the most pronounced. When the wound has subsequently healed smoothly, and the excitation produced by it has ceased, the vascularity will of course sink. The peripheral nerve apparatus, which I consider in the light of small nerve centres, will then come into full activity, and the vascular tonus will be re-established.

I candidly confess that this perhaps bold hypothesis, however convenient it seems, necessitates for its support a very cautious experimentation, which I have attempted as follows:

If it is true that division of the sciatic nerve excites the dilating vaso-motor filaments, it is to be premised that a repeated section of the peripheral nerve stump must renew the stimulation and increase the dilatation of the vessels. This supposition is confirmed by experiment. I have previously mentioned a similar observation on page 497 of the 8th vol. of this *Archiv.* In the mean time I have repeated such experiments, and in several cases obtained an increase of temperature in the corresponding foot, amounting from two to four degrees, when a section of the peripheral part of the divided nerve was excised. As an example, I give the following case:

The spinal cord of a young suckling pup was severed on the 8th of December, 1873; the following day the lumbar portion was destroyed, and hereupon the right sciatic nerve divided. The temperature of the right hind leg rose to 34.1°, that of the left sunk to 21.5°, while the temperature of the blood had fallen to 37.7°. Upon excising a section of the peripheral extremity of the right sciatic nerve, it rose up to 36° in the right foot, while that of the left was reduced to 21°.

On the 23d of February the right sciatic nerve of a dog was cut, and on the following day the cord was divided, the temperature of the right hind leg 31°, the left 34.5°. Hereupon the right wound was re-opened, and the peripheral nerve stump was drawn out. During this operation the temperature of the foot rose to 31.7°, and after the removal of a further piece of the nerve to 38.5°.

9. In some cases, instead of removing a piece of the peripheral stump from the wound, I have hunted up a branch of the sciatic nerve, viz.: the anterior tibial, next to the internal malleolus, and divided it. In this procedure the original wound was therefore in no way disturbed; still I observed in every case a rise of temperature of the foot by several degrees.

The reader may object to this reasoning, that the vascular dilatation following section of the nerve cannot well depend on the irritation of the same, as irritation of the nerve could not but produce *vascular contraction.* The same objection I have raised myself also, but, not relying on the statements of authors, have experimented as to the effect on the temperature produced by stimulation of the sciatic nerve. Strangely enough, I have found that stimulation of the sciatic nerve by different agencies, does *not*, as is usually maintained, induce contraction of the vessels of the foot, but *always dilatation.*

I have in vain attempted to account for the contradiction between my experiments and those of other observers, but in whatever way I changed my experiments, I have only observed dilatation of the vessels to follow stimulation of the sciatic nerve of the dog.

10. In some cases I have laid bare the sciatic nerve, and, without severing it, irritated it with an induced current. As an example, I give the following case:

The cord of a quite young suckling pup was divided on the 7th of January, 1874, and thereupon the lumbar part destroyed with a probe, which operation the animal survived three days. On the second day after destruction of the lumbar portion of the hind legs, which had been very hot, immediately cooled down to 28°, and did not present any great difference in temperature from the fore feet.

In the mean time the temperature of the rectum had also fallen to 38.1°. The left sciatic nerve was now laid bare and irritated from three to four minutes with an induced current, without disturbing its position. While the nerve was being exposed, the mercury rose to 29.5° between the toes of the foot. Immediately after commencing the stimulation it fell for a moment to 29°; hereupon rising rapidly to 35°. The right foot remained as cool in the mean time as the fore feet. Fifteen minutes after the excitation the temperature of the left hind leg had again become reduced. About six hours later the difference between the right (30°) and the left hind leg (30.5°) amounted to but 0.5°; on the following day, the 10th of January, the temperature of the blood, as usually after the destruction of the lumbar cord, had sunken gradually to 33.3°. Now an experiment was made which rather belongs to the next category. The left sciatic nerve was again searched for, severed, and its peripheral end irritated with an induced current. The temperature of the left foot rose rapidly from 23.5° to 27°. On the same day the temperature of the rectum subsequently sunk to below 30°, and the animal died.

11. In a great number of cases I have placed the peripheral stump of the divided nerve on the electrodes, and irritated it continuously with an induced current. In every case I observed a rise of temperature. Only at the beginning of the excitation could we notice occasionally a reduction by some tenths of a degree. This however is naturally accounted for by the fact that in the beginning, the tetanus of the muscles pushes the thermometer out of its place. From the numerous experiments performed, I will but cite the following:

Having severed and partly destroyed the lumbar cord of a dog on the 23d of December, 1873, we divided the right sciatic nerve on the 28th of the month. Before the section was performed the temperature of both hind legs was the same; after the operation the corresponding foot became warmer by 11°, viz.: rising from 23° to 34°. On the 30th of December, two days later, the temperature of the right foot had again fallen to 24.5°. Hereupon the peripheral stump of the sciatic nerve was searched for and grasped with the forceps, during which procedure the thermometer, between the toes, rose to 30.8°. On placing it across the electrodes, and passing an induced current through it, the mercury rose rapidly to 34.2°, while the left hind leg remained stationary at 24°. Two days later, the 1st of January, 1874, the peripheral extremity of the nerve was again subjected to the same procedure. The temperature of the corresponding foot rose from 26.2° to 27° during the preliminary section, and reached 32° during the faradic irritation.

A dog's spinal cord was divided on the 24th of March, and his left sciatic nerve severed on the 8th of April. On the 13th of April both hind legs were remarkably cold, the right showing 17.2°, the left 17.5° (rectum 38.2°). On laying bare the peripheral end of the nerve, and irritating it violently with a faradic current, the temperature of the right leg rose to 32.8°; at the same time the left reached 24.5°.

In these cases the excitation of the sciatic nerve was performed only after the dilatation consequent on section of the nerve had disappeared. If the nerve is irritated soon after its division, a great increase of temperature could not of course be observed, as the maximum is almost reached by the division alone. Still the existing dilatation and heightened temperature is increased, even in these cases, as the following instance will show:

On the 8th of January, 1874, the cord of a bitch was divided, and four days later the left sciatic nerve was severed. The temperature of the left hind leg rose from 29.5° to 36.7°, immediately after the peripheral end was isolated and connected with the electrodes. On commencing to tetanize the nerve, the temperature sunk from 36.7° to 36.5°, and rose again during eight minutes' continuous tetanization, to 37.7°. After removing the electrodes the temperature of the foot continued rising, and reached even 38.2°. At the same time the right foot showed 28°, the rectum 39.5°.

12. As galvanic irritation proves often a source of error, I have confirmed the results by chemically irritating the nerve, by means of a saturated solution of salt, as follows:

The peripheral extremity of a nerve severed the previous day was dissected off as far as possible, and then dipped into a small cup containing a saturated solution of Na. Cl. As the preliminary steps in this operation will alone cause considerable irritation, the temperature might be expected to rise before the application of the chemical irritant. Thus, while preparing the nerve in one case the temperature rose from 22.3° to 32.3°. Hereupon the nerve was dipped into the solution. After the lapse of half a minute the mercury commenced rising, reaching 35.6° within three minutes.

In another case, similar to this, the thermometer rose from 28.5° to 32.2° during the application of the irritant.

13. I have also observed an increase of temperature similar to that produced by irritation of the divided nerve, after excitation of the lumbar cord.

On the 18th of February, 1874, the right sciatic nerve of a dog was divided, and on the 21st of the month the lumbar cord. The case took an unfavorable course, the wounds becoming purulent. February 25th the temperature of the rectum had sunken to 36.2°, and the legs felt remarkably cold. Both hind legs had a temperature of but 18°. The lumbar cord was now irritated with a strong induced current, the steel electrodes being introduced from above and from below. While the irritation was continued fifteen minutes, the temperature of the left foot rose from 18° to 26.5°; the right hind leg, however, as well as the fore paws, remained cold. The temperature of the rectum kept sinking, soon reaching 35.1°. The augmentation of temperature in the *uninjured* leg (8.5°) is of so much greater moment, therefore, as the temperature of the blood was rapidly falling. On discontinuing the irritation, the temperature of the left leg sunk rapidly, and in the evening all extremities were of the same temperature.

The cord of a young bitch was severed January 22, 1874, and the left sciatic nerve on January 28. February 2d the lumbar cord was irritated with an induced current, the temperature of the right hind leg being 23°, of the left 27°. During the excitation the temperature of *both* hind legs rose rapidly—on the right side to 32.1°, on the left to 34°. At the same time the temperature in the rectum rose from 40.5° to 41°. One hour later the right hind leg again became reduced to 24.5°, the left to 31.2°.

The striking difference between this case and the previous ones, is the rise of temperature observed also in the non-paralyzed foot after irritation of the lumbar cord; but this fact is easily accounted for. Though irritation of the cord can influence no longer the nerves of the *left* foot after section of the sciatic nerve at the level of the thigh, the excitation will extend to the entire intact region of the left lumbar plexus, and, besides, to all parts supplied by branches of the severed nerve, originating above the place of section. Thus, dilatation of the vessels in the *left* thigh is produced by the still existing nervous connection, and this increases the circulation in the foot on physical grounds. Besides, the rise of temperature of the mass of the blood cannot but be noticed also in the paralyzed parts. I have not been able to account for the absence of any increase of temperature in the case previously mentioned.

14. In one case I irritated the lumbar cord with a constant current, using for the purpose a Pincus battery of forty elements.

The right sciatic nerve of a dog was cut February 11, 1874. February 26, fifteen days later, the wound had perfectly closed, and all four feet were equally cool. The spinal cord was now cut. Half an hour later the right *paralyzed* hind leg showed but 20°, the left 36.5°. The fore feet were also very warm, having a temperature of 35°. The right hind leg, corresponding to the severed nerve, was therefore cooler, by 15° to 16° than the other extremities.

This experiment, and several others, seem to show that section of the cord augments cutaneous temperature not only in the hind legs, but also in the fore part of the body, and that this universal vascular dilatation is effected through nervous influence, as it was not apparent in the right hind leg, isolated from the central nervous system by a section of the sciatic nerve fifteen days previously.

During the next days following division of the cord, the temperature of the left hind leg sunk slowly, that of the fore feet rapidly. On the 15th of March all the feet were equally cool, and at this time the temperature of the blood commenced to fall from its previous height (40°). On the 7th of March the temperature of the blood amounted to but 36°. The lumbar cord was now irritated.

During the preparations for the experiment the temperature of the blood becomes reduced further to 35°. Before commencing the irritation of the cord the temperature in the right paralyzed hind leg amounted to 15.2°, in the left 15.8°. After the irritation had been continued for three-quarters of an hour the temperature of the right hind leg increased slowly to 18.4°, that of the left to 22.6°. At the end of the experiment the temperature in the rectum had fallen to 33.6°. After discontinuance of the irritation the temperature of the right hind leg sunk again rapidly.

This experiment is of special interest as a mode of irritation was employed productive of no tetanic contraction of striated muscles. The rise of temperature in the left foot by 6.8°, while the temperature of the blood had at the same time become reduced by 1.4°, is also very remarkable.

15. In a number of cases I have further examined what influence on the temperature of the other leg is exerted by irritation of the *central* extremity of the severed sciatic nerve. For this purpose the cord was previously divided to permit reflex action only in its lumbar part. In this case I have regularly found a rise of temperature by several degrees in the other hind leg. Irritation of the centripetal filaments of the sciatic nerve is therefore productive of vascular dilatation in the other leg by reflex action, for which the centre is found in

the lumbar part of the cord. As an example take the following case:

On the 25th of Jan. the cord and on the 29th the right sciatic nerve was severed. The central extremity of the divided nerve was now irritated with an induced current for ten minutes, the nerve being previously dissected towards the pelvis and placed across the electrodes. Before the excitation was begun the thermometer placed between the toes showed 29°. Immediately after commencing to irritate it fell to 28.9° and then rose again to 30.7° and after the removal of the electrodes to 31° In the meantime the temperature of the left hind leg had increased to 38°. The moderate vascular dilatation in the right leg, caused by reflex action, is thus quite inconsiderable when compared with the great rise of temperature in the left foot, dependent on section of the nerve. The following day, the 30th of Jan., irritation of the same central stump was repeated. Before the experiment the temperature of the right hind leg amounted to 22°, of the left to 35° (rectum 39.8°). The nerve was now taken out of the wound and irritated for fifteen minutes with an induced current. The temperature of the right foot now rose from 22° to 27.6°, and, after removal of the electrodes, to 28.2°. It then commenced to sink slowly and became reduced to 23.5° after one hour and a half. In the meantime the left paralyzed foot had cooled down from 35° to 34°.

In this case there was, therefore, an augmentation of temperature through reflex action by 2°.

The spinal cord of a dog was divided March 24th, the left sciatic nerve April 8th. The temperature on the 9th of April amounted to 18.5° in the right, 36° in the left hind leg. The central extremity of the divided nerve was now irritated. Immediately the temperature of the right foot rose to 24.6°—of the left sunk from 36° to 34°.

Observing the order of the experiments it must be granted that their results agree. We have seen that irritation of the sciatic nerve caused by mechanical, chemical, or electric agency, produces vascular dilatation in centrifugal regions. We can therefore assert that the sciatic nerve contains dilating vaso-motor filaments. We have further seen that irritation of the lumbar cord by means of an induced or galvanic current produces a similar result as chemical or other stimulation of the sciatic nerve. We can therefore assume that the dilating vaso-motor filaments take their origin, for the greater part at least, in the lumbar part of the spinal cord. We have finally found that the activity of these filaments can be also excited by reflex action, starting in irritation of the central extremity of the other sciatic nerve, and that the reflex centre is seated in the lumbar cord.

However well these facts agree amongst themselves the greater is the contradiction between my observations and those of other experimenters. It was unhesitatingly believed that irritation of the peripheral end of the divided sciatic nerve produced vascular contraction in the parts supplied. I maintain the opposite, having always witnessed vascular dilatation. In galvanic irritation of the cord many observers have found contraction of the arteries of the foot—we dilatation. Finally, according to Brown-Sequard, reflex action starting in irritation of the central stump of one sciatic nerve ends in contraction of the vessels in the other foot; according to my experiments, in dilatation. It would be out of place to attempt to reconcile these results with the theories of others until the latter have been proven. I have before maintained the hypothesis that the vascular dilatation observed after section of the sciatic nerve is an active process; which hypothesis seems so much more plausible after having proved by a number of experiments that the same can be produced by irritation. Taking this as a standpoint many facts appear in an entirely different light. I had previously defended the view that the rise of temperature in the hind legs after section of the spinal cord depends on a paralysis of vaso-motor influence. At present I should incline to the opinion that excitation of dilating vaso-motor nerves plays an important part in the process. I have also believed that the vascular dilatation consequent on destruction of the lumbar cord is dependent on the loss of the supposed vaso-motor centre it contains; but this would not explain the return of arterial tonus, which the assumption of a *temporary* (dilating) vaso-motor excitation satisfactorily accounts for. It cannot be denied that these deductions tend materially to overthrow the proofs which I have formerly urged for the existence of vaso-motor centres in the lumbar cord. Still I can yet maintain with much assurance that the lumbar cord is a centre for *nerves supplying vessels.* In proof of this I need but mention that this is the centre for the process of erection. Besides, I have demonstrated above that this is also the centre of a reflex action ending in vascular dilatation in other parts. The lumbar cord is therefore certainly *a centre for dilating vaso-motor nerves.* As to its connection with *contracting* vaso-motor nerves I can offer no proofs.

After having been forced to the conclusion that simple division suffices to excite dilating vaso-motor filaments, the usual interpretation of Claude Bernard's experiment on the cervical sympathetic seems to me decidedly suspicious. It is usually claimed that the vascular dilatation thus produced depends on a withdrawal of vaso-motor influence. But why is this withdrawal so temporary? Are there new nerve tracts formed within a few days? How easily is this difficulty overcome by assuming a temporary excitation of dilating vaso-motor filaments by the division. This however would not explain the contraction of vessels observed by many experimenters after galvanic irritation of the cervical sympathetic. If division acts as an exciting cause on the nerves, why is the irritation not heightened by galvanic stimulation, but strangely changed to the contrary? At present I can offer no answer to this question; but I would only remark that the cervical sympathetic has much more complicated relations than the sciatic nerve. If I sever the latter and irritate its distal end I have reason to suppose that all consequent phenomena are due to activity of its peripheral terminations. But if I divide the cervical sympathetic and irritate its upper end, I excite not only centrifugal fibres going to cranial vessels but also centripetal filaments entering the spinal cord and brain. Thus, complicated reflex processes can occur as the vessels receive other fibres as well as those from the cervical sympathetic. For these reasons experiments on nerves of the extremities seem to me more apt to lead to plain results. I will not therefore attempt to explain the appearances on irritation of the cervical sympathetic, merely maintaining that the *immediate* consequences of its section are due to irritation thus produced. Many other facts of this description refer probably to similarly *active* processes, as are the congestions of viscera after section of the splanchnic and hyperæmia of the eye after division of the fifth nerve.

Guenther observed in a stallion, incomplete erection of the penis after section of the pudic nerve. This experiment is usually understood to depend on vaso-motor paralysis; but to me it seems now more probable that a slight irritation of dilating vaso-motor filaments is the cause, and I would not give up this interpretation on account of its variance with Eckhardt's statements as to the course of the nervi erigentes. I have myself repeatedly observed a moderate distension of the erectile bodies of a dog's penis for a few days after section of the lumbar cord. This slight erection ceased soon, but could not be increased by reflex irritation. According to my opinion there was in these cases an irritation dependent on simple division of the cord.*

Certain other appearances in the organic nervous system, consequent on section of nerves, are probably also of an active nature. Dupuy saw in horses, after section of the cervical sympathetic, violent perspiration on the corresponding half of the head. It is also well known that the submaxillary gland secretes more energetically after section of the chorda tympani, probably from the irritation caused by the section.

The theory that section of a nerve is only productive of paralytic appearances, is so deeply rooted and exercises such influence in our ideas of nervous physiology, that I deem it worth while at present to multiply examples to overthrow it. In the sixth volume of this *Archiv* I have described some experiments on the influence of the vagus on the movements of the œsophagus of the frog. I have there demonstrated that simple division of the vagus suffices to produce energetic movements of the œsophagus, lasting for quite a time. On irritating the distal nerve extremity, the movements will become still more energetic. Here is therefore a case of phenomena consequent on section of a nerve similar to those produced by its irritation. The filaments of the vagus I consider, therefore, to react like the dilating vaso-motor fibres, contained in the trunk of the sciatic nerve, viz.: actively. It will not be out of place here to

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*Dr. Rud. Tauszky reports, *Wiener Med. Presse*, No. 31, 1874, a case of priapism, observed thirty-six hours after death, in the Vienna Pathological Institute. The subject had received a fall five days before death, resulting in fracture of the fourth, fifth, and sixth cervical vertebræ, crushing the cord, with consequent total paralysis, and *erectio-penis*, which latter depended on congestion, as it disappeared on opening the veins.—*Translator.*

mention an experiment which I related in the congress of naturalists, at Rostock, in 1871, with which I have not busied myself since. I killed a frog, of dark green color, by destroying the cord and brain, with a probe, through an opening in the skull, and thereupon excised the heart. The circulation and nervous centres were thus completely annihilated. I then cut the integument of the back, through its center, from head to coccyx, with a pair of very sharp scissors, by which I exposed a double row of symmetric nerve fibres, going from the cord to the integument. I now divided all cutaneous nerves on the right side, and found, in a short time, that the color of both halves of the body was not the same. The skin supplied by the severed nerves became lighter in hue, finally of a waxen yellow, while the other half remained green for a long time. The brightening of cutaneous color depends on the stellate cutaneous pigment cells, which assume a round form. This change of shape is therefore of more rapid occurrence when the nerves are cut. By many confirmatory experiments I have proved that the nerve section is the actual causal fact for the change of cutaneous color. I have further found that division by means of sharp scissors acts as an exciting cause; especially is this confirmed by the hastening of the process by repeated nerve-divisions. On irritating these nerve filaments with electricity, the change of color is also hastened. We have here a case in which section and irritation of a nerve are productive of analogous results, differing only in degree.

The belief that section of a nerve can never produce permanent appearances of excitation, has become so deeply rooted, mainly because nerves supplying striated muscles have been mostly the subject of research, in which experiments it was found that the corresponding muscles underwent one contraction and then fell into a state of relaxation. On this account the conclusion seemed natural, that a sharp division acts but as a momentary irritant. If we observe, however, the state of a divided sensitive nerve, we cannot but admit that it is in a more or less permanent condition of excitation. Thus when a nerve trunk containing sensitive filaments, for instance, the median or ulnar nerve, is cut in man, a sharp tinge of pain is experienced during the operation, after which, the muscles supplied by the nerve pass into a permanent relaxation, but even if no actual pain is felt thereafter, a vague sensation will always remind the patient that he possesses a wound, and that the affected nerve is in a state of excitation. This vague sensation is always perceived in the course of the nerve; for instance, in the fingers, in the case of the median nerve, and, doubtless, originates in the wound. Hence, it is apparent that sensitive nerves do not at all pass into an immediate passive condition after their division, but for a time are still in a state of moderate activity, conveying to the brain impressions apparently starting in their peripheral distribution. Is it therefore probable that motor filaments, which, as far as we are aware, do not differ from sensory fibres, pass immediately into an inactive condition, after being severed from their origin? Not at all. Surely the motor filaments remain in a state of moderate excitation, but this is not manifested at their peripheral termination, by the striated muscles, as these are too inert an apparatus to be stimulated to activity by so feeble an excitation. The ganglionic cells, however, of the brain, the seat of consciousness, constitute a most delicate index, which responds to the slightest impression conveyed by the nerves, by activity, perception. The peripheral ganglia of the vessels seem to me possessed of similar sensitiveness as the cells in the brain, and hence are easily affected in their activity by as slight a nervous influence as that called into action by the division of the nerve.

Besides, I would refer here to a fact related by Schiff, which would at least indicate that in some cases even striated muscles will react to this feeble stimulus. After section of the hypoglossal nerve, Schiff saw a continued trembling of the muscles of the corresponding half of the tongue, which probably depended on the excitation of the nerve, by its division.

If my views gain ground, it will be evident that each excised section of a nerve does not represent an inactive tissue, but a nerve in a state of moderate excitation. The passive nerve current of Du Bois Reymond would therefore be a current enfeebled by negative variation.

On concluding these remarks I entertain the hope of having converted the reader to my views. At any rate I believe I have proven, that the previous doctrine of the nature of vaso-motor innervation, is hardly adequate to account for the facts observed. If any one, however, will bring forth a hypothesis better calculated to explain the results of these researches, I shall not hesitate to accept the same. I will yet state that in our laboratory, a new series of experiments is being performed, to throw more light on some of the points mentioned in this essay.

Art. VI.—NOTES ON THE NITRITE OF AMYL.

By J. Crichton Browne, M. D., F. R. S. E.

From the Practitioner.

IN June, 1873, when administering the nitrite of amyl to a girl who was in the *status epilepticus*, and had been unconscious for some hours, I was much struck by the fact that in about a minute after the inhalation was commenced, there was what may be called an attack of yawning. The patient yawned profoundly and repeatedly. Never having before witnessed yawning during a state of coma, it at once occurred to me that that modification of respiration in this case must have been induced by the nitrite of amyl, which always when inhaled hastens and deepens breathing. That inference I was able to corroborate forthwith. Whenever the inhalation was interrupted, the yawning ceased; whenever it was resumed, the yawning recommenced. In another case of the *status epilepticus*, which occurred about a month afterwards, similar phenomena were observed. The patient, although so completely comatose that no responsive movements followed on tickling the soles of the feet, or pricking the toes, immediately yawned in the most ordinary way, as if just upon the verge of much-needed sleep, whenever a piece of lint, soaked in nitrite of amyl, was held before the nose and mouth. When that was done, two or three full inspirations were succeeded by one of extreme depth, accompanied by depression of the lower jaw and elevation of the ribs and scapulæ. The experiment was repeated many times, until it was quite evident that the crowning expression of drowsiness was induced by the nitrite of amyl. Some months later, Dr. Herbert Major again noticed yawning in a general paralytic patient, who was quite unconscious and at the point of death, and to whom he was administering the nitrite of amyl with a view of securing a temporary rousing. More recently, on the 24th of July last, the same effect of the nitrite of amyl was seen well exemplified. Two male patients were at that time prostrated in the *status epilepticus:* one of them, M. C., had passed through some hundreds of fits in the course of three days, and was permanently unconscious and much exhausted; and the other, J. A. M., had had nineteen fits in twenty-four hours, and was in a condition of stupor, taking no notice of what was going on around him, and making no spontaneous movements. At 2 p. m. nitrite of amyl, which had been tried at the beginning of the outbreak without perceptible benefit, was again given as an inhalation to M. C., who was then *in extremis*, lying with livid features, pin-point pupils, a pulse of 116, and a temperature of 102°, and breathing stertorously. When the inhalation had gone on for fifteen seconds, there was a voluntary movement of the right hand and an attempt to raise it, and this was speedily followed by acceleration of the respiratory movements and then by distinct yawning. The patient yawned six or seven times consecutively and prodigiously, and then turned his head and emitted a kind of sob. While the yawning was going on the pulse was somewhat quickened, and a dull flushing of the head and face became visible. The flushing traveled about half-way down the neck, but not further, and no blotches appeared on the chest, abdomen, or limbs. Three minutes after the inhalation a copious perspiration burst out over the forehead, face, and neck. At the same time an inhalation was administered to J. A. M., who could not be awoke out of his heavy slumbrous torpor. In ten seconds he likewise moved his right hand, and in fifteen seconds he likewise began to yawn. It would perhaps be more correct to say that he manifested a tendency to yawn. He opened his mouth to the widest possible extent several times in succession, but with that movement there was no prolonged inspiration, no raising of the shoulders, and no characteristic sound. With the movements of the jaw came obvious flushing and considerable reanimation. The patient looked about him, lifted his head from the pillow, and seemed more alive to surrounding circumstances than he had been for twenty-four hours previously.

Two days subsequent to these observations on M. C. and J. A. M., my colleague, Dr. Merson, who had witnessed them, was administering the nitrite of amyl to a retriever dog that had been in continuous convulsions for some hours, when he was much surprised to notice in it precisely the same movements that he had seen in J. A. M. Whenever the amyl was held before the nostrils of the animal, which at the time was quite unconscious, it beat the ground with its fore paws, and opened and closed its mouth with rhythmic regularity. To make quite sure that these movements were not accidental or due to some other cause, Dr. Merson interrupted and resumed the inhalation many times. He found that invariably when it was resumed after an interruption these movements occurred, and that they were not induced by other kinds of stimulation.

These phenomena, as observed in the dog and in patients in a state of coma, seemed to me to be highly interesting, and to point to some hitherto unsuspected action of the nitrite of amyl. Having administered that agent to hundreds of patients in a state of consciousness, I had never seen yawning included amongst its effects; and having searched its literature, I found no record of any such action having been produced by it. That the yawning, complete or partial, in the cases described, and in the dog, was really ascribable to the nitrite of amyl, is evident, I think, from the nature of the observations made and referred to, as well as from the general observation that yawning is not known to occur in states of coma or unconsciousness due to disease in which nitrite of amyl has not been administered. Arising ordinarily out of a sense of fatigue or an oppression of the respiratory organs, it does not occur during morbid fatigue nor pulmonary engagement. I have watched innumerable cases of coma with great exhaustion, and every degree of lung congestion, and in none of them except those in which the nitrite of amyl was used have I seen yawning. That the yawning was due to some specific effect of the nitrite of amyl, and not to a mere general stimulation of the pulmonary mucous membrane under certain conditions, is deducible from the fact that other stimulants under like conditions do not produce the same effect. Thus chloroform, ether, and ammonia, given by inhalation in states of coma, do not bring on yawning as the nitrite of amyl does.

Curious to trace out further the action of the nitrite of amyl when inhaled during unconsciousness, I have during the past month administered it in this way to eighty-seven persons when sleeping, and to many of these upon several occasions. Great difficulties of course obstruct the attainment of any definite results in this way, as the most general and immediate consequence of the inhalation of the nitrite of amyl during sleep is sudden waking. At the moment that the blush appears upon the face, the person operated upon starts up, makes a number of voluntary movements, and, having passed through a few seconds of bewilderment, not unnatural after so rude and extraordinary an interruption of repose, is fully alive to all that is going on around. With great care, however, and the use of small quantities of the nitrite, the inhalation may be carried out and its full effects developed during the continuance of sleep. My most successful observations have been made upon epileptic patients, who sleep heavily and are not easily disturbed. Altogether I have been able to make fifty-seven satisfactory observations; that is to say, I have succeeded fifty-seven times in administering the nitrite of amyl without waking the patient, and in noting the effects which followed the inhalation. It will certainly be regarded as remarkable that in forty-two out of these fifty-seven observations there were recorded distinct movements of the mouth, consentaneous with blushing of the countenance. In only one out of the forty-two observations in which movements of the mouth are reported did yawning occur. That observation was made upon a lad who was attacked by a severe fit when asleep, and to whom the inhalation was administered about five minutes after the fit, while his features were still livid and his breathing stertorous. No sooner had the flush overspread his face than he turned upon his back and yawned to the top of his bent five times, then falling again into a heavy sleep. In none of the other forty-two observations is yawning recorded, but in all of them movements kindred to yawning were noticed; that is to say, in all of them there were movements of the mouth. These movements were exceedingly various, the most common, however, being a short munching movement of the lower jaw, which was depressed and elevated as if in the act of chewing. The next most common movement was a smacking of the lips, as if in the act of tasting. It will perhaps be as well to quote from my note-book a few illustrations of the movements observed. In twenty-eight of the successful observations, movements of the hands, antecedent to or contemporaneous with those of the mouth, were noted.

1. Mary C., epileptic, *m* 5. In twenty seconds, deepened breathing; in thirty seconds, distinct flushing; in thirty-five seconds, extension of both hands; in thirty-eight seconds, munching movement of lower jaw and movement in throat, as if swallowing was going on.

2. Ann W., epileptic, *m* 8. In ten seconds, deepened breathing; in fifteen seconds, slight flush; in twenty seconds, deep flush; in sixty seconds, munching movements of the lower jaw, kept up for thirty seconds, when sudden waking occurred.

3. Emily L., epileptic, *m* 10. In five seconds, deepened breathing; in nine seconds, flush, and the right hand suddenly raised above the head, after which munching movements of the lower jaw began.

4. Elizabeth W., epileptic, *m* 15. In ten seconds, quickened breathing; in twelve seconds, movement of the right hand; in fifteen, distinct smacking of lips, continued for some time.

5. Jane A., epileptic, *m* 5. In twenty seconds, deep flush, then short cough, then smacking of the lips.

6. James G., epileptic, *m* 10. In ten seconds, flushing of the face; in fifteen seconds, retraction of the lips, then loud grinding of the teeth.

7. Paul G., epileptic, *m* 10. In ten seconds, deepened breathing, becoming audible and stertorous, then movements of the lips, then swallowing, and then muttering and mumbling.

8. Job L., epileptic, *m* 5. In ten seconds, deep flush, with blowing movements of the lips, followed by movements of the hands.

9. Thomas H., epileptic, *m* 5. In five seconds, deepened breathing; in ten seconds, smacking of the lips, with movements of the hands, after which he turned over in bed.

10. Thomas W., epileptic, *m* 5. In ten seconds hand raised to head, over which it made a rubbing movement; in fifteen seconds, munching movements of the mouth.

The same patients in whom these movements were observed were also caused to inhale during sleep ether and aromatic spirits of ammonia, with the view of testing whether other stimuli applied to the respiratory tract would induce like movements. It was found that ether, cautiously administered, merely deepened sleep, without producing any movements whatever; and that when movements were occasioned by it, these usually commenced around the eyes, and not the mouth. Ammonia always awoke the patient, the first movement being a sudden and violent toss of the head backwards.

As the result of these observations, it appears that nitrite of amyl, when inhaled during a state of unconsciousness, has a specific action upon the motor centre of the mouth, and calls into action, by preference, the muscles of the lips and lower jaw. The mode in which it performs this action, whether reflexly, or through the agency of the vaso-motor apparatus, is as yet only a subject of speculation. The fact that the movements which it evokes are consentaneous with the appearance of flushing of the face, gives probability to the latter hypothesis; while, on the other hand, one observation, that when administered hypodermically the nitrite failed to induce yawning in a case in which it had induced it when inhaled, seems favorable to the other view. But whatever may be the explanation of the action in question, there can be no doubt that it is a significant fact, and worthy of further investigation.

Reviews and Bibliographical Notices.

I.--THE MECHANISM OF THE CENTRAL NERVOUS SYSTEM.

I. HUGUENIN, DR. GUSTAV. ALLGEMEINE PATHOLOGIE DER KRANKHEITEN DES NERVEN-SYSTEMS. Ein Lehrbuch fuer Aertze and Studirende. Von Dr. Gustav Huguenin. I. Theil: Anatomische Einleitung. Mit 130 Holtzchnitten. Zurich: 1873. 296 pages. *(General Pathology of Diseases of the Nervous System. A Manual for Physicians and Students. Part I. Anatomical Introduction, &c.)*

II. MEYNERT, DR. THEODOR.

1. BRAIN OF MAMMALS. Stricker's Handbook of Histology. English Translation. Page 650-766.

2. ANATOMIE DER HIRNRINDE U. IHRER VERBINDUNGSBAHNEN, MIT DEN EMPFINDENDEN OBERFLAECHEN UND DEN BEWEGENDEN MASSEN. *Leidesdorf's Lehrbuch der Psychischen Krankheiten.* Erlangen, 1865. Pages 45-73. (*Anatomy of the cortex of the brain and its mode of connection with sensitive surfaces, and the apparatus of motion; in Leidesdorf's Manual of Mental diseases.*)

3. BEITRÆGE ZUR THEORIE DER MANIAKALISCHEN BEWEGUNGS ERSCHEINUNGEN NACH DEM GANGE UND SITZE IHRES ZUSTANDEKOMMEN. (*Arch. fuer Psych. u. Nervenkrankheiten.* Band II. Heft 3. P. 622, 1870.) (*A treatise on the theory of maniacal motor-phenomena, according to their course, seat and the conditions on which they depend, etc.*)

III. LUYS, M. J. RECHERCHES SUR LE SYSTEME NERVEUX CEREBRO-SPINAL, SA STRUCTURE, SES FONCTIONS, ET SES MALADIES. Par J. Luys. Paris, 1865. Atlas de Quarante Planches.

In the past few years, two notable attempts have been made to declare the mechanism or interior plan of structure of the central nervous system The first in order of time was that of Luys, the title of whose work will be found above, and the last is that of Prof. Meynert, of Vienna. Both have had, and now have, students of and adherents to, their views. Especially is this true of the latter. The work of Dr. Huguenin, of Zurich, the title of which stands at the head of the above list, is by one of Prof. Meynert's students. But he is something more than a student. He would seem to be a critical expounder of the system of his master, from whose views he has not hesitated, in many respects, to dissent. It is to a brief survey of the contents of the first few chapters of this work that we would, in this article, more particularly invite the attention of the reader. But this will hardly be possible without describing in outline the system of Luys, as well as Meynert's, whose system is adopted in the main by Huguenin.

Luys followed the plan of making a vast number of sections of the prepared nervous substance of all parts of the central nervous system, and of studying them under low powers of the microscope, in due order, and then of combining the items of information obtained in this way, so as to form a scheme of distribution of the histological elements of the same. The following is an outline of his system :

First we have the masses of the central nervous system, the ganglia, variously divided, consisting chiefly of nerve cells. Belonging to these, and completing the nervous apparatus, we have, according to M. Luys, four great systems of nerve fibres, besides simple commissural fibres: Two of them are called *converging.* These are called respectively *inferior* and *superior.* The former includes the whole of the so-called sensory peripheral nervous system. All cerebral and spinal sensory nerves (not to mention the sympathetic) throughout, belong to this class, both in the spinal cord and out of it. From all parts of the body, with few exceptions, this class of fibres converge on the spinal cord, and ganglia at the base of the brain, more particularly the latter. This, as we have said, is the *inferior converging system*

The *superior converging system* consists of all those fibres which begin in the cells of the cortical substance of the hemispheres, and converge toward the base of the brain on certain ganglia there situate.

As the *inferior* converging system of fibres is destined to convey impressions from all or nearly all non-nervous parts of the body—impressions physical in their nature or origin—so on the other hand, the *superior* converging system of fibres is destined to convey impressions made at the peripheral end of this system of fibres, perhaps mental instead of being physical in their nature and origin. Both these systems converge at the base of the brain. Both systems at their periphery, have peculiar forms of apparatus, suitable for receiving the impressions adapted to them. On the one hand we have the peripheral apparatuses, of vision, hearing, smell, taste, touch and so on, on the other the apparatuses of thought, lodged in the cortex of the brain.

The other two are the *diverging* systems of fibres, also called *inferior* and *superior.* The former—the *inferior* diverging system of fibres—proceeds from the motor cells of the cord and ganglia at the base of the brain, and leads outwards from these centres toward the periphery of the body. The motor fibres to the muscles, and to all glandular structures whose actions are normally influenced by the nerves, belong to this system.

The latter—the *superior diverging* system of fibres—leads from certain of the ganglia at the base of the brain upward to the cortical substance of the hemispheres. The fibres of this system are destined to convey impulses or impressions from the great sensory ganglia upwards to the cells of the cortical substance, which latter are in this way excited to action, just as truly as the motor-muscular nerves convey impressions to the muscles that make them act.

To these must be added certain tracts of commissural fibres like those of the *corpus callosum*.

Such, in broadest outline, is the system of Luys. To carry a view of it out into detail, would not be possible here, nor consistent with our present plan, the aim in which is to give simply an outline.

The work of Dr. Huguenin opens with a short but very excellent chapter on the "Development of the Brain and Spinal Cord." The next is devoted to a "General Scheme of the Structure of the Brain and Spinal Cord."

This is the part of the work to which attention is chiefly invited in this notice, in connection with corresponding parts of the memoirs cited from Meynert. If we should take the brain and remove the hemispheres, excepting the ganglia at their base, and then should remove the cerebellum, we would have left above the spinal cord proper, what is called in the system of Meynert, the "brain-stem," (Hirnstamm) or in more common language, the "peduncles" of the brain. It includes in the system of Meynert, the crura cerebri, the corpora striati, the thalami optici, corpora quadrigemina, and the whole of the medulla oblongata. Spread out above this is the "brain-mantle" (Hirnmantel) or cortical layer of the cerebral hemispheres.

After making the ordinary macroscopic distinction of the nervous substance of the nerve centres, into the white and gray, Dr. Huguenin follows Meynert in dividing the gray matter of the central nervous system into four "categories" or groups. They are as follows :

1. "The superficial gray substance of the hemispheres out of which, collectively, the descending fibres take their origin.

2. "The gray substance of the great ganglia at the base of the brain. Here it is to be remarked that these masses are to be divided into two categories, or those which stand in connection with sensible or sensitive surfaces, and those which do not. This lays down the radical functional distinction to be made between the ganglia of the second category, viz.: motory and sensory.

3. "The so-called 'gray tube' or canal (Roehrengrau), of Meynert, an immediate production of the 'medullary tube' and the 'brain sac.' It invests the central canal of the brain and spinal cord, which extends from the infundibulum through the

aqueduct of Sylvius, and the space (calamus scriptorius, Rautengrube) on the posterior surface of the medulla oblongata, corresponding to the fourth ventricle, and so on through the whole length of the central canal of the spinal cord, and is known as the gray substance of the medulla oblongata and spinal cord.

4. "The gray substance of the cerebellum and its appendages, and which has manifestly a structure wholly peculiar, and is devoted, without doubt, to peculiar functions.

"These different categories of gray substances stand connected together by means of a vast number of bands of nerve fibres," &c. To understand Meynert's scheme of the nervous system, even in its outlines, it is necessary to fix these categories of gray nerve substance clearly in mind. Dr. Huguenin next proceeds to declare the great systems of fibres, which, according to Meynert, connect the masses of gray nervous substances with each other in the order of their importance, and in view of the functions they are known or supposed, to subserve.

To survey the above-mentioned system of fibres we must begin with the first category of gray matter, or the cortex of the brain.

Extending from the cells of every part of the inner surface of the cap formed by the cortex of the cerebral hemispheres, are vast numbers of fibres, which are divided at once into two classes:

1. Those which serve to connect different parts of the same hemisphere together, or of the opposite hemispheres, and which are called the "association, or arch system," (bogensystem). These fibres exist for the purposes of securing concerted and harmonious action between different parts of the brain. They are the commissural fibres of our older works.

2. Those which do not terminate as they began, in the cells of the cortex of the hemispheres, but converge at the base of the brain on certain ganglia there situate, in the cells of which ganglia the fibres in question chiefly terminate. These fibres include the superior converging and diverging systems of Luys. They constitute the "projection system," No. I, of Meynert. The fibres of this vast "projection system" are wholly confined to the *interior* of the cerebral hemispheres, and constitute what is called ordinarily the white or medullary matter of the hemispheres.

At one end, as we have seen, these fibres terminate in the cells of the cortical substance of the brain; at the other, with certain exceptions, in the great ganglia at the base of the hemispheres.

Leaving the gray substance of the first category, or cortex of the cerebral hemispheres, we will descend to the great ganglia at the base of the brain, or category of the second kind. These are not only the lower termination of the fibres of the first projection system, but they constitute the origin of the fibres of projection system No. II. The number of fibres composing this second system is very small as compared with that of the first system, already described. Hence these ganglia of the second category constitute a "reduction region" (reductiongebiete), or a medium for reducing the number of fibres that shall pass downward into the medulla oblongata to constitute the second projection system. In other words, the fibres which descend from the cortex of the brain, and are projected into the upper surface of the great ganglia at the base of the brain, terminate, as a rule, with certain exceptions, in the cells of these latter ganglia. It is only by means of the cells of the ganglia (of the second category) that the fibres of the first projection system become connected with those of the second projection system. Hence the ganglia of the second category are called "interruption masses" (unterbrechungsmassen). The fibres which enter the ganglia in question are interrupted in their course as fibres by the interposition of cells. The cells receive a vast number more fibres from the first projection system than they give off to the projection system No. II. Hence this region, as already remarked, has been called by Meynert, a "reduction region" (reductiongebiete), &c.

The ganglia of the second category are the thalamus opticus (sehhuegel), corpus striatum (streifenhuegel), the nucleus lentiformis (linsenkerne). These three ganglia lie in pairs, separated by the space called the third ventricle. Finally we have the corpora quadrigemina (vierhuegel), lying behind the first mentioned ganglia, and, unlike the others, closely united on the middle line.

The second system of nerve fibres, (Projection System No. II.,) springs from the aspect of the ganglia, just mentioned, opposite to that of the cortex of the cerebral hemispheres. Some of the fibres of projection system No. II, terminate in various points in the medulla oblongata, such as the centres for the motor nerves connected with the special senses, or for the control of peculiar muscular groups, such as the centres for the facial, or hypoglossal, or pneumogastric, or other nerves; or in various parts of the gray central substance of the spinal cord (Hoehlengrau), all the way from the medulla oblongata to the lower end of the spinal cord.

The great mass of the fibres of this second projection system, after they depart from the ganglia at the base of the brain, descend, under the name of the *crura cerebri* or *peduncles of the cerebrum*. At this point, a capital feature of Meynert's system comes in. He divides the fibres of projection system No. II, into two classes:

First. Those which arise in the *nucleus lenticularis* and the corpus striatum, and convey the orders of the will downward toward the muscles, and, *second*, those fibres which proceed downward from the thalamus optici and the corpora quadrigemina, and do not convey the orders from the will, but reflex impulses, of which the ganglia, just mentioned, are the seats. These two great divisions of the "brain-stem" (Hirnstamm) are called respectively, the "basis cruris cerebri," and the "tegmentum cruris." Let it be remembered these two divisions of the "brain-stem" contain the fibres, both sensory and motor, of projection system No. II. The fibres of the "basis cruris cerebri," which convey downward the orders of the will, from the corpus striatum and ganglionic masses associated with it, as they pass downward, terminate below, as already said, some of them in special motor centres, lodged in the pons varolii and medulla oblongata, to be switched off in various directions, while the bulk of them pass lower down, and decussate, probably, in the medulla oblongata, and enter into the opposite lateral column of the cord, to terminate finally in the cells of the gray matter of the spinal cord, at various points, even down to its lower part it is probable.

The fibres that compose the "tegmentum cruris," pass downward also, and as they do partly, at least, enter the antero-lateral column of the spinal cord of the same side, and, in part, cross over in the medulla oblongata, to the opposite half of the cord. They terminate, at various heights, in the gray matter of the medulla oblongata and cord. Whether the fibres descending from the "basis cruris" and the "tegmentum," terminate in different tracts of the gray matter of the cord, is still, in a measure, a matter of inference. So much, then, for the fibres of projection system No. II, which serve to connect the great ganglia, at the base of the brain, with the small ganglia, in the pons, medulla, and the gray central matter of the whole length of the spinal cord.

We repeat, that the fibres which pass down in the *basis cruris cerebri*, in the manner described, convey the orders of the will on their way from the cortical substance of the hemispheres toward the voluntary muscles, and that the fibres which pass down in the "tegmentum cruris" become the conductors of reflex impulses, which are excited by sensory impressions, transmitted to the optic thalami and corpora quadrigemina, by means of the cranial and perhaps other sensory nerves.

So much, in brief, as to the disposition of the fibres composing projection system No. II. Next in order, we have projection system No. III. It is composed of all those fibres which connect the gray matter of the spinal cord and medulla oblongata, with the non-nervous, motor and sensory parts of the body, such as muscles, membranes, etc., and which takes the name, commonly, of the peripheral nervous system. It embraces both the sensory and motor nerves of the whole body, excluding, perhaps, some of the nerves of special sense.

A few words must be said as to the gray matter of the spinal cord. In the opinion of Meynert, the gray matter of the cord had, originally, the form of a prolonged tube (Hoehlengrau—Roeh engrau), of course hollow in its interior. This tube extends from the lower end of the cord, upward to the medulla oblongata, at which point it is laid open, from behind or above, so as to display its interior, constituting what is called the floor of the fourth ventricle, and thence upward and forward, along in the base of the brain, through the aqueduct of Sylvius and the third ventricle, lining its sides and floor, and as far forward as the infundibulum, where it terminates. This tube receives the fibres of projection system No. II, and gives off the fibres of projection system No. III. About its upper or cephalic end, and in close relations therewith we have grouped the special ganglia of the second category of gray matter, already described.

The fibres of projection system No. III, exceed in number those of system No. II, in a degree quite equal to those of projection system No. I. The gray matter of the "medullary tube" constitutes a region in the central nervous system, for the *multiplication* of nerve fibres, in contrast with the ganglia of origin of the fibres of the second projection system, which constitute, as we have seen, a "reduction region" (Reductiongebiete).

To restate, briefly, the general plan of structure of the central nervous system, as set forth by the school of Meynert: We begin with the gray matter (cells), of the cortex of the cerebral hemispheres, and (putting the association system out of sight, for the moment) find a great system of nerve fibres arising therein, to converge on certain great ganglia at the base of the brain, in which ganglia, the fibres, as a rule, terminate, by becoming connected with nerve cells. The fibres are thus interrupted in their course, and, by this means, they are enabled to form, through the agency of the cells, a complicated system of connexions with special groups of nerve cells, in various directions. The ganglia in which this interruption of the continuity of fibres is effected, are called, in Meynert's scheme, "interruption masses" (Unterbrechnungsmassen). From these ganglia a second set of fibres spring, which pass downward in the *basis cruris* and *tegmentum*, to terminate in the cells of the *gray medullary tube*, which occupies the axis of the spinal cord, and parts above. This is projection system No. II.

Then the cells of the gray medullary tube give off the peripheral nerves, both sensory and motor, to constitute the projection system No. III.

Now we come to projection system No. IV, or that which connects the gray matter of the cerebellum (fourth category of gray matter), with the three other categories of gray matter, namely: the cortex of the cerebrum, the ganglia at the base of the brain, and the gray medullary tube of the cord, &c. As regards the cerebellum, Meynert looks upon it as an apparatus wholly peculiar, as indicated by its position and structure, and deserving a separate consideration. The fibres that pass or are projected out of the cerebellum, are divided into three pairs of bundles, called *crura*, or *peduncles*, which, as is well known, are named according to their respective positions—superior, middle and inferior. The former connect the cerebellar hemisphere of one side with some part, at present unknown, of the cortex of the hemispheres of the opposite side. The middle crus or bundle passes forward into the pons varolii, where its fibres enter into connection with nerve cells that belong to the second category of gray matter at the base of the brain, and the third bundle passes forward and downward to terminate in the cells of the third category of gray matter, represented in the gray medullary tube of the cord. This third bundle from the cerebellum, before it enters the cord, divides into two parts. The first (corpus restiforme) becomes continuous with the anterior column of the spinal cord, perhaps of the opposite side, while the second (funiculus cuneatus and gracilis) by a very circuitous course, becomes continuous with the posterior column of the spinal cord.

So much for the system of Meynert in outline, not only as set forth by himself, but by Dr. Huguenin, who adopts Meynert's views in the main.

As regards the word "projection," as applied to the different classes of fibres. it would seem to have one of two significations, perhaps both. It either has reference to the fibres themselves as being "projected" from the masses of gray matter in which they take their origin, or, what is more likely, it has reference to the centripetal impulses, which, made at the sensory surfaces, pass inward until they arrive at the cortex of the cerebral hemispheres, against which they are finally "projected," either to be retained there through some incidental material change in the cortex, or to be reflected back again towards the periphery, for purposes of motion.

As regards the function of the cortex in general, Meynert holds it to exist—at least we so understand him—for the purpose of storing up in some way the ingoing "impressions" projected into it. He does not consider it a part of the nervous system, for the origination of impulses from the activity of the mind and of communicating the same to other parts of the nervous system.

There is one point in this connection that we will mention as possibly having some bearing on this subject.

Meynert holds it as at least probable that the retina of the eye, as well as the olfactory bulb, is essentially a detached portion of the cortex of the cerebral hemispheres. If so, the way in which these portions of cortex receive their impulses may throw some light on the source of the impulses that arise in the cortex of the hemispheres. Take the retina for example: In this case, it would seem, if it is to be regarded as the analogue of the cortex, that its impulses are received directly from the outer world, light being the agent for producing them. It takes cognizance of the wonderfully delicate pulsations of the "ethereal medium," of which light is supposed to be a result. The retina does not exist for "storing up" the pulsations in question, but for *receiving* and transmitting them. If this is so why may we not infer that the cortex is similary constituted? But, if so, where do the impressions come from that are to be compared with those made on the retina? Some would answer readily enough, from the free and independent activity of the mind, the will, but not so Meynert. But we must pass by reflections like the above, to be indulged at another time, if at all.

It is entirely apart from our present purpose to enter at this time more into details, to do which satisfactorily would require a long series of articles. For the views of Meynert are most carefully worked out, in view, not only of the nature and visible disposition of the histological elements of the nervous system, as seen by the microscope in man, and all classes of lower animals duly compared, but in view of the outer form and gross appearances generally of the nervous system, of its mode of development, of the results of physiological experiment, and of pathological observation. It may be readily imagined that where so much remains yet to be done, and where materials so heterogeneous have been employed, that the system embraces in its various parts many things uncertain and hypothetical, and that will have to be modified or rejected with the advance of knowledge. But, no one would seem to be more sensible of this state of things than Professor Meynert himself. But of all the attempts thus far made to declare the inner architecture of the central nervous system, this of Meynert's is certainly the most noteworthy and comprehensive.

To what extent it is successful will shortly be shown by the practical use that can be made of it in analyzing and interpreting the problems of pathology that daily arise in clinical experience. But this test of utility is already being applied by Meynert, and is promised soon on his part by Dr. Huguenin, to whose practical work on diseases of the nervous system, the monograph whose title stands at the head of this article, is simply the "anatomical introduction." Hereafter we shall fully and plainly consider Meynert's views, in their practical as well as scientific relations.

In this notice of the outlines of his system, we have endeavored to meet what has seemed to us a practical need on the part of many readers, who seem to have found it difficult to comprehend the somewhat novel and abstruse views of the great Vienna neurologist, which seem to us to have a high degree of practical importance when they are once understood.

II.—FRIEDREICH: PROGRESSIVE MUSCULAR ATROPHY.

Ueber Progressive Muskelatrophie; Ueber Wahre und Falsche Muskelhypertrophie. Von Dr. N. Friedreich, Prof. der Med. in Heidelberg. Mit 11 Tafeln. Berlin: 1873. Quarto; 358 pages. (*On Progressive Muscular Atrophy and on True and False Muscular Hypertrophy, &c.*)

This elaborate and masterly monograph, on a class of diseases of singular interest, is comprised in eleven chapters. The first seven are devoted to progressive muscular atrophy; the eighth, to a consideration of the relation of pseudo-hypertrophy of the muscles to progressive muscular atrophy; the ninth, to the relation of bulbar-paralysis to progressive muscular atrophy; the tenth considers the relations of the latter form of disease to bone atrophy, while the eleventh, and last, is occupied with a consideration of true muscular hypertrophy.

The present work is not simply an elaborate and critical discussion of the literature of the subject, but a solid and extensive contribution to our knowledge of the important forms of disease of which it treats. We shall endeavor, in this notice, to place our readers in possession, as far as we can in so brief a space, of the views of our author in regard to the subjects in hand.

For some years past, he has been publishing the results of his studies in Virchow's *Archiv*, but in this work we find them all summed up with a large amount of new matter. The original part of the work, as regards matter, consists in a critical study of twenty-five cases, involving the various forms of disease falling within the plan of the work. The survey of the literature of the subject appears to be exhaustive and complete, up to the date of publication. The eleven plates or drawings exhibit a high degree of artistic finish and skill.

In the introduction the author, first of all, seeks to separate those muscular atrophies which depend on simple disuse, or on a general damage to nutrition, such as happens in tuberculosis, diabetes, or in the course of typhus or various wasting forms of disease, from those cases which are confined to groups of muscles, and occur in connection with—perhaps as a consequence of—disease of the nervous system.

In such cases, in the words of the author, "the question in the foreground has always been, whether, in progressive muscular atrophy, we have in the disease of the muscular apparatus to do with changes that depend on disordered nervous influence, or that depend upon and are but the expressions of a primary disease of the nutrition of the muscular tissue; or if, in one word, we have to deal with a *neurosis* or a *myopathy*."

Into the discussion of this question, our author enters with remarkable fullness, and more than ordinary penetration and powers of analysis and generalization.

Dr. Friedreich begins with the case first reported by Cruveilhier in 1832, who believed it to be one involving disease of the anterior columns of the cord, but the autopsy showed the cord and brain to be healthy. Also the same celebrated observer reported, in 1848, two other cases in nothing ("rien, absolument rien") abnormal as regards the cord or brain. These cases led Cruveilhier to the opinion that the essential seat of the disease must be either in the motor nerves, or in the muscular tissue itself, an opinion that was afterwards modified so as to admit atrophy of the anterior roots of the related nerves.

He then passes to the positions taken by Aran and Duchenne, that the disease is not a neurosis, but an idiopathic muscular disease, designated by such phrases as "atrophie musculaire progressive" and "atrophie musculaire avec transformation graisseuse." Subsequently, however, Duchenne abandoned this earlier opinion, and adopted the neurotic view, in which he, with Joffroy, fixed the essential seat of the disease in the anterior horns of gray matter of the spinal cord, more especially in the large ganglion cells there found. The disease was of the nature of an atrophy of the cells in question. Thus quite early were observers divided into two parties in regard to the pathology of the disease in question. Among those who have held to the belief that the disease of the muscle is the result of disease of the nervous system, we have Romberg, Schneevoogt, Valentiner, Cohn, Frommann, Virchow, Bergman, Jaccoud, Ollivier, Erb, Trousseau, Charcot and Joffroy, Lockhart Clarke, Hayem, and many others.

Among those who have held that the disease is independent of the nervous system, and that it arises in the muscle itself, or idiopathically, we have such names as those of E. Meryon, Wachmuth, Oppenheimer, Hasse, M. Meyer, Friedberg, Malmsten, Roberts, and among others, the author himself; for he says "it is one of the chief purposes of this work to demonstrate the primarily myopathic nature of the form of disorder known under the name of progressive muscular atrophy, and that it is, in its genesis, independent of the nervous system."—(P. 8.)

Dr. Friedreich would appear to have made very extended use of Middledorpf's trochar (harpoon), for the removal of small portions of diseased muscles during life, for microscopical examination, to aid in forming a diagnosis as to the nature and progress of the disease. He does not seem to have experienced the difficulties in its use that Duchenne speaks of as following the use of his own trochar. Middledorpf's instrument consists of a trochar sheathed in a canula, which latter, when once introduced, is permitted to remain until the muscle has been penetrated in various directions, for the purpose of obtaining samples from many different parts, and all through the same wound in the skin. By this means, the most formidable practical objections made by Duchenne may be avoided, according to Dr. Friedreich.

In chapter second is a very exhaustive consideration of the pathologico-anatomical changes of the muscles in progressive muscular atrophy.

Meryon, according to Dr. Friedreich, was the first to describe and characterize the change which the muscular fibre undergoes in the disease in question. He described it as a fatty degeneration of the primitive muscular fibre, with consecutive atrophy of the same, and destruction of the sarcolemma as the essential process. In this view, a large number of later observers agree in the main. This class of observers described a gradual diminution, in volume and weight of the muscular fibre, with gradual obscuring of the transverse, and finally, of the longitudinal, striæ of the fibre, by means of the deposit of fine granules, partly of fat, in the primitive muscular fibre, which process or processes, passed on, until the fibre disappeared in parts, or altogether, and was partly or wholly replaced, in many instances, by fat granules, or even fat globules of large size.

On the other hand Robin held, so early as 1854, and many others with him, since, that the morbid change is not a fatty degeneration, but a much more complicated change in the muscular fibre. This eminent microscopist characterized the change as a gradual diminution of the volume of the muscular fibre, with gradual obscuration of the transverse and longitudinal striæ, with a development of granules in the substance of the atrophied fibre and the formation within the same of small, elongated, proteinaceous bodies, which replace the fibre, and then so disappear that the sheath or sarcolemma of the fibre, collapses, so that its walls touch each other. Then come, at a later period, fat granules and fat cells. But, in the early part of the process, according to Robin, there is no fat, but a change comparable in all respects to that which is called inflammatory—the exudation products being strictly proteinaceous, and soluble in acetic acid instead of ether—as they would be if fatty. Then Friedburg* appears to have demonstrated under the name of "myopathic paralysis," the inflammatory change the muscular fibre undergoes in progressive muscular atrophy. Our author then quotes the statement of Foerster† as to the appearance and order of the changes the muscular fibre undergoes in the disease in question, and which is so clear and satisfactory that we will translate it. He says: "The bundles of fibres become smaller and lose their transverse and longitudinal striæ, and the sarcolemma of the primitive fibre appears finally to be filled with a finely granulated but otherwise homogeneous mass, in which are to be seen, in

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*Friedburg, *Pathologie u. Therapie der Muskellaehmung.* Weimar, 1858.

†Foerster, *Handbuch der Speciellen Pathologischen Anatomie.* 2 Auflage, 1863, s. 1012.

some cases sparsely, in others thickly scattered yellowish brown nuclei, and often fine fat granules, which lie strewed in the homogeneous mass. In the nuclei of the sarcolemma there is frequently to be seen a decided increase in number, by reason of division, so that a single spot, instead of one or a few, will contain a great mass of smaller round or oval granules. The final atrophy proceeds in a twofold manner. Either the fibres, by reason of the absorption of their contents, become continually smaller, until only the sheath of the fibre remains as a pale narrow stripe, filled in with fat and connective tissue, in which even the sarcolemma soon disappears, or the homogeneous mass in the sarcolemma disappears in single portions, the remaining segments assuming an oval form, while between them the sarcolemma collapses, so as to give the fibre a broken irregular appearance. These oval bodies gradually diminish in size, so that finally they present the form of delicate spindeform bodies and then disappear altogether. Or, again, the sarcolemma may divide completely, so as to form a true wall for the oval bodies already described, which then present the appearance of a series of oval cells ranged in the track of the original fibre. The oval masses are penetrated throughout by fat granules, and at last are resolved into albuminous and fatty molecules. In many cases the number of fat globules is very great in the oval bodies into which the diseased fibre has broken up, and in such cases they present the appearance of the granule cell. Yet, I have never seen the fat granules, merged into large fat drops, to constitute the ordinary fat cell. Neither have I ever seen the formation in any other way of fat cells in the interior of the primitive fibre." (Page 49.)

In nearly all cases of the disease that have been carefully examined, there has been an absolute increase of connective tissue in the affected muscle.

In a careful statement by Schueppel the appearances are described as corresponding closely to those described by Virchow under the title of "parenchymatous muscular inflammation."

Our author then enters on a most extended and careful independent statement of the details and nature of the change the muscular tissue undergoes, and sums up his own opinion as follows:

"The affection known clinically, and accurately characterized under the name of progressive muscular atrophy, owes its existence to, or consists of, an inflammatory process within the primitive muscular fibre—it is a true *polymyositis chronica progressiva.*

"The first changes begin in the *perimysium internum* as a hyperplastic increase of the interstitial connective tissue in its most delicate bands, between the primitive muscular fibres. At the same time there arise the appearances of irritation, in a greater or less degree, in the primitive muscular fibres, in the form of swelling and increase of the muscular corpuscles, especially of their granular contents; and in part also a parenchymatous granular infiltration of the transversely striated substance of the primitive fibre.

"With the increase of the interstitial connective tissue, the muscular fibres in different ways are destroyed, partly through simple atrophy and progressive dissolution of the structure of the fibre, obscuring its transverse, or sometimes earlier, its longitudinal striæ, and then the separation (zerklueftung) of the fibre transversely into segments, and partly through fatty degeneration, &c. The final result of the changes is the more or less perfect fibrous degeneration (cirrhose) of the muscle. As an accessory, but inconstant and in no way essential process, we find at an early period, or from the start, a diffuse lipomatous change of muscular fibre accompanying the more typical form of the disorder." (Page 62.)

This view Dr. Friedreich affirms as the result of careful comparison of the morbid histological changes which occur in inflammation of the muscles and in this disease. We cannot enter more into details but it appears pretty certain from the cases and detailed observations made, that the process which leads to the destruction of the muscular fibre in progressive muscular atrophy, is an *irritative* one—essentially inflammatory. He also fully describes the morbid microscopic changes that occur in typhous disorders, and finds an essential agreement between them and those that are found in progressive muscular atrophy.

Then our author enters upon a very minute examination of the normal histology of muscular tissue, for the purpose of obtaining a standard of comparison. The researches of Lebert, Remak, Kœlliker, Max Schultze, Weismann, F. E. Schultze, Eberth, Wilson Fox, Stricker, Bœttcher, O. Weber, Clarke (J. L.), Deiters, Margo, and others, are referred to or summarized. Into this part of his work we cannot follow him at present, but we may say in passing, that he would throw a light from the structural changes that muscle suffers in progressive muscular atrophy backward on the normal histology of the striped muscular fibre.

His opinion seems to be that the ultimate muscular fibril is composed of spindle-shaped cells, which become transversely striated in their substance, and are fused together where they overlap each other, so as to form a continuous fibre.

Our author next considers the "pathologico-anatomical changes" that occur in the nervous system in this disease. Beginning with the original cases of Cruveilhier, he recalls the state of the nerves and spinal cord in the cases mentioned by Reade, Dumenil, Vulpian, Trousseau, Jaccoud, Recklinghausen, and others. In all the cases reported by the observers above, the anterior or motor roots of the spinal nerves were found atrophied, or otherwise diseased, and, in many of them, morbid changes in the anterior column or corresponding gray matter of the spinal cord. The cord was more particularly involved in the cases mentioned by Schneevoogt, Valentiner, Bandrimont, Dumenil, O. Schueppel, Lockhart Clarke, Hayem, Charcot, Joffroy, and others. The observers referred to, speak of congestion of either the gray or white substance, or both, and granular exudation in the substance of the cord, an increase, in some cases, of the amylaceous bodies found in the cord, and in particular in the anterior horns of gray matter in the cord, with which the anterior roots of the spinal nerves are in close relation. There was often change, or even destruction, of the "ganglion cells" there situate, and comparative, perhaps absolute, increase of the connective tissue of the cord and nerve roots. In fact, there was evidence of the most indubitable kind, of irritative change, especially in the anterior columns, and anterior horns of gray matter, with consequent degeneration and atrophy of the cells and fibres of the affected parts of the cord.

In certain other cases, the changes were not confined to the parts of the cord corresponding to the implantation of the anterior or motor roots of the spinal nerves, but were observed, also, in the posterior roots and corresponding parts of the cord. Such cases have been reported by Menjaud, Clarke, Dumenil, etc. In a few cases, no change in the roots of the spinal nerves could be discovered, but were found in the cord itself. Frommann and Clarke have reported such cases. Then, again, a few cases are mentioned in which the pathological changes in the nervous system were apparently limited to the white substance of the posterior columns of the cord. (Virchow, Schwarzenski, Clarke.) In a few cases again, as in those reported by Lockhart Clarke, Gairdner, Gull, Grimm, and others, the gray substance alone, of the cord, appeared to be the seat of morbid structural change, and not in a notable degree of the cells of the anterior horns of the gray matter.

Finally, a long series of cases are cited, in which there was "perfect integrity of the spinal cord in all its parts," but in which, however, there was muscular atrophy. As examples, he cites cases from Cruveilhier, Romberg, Landry, Oppenheimer, Hasse, Friedberg, Duchenne, Vulpian, Dumenil, Trousseau, Jaccoud, Recklinghausen, Cohn, and many others.

As regards the strictly peripheral parts of the nervous system, they have often been found in a state of disease, especially in an atrophied state, so that often the neurilemma alone is left. In the sympathetic, especially its ganglia, there has been, in many cases, fatty degeneration, granular infiltration, and an increase of the connective tissue. But little mention is made of the *rami communicantes* of the sympathetic, but in one case reported by Jaccoud, in which, while the cord was apparently normal, and the anterior roots of the spinal nerves were atrophied, the same was found true, in a marked manner, of the communicating branches in question.

The ganglia on the posterior roots of the spinal nerves were found in a more or less degenerated state, as evidenced by the presence of fat and a variable amount of dark pigment granules, in various parts of the cells and other morphological constituents of the ganglia, which corresponded to the seat of disease.

After this "casuistik," or report of cases from other authors, Dr. Friedreich passes to a statement of the results of his own personal investigations. In two cases he found no morbid changes in any part of the nervous system—in one case in the peripheral portion only of the nerve trunks, connected with the affected muscles. But, in such a case, the atrophy and final degeneration of the nerve fibres, never attacked nerves outside of the bundles supplying affected muscles. Many such cases did our author find, where the peripheral portions of the nerve trunks leading to the affected muscles alone were affected.

Occasionally, disease of the anterior or posterior roots of the spinal nerves was found, and also evidence of irritative change in the spinal cord, more particularly in the gray matter. As to the nature of the morbid structural changes the various parts of the nervous system have been observed to suffer in this disease, they appear to Dr. Friedreich to consist of a "partly inflammatory and a partly regressive nutritive process, leading to atrophy," and, in extreme examples, to total destruction of the parts involved. He would designate the affection of the nerve trunks as a "*perineuritis*," or "*neuritis interstitialis chronica.*" In fact, he thinks the morbid process set up in the nerves and nerve centres, is of the same kind as that affecting the muscles.

In the next chapter the facts are discussed, which, in the judgment of the author, show the secondary nature of the morbid structural changes in the nervous system. Beginning in the muscle, the disease, as it affects the nervous system, is divided into three stages: 1st, "that in which the changes are entirely limited to intramuscular nerve branches;" 2d, "that in which the disease passes beyond the limits of the diseased muscle, and extends more or less far along the motor or mixed nerve trunks, even to the nerve roots themselves;" 3d, "that in which the changes have spread into the substance of the spinal cord." Instead of passing from the centre to the periphery, the morbid process is centripetal, passing from the muscle to the nerve centre. The origin of the disease is in the muscle, and from hence it spreads along the related motor nerves, toward the nerve centres. This is why the anterior roots of the spinal nerves are so often the seat of morbid action. Dr. Friedreich sums up this chapter in the following manner:

"Progressive muscular atrophy, beginning as a primary chronic myositis, is capable of producing secondary disorders in the nervous system, consisting in an encroachment on the intramuscular branches, and then extending along their course until the nerve trunks, until the nerve roots, are affected with the chronic neuritis. Lastly, the affection passes on into the spinal cord, producing there a chronic myelitis, which may subsequently spread in different directions in the cord. The inflammation progressing within the nerve fibre, may in any position come to a stand-still, but it must be extended to, in order to awaken disorder in, the nervous apparatus, and is essentially dependent for its source on the more or less active character of the tissue disorder in the muscle.

"In accordance with this view alone can we explain easily such widely discrepant kinds of post-mortem lesions in the nervous system as we meet with in different cases.

"Besides, there are certain regressive trophic disorders of the peripheral nerve fibres, as well as of the large ganglion cells, lying in the anterior horn of gray matter of the cord, to be ascribed to disordered motor functions." (Page 124.)

Dr. Friedreich supports his view as to the secondary nature of the nervous disease, by a reference to some analogous instances. For example, he cites the "secondary nerve alterations in ataxia and elephantiasis," and in lepra, tetanus, etc. But we pass these discussions by in order to give a summary of his elaborate chapter entitled "Criticism of the Explanation of Progressive Muscular Atrophy by means of the Neurotic Theory."

He discusses the appearances found after death in the case of those who have during life suffered amputations. Not only are the divided nerves found altered—atrophied—but the gray and white columns of corresponding parts of the cord. This he ascribes to an ascending neuritis, or some similar change, which, beginning in the stump of the amputated member, extends to the cord, and so affects it. He thinks these results are perfectly to be compared with those observed in progressive muscular atrophy.

If Waller's doctrine were true, it would be an insuperable difficulty in some cases to our author's views. But he does not believe Waller's doctrine, as a rule, true. He thinks it has many exceptions, especially when we enter the domain of pathology.

Unlike Vulpian, and in agreement with many authors, and among them Dr. S. Weir Mitchell in our country, he believes greatly in neuritis, and its extension along the course of a nerve from one point to another—as from its peripheral to its central extremity. As an instance of this form of disorder, he mentions the acute ascending paralysis (paralysie ascendante aigue) of Landry. In this case we begin, possibly with some affection of a peripheral nerve, and apparently from this we have developed congestion, or some active process in the cord, which rapidly ascends the same until the respiratory centres are involved, and death ensues. He thinks if we may have such a form of disorder begin peripherally and rapidly pass by extension to the cord, that we need not be surprised at the view that in case of progressive muscular atrophy, though the disease begins peripherally in a muscle, that it should extend along the related nerve trunks so as to involve the spinal cord.

To the support of his views, our author brings the form of disease known as "ascending chronic neuritis," of Dumenil, and "progressive nervous atrophy" of Jaccoud.

Our author takes occasion to attack the view which affirms the existence of trophic nervous action, and, as it seems to us, on insufficient grounds. He rather thinks that where trophic disorder, say of a muscle, arises, as an apparent consequence of morbid nervous influence, that the case is to be compared to that of inflammatory lesion of a nerve, in which an inflammatory process is propagated along a nerve, toward either its peripheral or central end, and leads to various active processes in parts distant from the seat of original injury. But for our own part we are not at all prepared to agree with our author in regard to trophic nervous action, and hence cannot fully agree with him in his conclusions as to the mode of production of certain structural changes, occurring in muscles, not to mention other parts of the body.

Our author enters at length into a discussion of those cases reported, of injury to the motor nerves, with assumed consequent trophic disorder of related muscles, etc., and concludes, with a certain show of justice, that there is no evidence that the disorders in question should be attributed to simply "nervous influence." In each case he holds it as probable that the trophic lesions are simply inflammatory processes, induced by an extension of inflammation along related nerve trunks. Then he enters on an examination of disorders of the sensible nerves, in which, beginning with the celebrated observations of Magendie, concerning trophic disorders following division of the trigeminus, he runs through all the best known reports of cases, whether as regards those involving anæsthesia or the contrary, and reaches the conclusion already mentioned. He refers particularly to the cases of cutaneous disease that have been supposed to be neurotic in origin, such as *zoster* or *zona*, such as accompany or follow for example certain acute neuralgic affections.

He discusses especially certain cases reported by Baerensprung, who was perhaps the earliest writer of note to call attention to the neurotic character of *zoster intercostalis*. He asserts that in every instance where a microscopic investigation was made after death, there was found the evidence of neuritis in the nerves involved. But it is impossible, in this notice, to follow our author in the succeeding pages of this long chapter, in which he seems to have ransacked the whole of German and French literature, for his matter, aside from what he has himself contributed. He sums up this part of his work in the following manner: He says: "As the final result of our investigations and considerations, we are brought to see that there is no demonstrable argument that progressive muscular atrophy has its *origin* in either the central or peripheral nervous system, and we are compelled to admit a primitive disorder of the muscular tissue itself." (Page 186.)

But what is the *pathogenesis* or mode of production of the assumed "primitive disorder of the muscular tissue?" On this question Dr. Friedreich does not throw any clear light. He contents himself apparently with his effort to overthrow the neurotic theory of its origin.

In examining this case it is necessary to note, that the fate of nervous trophic action in general is not necessarily involved in it. All that Dr. Friedreich says may be true, as to the facts of progressive muscular atrophy, without the necessity arising for abandoning nervous trophic action, as true or probable, under certain circumstances.

Suppose we should take a group of affected muscles in this form of disease, and should find after death no recognizable nerve lesions except in the periphery of nerves involved in the substance of the affected muscles, the question arises as to how a limited group of muscles can have excited in them an irritative, inflammatory process, such as the disease in question seems to be.

Why limited to a peculiar group of muscles as is often true? Why not affect all the muscles in some degree? Dr. Friedreich does not answer in a satisfactory manner these questions. But it is not necessary for us to discuss the subject of trophic nervous action in this notice, since it is partly the subject of the lecture on the "pathology of the vaso-motor nervous system," in the present number of the JOURNAL. To that lecture we would refer the reader. To what is there said we have nothing to add in this place. We can only say that, critical and thorough as Dr. Friedreich's discussion is, it has not been sufficient to convince us of the unreality of trophic nervous action, or that the disease of the muscles found in progressive muscular atrophy, may not have a nervous origin. We do not believe traces of organic disease of the cord are necessary as proof of such origin, or that the absence of such traces disproves it. We are all the more inclined to this belief, after reading the able paper of Professor Vulpian, of Paris, which was placed before our readers in the April number of the JOURNAL, and in which, while a distinct system of trophic nerves is denied, yet nervous trophic action, as a fact, is not only admitted, but powerfully supported by convincing arguments. As to the remainder of the work, which relates to true and false muscular hypertrophy and bone atrophy, and the relations of the same to progressive muscular atrophy, we do not have space for examining them as they deserve in this number. They may be made the subject of a review on some future occasion. But to all such as desire to become possessed of the most full and able monograph yet produced on the subject it treats, both as regards the literary survey and original materials, we would commend this of Dr. Friedreich.

III.—HITZIG: FUNCTIONS OF THE BRAIN.

UNTERSUCHUNGEN UEBER DAS GEHIRN. ABHANDLUNGEN, PHYSIOLOGISCHEN UND PATHOLOGISCHEN INHALTS. Von Dr. Eduard Hitzig, Privat Docent a. d. Universitaet, Berlin. Mit Holzschnitten. Berlin: 1874; 276 pages, 8vo. (*Researches in regard to the Brain. Physiological and Pathological Treatise. By Edward Hitzig, Instructor at the University of Berlin. With wood cuts.*)

We have endeavored in this and the preceding numbers of this journal, to keep our readers informed as to all the recent advances in cerebral physiology that have recently been made and published in the periodical medical literature of Europe and our own country. Dr. Hitzig has been one of the pioneers in this line of investigation, and in the volume before us we have a collection of papers on the subject, some of which have already appeared in German periodicals during the past four years, and others containing the results of newer investigations, which here appear for the first time in print. It is the first and only elaborated volume on the subject, that has, as yet, been offered to the public.

The essays in the book before us are divided by the author, in his introductory remarks, into three groups. The first of these includes the first five chapters, all of which bear on the subject of the localization of the functions of the brain; the second, consisting of the sixth, seventh and eighth chapters, treats of hitherto little known disorders of muscular innervation; and the third, comprising all the remaining portion of the book except the last chapter, is devoted to the subject of the organs of equilibrium. The twelfth and last chapter of the book, on the artificial production of epilepsy by wounding the cerebral cortex, and which, as the author explains, was introduced as an afterthought, though out of place, belongs properly to the first of these three groups.

The first two chapters or papers, which were originally published in Reichert and Du Bois Reymond's *Archiv*, one of them as far back as 1870, contain the account of the original experiments, partly performed with the collaboration of Dr. Fritsch, by which they first determined the existence of centres for muscular movements in the cerebral cortex. These experiments are well known and have been previously alluded to in these pages. They consisted in exposing and irritating the cerebral surface at various points, with a mild galvanic current, by which means they found that certain groups of muscles reacted to the excitation of circumscribed regions of the brain's surface, and in this manner determined the centres for the flexor and extensor muscles of the limbs, the muscles of the neck, and those of the face. The author and his coadjutor, in some instances, employed the induction current, but found it of less constant value in producing these reactions. They also experimented with the method of removing a determined circumscribed portion of the brain, and then observing the effect of this operation on muscular movement, and obtained, not altogether conclusive, but still suggestive results. We cannot here detail the whole of their experiments. The general conclusion which they arrived at, and which, in the main, is supported by the majority of the subsequent experimenters in this line of research, is, that a very considerable portion, almost one-half, of the cortex of the brain stands in direct connection with muscular movements, and also, that some mental functions, and probably all, are dependent on certain circumscribed cortical centres in their action on the material body, or its reaction to consciousness.

In the second chapter or article, the various influences which bear more or less directly on the experiments are discussed; the action of the respective poles of the exciting current, the influence of narcotics, and of apnœa, the limits of some of the cortical centres are given with more exactness and detail; and the article closes with some general remarks on the experiments and their results. It was found that etherization, though carried to the point of complete abolition of reflex irritability, as tested on the conjunctiva, only partially affected the electric excitability of the centres for muscular movement, and also that it was destroyed neither by morphine narcotization, nor by apnœa.. These facts, with the point developed by the experiments of ablation, which were afterwards in a manner repeated by Nothnagel, as to the apparent loss of the muscular sense in the injured animal, are very suggestive. Dr. Hitzig, however, criticises Nothnagel's opinion that mental or spiritual functions cannot be rigidly localized in the brain cortex, holding that he reads the phenomena wrongly. His own views, based on these experiments, are given above, but we may be allowed, in this connection, to repeat them, quoting his exact words. He says, quoting from the previous paper: "It follows from the sum of our experiments, that thought is by no means as Flourens and others have believed, a kind of general function of the brain, the expression of which may be made from it as a whole, but not from single regions, *but that it is much more certain that some psychological* (seelische) *functions, probably all of them, are dependent, either in their action on matter or their reflexion from the same, on certain circumscribed cortical centres.*" And he here adds: "For the correctness of this view, in fact, is shown with all desirable logical clearness from our experiments, and we consider this truth as the most valuable result of our labors."

We see no reason for not accepting this view of Dr. Hitzig, to the extent that it is through these centres that the will acts, and consciousness receives its impressions, and this we understand from the above is the sense in which he himself holds it. The term "circumscribed" is not used by him in the narrowest sense; he holds that the centres may be definitely located in certain regions, though their exact boundaries are not absolutely determined, and that the apparent difference of opinion between Nothnagel and himself may be perhaps dependent on a misunderstanding of the exact signification intended by the word.

The third paper is devoted to a review and criticism of the investigations of Prof. Ferrier, which have been noticed in the January number of this journal. This chapter is mainly polemical. Dr. Hitzig feels aggrieved at the manner in which his prior researches are noticed and their want of appreciation from the British author. He quotes the paragraphs in which Dr. Ferrier has made mention of his labors, and accuses him of misrepresentation and appropriating, or at least claiming originality in discoveries which had already been made by his German predecessors. The paragraphs quoted, do not, indeed, do justice to their work, inasmuch as they convey the idea that Drs. Hitzig and Fritsch had made only imperfect and inconclusive experiments, the signification of which they only partly understood. The author enters in his own name and that of his coadjutor, his protest against this treatment on the part of Prof. Ferrier, and proceeds to review in detail the points of his papers on the physiology of the brain, which have appeared in the *West Riding Lunatic Asylum Reports* and the *Journal of Anatomy and Physiology*. As this is, in many respects, one of the most important of all the papers in the book, and as it is here given in print for the first time, we shall follow Dr. Hitzig's review in detail and in his own order, so far as our space will admit.

He first takes up the methods employed by Prof. Ferrier, and gives them a thoroughly hostile criticism. He first comments on the small number of vivisections and experiments from which the English author's conclusions were drawn, up to the time of the publication of his paper, and the acknowledged lack of perfect success in some of them, and remarks that, even if the discoveries made were altogether new to science their publication under these circumstances might be of doubtful propriety, and then asks what is to be said of the publication of conclusions drawn from such insufficient materials, three years after another's prior announcement? As regards the comparative value of the induction and galvanic currents, he claims that though he has used the former less than the latter in his experiments, he can yet claim to have had a larger experience with it than Ferrier shows in his account of his investigations. He criticises very severely the employment of the powerful currents used by the latter, and gives at length the disadvantages of their use, claiming that Ferrier must have been ignorant of the laws of the ramifications of currents in moist, non-prismatic conductors, and that some of his detailed experiments display this ignorance to the fullest extent. Finally he claims that the precautions taken by Ferrier to exclude reflex movements from complicating the results were altogether insufficient, as the animals struggled, bit and cried while the experiments were being performed.

Next, taking up the results of the investigations of the English author, Dr. Hitzig states the principal difference between them and those of his own researches to be, first, that Ferrier found nearly the whole cerebrum excitable, especially the frontal and larger portion of the occipital and temporal lobes, while, in the experiments performed by Fritsch and himself, these latter, and also a portion of the frontal lobes did not react to irritation. The second principal difference is in the fact, that Ferrier describes several different points of excitation for the same group of muscles, and also produced from the same point in the cortex movements of altogether different groups, while the German investigators only considered those points as centres from which special muscular movements were produced with an extremely slight current intensity; and for each muscular group but one of these centres was found. This difference is to be explained, the author thinks, by the difference in the currents employed. He points out some of the contradictions which some of Dr. Ferrier's experiments display between themselves, and states that he has experimentally tested his results on dogs, cats and Guinea-pigs, and gives in detail a comparison between effects of irritation of the brains of each of these by the two methods, pointing out the special differences in the reactions of each in experiments on each species, illustrating the points he makes with wood cuts. We cannot follow him in all these details of his test experiments; they appear to have been quite numerous and carefully performed. The points which Ferrier had fixed as the centres are taken up *seriatim*, and his observations subjected to a searching criticism. The only discovery for which he gives credit to Ferrier is that of the part which controls the movements of the mouth and tongue in eating. He says, summing up the whole: "To recapitulate, Ferrier has, by a very objectionable method, and in an extremely superficial manner, shown that by powerful electrical irritation of the anterior and basal portion of the brain in dogs and cats, the motions of eating may be induced. In this consists his merit. On the other hand he has not, in a single instance, accurately relocated any of the excitable points discovered by us; he has, indeed, made a number of statements concerning inconstant reactions or those brought about by imperfect methods; and finally he has adorned his work, without credit, with discoveries that belong to us and not to him."

We have given the more attention to this chapter, because it seemed to us a very important contribution to the history of the subject, and it is only justice to hear both sides. Dr. Ferrier had charged the investigations of Hitzig and Fritsch with incompleteness, and had suggested that they did not fully appreciate their significance, and in this volume Dr. Hitzig makes his reply to these charges. Except, perhaps, in the above quoted passage, we have not attempted to reproduce any of the bitterness and sarcasm with which he spices his criticism. It is to him a matter of personal honor to maintain his priority in these researches, and he has, in our opinion, put it sufficiently beyond question in this paper. Still, we can only say that in a scientific polemic strength of language is not essential, and we do not praise it when it appears. Dr. Hitzig's arguments would have been none the less valid and his demonstration fully as clear, had he refrained from one or two passages which occur in this article.

The objection taken by MM. Carville and Duret, and others, that the excitation of muscular groups is due to the diffusion of the currents through the brain and not to local centres in the cortex, is only mentioned in the introductory remarks at the beginning of the volume, the articles which compose the body of the book having been written before their publication was received. Our author there calls attention to the fact that the consideration that the brain is a moist conductor was always kept in mind in his experiments, and that therefore the final result of the investigations of MM. Carville and Duret was taken as a premise in his own researches.

The fourth article, which originally appeared in the *Archiv f. Psychiatrie u. Nervenkrankheiten*, III, 2, is an account of an interesting abscess, situated in the anterior central convolution of the right side, which during life caused a motor disturbance, affecting the facial muscles of the left side in all general movements, such as laughing, etc.

The fifth paper treats of the equivalent regions in the brains of dogs, apes and men. The preceding number of this journal contained an abstract of a paper by our author, on the electrical examination of the brain of an ape, the results of which are embodied in the chapter before us. The comparison with the human brain is afforded by the case described in the preceding chapter, and others reported by Wernher, Griesinger, Lœffler, and Theodore Simon. From these cases he finds that lesions in the upper portion of the parietal lobes (scheitellappen) produce disorders of motility in the extremities, while in the base of the same they cause similar disturbances in muscles of the mouth and tongue. If a large part of the anterior central convolution is involved a greater number of groups of muscles is affected, and especially those of the face.

The sixth and seventh chapters concern the subject of certain anomalies of muscular innervation. In the first of these the contractions so frequently following hemiplegic attacks are taken up and examined. Dr. Hitzig is not satisfied with the explanations which have been offered of these phenomena, that they are due to the predominance of the flexors, or, as some have thought, to inflammatory processes in the brain, or to an abnormal irritation of the motor fibres in the flexors. Of all the theories hitherto advanced he considers that of Bouchard to come most within the bounds of probability. Bouchard considered the earlier contractions to be due to an inflammatory process, and the later ones to be the product of the increase of connective tissue in the cord, working as an irritant on the motor nerves of the limb. Our author, however, calls attention to the fact that in some cases these contractions are only observed during the movement of certain other muscles or sets of muscles, and in other cases that they are absent after a more or less prolonged period of rest, and are again brought on when the will acts to incite voluntary movements. He considers these involuntary muscular actions which we call contractions to be of the nature of co-ordinated movements, and when irritative phenomena take place in a section of the nervous centres which has to do with the co-ordination of muscular movements, their regulation becomes interfered with and their action is abnormal. The liability to this abnormal condition of innervation seems to depend on the functions of the members; it is most common in the upper extremity, which has in all respects the most manifold movements to perform, and whose nervous connections in the central organs may be assumed to be the most complicated. When a hemorrhage takes place in the brain a certain number of the morphological elements are directly involved and others still are mechanically embarrassed to a greater or less degree; when, however, by the gradual process of recovery, the capacity for action again appears, this abnormal function steps in, and according to the course of the morbid process, either returns to the normal or becomes stronger and more lasting with time.

The summing up of both chapters is given as follows: "That interruptions of conduction of either central or peripheral nerves, in men as well as in animals, produce irritative conditions in certain motor tracts of the central nervous system, which according to the severity and location of the lesion, as well as to the other individual peculiarities, may take on extremely diverse characters."

In the eighth chapter two interesting cases of secondary affections of motility following peripheral injury are described. In one of these a bayonet wound of the thigh was followed by lasting pain in the cicatrix, with epileptic attacks affecting especially the right side, but after the excision of the scar these both slowly disappeared, but choreiform movements appeared whenever the patient attempted to speak, move, or even stand. In the other case, an injury to the ulnar nerve was followed by secondary rheumatic pains, tremor and epileptiform attacks. These motor phenomena in these two cases are classed by the author among the abnormal co-ordinated movements mentioned in the preceding chapter, and he calls especial attention to them since previously tetanus and epilepsy among the secondary central affections of the nervous system, caused by peripheral injuries, had been almost the only ones that had received attention.

The ninth chapter is a reprint from Parts 5 and 6 of Reichert and Du Bois Reymond's *Archiv* for 1871; its subject is the disorders of muscular innervation, produced by galvanization of the head, and the consciousness of relations in space. An abstract of this paper, taken from the *Berliner Klin. Wochenschrift*, by Dr. D. F. Lincoln, appeared in the *Boston Medical and Surgical Journal* for Oct., 1872, and it may be therefore not unfamiliar to American readers. The author found that a mild galvanic current passed directly from one mastoid fossa to the other, caused a sensation of vertigo and uncertainty as to position; with a somewhat stronger current, external objects appeared to move, and a still stronger one produced an inclination of the body toward the anode, with nystagmus, rotation, etc. The opening of the current caused the subject to lean in the direction of the cathode, while external objects appeared to move in the opposite direction. The explanation offered by Dr. Hitzig is according to the theory of electrotonus: an increase of sensibility at one pole and a decrease at the other, with false impressions on the muscular sense inducing voluntary movements to preserve the equilibrium. The optical appearances and the nystagmoid movements are similarly explained.

The tenth chapter, which is here published for the first time, is a supplement to the preceding. In it the author takes up in review the views of several recent writers in regard to the results of cerebral galvanization and the sense of equilibrium. He first notices the statement of Wundt, that at the beginning of the electrization the movement of the eyes is toward the side of the cathode and not to the anode as he himself has stated, and thinks that he perhaps used too strong a current, inducing a closing contraction of certain muscles on the side of the negative pole, which should have been avoided. Next he notices the memoirs of Breuer and Mach, on the part which the semi-circular canals of the ear take in the sense of equilibrium, in which he maintains his views expressed in the previous chapters as to the direct function of the brain in this respect.

The eleventh chapter, entitled *Investigations in the Physiology of the Cerebellum*, gives an account of experiments performed on rabbits by ablation and irritation of this portion of the nervous centres. He describes the movement of the eyes and body and holds that these experiments caused a very high degree of vertigo in the animals. He produced these movements and the vertigo by cooling the cerebellum with ice or cold water. He closes the chapter with the following words: "I believe that I have shown that we can produce all the disorders of muscular innervation which follow galvanization through the head, and which are to be considered as vertiginous phenomena, either all at once or singly, even after the normal condition of the cerebellum is either locally or generally altered.".

The clinical observations of epilepsy following injuries to the cerebral cortex are of so suggestive a nature that experimental investigations on the subject are of the highest interest. This forms the subject of the twelfth chapter. Four experiments are detailed and the results seem to show beyond question that it may be artificially produced long after the wound which is its immediate cause has apparently healed. The question as to the pathological process within the brain is as yet unrevealed. The author admits that the question is only opened by his experiments but hopes to undertake at least a part of their solution.

We may, before closing this notice, perhaps be allowed a few general remarks on points of the subject not directly mentioned in the work before us. The articles it contains were, for the most part, written before the most recent investigations of the subject. That the author however is not at present idle is plainly shown by his contributions in the current medical literature, one of which is noticed in the present number of the JOURNAL. In it he notices the recent experiments and criticisms of Dr. Burdon Sanderson, which, in the main, are the same as those of Putnam, published in the *Boston Medical and Surgical Journal.* His reply as given seems to us to meet the objections offered by Dr. Sanderson, but there is another consideration which he has not at least dwelt upon to any extent. He says if it be proved that the corpus striatum is excitable it does not therefore follow that the hemispheres are not. We should say that if the cortical centres are the points from whence the will acts, the electric irritation applied at any point on the routes of conduction to the motor ganglia might produce the same effects. Moreover, if special muscular groups are incited to action by the irritation of particular points in the deeper lying portions of the cerebrum, as for instance, the corpus striatum, it only complicates the subject but disproves nothing. We hope, indeed, in view of the practical importance of the subject, its suggestiveness as regards diagnosis, etc., that the views of those who hold to localized centres maybe proved beyond dispute.

There is still another point which we would mention in this connection. It seems to us that physiological experimentation can call to its aid in this very difficult research, the aid of anatomy, both human and comparative. It seems as if the anatomical and physiological homologies between the brain and cord might perhaps be as fully worked out by careful research as are those of their bony envelopes, in an anatomical view alone. The writings of Meynert and others, and especially the very recent papers of Professor Betz, of Kiew, have a bearing in this direction, and we trust that the line they have indicated will be still farther followed out.

Whatever may be the result of these investigations they cannot but add immensely to our knowledge, and if, as we believe they will, they demonstrate the existence of material cortical brain-centres for motion and sensation, to Dr. Hitzig will belong no small share of the honor of opening up and developing this most important chapter of modern physiological progress.

IV.—MAGNAN: ALCOHOLISM.

De L'Alcoolisme, des diverses Forms du Delire Alcoolique, et de leur Traitement. Par le Dr. V. Magnan. Ouvrage couronne par l'Academie de Medicine. (*Alcoholism, the various forms of Alcoholic Delirium and their Treatment.* Paris, 1874. 282 pages).

The opening paragraph of the introduction of this work sufficiently indicates the reason the subject was taken up, and the fact that the great question that has agitated this country is also before public attention abroad. The author begins with the following words: "Although for some years back the questions relative to alcoholism have attracted general attention, recent events have amply demonstrated that the united efforts of all classes, physicians, philosophers and legislators, cannot be too soon employed to ward off the imminent danger that menaces at once health, morals, and society." The present work is the contribution of an eminent physician and man of science, to this end; one to whom we owe, as much as to any one, our knowledge of very many of the morbid processes and conditions which follow the excessive use of alcoholic compounds. It is a striking fact that it is to the French medical men that we owe most for the scientific investigation and description of the injuries to the physical and intellectual system which follow this excess. In our own country and in England the moral and physical dangers have been ably portrayed, and popular movements for reform in this direction have been more extensive and perhaps also more successful; but for the scientific basis of the argument for humanity, we are most indebted to continental writers and observers. We need only refer to the memoirs of Lasegue, Racle, Motet, Legrand du Saulle, Marce, Voisin, Dagonet, and many others, in support of this statement.

In the present work, for which M. Magnan received the Civrieux prize for 1872, from the Academy of Medicine, of Paris, he has given us a study of the subject of alcoholism in its various aspects and with all its pathological accidents, which is based on physiological experimentation and clinical observation, together with critical remarks on the literature of the subject already extant. We will endeavor to give in this notice, in brief, some of the principal points of the work, following the author's own arrangement.

In the first chapter the direct and immediate effects of the ingestion of alcohol and absinthe in man and animals are treated of. He illustrates the difference between the effects of these two agents by experiments on dogs, and publishes clinical observations of MM. Challand and Motet as to their comparative effect on man. From these experiments it is clearly shown that absinthism, as it has been termed, or the effect of the use of absinthe, is not to be regarded merely as a variety of alcoholism, but that apart from the ordinary phenomena of the latter, peculiar symptoms are produced; and while alcohol needs first to prepare the way by prolonged usage, absinthe can develop its effects, as it were at once, in vertigo, hallucinations and epileptic convulsions very different from the paralytic action of alcohol. Among the observations of its similar effects on the human subject M. Magnan reproduces, entire, one from M. Motet which is the more conclusive as the phenomena were observed in a man habitually sober, but who under the influence of bad companions was led for a short time to indulge to excess in absinthe, thus placing them in the same category as those that are observed in animals subjected to experiment, and free from the complications of habitude. We have already published abstracts of some of M. Magnan's papers on this subject in former numbers of this journal and may therefore excuse ourselves from longer dwelling upon it. The comparison, or rather the statement of the difference of the effects of alcohol and absinthe on the system, is continued in other parts of the work, and in many of the cases reported the alcoholism was complicated with the effects of the use of the latter agent.

The second chapter treats of the effects of alcohol and its compounds on the system at a later stage, the *delire alcoolique*, or the simple alcoholic delirium, characterized by sleeplessness, irritability, hallucinations and illusions, and under the influence of any exciting cause attacks of what we know under the general term of delirium tremens. The hallucinations are generally of a painful nature, at least to the subject, quickly changing, and usually more or less connected with the ordinary occupations or surrounding conditions of the patient. The delirium may take on a maniacal, melancholic, or stupid form, with all gradations between, but these forms are of little importance as regards prognosis or treatment. The sensorial troubles that accompany this condition, the affections of the special senses of sight, hearing, taste and smell, as well as of the general sensibility, are also described in short in this chapter and are treated of again in that part of the work which has to do with the various special complications of chronic alcoholism.

M. Magnan divides the subjects of this disease into three classes, according to the clinical history, the antecedents and the prognosis. The first of these comprises those who exhibit the well-defined characters of the alcoholic delirium but recover quickly and without any special unfavorable symptoms or relapses. Two illustrations of this form are given.

The second group includes those who readily recover, but as readily relapse under favorable conditions; and the third, that class of patients who are specially liable to relapses, and whose recovery is often so far incomplete as to properly be considered only as an amelioration and not a perfect cure. These are often the victims of heredity, and both of the examples given are of this class, but the morbid predisposition may be first acquired through the excesses of the individual himself. In these cases the alcohol acts as an immediate excitant to the predisposed constitution. We do not see the same order of symptoms as in the other cases; the delirium may be marked and all the intellectual faculties affected before the physical system is fully under the influence of the poison.

The treatment of the disease must be on general principles. The author, after indicating in a general way the therapeutic measures, discusses the subject of inebriate asylums and the duration of the treatment by sequestration and the prophylactic measures. He hopes much from the establishment of societies for temperance and efforts for the general elevation of the public morals. Some of the means he advises, the substitution of pure wines, beer and cider for the stronger alcoholic liquors, are hardly up to the American ideas of reform, but the greater physical evils of alcoholism are the ones he seeks to counteract, and these milder beverages are certainly less productive of evil than the adulterated wines, fiery brandies, and the nerve-shattering absinthe, that are at present so extensively used in France and elsewhere. There are other sides to the question than the merely physical one, with which this work has mainly to do, and we may venture to assert the question of temperance, in its moral and social bearings, will yet engage the attention of the citizens and governments, not only in France but also in Germany, and wherever prosperity and the progress of modern civilization have broken in upon the original simple habits and tastes of the people.

In the third chapter the author enters the subject of febrile delirium tremens, which he sharply diagnoses from the other form already described as the *delire alcoolique*. In the two there is the same toxic delirium, and though there is in this febrile form generally a more acute and intense delirium than in the other, yet we sometimes find it in only a moderate degree, and the differential diagnosis cannot therefore be made on this ground. Neither the intellectual phenomena, nor the majority of the physical appearances will serve for this purpose; the truly characteristic features, according to the author, are, first and foremost, the symptoms of the temperature, as revealed to us by the thermometer, and then the disorders of the motor system, the persistent tremors and convulsions, continuing even during sleep, and indicating an intensely irritative action, affecting the nerve centres, and especially the spinal cord. The temperature cannot be correctly estimated by the hand, on the surface, but the thermometer is absolutely necessary. Whenever in a patient, the subject of acute alcoholism, and not suffering from any intercurrent thoracic or abdominal affection, the thermometer in the rectum, after oscillating for several days in the neighborhood of 102° F., rises to 104° or 106°, the prognosis will be unfavorable, and the severer form of the disease may be assumed to be present. In the milder form of alcoholic delirium the thermometer may rise to 100° or 102° with and in consequence of the extreme agitation to which the patient is subject, but this limit is rarely passed. So in regard to the motor troubles, it is needful to observe their intensity, their duration, and their degree of generalization, in order to distinguish between the transient accidents of the milder phase and the persistent and exhausting accompaniments of the more severe and perilous form. The cases that are reported illustrate these points well, besides in most cases giving also the results of very careful autopsies performed on subjects who have died while suffering from the disease.

The differential diagnosis between the form just described and the ordinary delirium tremens complicated by or associated with intercurrent affections or injuries which may induce more or less of the febrile condition, with elevation of the temperature, forms the subject of the second section of the chapter. The febrile form nearly always appears after a recent excess, while the other declares itself during chronic alcoholism, without any such immediate cause, but depends solely on the intercurrent affection. In the former the fever is the essential symptom, while in the other, it is entirely due to the accompanying ailment, and follows the usual course in that disease; the motor disturbances are different in the two forms: in the simple form they may assume very different degrees of intensity, but they are not generalized throughout the whole muscular system as in the other phase. The prognosis and post-mortem appearances in the two forms are very different: in the one the favorable or unfavorable termination, and the morbid conditions observed in the autopsy, are dependent on the intercurrent ailment, while in the other they depend on the severity of the attack itself.

As regards the treatment of the febrile type of alcoholic delirium, M. Magnan recognizes three principal indications: (1) to protect the patient against his own violence; (2) to favor the elimination of the poison; and, (3) to sustain and increase his vital forces. The first and most important of these objects is embarrassed by the difficulty of applying the proper restraint without, in a measure, inviting some of the very evils we wish to avoid: stases of the blood, congestions, etc., and hindering the elimination of the poison by preventing the free action of the lungs. The author states his objections to the ordinary methods of fastening the patient to the bed, and the employment of the camisole or strait-jacket, and gives a description of the appliance used by himself and his colleague, M. Bouchereau, at the asylum of St. Anne, in order to obviate these objections.

The second indication is generally amply favored by the natural conditions, the activity of all the excretory organs, and the third must be met according to the judgment of the physician, in each particular case. M. Magnan advises, on general principles, the employment of nourishment, coffee, dilute wine, quinine, and beef tea. As to the use of chloral in this affection, he remarks that, notwithstanding its value in various mental disorders, in calming the agitation and procuring sleep, it is only needful to keep in mind its physiological properties to perceive that it is contra-indicated in a disorder which in itself compromises the respiratory and circulatory functions to so great an extent. He considers, moreover, that those cases which have been recorded of the cure of delirium tremens by this drug, have been, for the most part, examples of the simple alcoholic delirium, and not of this formidable febrile type.

Although in all cases of grave significance, the febrile form of delirium tremens is not invariably fatal. Two instances of recovery are among the illustrations given by the author, though in only one of these was the cure complete; the phenomena of chronic alcoholism remained in the case of the other after the subsidence of the acuter symptoms. Both of these cases, like the others in the work, are reported with the greatest care and thoroughness, and are illustrated with diagrams giving the thermometric trace, and, in one, the conditions of the sensibility of the respective sides of the body also. The whole chapter is, in its way, a model of scientific medical observation and analysis.

This distinction of the febrile type of the alcoholic delirium from the more simple form has not been universally recognized, either by M. Magnan's predecessors or by those who have written on the subject of alcoholism very recently, since the first appearance of this work. His points, however, seem well taken, and, inasmuch as it brings to the aid of the diagnosis and prognosis the employment of an instrument of precision like the thermometer, the distinction is practically of the greatest value.

The fourth chapter, the longest in the book, comprising nearly ninety pages, is devoted to the consideration of the various symptoms classed under the general head of chronic alcoholism. We cannot give the space to this portion of the subject which it really deserves in the present notice, but will hastily go over a few of the principal points embraced in the chapter. The author first notices the intellectual disorders, the apathy, the indifference to moral and social relations, the hypochondriac and melancholic tendencies, and the final disappearance of reason. In the second division he reviews the general physical symptoms, both motor and sensory, of the action of alcohol on the nervous system, the degenerations and other lesions of the tissues, and the convulsive accidents incident to the advanced stages of alcoholic poisoning. He criticises the division made by Magnus Huss of the spinal accidents of chronic alcoholism, holding that individual cases must be classed under several of the different heads at once.

Several observations of chronic alcoholism tending toward dementia and general paralysis are next given, with the results of careful autopsies, in the latter cases, which revealed lesions of the brain, the arteries, and the internal viscera, similar to those produced experimentally on animals; fatty and atheromatous degenerations, and chronic interstitial inflammatory products.

A long section is given to the description of the hemiplegic and hemianaesthesic form of chronic alcoholism, and several very interesting cases are detailed, but a review such as it deserves would require more space than we are able to give. In the last section of the chapter a short space is given to the treatment of chronic alcoholism. After alluding to the precautions which must be employed, the sequestration and restraint, the hygienic regulations, and avoidance of extremes of cold or heat, and everything that might produce disorders of the circulatory system, the author recommends the use of revulsive applications to the skin and in the digestive passages, tonics, and the iodide of potash in certain cases with a tendency to general paralysis. Arsenic, also, as recommended by Foville in cases of general paralysis, may be employed with advantage in chronic alcoholism when this tendency is well marked.

In the last chapter, which is a short one, the subject of the combinations of alcoholism with various mental affections, and its association with intercurrent disorders is taken up. We will offer a few words only in regard to these points. The author shows that alcohol, acting on a system already under the influence of a disease, may be merely an excitant, hastening or aiding the development of the proper course of the malady, or it may so exert its own toxic powers as to completely mask the actual symptoms of the disorder. In this connection he discusses and offers observations of cases of dipsomania, general paralysis, epilepsy, and various mental disorders. In regard to the second point, the association of alcoholism with intercurrent disorders, he likewise shows that the individual accustomed to the use of alcoholic drinks, but giving no very apparent signs of their deleterious effects, loses this immunity when his physiological equilibrium is disturbed by any accidental affection which can diminish his natural power of resistance to the poison.

At the present time when the subject of intemperance and its physical, moral, and social effects are engaging so much of the public attention, and very properly also that of the medical profession, a work like the present should be especially welcome. It is a valuable contribution to our knowledge of the pathological conditions produced by the use of alcohol in the human system, by a careful and competent scientific observer, and, as such, it is, we think, well worthy of a careful examination by everyone interested in its topic. We consider that society in general, as well as the members of our own particular profession, is indebted to its author for its production.

V.—FLINT: PHYSIOLOGY OF MAN.

THE PHYSIOLOGY OF MAN; DESIGNED TO REPRESENT THE EXISTING STATE OF PHYSIOLOGICAL SCIENCE AS APPLIED TO THE FUNCTIONS OF THE HUMAN BODY. By Austin Flint, Jr., M.D. Vol. V (with a general index to the five volumes). Special Senses: Generation. New York: D. Appleton & Co., 1874: pages, 517. Chicago: W. B. Keen, Cooke & Co.

We congratulate, most sincerely, the learned author of this work on the completion of his arduous task. There is almost the same discouragement, it would seem, attending the production of an elaborate new work on physiology, that must be felt in attempting to prepare an exhaustive work on organic chemistry. So rapid has been the progress of physiological research, in many respects, the last few years, and so various are the views that may be or have been taken of many of the still open questions in physiological science, that a work in order to be satisfactory, must often be re-edited to bring it fully up to the uncertain level of a constantly growing science. Hence the advantages that a mere manual possesses which deals in generalities, and on account of its size may be often recast to adapt it to exigencies that may arise. It is true, no striking discoveries are made these times which completely overthrow a doctrine, but in the general advance a work on physiology a few years old, becomes in a measure obsolete. Dr. Flint's work has not wholly escaped the fate to which allusion has been made. We have been especially impressed with this fact, upon reading, as we have done, his fourth volume on the Physiology of the Nervous System. Admirable as it may be as a piece of literary and scientific work, it is by no means up to the mark of to-day.

This is, however, no peculiar fault of Dr. Flint's work. It inheres in all. No one probably would feel the force of these remarks more than the author himself, and we can safely say, with him, of this great work, that "while the previous volumes might be modified by the addition of new facts, they contain comparatively little that has been disproved by recent investigations." For of all our indigenous authors on physiology, he is second to none in his qualifications, whether literary or scientific, to declare the present state of physiological science, either as regards matter or methods.

But whatever may be said of his earlier volumes, it can hardly be said of his last, issued during this present year. It will serve to show as well, if not better, than either of the others, the range of our author's acquaintance with the literature of the subjects in hand, and at the same time his style and method.

This volume is given up to the consideration of the organs of sense and the physiology of generation. The first of these will alone engage our attention—the chapters on these subjects show the author's careful research and good judgment, the anatomical descriptions are full and satisfactory, and his knowledge of the literature most thorough. Still we notice a few omissions in this regard that we wish might have been avoided; they do not, however, in any measure, affect the value of the book as a contribution to physiological literature and a summary of our present knowledge of the subject. We, in the Western States, can appreciate even more fully than its author the difficulty of producing a work faultless in this respect.

We are compelled to offer a single criticism on what appears to us, in some measure, an unsatisfactory conclusion in regard to a rather important physiological question. In speaking of what has been called the muscular sense, Dr. Flint says: "It seems that the weight of evidence is decidedly in favor of the view that there is no distinct perception of muscular action, aside from general sensibility, that can properly be called a muscular sense." Without claiming that this sense is proved beyond question to exist, we would still hesitate to hold that the presumption was altogether against its existence. Indeed, we are rather of the contrary opinion, and the authorities and evidences in favor of there being such a sense seem to us to be, at least, considerable. We may ask, perhaps, what is the sense of resistance to muscular effort which our author mentions in the immediate context of the passage quoted, and, which he states, has little dependence upon the sense of touch. The writings of Gerdy, Marce and others on this point and some still more recent investigations by Leyden have a very important bearing on the decision of this important question.

The portion of the work that is devoted to the two special senses of sight and hearing is especially of value. The chapters in regard to the former have, as Dr. Flint modestly states, been revised by Prof. H. D. Noyes, who has brought to the task the peculiar qualifications of a specialist in this department; those devoted to the auditory sense are the work of Dr. Flint alone. Both seem, in all respects, up to our present state of knowledge, and are exceedingly valuable as presenting more fully than in any other recent work of the kind, a summary of what is known in regard to these two important functions. The chapters on the special senses of tact, taste, and smell, are also, for the most part, satisfactory exhibits of the present state of science; in regard to the first, we may say that Dr. Flint mentions the special sensations of temperature, titillation, etc., though he seems inclined to refer them all to the general sensibility. He does not mention, however, the sensation of pain, which has been, by some, considered as a special sense, apart from that of tact. Many facts of observation and some recent experiments, those of Horvath for instance, seem to favor this view.

The preceding volume contained the greater part of the matter, the consideration of which comes within the special scope of our journal, and the length of our notice of the present one is no index to our estimate of its nature.

We have been frank in noticing those points where, in our judgment, it seems to be open to criticism; that they are few in number our notice will testify; they are the exceptions which prove the rule. The work is an honor to its author and to American medical science, and the hope of Dr. Flint as to its usefulness to the medical and general public is amply justified.

VI.—MAUDSLEY: RESPONSIBILITY IN MENTAL DISEASE.

Responsibility in Mental Disease. By Henry Maudsley, M. D., F. R. C. P., Professor of Medical Jurisprudence in University College, London, etc., etc. 1874.

This is an interesting book from an able author, upon an important subject.

The first two chapters are mainly introductory. The author is clearly a somatist, regarding, in common with nearly all mental psychologists of to-day, a disordered mind as a result of a disordered brain. He characterizes insanity as substantially "a disorder of the supreme nerve-centres, the special organs of mind, producing derangement of thought, feeling and action, of such a degree or kind as to incapacitate the individual for the relations of life." His definition of mind, *physiologically considered*, being "the sum total of those functions of the brain which are known as thought, feeling and will. By disordered mind is meant disorder of those functions."

The theory that morbid mental manifestations are the result of a morbid condition of the brain, is not inconsistent with the fact that insanity is often the result of what are called moral causes, and is cured by what are called moral means. Such are the intimate relations of mind and matter, for instance, that long-continued and intense application of the former may bring the latter into such morbid condition as to result in diseased mental phenomena. Then, by mental rest, the mind's physical instrument repairs, and the mind resumes its healthful manifestations. Pathology of the brain, as studied by the aid of improved instruments, teaches more and more clearly that morbid physical changes are concomitants of insanity; so that in the so-called functional diseases of the brain, or diseases in which we have hitherto failed to find actual physical changes, it is held that these changes do presumably exist in the intimate elements of nerve structure to which our senses have not, as yet, gained access. "It is believed that as, by means of the spectroscope, we have discovered facts that before were quite beyond our ken, or, as by means of the telescope, we have discovered stars which, without its help, would have remained unknown to us, so the time will come when, by the invention of improved instruments, the insensible movements of molecules will be as open to observation as are the molar movements of the heavens; and when those that come after us, will not fail to discover the physical causes of derangements which we are now constrained to call functional."

The "borderland" between sanity and insanity, is discussed; showing that the mental status in many cases is debatable; that the dividing line is not sharp and well defined, but diffused; and that it lies somewhere between undoubted insanity and doubtful sanity. These cases are often perplexing and embarrassing to the medical witness, when called to interpret acts which offend the law, and where mental impairment is pleaded in excuse. The author fairly discriminates between mere eccentricity and insanity. Though eccentricity is often the outcome of the insane temperament, it does not necessarily imply disease, or a marked change in the individual, as compared with himself at a former period of undoubted insanity,—mental derangement does.

In regard to certain physical conditions of moral degeneracy, the author makes three definite propositions: That "there is an insane temperament which, without being in itself disease, may easily and abruptly break down into actual disease, under a strain from without or within"; that "moral feeling, like every other feeling, is a function of organization"; that "an absence of moral sense is an occasional descent from an insane family."

In his classification, the author still adheres to the old term, "moral insanity," to designate a certain type of mental disease. The moral insanity of Pritchard, Esquirol, and our own distinguished countryman, Dr. Ray, consists, as has been characterized substantially by them, of perversion of the affective or emotional faculties, while the intellectual are not involved, and may exhibit itself in morbid fear or love, or hatred, dread, sadness, exaltation or depression of the feelings,—and, in short, by any exaggeration of the affective faculties without delusion, or intellectual aberration, or appreciable impairment of the intellectual faculties.

It is now more than half a century since "moral insanity" was first promulgated, but still the doctrine has never found favor in the higher courts of justice, in this, or any other country, nor with many of the best medical interpreters of morbid mental phenomena. It is noteworthy that nearly all the cases cited by authors, in illustration of this so-called type of insanity, are those that occurred many years ago.

In looking over reports on insanity from the hospitals in Europe and this country, we rarely see a single case, of many thousands, classed as moral insanity. It seems a pity that so able an author as Dr. Maudsley cannot emancipate himself from the use of a term so objectionable as moral insanity, when upon page 172 of his book he says: "One cannot truly say, however, that the intellect is quite clear and sound in any of these cases, while, in some, it is manifestly weak." Those who oppose the use of the term as incorrect and mischievous, claim only what is admitted in the paragraph above quoted: that in all cases of undoubted insanity there is some degree of intellectual impairment, and that, therefore, there is no excuse for perpetuating the use of a term that is not warranted by any known distinctive pathological condition, and which has proved a stumbling-block to learned judges, lawyers, ministers of the gospel, and the medical profession.

The chapters embracing partial, intellectual or ideational insanity, epileptic insanity, senile dementia, and the prevention of insanity, are able, clear and instructive, but want of space forbids the further notice of them. The book as a whole is among the best of all the contributions of the author to medical literature. R. J. P.

VII.—NOMENCLATURE OF DISEASES.

Nomenclature of Diseases. Prepared for the use of the Medical Officers of the United States Marine Hospital Service, by the Supervising Surgeon, John M. Woodworth, M. D.

Dr. Woodworth has reproduced in this volume the classification and English-Latin terminology of the provisional nomenclature of the Royal College of Physicians, London, which was first published several years ago. The alterations mainly consist in the transposition of the Latin and English terms, and the omission of the French, German and Latin Synonyms. The error committed in a former reprint of the nomenclature published in this country, of leaving out the index, has been avoided by Dr. Woodworth,—he reproduces the index, carefully verified, and occupying more than one-third of the book.

This official adoption of the work will in all probability open the way to its general use, a result which is, we think, in all respects a desirable one. In all events the thanks of the profession are due to Dr. Woodworth and the bureau under his charge for its reproduction in so convenient and attractive a form.

Editorial Department.

REVIEW FOR THE YEAR.

THE present number closes our first volume. That it is not inferior in any respect to the earlier numbers, we presume few would question who have taken the pains to examine them. It fully equals either of them in size, and does not fall behind them in variety and value of contents. We feel that we have fulfilled, at least in a reasonable measure, the promises of our "Prospectus." Recognizing the practical impossibility, in our own country, and the difficulty in any, of maintaining a periodical at once scientific and practical, having any considerable size, or comprehensiveness in range, which shall include only "original" matter, of real value, we neither expected nor have pretended to fill our journal with such material. Notwithstanding this we have been able to present our readers with original communications of interest to the profession, and have in prospect a large increase in this respect for the second volume. Though this is so, our main effort for the present will continue to be in the direction of making our readers acquainted with the immediate progress of neurology in all parts of the world.

A glance at the table of contents of the present volume, and at the list of names of contributors to the department of medical science with which we are occupied, to whose labors we have referred, will show better than can be done in any other way, the range of the JOURNAL in this respect. In the first department, where we have not had suitable original communications, we have endeavored to present the best and most useful selections and translations that were at hand, such as would possess something more than a transient interest.

The "Review" department we feel has not fallen behind that of any other medical periodical in our own country, and has been the medium of communicating to our readers comparatively full and critical surveys of many important works. We have spared no needed effort to secure every new monograph or work of value published abroad, that we might present the substance of them to our readers, at least so far as our space would permit. In this department the matter for the ensuing year promises to be of unusual interest. This is intended to be one of the most important features of the JOURNAL, and no effort will be spared to secure in it thorough and conscientious work.

The Editors see no good reasons for abandoning an "Editorial Department," hence it will be continued, and in it will be discussed such questions as require notice but cannot find a congenial place in the other departments of the JOURNAL.

The "Periscope," that has been so well supported during the past year, has been the means of conveying, in a condensed form, a great variety of matter of interest in regard to the anatomy, physiology and pathology of the nervous system. Great pains is to be taken, during the course of the second volume, to enrich this department with the most recent items of interest that may be made known, whether at home or abroad. The JOURNAL is intended, in fact, to be at one and the same time, a medium for the communication of new matter to the profession, and for faithfully reflecting the progress made everywhere in the domain of neurology. We have not published every item even of value that has come to our notice, but only a *selection* of such matters as seemed at the time most important. We could easily have filled twice the number of pages with useful intelligence. But, in its sphere, we feel our journal will compare favorably with any other in our own country, so far as matter and editorial labor are concerned. In this statement we are confirmed by the unsolicited expressions of favor that have reached us in various ways, but especially by letter and press notices, from those most competent to form a judgment in such a case, not only in our own country but abroad.

For these numerous kindly and encouraging notices, we would return our grateful acknowledgements.

The success of the JOURNAL, has, taken altogether, exceeded our expectations. The burden it has imposed, both in labor and expense, was not undervalued, and has been lightly borne. We are fully confirmed in our judgment as to the necessity for such a periodical, and wish to announce definitely that there has not been, and is not now, a thought of its discontinuance, whatever changes the future may bring.

To those who have encouraged us by becoming subscribers we return our thanks and heartily invite them to continue their patronage, and to use their influence as may seem best to them to increase its circulation. We assure them we will endeavor to make a journal worthy of the support of the profession, and, if we can, such as will be a credit to the periodical medical literature of the country.

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Necrology.—We have to notice with this issue of our journal the death of two illustrious workers in the department of neurological medicine.

Dr. Theodore Simon, whose death occurred in Hamburg last July, at the early age of thirty-three, was one of the most promising of the younger physicians of Germany, and his loss will be severely felt. He held, since 1869, the position of one of the chief physicians of the General Hospital at Hamburg, having previously acted as assistant physician at the Insane Asylum of Friedrichsburg, near that city. He is best known by his various contributions to the periodical medical literature of his country, and especially to the *Archiv f. Psychiatrie und Nervenkrankheiten*. Among his papers in the *Archiv* we may mention a series of articles "On the conditions of the spinal cord in dementia paralytica and granular myelitis," and his latest contribution "On mental disorders occurring in the course of acute articular rheumatism." He was also a frequent contributor to other journals, and is the author of several valuable monographs on psychiatrical subjects. We can safely say of him that there are few who in so short a time have done so much and done it so well.

Dr. Francis Edmund Anstie, the editor of the *Practitioner*, died in London, September 12th, in his forty-first year. His death is said to have been due to exposure while investigating the causes of an epidemic occurring in a large school near London, though its immediate cause was a dissecting wound of the finger.

Dr. Anstie was well known in this country by his two principal works, "Stimulants and Narcotics," and "Neuralgia and its Counterfeits," and from his position as editor of the *Practitioner*, to the pages of which he was a frequent contributor. He was one of the active workers in the department of physiology and therapeutics, and his death at this comparatively early age, is a serious loss to science. To what eminence he might have attained had his life been spared, no one can tell, but his name will not be forgotten among those who have contributed much for the advance of medical science.

Correction.—Our attention has been called to a historical mistake in our last number. In our notice of the Society of Neurology and Electrology, we stated that it was organized before the Neurological Society. This is incorrect in so far as the Neurological Society had had an actual though not a very active existence for two years, which had escaped our recollection, and its recent revival or reorganization was alone in our minds. We take pleasure in making this correction, as the mistake was altogether an oversight.

Periscope.

a.—ANATOMY AND PHYSIOLOGY OF THE NERVOUS SYSTEM.

Functions of the Brain.—Dr. J. J. Putnam, *Boston Med. and Surg. Jour.*, July 16, describes an experiment undertaken by himself to test the validity of the objections of Carville and Dupuy to the results of Ferrier. He operated by exposing the brain and finding the centres for definite movements and the minimal current strength to produce these, next cutting underneath these centres, making a thin but good sized flap, and then attempting to produce the reaction with the flap in place and with it turned back. He found that the movements could be brought about by electrical irritation of the exposed surface underneath the flap when it was turned back, but not to any appreciable extent, if at all, when the current was applied to the surface of the flap itself when it was in place. This he holds to be not in support of the theory of Carville and Dupuy, that the reaction is due to the diffusion of the current to distant motor centres, and not to direct stimulation of such centres in the cortex at the point of application.

Burdon Sanderson, *Centralbl. f. d. Med. Wissensch.*, No. 33, July 11, experimented by first determining in an anæsthetized cat the centre described by Hitzig and Ferrier, for the movements of the limbs and facial muscles, and the amount of irritation needed to call it into action, and then removing the superficial surface down to the corpus striatum, and there applying the current. The following results were attained: (1) The movements of the muscles of the opposed side of the body were produced with a weaker current than that applied to the surface; (2) that the point, the irritation of which on the surface of the brain, produces movements of specific groups of muscles, also is to be found on the upper surface of the corpus striatum; (3) that the locality in the corpus striatum corresponds to that on the brain surface. If the deeper portion of the corpus striatum is irritated, the animal opens the mouth, and alternately extends and retracts the tongue.

This experiment, says Dr. Burdon Sanderson, is therefore of importance, first because of the greater ease with which the phenomena are produced and observed; and second, because the "centre for the facial nerve" has been most distinctly and definitely located. If now it can be pointed out that this centre does not really exist, doubt arises as to the other motor centres, and the localization of movements in the cortex generally.

Similar investigations to the above are published by H. Braun, *Eckhard's Beitrage*, VII. (Abstr. in *Centralblatt f. d. Med. Wissensch.*)

Dr. E. Hitzig publishes, *Centralblatt f. d. Med. Wissensch*, No. 35, July 25, the following:

By the ablation in the region of the posterior lobes (Gyri *n. o.* Fig. 3 of his work, *Untersuch. ueber das Gehirn*), blindness of the eye of the opposite side and dilatation of the corresponding pupil are produced. The phenomena of the unilateral blindness are so characteristic that an error in this regard is impossible. On the other hand, there are produced by this method some slight additional injuries, the influence of which he has not yet accurately determined. Still, the opinion that we had here to do with the hemisphere is supported by the fact that the irritation of the same point was followed by a strong and lasting contraction of the pupil.

In regard to the above communication of Dr. Burdon Sanderson, he offers the following remarks :

(1.) The localization of the points for irritation on the cerebral surface does not agree with those described by himself.

(2.) That the corpus striatum is excitable had been previously proven by himself and others. But it does not follow from this that the hemispheres themselves are not also excitable.

(3.) In regard to this knowledge of the excitability of the corpus striatum, he had long before instituted special experiments, and had abundantly proved the want of dependence of the phenomena caused by exciting the hemispheres on this ganglion. The most conclusive experiment is described on pages 48 and 49 of his *Untersuchungen.* If his " lance rheophore " is pushed from the convexity outwards, the contractions cease very soon after the corpus striatum extern. is passed, notwithstanding the electrode is now much nearer this ganglion than when we merely irritate the convexity. Similar results follow when we make a deep cavity with the Daniels spoon, either in the anterior or posterior margin of the irritable zone, and then irritate the walls of this cavity. The corpus striatum can be almost reached without producing convulsions if the electrodes are applied to the side of the cavity furthest from the irritable zone, while they take place immediately when they are applied to the other side.

(4.) He considers it better to employ the galvanic current for researches in which accurate localization is required.

(5.) The results of paralysis experiments contradict the views of Burdon Sanderson.

FUNCTIONS OF THE OPTIC THALAMI.—Dr. H. Nothnagel, *Centralblatt f. d. Med. Wissensch.*, No. 37, gives the following conclusions, drawn from his experiments on rabbits, in regard to the functions of the optic thalami.

1. They have nothing at all to do with the innervation of voluntary movements.

2. Just as little does their extirpation produce any direct disturbance of the cutaneous sensibility.

3. On the other hand they appear to stand in direct connection with the " muscular sense."

A full description of his experiments is promised.

The Finer Structure of the Human Cerebellum.—Dr. Camillo Golgi, *Arch. Ital. per le Malattie Nervosi, 11*, 1874. (Abstr. by Obersteiner in *Psych. Centralblatt.*)

The results here communicated were for the most part obtained by means of a method first employed by Golgi, consisting in, first, hardening a small piece of brain in bichromate of potassa, and then coloring it with a one per cent. nitrate of silver solution; according to the longer or shorter duration of the hardening process, the various tissue elements are distinguished.

I. First, he describes a system of penetrating fibers which run parallel to the surface, especially in the inner third of the molecular layer. (For the most part these should be referred to the transverse connective tissue fibres described by Obersteiner which are peculiar to this region, and those fine fibres which Meynert hesitated to call fibres proper, but preferred to consider as transversely extended processes of the Purkinje cells. Ref.)

II. The existence of actual ganglion cells in the molecular layer was very easily proven by the method employed, but these will be present in the same proportion as in an equally large piece of the cerebral cortex (silver coloring is always a deceitful method ; these results ought invariably to be confirmed by other methrods). These cells possess from four to six, or more, processes, of which one is to be considered as properly nervous (axis cylinder process). In most cases this originates on one side of the cell and passes outward or downward. After going a little distance it begins, after it has become more slender, to give out fine branches, which again divide up into still smaller branches.

III. In regard to the Purkinje cells and their processes, nothing particularly new is given ; the central process generally passes directly to the medulla, but on its way gives off small branches, which connect with the granules of the granular layer, and have a great tendency then to pass outward to the most external cortical layer, there to enter the small ganglion cells.

IV. The granules of the granular layer are cells with from three to six processes, of which one is nervous while the others only serve to connect the cells with each other.

The larger nerve cells contained in this layer and their processes are very well shown by Golgi's method.

V. The medullary fibres exhibit, in the midst of the medullary branches, and especially also in the margin of the granular layer, very numerous ramifications, which join one another and also enter the granules.

In conclusion, Golgi holds that four systems of nerve fibres of different origin are present in the cerebellum.

1. The system of small ganglion cells in the molecular stratum.
2. The system of the large Purkinje cells.
3. The system of the granules.
4. The system of large ganglion cells, which we find in the granular layer.

Function of the Semicircular Canals.—Prof. A. Crum Brown, of Edinburgh, maintains, *Jour. of Anat. and Phys.*, May, 1874, that we possess a sense of rotation apart from and quite distinct from our other senses. The organ of this special sense he considers to be the semicircular canals, and he explains the difference in sensation from rotation in different directions by the arrangement of the canals. "So far as we know," he says, "a nerve current can vary only in intensity, and not in kind, so that if irritated at all, whether by right handed or left handed rotation, the nerve would convey the same message to the central organ. The solution of this difficulty which I proposed is as follows: Each canal has an ampulla at one end only, and there is thus a physical difference between rotation with the ampulla first and rotation with the ampulla last, and we can easily suppose the action to be such that only one of these rotations (say that with the ampulla first, in which case, of course, there is a flow from the ampulla into the canal) will affect the nerve terminations at all. One canal can therefore, on this supposition, be affected by, and transmit the sensation of rotation *about one axis in one direction* only, and for complete perception of rotation in any direction about any axis *six* semicircular canals are required, in three pairs, each pair having its two canals parallel (or in the same plane), and with their ampullæ turned opposite ways. Each pair would thus be sensitive to any rotation about a line at right angles to its plane or planes, the one canal influenced by rotation in the one direction, and the other by rotation in the opposite direction."

The arrangement of the canals favors, in his opinion, this theory, and the relation is the same in all animals. He states this relation thus: "In each ear there is one canal (the exterior) in a plane at right angles to the mesial plane, and two other canals (the superior and the posterior) in planes equally inclined to the mesial plane. In no other way is it possible to harmonize the bilateral symmetry of the two ears with the condition that each of the three axes shall have two *oppositely turned* canals in planes at right angles to it."

VARIATIONS OF NERVES.—Prof. Wm. Turner, in his *Journal of Anatomy and Physiology*, May, 1874, describes some anomalies in the arrangement of nerves which have come under his observation. He describes cases in which a branch of the fourth nerve supplied the orbicularis palpebrarum muscle, arising either directly from the nerve or from a plexus formed by filaments from it and the nasal nerve. In another case a branch from the fourth nerve distributed filaments to the periosteum of the orbit.

Cases were also seen in which the middle of the three supraclavicular nerves passed through a foramen in the clavicle in its course to the pectoral integument. This variation is stated to be more common than is usually supposed. Cases have been recorded by Bock, Gruber, Luschka, Clason and Cruveilhier.

Several instances of accessory roots to the phrenic nerve, derived from the nerve to the subclavius, are mentioned; also, branches from the same nerve to the external anterior thoracic and to the sterno-mastoid muscle. In one subject a branch from the ulnar nerve to the internal cutaneous was observed, and in still others, peculiarities of distribution of the branches from the brachial plexus are described. In one case an anomalous branch from the musculo-cutaneous in the upper part of the left leg, and joined the external saphenous at the external malleolus.

C. T. Hunter, M.D., *Phil. Med. Times*, July 4th, gives the following account of an anomaly of the right phrenic nerve observed by him:

"In tracing the nerve to its apparent origin I discovered that it arose from the anterior branches of the third and fourth cervical nerves, the normal arrangement. In addition, however, it received a communicating branch from the axillary plexus, which branch joined the phrenic nerve at a point immediately above the subclavian vessels. The course of the phrenic nerve was almost vertically downward from its origin along the outer edge of the scalenus-anticus muscle, and when it reached the base of the neck, instead of crossing the first part of the subclavian artery and passing between the artery and the vein, as in the normal distribution, it took its course in front of the third part of the subclavian vein, and descended into the thorax behind the right internal mammary artery, thence to its normal position on the outer side of the right vena innominata and superior vena câva. On examining the left side of the subject's neck I found the phrenic nerve in its normal condition."

PERSISTENCE OF SENSIBILITY IN DIVIDED NERVES.—At the session of the *Soc. de Biologie*, May 23d (reported in *Gaz. Med. de Paris*), M. Arloing, for himself and M. Tripier, made a communication on the persistence of sensibility in the peripheral extremities of divided nerves. Since 1869 these experimenters have been seeking to prove that the zone of distribution of a nervous branch is not closely limited, and that the persistence of the sensibility in the integument supplied by the divided nerve coincides with the sensibility of the peripheral portion of that nerve.

Since 1869 they have sought to determine the causes and conditions of the persistence of sensibility in the peripheral extremities, by paying attention to very diverse nerve branches: the fifth, seventh and eighth cranial pairs and the nerves of the extremities. They have invariably found the peripheral portion of these nerves sensitive after division, even in the solipedes, in which this fact had not been previously observed. In order to discover this sensibility it was needful to test the most peripheral portion. In all cases the sensibility of the peripheral end is due to the presence of nerve fibres which still retain their connection with the trophic and perceptive centres, after the section. This sensibility exhausts itself as we go higher up in the nerve trunk, and this fact explains the disappearance or noticeable diminution of the sensibility of the peripheral end when the divisions are made far from it.

THE COMMUNICATIONS BETWEEN THE CEREBRAL VENTRICLES AND THE SUBARACHNOID SPACE.—By Axel Key and G. Retzius, *Nord. Med. Ark.*, vi. 1874 (Abstr. in *Rev. des Sciences Medicales*).

In a previous very important memoir the authors have attempted to show, by the aid of injections, that the entire nervous system (central and peripheral) is suspended in a kind of serous cavity.

This serous system is constituted by the subarachnoidean space and its dependencies, among which we must include the envelopes of the vessels which penetrate the brain, and also the serous spaces and lymph passages of the sensory organs It is this cavity, with its multiple prolongations that contains the cephalo-rachidian liquid.

In the present work the authors seek to prove that the cerebral ventricles are themselves a part of this extensive serous system, and they here describe the apertures by which these cavities communicate with the subarachnoid spaces.

These openings are three in number. One, discovered by Magendie, who first described the cephalo-rachidian fluid, is situated in the middle of the inferior wall of the fourth ventricle.

This passage for the ventricular liquid opens into a large subarachnoid space on the under surface of the cerebellum (*cisterna magna cerebello-medullaris*).

The two other openings are bounded by the extremities of the *recessus lateralis* of the fourth ventricle, the internal part of the *flocculus*, and the usually semi-lunar shaped anterior border of the inferior wall of the fourth ventricle. The space so formed is contracted by the choroid plexus. The glosso-pharyngeal and pneuomogastric nerves pass in front of it, hiding it, in great measure,. and only permitting it to be well seen when they are drawn backwards.

Influence of the Nervous System on the Blood-vessels.—Putzeys and Tarchanoff, *Centralblatt f. d. Med. Wissensch.*, August 29, at the suggestion of Goltz have followed up the subject of his recent paper (*Ueber gefusserweiternde Nerven*), in *Pflueger's Archiv.* They have sought to subject the facts communicated by Goltz, to a thorough analysis, by means of new methods of research. Goltz investigated the alterations of the temperature of the skin and the toes in dogs, which were produced by irritation and section of the cord and nerves. In the theoretical considerations which he deduces from his observations, he states: (1) The existence of local nervous mechanisms in the peripheral parts, which cause *tonus*, and, to a certain degree, regulate the circulation; (2) The presence of dilator fibres in the sciatic nerve, which can be put into action, first, by simple section, and then afterwards by electrical and chemical irritation.

From these facts he was led to consider the increase of temperature which follows the section or irritation of the lumbar cord, not as a passive result produced by the parålysis of the nerves, but as an active phenomenon.

A. The first task of Putzeys and Tarchanoff, was the investigation in regard to the connection between the condition of the vessels, and the temperature of the paralyzed member. They publish the following experiments:

(1) If the sciatic nerve of a dog is divided in the upper thigh, and the toes then amputated in both hinder limbs, it is readily seen that the outflow of blood on the paralyzed side is much more considerable than on the other, where it is either very slight, or even barely perceptible. The same result follows the experiment in ducklings and frogs.

Finally, if we sever the roots of the sciatic nerve at their exit from the spinal canal, there likewise is seen an increased outflow of blood on that side.

(2) If now the peripheral portion of the divided nerve in these animals is irritated by means of an induction current, we immediately observe a direct reversal of the phenomena,—the outflow of blood ceases on the paralyzed side, and becomes very noticeable on the other. The same result follows irritation by common salt. This effect may be explained by the muscular contraction and tetanus that is provoked by the irritation.

The authors curarized young dogs and frogs, and still observed in them the same phenomena as in those not poisoned, with the exception in the case of the frogs, that the outflow of blood was only decreased, and not entirely stopped.

In order to make sure as to the condition of the vessels, they examined them directly, and were able, in ducklings, to see a dilatation almost immediately after the operation; the vessels of the foot were extraordinarily apparent—those of the natatory membrane showed more abundant ramifications, the blood was more arterial and redder than in the sound foot; irritation of the nerve of the side operated on with a moderate current, caused the vessels to again contract, while those of the opposite side became visible. Similar changes took place in the microscopic appearances of the frog's foot, which exhibited a decided turgescence of the vessels after section of the nerve.

If, on the other hand, the sciatic nerve is irritated after the section, either in the pelvis or in the thigh, the microscope reveals a contraction of the arteries which may progress as far as their actual closure.

(3) If, in the above experiment (2), the excitation is maintained for some minutes, the contraction is seen to give way to a dilatation, which can be only considered as a result of over-irritation, and not as an exhaustion phenomenon. If, then, the more peripheral portion of the nerve is irritated, the vascular contraction is observed anew.

B. (1) The division of the nerve, as in the cases of Goltz, caused a very notable increase of the temperature in the injured limb; but, if the peripheral extremity of the nerve in a curarized dog is irritated, there follows, instead of an increase, a lowering of the temperature, which, to be sure, does not progress so far as to make the paralyzed side as cool as the sound one, but the decrease ranges from 1½ to 2° C. (=2.7°–3.6° F.)

As was observed by Goltz, the temperature of the injured limb in dogs, is found, in the lapse of about three weeks, to be the same as that of the sound side.

If the effects of the section and irritation of the sciatic nerves on the condition of the vessels and the temperature of the part are compared, a parallelism is at once noticed; the dilatation of the blood-vessels corresponds to the increase, and their contraction to the decrease of temperature.

C. If the toes of both hind feet of a frog are cut off before the division of one sciatic nerve, the blood is seen to flow from the wounds on the paralyzed side in much greater quantity than from the other, where it merely trickles, if it runs at all. If now a new cut is made, some ten days later, in the membranes of the feet, it will be seen that blood trickles from both in about equal amount; then, if the spinal cord is divided in the middle of the back, or its under part is contused, a phenomenon, directly contrary to that first described is noticed,—very little, or no blood, flows from the paralyzed foot, while, on the other side, it flows freely. Some days later, the difference has again almost disappeared.

If the section of the cord is repeated in such a manner that greater intervals elapse between each injury, we still see the increase of hemorrhage from the limb which yet retains its nervous connections.

These last experiments also support those of Goltz; they show that a perfect agreement exists between the condition of the vessels and the bodily temperature, and, that the gradual return of the temperature to the normal, is the consequence of the re-instatement of the *tonus*.

From these facts the authors draw the following conclusions:

(1) The restoration of the *tonus* in vessels that have lost their connection with their automatic centres in the brain and cord, cannot be explained, as Goltz has already stated, except by the existence of local peripheral apparatuses, perhaps nervous in their nature, which we are not disinclined to place in the same category with those we find in the intestines.

In the first instance, the *tonus* was dependent on these local mechanisms; in the second, on the centres situated in the spinal cord. When the vessels are withdrawn from the influence of these latter, the others require a certain time to develop their full power. This increase of their activity is especially favored by the great amount of blood which flows through the paralyzed vessels. We are futher aware that, after the division of the spinal cord, their irritability is perceptibly raised. Ought it not, therefore, to be admitted that in a precisely similar manner the activity of the peripheral mechanisms which have the functions of regulating the vascular *tonus*, is in control after the divisions of the nerves which come to them?

(2) The sciatic nerve contains vaso-motor fibres.

(3) It is not yet proved that it also contains dilator fibres (in the sense accepted by Goltz), nerves which, moreover, according to our views, are unnecessary for the explanation of the various phenomena.

(4) The division of the nerves and of the cord, doubtless causes a very transitory excitation, and paralysis follows almost immediately.

(5) The dilatation of the vessels and the increase of the temperature that were often observed on galvanic irritation, are the effects of over-irritation.

(6) We may still add that the vaso-motor fibres after their section, are in a state of latent excitation, which, in consequence of energetic irritation, gives way to exhaustion.

We may also suppose that, under the same circumstances, the power of the local mechanisms is gradually exhausted. Also, without the intervention of dilator nerves, the increase of temperature that is observed after irritation or repeated section of a nerve, may be explained in the same manner.

If Goltz did not notice the contraction of the vessels immediately following irritation of the divided sciatic, it is to be attributed to the fact that thermometric observation, which he chiefly employed, is not the most favorable method of taking note of variations of temperature of very short duration, for, as is well known, the animal tissues change their temperature but very slowly.

THE POSTERIOR SPINAL ROOTS.—G. Gianuzzi, *Ricerche eseguite nel Gabinetta di Fisiologia della r. Univ. di Siena.* (Abstr. in *Centralblatt*, and in *Rev. des Sciences Medicales.*)

Some four years since, Gianuzzi noticed that the sensitive nerves, separated from their nutritive centre (the spinal ganglion), preserved their excitability longer than the motor nerves under the same conditions. Fede contested this discovery, supporting himself on the fact that a mixed nerve loses its electro-motor power four days after its division. The sensitive nerves are therefore altered in the same time, and as profoundly as the motor. New researches, however, have enabled Gianuzzi to maintain his own views. In fact, the researches of Du Bois Reymond, Schiff, Valentin, and those carried on by himself, show that divided mixed nerves preserve their electro-motor power for more than four days.

If we divide at the same time a certain number of the posterior spinal roots, they lose their excitability at the end of eight or ten days, and their fibres become fatty. If, on the other hand, we divide only one, it preserves its excitability for an unlimited period. At the expiration of even a month, the irritation of the central extremity of the root causes severe pain. By microscopic examination, we find that not all the fibres are degenerated—that a certain number of them are perfectly healthy, forming, sometimes, a clearly isolated bundle. Gianuzzi concludes, supporting himself with these facts, that there exists a kind of recurrent sensibility of the posterior roots. He holds that some fibres first leave the cord to enter a spinal ganglion, and then re-enter the cord, to leave it a second time at the level of an adjoining root. Lockhart Clarke, moreover, has described an analogous disposition.

When we separate the posterior roots, in a dog, from the spinal ganglion, they finally degenerate, not merely in the free portion, but also in their intra-spinal tract. But, in spite of this complete destruction of the posterior roots, the posterior columns of the cord remain sensible. These columns are therefore excitable in themselves, and do not owe their excitability to the sensible roots that traverse them.

b.—PATHOLOGY OF THE NERVOUS SYSTEM AND MIND, AND PATHOLOGICAL ANATOMY.

ALCOHOLISM.—At the sitting of the medical section of the French Association at Lille, Aug. 21 (reported in *Rev. Scientifique*), M. Leudet gave an exposition of the symptoms and progress of alcoholism as observed among the higher classes. He insisted on the frequency of this affection among the rich, and on the difficulty of its diagnosis when unsuspected. He called attention to the dyspeptic troubles, and ulcers of the stomach may sometimes be cured in spite of intemperate habits, but they undergo alternations of appearing and disappearing, until at last intestinal hemorrhages or hæmatemesis, by their abundance, bring on a catastrophe: the alcoholic jaundice, the affections of the liver, and cirrhosis in particular, which are observed more frequently in the well-to-do than in the laboring man as the result of drinking; the diarrhœas, which may last for a considerable time, the paralytic accidents, the singular phenomena of hyperæsthesia, wrongly attributed to rheumatism, and finally the gout, the development of which is undoubtedly advanced by the abuse of alcohol.

With the alcoholic icterus, M. Leudet cited a remarkable case of jaundice in a person who took large quantities of chloroform, and thus produced a peculiar kind of intoxication with loss of sensibility, and he remarked, in connection with this case, that in chronic chloroform poisoning he was able to obtain complete anæsthesia with very small doses of opiates, which ten times the amount would not have produced anteriorly to the chloroformization.

M. Verneuil, the president of the Section, supported the views of M. Leudet, and dwelt on the importance of the diagnosis of alcoholism in surgical operations, and on the tendency of the tissues of an alcoholic subject to take on necrotic inflammations after very slight injuries; he also noticed the frequency of pulmonary congestions in alcoholic patients under chloroformization.

THE CHEYNE-STOKES RESPIRATORY PHENOMENON.—We translate the following, by Dr. Huchard, from the *Union Medicale*, No. 30, April 25. Articles on the same subject by Traube, Filehne, Heitler and others have appeared in the *Berliner Klin. Wochenschrift* and other papers during the present year.

In the year 1856 Cheyne called attention to a respiratory rhythm which he first observed in a man affected with fatty degeneration of the heart, and who was suffering from an apoplectic attack. Still earlier, in 1854, Stokes, of Dublin, regarded as an almost pathognomonic symptom of cardiac steatosis in its later stage, this symptom of which he gives this exact description: "After a period during which the suspension of the respiratory function is, to all appearance, complete, there follow inspirations, at first short and feeble, but which augment progressively in force and depth, and acquire finally an extreme violence, and then gradually subside again till they are almost unnoticeable, beginning another period of apnœa."

Theodore von Dusch, in his *Treatise on Diseases of the Heart*, published in 1867, notices the existence of this symptom in these diseases, tumors of the brain, meningitis of the base, uremic coma, and in one severe case of pericarditis. Traube has also observed it in a case of contraction with aortic and slight mitral insufficiency, without fatty degeneration of the heart and without alterations in the cranium; he also observed it in certain cerebral affections (tumors, hemorrhages, and the latter stages of tuberculous meningitis). This symptom, which may recur many times in a relatively short period, and which must be carefully noticed, is produced, according to Traube, by the circumstances which may determine a diminution of the arterial supply of the medulla or locality of the respiratory centre (aortic or mitral disorders, fatty heart, cerebral hemorrhages, meningeal or intraventricular effusions, which diminish the intracranial circulation by an amount of blood equal to the effusion). The respiratory centre is thus rendered anæmic and less excitable. But, for the normal action of the cells of this centre, a certain amount of carbonic acid in the blood is required ; this quantity augments in the inspired air or in the blood, in consequence of cardiac or pulmonary disease, the breathing becomes accelerated, then diminished in frequency, and may be suspended entirely. But, if the medulla is less excitable, more excitation is needed, "by the apnœa such a quantity of carbonic acid as is required is accumulated, it then quickens the respiration until the carbonic acid is eliminated, the excitation then being insufficient a new pause takes place."

But the respiratory centre does not alone act in producing respiration ; it receives centripetal fibres (sensitive filaments from the pneumogastrics, the skin, and the mucous membranes), which transmit the sensitive impressions that make up the *besoin de respirer ;* these impressions collected by the cells of this centre are reflected on the phrenic and other nerves, the motor nerves of inspiration. But, in the normal condition, the peripheral ends of the pneumogastrics receive, says the author, blood richer in carbonic acid than that which goes to the other sensory nerves of the body. After the division of the vagi the respirations are separated by long intervals, and take on the dyspnœic character, because the vital knot only receives impressions from sensory nerves other than the vagus, and in order to render these nerves impressible, blood containing as much carbonic acid as is normally contained in the arteries of the lungs is required. Then this centre is more excited by the sensitive nerves of the skin and mucous membranes than by those of the lungs, and the respiration becomes dyspnœic. From this stage the mechanism of the Cheyne-Stokes respiration is easily understood from the explanation of Bernheim, who gives it briefly thus :

"During the pause, carbonic acid accumulates in the pulmonary system, the pneumogastrics are alone excited, the respiration is superficial, carbonic acid continuing to accumulate in all the arteries of the body, the sensory nerves are also excited and the breathing becomes dyspnœic. But these deep respirations eliminate the carbonic acid ; directly there is not enough to excite the sensitive nerves of the periphery, the breathing again becomes superficial, at last there is not enough to excite the vagi, and respiration is suspended."

Traube has called attention to the influence of morphia on the production or exaggeration of the Cheyne-Stokes respiratory phenomenon, especially in the affections of the heart and brain which can develop it, and explains that morphia diminishes the excitability of the nervous centres and that it adds its action to that of the ischæmia of the centres themselves.

Ziemssen, of Erlangen, has remarked that during the pause the pupils are contracted and insensible to the light, that they dilate at the first breath ; further, that during the pause he has observed a slight lateral deviation of the two ocular globes, repeating itself regularly on the same side, the suspension of the intelligence, the smallness and irregularity of the pulse, and its frequency remaining the same. Faradization caused a disappearance for a while of these symptoms in the patient, but he finally died.

Bernheim cites an observation of a case of mitral contraction which, after giving rise to symptoms of an anæmia of the mesocephalon, produced the respiratory phenomenon of Cheyne-Stokes. Several times, faradization of the sensitive nerves of the nostrils, the buccal and laryngeal cavities caused the respiratory troubles to disappear and brought the patient again to life.

In conclusion, therefore, in certain special cases, we observe the symptoms of an intermittent paralysis of the centre for respiration, characterized by certain disorders of breathing and closely resembling, in a pathogenic point of view, the ischæmic paralysis, characterized especially by intermission of phenomena. Sometimes this respiratory phenomenon is only slightly developed, one of its stages, the pause, may be lacking, but this ought not to invalidate its existence or prognostic value. Also, in a case of poisoning by coal gas, the respiration offered the following rhythm : Five or six labored and noisy respirations alternating with four or five superficial ones ; this series continued through some sixteen seconds, but there was in no case any suspension of breathing.

This symptom is always a grave prognostic but not always mortal. Such therapeutic measures may be of value as the electrization of the phrenic nerve, or the sensitive nerves of the face, thus suppressing the suspensions and rendering the respiration continuous ; but the use of morphia is to be condemned in all such diseases as provoke cerebral anæmia.

THE DISEASE OF THE MYSTICS.—Dr. Charbonnier has lately brought under the notice of the Academie de Medicine, in Paris, the disease under which Louise Lateau, the famous ecstatic, is now laboring. In a general review of the so-called mystics, he demonstrates that two natural conditions are indispensable to reach the mystic stage : the one, to profess a religion of which incarnation is one of the fundamental principles, so as to allow man to represent his divinity according to the form which accords with his desire and the climate he inhabits ; secondly, to submit himself to a debilitating regimen, which, while diminishing the activity of his physical functions, suppresses the superior faculties of his soul, to the advantage of the imagination—the *folie du logis.*

Amongst the Hindoos, as amongst the Christians, the ecstatic undergoes a long preparation, consisting, for the corporeal part, in proceedings likely to annihilate all the functions, and for the soul, in such as suppress the functions of relation and the exercise of the will, and in the concentration of all the strength in order to fix the attention on a single object. He says that ecstasy has never been produced at once and in a healthy subject. The history of all mystics bears witness to this, and Louise Lateau has appeared once again to confirm this law—a law which takes the occurrences which have lately taken place at Bois d' Hama from the category of the marvelous.

Louise Lateau, who began to show signs of the ecstatic state when, about 18 years old, was accustomed from her earliest infancy to the more than frugal regimen of the Hindoos—to the enforced fasts of poverty, to privations of all kinds, watchings, fatigue, and, in fact, everything which could impede the functions of nutrition. On several occasions she has manifested true suspensions of functional action, characterized by the absence of urinal discharges, constipation, sleeplessness, spasms of the epigastrium, and complete want of appetite—a group of symptoms which are often found assembled in Esquirol's observations respecting maniacs. This stoppage of the functions, this living mummification of the organs and tissues, sometimes lasts a month in Louise Lateau. Dr. Charbonnier believes this condition to be the same which is often met with in insane persons, who refuse to take nourishment with the greatest obstinacy.

When Louise Lateau had arrived at a certain stage of debility, she suffered from neuralgic attacks, the inseparable companions of an impoverishment of the blood in common with sleeplessness. *Sanguis moderator nervorum, sanguis somniferus.* These nervous phenomena have followed a sequence parallel to the aggravation of the general condition : painful neuralgias of the face, headaches, transient troubles of the intellect, slight attacks of delirium. After enforced feeding, which is prolonged more than a month, and hemorrhages, first by the mouth, then by the womb, and afterwards from the feet, hallucinations attended with delirium came on, which lasted several days. Some weeks afterwards an ecstasy came on. Such was the progress of the phenomenon.

Dr. Charbonnier, in an exhaustive memoir on the mystics, which he has lately presented to the Academy, makes an abstract of the chapter in it relating to the ecstatic state, showing the modifications of the organs of those afflicted by this disease, modifications which may explain how life is sustained, notwithstanding such prolonged abstinences, and which he terms the "substitution of organs"—a law which, according to him, is intercalated between the law of Malthus and that of Darwin.—*Brit. Med. Journal.*

HYDROPHOBIA.—Dr. Moriz Benedikt, *Wiener Med. Presse*, July 5, gives a paper on the anatomical alterations of the brain in dogs which have succumbed to this disease. He studied the subject by means of seven sets of frontal sections through the whole hemisphere, which he illustrates by means of a wood cut. By this means he detected some very striking morbid changes, which only the imperfection of the earlier methods of research allowed to escape detection.

First of all he observed a marked congestion of the vessels which wind between the convolutions, and a very striking example of the so-called "multiplication of inflammatory nuclei" on their walls and in the processes from the pia mater in their vicinity. Besides this he observed a transparent, strongly refracting, exudation, containing numerous points of inflammation, surrounding these vessels, consisting mainly of an increase of granular matter (granular disintegration).

The alterations in the gray substance were not less striking. There were to be seen with the naked eye or with a pocket lens, numerous cavities, which, magnified thirty to ninety diameters, were seen to be filled with a strongly refracting substance, consisting either of fine or coarsely granular matter in which we see scattered single granules as large as a swollen blood-globule. These bodies are perfectly colorless and transparent. In either form the so-called inflammatory nuclei are present.

In the matter which filled these larger cavities, ganglion cells were observed, either free or immersed for the greater part of their surface.

Besides these there were smaller cavities, filled with a somewhat smooth, shining substance. The vessels also, especially the smaller ones, had a hyaloid appearance. Many spots were strongly injected.

A peculiar condition was observed in the cutting of the cerebral substance ; the slightest pressure produced a shining exudation which the microscope proved to be myeline. Such an appearance was noticed by the author in one other diseased condition, viz. : in the spinal cord of a horse which died of rheumatic tetanus. It indicates a softening and extensive chemical alteration of the medullary nervous substance.

It must be remarked that not all but only a certain portion of the interlobular processes of the pia mater showed signs of inflammation.

We see from these results that the anatomical process in this disease consists in an acute inflammation with numerous hyaloid degenerations which, without doubt are produced through imbibition of the tissues through transudation, and with the hyperæmia and granular multiplication there is seen that form of diffuse local inflammation that Lockhart Clarke has designated granular disintegration.

There is no doubt that the anatomical process is the same in man as in the lower animals, although the symptoms offer numerous differences. The usual *post-mortem* appearances that have been described are hyperæmia and softening, but these have no very special signification, since they may be the result of asphyxia.

Dr. F. W. Lorinser, *Wiener Med. Wochenschr.*, Nos. 14 and 15 (Abstr. in *Cbl. f. d. Med. Wissensch.*), denies any specific poison in the dog which can produce this disease, and considers that apprehension and fear of its outbreak, kept up and increased through a considerable period, to be the actual cause of the affection, and that if the fear of it could be abolished the disease would cease to exist.

Dr. Fereol read before the *Acad. de Medicine, Paris*, July 21 (reported in the *Gaz. Med. de Paris*), a paper entitled : *Note sur un case de hydrophobie rabique survenue deux ans et. demi apres le morsure d'un chien enrage.* After giving the details of a long observation he summed up as follows:

(1) The incubation of hydrophobia, generally limited to the first two months after inoculation, may, in exceptional cases, extend itself to a much longer time, to eighteen months, or even to two years and a half.

(2) The symptoms of hydrophobia, habitually very uniform, may take on very different appearances under the influence of various elements (mental alienation, alcoholism, hysteria, etc.). But there are certain symptoms, such as the respiratory spasm, the particular manner of expectoration, the symptom described under the name of *earophobia*, that belong solely to this disease, and which ought, in most cases, to allow us to diagnose it under all its complications.

(3) If the *essential* or imaginary hydrophobia, which usually terminates in recovery, may end fatally, we ought to find in the symptoms sufficient

reasons to convince us that we do not have to do with a genuine case of madness.

(4) The bronchial foam plays an important part in the expectoration in sufferers from rabies, and the principal symptoms, as well as the principal lesions in hydrophobia in the human species, are grouped around the respiratory function. It is this respiratory character that distinguishes the rabid hydrophobia from all other hydrophobias.

ANÆSTHESIA AND HEMIANÆSTHESIA.—Dr. Virenque, *Brochure*, Paris (Abstr. by Obersteiner, in *Psych. Centralblatt*).

Unilateral loss of sensibility occurs either in the course of hysteria or hystero-epilepsy, or it is due to the destruction of certain parts of the brain, either from hemorrhage, softening, or compression by a tumor, or it may be caused by an injury.

In the two first-named cases (hysteria and hystero-epilepsy) the paralysis is remarkably variable, usually incomplete and not infrequently accompanied in hystero-epilepsy by partial contraction on the affected side.

But when the hemianæsthesia is connected with a destruction of substances in the cerebral centres, it is on the other hand, constant, persistent, always limited to the affected side, and changes, in case no further damage takes place in the brain, only very slowly, for better or for worse.

While Turck has observed hemianæsthesia to follow the destruction of medullary substance situated in the basal ganglia, it is also often met with after injury to the crura cerebri, which last cause may be diagnosticated from the first by the non-partaking of the special senses of sight, hearing, smell, etc., in the disturbance.

Seven observations of hysteria and hystero-epilepsy in which hemianæsthesia occurred, give the following conclusions :

(1) That this form of anæsthesia first appears after evident symptoms of hysteria have existed, for a longer or shorter period.

(2) According to the statistics of a large number of cases, a greater number of these paralyses occur on the left than on the right side ; in the seven cases reported, in five it was limited to the left side.

(3) The course and location of the paralysis, like the other symptoms of this neurosis, show a changeableness and inconstancy.

In conclusion, five case of hemianæsthesia (mostly in persons of drinking habits) are reported, in none of which death occurred, and *post-mortem* appearances are therefore wanting.

COLOR BLINDNESS.—A. Fick, *Wurzb. Verhandl.* ii, 129-133 (Abstr. in *Centralblatt f. d. Med. Wissensch.*), states that the usual view that the normal red blindness of the median and the perfect color blindness of the equatorial retinal zone is due to the latter containing fibres only sensitive to blue and the former to blue and green, cannot explain the facts. If this view were correct, then any irritation whatever ought to produce the sensation of blue in the equatorial, and of blue green, or bluish green, in the median zone.

On the other hand, all the phenomena are readily accounted for if we suppose that all three kinds of fibres are present in all parts of the retina, but that their reaction to light of different wave-lengths is different, so that the difference of the irritability of the three kinds is greatest in the middle and gradually diminishes toward the equatorial zone. Pathological color-blindness is then to be considered as due, not to the absence of any of these fibres, but to a diminished difference in their irritability.

THE MALADY OF AZANON.—Dr. A. San Martin, *Siglo Medic.* Feb. 9 (Abstr. in *Virchow and Hirsch's Jahresbericht*), describes a peculiar disease of the spinal cord, endemic in several districts in Spain, recently come under medical observation, and which, from the nativity of the first subjects which were examined in Madrid, has received the title of Azanon sickness. This place, in the province of Guadalajara, is situated on a high, arid and sterile limestone plateau, on which pellagra is endemic, as well as in the surrounding country. Chronic nervous diseases (locomotor ataxy in particular) are also frequent. The disease, about the end of the year 1871, without any accompanying noticeable climatic or nutrition changes, began to attack hitherto perfectly healthy persons, without respect to age, sex or constitution, and showed itself nearly at the same time among the residents of several adjacent localities. A series of symptoms indicating an affection of the anterior columns of the cord, are characteristic of this disease ; the patients, without being feverish, have convulsive tremors in the lower extremities which they are unable to control, with paresis of the muscles, more especially the flexors, paralysis of the sphincter of the bladder so that the urine continually dribbles away, and at the same time they complain of formication in the affected part, sometimes also of pain in the sacral region, while pressure over the spinal column provokes no pain. Otherwise sensibility is not affected ; there is neither neuralgia nor anæsthesia, the co-ordination of the movements is perfect, nor are any further disorders noticeable in the bodily or mental functions. All therapeutic measures have so far proved useless.

As regards the anatomical alterations in this disease, we are left entirely to conjecture, as in none of the patients has it yet proved mortal ; the cause is also left in obscurity.

Since the attention of the Spanish physicians has been directed to this affection, communications have been received from various other regions in the provinces of Guadalajara, and from a number of localities in the provinces of Zamora, Soria, and others, showing that this malady as an endemic disease has a considerable extension throughout Spain. One case is mentioned in which the paretic symptoms had begun to make their appearance in the upper extremities and in the tongue ; in others it was observed that the patient moved the affected limbs with more ease when in the dorsal *dembitus* than when in the upright position, and the sensibility in the paralyzed members was somewhat diminished. One observer (Dr. Adradas) noticed that in a series of cases the disease followed a severe cold, and therefore considers that it is not a central but a peripheral paralysis. ("How about the paralysis of the sphincter vesicals?") The circumstance that it occurs where pellagra is also endemic, is worthy of consideration, in the opinion of the German reviewer.

Spasmodic Asthma.—Dr. C. T. Williams, *British Med. Jour.*, June 13, in an interesting paper, upholds the theory that asthma is due to spasm of the bronchial muscle, and combats the view advanced by Dr. Berkart, that it is due to deficient elasticity. When it arises from local causes he attributes it to the irritation acting on the pneumogastrics, and exciting it by reflex action. When it arises from emotional disturbance the irritation is centric and acts through the pneumogastrics on the pulmonary plexus. That due to indigestion is similarly excited, and when the disorder is due to disordered state of the blood, as from gout, or skin disease, the blood is to be considered as producing the local irritation.

The tendency to asthma which is observed sometimes after measles or hooping-cough, is attributed to enlargement of the bronchial glands compressing or irritating the vagi; heredity is also to be taken into account when we consider the asthmatic tendency. The close connection which asthma has with other nervous affections, the fact that it is found to alternate with gastrodynia, neuralgia, etc., is an evidence of the nervous nature of the disease.

In regard to treatment, Dr. Williams recommends first of all, the removal of the exciting cause when possible. If the glands are enlarged, iodide of potassium is often very useful. Gout, syphilis, and skin diseases must be treated according to their special indications. In hereditary asthma he finds benefit from the use of gymnastics, and change of air if the surroundings are not favorable; the smoky atmosphere of cities being generally better than that of the country for those suffering from this disease.

The medicines for neurotic asthma are the stimulant and sedative anti-spasmodics. Among the former he classes alcohol, coffee, sp. ætheris, and nitrite of amyl. Dr. Williams does not find these, as a rule, as efficacious as the sedative anti-spasmodics, though he has seen coffee and spirits of ether give relief. Nitrite of amyl has generally failed, besides it is a dangerous medicine to trust to patients.

The sedative class includes the larger number of remedies, among them stramonium, lobelia, Indian hemp, tobacco, belladonna, etc., and they are often wonderfully effective. The various pastilles and papers are often very useful. In very severe spasms, when exhibition of medicines by the mouth seems impossible, the hypodermic injection of morphine or atropine often gives instant relief, but this is not admissible if there be much emphysema. The same rule applies to chloroform, which, when there is no lividity, generally relieves. As this is not exactly a safe remedy to trust to patients, Dr. Williams has employed chloral, which he recommends. In slight cases it should be administered in doses of twenty or thirty grains once or twice during the night; in severe cases fifteen or twenty grains every three or four hours till sleep is induced. In the majority of cases this treatment has succeeded in allaying and diminishing, or even abolishing, the attacks.

Temperature in Encephalitis.—T. Charlton Fox, *Brit. Med. Journal*, June 6, calls attention to the variations of temperature in encephalitis, and states that in his observation, it is always higher in the morning than in the evening. He gives in brief the account of a case in which this proved a valuable aid to the diagnosis, although, as he did not attend the case to the end, his judgment was not acted upon. He states that in cases where there is doubt he makes it a rule to observe both morning and evening temperature before excluding the possibility of encephalitis.

Relations of Affections of the Optic Nerve to Cerebral Lesions.—M. Raymond reported to the *Soc. de Biologie*, June 27 (Abstr. in *Gaz. Med. de Paris*), a case of tumor of the cerebellum, which was observed at the hospital of La Pitie, in the service of Professor Vulpian. Appended to his report he gives some bibliographic notes relative to the alterations of the optic nerves, consecutive to cerebral tumors, communicated to him by Dr. Landolt. We give the concluding paragraphs as to the opthalmoscopic phenomena that are observed.

1. Atrophy of the optic nerve, without antecedent neuritis, is found in many different affections of the cerebrum, the cerebellum, and the spinal cord : tumors, softening, dementia, locomotor ataxia, etc., without our being able to draw any conclusion as to the nature or locality of the disease from the opthalmoscopic appearance of the papilla.

2. Optic neuritis (true). Reddish swelling, yellow tint of the papilla and surrounding retina, which has lost its transparency, become turbid, and full of leucocytes. Active and passive dilatation of the vessels, occasionally retinal apoplexies, hypertrophy of the connective tissue.

This neuritis is almost without exception, followed by a complete atrophy of the papilla and of that portion of the retina which takes part in the inflammation ; so that we find the papilla pale, surrounded by circles (whitish ring of newly-formed hypertrophied connective tissue).

This form of optic neuritis is nearly always the result of the extension of a meningitis. It is evident that this meningitis itself may be either a simple meningitis or a complication of some other cerebral disease.

3. Swelling of the optic papilla by venous stasis.

We call this also an optic neuritis, because the opthalmoscopic appearances very much resemble those of a true neuritis, but it is not a true inflammation.

It is the result of venous stasis. The dilated and tortuous veins accompanying apoplexies, have given rise to a serous effusion, which fills the tissue of the papilla, and causes a swelling much more considerable than that of neuritis properly so called. The color is rather grayish, semi-transparent. The arteries are in great part hidden in the swollen tissue. The retina is less altered than in the preceding case. This œdema of the papilla may come to an end in a short period, without optic atrophy or loss of sight ensuing, but nearly always the cause is of longer duration. A true neuritis may develop itself, or without the intervention of a veritable inflammation, this œdema of the papilla is followed by an atrophy, which, however, does not usually extend itself over the adjoining retina, forming the circle which we find after a true neuritis.

This process is sometimes the result of a compression of the cavernous sinus (in consequence of a stasis in the ophthalmic vein), but it is oftener the result of an augmentation of the intracranial pressure, causing a stasis of the liquid of the intervaginal space of the optic nerve, which is in direct communication with the arachnoidean space of the brain.

The swelling of the optic nerve from venous stasis, may therefore accompany all the cerebral diseases that augment the intracranial pressure: hydrocephalus, tumors, etc.

The localization of the cerebral disease by the simple ophthalmoscopic image (without other symptoms), is therefore nearly always impracticable. As we have explained, we may sometimes draw some conclusions as to the nature of the malady, but not always.

The only case in which we are able to draw a certain conclusion as to the nature of the disease, is when we find tubercles of the choroid; then we can correctly diagnose tuberculous meningitis, simply from the ophthalmoscopic examination.

Hypertrophy of the Ear after Excision of a part of the Sympathetic.—A. Bidder, *Centralbl. f. Chirurgie*, No. 7, 1874 (Abstr. in *Centralbl. f. d. Med. Wissensch.*), observed one month after the resection of 1½ mm. of the left cervical sympathetic in a half-grown rabbit, that the left ear was noticeably wider and slightly longer than th right; also hyperæmic and warmer. The left pupil was half the size of the other, and the eyeball more sunk in the orbit. He attributes this increased growth of the ear to the removal of the influence of the sympathetic, allowing a greater influx of nutritive material in the still young animal, in which nutritive change was rapid and active.

Miliary Sclerosis.—W. B. Kesteven, *Brit. and For. Med. Chirurg. Review*, July, 1874, gives a list of twenty different pathological conditions, in which his notes contain observations of this alteration in the nervous tissue of the brain and spinal cord. These observations include cases of acute meningitis, locomotor ataxy, idiocy, tetanus, myelitis, hydrorhachis interna, dementia, general paralysis, puerperal mania, and many others. From the variety of these conditions he concludes that it belongs to some chronic morbid condition of the nerve tissue, and that when found after death from acute disease, we must assume that it pre-existed. The suggestion that it is a post-mortem change, resulting from the action of alcohol upon the tissues is rejected, since it is found in preparations which have not been submitted to this action at all, and because it is not invariable in specimens which have been preserved in alcoholic liquids. He concludes that "so far as the present amount of evidence permits, this extends no further than to the inference that in a large number of diseases of the nervous centres, the significance of miliary sclerosis is a slow change or degeneration of the neuroglia, preceding or underlying the group of symptoms which have constituted the antecedent malady."

Zona and Intercostal Neuralgia, connected with Cancer of the Lung.—M. Ollivier, at the *Seance* of the *Soc. de Biologie* May 2, (Abst. in *Rev. Scientifique*), communicated the observation of a case of zona, followed by an intense neuralgia, and apparently connected with a cancer of the pleura and the lung. The subject was an old man, who was attacked two months subsequent to the zona with a very intense intercostal neuralgia. During the crises the affected parts were covered with minute drops of sweat, the patient affording a beautiful example of the local perspiration which has been before described in a valuable paper by M. Ollivier. The pain on percussion was so great, and the expansion of the thorax so slight, that the examination of the chest could not be made in detail.

At the autopsy there was found a cancer of the lung and left pleura. The neoplasm surrounded not only the intercostal nerves of that side, but also the envelopes of the dorsal portion of the cord. M. Ollivier thinks that the nervous alterations, consecutive to the propagation and invasion of the pulmonary cancer, caused first the zona and next the intercostal neuralgia.

Reflex Neuralgia of Traumatic Origin.—M. Ollivier reported, at the session of the *Soc. de Biologie*, April 18 (Abst. in *Rev. Scientifique*), the observation of a case of traumatic neuralgia, developed at a distance from the injured point, and which enters into the class known as indirect or reflex neuralgias. A woman thirty-two years old, received a violent blow of the fist at the level of and two centimetres within the fifth intercostal space. She felt at the time a severe pain, which lasted two hours and then disappeared. Ten days later she felt lancinating pains, or twitches in the place struck, perfectly localized within a space the size of a ten-franc piece. Two months afterwards, independently of these twitches, there came on pains which were felt successively in the regions innervated by the internal cutaneous, and which next invaded the infra and supra-clavicular, and cervical regions. These pains were violent, continuous, and accompanied with exacerbations, which, always commencing at the point which received the original injury, extended outward to a considerable distance. A few injections of morphia at the spot originally injured cured the neuralgia. M. Ollivier considers that the contusion modified the circulation, or the molecular condition of the cord, in such a way as to produce at the terminations of the sensory nerves the painful sensation. This interpretation seems to him more just than the opinion which admits the propagation of the irritation by the aid of anastomoses, the existence of which is not at all determined at the level of the fifth intercostal space.

Physiological theory of Hallucinations.—We copy from the *Revue Scientifique*, May 23, the following notice of a *brochure*, entitled "*Theorie Physiologique de l' Hallucination*," by Dr. Ant. Ritti, lately published in Paris.

"The work of M. Ritti has for its basis the doctrines of M. Luys on the constitution of the nervous centres. We will attempt to give them in a few words. In the interior of the optic thalami, by sections regularly made, we find superficially four nuclei of gray matter, described under the names anterior, middle, median, and posterior centres. Each of these centres is a ganglionary mass, and, as such, receives afferent and gives out efferent fibres. As regards the directions of these fibres, descriptive and comparative anatomy, joined with pathological anatomy and experimental researches, enable us to determine them with sufficient accuracy. We are thus enabled to know that by the afferent fibre the anterior centre is connected with the olfactory nerve, the middle one with the optic, the median with the nerves of general sensibility, and the posterior with the acoustic nerve, and that, moreover, the afferent fibres connect these centres with the corresponding regions of the cortical layer.

"The optic thalami are therefore intermediaries between the organs of sense, where the impression is received, and the cortical portion of the cerebrum, where it is perceived. But what idea ought we to have of the *role* of this intermediary? M. Luys considers it as the *sensorium commune*, the place where, to use the expression of M. Ritti, the material action of the impression is transformed into psychic action. The different nuclei play the part of modifiers, in respect to the impressions they receive; they absorb these impressions, they elaborate them after a fashion by making them undergo a metabolic action, which, in giving them a new form, renders them more perfect and more assimilable by the elements of the cortical substance, where they are finally received.

"The physiological action in perception may therefore be summed up as follows: (1) Exterior impression; (2) transmission of the impression as far as the optic thalami; (3) modification of this impression in the thalami; (4) finally perception in the cortical layers. These facts being admitted, M. Ritti deduces from them his theory of hallucinations. Let us suppose that the exterior impression and its transmission to the optic thalami be suppressed, and that then the cells of the thalami enter into action, from any cause whatever, either from a circulatory disturbance or any other accidental cause, there will first be put in action a certain amount of molecular movement, which, according to the theory, becomes at once transformed into psychic action; next this movement follows its usual routes, and irradiates toward the cortex, where, by the ordinary mechanism, the action of this substance is performed as it were spontaneously. But this hypothesis is borne out in some if not in all cases of hallucination. The hallucinatory phenomena consisting in the perception of sensations, with any external object giving rise to them, the sensorial element will be due to the irritation of the ganglion cells of the optic thalami, whence the externality of the phenomena; and the psychic element will be the result of the consecutive vibration of the cells of the cortical substance. The morbid dynamism of this anatomical pain, united by the white cerebral fibres, produces the phenomena of hallucination. (p. 51.) Other authors have sought the anatomical localisation of the hallucination in a special irritation of the external organs, but M. Ritti has well shown, following Baillarger, that this hypothesis is irreconcilable with a great number of facts, for example, the persistence of the hallucination after the destruction of the sensorial nerves or even the exterior organs of sense. This first opposing hypothesis being disposed of, two new ones may present themselves. We may consider the point of departure for the false impression to be either in the cortical substance itself, or rather in an intermediate point other than the optic thalami, between this and the external organs, such as the nuclei of origin of the sensorial nerves. M. Ritti objects to these two theories, that they do not explain all the facts. His own, on the other hand, has the merit of accounting for them. The automatic irritation of the cells of the thalami may extend itself to one of these nuclei; it is easy, therefore, to understand how the hallucination may be limited to a single sense, in the same way we see how it may progressively invade the others. It may happen that only one of the thalami is the locality of the irritation, and we have in this case a unilateral hallucination. And, in fact, alienists have observed cases in which the patient heard voices only with his right ear, and had the hallucinations of sight only in his left eye.

"Moreover, the thesis of M. Ritti contains a great number of anatomico-pathological proofs, and 'we may admit without prejudice to the true spirit of science, that the lesions of the optic thalami found in the insane, who have for a long period suffered from hallucinations, that this limited region of the encephalic mass is not unconnected with the production of this morbid symptom, and that there is here the relation of cause and effect.'" (p. 64.)

We will not attempt in this place to ascertain whether this theory, which limits to the optic thalami the anatomical location of hallucinations be not a little too exclusive, and whether there are not cases of hallucination connected with simple lesions of the cortex, because we can here give only a very rapid *resume* of the work before us.

Transfusion in Insanity.—Limited at first to cases of profuse hemorrhage, the employment of transfusion has gradually been extended to cases of various alterations of the blood. Professor Livi, of Modena, in practicing, with success, repeated transfusions of venous blood in an old demented pellagrous subject, emaciated and prostrated with a persistent colliquative diarrhœa, has inaugurated a new application of this mode of treatment. Professor Caselli has also performed, on the 10th of last May, a direct transfusion from the carotid of a lamb to the median vein of a young atonic lypemaniac, with a tendency to strange cataleptic attitudes.

The object of these tentative attempts which was to relieve an unhappy existence, condemned to a strange and painful delusion, has been so far attained, that M. Livi has repeated the operation on other aliens, and Dr. Ponza has followed his example in the asylum of Alexandria. On June 21st last, he invited all the principal medical men in that institution to observe a transfusion in two young lambs, and next in a pellagrous insane person, suffering for many months from diarrhœa. Phlebotomy was first practiced, against indications of plethora.

A second transfusion has also been performed at the asylum of Reggia, by Dr. Trebbi, on a lypemaniac, with ideas of persecution, and a tendency to suicide. A third has since been performed, by M. Caselli, on another lypemaniac of the anxious type, with ideas of persecution. About eighty grammes (two and a half ounces) of blood were transfused; some cyanosis was observed, but it promptly disappeared.

The results of these attempts will doubtless soon be published, but if this ingenuous idea of inoculating, so to speak, with a new life the atonic insane which fill our asylums is crowned with success, we may say that a new era will be begun in the treatment of psychopathics, rebellious to other means. Moreover, the experiments are still being carried on, and two other transfusions were performed, June 30th, at the asylum of Alexandria. (*Gaz. della Cliniche*, No. 26.) *L'Union Medicale*, July 30.

NEUROSES OF THE JOINTS.—Dr. Moritz Meyer contributes to the *Berliner Klin. Wochenschrift*, No. 26, June 29, an article on the peculiar neuralgic affection of the articulations which were described by Brodie, some fifty years since, under the designation "hysteric joint affections," and which have been more recently noticed by Stromeyer and Esmarch. The disease consists in an affection of certain nerves of the capsules of the joints or their surroundings, and the pain frequently renders the articulation immovable, or useless for a long period, or even throughout life. Its etiology varies; it may exist in persons of nervous habit, without any known cause whatever, or it may be traced to psychic or emotional disturbance, injuries of various kinds, and, sometimes, in a reflex manner, to gastric disorders, or irritation of the urinary or genital organs.

The diagnosis from inflammatory affections of the joints is important, and the distinction must be carefully made. The leading points of difference are given as follows :

(1) In the purely neuralgic trouble, the nightly exacerbations are almost invariably wanting.

(2) The painful joint is usually very sensitive to light contact or touch, while powerful pressure and striking the joints together is not particularly painful.

(3) Œdema and swelling of the skin and cellular tissue not infrequently appear and disappear.

(4) The temperature of the affected joint sometimes alters periodically.

(5) In spite of the inactivity, lasting even for months, the muscles are still only comparatively slightly wasted.

(6) The very insignificant objective symptoms are altogether out of proportion to the complaints of the patient.

(7) If the attention of the patient can be diverted, the invalid often forgets his symptoms, and the physician himself can flex the painfully rigid limb to a greater or less degree.

The cure sometimes takes place spontaneously, through the outbreak of a hysterical attack, through faith, or in other cases after sudden impulsive exertion, escaping from danger, etc. When the neuralgia is of reflex origin, the cessation of the primary disorder may bring it to an end.

As regards treatment, Stromeyer and Esmarch have employed moral treatment, with nutritious diet, change of scene, etc., with excellent results. Together with these, they recommend passive movement of the affected articulations, and finally, the active use of the limbs. Dr. Meyer adds that the induced current, directly applied to the painful spot, is also a very effective aid. Several cases are reported in which this formed a prominent feature in the treatment, and was followed by cure.

PARALYSIS OF THE CERVICAL SYMPATHETIC.—W. Nicati, *Annales de Societe de Med. de Gand*, January, 1874. (Abstr. in *Allg. Med. Central-Zeitung.*)

This disease is, according to the author, not infrequent, and he gives the following description of its appearance:

(1) The prodromal stage is characterized by irritative phenomena and cramps of the muscles which are dependent on the influence of the cervical sympathetic, mydriasis, exophthalmus, lowering of temperature, and quickening of the pulse. These may either disappear, the normal condition returning (as was noticed by Vernieuil, after opening an abscess in the neck), or they may pass on to paralysis.

(2) In the first phase of the paralytic stage, we observe the alterations in the pupil and vessels of the eye, that have been noticed in the experiments on animals; namely, partial closure of the eye, contraction of the pupil, diminution of intraocular tension, turning of the eye inwards, and then, injection of the vessels, increase of temperature, and frequent perspiration on the paralyzed side.

(3) After some months or years, a second stage of the paralysis appears. The paralyzed side becomes less full, pale, its temperature falls, and the transpiration becomes greater than on the sound side.

(4) Between these two phases of the paralysis, there is an intermediate stage—that of imperfect atrophy.

As regards the pathogeny and pathology of this affection, the author mentions lesions of the trigeminus, which contains part of sympathetic fibres; also, lesions in the neck, in the breast and in the cord. In a special chapter he treats of disorders of the centre for the sympathetic in the medulla, and which, in his view, may produce Basedow's disease and diabetes.

DIPHTHERITIC PARALYSIS.—Sir John Rose Cormack, M. D., read a paper on this subject before the British Medical Association at its late Norwich meeting, which is thus reported in the *Brit. Med. Journal*, August 29th:

The author began by describing a minutely-observed, very severe, but typical case, of diphtheritic paralysis. The main object of the paper was to elucidate the natural history of the affection, which he looked upon as the true guide to the prognosis and treatment of each case—so far as a guide exists, irrespective of the individual peculiarities of the patient, and the character of the disease in respect of the district, season, and race in which it occurs, and the constitution of the prevailing epidemic, should the disease be prevailing as an epidemic. The most skillful physician cannot cure pneumonia, typhoid fever or diphtheria; but he can guide to recovery many cases of these diseases, which would be lost by the routine administrator of remedies. Medicines are sometimes exceedingly useful in diphtheritic paralysis, as well as in the earlier stages of diphtheria; but in each case, and in each epidemic, we find that the efficacy of particular remedies varies with the variation in the therapeutic opportunities. The author regarded a generous, easily assimilated diet, in all stages and forms of diptheria, as generally the basis of the treatment: ferruginous medicines were nearly always useful, but, like all other medicinal agents, they had their times for being given and for being withheld. In diphtheritic paralysis, the persistent use of local stimulants and small blistering bands (according to a method described), constituted, perhaps, the most valuable treatment. Electricity had its opportunities, and was, sometimes, most useful. Change of air, the douche, and short courses of nux vomica, were agencies which frequently gave a start to a lagging recovery. Still, we must never lose sight of the fact that the paralysis has a definite career to run; and that, if the patient were only to eat and drink well, and digest his aliment, he will, at the end of a longer or shorter time, be restored to health,—provided always, of course, that no insuperable obstacles to recovery exist, such as implication of the muscles of respiration in the paralysis.

In discussing the pathology of the affection, the author referred to recent German and Italian physicians, who had described necropsies in cases of diphtheritic paralysis, in which they found a structural change in the gray and white matter of the medulla, which some of them have termed *disseminated myelitis.* The author looked on these appearances, in the cases referred to, as secondary diphtheritic paralysis, though it has its own peculiarities and specialties, is similar in kind to the paralysis which we meet with as a sequel of typhoid fever, relapsing fever, scarlatina, and dysentery. In all it is peripheric. Its invariable starting-point is the velum pendulum palati; and that is a distinctive peculiarity between it and the paralysis following typhoid fever, relapsing fever, scarlatina, and dysentery.

c.—THERAPEUTICS OF THE NERVOUS SYSTEM AND MIND.

ELECTRICITY.—A. D. Rockwell, *Phil. Med. Times*, July 25, narrates five cases of herpes zoster, which were relieved by electricity. His conclusions are as follows:

(1) That the pain of herpes, no matter where the seat of the eruption may be, is generally susceptible of speedy and effectual relief by the use of the galvanic or faradic current.

(2) That when the eruptions take place on the head, the galvanic current alone has power to relieve the pain.

(3) When the disease is confined to the trunk or the extremities, the faradic current will relieve pain and hasten recovery, and is preferable to the galvanic.

AT A MEETING of the New York Neurological Society, held September 7, Dr. Clinton Wagner read a paper upon certain nervous diseases of the throat. In the treatment of this class of diseases, he employs, largely, electricity. This agent, he says, is not a new one in the treatment of aphonia. In 1800, Grofangiesen, of Berlin, applied it to the neck, but Mackenzie has the credit of having first applied it to the mucous membrane of the larynx over the paralyzed muscle. A band is fastened around the neck, to which one pole is attached over the thyroid cartilage, the other to a metallic sound, bent at the proper curve, and passed over the epiglottis into the larynx, by aid of the laryngoscope. Dr. W. thinks it immaterial whether the negative or positive pole is introduced into the larynx. He generally applies the constant current from a twenty-cell battery, but never uses more than from four to eight cells for internal application. In cases of long standing, he has sometimes employed the interrupted current, and has, in such cases, obtained more decided results from it than from the constant current, probably owing to the greater muscular excitation produced. In regard to pain, the patients complain of the constant as more severe. W.

Mode of Action of Emetics.—M. Chouppe communicated to the *Soc. de Biologie*, July 18 (reported in *Le Progres Medical*), the results of his researches in regard to some points as to the mode of action of emetics.

From numerous experiments made in the laboratory of M. Vulpian, it was found that emetine, in emetic doses, did not cause vomiting when injected into the subcutaneous cellular tissue or the veins, after the pneumogastrics had been previously divided. Tartar-emetic and apomorphine, on the other hand, produced the same effects whether these nerves were divided or intact. We may, therefore, allow that emetine acts always on the peripheral terminations of the vagi, while tartar-emetic and apomorphine act directly on the medulla, very probably on the nucleus of origin of the pneumogastrics.

Oxygen Inhalation.—Ananoff, *Centralbl. f. d. Med. Wissensch.*, No. 27, gives the results of experiments as to the effect of the inhalation of oxygen, carried on by himself, in the laboratory of Prof. Sabelin, in St. Petersburg.

Starting from the experiment of Uspensky, on the influence of artificial respiration on increased nervous irritability, he sought to ascertain the part which oxygen played in bringing about the effect. Two rabbits of similar size and weight were poisoned with strychnia, and one submitted to artificial respiration with oxygen gas, and the other with atmospheric air. The first showed no tetanic cramps during twenty-eight minutes that the artificial respiration of the gas was kept up, and lived ten minutes after its cessation, dying finally in slight convulsions; in the case of the other, cramps ensued in three and death in ten minutes. In order to discover the effect that the increased pressure alone might have, two similarly poisoned rabbits were submitted to artificial respiration, one from a gasometer under pressure, and the other under the ordinary conditions. In the first, convulsions ensued in seven minutes, and death in twenty; the second succumbed in six minutes.

A third experiment was then undertaken to ascertain the different effects of oxygen gas, and an increased pressure of atmospheric air. Of two strychnized rabbits thus experimented with, the one inhaling the gas showed no convulsions during the twenty-seven minutes the respiration was kept up, and only succumbed after thirty-two minutes, while the other exhibited tetanic spasms, upon irritation, in four minutes, which became spontaneous in seven, and carried it off in thirteen minutes.

From these results, Ananoff assumes that such effects on the increased reflex irritability, can only be expected with certainty when pure oxygen is introduced.

ALCOHOL.—Parkes, *Proc. Roy. Soc.*, 1874, No. 150. (Abstr. in *Centralbl. f. d. Med. Wissensch.*) "On the Influence of Brandy on the Bodily Temperature, the Pulse, and the Respirations of Healthy Men."

Extensive and carefully made observations on two English soldiers showed that cognac, in moderate doses (=60 ccm. absolute alcohol), without producing drunkenness, during rest, and with empty stomach, somewhat diminished the temperature in the rectum, not constantly indeed, but never increasing it. The decrease followed in one to two hours after the ingestion, and disappeared during the third. In earlier experiments (P. R. S., 1870), this escaped notice altogether, because, as the author now states, the alcohol was always taken on a full stomach. The pulse increased during rest, some five or ten beats a minute, and still more during movement. A slight absolute decrease of its frequency took place after the disappearance of the effect of the alcohol. The pulse was then also softer and fuller, as could be determined by the finger and the sphygmograph. The cephalic vessels showed a considerable fullness. The influence on the respiration was but slight; occasionally there was observed an insignificant alteration in the number and the depth of the respirations.

PROF. BINZ, of Bonn, *Journal of Anatomy and Physiology*, May, 1874, discusses the effect of alcohol on warm-blooded animals. He comes to the conclusions that it acts as a depressant of the temperature; that it decreases the metamorphosis of tissues; and that in certain cases of active fevers it acts as an antipyretic, while quinine and other remedies fail. As regards the question whether it is to be considered as a food or not, he holds that it depends upon circumstances. In ordinary conditions it is not required to sustain life. When, however, tissue metamorphosis is increased from any cause, such as cold, etc., it is another matter; it then becomes a direct food, since burning without heating, it yields warmth and power of tension, and it becomes an indirect food by decreasing the metamorphosis of tissue.

Only pure and good alcohol can be made use of in this way; the ordinary mixtures are injurious.

CHLORAL.—Dr. Lauder Brunton, *Jour. of Anat. and Phys.*, May, 1874, calls attention to the value of heat as an agent in the treatment of a person suffering from an overdose of chloral, and as a means of preventing death from that cause. He mentions the testimony of Richardson and Stricker in this regard, and gives tabulated statements of several experiments performed on Guinea pigs, by himself, in the laboratory of Dr. Burdon Sanderson.

He gives the following directions for treatment in the case of a person suffering from an overdose. "The patient should be put to bed and the temperature of the body maintained by warm blankets and hot water bottles to various parts of the body, and especially the cardiac region. Warmth over the heart is an excellent stimulant to the circulation, which, like the respiration, is enfeebled by chloral, the heart, according to Rajewsky, being more or less paralyzed by the drug. If respiration threatens to fail it should be maintained artificially, so as to allow time for the chloral to be excreted and the normal functions to be restored."

Chloral and Bromal Hydrates and Iodoform.—Dr. J. G. McKendrick communicates an interesting paper on the physiological action of these substances, to the *Edinburgh Med. Journal*, for July. After referring to the researches of Rabuteau, Steinauer and Dougall, he gives the results of his own experiments with these agents, and an analysis of each group of symptoms thus produced. Stated in brief the analysis of the action of bromal hydrate is as follows:

(1) The preliminary excitement indicates either a special stimulant action on the hemispheres or an action on various organs, giving rise on transmission to the cerebrum, to sensations of uneasiness or pain, or both of these actions combined. The author favors this latter supposition.

(2) The rapid and extreme contraction of the pupil is probably due to a paralysis of the sympathetic with, also, irritation of the cranial origin of the third nerve.

(3) The slight contraction, with subsequent dilatation of the blood-vessels, is considered to be due to an action of the drug on the vaso-motor centre in the medulla.

(4) Its action on the heart, first slowing and then stimulating it, with perhaps again a later period of diminished action, is considered to be due to interference with the sympathetic, and, to a slight degree, with the intrinsic ganglia.

The excessive secretory action which it causes is inferred to be due to a paralysis of the sympathetic, and to irritation of the vaso-motor and inhibitory filaments from such nerves as the chorda tympani.

The author next gives a comparison between the action of bromal hydrate and chloral hydrate, as follows:

(1) "Bromal hydrate is a more active substance, physiologically, than chloral hydrate. A rabbit weighing four pounds requires about twenty grains of chloral to cause death, whereas, four or five grains of bromal would be quite sufficient to kill.

(2) "Chloral hydrate produces, in small doses, or soon after a large dose, marked hyperæsthesia, followed by anæsthesia. Bromal hydrate never produces hyperæsthesia, and anæsthesia only when the animal is in such a state of coma that there is no hope of its recovery.

(3) "Chloral hydrate does not usually produce great contraction of the pupils. Bromal hydrate always does.

(4) "Chloral hydrate acts chiefly on the cerebral hemispheres, and never, so far as I know, has it been known to cause convulsions. Bromal hydrate acts less vigorously on the hemispheres and more on the ganglia at the base

of the brain and on the spinal cord, the animal frequently dying in a state of opisthotonos.

(5) "After death from chloral hydrate fluid is rarely found in the shut sacs of the body. In the case of bromal hydrate fluid is almost invariably found.

(6) "Chloral hydrate does not usually stimulate the salivary glands to the same extent as bromal hydrate does, but in this instance there are exceptional cases in which the chloral hydrate causes excessive secretion of saliva in animals."

Dr. McKendrick next gives briefly the results of experiments with a solution of iodoform, one grain to five of alcohol and fifteen of water. The effects were very similar to those produced by chloral, except that there was no period of hyperæsthesia; there seemed to be an irritation of the nostrils; the effect seemed more powerful than that of chloral, the lethal dose being smaller; there were no convulsions; the pupils were only slightly contracted, and there was no fluid in the cavities of the body.

ANTAGONISM OF BROMAL HYDRATE AND ATROPINE.—At the meeting of the Medico Chirurg. Soc., of Edinburgh, May 6 (Rep. in *Edin. Med. Journal*), Dr. McKendrick stated that he had recently observed a well-marked instance of physiological antagonism between bromal hydrate and atropine. Bromal hydrate, when administered in a dose of three grains to a medium sized rabbit, produced contraction of the pupil, dilatation of the blood-vessels, profuse salivation, œdema of the lungs, and effusion of fluid into the pleural, pericardial, and peritoneal cavities, and other effects. He had observed that half a grain of atropine arrested the hyper-secretion, and in various cases saved life from what was afterwards found to be a lethal dose. The detailed results he expected soon to communicate to the society. In the meantime he announced the fact as an example of the existence of a true physiological antagonism between certain active substances, as had been so well shown by Fraser and others.

HASCHISCH.—A. Naquet, *Comptes Rendus*, lxxvii, (Abstr. in *Deutsche Klinik*) gives the results of experiments on himself and others with the Indian hemp, using for convenience the tincture rather than the other preparations. He found it to be very inconstant in its effects, the same dose producing very different results on different individuals, and on the same person at different times. He classes the psychic symptoms under two categories: those proper to the drug, and those peculiar to the individual. Among the former are hallucinations, which give to the person under the influence of the drug the impression that he is moving, riding, swimming, flying, etc., or that he has lost all weight; later, an increase in the usual ideas which follow each other with extreme rapidity, so that abrupt transitions from weeping to laughter are frequent. Naquet also found as a constant symptom of haschisch intoxication, an inclination to play upon words and ndulge in grammatical discussions.

Bromide of Camphor.—M. Bourneville made a communication to the *Soc. de Biologie*, June 13 (Rep. in *Rev. Scientifique*), on the physiological action of bromide of camphor (C. 10, H. 16, OBr.), in which he gave only the points which seemed to him best established, at the present state of our knowledge. In his experiments, some twenty in number, he always administered a solution of the salt by the hypodermic method. According to him bromide of camphor produces: (1) A diminution in the number of the cardiac pulsations, which fell, for example, in a kitten three months old, from 188 to 80, after it had received sixty centigrammes of bromide of camphor; (2) a diminution in the number of respirations, which in the same animal descended from 70 to 16 in the same time, that is in fifty-eight hours; (3) a lowering of the temperature, which in the same case fell from 38.8° C.(=101 F.) to 21.7° C. (=70° F.), an hour before death; (4) a soporific action, which is the more marked the greater the dose, and so powerful with a large dose (five to eight grains), that, unless we watched the heart-beats and the respiration, we would think the animal dead.

According to M. Bourneville, the ability of the organism to accustom itself to this agent is almost *nul*. In Guinea pigs, which had received for many days injections of five or ten centigrammes, the lowering of the temperature, with other symptoms, remained always the same. Moreover, bromide of camphor always produces, at least in Guinea pigs, a rapid wasting.

From these experiments it seems that bromide of camphor is a powerful sedative. The author has given it at the Saltpetriere, in the service and under the direction of M. Charcot, to a number of patients suffering from affections of the nervous system (hysteria, insomnia, epilepsy, chorea), and promises to give the society, at a future time, the results of his new physiological researches and therapeutical experiments.

A fuller account of these researches is published by M. Bourneville in his journal, *Le Progres Medical*, No. 25.

Hydrotherapy in Inflammatory Cerebral Affections.—Dr. C. F. Rohrer, *Deutsches Archiv. fur Klin. Medicin, XIII., XIV., and XV.*, 512, publishes an account of some fifteen cases of inflammatory affections involving the brain or its envelopes, or with marked cerebral symptoms, in which the cold douche was employed with good result. He sums up its effects as follows:

(1) It lowers the temperature of the body, by abstracting warmth.

(2) It is antispasmodic and calmative in tonic and clonic conditions of the muscular system, in consequence of cerebral irritation.

(3) Derivative: When the water is applied locally on the skin of the head or neck, we see redness of the skin to ensue when a sharp stream of cold water is used. With heightened temperature of the skin and lower temperature of the water, this action is the more intense.

(4) Irritative to the peripheral nerves: A very intense reaction takes place in a healthy individual, when cold water is dashed over him in a warm room, or even when it is applied otherwise. The excitation of a powerful respiratory impulse is the centripetal result of the peripheral irritation.

The indications for this method of treatment, according to the author, are as follows:

In all febrile conditions.

(*a*) In symptoms of hyperæmia or irritation of the brain.

(*b*) In meningitis, without regard to cause, especially as soon as there is any suspicion of an inflammatory cerebral affection.

(*c*) In very high temperature of the body, with resulting cerebral disorder.

(*d*) In toxæmias (infectious diseases), with greater or less degree of febrile disturbance, if cerebral disorders (sopor, coma, furious delirium) are present.

(*e*) In disturbances of the circulation in the region of the right ventricle, as a result of insufficient respiration: hypostasis, croupose pneumonia.

CROTON CHLORAL.—Dr. Jules Worms publishes, *Bull. Gen. d' Therapeutique*, May 30, an account of several cases in which he employed this medicament. We can give here only his conclusions, which, as he says, are not final, as he intends to continue his investigations with care. He sums up as follows:

(1) It is hypnotic in certain cases, in doses of from fifty to seventy-five centigrammes (=7.11 gr.)

(2) It is not as generally tolerated as chloral.

(3) It appears scarcely suitable for administration by the hypodermic method.

CAFFEIN.—O. Schmiedeberg, *Arch. f. Exper. Path.*, 1874, 62–69 (Abstr. in *Centralblatt f. d. Med. Wissensch.*), reports the different actions of caffein on the two species, *Rana temporaria* and *Rana esculenta.* In the former it first produces a stiffness of the muscles, gradually extending from the place of application to the more distant parts, without bringing on tetanic attacks. The increased irritability sets in the second or third day of the poisoning. In the other species, on the contrary, caffein produces violent attacks of tetanus, and a certain stiffness of the muscles is only noticeable at a later period, and then not to the same degree as in *R. temporaria.*

In order to explain this difference, Schmiedeberg assumes a different constitution of the muscular substance in the two species, together with an unequal susceptibility of the spinal cord.

The muscular rigidity, according to this author, is of a similar nature to that which takes place after death—it advances *pari pasu* with the decrease of irritability. Caffein acts entirely different from other muscle poisons (saponin, cyclamin), which affect the muscular irritability without causing rigidity.

SUBCUTANEOUS INJECTIONS OF CHLORATE OF MORPHIA.—M. Chouppe offered the following note on the mode of action of hypodermic injections of morphia, at the session of the *Soc. de Biologie*, July 18 (reported in *Gaz Med. de Paris*), August 1:

"The results which I submit to the Societe de Biologie, are the conclusions from more than one hundred experiments performed on myself, which have enabled me to analyze in detail a number of little-known subjective phenomena.

"I believe that we ought to attribute to hypodermic injections of morphia a local anæsthetic influence. I hold this opinion on the basis of the two following facts:

"First, when the injection is performed *loco dolenti*, the effect of the drug is felt more quickly (about two or two and a half minutes) than when it is given remote from the seat of the pain. Moreover, when the injection is performed at the painful point, the pain ceases before the general effects of the morphia are experienced. When, on the other hand, it is performed in any other part whatever, the general effects are first experienced, the subject feels a notable sensation of well being, while the pain, though diminished, is still indistinctly felt.

"The second proof is more direct. It is supplied by the study of the sensations experienced at the point where the puncture is made.

"A solution of chlorate of morphia, strength of 1-30, injected under the skin, causes no pain at the point of injection. Distilled water causes severe pains, with irradiations for some little distance. The solution of 1-150 is very painful, while one of 1-50 causes no pain. It appears to me impossible to account for this difference, except by an anæsthetic action on the nerves of the subcutaneous tissues.

"The local action of morphia being thus demonstrated to my satisfaction, it appears to me always preferable to make the puncture at the seat of pain; farther, it seems much more advantageous to employ concentrated solutions, and, for a small dose, to inject very little water. Nevertheless, as the final result is always the same, it is not needful to too persistently oppose individual opposition."

THE TREATMENT OF CHOLERA BY SUBCUTANEOUS INJECTION OF CHLORAL HYDRATE.—A writer in the *Times*, of Aug. 17th, calls attention to a plan of treatment of epidemic cholera which has been followed in India with apparent success. It consists in the subcutaneous injection of a solution of hydrate of chloral (ten grains in one hundred drops of water). The writer copies from the supplement to the *Gazette of India*, of Feb. 14th of this year, a report on the subject from Mr. Higginson, Civil Surgeon at Kheri, in Oudh, and a letter from Dr. Hall, who claims the merit of having originated the method. In a letter to the commissioner of Sitapur, dated October 18th, 1873, Dr. Hall states that in the *Indian Annals of Medical Science* for March, 1870, he advanced the theory that, in the collapse stage of cholera there was intense irritation of certain sets of nerves; and that hence the use of sedatives was indicated. These he recommended, should be given by hypodermic injection; it being of little use to give medicines by the mouth. Subsequently, having in conjunction with Surgeon Major Collis, ascertained by experiment the powerfully sedative action of hydrate of chloral, he tried the remedy with success on a soldier in the collapse of cholera, recovery being established in about five hours. He gives the following directions for the use of the medicine.

"I attach great importance to the strength of the solution used (one part in ten of water); if it is too strong it probably will not be absorbed in the blood, and only cause ulceration or sloughing.

"The following is the plan of treatment that I recommend for the three stages of cholera. During the premonitory diarrhœa (which is almost always painless), thirty drops of dilute sulphuric acid in a large wineglassful of camphor water, every hour. If this does not stop it, and vomiting comes on, commence the hypodermic injections at once. If the patient is first seen in collapse, inject ten grains, dissolved in one hundred drops of water, in four or five different places, according to the size of the syringe. This will probably be enough, but if reaction does not commence within an hour inject again. There is generally a great craving for cold water, which may be given in any quantity. Never mind if the patient vomits; as reaction proceeds he will retain it. But no opium or stimulants are to be given in collapse.

"After reaction if secondary fever supervenes, quinine in varying doses, every one or two hours, may be given with milk, beef tea, and mild stimulants."

Mr. Higginson, in his report to the deputy commissioner at Kheri, states that he has treated nineteen cases of cholera according to the method recommended by Dr. Hall, and that death had occurred in only two of these; one being a case in which the directions were not properly carried out, and the other an exceedingly severe and rapid case. The seventeen patients who recovered were in various degrees of collapse when first seen. Regarding the manner of administering the remedy, he says:

"The injections were made in the arms and thighs, the canula of the syringe being plunged pretty deeply into the flesh. The greatest quantity of chloral used in any of my cases was sixteen grains, or eight injections. If the case was a bad one, four injections were made at once; then nothing was done for an hour, when the treatment was repeated if necessary. As a rule sleep was induced within two hours. Nothing else was done, except mulling (shampooing) the limbs, and giving occasionally some cold boiled water."

The result of the treatment is thus described by Dr. Higginson:

"Chloral hydrate, being a powerful sedative, soothes the irritated nerves and so relaxes the contracted vessels; the blood is once more uniformly distributed, and consequently the pulse reappears at the wrist; the cramps and burning abdominal pains subside, sleep is induced, the respiration becomes regular, the discharges lessen, the face fills out, the voice becomes stronger, and the natural secretions are restored."

The Deputy Surgeon General at Lucknow, in a letter to the Senior Secretary to the Chief Commissioner at Oudh, dated November 10th, of last year, recommended that the report of Messrs. Higginson and Hall "should be printed in its entirety for distribution to the various medical officers, civil and military, within the circle."

The success with which Mr. Higginson has met (the recoveries amounting to eighty-nine per cent.), is certainly such as to encourage a further trial of the hypodermic injection of chloral hydrate in cholera. At the same time, much more extensive observation is required before any definite conclusion can be arrived at as to the value or inutility of the treatment. We shall watch with interest any additional observations on this important subject.—*Brit. Med. Jour.*, Aug. 22.

ERGOTIN.—Dr. H. Koehler, *Virchow's Archiv*, 60 Bd., 3 and 4 Hft., p. 384, gives the result of an experimental investigation as to the physiological action of the Bonjean and the Wiggers preparations of ergotin. These are not chemically pure alkaloids, corresponding to quinine, morphia, etc., but mixtures, the one containing all the constituents of ergot which are soluble in water, and the other (Wiggers) those which are insoluble in the same. No previous researches had established a comparative statement of the physiological action of these two extracts; the majority of experimenters had employed only the one soluble in water. Dr. Kœhler therefore instituted a comparative investigation into their action (experimenting with frogs, dogs, and rabbits,) on the movements of the heart, the blood pressure, temperature, respirations, and functions of the nervous system and the muscles. Any inquiry into the alterations in the blood and secretions, by the one or the other of these extracts, seemed to him premature, so long as we are yet ignorant of their effective chemical constituents. The method of experimenting (injection into the veins) gave no opportunity to observe direct alterations in the digestive functions. In part of the investigations Dr. Eberty participated, and some of the results were published in his inaugural dissertation, in 1873. The following is the summary of the researches as given by Dr. Kœhler.

(1) The constituents of ergot, which are soluble in water (ergotin Bonjean), and those which are soluble in alcohol (ergotin of Wiggers), differ throughout, not only in their relations of solubility but also in their physiological effects.

(2) The Bonjean ergotin excites the inhibitory centres in the heart, and the vaso-motor centre in the medulla; slackening of the pulse, narrowing of the calibre of the arterioles, and increase of the blood pressure are the consequences. Very large doses cause immediate paralysis of the heart, in which its muscular structure is unexcitable by the induction stream. These effects on the heart and blood pressure are not produced at all by the Wiggers ergotin.

(3) On the other hand, the constituents of the Wiggers ergotin, of which only very slight traces are occasionally detected in the watery infusion of ergot, cause effects which are to be placed by the side of those of the acrid narcotics. Not only does the irritation of the mucous membrane of the stomach and bowels from large doses of ergot appear to depend upon this, but animals poisoned with the Wiggers ergotin suffer tonic cramps and are shaken by violent convulsions—phenomena which were not observed after the introduction of large quantities of the Bonjean preparation.

(4) The constituents of both kinds lower the bodily temperature.

(5) Both also cause a retardation of the respiration; the Wiggers ergotin is, in this respect, more energetic than the other. Only in dogs was the respiration extremely rapid after the injection of the Bonjean ergotin.

(6) The Bonjean ergotin reduces the irritability of the peripheral motor

nerves if it comes more or less directly in contact with them ; the Wiggers ergotin, on the other hand, heightens their irritability. The pupils are dilated by both—still, myosis was also observed.

(7) Both diminish the irritability of the peripheral sensory nerves.

(8) Neither kind exercises any deleterious influence whatever on the striped muscles.

(9) Where the vessel contracting, pulse slowing, temperature and reflex action lowering effect is required, the Bonjean ergotin, in which the acro-narcotic elements of the drug are wanting, should be employed. The Wiggers ergotin is not available as a hæmostatic, and, with its active narcotic elements it is inferior also as an agent for reducing the temperature and reflex activity.

(10) Inasmuch as the heightened irritability of the peripheral motor nerves, induced by the constituents of ergot soluble in alcohol (apart from the irritation caused by anæmia. *Wernich*), plays a part in the parturient action of ergot, the ergot in substance should be preferred to the Bonjean ergotin for the production of labor pains.

MUSCARINE.—M. J. L. Prevost presented a new communication to the *Academie des Sciences*, Aug. 10, on this agent. In a later series of experiments than those heretofore given (see July No. of this journal, p. 410) he had studied its action on the pancreatic, biliary, and urinary secretions, points which had not been studied before. These experiments showed that muscarine produced a hypersecretion from the liver and pancreas, while it diminished the urinary secretion to the point of complete suppression for a certain time. Atropine caused a suppression of these phenomena, and can therefore be considered as the physiological antagonist of muscarine.

The experiments were performed in the physiological laboratory of the University of Geneva, with the assistance of MM. David and Murisier, *internes* of the Cantonal Hospital of Geneva.

DELPHINE.—In a communication offered to the *Soc. de Biologie*, July 25, M. A. Rabuteau details two experiments with this alkaloid, of which he remarks as follows :

These two experiments establish very clearly a relation between the effects of delphine and those of such poisons as act in the manner of curare, such as the calabar bean, aconitia, the iodides of tetramethylammonium and of tetralammonium. We see, in fact, first the difficulty and next the impossibility of voluntary movements without any abolition of muscular contractility, and finally death by asphyxia or syncope, according to the rapidity with which either the dilator nerves of the chest or the automotor ganglia of the heart are paralyzed. The elevation of the temperature in the dog which was the subject of the first experiment, the presence of sugar, in small amount, it is true, in the urine of this animal, are likewise symptoms that we observe in cases of poisoning by curare.

Dr. Rabuteau next gives a comparative statement of the appearances observed in frogs poisoned respectively with delphine and veratrine, two agents which have been classed together erroneously, veratrine being a muscular poison. This he sums up as follows :

From what precedes, it results : (1) that delphine does not act on the muscular system, but on the nervous system, after the manner of curare ; nevertheless it seems to diminish the sensibility in a notable manner ; (2) veratrine acts on the muscular fibre which it first tetanizes, and then abolishes its contractility, as has been well shown by our colleague, M. Prevost, in 1868, who is moreover well assured that the muscular rigidity does not depend on any action exercised on the spinal cord, as is the case in poisoning by strychnine. It does not seem to diminish the sensibility as much as veratrine.

ACONITINE.—M. Alphonse Paquet, at the meeting of the Medical Section of the French Association for the Advancement of Science, at Lille, Aug. 21, (reported in *Rev. Scientifique*) presented a paper on the use of aconitine against the severe accidents consecutive to traumatism. His memoir was based on three observations of tetanus and trismus, two of traumatic erysipelas, and three operations for strangulated hernia, one case in particular, of umbilical hernia in a woman aged 50, in which aconitine was employed with perfect success. The powerful action of this alkaloid on the nervous and circulatory systems, the rapid and considerable fall of the pulse and the temperature which follow its use, seemed to him to recommend its usage as a preventive and curative medication for the tetanic, inflammatory, and even the infectious accidents which may follow accidental or surgical injuries. The aconitine used in his experiments was obtained from the laboratory of Merck, of Darmstadt, the preparation best known and most easily procured. It contains pure aconite, napelline, less active than aconitine, and aconelline, which seems not to possess the toxic properties of aconitine. It is ten times less active than the crystallized aconitine, which, besides its toxic effects in the dose of one milligramme (=.015 grains), is extremely difficult to procure.

CHERVIN'S TREATMENT OF STAMMERING.—At the session of the *Academie de Medicine*, Aug. 25, M. Moutard-Martin presented a report of a commission of the Academy, appointed at the request of the Prefect of the Seine, on M. Chervin's method of cure for stammering, the following abstract of which is taken from the *Gaz. Med. de Paris.*

The reporter commenced by stating the existence as constant facts in this disorder, of respiratory troubles and irregularity in the movements of the tongue, which fails to respond instinctively and without effort to the orders of the will.

According to M. Chervin, the respiration is embarrassed and constrained in its action during the utterance of words by nervous movements of the tongue, the lips, the facial muscles of the stammering individual. His method, which is essentially rational, consists in regulating the respiration in its two movements, in prolonging the expiration so as to permit the articulation of an entire phrase without stopping, in combating the choreic condition of the muscles, and finally, in directing the normal positions of the tongue and lips and the degree of opening of the mouth in the pronunciation of letters, syllables and sentences. Finally he finishes by teaching the tone and expression of phrases.

Stammerers do not stammer in singing; song is a gymnastic exercise of the respiration and the vocal organs, which are sustained and guided by the rhythm. This exercise forms the basis of M. Chervin's treatment, first exercise for the respiration, then for the muscles; gymnastic exercise is one of the most efficacious methods of treatment for chorea.

After a period of perfect silence, which ought to precede the treatment in order to give rest to the organs and break up vicious habits, the treatment begins by exercises in respiration, followed by practice in the pronunciation of vowels, which commences the gymnastic of the organs of articulation proper, and then begins also the demonstration of the positions which the tongue and the lips should take, and the shape of the mouth in uttering each letter of the alphabet. To these first exercises succeed assembled letters, vowels and consonants, in the different positions which they may take, and finally words and phrases with their proper intonation and expression.

The basis of the exercises is imitation. The teacher executes himself every movement that he orders: respires with his students, utters the different sounds with them, and pronounces the phrases they repeat with him. He is, for them, the instrument which guides and supports the singer.

The duration of the treatment is very short, it lasts but twenty days, but these are well employed. M. Chervin holds his students from 8 A. M. to 6 P. M.; he gives them four hours of lessons each day, and during the remaining time they should preserve a strict silence, so as not to fall back into their bad habits.

The Commission of the Academy of Medicine assisted at many lessons and the last day they reviewed and interrogated each one of the students who had followed the course, fifteen in number. Among these fifteen, fourteen spoke readily and easily, though some uttered words and phrases with a rhythm. One of the two women who were treated, although much improved, was not fully cured; but she was an Alsatian, understanding French with difficulty, and did not know how to read, all of which complicated the treatment.

On Friday, Aug. 21, the Commission saw again seven of the pupils a month after the termination of the treatment, among them the Alsatian, whose condition was still notably improved; the other six spoke positively well; the other seven who were not seen responded by writing that they were cured and satisfied.

In view of these facts the Commission proposed to respond to the Prefect of the Seine:

1. That in a scientific point of view M. Chervin's treatment for stammering is rational;

2. That it is attended with very remarkable results, and may be of signal service;

3. That one of its important advantages is the promptness of its results, which seem to be permanent as determined by the Commission in a certain number of cases;

4. That it should be encouraged and aided in the good work it is called to perform.

THE FOLLOWING FOREIGN PERIODICALS HAVE BEEN RECEIVED SINCE OUR LAST ISSUE.

Allgemeine Medicinische Central-Zeitung.
Allgemeine Zeitschrift fur Psychiatrie und Psychisch. Gerichtl. Medicin.
Annales Medico-Psychologiques.
Archiv fur Anatomie, Physiologie, und Wissenschaftl. Medicin.
Archiv fur Path. Anatomie, Physiologie, und fur Klin. Medicin.
Archiv fur die Gesammte Physiologie der Menschen und Thiere.
Archiv der Heilkunde.
Archiv fur Psychiatrie.
Archives Generales de Medicine.
Archives de Physiologie, Normale et Pathologique.
Archivio Italiano, per le Malattie Nervosi.
Berliner Klinische Wochenschrift.
British Medical Journal.
British and Foreign Medico-Chirurgical Review.
Bulletin Generale de Therapeutique.
Centralblatt f. d. Med. Wissenschaften.
Der Irrenfreund.
Deutsches Archiv f. Klinisches Medicin.
Deutsche Klinik.
Dublin Journal of Medical Sciences.
Edinburgh Medical Journal.
Friedrich's Blaetter f. Gerichtl. Medicin.
Gazette Medicale de Paris.
Gazette des Hopitaux.
Giambattista Vico.
Il Galvagni.
Jahrbuch f. Kinderheilkunde u. Physische Erziehung.
Jahresbericht u. d. Leistungen u. Fortschritte in der Gesammt. Medicin.
Jenaische Zeitschrift f. Medicin u. Naturwissenschaften.
Journal of Anatomy and Physiology.
Journal de l'Anatomie et de Physiologie, etc.
Journal of Mental Science.
Lo Sperimentale.
L'Union Medicale.
Medicinisches Correspondenzblatt des Wurtemb. Aerztl. Vereins.
Medicinisches Jahrbucher.
La Nuova Liguria Medica.
Progres Medicale.
Psychiatrisches Centralblatt.

Rivista Clinica.
Revue des Sciences Medicales.
Revue Scientifique.
Schmidt's Jahrbucher der In- und Auslandischen Gesammten Medicin.
The Practitioner.
Vierteljahreschrift fur die Prakt. Heilkunde.
Wiener Medicinische Press.
Zeitschrift f. Biologie.

The following domestic exchanges have been received:

American Journal of Insanity.
American Journal of Medical Sciences.
American Journal of Obstetrics.
American Journal of Pharmacy.
American Journal of Syphilography.
American Medical Weekly.
American Naturalist.
American Practitioner.
Atlanta Medical and Surgical Journal.
Boston Medical and Surgical Journal.
Canada Medical Record.
Chicago Medical Journal.
Clinic.
Cincinnati Lancet and Observer.
Cincinnati Medical Times.
Detroit Review of Medicine and Pharmacy.
Indiana Journal of Medicine.
Medical Examiner.
Medical Herald.
Medical Investigator.
Medical News and Library.
Medical Record.
Medical and Surgical Reporter.
New York Medical Journal.
Pennsylvania Journal of Medicine.
Pacific Medical and Surgical Journal.
Philadelphia Medical Times.
Psychological and Medico-Legal Journal.
Richmond and Louisville Medical Journal.
Sanitarian.
St. Louis Medical and Surgical Journal.
Texas Medical Journal.
U. S. Medical and Surgical Journal.
Virginia Medical Monthly.

BOOKS, ETC., RECEIVED.

Note.—The foreign works in this list may be obtained through Messrs. B. Westermann & Co., No. 524 Broadway, New York.

A Practical Treatise on the Diseases of Women. By T. Gaillard Thomas, M. D., etc. Fourth edition. Thoroughly revised. With one hundred and ninety-one illustrations on wood. Philadelphia: 1874. Henry C. Lea. Chicago: Jansen, McClurg & Co. 8vo.; 801 pages.

Anthropogenie. Entwickelungs-geschichte des Menschen. Von Ernst Haeckel, Professor an der Universitaet Jena. Mit 12 Tafeln, 210 Holzschnitten und 36 Genetischen Tabellen. Leipzig: 1874. Large 8vo.; 732 pages.

Clinical Lectures on Diseases of the Nervous System. By Wm. A. Hammond, M.D. Reported, edited, and the histories of the cases prepared with notes, by T. M. B. Cross, M.D. New York: 1874. D. Appleton & Co. Chicago: Jansen, McClurg & Co.

Ueber die Bedeutung des Nervensystems. Ein popularer vortrag. Von Dr. Georg Hermann Meyer. Stuttgart: 1874. 12mo.; 36 pages.

Essai sur les Variations de l'Uree. Par Louis Foulhoux, Docteur en Medicine de la Faculte de Paris. Paris: 1874. 8vo.; 136 pages.

Handbuch der Speciellen Pathologie und Therapie. Herausgegeben von Dr. H. v. Ziemssen. Zweiter Band. Acute Infectionskrankheiten. II. Theil. Leipzig: 1874. 8vo.; 753 pages.

Transactions of the Medical Association of the State of Alabama. 27th Session, 1874. Montgomery: 1874. 8vo.; 421 pages.

Direct Local Medication in the Treatment of Chronic Catarrhal Inflammation of the Nasal and Pharyngo-nasal Cavities. By Thos. F. Rumbold, M.D., St. Louis, Mo.

A Review of the Recent Researches in the Pathology and Treatment of Cancer.

Report of cases treated by Electricity. Reprinted from the Transactions of the Medical and Chirurgical Faculty of Md.: 1873. By J. J. Caldwell, M. D.

The Connection between Excessive Nerve and Brain Worry, and Bodily Disease, with Pathology and Treatment of Cancers, Tumors, etc. By J. J. Caldwell, M. D.

Lecons sur l'Appareil Vaso-Moteur (Physiologie et Pathologie) faites a la Faculte de Medicine de Paris, par M. le Professeur A. Vulpian. Redigees et publiees par M. le Docteur Carville. Tome I. 8vo. Paris: 1874.

Second Annual Report, Charter of Incorporation and By-laws of the Woman's Hospital of the State of Illinois. No. 229 Thirtieth Street, Chicago, January 1, 1874.

Transactions of the Wisconsin State Medical Society for the year 1874, with the Constitution and By-laws of the Society, and a list of its members. Vol VIII. Milwaukee: 1874; 119 pages.

Transactions of the Medical and Chirurgical Faculty of the State of Maryland. Seventy-fifth Annual Session. Held at Baltimore, Md., April, 1874. 8vo.; 199 pages.

Cell-life, the Source of all Power, both Physical and Mental, in the Human Economy. The Annual Oration delivered before the Medical Association of the State of Alabama, April 14th, 1874. 8vo.; 31 pages.

College of Physicians and Surgeons of Indiana. First Annual Announcement. Session of 1874 and 1875. Indianapolis, Ind.

Fifth Annual Announcement of the Woman's Hospital Medical College of Chicago, Ill. Session of 1874–5.

Ninth Annual Report of the Chicago Hospital for Women and Children. For the year ending Feb. 26, 1874.

Arbeiten aus der Physiologischen Anstalt, zu Leipzig. Achter Jahrgang: 1874. Mitgetheilt durch C. Ludwig. Leipzig: 1874. 8vo.; 280 pages.

Vier Psychologische Vortraege. Von Dr. E. Fortlage. Jena: 1874. 8vo.; 136 pages.

Physiologie de la Volonte. Par A. Herzen. Traduit de l'Italien par le Dr. Ch. Letourneau. Paris: 1874. 12mo.; 191 pages.

Chimie Appliquee a la Physiologie a la Pathologie, et a l'Hygiene. Avec les Analyses et les Methodes de Recherches les plus Nouvelles. Par E. J. Armand Gautier. Paris: 1874. Tome Premier. Chimie Appliquee a l'Hygiene. Chimie Appliquee a la Physiologie. 1re partie, 8vo.; 592 pages. Tome Second. Chimie Appliquee a la Physiologie. 2e partie. Chimie Appliquee a la Pathologie. 8vo.; 598 pages.

Des Troubles de la Sensibilite Generale, dans la Periode Secondaire de la Syphilis et Notamment de l'Analgesie Syphilitique. Par Moustapha Faid, Docteur en Medicine, etc. Paris: 1870. 8vo.; 132 pages.

De l'Hemiplegie Pneumonique. Par le Dr. Raphael Lepine. Paris: 1870. 8vo.; 39 pages.

Recherches sur l'Hypnotisme ou Sommeil Nerveux. Par MM. les Docteurs Demarquay et Girand-Teulon. Paris: 1860. 8vo.; 56 pages.

Etudes Experimentales sur le Fluide Nerveux et Solution Definitive du Probleme Spirite. Par A. Chevillard. Paris: 1869. 8vo.; 38 pages.

Des Symptomes Intellectuelle de la Folie. Par Eugene Semerie, Docteur en Medicine. Paris: 1867. 8vo.; 104 pages.

De la Sacro-Coxalgie. These presentee au Concours pour l'Agregation (Section de Chirurgie). Par le Dr. E. Delens. Avec 2 planches en lithographie. Paris: 1872. 8vo.; 114 pages.

Transactions of the Wisconsin Academy of Sciences, Arts, and Letters. Vol. II. 1873–4. Published by Order of the Legislature. Madison: 1874. 8vo.; 254 pages.

Lecons sur la Physiologie et l'Anatomie comparee de l'Homme et des Animaux. Faites a la Faculte des Sciences de Paris. Par H. Milne Edwards. Tome dixieme, deuxieme partie. Appareil de la Locomotion. Paris: 1874. 8vo.; pages 252—515 incl.

Der Darwinismus und die Naturforschung Newtons und Cuviers. Beitrage zur Methodik der Naturforschung und zur Species frage. Von Dr. Albert Wigand. Erster Band. Braunschweig: 1874. 8vo.; 462 pages.

On the Functions of the Sympathetic System of Nerves, as a Physiological Basis for a Rational System of Therapeutics. By Edward Meryon, M. D., F.R.C.P. London: 1872. 8vo.; 68 pages.

A Manual of Psychological Medicine, containing the Lunacy Laws, the Nosology, Ætiology, Statistics, Description, Diagnosis, Pathology and Treatment of Insanity. With an appendix of cases. By John Charles Bucknill, M. D., Lond., etc., and by Daniel Hack Tuke, M.D. Third edition, revised, illustrated and much enlarged. Philadelphia: 1874. Lindsay & Blakiston. Chicago: Jansen, McClurg & Co.

Wo sind die Seelenstoerungen in ihrem Beginne zu behandeln? Oder die Nuetzlichkeit und Nothwendigkeit der offener Kuranstalten fur Nervenkranke und Leichtverstimmte. Von Dr. Walther, in Carlsdorf. Neuwied and Leipzig: 1874. 8vo.; 30 pages.

Chicago Relief and Aid Society. Report of the Committee on Sick, Hospital and Sanitary Measures. (From the General Report of the Society.) Printed for the Chicago Relief and Aid Society, at the Riverside Press: 1874. 63 pages.

A New Method of Treating Malignant Tumors by Electrolyzing the Base. By Geo. M. Beard, A.M., M.D. (Reprinted from Dr. Beard's "Archives of Electrology and Neurology" for May, 1874.) 16 pages; 8vo.

Atmospheric Electricity and Ozone, their Relation to Health and Disease. By Geo. M. Beard, A.M., M.D. (Reprinted from the Popular Science Monthly, Feb., 1874.) 16 pages; 8vo.

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